Introduction

The notion of cause is replaced by the notion of law. Instead of causal relation, we have the conception of a continuous succession of events logically connected with one another by an underlying principle...

Sir Edmund Whittaker

1 Only rarely is motivation said to be a fact of human experience, that is, a mental event, which determines the course of action. The idea of motivation does not originate from what men say either about their own experience or about their own behavior. It is not one of the “indigenous problems” of psychology.

2 Nor is motivation a fact of behavior. There is not one feature or aspect or characteristic of behavior to which we invariably have reference when we say that some behavior is motivated. Although some writers have suggested behavioral criteria to define motivation, these attempts to specify what is meant by motivation are not very compelling. There is little agreement among the different proposals about what the defining criteria should be. We may say, for example, that an animal that has been deprived of food is hungry, or that it has a hunger drive, or that it “looks” motivated. But even though we may agree on this, it is not so clear that we can agree on what characteristic of its behavior makes the animal look motivated.

3 What one proposes as a definition of motivated behavior seems to depend more upon his theoretical commitments than upon anything in the behavior itself. Any solution to the problem of what it is about a particular behavior that makes it appear motivated will therefore depend upon how we regard behavior in general and how we explain it in general. Thus, motivation seems to be neither a fact of experience nor a fact of behavior, but rather an idea or concept we introduce when we undertake to explain behavior.

The Explanation Of Behavior

4 Sometimes we are fortunate enough to observe behavior occurring as a direct response to prevailing stimulus conditions in the environment. In such cases no very elaborate explanation is necessary; we may simply cite the eliciting conditions. In these cases of reflexive responses, the behavior of the organism becomes nearly as predictable as the behavior of simple physical systems, and our explanation can be correspondingly simple. More frequently, though, no identifiable external stimulus can be specified for a certain act. In this case behavior might be explained indirectly as the result of stimuli that have been effective in the past or it might be explained indirectly as a result of the physical structure of the individual, or of its prior experience. But all such explanations would be relatively indirect compared with the simple idea that there is a single active internal agency which, if it could be located, would provide a direct explanation. If such an internal agency or cause of behavior could be found, our explanation of all behavior could
then be as simple as it is in the case of the reflex. Typically, the search for such an agency is fruitless, and it is then that we take the much easier course of hypothesizing the existence of an appropriate agency. Different theories of motivation are distinguished primarily by the different sorts of motivating agencies that they hypothesize.

5 In this sense the most enduring theory of motivation is that which attributes a man's behavior to the results of his own mental processes. We can designate this traditional approach to the problem of explaining behavior by any name we wish, since it has no accepted name (prevailing doctrines often don't). Let us call it rationalism or, more precisely, traditional rationalism.

**Traditional Rationalism**

6 The naive and traditional explanation of human behavior is that we act because we have reasons for acting. Because we have free will, our reasons constitute a sufficient account of the whole matter. Such was the common view of the Greek philosophers, and such is the common view of the layman today. Traditional rationalism, of course, receives considerable support from our continuing use of it in our day-to-day contact with people. We hold our fellow man personally responsible as the author of his actions, and society expects him to describe his own actions in terms of intention, awareness, and purpose. We teach our children to use these words by making our transactions with them contingent upon what we consider to be their proper usage. We all do this, even the most behavioristic of us, because that is, in turn, what we have learned to do.

7 We attribute a man's behavior to events going on in his mind. This is the common and familiar variety of explanation which provides the point of departure for all other theories of motivation. All alternative conceptions of motivation and all alternative motivation constructs arise as reactions to this traditional rationalistic doctrine.

8 There are two distinguishing characteristics of traditional rationalistic explanations of behavior. These explanations are almost invariably (1) teleological and (2) untestable.

**Teleology**

9 When we speak in everyday language about the reasons for some behavior or about its purpose, we usually have reference to the mind, and more specifically to the conscious intentions of the person behaving. And to the extent that the individual has some purpose or intention that is focused upon the future, such an explanation is said to be teleological. Today we tend to restrict teleological explanations to human behavior because of our conviction that only man can foresee the consequences of his actions. The idea that purpose always implies intention and that some reasoning intellect, either man's or God's, must be the author of the intention is a feature of Christian philosophy; it was formalized by Augustine and the other codifiers of Christian theology. By contrast, Greek philosophers found it possible to consider purpose, and even reason, as characteristics of nature quite apart from any conscious intention on the part of man. Thus, Aristotle proclaims that in some cases an event is explained when we know what end it serves.

1. Skinner makes the point this way: "A rat does not always respond to food placed before it, and a factor called its 'hunger' is invoked by way of explanation. The rat is said to eat only when it is hungry. It is because eating is not inevitable that we are led to hypothesize an internal state to which we may assign the variability. Where there is a no variability, no state is needed. Since the rat usually responds to a shock to its foot by flexing its leg, no 'flexing drive' comparable to hunger is felt to be required" (Skinner, 1938, p. 341).

2. The idea that man is personally, morally responsible for his own acts is also due to the early Church Fathers. The blamable part of the human personality was called the soul. The Greeks, on the assumption that man always sought to do good, attributed evil to human error or ignorance.
Another sort of cause is that on account of which a thing is done. For example, bodily health is a cause of walking exercise. Why does a man take exercise? We say it is in order to have good health; in this way we mean to specify the cause of walking.\(^3\)

10 This earlier teleological concept, the idea of final cause as something apart from intention, was most suited to processes of growth, development, and fulfillment. For example, a block of stone becomes a statue with the sculptor as a causal agent or a child grows up and becomes a man. The stone and the child represent the unfulfilled but potential matter to which the statue and the man give form and fulfillment. They in turn are the final causes of their respective developments.

11 Whether we speak of purpose in the sense of final cause or purpose in the sense of intentions, both usages have a common element; some events are explained for some people when a justification for them is found. Justification is one variety of explanation. Some, like Aristotle, insist that there are certain events in the world that can be given meaning only in terms of the reasons for which they occur. But the meaning that is found in these instances is invariably evaluative; it is justification. Consider as an example the ancient argument that the appearance of purpose throughout nature is proof of the existence of a Creator. Aside from the difficulty that the argument presupposes what it purports to prove (i.e., the existence of purpose), it has the additional difficulty that after invoking a Creator such an explanation, or justification, stops with its account of nature as though nothing more were of interest. Invoking a Creator perhaps justifies creation but tells us nothing about creation; it has only restated the problem.\(^4\)

12 So, too, when we demand of a person the reason for his actions, his statement that he behaved as he did for such and such a reason only restates the problem; at best, we still have to explain why he had the reason he had. In the meantime, however, we may attach blame or praise to his purposes, and, indeed, it is probably just the evaluative freedom we have with another person’s motives that gives us such a sense that his purposes are important. If we were to give up the notion of purpose as the cause of action we would lose one of the principal objects of our own affections and aggressions.

**Untestability**

13 The most serious limitation of traditional rationalism is not that its explanations are teleological, but that they are inherently untestable. The events that are presumed to explain behavior are supposed to occur in the mind and be available only to the individual himself. Others have no ready or certain access to the hypothesized events. Moreover, there are no explicit hypotheses about how the mind is supposed to work so as to produce behavior; the relationships between these inaccessible mental events and observable behavior are so ill-defined and elusive that we cannot lay down any rules to indicate how the mind itself works. How does an intention to act (granting that an intention to act can produce the action) itself arise from the individual’s perceptions, knowledge, feelings, and so on? There are no rules to guide us.

14 We should note that it is not just the case that such rules have not been found; typically, the proponents of traditional rationalism insist that there are no such rules. They say that the mind of man cannot be bound by lawfulness; it operates creatively and dynamically rather than according to fixed, predictable principles. Plato said that the psyche is that which moves itself; it has laws of its own being and needs no others.

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3. Aristotle’s views on causation are given in Physics, Book 2, ch. 3, and de Artima, Book 2, ch. 4. He believed a phenomenon was not fully explained until its purpose, its final form, and its physical causes had all been accounted for.

4. The point was made by Hume (1779) when he said that the argument (for the existence of God) from design was invalid because it involves the assumption that the existence of a creator was a sufficient cause of creation, whereas at most the existence of a creator is only necessary. The argument itself is that the world presents endless indications of means adapting themselves to ends, which could only have occurred as the deliberate action of a powerful intellect.
Similar statements about the inherent unpredictability, the untrammeled freedom of the human mind have come rattling down the ages. These assertions are, in fact, a crucial part of the traditional rationalistic doctrine and it is for this reason, as we will see later in this chapter, that traditional rationalism does not constitute a “theory” of motivation; nor does it provide an explanation of behavior in any real sense. That is, it is not a coherent, consistent, testable set of propositions about behavior; indeed, it is in large measure a denial that such a set of propositions can be found.

**Mechanism**

15 In his restless quest for understanding and certainty man has sought to find the causes of all natural events, including, sometimes, human behavior. One of the oldest and most time-honored alternatives to the traditional rationalistic approach to explanation is mechanism or physicalism. To summarize it briefly, this is the doctrine that all natural events have physical causes, and that if we knew enough about physical and mechanical systems we would then be able to explain, at least in principle, all natural phenomena. The mechanist has the faith that when all the mechanical factors have been accounted for there will be nothing else left to explain.

16 This faith is supported in part by the predictability of physical objects in everyday life. We throw rocks and we observe that they behave in a reasonably predictable manner. If we make our observations on a billiard table we find that the predictability of the balls appears to be limited only by our skill in applying energy to them. The mechanist starts on the basis of a number of such observations and proceeds by analogy to the hypothesis that all events in nature have a similar machine-like predictability.

17 The doctrine of mechanism is based upon several distinct precepts, and it is important not to confuse them. Mechanism views all the phenomena of nature in the same light; while the rationalist makes a special case of man's ability to reason, the mechanist is concerned with finding principles that will include the behavior of man among the other phenomena of nature. Man's ability to reason provides no grounds for introducing exceptions to the laws of nature. His intellectual activity must be derived somehow from other, simpler principles.

18 The mechanist is also a determinist. While the rationalist assumes free will, the mechanist assumes that there are systematic laws of behavior that can be discovered. He assumes that if these laws were known they would permit behavior to be predicted. He may or may not involve the mind of man in his explanatory schemes, but if the mind is included, then it too must follow determinate laws.

19 The third distinguishing characteristic of the mechanist is his assumption that the world of physical events not only provides the pattern of what is natural and what is lawfully determined in nature but also provides the substance for all phenomena. Thus, he is a materialist. Behavior is not only a natural phenomenon, and lawfully determined, but is determined by precisely the same physical laws and forces that apply throughout nature. The ultimate and only reality, it is assumed, is physical in character.

20 There were Greek mechanists, but they were a minority group, and their influence was small compared with that of either Plato or Aristotle. Any substantial gains for the mechanistic position had to await its development and success in the physical realm itself; this occurred only in the seventeenth century through the work of such giants as Galileo and Newton. Wider scientific acceptance of mechanism had to wait still longer until it had been applied to the phenomena of the biological sciences.

21 Faith in the mechanistic doctrine has usually extended far beyond its usefulness in explaining the phenomena to which it has been applied. For example, most and perhaps all of our motivational concepts, such as drive and incentive, were developed and popularized during the interval between the introduction of mechanistic assumptions into psychology and the time when these concepts were put to empirical test. As a consequence, our theorizing made considerable use of drives and incentives and their postulated properties long before the usefulness of these concepts had been demonstrated by their ability to
explain behavior. Indeed, virtually the entire history of motivation theory is devoted to declarations by this or that theorist that we must find the forces underlying behavior and the physiological causes of behavior if we are ever to explain it. The urgency with which this program has been proclaimed has, unfortunately, not always been matched by the development of what we now consider to be the proper fruits of science, namely, adequate explanatory theories. We will consider shortly what is meant by an adequate explanatory theory, but first we must note some objections that have been raised to the mechanistic doctrine.

Empirical Determinism

22 The traditional difference of opinion regarding what constitutes an adequate explanation of any natural phenomenon has centered about purpose and teleology. The scientist has always been reluctant to admit that there are purposes operating in nature, preferring to rely upon what he views as the "real" or physical causes of things. On the other hand, rationalists, humanists, the clergy, and most thoughtful laymen have felt most at ease with, or even insisted upon, teleological accounts of certain natural phenomena, such as human behavior. The question at issue has traditionally been whether physical causes provide a total explanation, or whether teleological principles had to be added for some phenomena. Before David Hume (1739) did so, no one seemed to question whether physical causes were necessary, but only whether they were sufficient. Hume asked, How can we know the nature of causation? How can we know if causes really produce their effects? Hume's skeptical epistemology led him to the realization that the best evidence we can ever obtain is that two events invariably occur together, one preceding the other, always in the same order, and neither occurring alone. The imputation of causation, the abstract conception that the prior event necessitates the subsequent event, is an inference which goes beyond the evidence. There may be such a thing as physical or material causation, of course, but we can never be sure whether nature operates mechanistically since all we can know is the successive experience of successive events. It is in the nature of the human mind, Hume asserted, to transcend the data and infer a causal relationship between the two events if we invariably experience them one before the other.

23 Although scientists have characteristically operated in an empirical and pragmatic manner, philosophers, even philosophers of science, have tended to lag conceptually behind. Hence, for many years Hume's point was regarded as undue skepticism or as mere sophistry. Most men felt bound to commit themselves metaphysically either to a rationalistic position, to a mechanistic position, or to some dualistic combination of the two.

24 As far as science is concerned, its object is not to discover the ultimate nature of reality, but rather to explore empirical relationships and derive useful generalizations from them. The question of what sort of causation is involved in explanation is an unnecessary impediment, a philosophical encumbrance, to the conduct of science. It is futile for the scientist to be concerned with whether an event occurred because some other event compelled it to occur; much more to the point is that an event occurs and its occurrence can be correlated with certain sets of conditions. Of course we wish to refine our observations and improve our ability to control conditions until a point is reached where perfect or near-perfect correlations are possible and where very powerful general descriptive laws can be found. But science does not wait for the final solution of the causation problem. We must proceed to view empirical correlations as the subject matter of science without committing ourselves to either a teleological, purposive, or materialistic

5. Recent scholars have sought to analyze more carefully the meaning of terms like "cause," "effect," and "necessitate" (Bunge, 1959; Smith, 1960). Usually, the conditions of temporal and spacial contiguity between cause and effect call for their most serious consideration. These considerations are serious, weighty, and highly involved logically — particularly when contrasted with the elegance of the empirical laws to which they are purported to be relevant.
philosophy. Nor do we need to go as far as Hume and say that we can never transcend empirical correlations. We may believe that, or we may take the more optimistic position that the empirical correlations we observe will ultimately be undergirded by a more profound understanding of causation. By adhering to a descriptive or correlational approach we may at least leave the way open for such a possibility.

The empirical approach is noncommittal; it provides a convenient vantage point from which we may survey other, more highly committed approaches. Psychology, particularly the area of motivation, is confused enough by the practice of regarding motives, or drives, or instincts, or needs, as the causes of behavior. If we are to describe behavior from a point of view which does not restrict us to any particular theoretical or philosophical position, then it is necessary that we adopt a terminology which leaves these questions open. Thus the relationship which exists, for example, between a stimulus and a response will be described throughout this book, not in causal terms, but in neutral and descriptive terms. We will say that deprivation and stimulus conditions determine behavior, or that some behavior is under the control of some stimulus.

The crucial insight here is that the empirical attitude does not imply a rejection of the principle of determinism. Quite the contrary; it will be argued that behavior is determined, not because forces act on the organism to make it behave, nor because the behavior was willed by some reasoning intellect, but is determined simply in the sense that it is intrinsically predictable. Behavior is determined in that it is lawful.

The doctrine that I have called empirical determinism thus keeps the first two propositions of the mechanistic doctrine, namely, that behavior is a natural phenomenon and that it is determined, and rejects only its mechanistic or materialistic bias. It will be argued that a “causeless” account of a phenomenon can constitute an explanation of it even though it fails to provide any justification, or indicate its physical basis. It will also be argued that accordance with an empirical law constitutes just the kind of explanation we want, provided only that the empirical law is contained in a systematic theory. Before proceeding with this argument let us digress briefly to consider some objections that might be raised to this position.

Hempel and Oppenheim (1948) have discussed a number of such objections. They consider, for example, the argument that a strictly empirical explanatory system is not applicable to behavior in humans because of the enormous complexity of the human subject and the unique character of his behavior. Hempel and Oppenheim contend that the only real question here is whether phenomena as complex as human behavior are susceptible to adequate explanation. This is an empirical question; can laws of sufficient breadth and generality be discovered and can sufficient precision be obtained in specifying the appropriate antecedent conditions? The uniqueness and the irrepeatability of observations do not distinguish behavior from other observable phenomena. Irrepeatability is no less a problem in physics, or even astronomy, than it is in psychology; all observations are unique. The only strategy by which science can proceed at all is to concern itself with common features of and abstractions from unique observations.

Is such a system of explanation applicable to psychological phenomena in view of the fact that so many of the theoretical entities in psychology are not directly observable? The answer, again, is that psychological phenomena and theoretical constructs do not differ appreciably from those in physics or from those in any other science in this respect. So long as there are methods for determining with reasonable clarity and precision the hypothesized variables there is no special problem here at all. The only real question is whether psychological theories are to be based entirely upon empirical observations or whether they are to be based partly upon other, “transempirical,” sources of knowledge.

Another question is whether psychological explanations, which have historically involved reference to purposive behavior, call for a different mode of explanation. If “purposive” pertains to Divine purpose or to some inscrutable intention on the part of the individual, then, it is true, the approach fails to provide an adequate explanation. But if these kinds of purpose really have no empirical reference, then it is not clear that behavior which is purposive in this sense is susceptible to any sort of explanation. On the other hand,
if we mean by “purposive behavior” only that form of behavior which is highly correlated with its consequences, i.e., if we use the phrase in a purely descriptive or empirical manner, then there is no difference in principle between purposive behavior and any other kind of phenomena.

31 Perhaps the most fundamental practical objection that might be raised to empirical determinism is that it fails to tell why an event occurs; it only describes how and when events occur. When all is said and done a phenomenon is explained when it is put into terms with which we are familiar and shown to be an instance of a principle with which we are familiar. As Bridgman (1932) has said, an explanation is that kind of account that puts the curiosity at rest.6

32 We may ask what is the frame of mind of a man whose curiosity is only “put at rest” by an account of why things happen. What are such men really looking for? In the case of behavior, there seem to be two different kinds of accounts that men may be seeking when they ask “why?” One is justification, and the other is an application of the mechanistic doctrine. Thus, some of the time when someone is asked “why did you do that?”, what is expected is a justification of the action. At other times it seems clear that what is demanded is an understanding of the physiological or neurological machinery that produced the effect.

33 In general, a satisfactory answer to a “why” question is a statement involving terms with which the inquirer is familiar. The difficulty of explanation in psychology is that those who ask the psychologist “why” come to him quite familiar with justifying action and quite familiar with the reality of the physical body and seek some explanation in these terms. On the other hand, the scientist, who is familiar with the empirical regularities in his science, does not seek the why of them. Insofar as the psychologist asks why, it is because he is curious about moral questions of justification or about the mysteries of neurology, either of which he may have legitimate reasons for wanting to relate to behavior. But the psychologist asks why only when he wants to transcend or extend the boundaries of his science and not when he is working within them.

The Evaluation Of Theories

34 It was noted above that if an empirical law is to provide an explanation of a phenomenon it must be part of a systematic theory. All this means in effect is that explanation in science requires that a phenomenon be systematically related to other phenomena. When we have a model for producing such a systemization, then we have a theory. Since there is ample opportunity in current usage for confusion about what does and what does not constitute a theory, let us consider in some detail just what is involved.

35 From a logical point of view, a theory involves at least (1) a number of terms, and (2) a number of relational rules tying the terms together or interrelating them in some way. In purely formal systems, such as the different branches of mathematics, the terms are undefined. However, if a theory is to have empirical usefulness and testability it must have a third property: its terms have to be defined or related in some way to empirical events. There are a few examples of theories in psychology in which the primary emphasis is placed upon formal structure and in which little interest is attached to the problem of relating to empirical events. One such example is Lewin’s topological psychology (1936). Lewin went to considerable effort to develop a formal (or what might be called a preexperimental) structure which would be able to encompass the full richness and complexity of human behavior as the facts became known. The terms that constitute the formal language of the theory, i.e., terms like “valence,” “force,” and “tension,” are explicitly related to each other, but are only poorly tied to observable events in the empirical world. Lewin’s theory

6. “. . . an explanation consists in reducing a situation to elements with which we are so familiar that we accept them as a matter of course, so that our curiosity rests” (Bridgman, 1932, p. 37).
demonstrates a remarkable degree of sophistication, complexity, and internal coherence quite apart from the empirical question of whether people actually behave in the way the system prescribes. One could entertain oneself at length with the purely formal properties of the theory without applying it to the explanation of behavior.

A second example of a theory which provides a certain measure of formal adequacy we might cite Hull's hypothetico-deductive theory of rote learning (Hull et al., 1940). This theory was admittedly just a model to describe the "behavior" of an idealized "subject" learning a list of nonsense syllables, and again, it would be of considerable formal interest even if it should turn out that no actual subject ever demonstrated such behavior. What makes these semiformal systems of interest, apart from their empirical possibilities, is that they contain sets of relational rules which together give structure to the theory. Such a structure is called the syntax of a theory. The syntactical rules indicate how the terms of the theory fit together to provide an explanatory network; they describe how to operate with the theoretical terms in order to relate them to each other and to the data.

The syntax of a theory may be precise and formally rigid, as in Hull's hypothetico-deductive theory where mathematical relationships were formulated between all of the terms; or it may be left loose and qualitative; and terms may be left undefined, as in mathematical systems. All gradations occur. But since it is by relating the terms of the theory to each other (and to the data) that a theory is used, a theory without adequate syntactical rules is no theory at all. For example, the traditional view that man's behavior is explained by his rationality lacks any syntax because it assumes that man's behavior cannot be described by any set of rules. And this, the most serious limitation of traditional rationalism, is why this doctrine cannot properly be called a theory of behavior. One property we may require of a theory is that it should have an explicit syntax.

**Empirical Base of a Theory**

Philosophers of science and model builders may be chiefly interested in the structure of a theory considered just as a formal system. But to the scientist a much more interesting and important matter is the tying down of the logical or formal structure of the theory to empirical data so that the theory may be tested. This tying-down process is often called "anchoring." It is a different kind of process from the construction of the formal structure of the theory, and in many instances occurs quite independently of it. The branch of logic which deals with such definitions is called semantics. One difficulty in semantics is that a theory will ordinarily possess two kinds of terms: those that are theoretical, or not directly observable, and those that are empirical, or more or less directly observable. The theoretical terms of a theory (which I will call its theoretical constructs) are known by the fact that they are interrelated syntactically; they constitute the terms in the formal theory. But they must also be given empirical reference by being tied semantically to empirical terms (which, for reasons given below, I will call the empirical constructs of the theory).

From the point of view of the formal purist, some of the most popular theories have unfortunately been developed by theorists who have not paid enough attention to the empirical roots of theory making. For example, in psychoanalysis the semantic linkages of the theoretical constructs to empirical observations are so weak that it sometimes appears as though only the theorist himself could possibly know what he was theorizing about. Psychoanalysts seem at times to make a deliberate effort to create a mystique: the empirical anchoring of the terms of the theory is supposed to be left intuitive and loose. Freud believed that the rigid definition of theoretical terms should be the end rather than the means of theoretical advancement.

The view is often defended that sciences should be built up on clear and sharply defined basal concepts. In actual fact no science, not even the most exact, begins with such definitions. The true beginning of scientific activity consists rather in describing phenomena and then in pro-
ceeding to group, classify and correlate them. Even at the stage of description it is not possible to avoid applying certain abstract ideas to the material in hand, ideas derived from various sources and certainly not the fruit of new experience only. Still more indispensable are such ideas which will later become the basal concepts of the science as the material is further elaborated. They must at first necessarily possess some measure of uncertainty; there can be no question of any clear delimitation of their content. So long as they remain in this condition, we come to an understanding about their meaning by repeated references to the material of observation, from which we seem to have deduced our abstract ideas, but which is in point of fact subject to them... It is only after more searching investigation of the field in question that we are able to formulate with increased clarity the scientific concepts underlying it, and progressively so to modify these concepts that they become widely applicable and at the same time consistent logically. Then, indeed, it may be time to immure them in definitions. (Freud, 1915, pp. 60-61)

40 Although some might despair at such laxity, Freud always put difficult problems into historical perspective, and here he has indicated quite realistically how a scientific theory develops. We find that the logical analysis of the formal structure of a theory into its syntax and semantics does not tell us all about it, and in fact, such an analysis may have little historical or practical validity.

41 Still, the most useful theories of behavior are those in which theoretical constructs are coordinated at least tentatively with behavioral data. A theory must contain hypothetical relationships that tie together the empirical and theoretical terms. A second property we may require of a theory is that it have an explicit semantics.

**Data Language**

42 It should be noted that the specification of the empirical terms of a theory, the selection of facts which it is to explain, is not entirely arbitrary but is dependent also upon usage and habits of observation. “Response” is usually considered an empirical term, but it is not purely empirical. Its meaning is determined as much by the theory in which it is designated an “empirical observation” as by what happens in the real world. Thus, it is only relatively empirical. It is empirical by comparison with the theoretical constructs, but it is theoretical compared with the more basic terms of everyday language. To emphasize the quasi-empirical-quasi-theoretical nature of such terms we will call them empirical constructs. The empirical constructs, taken together, constitute what we will call the data language.

43 Data language serves as a foundation for a theory by tying it down empirically; it is also the language scientists use to talk among themselves. As Estes has put it, the data language... includes the terminology needed for the description of observations and operations. In psychology the chief function of the data language is the description of behaviors and of the situations in which they occur. The terms used in description must be limited to those for which agreement upon usage can be obtained from workers in the field regardless of theoretical biases and which are free of any reference to theory (that is, to the theory for which the set of terms in question functions as data language). (Estes, 1954, p. 321)7

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7. I have taken much of the language Estes uses in the logical analysis of theories for the present discussion. One point Estes makes I cannot agree to, however, and that is that a data language is theoretically neutral. It seems to me that much of a data language is determined in the same way as the other contents of a theory, namely by the constraints of our own experience, and by our previously established habits of speculation and observation. In the storybooks, the scientist is able to study a perfectly arbitrary selection from among possible phenomena, but in fact, this selection is determined in large part by what he believes to underlie what he observes. Stevens put it: “There are only constructs... A datum is a construct” (1935, p. 523). Margenau (1950) has examined thoroughly the implications of this position for physical theory.
44 Some writers define the data language of their theory explicitly (e.g., Skinner) but more often they leave it unanalyzed, implicit (e.g., Lewin, or the Gestalt psychologists).

45 One of the difficulties in assessing different theories of behavior is that the different theories have tended to isolate themselves by the use of data languages which are unintelligible to theorists of other persuasions. Thus, it is only with considerable effort that a S-R-learning theorist and a psychoanalyst can communicate with each other. The difficulty is that what one accepts as an empirical observation the other may not. Each has no difficulty in a linguistic community which accepts the same data language, but the mixture of data languages from two independent sets of workers can be disastrous. Some of the great conflicts in the history of theoretical psychology can be traced to the problem of mutually unintelligible data languages. For example, the continuing lack of harmony between molar and molecular points of view may be attributed to the use of the word “response” for very different kinds of events for which very different kinds of psychological theories seem appropriate.

46 Consequently, we must consider that the semantic problems in a theory of behavior extend not only from the theoretical constructs down to the empirical constructs of the data language, but from there on down to facts of experience upon which anyone speaking the language can agree — what Carnap (1936) has called the physical-thing language. Ideally, the data language would be linked to the thing language, the language of common perceptual experience, through operational definitions. As an alternative to such rigid linkages, the theorist may use the technique commonly employed in the physical sciences, sets of reductions. That is, he might identify a term by means of several functional properties.

47 An important question is whether the ultimate language to which the data language is reduced must be the physical-thing language. This question lies at the very heart of the traditional difficulty of studying complex human behavior. In the rationalistic variety of explanation, the language of human experience is asserted to be the only one that is valid for the study of man. To be sure, there may be a perfectly good language of human experience which is more or less independent of the language of physical things, but this language has shown itself to be of relatively little use for the purpose of developing a theory of human behavior. There is so much trouble communicating with such languages that we cannot test assertions stated in them. And perhaps above all else we require that a theory of behavior be testable. Therefore we may require of a theory that it have a precise and explicit data language, precise enough and explicit enough that it is possible to test assertions derived from the theory.

48 Figure 1-1 depicts some of the formal properties of theories we have been considering.

The Constructs Of A Theory

49 Viewed solely as part of a conceptual model, the set of theoretical constructs in a theory are all of the same sort, viz., symbols to be put down on paper. But imbued with the purposes and presuppositions (and biases) of the theorist, the theory takes on new dimensions, and all its constituent constructs acquire conceptual properties of their own. They differ in temporal duration; some are enduring, like “dispositions,” others are temporary state variables, like “drive,” while still others are fleeting determinants of behavior like “percepts.” They differ also in temporal order; the consequences of stimulation precede the final determinants of the response. Constructs also differ with respect to their causal relationships; stimuli are generally assumed to cause the responses they control.
Empirical Reference of Constructs

50 Perhaps the most striking way in which constructs differ is with respect to their hypothetical or actual empirical reference. Spence (1944) has suggested a four-fold classification of the referents commonly used in psychological theories: animistic, neurophysiological, response-inferred, and intervening. With constructs of the animistic variety there is no anchoring to empirical observables. Presumably everyone is just supposed to know the signs of these intuitive psychological variables. The reason these sorts of constructs are called animistic is clear: they operate on the individual as though they had reason and will of their own, and these cognitive and volitional powers are not subject to further analysis.

Physiological Reference

51 The neurophysiological type of construct is another favorite; its empirical reference is most frequently to a hypothetical neurological mechanism which is not now directly observable, but which, at least in principle, could be observed. One example is the concept of decreased synaptic resistance as a basis of learning. As a mechanism it sounds plausible, but as an empirical fact it has the status of a “promissory note,” that is, we invoke it hoping that some day the physiologist will confirm its existence for us.

52 It is not clear that either the physiologist or the psychologist has gained much by the one telling the other what he should look for. It is also not certain that the surplus meaning that the psychologist attaches to his
constructs by means of these promissory notes makes them any more valuable. Certainly they become no more useful for describing the facts of behavior; their only possible utility would seem to be to establish a true bridge science of physiological psychology. But their major application is not ordinarily as constructs in new physiological theories, but as ancillary elements in behavioral theories. In this application they would seem to be something like status symbols.

53 The theorist may be simply indicating his faith in a mechanistic variety of explanation when he posits neurophysiological constructs; he may simply be showing that he believes that ultimately the facts and laws of psychology will be reducible to the facts and laws of physiology. But if the theorist would validate a theoretical construct in terms of, say, neural synapses, then neural synapses must be included in the data language of his theory, and he would then have a theory about neural synapses rather than one invoking them. He still would not have validated the original behavioral construct; that can only be done empirically. If the theorist attempts a reductionistic coup by translating all his data language into the language of physiology, then he is thrown back by the fact that physiology lies no closer to “reality” but is itself only an organization of empirical correlations. The reductionistic psychologist might just as well have stayed where he started. Translation of the stimulus into its neurological correlate and the response into its neurological correlate has brought him no nearer to demonstrating that the stimulus really causes the response. After the translation, the psychological theorist (who has now become a physiological theorist) may observe a correlation between this neural activity and that, but he is no closer to having shown that the one causes the other.

Mentalistic Reference

54 In an earlier day it was more frequent for the psychologist to seek validation of his constructs by shifting to quite another level, that of “human experience.” Again, this sort of validation of a theoretical construct is inadmissible. If human experience can be included in the data language, then so much the better, but it cannot be invoked to lend ancillary validity to a construct that is otherwise anchored to behavioral phenomena. Because its reference is outside the theory, introducing it does nothing to bind together the structure of the theory. Its surplus meaning must always remain surplus.

Response-Inferred Constructs

55 The other two categories of theoretical constructs which Spence lists, response-inferred and intervening, are the bread and butter of psychological theorizing. Response-inferred constructs are those that are linked by coordinating definitions to just one kind of empirical construct—a some single response phenomenon. Now, there is a hazard in relying upon a single semantic linkage. It means that, in one sense, the construct cannot be used to explain the phenomena with which it is linked because such an explanation would be circular. Theorists are rarely so naive, however, and certainly they rarely work with such uninteresting constructs, and perhaps he has in mind some more or less implicit secondary linkages which give further conceptual properties to his constructs. Or the theorist may simply not feel ready to incorporate them into the definition (the primary linkages) of the construct, preferring to leave them tentative. The onus of circularity that is often put on response-inferred constructs is really only justifiable in those cases where the theoretical construct is used to explain exactly the same features of behavior from which its existence is inferred. To the extent that a variable is used across different situations or different subjects or different behavioral events, it acquires some practical sort of validity.

Intervening Constructs

56 The intervening-variable type of constructs are those which are embedded in a network involving several empirical constructs, and in particular, they intervene between the stimulus and the response. If they are given semantic and/or syntactical linkages to both the stimulus and the response, then they are said to be
anchored on both the antecedent and the consequent side. Spence has tried to make a sharp distinction between the intervening-variable and the response-inferred type of construct, and he has made clear his preference for the intervening variety. However, I doubt that the distinction can be maintained as sharply as Spence would like. It frequently turns out that the stimulus is not empirically independent of the response; we sometimes don’t know what the effective stimulus is until the responses of the subject are analyzed. Consequently, an intervening construct variable is often logically reducible to one of the response-inferred variety.

But the point of Spence’s argument remains and it is a critically important one: our theoretical constructs have to be tied by two or more semantic relationships (possibly via some further syntactical linkages) to two or more empirical constructs. The whole set of theoretical and empirical constructs then may be visualized as a sort of network. Sometimes it is called a nomological net. A theory in which there is a simple one-to-one correspondence between empirical and theoretical constructs is hardly better than a theory with none. Such a theory would consist merely of a restatement of empirical relationships that had been obtained, and would contribute no economy, power, or possibility of further insight to a purely empirical description.

**Radical Empiricism**

One way to adhere to the formal criteria of science is to exclude all theoretical constructs from consideration, i.e., to use no terms that cannot be immediately reduced operationally to the physical-thing language. This means a dedication to finding lawfulness among empirical constructs. This approach may be called radical empiricism. The scientist who adheres to the program of radical empiricism gains some safety from it since he never has to revise his theoretical constructs or his syntactical structure — because he doesn’t have any. But there is relatively little else to recommend this approach.

Of course, those who speak loudest against theories may only be voicing their distaste for the animistic and neurophysiological sorts of constructs that so frequently occur in them. Or the advocates of atheism may be against the sort of causation that tends to be implied in many psychological theories. But one does not have to abandon theory altogether to overcome the limitations of animistic and neurological constructs. In fact, the formal and empirical structure of most of our theories of behavior may be quite easily divorced from the surplus meaning that is characteristically associated with them. It is certainly possible to have a theory of behavior involving terms with no surplus meaning, terms which do not commit the theorist to a particular variety of explanation, or betray his personal belief in one.

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8. The designation “intervening variable” was coined by Tolman (1936). Skinner has pointed out, however, that this designation is not a particularly good one unless what is meant is some sort of transempirical, i.e., mentalistic or physicalistic, intervention between stimulus and response (Skinner, 1959, p. 320). Tolman probably had no such intention.

9. I am referring here particularly to Skinner’s disavowal of theory (1950). He says that he would have theory excluded from a science of behavior if that word refers to “any explanation of an observed fact which appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions” (p. 193). Skinner clarifies further the object of his dissatisfaction: theories incorporating real and conceptual mental and neural terms. “When we attribute behavior to a neural or mental event, real or conceptual, we are likely to forget that we still have the task of accounting for the neural or mental event. When we assert that an animal acts in a given way because it expects to receive food, then what began as the task of accounting for learned behavior becomes the task of accounting for expectancy. The problem is at least equally complex and probably more difficult. We are likely to close our eyes to it and to use the theory to give us answers in place of the answers we might find through further study” (p. 194).
Radical empiricism is too big a price to pay. All that it buys can be purchased more cheaply by demanding that constructs be validated solely by being tied syntactically and semantically to the data language. This demand appears simple and straightforward, but it is contrary to the common practice of attempting to validate a construct or give it greater reality status by transcending the data or by going to another level of discourse. What makes one construct “better” than another is not that it is more real, but that it is tied in more different ways to empirical constructs, that it has what Koch (1941, p. 30) has called a “plurality of observable symptoms.”

It is not the theorist’s inclusion of a neurological or mentalistic construct which is wrong but his insistence that a construct can be given status within a theory by means outside the theory itself. He is subscribing to the belief in some transsemiprical validation. He believes that he has somehow really gotten hold of the causal agencies when he refers to a synapse, or a S-R connection, or a drive, or a cell assembly, or an expectancy. But at our present stage of understanding we can know no more than that our observations correlate. The fourth and final thing we should require of a theory, then, is that its theoretical terms be validated solely by their empirical usefulness. We will give no weight to any sort of claimed transsemiprical validation.

Compromise Character of a Theory

We have come to a point where logical analysis fails us. We want to require of a theory that its terms be anchored to empirical observations as definitively as possible, but at the same time we must recognize that these linkages are always tentative; theories are always in a process of formulation, or should be. The skill of a theorist is shown by his ability to alter the semantic structure of his theory as new evidence becomes available. And at the same time, the structure of the theory dictates to some extent what observations will get made and even how they will be made and recorded. So a theory must specify its constructs with some precision, but it must also allow for further conceptual reorganizations. We want to require of a theory that it provide some measure of economy over a raw description of certain phenomena, but it must not be too economical; it must provide for some syntactical complexity if it is to prove useful. We want to require that a theorist speak out as plainly and explicitly as possible, but we have to recognize that the formal statement of a theory must always remain somewhat behind the insight of the theorist.

So our portrait of the useful theory begins to take shape. A good theory is a tentative compromise between economy and complexity, explanatory power and mere descriptive tautology, precision and richness. We require that a theory meet certain standards of formal adequacy: its syntax, semantics, and data language must be set forth with some precision and explicitness. But we may wish to relax these formal standards if

Undeniably there is a correlation between what variety of explanation a theorist favors and the sort of constructs he puts into his theory. But there is no necessary connection between the two. Thus, animistic constructs tend to be teleological, but they are not necessarily so. Neurophysiological constructs may indicate the theorist’s faith in material and efficient causation, but they may have reference only to the empirical regularities in physiology. The response-inferred and intervening types of constructs tend more than the others to be of a purely empirical or mathematical nature, but they too may be based upon other varieties of explanation. Thus, Hull generally believed that the constructs in his theory had physiological reality, and Tolman believed that behavior was intrinsically teleological. A response-inferred or intervening type of construct may be proposed by a theorist with an acausal view of nature, or one who, for some other reason, prefers a purely empirical approach.

The plurality of empirical symptoms is a measure of the fruitfulness of the construct, as opposed to the commonness of the symptoms which is a measure of its generality. Another way to state the importance of a plurality of empirical symptoms is to say that a construct should be “overdetermined” by its empirical coordinations.
doing so can lead to further theoretical gains in the long run. We can expect an occasional departure from these standards, particularly with the more interesting and important theories.