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CHAPTER 5

Theorizing

Behaviorist theorizing occurs in many forms. At the two extremes are the hypothetico-deductivism of Hull and the descriptivism of Skinner.

The status of Hullian theoretical concepts is somewhat ambiguous. Some are clearly hypothetical constructs. Others appear to be intervening variables but operate as hypothetical constructs.

Hull's hypothetico-deductive method is not like deductive systems in logic and mathematics. Instead, his postulates define concepts as hypothesized quantitative functions of empirically defined variables, and they state hypotheses as to how these concepts combine to determine behavioral variables.

The purpose of a theory, according to Hull, is to mediate the deduction of theorems about observable behavior. Theorems are experimentally tested and either add confirmation to a theory or force its revision. Theory develops through this process of deduction, test, and revision. Explanation is achieved when the description of a phenomenon is deduced as a theorem from a theory.

Skinner's theoretical concepts consist of intervening variables, such as drive, and private events, consisting of covert stimuli and responses. Although the status of the latter is not clear, they are best viewed as hypothetical constructs.

Skinner objects to theories on the grounds that they inhibit fruitful research. Instead, he advocates a descriptivism with an emphasis on discovering orderliness in behavioral data. Theory, for Skinner, consists of economical descriptions of functional relations which subsume a number of behavioral regularities.

Both Hullian and Skinnerian meta-theory are open to criticisms from a nonlinear model of the development of science. This model questions behaviorist assumptions about the continuity of theory development and the relationship of theory to data.

Despite behaviorist attempts to link concepts to the behavioral data language, the relationship of concepts to observation is underdetermined. This linkage is ultimately psychological. Within this context, behaviorist meta-theory can be viewed as a decision to specify concepts precisely enough for scientists to learn to achieve intersubjective agreement, prediction, and control while using them.

Observed regularities in behavior eventually give rise to empirical generalizations, which, in turn, lead to the formulation of scientific laws. Although these generalizations and laws are helpful in achieving the behaviorist goals of prediction and control of behavior, these twin goals are reached even more effectively through the creation of a theoretical system. Such a system mediates prediction and control of a variety of behaviors in a wide variety of situations, and it integrates a broad range of seemingly unrelated phenomena and laws under relatively few basic principles. Thus, a comprehensive and integrative theory uncovers coherence within the domain of the behavioral science and thereby facilitates explanation and understanding.

Behaviorists create theories in a number of ways, each of which represents a branch from the next node considered within the behaviorist conceptual tree diagram. Two of the most influential methods of theorizing are those of Hull and Skinner. The theories of these two are illustrative, not only because their methods are widely followed, but also because they represent two extremes within behaviorism. Between the hypothetico-deductivism of Hull and the descriptivism of Skinner lies a range in which the theories of a large number of behaviorists fall. Because they sit at the extremes, the meta-theories of Hull and Skinner provide relatively clear case studies for the conceptual issues to be examined in this chapter.

HULLIAN THEORIZING

Hull's Constructs

Hullian constructs pose special problems for interpretation. In certain cases, including the anticipatory goal reaction, r_g, it is clear that a hypothetical construct is intended.[1] However, for many of his major constructs, including habit strength, ${}_sH_R$, and drive, D, the logical status is not as obvious. On the one hand, he explicitly claims that all his constructs are intervening variables as conceptualized by Tolman.[2] On the other hand, when he discusses these constructs, they do not sound like mere convenient groupings of mathematical terms with no independent existences.[3] For example in discussing ${}_sH_R$ he states:

> It is important to note that habit strength cannot be determined by direct observation, since it exists as an organization as yet largely unknown, hidden within the complex structure of the nervous system. This means [it] can be determined, i.e., can be observed and measured, only indirectly. (1943b, p. 102)

Some of Hull's expositors claim that these statements are merely supplementary physiological speculations, heuristically helpful in the process of theorizing, but not part of the theory *per se*.[4] Indeed, at one point Hull says of a statement of this sort that it is a "kind of background" and "not properly a part of the present system."[5] According to this interpretation, the real theory and the meaning of its theoretical terms consists solely of the quantitative statements supplied by the postulates. Other commentators, on the other hand, maintain that all statements about a theoretical construct contribute to its meaning and must be included in the theory.[6]

Although there is no clear-cut way to determine when a statement is part of the theory and when it is merely supplemental background, one possible criterion is its role in mediating deductions from the theory. Certainly if a statement is necessary for a deduction claimed by the theory, then it is part of the theory. With this criterion in mind, consider the construct S_D, the hypothesized drive stimulus, introduced by Postulate 6: "Associated with every drive (D) is a characteristic drive stimulus (S_D) whose intensity is an increasing montonic function of the drive in question" (Hull, 1943b, p. 253). Using the S_D concept, Hull derives predictions of performance at various levels of drive. Some of these predictions are mediated by the laws of stimulus generalization (Postulate 5). However, the laws of generalization do not apply to S_D unless it is attributed the physical properties of a stimulus.[7] Thus, S_D operates within the theory like a hypothetical construct rather than a purely conceptual intervening variable. It appears, therefore, that no general statement can be made concerning the logical status of Hullian constructs, and each one must be examined separately.[8]

Hull's Postulational Method

Ambiguities arise also in interpreting the logical structure of Hullian theory. On the one hand, Hull, in his early "miniature systems," announces that the systems of Newton and Euclid—with their definitions, axioms, postulates, and theorems—are the models for his own theorizing. His early theories, in fact, are formally presented according to that scheme.[9] It therefore appears that Hullian theory consists of an uninterpreted axiomatized calculus given empirical meaning only through coordination at certain points with experimental variables by a set of correspondence rules.[10] Since not every theoretical term is associated with a correspondence rule, some terms receive only implicit definition and therefore function as hypothetical constructs rather than intervening variables. This impression is further compounded by the fact that Hull divides his term into "defined" and "undefined" and includes his theoretical terms among the latter.[11]

On the other hand, a close examination of Hullian theory reveals that while the form of an axiomatized system is maintained, the theory does not operate as one. First, the postulates define theoretical concepts in terms of experimental variables, and the theory is therefore not an uninterpreted calculus in need of correspondence rules. For example, Postulate 3 of one miniature system (Hull, 1937) states:

> A characteristic stimulus-reaction . . . always marks reinforcing states of affairs. . . . The particular stimulus-response combination marking the reinforcing state of affairs in the case of specific drives is determined empirically. (p. 16)

and "reinforcing state of affairs" has already been defined in Definition 1.

Second, by "undefined term" Hull does not refer to terms given only implicit meaning by the theoretical context. He is simply noting the fact that since terms are defined with other terms, at some point definitions are either circular or some terms are simply left formally undefined. It is a matter of convention which terms are chosen to remain formally undefined and to be introduced either by ostension or through ordinary language with its pre-scientific meanings.[12] Hull occasionally leaves his theoretical terms as undefined because he finds it cumbersome to try to provide them with formal definitions. Otherwise the theoretical concepts are introduced by postulates which state equations defining the concept as a mathematical function of experimental variables.

Thus Hull's "hypothetical-deductive" technique cannot be understood in the same sense as it is used in logic and mathematics. His postulates seem to serve a different purpose. Hull begins with a small number of empirical findings—response rate as a function of number of reinforced trials, for example. Although number of reinforced trials is the only variable manipulated, it is obvious that the response rate is a function of a number of variables presumably kept constant (e.g., drive, delay of reinforcement) as well as some which are unknown. It is possible to speculate on how all these variables enter into the equation determining response rate. However, this "long equation" stating response rate as a function of every relevant variable would be unwieldy for suggesting further experimentation. Instead, Hull divides up the long equation into smaller groupings, each of which defines a new quantity such as habit strength and inhibition. Therefore, one role of the postulates is to define these new concepts as an hypothesized quantitative function of empirically defined variables. The second role is to hypothesize how these theoretical variables combine to determine the behavioral variables.[13]

The concept is thus anchored on both sides, and the postulate set therefore consists essentially of definitions and hypothesized empirical relationships. Why then call them "postulates"? Hull gives three reasons. He identifies "postulated" with "hypothesized" and argues that his statements are hypothesized in the sense that his guesses have not yet been proved valid. Second, he argues that since none of his postulates have been shown to be underivable from the others, it is only a supposition that each is a postulate rather than a theorem.[14] Third, he notes that since the postulates define each intervening variable in terms of only a limited subgrouping of all the variables responsible for behavior, predictions about behavior can be made only by combining the various postulates to deduce the behavioral outcome. Consequently, it is a theorem deduced from a set of postulates which is tested, and not any single postulate.[15] Therefore, a postulate is a "postulate" in the sense that it cannot be *directly* tested and so always remains hypothetical.

Role of Hullian Theory

For Hull and his followers the main purpose of a theory is the deduction of theorems about observables. These theorems can be experimentally tested, and if the results conform to the theorem, then the theory gains in its degree of confirmation and the confidence with which it is maintained. Lack of observed congruence with a theorem calls for revision of the theory.

By this process of test and revision, a theory is gradually developed that generates increasingly greater numbers of valid theorems. Therefore, a theory mediating a large number of testable theorems is preferable to one which mediates fewer, because the possibilities of test and revision are greater with the former.[16] Since theorems are statements about future behavior as well as assertions about how the environment determines behavior, they play a critical role in the prediction and control of behavior.

Theory furthermore mediates the explanation of behavior. When a behavior occurs, a description of it may be stated as a theorem. If this theorem is deducible from the theory, then the behavior is explained. This identification of explanation (and prediction) with logical deduction from a theory is congruent with the general behaviorist understanding of objectivity in science. If explanations or predictions are offered on the basis of intuition or empathy, then there is much room for disagreement and controversy. In contrast, logical deductions from clearly stated postulates are more certain to command universal assent.[17] Thus, objectivity in explanation and theory construction, just as with observation, is closely identified with intersubjective agreement among scientists.

Besides mediating prediction, control, and explanation, theories also organize and direct research. The initial postulates are framed so as to conform to the current data on an *ad hoc* basis. Once formulated, however, the system generates theorems about experiments not yet performed, thereby suggesting further research. Furthermore, by simplifying the "long equation," intervening variables help clarify important relationships which may then be experimentally investigated. Thus the Hullian postulates are a source of hypotheses to guide a research program.[18]

SKINNERIAN THEORIZING

Skinner's Constructs

The concepts within Skinner's theory fall into two categories. The first consists of a set of intervening variables, including "reflex reserve," "drive," and

"emotion." Each is anchored to both dependent and independent variables and summarizes the changes in the functional relationships between them produced by certain operations. For example, an operation like food deprivation will change the relationship between a schedule of food reinforcement and response rate. To summarize the correlated changes, the concept "drive" is introduced. However, because of the objections to intervening variables discussed in the preceding chapter, Skinner eventually eliminates nearly all of them from his theoretical system.[19]

The second category of Skinnerian constructs consists of "private events." These are covert stimuli and responses which play a fairly prominent role in Skinner's interpretation of behavior. They are attributed the properties of overt stimuli and response, and they are assumed to occur under the functional control of the external environment. Skinner adds that they are not involved in the causation of behavior but are instead "collateral byproducts" of the environmental variables responsible for both covert and overt events.[20] Strictly speaking, however, since in Skinner's interpretation covert events can function as discriminative stimuli, reinforcing stimuli, and punishing stimuli for overt behavior, they clearly do play a causal role.[21] Nevertheless, they do not play as important a role as has traditionally been thought or as might be supposed on the basis of introspection.

From these considerations, it seems that Skinner's private events qualify as hypothetical constructs which generally meet the behaviorist strictures described in chapter 4. Problems arise because an essential feature of a hypothetical construct is that it is inferred rather than observed. However, as discussed in chapter 2, in rejecting intersubjective agreement as a criterion for scientific observation, Skinner opens the way for the acceptance of private observation. This may mean that a verbal response ("tact") to a private stimulus can count as an observation. Therefore, private events may be said to be observed rather than inferred. In this case private events are not hypothetical constructs, and their descriptions are part of the behavioral data language.

On the other hand, Skinner often speaks of private events as inferred.[22] Furthermore, there are good reasons why private events ought to be considered inferred. First, if behaviorist psychology is to be the "psychology of the other one," in Meyer's felicitous phrase,[23] then even if the subject may be said to be observing the private event, the experimenter, representing the science, must be said to infer the private event.

Second, Skinner's statement that private events are the discriminative stimuli for certain verbal responses is, at present, no more than a hypothesis. No proof is currently available to show that any verbal responses enter into causal relationships with private events as required by the hypothesis, or that these private events are stimuli and responses conforming to the same laws as their overt counterparts. Therefore, private stimuli and responses are inferences. Even the subject's verbal responses provide no observational evidence for the hypothesis since they are in the form "I have a toothache" rather than "Stimulus X is occurring in me."[24] Furthermore, many covert events, such as the autoclitics Skinner hypothesizes to be involved in editing speech, are not observed even in the sense of acting as stimuli for verbal reports.[25] For these several reasons it seems appropriate to judge Skinner's private events as inferred and therefore as hypothetical constructs.

Skinner on Theory

Because Skinner objects to both intervening variables and hypothetical constructs, it is not surprising that he objects to theories containing theoretical concepts. Moreover, Skinner also claims that psychological theories in general are neither necessary nor desirable. By "theory" he means

> 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. (1950, p. 193)

This definition is intended to include physiological and mental constructs, as well as inferred explanatory events.

It is not clear whether Skinner's own hypotheses about private events escape this definition. To be sure, private events are described in the same terms as overt behavior and measured in the same dimensions.[26] But whether private events are "events taking place somewhere else, at some other level of observation" is not obvious. They are "somewhere else" in that they are on the other side of the skin from overt behavior, and they must be observed by means other than those used to observe overt behavior.

Nor is it obvious whether Skinner's definition of "theory" covers statements about intervening variables. A well formulated intervening variable is defined by a set of observable variables. It is not a distinct event located elsewhere but is rather a shorthand for observed relationships.[27] To be sure, it is sometimes measured in its own dimensions, such as "habs" for the Hullian $_sH_R$, but these new dimensions are merely transformations of old units, just as "ergs" is merely a transformation of dynes and centimeters.

Whatever the range covered by Skinner's definition of "theory," he argues that theory has a detrimental influence on the development of psychology. He is particularly concerned with the methods, especially hypothetico-deductive techniques, prescribed for the scientist to follow in formulating theory. He admits that at certain points in the history of science and for certain subject matters, these methods may be appropriate, but not for psychology whose subject matter, behavior, shows lawfulness at the level of observables.[28] According to Skinner, scientists who are taught to use these methods exclusively are limited in their ability to do science. For them, theorizing is conceived as a mysterious cognitive activity, and excessive attention is paid to it

rather than to behavior.[29] Such scientists will be motivated by the attempt to test theorems of the theory rather than the search for lawfulness in behavior.[30] Their research will be useful only so long as the theory remains in vogue. Once the theory is rejected, the research becomes meaningless.[31]

Theory, he claims, also interferes with the search for lawfulness when it is used as a substitute for that search. When behavior appears to lack orderliness, theorists often invent theoretical processes postulated to show the orderliness apparently absent instead of determining and controlling the variables responsible for the behavior. Orderliness is thus relegated to the theoretical postulates rather than uncovered in the behavior.[32] These postulates are never directly tested as are the theorems deduced from a conjunction of postulates. Consequently, a postulate can never become a fact.[33] On the other hand, if the proper attention is paid to the variables controlling behavior and an appropriate behavioral unit is chosen, orderliness appears directly in the behavior and the postulated theoretical processes become superfluous.[34]

Evaluation of Skinner's Claims

Skinner's objections to the introduction of intervening variables and hypothetical constructs were discusssed in the preceding chapter. With regard to theory construction, Skinner's own research achievements and those of his followers demonstrate that a research program can proceed and even flourish with a minimum of theoretical concepts and in the absence of hypothetico-deductive methods. Theories, in Skinner's sense, are not *necessary*.

That they are also *undesirable* is more difficult to prove.[35] While it is true that Hull's system failed to achieve the theory he envisioned, its heuristic value continues as it and its lineal descendants continued to serve as a source of concepts and experimental hypotheses. Furthermore, it is not clear that the failure of the Hullian system can be attributed to the hypothetico-deductive method.[36] Perhaps the same method on a smaller scale and with more conservative inferences in formulating postulates might meet with more success. Finally, while it is true that some research generated by Hullian theory is no longer useful, much of it stands on its own, and has in some cases been incorporated into other systems, including those of Skinner's followers.[37] Assuming that Hullians used valid experimental procedures, their data are valid. Whether they are also relevant depends on the current standards of relevance. Not every instance of orderliness in behavior found by a Skinnerian will prove to be useful either.

The force of Skinner's second complaint, that postulates cannot become facts, is unclear for several reasons.[38] First, Hull freely admits that his postulates are not open to direct confirmation, but this is not a source of embarrassment for him. The purpose of the postulates is to mediate deductions, not to describe facts. Thus, Skinner's complaint is at base really a restatement of his objection to the hypothetico-deductive methodology. Furthermore, it is not even clear that Hull's postulates cannot be directly tested. Since Hull's postulates are a set of assumed empirical laws rather than an axiomatized uninterpreted calculus, there are ways to test them directly.[39]

Skinner is correct in his third charge that at times psychologists have substituted orderly postulates in place of the search for orderliness in behavior. However, this may show merely that any method may be abused rather than that abuse is an inevitable consequence of a particular method. The search for orderliness in behavior and the avoidance of explanatory fictions may also be abused, as when the results of an experiment are presented in the form of reams of uninterpreted cumulative records.[40]

Skinner's Descriptivism

In place of the kinds of theories he objects to, Skinner offers an alternative, the "experimental analysis of behavior."[41] In this approach, research is guided not by the attempt to test theorems but by the search for orderliness in behavior. This search is based on the scientist's intuition and hunches rather than on the rules of "scientific method." Experimental research is directed toward controlling the subject matter, thereby discovering and demonstrating lawfulness. This lawfulness is stated in empirical functional relationships between behavioral and environmental variables and expressed in concepts chosen for their usefulness in effectively capturing the orderliness in the data. Thus, concepts emerge from the experimental program rather than from *a priori* derivations.[42] Skinner's functional definitions of stimulus and response are examples of this method at work (see chapter 3).

Theory, in the acceptable sense, evolves in the attempt to present the collection of empirical fact in a formal and economical way. A formulation using a minimal number of terms to represent a large number of experimental facts is a theory.[43] As the theory develops, it integrates more facts in increasingly more economical formulations. Theoretical concepts thus merely collate observations and do not refer to nonbehavioral processes. A Skinnerian theory is, therefore, a simple, comprehensive, and abstract description of a corpus of data.[44]

A Skinnerian theory is to be evaluated by its effectiveness in enabling scientists to operate successfully on their subject matter. Since theories are selected by criteria of "effectiveness," "success," and "expediency" for the scientist, the development of theory is closely tied to the psychology of the scientist. For this reason, Skinner emphasizes the importance of a science of science, that is, an empirical study of the behavior of the scientist, to develop empirically founded canons of scientific methodology.[45] Presumably, the scientist is reinforced by the discovery of lawfulness as well as the prediction and control of behavior, so that theories which facilitate these achievements

will be chosen. In the long run, contingencies of human survival come to influence the behavior of the scientist, and these will come to exert some control over the development of scientific theory and practice.[46]

Although Skinnerians tend to emphasize their differences with the Hullian scientific method, there are many commonalities. These, as well as the experimental analysis of behavior, are illustrated in Skinner's classic treatment of schedules of reinforcement. Skinner shows that many of the effects of schedules of reinforcement can be understood as the result of conditions prevailing at the time of reinforcement.[47] Presumably, this formulation is an admissible theory in Skinner's sense in that it is an empirical statement which economically describes and integrates a wide array of behavioral findings. This theory explains various experimental results because the results can be deduced from the theory. In this sense, Skinnerian and Hullian explanations are alike in that both explain by theoretical deductions, although the forms of the two theories are, of course, quite different.[48]

Second, Skinner's theory suggests further experiments to test the theory. Many of Skinner's experiments consist of manipulations of the conditions prevailing at the time of reinforcement to see if the effects of the schedule change according to what is expected from the theory. Thus, for both Skinner and Hull, a theory provides suggestions for further experiment. For Skinner a suggestion is useful for gaining greater control over behavior, while for Hull its benefit is as a theorem to test a postulate.

A NONLINEAR MODEL

In both the Hullian and Skinnerian approaches, theory development is seen as a continuous linear process. For Hull, a theory develops as its theorems are experimentally tested. Experimental observations then suggest how the postulates are to be adjusted to accommodate the data. For Skinner, theory develops as the variables controlling behavior are isolated and experimentally explored. Observed functional relations are then systematized in an economical and efficient formulation that gradually comes to subsume an increasingly comprehensive body of data. Thus, in both views, theory development is an evolving process reflecting the interaction of theory and observation.

This linear model, however, is open to two objections.[49] First, this model assumes that observation is independent of theory. Observations are thought to constitute a separate body of facts which test or are subsumed by a theory. However, if observation is theory-dependent then the linear model is flawed. Theory dictates to some extent what the observations are.[50] Thus the congruence of fact and theory does not necessarily represent a gradual convergence of theory with the independent subject matter it is to describe and explain. Instead, the congruence is partly the result of a theory's determining what is taken as a fact. Major shifts in theory occur not because observation

fails to correspond to theory but rather because a rival theory, with its own notions of fact and observation, overthrows the old theory in a "scientific revolution." Thus, theory development is a discontinuous process of discrete jumps.

Second, the linear model of theory development assumes a clear-cut relationship between theory and data. Experimental results are supposed to dictate adjustments or expansion of the theory in specifiable ways. However, in practice, the relationship between observation and theory is rarely well determined. For one thing, an experimental result is never related to only one statement of the theory. Instead, the whole theory is tested by an experiment, or alternately, the whole theory subsumes the experimental results.[51] Therefore, it is not clear which statements of a theory are implicated by a particular observation. Furthermore, every experiment also involves a "background theory," that is, certain beliefs about the experimental apparatus as well as the instruments used for observation. Therefore, an experimental result may implicate the background theory as well as the theory in question.

Thus, observations do not clearly dictate theory development. In some cases an observation may lead a theorist to adjust a theory in ways that prove fruitful, and the modified theory predicts and explains a more comprehensive array of facts. In other cases, an observation may lead a theorist to a purely *ad hoc* adjustment in the theory to accommodate the data. The adjustment does not flow organically from the theory; its sole purpose is to save the theory in the face of contradictory evidence.

In yet other cases, a negative finding may be regarded as an anomaly and have no impact at all on the theory. Supporters of the theory may judge that it is premature to try to accommodate the anomaly immediately and that it should be addressed at a later time. Or else, supporters of the theory may doubt the authenticity of the anomaly. Thus, it is possible to maintain a theory despite adverse evidence by a series of *ad hoc* adjustments or by ignoring the negative data.[52] Even a theory universally acknowledged to have been refuted may be held onto because of its heuristic benefits. Thus, a theory is abandoned, on this view, only with the arrival of a competing theory which accommodates the data better than the old one and is a more powerful heuristic. Scientific progress is therefore the result of revolutions which represent discontinuities in theory development.

Reply

The first criticism of the behaviorist linear model of theory development is based on the thesis which denies the distinction between theory and observation. As discussed in chapter 3, this thesis is itself not firmly established. For the most part, behaviorists deny its relevance to their own research. Furthermore, even if the thesis is valid, it is not particularly damaging to behav-

iorist meta-theory. It simply asserts that a science of behavior is like any other normal science. Each tests its theories by observation, and each draws a distinction between theory and observation that is relative to a paradigm. Within this paradigm, theory development is reasonably continuous.

The second criticism is somewhat off the mark. It assumes that behaviorists claim to have a strict algorithm dictating how observations generate changes in theory. This, however, is generally not the case. Most behaviorists recognize that, in practice, the revision of a theory in the light of experimental results is a creative act of the theorist. This is especially true for Skinnerian meta-theory in which great emphasis is placed upon the intuitive hunches of the scientist.[53]

At a more fundamental level, both criticisms from the nonlinear model are misguided. The linear and nonlinear models are not incompatible; they are simply on different levels of logical discourse. The nonlinear model conceptualizes scientific progress as a succession of scientific paradigms, each containing its own ideas about observation, theory, and theory development. It is thus the paradigm that dictates how normal science is to be carried out, not the nonlinear model. The behaviorist linear model is properly regarded at the level of a scientific paradigm legislating how normal science is to be implemented. As such it is not incompatible with the nonlinear model which is a theory *about* paradigms and is therefore on a more abstract level.

CONCLUSION: THEORY, STRATEGY, AND VALUES

In the philosophy of science reconstructed in part I, behaviorist science begins with observations reported in the behavioral data language. Although behaviorists agree that this language alone is insufficient to operate as a science, they are divided over how to supplement that language to create a true science. The tension stems from the conflict between the impulse to strengthen behavioral theory by expanding the kinds of theoretical concepts admitted and the desire to protect the empiricalness and objectivity that are the rationale of behaviorism.

Operationally defined concepts are readily accepted because they are very closely linked to the behavioral data language, the guarantor of empiricalness and objectivity. Dispositional concepts, introduced by reductive chains, are also met by little resistance although the acceptance of such partially defined concepts means dropping the requirements of strict operational definition. Well formulated intervening variables are defined by the stimulus as well as the response side and are therefore fully determinate, although open, concepts. Nevertheless, the introduction of intervening variables generates much opposition, based mostly on fears that intervening variables will be misused, permitting the introduction of subjective and nonempirical concepts. Disputes over intervening variables, therefore, are basically disagreements over strategy.

Opposition to the introduction of hypothetical constructs is even greater than that to intervening variables. Because these concepts are loosely linked to the behavioral data language there is more opportunity for misuse. Nevertheless, most behaviorist theories admit such concepts although with tacit restrictions.

Finally, there is disagreement over how theoretical constructs are to be interrelated to formulate a theory. The extremes of this controversy are Hullian hypothetico-deductivism and Skinnerian descriptivism. This disagreement is not merely a dispute over the formal expression of a theory. It is more a controversy over the role of theory in the explanation of behavior and in the organization of experimental research. Again, the disagreement is one over strategy. Opponents of descriptive theories claim that such theories lack heuristic value while opponents of hypothetico-deductive theories argue that these obstruct and misdirect research.

Contrary to appearances, the question as to which types of theories and theoretical concepts are most fruitful and strategically the best is not entirely an empirical issue. Questions of "fruitfulness," "heuristic value," and "progress" are all relative to human goals and purposes. If there were agreement on goals and purposes, then perhaps the fruitfulness of a theory could be measured, and questions of strategy would have an empirical answer. However, in the absence of agreement of this sort, controversies over what is the "best strategy" are partly disagreements over values.[54]

Even for behaviorists, among whom there is general concensus that the goals of psychology are the prediction and control of behavior, criteria cannot be formulated in a universally agreed way. First, there is no common opinion on what is meant by the "behavior" to be predicted and controlled.[55] Second, there are sharp disagreements over measures of predictiveness and control. Is predictiveness to be measured purely by number of predictions or should the range of behaviors predicted and the number of species included be considered as well, and given how much weight? Is the prediction and control of human behavior the ultimate goal, or are all species created equal in the eyes of psychology? Is control to be measured by number of behaviors controlled or are socially important behaviors to carry special weight? Is the precise prediction and control of a few behaviors in a few species preferable to the less precise prediction and control of many behaviors among many species? All these questions involve values and are therefore not necessarily subject to resolution by scientific means.[56]

On the other hand, it *is* an empirical question as to what scientists actually do in developing psychological theory. Of course, Skinner is correct that scientists will pursue whatever they find to be most effective, but without a clear definition of "effectiveness," this prediction remains a truism. Skinner may even be correct that in the long run, contingencies of human survival will control the behavior of scientists. In the short term, however, scientists will adopt whatever they individually find most rewarding. To some extent the

ability to predict and control behavior is a critical reward for the scientist. However, in an immature science like psychology, where the association between the individual scientist's research and the ultimate goals of the science may be remote, other "irrelevant" rewards exert powerful control over the scientist's behavior. Social rewards in the form of prestige for adhering to a fashionable theory, grant money, admiration, and academic tenure, determine the development of psychological theory to a large, although unknown, extent. The success of a scientific paradigm is not always decided on rational grounds.[57]

It is not clear what role is played in the determination of a scientist's behavior by *a priori* prescriptions as to how science "ought" to proceed. Often these philosophical pronouncements follow the success of a theory. Philosophers formulate a *post hoc* formal reconstruction of what is often a successful case of informal theorizing. The formal reconstruction is then raised as the model to be followed by future theory. However, by the time the nature of this reconstruction is somewhat clarified by debate within the philosophical community, scientists are working on the next theory, which may not fit the former model. Philosophy of science is in a continuous attempt to catch up to science, and is in this sense an epiphenomenon of science, affected by science but having little impact in return.[58]

In any event, psychology contains too few successful theories to serve as models, and no successful models which have been logically reconstructed. Extrapolations from other sciences or from the history of psychology itself, even assuming the evidence is unambiguous, are not decisive, for psychology can always be distinguished from the other sciences, and the present can always be distinguished from the past. Opinions about the best strategy for psychological theory will remain, hunches for the foreseeable future—informed hunches and necessary hunches, to be sure, but hunches nevertheless.

Whither Objectivity and Empiricalness?

Behaviorist doctrine holds that the empiricalness and objectivity of theoretical concepts are to be achieved by linking these concepts to the behavioral data language. However, as this linkage becomes more remote, empiricalness and objectivity are jeopardized. Hypothetical constructs, having the most tenuous ties, are the most at risk. Behaviorist restrictions on the nature of these constructs help to ensure that hypotheses containing them are at least testable. Even with these restrictions, however, the anchoring of these constructs in the behavioral data language is indirect. One model of the nature of this anchoring is that of correspondence rules. However, correspondence rules are not entirely satisfactory. First, psychological theories are not axiomatized in the way required by the correspondence rule model.[59] Second, the notion of correspondence rules is itself open to a number of objections.[60] This leaves the anchoring of hypothetical constructs in doubt.

Intervening variables are well anchored in the ideal but not in practice. Tolman, the originator of the intervening variable, abandons their use, claiming that all useful concepts have surplus meaning beyond the defining equations.[61] One of the few groups of well formulated pure intervening variables in psychology, Skinner's concepts of drive, emotion, and reflex reserve, are eliminated by their creator, precisely on the grounds that having no surplus meaning they can be eliminated. Although Hull maintains his intervening variables to the end, it has been repeatedly shown that they are not anchored on both sides as he intends.[62] Thus in practice, the anchoring of intervening variables is also left in doubt.

Although operationally defined concepts seem more securely anchored, the linkage is not without its own ambiguity. Several operationists recognize that the defining operations are themselves in need of operational definitions.[63] To escape an infinite regress, operationists have suggested three possible resolutions. First, the regress can end when the listener and speaker achieve agreement on what the concept refers to. Second, the regress may end by using the fundamental operation of pointing at the concept's referent. Third, discrimination training, involving reward for response to examples of the concept, can teach the listener to differentiate the referent of the concept.

The problem with the latter two solutions is that they assume that pointing and discrimination training logically determine a unique concept. However, as Wittgenstein repeatedly shows, neither does.[64] Why is it that pointing is assumed to operate in the direction of shoulder to finger and not the opposite? In pointing at a table, how is it known whether "table," "wood," "heavy," or "table top" is intended? Furthermore, no finite number of exemplars used in a discrimination training uniquely determines a rule for the concept. It is possible to choose further instances of the concept in mutually contradictory ways, each of which will be consistent with the finite number of examples initially shown as instances of the concept. Similarly, it is always possible to follow a given rule in mutually contradictory ways, each of which is consistent with the rule.

Identification of concepts is thus logically underdetermined by explicit rules and training with exemplars. How is it then that in fact humans do share concepts and are able to communicate effectively? Wittgenstein's answer is that although training and explicit rules logically underdetermine concepts, psychologically they do not. Because of human nature, when people are given rules or training for using a concept, they tend to "go on" in the same way to apply the concept. Therefore, the concepts of individual members of a language community are congruent with one another and communication is possible. The infinite regress is avoided because human agreement is in fact achieved.

If a concept cannot be logically tied uniquely to its instances in the world, must the behaviorist abandon empiricalness and objectivity as impossible goals? Not necessarily. Because of the nature of the human mind, training and rules

do tend to tie a concept to the world in a unique way. However, this does not mean that all concepts are therefore equally well tied. Clearly, if humans are given only one examplar, they are less likely to apply the concept in the same way than if they are given numerous exemplars. Similarly, a set of vaguely specified rules is less likely to lead humans to apply the concept identically as compared to a set of clearly specified rules.

Given this irreducible underdetermination, behaviorists attempt to specify concepts by rules and training in such a way that humans will be likely to apply the concept in a common and useful way. The more precision in specifying a concept, the greater the likelihood of achieving this goal. That it is in fact achieved can be known only by induction from the degree of agreement and success achieved by users of the concept. Thus, intersubjective agreement plays a critical role in the formulation of behaviorist views of concept formation just as it does in behaviorist standards of observation. This is to be expected since observation, as opposed to mere sensory stimulation, involves concepts.[65]

Although behaviorist concepts are thus not linked to the behavioral data language in the ways that behaviorists assume as ideal, they are nevertheless not necessarily lacking in objectivity. Good behaviorist theories provide exemplars of theoretical concepts by applying the concepts to prototype experiments. These provide examples of how theoretical concepts such as "expectation," "habit strength," and "contingency of reinforcement," are applied. Students taught with these exemplars are, in effect, receiving discrimination training in applying a concept.[66] They also receive equations, demonstrations, verbal definitions, and other statements intended to link the concepts to the world.[67] The success of the training and the rules is determined by how well students and colleagues are able to achieve agreement, prediction, and control in using the concepts.[68] Not only is this achievement a measure of success for the training and rules, but it is also a measure of the concept's objectivity.

As a demand for precise rules and rigorous training, behaviorism is open to the challenge that it may over-regulate the use of concepts, develop rigidity, and thereby stifle human creativity and imagination. Perhaps there is a tradeoff between precision and over-regulation. In this case, behaviorism can be characterized by what it considers to be a fair tradeoff. Behaviorists are more impressed with the dangers of unbridled speculation in the history of psychology then they are with the inhibiting effect of precision. Ironically, then, behaviorist insistence on rigor, precision, and objectivity is based ultimately on intuition.