"WHAT IS LEARNED?"—A THEORETICAL BLIND ALLEY[1]

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Introduction

The pursuit of knowledge frequently has been hampered by misdirection. Questions lacking any possibility of being answered experimentally are often considered to be empirically meaningful. In many instances this inherent lack of meaning is so well hidden that much time and effort is wasted on attempted answers. The history of psychology has contained many such cases of "meaningless questions"; the search for the basic elements of consciousness and the attempt to estimate whether intelligence is determined by hereditary or environmental variables are but two of the more outstanding examples of such unanswerable questions.

The history of these questions appears to follow a common course of development. Initially the question provokes much interest and seemingly opposed speculations are offered as possible answers. Then the question is subjected to experimental attack. This usually intensifies the theoretical differences because much bickering is instigated about the adequacy of the experimental design as well as the correct interpretation of the experimental data. The result is that agreement is never attained. Gradually the interest in the question subsides and successive generations of scientists learn to avoid the question because they intuitively recognize the futility of pursuing it. Then, finally, the question's inherent lack of meaning is fully realized and exposed.

The recognition of the status of such unanswerable questions need not proceed in the above described haphazard manner with its consequent costliness in time and effort. Present-day philosophy of science has been concerned with establishing criteria for distinguishing between meaningful and meaningless questions. By the application of methodological analysis it is possible to demonstrate that certain problems are not resolvable, not because they are too profound, but rather because the questions they raise cannot properly be answered. It will be the major intent of this paper to demonstrate that the current controversial question of "what is learned" falls into the category of questions that cannot properly be answered.

The Question

The frequency with which learning theorists attempt an answer to the question of "what is learned" has led contemporary psychologists to invest this question with a conspicuous import. In fact, it is implied by such seemingly antithetical theorists as Tolman (15) and Guthrie (5) that *this is the crucial question* confronting the learning theorist; other controversial issues presumably stem from divergent conceptions of what is learned. For example, the current issue of latent learning appears to have its roots in the question of whether cognitive maps or stimulus-response associations are learned.[2]

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[2] Exception might be taken to this statement on the basis that the latent learning experiments were conceived in terms of the theo-

Experimental findings, however, appear incapable of resolving these opposed viewpoints. For example, Thorndike (12) in 1898, Muenzinger (10) in 1928, Adams (1) in 1929, and Guthrie and Horton (6) in 1946 all report behavior of animals in a problem box situation. It is interesting to note that their respective answers to the question "what is learned" appear to be at variance. Thorndike, and Guthrie and Horton, concluded that stimulus-response associations were learned, although disagreeing, to some extent, about the conceptual properties of the association. On the other hand, Muenzinger and Adams interpret their data to support the conception that "ideas" or "meanings"—concepts which appear to be equivalent to the more modern "cognitive map"—were acquired. This difference between these conclusions would not be too surprising if disparate findings were obtained. This was, however, not the case. Careful readings of these experimental reports suggest that these experimenters were observing similar behavior patterns of their subjects. Guthrie and Horton write, ". . . Adams' report is so conscientious that we are able to identify in our experiment all the behavior that Adams describes. . . ." One may conclude that if comparable data are employed to support diverse answers to the same question, then the major source of difficulty lies not in the seemingly opposed answers but, rather, in the question itself. An analysis of the question is therefore in order.

retical problem of whether or not reward was necessary for learning. Tolman (15) has recently stated quite explicitly that the results of the latent learning studies are just as inconsistent with an *S-R non-reinforcement* theory as with an *S-R* reinforcement formulation. The major contribution of these experiments, according to Tolman, is that they demonstrate that cognitive maps, rather than stimulus-response associations are learned.

AN ANALYSIS OF THE QUESTION

How do these conflicting conceptions of what is learned arise? At first glance it would appear that they are at the very core of the various theoretical conceptions. While Tolman (15) insists that cognitive maps are learned, Guthrie (5) is equally adamant that what is learned is an association between "some stimulation of sense organs and a corresponding muscular contraction or glandular secretion."

The fact that these conceptions are considered by most theorists and students in the field of learning to be both basic and crucial should not permit us to accept their relevance without question. No matter how deeply imbedded they appear to be in their respective theoretical structures, the possibility remains that these various conceptions of what is learned may be essentially external to the theory. For example, in theoretical physics, mechanical models serve as convenient tools of thought, but they are neither fundamental nor even necessary to the theoretical formulation.

Whether or not a conception of what is learned is basic to a particular learning theory can be determined only by understanding the structural requirements of a learning theory. It may appear strange to use Tolman's blueprint (13, 14) for theory construction as a guidepost in our methodological analysis. His early papers and the more recent "Cognitive maps in animals and men" stand toward one another as clashing antinomies. Tolman has violated methodological requirements which he himself promulgated. Much of the confusion surrounding the problem of what is learned would disappear if learning theorists, especially Tolman himself, would be more sensitive and sympathetic to the implications of these early papers (13, 14). A brief exposition of the structural requirements of a learning theory is, therefore, in order.

Tolman has expounded the pragmatic use of the intervening variable to bridge the gap existing between the independent and dependent variables. Rather than treat separately the relationship each independent variable bears to the many dependent variables, Tolman proposed the grouping of certain independent variables; these groupings or component functions would then be connected logically to constructed intervening variables; these in turn would be connected to one another, and finally to the dependent variables. This *intellectual construction* has as its aims the economic description of the known empirical relationships, and the prediction of new phenomena. Tolman's blueprint for theory construction has been used by himself, Hull (8) and others in their theorizing. The core of these theories has been the intervening variables, e.g., habit strength, cognitive map, etc. Basic to the entire question of what is learned is the specific interpretation given to these intervening variables (theoretical constructs). We shall see that secondary, and unnecessary, elaborations about the meaning of these intervening variables have led to what some have considered to be conflicting views as to what is learned.

There would be no confusion about the meaning of such terms if it were always remembered that these intervening variables serve as *economical devices* to order experimental variables in relation to the dependent variables. They are *"shorthand" descriptions,* and nothing more, of the influence on behavior of several independent variables. The *only meaning* possessed by these intervening variables is their relationship to both the independent and dependent variables. Because this point has been ignored or misunderstood, an immense amount of confusion concerning the "real meaning" of these intervening variables exists.

A somewhat similar situation existed in theoretical physics with regard to the gravitational action between distant bodies. For many physicists the idea of action at a distance was odious; it did not appear conceivable that a phenomenon could be "caused" by a relationship. In order, therefore, to make the phenomenon more "meaningful," different kinds of space-filling mediums capable of transmitting forces from one body to another were hypothesized. But there is no justification, as Bridgman (3) points out, "for the attitude which refuses on purely *a priori* grounds to accept action at a distance as a possible axiom or ultimate of explanation."

Nagel uses a neat example to describe the above type of error which has been labelled the fallacy of reification or hypostatization. He writes:

"Suppose, for example, that Smith invites to his home his foreign friend Forgeron, so that the latter may see for himself what family life is like in America. But suppose that after enjoying Smith's hospitality for some time Forgeron was to complain that though he met Smith's wife and children, had meals in common with them, took part in their recreations, and so on, he had never been introduced to Smith's Family Life. It is clear in this case just what is the nature of Forgeron's error: he mistakenly assumes that "Family Life" is the name of some supra-sensible "entity," distinct from the complex activities in which Smith and the members of his family are normally engaged" (11, p. 236).

In the very same sense the construct of learning, whether it be conceived in terms of modifications in cognitive maps or *S–R* connections, does not refer to an object, thing or entity as suggested by those who are concerned with the question of what is learned. These intervening variables possess no meaning over and above their stated relationships between the independent and the dependent variables. The basic error underlying the problem of what is learned is the assumption that these intervening variables are entities capable of being described and elaborated upon, independent of their operational meaning. The fallacy of reification yields the problem of what is learned. The realization that learning is not a "supra-sensible entity" disposes of it.

INTUITIVE MODELS VS. OPERATIONAL MEANING

It would be interesting, and perhaps fruitful also, to go beyond our initial methodological analysis in order to understand the psychological factors underlying the confusion surrounding the question under consideration. After all, psychological theorizing is just another form of behavior. Why cannot we attempt as psychologists to understand why meaningless questions are asked and concepts are reified?

It appears to this writer that the confusion surrounding the meaning of theoretical constructs stems from the failure to distinguish sharply between personal thought processes leading to the invention of theoretical constructs and the *operational meaning of the term.* The first is a problem in the psychology of creative thinking (in this case the psychology of the psychologist), the second is a problem in epistemology. Why Hull (8), for example, chose to define habit strength in the manner he did is a function of his intellectual abilities, his style of thinking, his personality, his interests, etc. In order to know what habit strength is, one need not know the intimate details of Hull's background or have a knowledge of his intellectual functioning; one need only know how this concept is tied down to "observable events," i.e., its operational definition. The thought processes of the psychological theorist may, as they usually do, involve the use of some sort of a model. It becomes necessary, when such models are used as *thinking aids,* that the model not be confused with the scientific concept stemming from it. Too often learning psychologists have been misled into believing that the intuitive models which they associate with their intervening variables possess existential properties above and beyond the operational definition of the intervening variable.

Although psychologists are very willing to admit the existence of individual differences in motor skills, aptitudes, and many other behavior characteristics, they appear markedly reluctant to admit that such differences may exist in creative thinking. Would it be valid to say one must think "phenomenologically" as does Tolman, or within a Darwinian framework, as Hull appears to do, or "algebraically" as does Spence, or in terms of physical models as does Köhler? It is granted that only a valid and extensive knowledge of the psychology of thinking could answer this question. If we refuse to ascribe certain unique and mystical qualities to the process of creative thinking, i.e., if we consider it just as another form of behavior, then we may extrapolate from our knowledge of other behavior. We know that people learn mazes in different manners, runners sprint with different styles, and, if we are to believe Kinsey, people woo in different fashions. Certainly within our limited knowledge of the variables involved in productive thinking, it would appear questionable to assume that only one style of thinking would be fecund; and it would be hazardous, as well as somewhat presumptuous, for any theorist to insist that every other theorist think in his style.

The above point obviously does not imply that theorizing is purely a matter of personal taste. At some stage in the development of a theory, the theorist must meet the requirements of his scientific audience, i.e., the operational definitions of his theoretical (intellectual) constructs must be made clear to everyone who is sincerely interested in them. This does not mean that the theorist must reveal his private cerebration (for purposes of clarity, it may be best if he did not), but that he must state the relations which his theoretical constructs bear to the observable variables so that their "validity" may be tested.

In evaluating the "validity" of a learning theory it should be recognized that the most we can expect is that the theoretical estimates of the dependent variable be in agreement with the observed responses in an experimental situation. All learning theories must fulfill this function, i.e., generate valid estimates of the to-be-observed response. In this sense *all learning theories are response theories.*

The above analysis leads the writer to conclude that Tolman's recent division between place learning as contrasted with response learning is not sufficiently cogent; it suggests a qualitative distinction where none exists. Such a distinction arises only if one considers differences in intuitive models to be basic to theoretical differences—a position to which this paper would take exception. Using the same sort of logic, one would also be forced to conclude that Guthrie's conviction that movements are learned or Hull's belief that receptor-effector connections are required are as invalid as Tolman's feelings that cognitive maps are learned.[8]

Our initial point was that the pseudoproblem of what is learned emerged from the methodological error of considering learning an entity rather than a process. Our second point is that this methodological error is due to the contamination of the operational meaning of theoretical constructs by their intuitive properties. This contamination has led to spurious formulations of theoretical differences. Rather than persistently pursue both the obvious and subtle connotative meanings of the words used in theorizing, it would be much more productive to attempt to relate the constructs systematically to the manipulable variables and behavior measures. Only then can the explanatory power, inherent in the concepts, be tested.

THE "CAUSE" OF THE CONFUSION

Why have learning theorists indulged so much in unnecessary elaborations of their theoretical constructs at the ex-

[8] In actual practice there are two differences which appear to separate the cognitive theorists as a group from the *S-R* theorists. Firstly, the two appear to possess somewhat different thinking styles. Aside from certain scientific requirements, the selection of the specific terms in a theoretical structure seems to be determined by the personal needs of the theorist. Perceptual and cognitive terms appear to be intuitively satisfying to the cognitive theorist while they tend to evoke suspicion among *S-R* theorists. I suspect that if Hull had labelled what he now calls habit strength "dynamic cognitive field expectancies" without modifying its postulated relationships to the independent variables, much of the opposition to this concept would disappear and probably some new opposition would arise from certain quarters. The second difference is related to the problem as to what point in the "theoretical bridge" between the independent and dependent variables should the nature of the response be indicated. The *S-R* theorist usually specifies the nature of the response relatively early, while the cognitive theorist does not specify the nature of the response until rather late in his theorizing. In fact, they have been frequently criticized, with some justification, because they do not always translate the cognitive map of the organism into specific action; in other words, they do not always bridge the gap between the independent and dependent variables. The point on the "theoretical bridge" at which the initial response estimate is introduced may prove to be an important stratagem in theory construction. Since I have emphasized the operational meaning of concepts rather than their connotative meaning, and because so few contemporary theorists have come to grips with the problem of the location of the initial response estimate, it is felt that the above two differences are not *basic.*

pense of determining their relationships to the observable variables, i.e., the substitutions of models for operational definitions? It is the writer's belief that the answer to this question lies in different conceptions of the nature of scientific explanation.

Again, it may be appropriate to interject into our discussion certain speculations concerning the psychological basis of misconceptions of scientific explanation. Most, if not all, psychologists were confronted with the question "Why?" prior to their exposure to scientific training. It is safe to assume that intuitively satisfying "explanations" were achieved during this pre-scientific era of personal development. There is a strong suggestion that some of these pre-scientific explanatory techniques have lingered on and have been confused with scientific explanation.

Feigl (4), in a brief perspicuous exposition, states that the demand for explanation "is answered by deductions either from empirical laws or from theories." He enlarges upon this dictum as follows:

"Deduction from empirical laws may be styled 'low grade' explanation. It merely puts the fact to be explained into a class of facts characterized by the same empirical law. Thus the explanation for the fact, e.g., that there is a mirror image of a bridge in a river, is achieved by subsuming this fact under the law of reflection in geometrical optics. This law is simply the common denominator of all the various phenomena in which light-reflection is the essential feature. A 'higher-grade' explanation we find in the Maxwell-electromagnetic wave theory, which serves as a basis for deduction for a variety of optical phenomena; reflection as well as refraction, diffraction, interference, dispersion, polarization, etc. etc." (4, p. 286)

Psychology has achieved only a few "low-grade explanations." For example, Hull was able to deduce from his initial goal gradient hypothesis the locomotion gradient of rats in a runway, blind alley elimination, the more rapid elimination of long blind alleys as compared to short blind alleys, the backward order of elimination of blind alleys, and similar phenomena. In a much less formal manner Freud has attempted to integrate such phenomena as slips of the tongue, dreams, hysterical symptoms, tics, etc., by assuming certain unconscious mental processes.

This requirement for a deductive component has too often been ignored in attempts at explanation. The reason for ignoring the requirement of a deductive component in scientific explanation stems from the failure of some writers to distinguish clearly the requirements of scientific explanation from those attributes of propositions which are capable of instigating in some a feeling of "understanding" (the "a-ha" phenomenon). The contention of this paper is that some of the arguments surrounding the question of what is learned have stemmed from such confusion, i.e., between scientific explanation and, for want of a better term, "psychological understanding." This sort of confusion has been particularly noticeable in many of the formulations involving physiological and/or phenomenological terms.

A common misconception among some psychologists concerned with discovering the physiological correlates of behavior is that their area of interest excuses them from the task of developing a theoretical system capable of generating valid deductions. Believing that they are dealing with the "real causes" of behavior,[4] they feel that the mere specification of the physiological factors or, more com-

[4] It has always been a mystery to me why, if one desires to be entrapped by the metaphysical problem of "real causality," one should be satisfied to stop at the physiological level without descending into the physicochemical, or the atomic, or the sub-atomic level.

monly, the mere speculation concerning the physiological processes is sufficiently explanatory. For example, Birch and Bitterman (2), in a recent polemic against *S–R* reinforcement theory, conclude that it is *necessary* to postulate a physiological process of sensory integration to explain conditioning. This conclusion is reached in spite of their admission that ". . . we know very little about the conditions under which sensory integration occurs." Actually, they do not specify any physiological variables or behavior measures to which "sensory integration" is connected. The result is that their formulation is incapable of generating any deductive implications—and consequently is void of any explanatory ability. Adequate explanatory systems in physiological psychology, as well as in other areas of psychology, must have a deductive component.

The above should not be interpreted as an attempt to make an invidious comparison between behavioral and physiological theories. The development of psychology can and should be furthered by serious and rigorous attempts at formulations of both behavioral and physiological theories. This writer believes (on an intuitive basis) that, at the present time, the program of independent development at both levels would be the most strategic, followed, of course, by an attempt at coordination. Another procedure would be to develop behavioral and physiological formulations interdependently and simultaneously. There is no reason why such attempts could not be successful. The tendency, however, has been merely to use some physiological-sounding terms, extracted in all likelihood from the private thought processes of the theorist, without specifying their relations (hypothetical or established) to the experimental variables and behavior measures. Although such a procedure provides a ready "answer" to the question under consideration ("what is learned"), these answers contribute nothing to our understanding of the learning process.[5]

A similar problem is raised by some theories which use phenomenological terms as labels for their theoretical constructs. It appears that for many learn-

[5] The problem of the physiological properties of intervening variables has been raised in a somewhat different form by MacCorquodale and Meehl (9). They argue that it is important to distinguish between two types of theoretical constructs, hypothetical constructs and intervening variables. The former "involve the hypothesization of an *entity, process,* or *event* which is not itself observed . . ." while the latter ". . . do not involve such hypothesization." These writers demand ". . . of a theory of learning that those elements which are hypothetical . . . have some probability of being in correspondence with the actual events underlying the behavior phenomenon. . . ." Referring to some of Hull's early theoretical papers in this JOURNAL, MacCorquodale and Meehl write:

> "We suspect that Professor Hull himself was motivated to write these articles because he considered that the hypothetical events represented in his diagrams may have actually *occurred* and that the occurrence of these events represents the underlying truth about the learning phenomena he dealt with" (9, p. 104–105).

Here again, the personal cogitations of the theorists are confused with the operational meaning of the concept. Although it may appear "clinically justified" to assume that Hull had some conceptions of physiological mechanisms coordinated to some of his theoretical constructs, the meaning of those constructs should be confined to their operational definition. Other theorists who work within the *S-R* reinforcement theoretical framework have no such intuitive physiological conceptions. Should we therefore have two (or possibly more) conceptions of these physiological sounding concepts, the constructs varying not in terms of their operational meanings but rather in terms of the individual's intuitive conception? For purposes of communication, it appears wise to separate all the private cogitations from the operational meaning of concepts. The important point is whether the behavioral theory can be divorced from the "physiologizing" without damage to the deductive capacity of the theory. In the case of Hull's theory it would certainly seem possible.

ing psychologists it is essential that their theoretical constructs be phenomenologically consistent, i.e., capture the quality of naive introspective experience. There is little doubt that the theoretical constructs used by psychological field theorists reflect "inner experience" in a more satisfying manner than do associationistic constructs. There is no basic methodological objection (or virtue) to the use of either set of terms *as long as they are related to observables, and as long as they are used in formulations with deductive capacities.* This rule, however, is not always followed. All too frequently, constructs by themselves (e.g., structuring, insight), which describe vividly phenomenological experience, are presented as scientific explanation.

I am convinced that the selection of any theoretical model, be it physiological or phenomenological, or for that matter, physical, mechanical or statistical, is in the last analysis a decision having *no truth character.* That is, in spite of the fact that the choice of a model may, and usually does, influence both experimentation and theorizing, *the choice itself* cannot be evaluated as being right or wrong. It is a matter purely of personal taste. The most we can do is to attempt, in a sincere and conscientious manner, to understand the implications of such decisions, but we should not be led astray by believing we can experimentally test their validity.

What then should the learning theorist do? Instead of seeking an answer to the question of what is learned in a manner based upon inadequate conceptions of scientific meaning and scientific explanation, it would be better for learning theorists to come to grips with their undertaking in a positive forthright fashion, i.e., to anchor their theoretical concepts to observables and unhesitatingly test the explanatory capacities of their formulations.

By means of such a constructive approach, the time wasted in needless arguments of the sort which has centered around the sterile question of what is learned would be reduced and, perhaps, even be eliminated.

SUMMARY

Contemporary learning theorists offer numerous and seemingly opposed answers to the question as to what is learned, the major difference among them being whether "cognitive maps" or "stimulus-response associations" are learned. The argument in this paper is that the problem of what is learned is a pseudo-problem. It stems from the methodological error of reifying theoretical constructs. This reification in turn is due to the failure to distinguish sharply and consistently between the operational meaning of intervening variables and the intuitive properties ascribed to these concepts. If we conceive the intervening variable as being an economical device by which experimental variables are ordered in relation to behavior variables, then this confusion will not arise. The basic difference between such intervening variables as "habit" and "cognitive map" can be specified only in terms of their stated relationships to the observable variables, not in terms of the connotations they arouse.

At the bottom of this seeming disagreement as to what is learned is the failure of many psychologists to utilize an adequate criterion of scientific explanation. All too often, propositions (usually involving phenomenological or physiological terms) which have no deductive capacities are accepted as explanations because they instigate in some a sense of "psychological understanding." It is proposed that learning theorists avoid the problem of what is learned and come to grips with their undertaking in a positive forthright fashion. This can be accomplished by

relating theoretical constructs to observables and unhesitatingly testing the explanatory capacities of their formulations.

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