Psychology and epistemology: The place versus response controversy

RON AMUNDSON
University of Hawaii at Hilo

Abstract

The conduct of any science is influenced by methodological commitments of its scientists. These commitments vary between theoretical schools, and become most apparent in the context of a theoretical conflict. Psychology is particularly influenced by psychologists' epistemological views, views on the nature of scientific knowledge and how it is best obtained. The notion of "epistemological parity" is here introduced as a mechanism by which a psychologist's substantive theory and epistemological views are adjusted one to another. The interaction between epistemology and substantive theory is examined in a historical case study of the "place versus response" controversy. This dispute matched a group of S-R behaviorists against a group of cognitive psychologists headed by E.C. Tolman. The issue is commonly called a "pseudoproblem" by behaviorists. It is argued that the great difficulty in resolving the issue was due to unacknowledged differences in epistemology between the parties. The two sides of the dispute each achieved epistemological parity, but in very different ways. Such theory/epistemology interactions influence the course of debate and experiment in ways unique to psychology. A second contribution of this study is its discussion of the pre-1950 roots of modern cognitive psychology.

Epistemology is the philosophical study of knowledge. Epistemologists typically address such interrelated questions as: (1) What is knowledge? (2) Under what conditions is knowledge attained? (3) What human capacities make knowledge possible? Since the seventeenth century science has been providing some rather exciting examples of knowledge. Naturally, science has received special attention from epistemologists. The more specialized of these epistemologists are now called philosophers of science. It should not be assumed that because epistemology was given its name by philosophers,
it is the sole concern of philosophers. It is often an important concern of scientists themselves. Any deep theoretical dispute in science will involve differences of opinion on the proper methodology of science. (In case anyone still doubts it, *The Scientific Method is a textbook myth.*) Scientists’ debates about methodology often revolve around disagreements about what scientific knowledge is (e.g. an organized set of predictions and observations? a description of unobserved reality?), and what means are best suited to achieving it (e.g. induction? model building? hypothetical reasoning?). So scientists have epistemological commitments. Historical study shows that specific commitments differ from one scientific school to another. As will be seen below, these commitments can play a crucial role in the course of experiment and theoretical debate.

It will be argued in this paper that the mutual constraints between epistemology and substantive theory are especially strong in the science of psychology. Epistemological commitments function as a sort of ideological background to substantive theory. The coincidence of certain kinds of epistemology and certain kinds of substantive theory can be observed in the history of psychology (Amundson 1983b), the most widely recognized case being the alliance between positivist epistemology and behaviorist psychology. I will attempt to illustrate the striking importance of epistemology by showing its influence within an actual psychological debate. An explanation will be proposed of why epistemology plays such a large role in psychological theorizing. The debate to be studied pits a group of cognitive theorists against a particular school of S–R behaviorists. It began in 1946 and lasted about a decade before being declared (by behaviorists) a “pseudoproblem.” Because of this focus, a secondary contribution of the paper will be its discussion of the development of cognitive psychology prior to 1955—an epoch regarded as prehistoric by many cognitive psychologists and philosophers.

The importance of epistemology to psychology is not accidental. It is not, for example, due to the “immaturity” of psychology. It is due to the special relations between the tasks of epistemology (especially one adopted by a scientist or a scientific school) and the task of psychology itself. Consider the three epistemological topics mentioned above: the nature of knowledge, the conditions under which it is most likely to be achieved, and the capacities possessed by humans which make them potential knowers. Of these three, the first two topics will arise in any science. Any scientist must decide when to treat a belief as an established fact (as knowledge, at least for purposes of conducting the science), and which research methods will be most productive. But not every science has a direct concern with the third topic, the human abilities which make knowing possible. This area is the special domain of psychologists. Much of the task of psychology (the psychology of learning
and/or cognition) is empirically to discover precisely what knowledge-gaining abilities the psychological subject possesses. To be sure, any epistemologically concerned scientist must make some assumptions about human psychology in order to address the other two topics. But only for psychology is the third topic directly addressed by the science’s substantive theories. Clearly the empirical discoveries of psychology are relevant to epistemology. So, if epistemology is relevant to the development and support of a scientific theory, then it follows that the discoveries of psychology are relevant both to epistemology in general, and (more importantly) to the epistemology and methodology of psychology itself. The psychologist has a dual role, both as student and as exemplar of human knowledge-gaining abilities. Psychology’s significance for epistemology is often recognised (Quine, 1969; Zuriff, 1980). Some think it gives psychology a special sort of epistemological validity, since the psychologist (unlike, say, the physicist) can empirically test his or her own epistemological principles, at least those relating to the third epistemological topic (Stevens, 1936; 1939; Wolman, 1971). I am not convinced that this duality is scientifically progressive, but it certainly influences the course of psychological debate. The following study will illustrate the interplay between epistemology, theoretical debate, and even experimental design.

It will be shown that psychologists are to some extent aware of the duality of their enterprise (more in one’s own case than in that of a theoretical opponent) and that they adjust their substantive theory and their epistemology one to the other. I will suggest a sort of mechanism for this adjustment, the principle of epistemological parity: Accept only those theory/epistemology pairs in which the epistemology would be appropriate to a psychological being truly described by the theory. I will suggest, in other words, that theorists in psychology operate under a constraint not found in other sciences. A psychologist cannot adopt methodological or epistemological views in which the “knower” as described by the epistemology is significantly different from the psychological subject as described by substantive theory. The psychologist who accepts epistemological parity considers him/herself to be on an epistemological par with the subject—scientific knowledge acquisition is similar in kind to the knowledge acquisition of the subject being studied. Since the natural sciences do not have substantive theories describing “knowers,” they are free of this constraint. In any scientific school, we should expect the accepted epistemology to defend the substantive theory against at least some of its theoretical adversaries. (Cases of methodological and epistemological support of substantive theories are common in the history of science.) This is to be expected in psychology as well. But if epistemological parity operates in the way I will suggest, the psychologist has available not only an epistemological defense of substantive theory, but also a substantive defense of
epistemological commitments. Such as scientist would feel especially secure in his/her epistemological views. After all, the substantive results of psychology can be cited in support of the epistemology. Vague philosophical speculation need no longer be relied upon.

There is a catch, however. More than one well-adjusted theory/epistemology pair is possible. Each of two competing schools might be able to cite substantive results in favor of its preferred epistemology, and epistemological principle in favor of its substantive theory. As will be seen, this actually happens. The case at hand involves two well-adjusted but distinct theory/epistemology pairs. One was the cognitive approach of E.C. Tolman and his associates. The other was the relatively strict behaviorism of a group of Tolman's theoretical opponents, whom I will (stipulatively) call "neobehaviorists." The debate involved the behavior of rats in mazes. As we shall see, the differing epistemological views of the disputants were as crucial to the debate as were any empirical facts. Substantive issues can be seen to have been heavily epistemology-laden. The extent to which this challenges the objectivity of psychology—including modern cognitive psychology—will be discussed.

Epistemologies and representations

The episode we are about to consider was labelled the "Place versus Response" controversy. Tolman's cognitive theory of maze learning claimed that the rat "learned place," while the S–R reinforcement theory dominant at the time claimed that the rat "learned response." (Details to follow.) The epistemology associated with the cognitive theory was scientific realism; that associated with the S–R theory was called "operationalism." Both the substantive and the epistemological contrast can be seen as disputes about representation. The two related questions are: (1) Does the best psychological theory attribute to the rat internal representations of (e.g.) the structure of the maze? (2) Do scientific theories in general provide descriptions (representations) of epistemologically remote (unobserved or perhaps even unobservable) but objective reality? Cognitivism and realism respectively answer "yes" to these questions; S–R theory and operationalism respectively answer "no."

Operationalism

Psychologists have used the term "operationalism" in so many ways that it has lost much of its content. (Leahey, 1980, calls its modern use "liturgical.") In the place/response dispute a particular version of operationalism had a
definite content. As a certain sort of positivism, the operationalism of the day stated that any theoretical (i.e. non-observational) terms used in psychology must be fully definable using only observational terms. Now, most of twentieth century philosophy of science has been concerned with the ways in which theoretical terms might (or might not) be given observational definitions. (For example, is it the individual term, the theoretical statement, or the entire theory which receives observational definition?) These niceties need not much concern us, as they did not much concern the operationalist psychologists who argued against Tolman. The important point is that operationalism identifies theoretical meaning with observational meaning. A theory, however complex, is in reality a shorthand description for an indefinitely long list of descriptions of observations. A theory is not a way of describing unobserved reality. Theoretical terms are, in principle, eliminable from scientific discourse. (In practice, of course, shorthand notations come in handy.) This eliminability was applied not only to the “mentalistic” concepts used by cognitivists. Even the unobserved structures postulated by S–R theorists like Hull were given the eliminability treatment. “…it is very important to realize that these covert responses [Hull’s mediating responses] and their associated stimuli need not really occur at all. They are simply conceptual processes…” (Goldstein, Krantz and Wiggins, 1965, p. 3, emphasis in original.) This is the principle of the eliminability of theoretical terms (ETT). It carries the implication that scientific theories do not describe unobserved reality. Any apparent scientific reference to such hidden reality can be shown (by ETT) to be an illusion. One simply translates the suspicious expression into its observational equivalent.

The group of operationalist behaviorists opposing Tolman accepted another methodological requirement, that of analysis of behavior into simplest behavioral units (SBU). This was the behaviorist version of elementism in psychology. Anyone who requires the translatability of theoretical terms must also be ready to specify the vocabulary into which the translation is to occur. SBU gave that specification. The term “neobehaviorist” will refer in this paper to an S–R behaviorist who accepts both ETT and SBU. This was the dominant orientation around 1950. It will surprise some to hear that neither E.C. Tolman nor Clark Hull were neobehaviorists by this definition. Both are commonly described as operationalists and/or logical positivists. Hull’s positivist sounding views were adopted from his very influential student Kenneth Spence, a genuine neobehaviorist (by the present definition). Hull’s work prior to 1943 shows no positivist influence (see Amundson and Smith, 1984; Smith, forthcoming). I have argued at length elsewhere that Tolman was a full-fledged realist (Amundson, 1983a). This conclusion was based on Tolman’s epistemological background, a careful reading of his supposedly
operationalist “intervening variable” discussions, and his endorsements of increasingly liberal and realistic modes of theoretical definition as soon as they were brought to his attention. Tolman’s operationalist reputation was based on a few early uses of jargon, “logical construct” and such. This non-standard interpretation of Tolman will be presupposed in the present work. The topics discussed will, however, further confirm his non-operationalist epistemology, both in methodological discussion and more importantly in action in theoretical debate and experimental activity.

Realism

Scientific realism is the view that good scientific theories give (potential) knowledge of a reality which is remote from the observations on which those theories are based. Clearly not all theories give such knowledge; some are false. But (according to the realist) a scientist is acting quite appropriately when he or she attempts to infer the nature of the unobserved world from observations of the world’s more accessible parts. The popularity of realism and of anti-realism vary with time and with scientific topic. Copernican astronomy and the kinetic theory of gases encourage realism regarding unobserved structures; quantum mechanics and Newton’s “hypotheses non fingo” lend comfort to anti-realists. Cognitive psychologists (both the contemporary and the pre-1950 varieties) are realistic. The cognitive goal is to infer the subject’s internal psychological makeup from observations of overt behavior. Operationalism is one form of anti-realism. As we shall see, it is a form especially compatible with S–R theories of learning.

Representations

Cognitive psychologists (cognitivists) attribute to the psychological subject complex internal states, the interactions of which result in behavior. But this is true of Hullian behaviorists as well (although operationalism came to discourage belief in the actual existence of such internal states). The distinct feature of cognitivism is that the typical cognitive state involves (or is) a representation. A subject which believes, or hopes for, some state of affairs has that state of affairs represented in a certain way in his/her mind. A

1Anderson has argued that there is a fundamental indeterminacy in issues regarding the nature of mental representations, and that “barring decisive physiological data, it will not be possible to establish whether an internal representation is pictorial or propositional” (1978, p. 249). Even this modest retreat from realism was greeted by Anderson’s colleagues with “disbelief and dismay.” It was seen to imply that “cognitive psychology is not possible” (p. 275).
subject who knows a language or a city, has a representation of the language’s grammar, or a cognitive map of the city’s geography, in his/her mind. Unlike non-cognitive internal states, such cognitive items as beliefs and mental images have contents. A belief is an internal state, but it is about an object in the outside world; the belief is true or false in virtue of the state of affairs in the objective world. A mental image is of some similarly external object. Hullian mediating responses and neurological structures, no matter how internal, show no such “aboutness.” The philosophical term for this aboutness is “intentionality.” Current philosophy of psychology concentrates heavily on problems involving intentionality ... the logical peculiarities of intentional ascriptions, the contributions of cognitive psychology to understanding the intentionalism of ordinary language, and whether it is possible or advisable to conduct cognitive studies while avoiding intentional idioms altogether (or, indeed, whether such studies would be cognitive at all). The nature of representation or of intentionality is by no means settled. But philosophers have come to one important consensus. It is this: the intentional idioms of cognitivism cannot be given operational definition into a nonintentional (e.g. stimulus/response) vocabulary. This does not imply that cognitive, representational states do or do not exist. But it does imply that the dispute between S–R and cognitive theorists is not a verbal quibble, to be resolved by proper operationalizing. Any such operationalization would rob the cognitive view of its intentional aspects.

The principle of epistemological parity may be at work in the use (or nonuse) of intentional idioms by psychological schools. A realist epistemology attributes to the scientist the ability to construct representations of the objective world which lies beyond direct observation. This requires inten-
tional descriptions of the scientists as referring to epistemologically remote features of the world, and of scientific theories as representing such features. These are just the kinds of abilities cognitivists attribute to psychological subjects. Conversely, operationalism forbids such objective reference. An operationalist's scientific activity is described as the development of increasingly accurate methods for anticipating future observations, and manipulating the environment in ways suitable to achieve these observations. Operationalists' psychological theories describe the subject as forming associative connections amongst sensations, or between stimuli and responses. The items associated are all within the subject's immediate epistemological environment. Intentional idioms are needed no more to describe the scientist's activity than to describe the psychological subject's. We will soon see less circumstantial evidence that these theory/epistemology pairings are results of epistemological parity.

During the Tolman/neobehaviorist debates the epistemological issues were submerged. Intentionality was not an acknowledged issue, despite the neobehaviorists's intuitive distrust of intentional idioms. Moreover, Tolman was widely reputed to be an operationalist. Many were puzzled that the problems were so resistant to solution, and expressed the hopeful view that the two approaches differed merely in emphasis, or in preferred vocabulary (on the even more hopeful assumption that the two vocabularies were intertranslatable). The invisibility of the epistemological differences allowed theoretical debate which may otherwise have seemed hopeless. But it also gave the false impression that clever experimental design would eventually lead to a resolution.

Place learning and cognitive maps

The best perspective on the place learning experiments is the notion of the "cognitive map," introduced by Tolman two years after the publication of the first place learning experiments. The experiments were cited in support of the notion of the cognitive map. In "Cognitive maps in rats and men" Tolman said that S–R learning theory described the animal's central nervous system as a complicated telephone switchboard. In contrast, Tolman's own theory depicted the animal's insides as more like a map control room, in which a "tentative, cognitivelike map of the environment" was constructed (Tolman, 1948, p. 245). Spence quite rightfully objected to this characterization of current neobehaviorist theory. But, polemics aside, the metaphor brings out an important semantic point about the two approaches. Unlike a switchboard, a map is a representation of a perceptually remote geographical
region. It is not a mere link in a causal chain. Tolman believed that psychology would only be successful in explaining behavior if it could discover the representational features of cognitive maps; "...how far these maps are relatively narrow and striplike or relatively broad and comprehensive" (p. 245). The place learning results indicate that rats "develop wider spatial maps which include more than the mere trained-on specific paths" (p. 256).

The notion of place learning was introduced in a series of papers called "Studies in Spatial Learning." The first two were collaborations between Tolman and his students B.F. Ritchie and Donald Kalish, and introduced two experiments intended to demonstrate place learning. Most of the debates centered on the experiment in the second paper, and I will discuss these below. But the first paper included an interesting discussion, explaining why the authors chose such an anthropomorphic term as "expectation" to describe the rats' behavior. The reason is that "the behavior exhibited by our rats is similar in important respects to human symbolic behavior." To illustrate this point the authors discuss a human who "reads, understands, and believes a sign like, 'There is bread in the kitchen', and then expects there to be bread in the kitchen."

In the first place, there is no known simple response which is uniformly associated with an expectation of bread in the kitchen. In fact, when there is no motivation, there is no response at all. However, none of us would wish to assert that because there is no response in such circumstances, there is no expectation. For this reason we must reject any explicit definition of 'expectation' in terms of any single response or set of responses. This is the point which the senior writer has stressed in all his discussions of latent learning. Now let us consider those cases in which the person is motivated and some response occurs. Even now there is no single response or set of responses which is uniformly associated with this expectation. A wide variety of responses may be observed, and all that they seem to have in common is that they all are functions of the relation between the location of the person who has the expectation and the location of the kitchen. ... If...the location of the kitchen is merely recognized as the place which is the terminus of all the paths which have been transversed in the past when seeking bread, then this person would be helpless when either all these old paths are blocked, or he is in a new location. ...[I]f the person is able to solve this problem and pick a new path ... we must suppose that his knowledge of the location of the kitchen is abstracted from the location of any of the paths, and is a function of the kitchen's spatial relation to the total environment. ... The problem we set for our rats demanded the same kind of abstract knowledge of the location of the food. (Tolman et al., 1946a, pp. 22, 23.)

This account is not flawlessly cognitivist, to be sure. The relevant cognitive function is not between the location of person and that of the kitchen, but between the person and the believed location of the kitchen. But the cog-
nitivism is apparent, and operationalism is explicitly rejected. At first this little parable seems to have two points, related but distinct. One point relates to the scientist's difficulty in explicating the concept of "expectation." The concept is such that no simply behavioral definition is possible, even after assuming constancy of motivation. The second point involves a specific claim about the feature of the subject's cognition which is responsible for the impossibility of operational definition of his "expectation." It is that his knowledge of the kitchen's location is abstracted from his past experience of arriving at the kitchen. This is shown by the fact that he is able to arrive at the kitchen by new routes, and from new locations. The subject possesses a meaningful and useful concept of "the kitchen's location" which is not reducible to his past kitchen-experiences.

I suggest that epistemological parity can be seen at work in this passage. There is one point being made, not two. The scientist's ability to talk meaningfully of the subject's "expectations" even in the absence of operational definitions is cognate to the subject's "abstracted" knowledge of the kitchen's location. The subject would be unable to give a complete listing of all the possible paths one might take to arrive at the kitchen (i.e. give an operational definition of the kitchen's location) and his knowledge is certainly not limited to past paths. Similarly the scientist cannot, and should not be expected to, give a comprehensive listing of all behaviors which might arise from a subject's expectation of bread-in-kitchen. This parable was presented simply as a justification for the use of a commonsense, cognitive, intentional expression within experimental psychology. I interpret it as a densely packed summary of the relation between realist epistemology and cognitivist psychology. But it certainly is not an argument for the truth of cognitivism. Like most cognitivists (and to the dismay of stricter behaviorists) the authors simply assume the legitimacy of the intentional idiom "expectation." They then draw the intentionalist implications for epistemology (realism, or at least anti-operationalism) and psychology (cognitivism). The scientist can have knowledge of the complex and epistemologically remote "expectation" of the subject, which knowledge is abstracted from the individual experimental situations providing evidence for such a structure; the psychological subject can have knowledge of the geographically remote goal, which knowledge is abstracted from the specific paths and environmental cues used for access to the goal. The parable's "person" has cognitive abilities shared with rats and psychologists.
Experiments pro and con

The place learning phenomenon shown in “Studies in Spatial Learning II” (Tolman et al., 1946b) should be seen as evidence for the development of a relatively rich cognitive map in a rat which had relatively little training. The experiment used a “plus” shaped maze, with starting points at the ends of two opposite arms (which I will call North and South) and goal boxes at the ends of the perpendicular arms (East and West). (See Figure 1.) Two groups of rats were trained, the “place learners” and the “response learners.” The place learning group was trained with food always in the (say) West goal box. Rats were released from both the North and South starting points, and so had to respond with a left turn from the South start and a right turn from the North start. The response learning group was trained with food always in the goal box to (say) the right of the point at which they had started; they were trained to respond with a right turn from either the North or South starting points. The experiments took place in normal rooms with rich extra-maze environments—home cages, lights, windows—and on elevated mazes which made the environment easily visible. Differences in abilities to learn the two tasks were taken as evidence of the relative “nativeness” or “primitiveness” of the forms of learning involved. The results were dramatic. All eight of the place learning rats reached the training criterion of 10 successive errorless trials, with the mean number of 3.5 trials needed to begin the errorless string. Of the eight response learners, only three reached the criterion after 72 trials over 12 days. In fact all of the unsuccessful response learners had learned

Figure 1. After Tolman, Ritchie and Kalish (1946b).
place—adopted the habit of always approaching the same goal box, thus keeping their performance at chance level. Place learning, it seemed, is more basic or natural to rats than response learning, in conflict with neobehaviorist theory.

Note the semantics of the “place/response” distinction. The response learning rat adjusts its behavior to stimuli from the very immediate environment—the learning exhibited is described in terms of the rat’s conditioned movements at the choice point. Place learning, on the other hand, is described in terms of the perceptually remote place, the geographical location of the rewarding goal box within the environment. Tolman had the place learning rat abstracting a representation of the maze/extra-maze environment from its experiences in running the maze. The place learner is in a cognitive state which intentionally represents the geography of the laboratory, and this is what allows the rat to respond in appropriate ways to varying situations.

Tolman’s critics had no great difficulty in achieving results interpreted as inconsistent with the primacy of place learning. Three methods were responsible for most of the refuting experiments. One was running the place and response learning groups under relevantly different experimental conditions. The second was the manipulation of the maze relative to the extra-maze environment—changing the position of the maze in the room, or the shape of the maze itself. The third was the use of environments with greatly reduced sensory environments. Circular, domeshaped, or symmetrical rectangular curtains enclosed some mazes, with little or no sensory information available to allow orientation relative to the laboratory.

(1) An interesting example of maze manipulation appears in Blodgett et al. (1949). A moveable maze, similar to Tolman’s, was placed in a normally lit laboratory. The maze was shifted between two possible positions on the East/West axis, so that the West goal box while the maze was in the easternmost position occupied the same room location as the East goal box when the maze was in its western location. In effect, the maze was moved the distance of one maze-length from east to west when changing locations. The place learners in this experiment were designated as the group which returned to the same room location whether this location happened to coincide with the West or the East goal box (and whether the rat started from the North or South arm). Another group, designated the “direction learning” group, was trained to go to the West goal box whether the maze was in its eastern or western location. The direction learners learned faster than the place learners. This was taken to show that the group Tolman has designated as “place learners” had actually been learning “direction,” and not “place” at all.
(2) The most lavish example of maze manipulation appears in Hill and Thune (1952). The experiment also features reduced sensory environments and different conditions for place and response learners. Three-arm mazes were used. The response learners were run within a uniformly lit circular curtain, with extra-maze cues eliminated as much as possible. They were run on three maze shapes, with a fourth run on a 180 degree rotation of the first shape. (See Figure 2.) Each of these mazes had a 90 degree right turn from the starting arm to the rewarding goal arm, with the nonrewarding goal arms at various left turn angles. The place learners were run on a different set of maze shapes. For them the curtain was opened at opposite sides, where a light and a buzzer were placed as extra-maze cues. The single stable feature of the maze was the rewarding goal arm, which was stationary at an oblique angle to the light/buzzer axis. The starting arm was placed at four different positions with respect to the goal arm, requiring right and left turns of 90 and

Figure 2. *After Hill and Thune (1952). S = start; F = food; L = light; broken line = curtain.*
135 degrees. The nonrewarding goal arms migrated to three different positions with respect to the rewarding goal arm. The place learner had to make four different turns into the rewarding goal arm, ignoring the nonrewarding goal arm which itself offered four different options. The only cue which could be used to identify the rewarding goal arm was its consistent orientation with respect to the light/buzzer axis. Each rat was given one run on each maze shape per day. Under these conditions the response learners exceeded chance performance on the third day of trials while the place learners remained at chance until the tenth day.

(3) The most underprivileged of the place learners actually had the luxury of a stable maze configuration. Thompson and Thompson (1949) designed an experiment to show that Tolman's place learning results were an artifact of "reactive inhibition," the tendency discovered by Hull for a rat to avoid the same responses on quickly sequenced trials. While ostensibly testing effects of time between allotted maze runs, a drastically impoverished sensory environment was created. A plus-shaped maze was placed symmetrically within a rectangular black curtain. The only available light came from two lamps suspended directly above the two goal boxes. Again the two groups were run under different conditions. The response group was trained with both lamps turned on. The place group was trained with only the lamp above the rewarding goal box turned on. Again, when trials were widely spaced (to dissipate reactive inhibition) response learners were superior to place learners. (Place learners were slightly superior under massed trials).

A cognitivist interpretation

I propose to interpret the place learning results from two perspectives. Let us begin with a cognitive interpretation of the neobehaviorist work on place learning. The cognitivist's primary claim is that the place learning rat acquires a cognitive map of its environment which represents in an abstract way various of the spatial relationships amongst features of the room and maze. We should note that the construction of this, or any other, map requires (1) a stable territory to be mapped, and (2) a relatively rich body of geographical information from which to construct the map. The use of sensory isolation and maze manipulation confounds these prerequisites for map construction. The place learners of Hill and of Thompson were cartographically disadvantaged—only a single light or a light/buzzer combination carried information about the room environment. With such a paucity of data, it sounds odd to

Thompson does note that the rats' home cages were in a temporary room about 10 feet from the curtain.
speak of these rats as “abstracting” from their sensory information at all. They could well have been responding to a single cue. This is most dramatic in the Thompson experiment. It would seem that these “place learners” are *learning place* only in the trivial sense that the experimenter chose to illuminate the same goal box (i.e. the goal box in a given room location) when the rats were started from different points. The rats, after all, had no sensory information by which to orient to anything but the light. One might expect that these rats would have gone to whichever goal box was illuminated—if the experimenters had chosen to light alternate goal boxes, the rats would not have returned to the same goal boxes. This is clearly not the kind of behavior which gives evidence of a cognitive map.

In addition to reducing the sensory environment for the (so called) place learners, Hill and Thompson each used special setups for the response learners. In these setups sensory information was so reduced that the rats could not have learned place by any means short of ESP. These response learners were at a loss to orient to anything but their starting point. (Hill's response learners could not have learned place even *with* ESP—no goal arm remained stationary during a day’s four trials.) While both of Hill's groups were subject to maze manipulation, the manipulation of the place learners’ maze was more severe. The response learners always had a 90 degree right turn available, and no way to tell whether its geographical direction was changing. The place learners had to learn four different turns into the stationary goal arm, seeing each maze only once per day. They were required to abstract the single stable feature of the maze—the constant oblique angle between the rewarding goal arm and the light/buzzer axis. Given such a combination of reduced information and unstable geography, even a human cartographer might feel challenged.

It was noted that Thompson’s rats did not “learn place” in any serious cognitive sense. They had no cognitive access to the geography of their surroundings, and were probably just orienting to the light. There is a similar semantic problem in the Blodgett experiment. Blodgett defines a “place learner” as one which returns to the same *room* location after the change of maze location within the room. The rat which returns to the same *maze* location is termed a “direction learner.” But remember that, according to the cognitive map notion, the rat abstracts the relationships amongst maze and room features. There is no specific prediction made of what a rat will do when the maze geography is changed with respect to the room. To be even

"While the door to this room was kept closed it did not prevent the rats in the home cages from serving as olfactory and auditory extra-maze cues." (p. 885) Given the great care with which all other extra-maze cues were controlled, these modest cues seem more an accident than a conscious attempt to provide a means of orientation, and I will ignore them.
more speculatively cognitivist, one might suppose that the rat in Blodgett’s experiment would find itself entertaining two hypotheses to account for the “continental drift” of the maze. One hypothesis is that the new configuration resulted from a westerly maze movement along the East–West axis. The second is that the maze rotated upon the axis of the (formerly) West goal box. Blodgett gives evidence that rats tend towards the prior hypothesis.

The neobehaviorists were not (yet) trying to show that place learning does not occur, only that it is not particularly natural or primitive to rats. But the comparison was rigged. Cognitive mapping was made difficult for the place learners, by sensory reduction and maze manipulation. It was made impossible for the response learners, by elimination of extra-maze cues. This shows why the “refuting” experiments had very little significance for a fully cognitivist theory. The significance was that (1) cognitive maps are more easily constructed in a geographically stable and sensorily rich environment, and (2) rats will quite readily learn a simple response when their environment provides them with nothing else to learn. These are not surprising discoveries.

A neobehaviorist interpretation

From a cognitivist perspective the neobehaviorist place learning experiments were unimpressive. The experimental setups were so different from Tolman’s that the results scarcely mattered to the nature of place and response learning. But neobehaviorists often referred to these results as “refutations” of the primacy of place learning. Were they intentionally misinterpreting Tolman? If not, what could account for such a lack of agreement on the relevance of experimental results? The answer is—epistemology. The neobehaviorists were not being obtuse. They were adhering to their two primary methodological principles, analysis in simplest behavioral terms, and the requirement for eliminability of theoretical terms. Let us consider the “refuting” experiments with these in mind. One feature of the experiments was the difference in environment between place and response learners, with the response learners in environments devoid of extra-maze cues. But after all, Tolman had claimed to be comparing rats’ abilities to learn response with those to learn place. Why not, consistent with SBU, give the response learning rats a task in which their abilities to learn were not confounded by response-irrelevant factors, such as the “spatial opposition of cues” (Thompson and Thompson 1949, p. 886) with which Tolman had burdened his response learners? A similar point could be made regarding the reduction of extra-maze cues for the place learners. In Tolman’s naturalistic setup, how could we know which of the many environmental cues were being responded to by the place learners?
With Hill's light-and-buzzer, and even more with Thompson's single light, the stimulus for the "place learning" response is quite clear. The sensory reductions in the neobehaviorist experiments were simply controls over variables Tolman had left uncontrolled. Without this sort of satisfaction of SBU, an acceptable operationalization of any place learning phenomenon would be impossible. Even aside from the control (reductions) of sensory cues, the tasks described as "place learning" by the neobehaviorists seem trivial to a cognitivist. Of the three experiments discussed, only Hill's place learners may have acquired something like an abstract, mappish conception of their surroundings (no mean feat in such an unstable environment). Blodgett's "place learners" were expected to ignore the features of the maze and return to a given room location, as if the maze were an irrelevant feature of the environment. Thompson's had only one cue as to food location, and no real information about whether that cue marked the same room location from one trial to the next. This seemingly odd concept of "place learning" can be explained by neobehaviorist commitment to ETT. What (the operationalist asks him/herself) is the scientific meaning of "place learning"? It cannot be "construction of a cognitive map" or "acquisition of an abstract geographical representation"—these mentalistic notions do not specify observable behavior. It is apparently not the acquisition of a simple response to a specifiable stimulus—this would presumably be "response learning." Only one plausible operational specification presents itself; the learning of "place" is the acquisition of a habit to return to a specific room location. All of Tolman's place learners (and none of his successful response learners) had acquired just this habit. The experiments of Blodgett and Hill showed that place learning, i.e. returning to a room location, is not particularly basic or natural to rats. Indeed it seems especially unnatural when the position or shape of the maze is changed with respect to the "place" in the room being learned. While Thompson's "place learning" task may seem trivial to the mentalistically inclined cognitivist, it does satisfy the operational definition of requiring a return to a given room location.

Tolman did not supply suitable operational definitions for such concepts as "place learning." When definitions consistent with ETT were put forth by neobehaviorists, and experiments designed around them, the claims made by Tolman for the primacy of place learning could be seen to be unfounded. Anyone who accepted an ETT account of theoretical terms, together with an SBU specification of observational data, could see that Tolman was wrong.
A “Final Solution”

Most neobehaviorists tried to refute the primacy, but not the existence, of place learning. An exception was H.H. Kendler. Kendler recognized the epistemological nature of the place/response controversy. His well-known “‘What Is Learned’—A Theoretical Blind Alley” (1952) was a purely epistemological attack, aimed at demonstrating that the very notion of place learning involved conceptual confusion. The issue of “what is learned” is a pseudoproblem. Like other strict positivist declarations of pseudoproblemhood, what Kendler declared “pseudo” was not the problem, but the non-positivist solution. Kendler was particularly vexed by Tolman’s suggestions that “places” and “cognitive maps” are learned. Tolman (he says) has forgotten that such theoretical concepts “... are shorthand descriptions, and nothing more ...” and that such concepts “[do] not refer to an object, thing or entity as suggested by those who are concerned with what is learned. These intervening variables possess no meaning over and above their stated relationships between the independent and the dependent variables” (p. 23, emphasis in original). Armed with this epistemology, Kendler declares that “… all learning theories are response theories” (p. 25, emphasis in original). The place/response controversy is a pseudoproblem not just because the issue has been generally misconceived, but because only one answer to the question is logically permissible. Any theory of learning is a response theory—no conceivable behavior could justify attribution of place learning or cognitive map acquisition. The issue is epistemological (or “logical”), not empirical.

In the bread-in-kitchen parable, Tolman had exploited the epistemological analogy between psychologist and subject to justify use of the intentionalist and non-operationalized concept of “expectation.” Kendler appeals to epistemological parity in a similar way, but with dramatically different results. He urges that psychologists reject the scientific importance of “intuitive models” (such as Tolman’s) in favor of operational definition. “After all, psychological theorizing is just another form of behavior” (p. 24). It is admitted that there are idiosyncratic elements to theory construction, but these elements are a problem for the “psychology of creative thinking.” We know that rats learn mazes in different manners, even though the end results are the same. What is important about scientific theorizing is not any supposed mystical qualities of scientific thought (as for example the belief that one is referring to a perceptually remote object when one uses a theoretical term) but, just as for maze learning, the end result. The end result for science is operational definition.

Kendler has established the analogy between the maze learning of a subject and the theorizing of a scientist. Each is engaged in learning. What would be
an appropriate psychological description of this scientist/knower? The psychological theory of the scientist must (like all learning theories) be a response theory. The scientist may employ idiosyncratic behavior in gradually adjusting his/her responses to the stimuli in his/her scientific environment. Such individual quirks are no more epistemologically important than a rat’s idiosyncratic behavior during maze learning. In each case the important issue is the psychologist’s/subject’s adjustment of responses to environmental stimuli. In scientific verbal behavior this adjustment is called “operational definition.” A scientist who offers an operational definition is, in effect, providing a small response-theoretic account of his/her own verbal behavior. He/she will, under the stimulus conditions specified in the definiendum, respond with the verbal item (the theoretical term) which is the definiens of the operationalization. And what of the realist scientist, who declines to give this kind of operational definition? This scientist’s theoretical verbal behavior will not be reliably related to his/her environmental (experimental) stimuli. Such a scientist may be able to generate reliable results, but only after considerable verbal behavior which is irrelevant to the results—somewhat like the rat which insists on a series of superstitious head-bobbings before entering the goal box. Tolman’s mystical daydreams about rats with maps in their heads had no more scientific importance than this.

Kendler’s epistemology supports his substantive theory—operationalism dictates that learning theories are response theories. Kendler’s substantive theory supports his epistemology— theorizing is behavior, and so is subject to response-theoretic analysis. Operationalism is the response-theoretic account of individual bits of scientific verbal behavior. Tolman’s bread-in-kitchen parable shows a similar interpenetration between epistemology and theory. The subject had indefinitely rich, abstract knowledge of how to reach the kitchen, which was mediated by a cognitive map, and which thus could not be reduced to a specified series of responses to stimuli. The scientist’s inability and lack of need to operationalize the subject’s knowledge results from the fact that the scientist shares these rich cognitive abilities with the subject. The subject’s abilities to cognitively map his surroundings is matched by the scientist’s ability to “map” the cognitive insides of the subject. In neither case is an operationalization available, or needed. Scientific realism is the cognitive-map-theoretic account of scientific verbal behavior.

Kendler’s operationalist manifesto inspired a great many responses in the literature. Most attempted to operationally distinguish place from response learning. The single purely epistemological response came from B.F. Ritchie (1953), one of the authors of the original place/response studies.5 Ritchie’s

5Unlike most of the “operationalist” psychologists, Tolman’s collaborators B. F. Ritchie and Donald Kalish actually had formal training in empiricist epistemology, Ritchie under Bertrand Russell and Rudolf Carnap.
paper was a parody of Kendler's, in which Kendler's operationalism was applied to the paradigmatically realistic science of geography. Ritchie discusses disputes about the shape of the earth. It is "shown" that the dispute between "ball theorists" and "disk theorists" is a mere pseudoproblem. Statements about the shape of the earth are to be operationally defined in terms of statements about travelling in certain directions in order to arrive at certain locations from other locations. "'What is the earth's shape' is such a pseudoquestion, and so should not be asked" (Ritchie p. 217). There is no such thing as the shape-of-the-earth, just as there is no such thing as what-is-learned. Each commits the fallacy of reification (Ritchie p. 219; Kendler p. 23). Ritchie's delightful little satire was one of the very few openly epistemological attacks on neobehaviorism to be published prior to 1960. It was too subtle for its time. It was reported in Psychological Abstracts as a straightforward methodological analysis of geography.

**Historical consequences**

On the received interpretation, Tolman was an operationalist who put forth rather sloppily constructed cognitive theories of behavior. He is commonly criticized as failing to follow his own rules of theorizing (his supposedly operationalist "intervening variable"). On the present account, this is a misunderstanding of Tolman's program. Not only was he an epistemological realist, but his realism played a significant role in his experimental work. Tolman's charade was not difficult. There were virtually no epistemological alternatives to operationalism in the psychological literature of the period. The logical positivists had in the 1930s refuted strict operationalism; these ideas were not introduced to psychologists until 1948 (MacCorquodale and Meehl 1948). In 1952 Kendler was anachronistically referring to operationalism as "present day philosophy of science" (p. 20). Aside from Ritchie's parody, there was no challenge to this claim of modernism. In the eyes of many contemporaries there was no alternative to operationalism. One was either an operationalist-theorist (e.g. Spence) or a non-theorist (e.g. Skinner). Tolman was clearly a theorist, but just as clearly an inadequate operationalizer. He must have been a sloppy theorist.

Tolman's theoretical notions were commonly misinterpreted, as might be expected. Many of his ideas carried intentionalist implications, and so seemed mentalistic to neobehaviorists. These were either ignored, or reinterpreted in nonintentionalist ways. The interpretation of "place learning" as "returning to room location" is a good example. The revised version carries no representational implications, and so is operationalizable. Moreover, *thus*
operationalized, the primacy of place learning is easily proven false. Other cases are the reinterpretation of Tolman's "sign-significant" relation as "stimulus-stimulus" (or "S-S") and the rephrasing of his talk about the rat's "expecting food at a location" as the "forming of an expectancy." Tolman had never used the "stimulus-stimulus" of the "expectancy" expressions. These were ways of translating Tolman's intentionalism into forms strongly analogous to Hullian theory. Hull spoke of a habit-strength connecting a stimulus with a response; the sanitized Tolmanian theory spoke of an expectancy strength connecting one stimulus with another. (For more examples of the laundering of Tolman's intentionalism see Amundson, 1983a, pp. 276–279.)

There were salutary effects of the camouflage of Tolmanian realism, and their worth was considerable. Cognitivist criticisms provided serious challenges to S-R learning theory from its beginning. These were met by increasingly elaborate revisions of neobehaviorism. "In its preoccupation of the disproof of the S-R theory, S-S theory [sic] has actually contributed to the development of the position it set out to destroy. In the face of criticism from the cognitive theorists, the S-R theorists were forced to develop new concepts, new ways of looking at old problems, and new experimental approaches." (Kimble, 1961, p. 237) Prior to place learning, these challenges included such issues as the sensory control of the maze, latent learning, and shortcut behavior. S-R theorists were able to respond to each of these challenges, but at theoretical expense. Neobehaviorist theory became increasingly complex and (many thought) ad hoc. By 1960 it was a cumbersome structure. It had not achieved the predictive force, theoretical unity, and operational clarity which had been its stated goals. In a sense it was ripe for the picking, vulnerable to revolutionary challenges. The "cognitive revolution" began at about this time. The prior forty (or so) years of cognitivist challenge did indeed contribute to the development of neobehaviorist S-R theory ... but in a way which led eventually to its demise.

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6The final neobehaviorist word on place/response is generally held to be Restle's (1957) "resolution" of the dispute. Restle argued rather convincingly that the place learning results could be accounted for on a discrimination learning model. This approach was implicit in the earlier "refutations" of place learning which restricted sensory cues for place learners. The newer generation of cognitivists was showing interest in other areas by this time (but see the defense of Tolmanian place learning by Galanter and Shaw, 1954). Even so, the transition from neobehaviorist to cognitive theories of discrimination learning was remarkably smooth (see Leahey, 1980b, p. 330 ff.). Even if Restle was successful in reducing place learning to discrimination learning (a point which I will not now dispute), cognitivism was only temporarily held at bay.
A contemporary case?

Given only the examples discussed above, any claim of the general influence of epistemological parity may seem a hasty induction. Are more illustrations available? In particular, are current illustrations available? I have elsewhere given at least prima facie evidence of several other historical parity influences. Concerning modern cases, I will here make two excuses, and mention one recent case which shows parity influences. The first excuse is that hindsight is more reliable than foresight. A methodological commentary on a currently developing field is far more complex (and risky) than one on a dispute seen from thirty years’ perspective. The second excuse is that parity effects are best observed in deep theoretical disputes, and the two most distinct theoretical schools existing today do not seem to be on speaking terms. The Tolmanians and neobehaviorists carried on continual exchanges both on substantive and methodological issues. Today’s operant behaviorists and cognitive psychologists barely acknowledge each others’ existence. (There was ten years’ latency in the published operant response to Chomsky’s review of Skinner’s Verbal Behavior.) Without open methodological dispute, parity effects tend to remain hidden. In the absence of a genuine operant/cognitive dispute, let us consider the following exchange between two cognitive psychologists, one of whom adopts the behaviorist point of view as a “methodological exercise.”

William F. Brewer gave us a paper with a remarkably perspicuous title: “There is no convincing evidence for classical or operant conditioning in adult humans” (Brewer, 1974). The paper provides a detailed review of a certain set of conditioning experiments on adults, those involving “dissociation designs.” Unlike most conditioning experiments, dissociation designs allow the psychologist to separate the effects of automatic conditioning from the effects of the subject’s conscious awareness of the conditioning contingencies present in the experiment (i.e. the awareness of which stimuli cooccur, or which responses are rewarded). Without the dissociation of these two possible explanations of response, conditioning experiments do not preferentially support conditioning theories over cognitive theories. Brewer describes several dissociation designs, and reports that evidence does not favor condi-

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7I have given evidence for parity effects in several other cases, including Skinnerian theory and two introspective schools (Titchenerian structuralism and the Wurzberg school, in Amundson, 1983b). There I argue primarily that strong epistemology/theory congruences are consistent with a parity influence; I do not show that parity itself was the cause of the congruence. A more detailed examination of a parity effect appears in Amundson (1982), a study of the “ethnoscience” movement in anthropology (also called “cognitive anthropology”).
tioning theories. When subjects are aware of the contingencies of conditioning (and when their awareness of the contingencies is suitably manipulated to distinguish the effects of awareness from the effects of conditioning) behavior seems to be under the control of conscious awareness, and not of conditioning.

Brewer discusses only the "crucial experiments" which will allow one to decide on genuine empirical grounds whether adults are conditionable. But how do dissociation designs separate conditioning from consciousness? Conditioning effects are assessed by the non-verbal contingencies of the experiment. Consciousness effects are assessed by one of a number of methods involving either verbal instructions to the subject, or the subject's ability to verbally report on experimental contingencies. It is presumed that verbal instructions to a subject can manipulate his or her awareness of contingencies, and that the subject's verbal reports are reliable indicators of any awareness which might have been gained. An example of dissociation design is the "informed unpairing" design, which involves simply telling a classically conditioned subject that the CS–UCS relation will no longer obtain. It is reported that of 13 "informed unpairing" studies using galvanic skin response (an autonomic response which would seem ideal for conditioning) 12 studies achieved GSR extinction by informing the subjects that there would be no more shocks (Brewer, p. 8).

These results would seem to be an extraordinary blow to the application of conditioning theory to adult humans. But Dunaly (1974) shows just how easily the results reviewed by Brewer can be dealt with by conditioning theory. His "methodological exercise" (pp. 46–52) shows that the results can be accommodated by means of auxiliary assumptions which are "antecedently plausible" (to a conditioning theorist). The assumptions amount to this: verbal behavior is itself an effect of conditioning. Brewer's dissociation designs are merely those which dissociate verbal from non-verbal behavior. Brewer treats the verbal aspects of behavior as directly reflecting conscious awareness. But if verbal behavior is itself conditioned, then the only impact of the experiments is to show that a subject's verbal behavior is reasonably consistent with his or her non-verbal behavior. For the "informed unpairing" design, the psychologist's verbal instruction serves as a discriminative stimulus to the subject. The subject's lack of response to the CS after verbal instructions shows only that the subject has learned to discriminate according to verbal as well as non-verbal cues. If verbal learning is at all useful, a conditioning theorist would expect it to be integrated with other forms of conditioning; the dissociation designs show that it is.

Brewer's paper could have been given an even more perspicuous title, for example "There is no convincing evidence for operant or classical condition-
ing in adult humans which does not presuppose the conditioning of verbal behavior.” This is by no means a trivial result. In fact Brewer’s paper is primarily addressed to fellow cognitivists who, like Brewer, reject verbal conditioning but suspect that conditioning may be effective on less complex sorts of behavior (see Brewer p. 1). Nonetheless, it is natural for a cognitivist to overestimate the strength of the refutation, as can be seen in the published discussion on the Brewer/Dunaly exchange (Weimer and Palermo, 1974, pp. 57–61).

Kendler’s claim about psychological theorizing was a claim about language; theoretical language (and presumably the language used in experimentation) is “just another form of behavior.” If this is so, and if adult human subjects are on an epistemic par with adult human psychologists, then verbal exchanges between experimenter and subject cannot “dissociate” conscious from conditioning effects. Both Brewer and Tolman presume the scientist’s verbal activity to be something richer and more abstract than a response to stimuli. This assumption sounds as if it might follow from substantive psychological discoveries—and indeed each scientist could cite empirical evidence on their collective behalf. But neither would have a chance were he playing according to Kendler’s (or Skinner’s) methodological rules.

**Methodological consequences**

It was suggested above that epistemology provides an ideological background for substantive psychology. If epistemological parity operates as it seems to, there is a better metaphor: epistemology and psychology stand in a figure/ground relationship. A psychologist can argue for a substantive theory (supported in the argument by background epistemological assumptions) or for epistemological principles (backed by the substantive results of theory). It must be acknowledged that no scientific tradition is free of presuppositions, and any important scientific dispute will involve methodological questions as well as those of empirical observation. But psychology is the only science which exhibits epistemological parity, because only psychology has as its topic the processes of knowledge acquisition. Total objectivity may be a myth in any human endeavor. An objective view of scientific epistemology is just one notch more difficult for the psychologist than for, say, the geologist or physicist.

Historical understanding tends to breed relativism, and relativism may spread to complacency. It has even been suggested that the history of science might best be “rated X” to working scientists (Brush 1974). A naive true-believer may contribute more to science than a sophisticated cynical. But a scien-
tist should be able to understand the historical context within which he or she works without viewing that context as a mere historical accident. To be sure, the historically conscientious cognitivist cannot cite cognitive mapping in refutation of neobehaviorist epistemology, or realist epistemology in refutation of neobehaviorist S-R theory. But even these deep epistemological principles can be accepted or rejected on reasoned grounds. Here are three methodological considerations which are not liable to the problems raised by epistemological parity: (1) Operationalism has been rejected in most other mature sciences, and by most contemporary philosophers; (2) Neobehaviorism (based on operationalist epistemology) is seen to have degenerated by the 1960s, while realist-based cognitivism was (and is to date) seen as progressive—the epistemology inherits the reputation of its associated theory; (3) The computational model demonstrated that it is possible to refer realistically to internal states without wallowing in mysticism and mind/body dualism. So today's realistic cognitive psychology is reasonably secure in its methodological preferences (thanks in part to the pre-1950 cognitive underground). Epistemological parity makes it appear to its practitioners even more secure than it actually is, just as it made rationalist neobehaviorism appear larger than life to an earlier generation.

References


8Gardner (1979) discusses epistemological disputes in nineteenth-century physics which are analyzed along lines similar to those which follow.
9I am grateful for the comments of two anonymous referees, one of whom pointed out the relevance of the Brewer/Dunaly exchange. Research was partially supported by the National Endowment for the Humanities during the summer of 1981.