Behaviorism, Latent Learning, and Cognitive Maps: Needed Revisions in Introductory Psychology Textbooks

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This paper critically assesses the scholarship in introductory psychology textbooks in relation to the topic of latent learning. A review of the treatment of latent learning in 48 introductory psychology textbooks published between 1948 and 2004, with 21 of these texts published since 1999, reveals that the scholarship on the topic of latent learning demonstrated in introductory textbooks warrants improvement. Errors that persist in textbooks include the assertion that the latent learning experiments demonstrate unequivocally that reinforcement was not necessary for learning to occur, that behavioral theories could not account for the results of the latent learning experiments, that B. F. Skinner was an S-R association behaviorist who argued that reinforcement is necessary for learning to occur, and that because behavioral theories (including that of B. F. Skinner) were unable explain the results of the latent learning experiments the cognitive map invoked by Edward Tolman is the only explanation for latent learning. Finally, the validity of the cognitive map is typically accepted without question. Implications of the presence of these errors for students and the discipline are considered. Lastly, remedies are offered to improve the scholarship found in introductory psychology textbooks.

Key words: latent learning, cognitive map, introductory psychology, learning theories

Latent learning is a topic that has received consistent attention in generations of introductory textbooks and is the focus of the present paper. This topic has been included in introductory textbooks since at least Cole (1953). Of the 48 introductory textbooks published between 1948 and 2004 that this writer examined, 36 highlight the topic of latent learning (see the Appendix for a list of textbooks). The topic is discussed in the first introductory textbook written explicitly for high school courses (Blair-Broeker & Ernst, 2003) and in European introductory psychology textbooks (e.g., Bottone,

2001). The present paper, then, has as its general purpose to assess the scholarship in introductory psychology textbooks' treatment of the topic of latent learning.

Introductory textbooks typically include a discussion of latent learning in the chapter devoted to the topic of learning. Following examinations of classical conditioning and operant conditioning (occasionally identified as instrumental learning), textbook authors then introduce latent learning. Latent learning is addressed in the context of the role of perceived limitations to, or advances in, psychology beyond "traditional" behavioral explanations of learning. Cognition in learning is viewed as one of the limitations or advances. Latent learning is said to demonstrate the importance of cognition in learning as well as a limitation of behavioral theory. Specifically, the procedure and results of the experiments of Tolman and Honzik (1930a, 1930b) are presented to show the necessity of cognitive factors for explaining learning (Tolman's cognitive map) and to show that behavioral explanations

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Figure 1. A schematic and an enlarged section of the 14-unit T maze used in the Tolman and Honzik (1930a, 1930b) studies. Figure 1a shows the arrangement of the blinds, numbered 1 through 14. Figure 1b shows an enlarged section of the maze including the dimensions of the stem and a blind alley. The placement of the gates in Figure 1b was the same for each of the 14 units. (Copyright 2003 by the University of California Press. Reprinted with permission.)

are inadequate due to their insistence that reinforcement is necessary for learning to occur. According to textbook authors, the Tolman and Honzik studies show unequivocally that reinforcement is not necessary for learning to take place. Before proceeding to a more detailed examination and assessment of the textbooks, a review of Tolman and Honzik's original research report is in order.

Tolman's Latent Learning Research

Tolman and Honzik took two groups of rats from the 1930a study

and one group from the 1930b study to prepare the data for comparisons that were then published in the 1930b report. The studies employed three groups of food-deprived rats. The researcher placed each rat in the start box of a 14-unit T maze (see Figure 1), and the rat was then left to its own devices to traverse the maze to its end. One group of rats had food available at the end of the maze from the 1st day of the study. A second group of rats never found food at the end of the maze for the entire study. The third group, however, had no

Figure 2. Tolman and Honzik's (1930b) data showing the average number (with a constant multiplier) of blind alleys each day for rats receiving food at the end of the maze from the 1st day of the experiment (hungry rewarded, HR), rats not receiving food at the end of the maze for the entire experiment (hungry nonrewarded, HNR), and rats receiving food beginning on Day 11 and continuing until the end of the experiment (hungry nonrewarded-rewarded, HNR-R). (Copyright 2003 by the University of California Press. Reprinted with permission.)

food at the end of the maze for the first 10 days, but on the 11th day, the experimenter placed food in the end box prior to the rat's entry into the maze. As Tolman and Honzik (1930b) noted, "The reward period [for this third group of rats] extended from the twelfth to the twenty-second day inclusive" (p. 259).

What was of such great interest then, as now, were the results for this third group of rats: on the day following finding food at the end of the maze, their average error rate decreased and their average speed from the start to the end of the maze increased when compared to those averages for the always-fed rats (Figure 2). The drop in errors and increase in speed occurred literally overnight. As textbook authors note,

rats in the third group had, through their wanderings, learned a great deal about the maze without ever getting so much as a morsel for their trouble. Tolman and Honzik (1930b) interpreted these data to mean that learning could take place in the absence of any reinforcement. Their interpretation continues to be viewed as correct in many introductory psychology textbooks.

Latent Learning and Behavioral Theories: A Brief Historical Review

The prevailing behavioral theories throughout the period of experimental research into latent learning were generally stimulus-response (S-R) association theories; two of the leading S-R association proponents were Clark Hull and Edwin Guthrie (Bow-

er & Hilgard, 1981; Viney & King, 2003). Deriving their theories from the classical conditioning principles discovered by Ivan Pavlov, the S-R theorists viewed behavior (the R) to have become associated with some event or aspect of the environment (the S) that was present when the R occurred. Thus, in the future, the presence of the particular stimulus would effectively lead to, or elicit, the particular response, hence the S-R association.

Hull developed an elaborate and systematic account of behavior beginning with the acquisition of reflexive S-R associations, with the response subsequently varying in strength due to variables such as primary motivation or drive, positive and secondary reinforcement, stimulus intensity, and incentive. Hull systematized these inferred mechanisms of learning into mathematical expressions in interrelated postulates and their corollaries (Hull, 1952; Viney & King, 2003).

One of Hull's great strengths was his skill in systematically crafting experimental tests of the numerous hypotheses he developed to verify his system. His investigations allowed him to provide an explanation for maze learning in general (Hull, 1934a, 1934b) as well as the theoretically more challenging latent learning (Hull, 1952, pp. 140–151).

For Hull, what occurred during latent learning was the establishment and modification of S-R associations due to a change in the excitatory, or reaction, potential (sEr) of a "bit of learned behavior" (Hull, 1952, p. 7). Hull viewed the reaction potential as a multiplicative function of the strength of the drive, the intensity of the stimulus, the motivating strength of the incentive, and, finally the habit strength of the particular response. To explain Tolman and Honzik's (1930b) results, Hull analyzed the relations among these variables as the rats traversed the maze with and without food, and then for those rats

that encountered the food on Day 11. Hull demonstrated that what he called the fractional antedating goal reaction combined with the shift in the magnitude of the incentive (i.e., the introduction of food) on Day 11 predicted the results that Tolman and Honzik had reported (see Hull, 1952, pp. 145–150).

Hull (1952) was not the only behaviorist to tackle the thorny question of what occurred during the latent learning experiment. Guthrie, a contemporary of Hull, also built his theory on the S-R association foundation. Yet Guthrie crafted his theory rather differently. Although both Hull and Guthrie were interested in what Guthrie identified as "the examination of the successive changes that constitute learning" (1946, p. 17), he nonetheless focused on what he described as "the central problem of learning, namely, what change occurs in behavior as the result of a single action" (p. 16). Guthrie's unit of investigation was an S-R occurrence and the association to which it gave rise. His position was that the *contiguity* of the stimulus and the response was responsible for the resulting association rather than "a subsequent reinforcement or reward which somehow works upon traces of the S-R event and confirms or destroys the associative connection" (p. 17; cf. Hilgard & Marquis, 1961).

Guthrie, then, viewed the behaviors of rats in the latent learning maze to be "acquired by associative learning ... built up out of associations" (1952, p. 278). In the rat's maze the presence of stimuli contiguous with a response on its first occasion will, when presented subsequently, elicit the same response. A single trial is sufficient for the learning to occur (i.e., for the association to be established at each choice point in the maze). Furthermore, Guthrie thought that the response itself produced a change in the stimulating environment by changing the organ-

ism's orientation to the stimuli (e.g., by entering a cul-de-sac in a maze) or by changing in some way the stimulating environment.

A further distinction between Hull and Guthrie was that Guthrie's penchant for theorizing was not matched by an investment in experimental research comparable to Hull's (Hilgard, 1956). Nonetheless, Guthrie's theorizing retained a prominent position throughout the debate over the explanation for latent learning.

Hull and Guthrie were the most prominent behavioral theorists in the debate, but they were not the only ones to demonstrate the robustness of the behaviorism of the time with regard to latent learning. For example, Meehl and MacCorquodale (1948) interpreted their experimental findings in terms of *reinforcement theory and drive conditioning*. Maltzman (1950) discussed the results of his research into latent learning in terms of "Spence's analysis of stimulus generalization and secondary reinforcement and his formulation of the fractional anticipatory response" (p. 183).

The hotly contested theoretical explanation for the phenomenon observed in the latent learning experiments eventually came to focus on the specific question of the role of reinforcement in learning. On the one side stood the likes of Watson, Tolman, Spence, Guthrie, and Leeper. Their view "denied, in one way or another, the necessity of assuming that goal-attainment or reinforcement is responsible for bringing about the hypothetical changes within the organism that are assumed to underlie the changed or learned behavior" (Spence & Lippitt, 1965, p. 119). On the other side stood Hull, Thorndike, Meehl, and MacCorquodale, who maintained that "learning requires the presence or operation of a motivating state (need, drive, purpose, tension, etc.), and the occurrence of some sort of reinforcing

state of affairs variously described as need satiation, reduction in drive level, tension reduction, pleasure, success, satisfactory after-effect, etc." (p. 119).

The ensuing debate was propelled by the experimental work of the preeminent learning psychologists of the time, and the debate lasted for 30 years. Experimenters and their doctoral students from each side of the debate devised increasingly sophisticated research to answer the theoretical questions that arose from each previous generation of experiments.

Yet notwithstanding the experimental, methodological, and theoretical creativity of many of the key figures in the history of psychology, by the mid-1960s many psychologists considered the matter of latent learning to be dead (Goldstein, Krantz, & Rains, 1965). However, the end of an era of inspired research productivity and sharp theoretical debate had come to an end not because one theory prevailed over all others. Rather, as Thistlethwaite's (1951) review of 30 years of latent learning amply demonstrated, it was because the issues that arose out of the extensive latent learning experimentation remained unsettled, and no resolution was thought to be forthcoming. Thus, what had begun as a lively, empirically based debate over fundamental issues in learning ultimately ended in a stalemate.

Latent Learning in the Introductory Textbook

With some exceptions (e.g., Kendler, 1968; Kimble, Garmezy, & Zigler, 1974; Ruch, 1963; Ruch & Zimbardo, 1971) Tolman and Honzik's (1930b) research has long been incorporated into introductory textbooks along with mention of Tolman's (1948) subsequent theorizing regarding cognitive maps. Where it was included, earlier textbooks occasionally offered a more balanced and

critical view of latent learning research. Cole (1953), for example, concluded as follows:

If we ask "what is the moral of the story of latent learning?" perhaps the main emphasis should be placed upon the fact that the rapid reorganization of skills under the stress of motivation cannot take place without the preceding skills. ... The moral does not seem to be that learning takes place without any reinforcement, since there was reinforcement in the sense that some events regularly followed others even in the preliminary runs before food was brought in. (p. 337)

Dember and Jenkins (1970) ended their discussion of latent learning with the conclusion that "a crucial test between the two approaches [S-R and cognitive] seems doomed to failure by virtue of ambiguities in both models" (p. 369). Lastly, Thompson and DeBold (1971) noted that "When reinforcement theorists were faced with the results of the latent learning studies, they began to expand the notion of drives in order to encompass the findings of the experiment within the S-R effect position" (p. 251).

Nonetheless, by the end of the 1970s and continuing into the present, the typical implicit logic of textbook authors in discussing latent learning has been more one-sided. The argument goes as follows: (a) Because learning occurred; and (b) because there was no reinforcement; (c) the S-R associations of Pavlov's classical conditioning as embodied in the behavioral theories of Hull, Spence, Guthrie, and others could not adequately explain the phenomenon of latent learning; therefore (d) some cognitive factor must be invoked to account for the learning that had occurred. In addition, (e) the cognitive factor that is said to account for this learning is Tolman's (1948) cognitive map. Also, as the behavioral theories of Hull, Spence, and Guthrie receded into mainly historical significance, Skinner became identified as the reigning behaviorist. The logic that had been

applied to the earlier behaviorists was subsequently focused on Skinner alone. Thus for 20 years, textbook authors have argued that because Skinner's learning theory is also based on S-R associations, requires reinforcement for learning to occur, and rejects cognitive, or mental, activity as explanations, latent learning is therefore also beyond the ken of Skinner's behavioral principles. The following quotes will illustrate this logic more clearly.

Bernstein, Roy, Srull, and Wickens (1988) noted that Skinner among other "Pioneers in the study of conditioning hoped to explain all learning by the principle of reinforcement and the automatic, unthinking formation of simple associations" (p. 271).

Coon (2004) includes his discussion of latent learning in a section headed "Cognitive Learning—Beyond Conditioning" (p. 228). He begins by asking, rhetorically, "*Is all learning just a connection between stimuli and responses?*" and follows with the answer: "Some learning can be thought of this way. But, ... even basic conditioning has 'mental' elements" (p. 228). Coon goes on to point out that "Cognitive learning is ... revealed by latent (hidden) learning ... [that] occurs without obvious reinforcement and remains hidden until reinforcement is provided" (p. 228).

Gazzaniga and Heatherton (2003) begin their section "Biology and Cognition Influence Operant Conditioning" with the assertion, "Behaviorists such as B. F. Skinner believed that all behavior could be explained by straightforward conditioning principles. ... In reality, however, there are limits to explaining human behavior through schedules of reinforcement" (p. 179). Subsequently, Gazzaniga and Heatherton (2003) cite Tolman to "challenge ... the idea that reinforcement is responsible for all behavior" (p. 180). Noting that "Tolman and his students studied

three groups of rats whose task was to travel through a maze to a 'goal box' containing the reinforcer, usually food" (p. 180), Gazzaniga and Heatherton go on to state that the results of this study imply "that the rats had learned a cognitive map of the maze and could use it when reinforcement became available" (p. 181).

Gleitman (1981) attempted to evaluate the robustness of behavior theory in chapter 5 of his textbook. He summed up the examples from chapter 4 by noting, "Here learning seems to be just as blind and mechanical as behavior theorists hold it to be. But there are other instances of learning whose essence seems very different, as in the cases of a student who 'gets the point' of a mathematical demonstration or of a garage mechanic who figures out why an engine stalls. These examples represent manifestations of reason or intelligence of a kind that ... Skinner largely ignored" (p. 132).

Similarly, Gray (2002) wrote under the heading "Tolman's Evidence that Place Learning Does Not Require Reward" that "Tolman used the term *latent learning* to refer to the learning that is not immediately demonstrated in the animal's behavior. In [Tolman and Honzik's] experiment ... the rats in group 3 learned the maze in the first 10 trials, but that learning remained *latent*, not manifested in their behavior, until the addition of the reward in the goal box gave the rats a reason to use their cognitive maps to run to that location" (p. 127).

Huffman, Vernoy, and Williams (1987) offered a generalized characterization of "behaviorists" as follows: "Researchers ... interested only in observable responses and tend[ing] to describe thinking as a type of response process that is no different from other response processes, such as pushing a lever with the paw" (p. 216). In their subsequent discussion of latent learning, Huffman et al.

assert that "Most learning theorists, certainly the behaviorists, hold that in order for learning to occur, a response must be reinforced" (p. 217). Because the behavior of the experimental rats of Tolman and Honzik had presumably not been reinforced prior to the 11th trial, "information in their cognitive maps remained latent until they discovered the food at the end of the maze" (p. 218).

Kassin (2004) begins his discussion of the cognitive perspective by noting that "Up to the day he died, Skinner (1990) steadfastly refused to speculate about internal mental processes," whereas "most psychologists now believe that it is important to understand internal cognitive processes—not only in humans but in animals as well" (p. 204). Kassin then cites Tolman's position that "animals in their natural habitat learn more than just a series of stimulus-response connections. They also acquire a 'cognitive map,' which is a mental spatial model of the layout—and they do so regardless of whether their explorations are reinforced" (p. 204). Finally, Kassin presents the "Tolman and Honzik (1930) ... classic experiment" as the evidence to support the conclusion "that animals learn from experience—with or without reinforcement" (p. 204).

Kosslyn and Rosenberg (2001) begin their discussion of cognitive learning by noting that both classical conditioning and operant conditioning "involve the storing of new information, which guides the behavior" (p. 190). They then go on to illustrate "the fact that cognitive learning is more than simply associations between stimuli and responses." Unfortunately, their attempt to illustrate this point conflates Tolman and Honzik's research into insight in rats with their latent learning experiments (see Ciancia, 1991, for a critical assessment of the insight studies). Nonetheless, Kosslyn and Rosenberg conclude that the results of the latent learning studies led Tolman to reason

“that unreinforced rats, in their wanderings around the maze, had developed a *cognitive map* of the maze, storing information about its spatial layout. However, they did not use the map until they were motivated to do so by the reinforcement” (p. 190).

In responding to the question “What is learned in conditioning?” Zimbardo (1988) began by noting that

Until recently, it was assumed that an individual played a passive role in conditioning, with associations formed and strengthened automatically by the reinforced pairing of stimulus events and behavior manipulated predictably by environmental events. What was learned was assumed to be fixed associations and specific responses. (p. 294)

It was Tolman, according to Zimbardo, who revealed “the importance of cognitive processes in stimulus-response learning” by placing rats in experimental situations “in which mechanical, one-to-one associations between specific stimuli and responses could not explain the behavior that was observed” (p. 295).

I will end this sample of illustrative treatments of latent learning by citing Tavris and Wade (2001). The paragraph that concludes their discussion of latent learning neatly summarizes the essential positions of the introductory psychology textbooks examined for the present article. Tavris and Wade write,

Latent learning poses problems for behavioral theories. Not only does it occur in the absence of any obvious reinforcer, but it also raises questions about what, exactly, is learned during learning. In the Tolman and Honzik study, the rats that were not given food until the eleventh day had no reason to run toward the end during their first ten days in the maze. Yet clearly they had learned *something*. Tolman (1948) argued that this “something” was information, in this case a mental representation of the spatial layout of the environment. (p. 221)

It is worth noting that although, in the words of Tavris and Wade, the rats “had no reason to run,” in fact

run they did, and with a resulting decline in errors.

An Evaluation of Introductory Textbooks

Introductory psychology authors across generations of introductory psychology textbooks argue that the latent learning research of Tolman and Honzik (1930b) demonstrates unequivocally that learning can take place in the absence of reinforcement. Behavioral theories are therefore unable to account for the results of the latent learning experiments because, as Bernstein et al. (1988) assert, “Pioneers in the study of conditioning hoped to explain all learning by the principle of reinforcement and the automatic, unthinking formation of simple associations” (p. 271). Yet introductory textbooks omit any discussion of the sharp debates that focused on explaining the latent learning phenomenon in general and on the role of reinforcement in learning in particular. The absence of any discussion of such an important historical debate results in textbook authors erroneously concluding that the behaviorists of the time (e.g., Hull and Guthrie) were unable to account for latent learning. On the contrary, the present examination of the writings of Hull and Guthrie demonstrate that S-R behavioral theory was quite adept at explaining this phenomenon. The experimental work of Hull along with his later collaborator Spence repeatedly offered experimentally derived and theoretically coherent solutions to questions raised by the latent learning research.

In addition, textbooks too often seriously mischaracterize the final result of the debate over the role of reinforcement in learning. Textbook authors claim a resolution for the debate that did not in fact occur, namely, that reinforcement was not necessary for learning to occur. Bernstein, Clarke-Stewart, Penner, Roy,

and Wickens (2000) note that the results of Tolman and Honzik's research demonstrate that "learning is more than the sum of reinforcement effects and stimulus-response associations" (p. 200), and Coon (2004) concludes that "*Latent learning* occurs without obvious reinforcement and remains hidden until reinforcement is provided" (p. 228). Gazziniga and Heatherton (2003) write that latent learning is a "form of learning that takes place in the absence of reinforcement" (p. 181), and Kassir (2004) completes his discussion of latent learning by noting that "Tolman was able to demonstrate that animals learn from experience—with or without reinforcement" (p. 204).

Yet what one finds in the history of the debate is quite the contrary: There was no resolution at all. The debate was not over because one theoretical position demonstrated itself to be scientifically superior to the other. The debate ended because it became clear to psychological researchers and theorists that given the hypothetical nature of the variables thought to be involved in latent learning, no empirical solution was likely.

The missing history and erroneous conclusion noted above were compounded when the learning theories of Hull, Spence, Guthrie, and others were consigned to the history textbooks while Tolman was being declared the winner of the debate. As Hull and Guthrie were retired from their prominence as behaviorists, Skinner assumed their former position as the behaviorist-of-record in introductory textbooks. Like Hull and Guthrie, Skinner's behavioral theory is said to be an example of S-R associationism. Like the earlier forms of S-R association, Skinner's behaviorism is said to require reinforcement for all learning to take place.

This assertion is wrong on both counts, however. First, behaviorist

though he was, the principles of behavior discovered and elaborated by Skinner, his colleagues, and students are fundamentally different from those of Hull, Thorndike, Guthrie, and other psychologists who breathed so much life into the debates over the role of reinforcement in learning. Indeed, Skinner (1950) clarified these distinctions at the height of that debate in a paper that has continuing relevance for students of experimental methodology (and equal relevance for the authors of introductory psychology textbooks).

Skinner's (1950) focus was not on an "association" inferred from some apparent stimulus-response connection. That is, one did not learn associations that were subsequently manifested in behavior. Rather, Skinner identified his subject matter as behavior itself, behavior to which he gave the term *operant*. The methodology that Skinner developed to investigate operant behavior empirically, now referred to as behavior analysis, was designed to discover the observable environmental variables that could be shown to be the determinants of behavior (Hilgard, 1956; Pierce & Cheney, 2004).

Skinner (1950) also never claimed that reinforcement was necessary for learning to occur. As the previous historical review makes clear, Hull, among other learning theorists, did indeed make such a claim. Hull's conclusion was based largely on experiments conducted to investigate the hypothetical variables that were part of his elaborate and systematic theoretical account of learning. Again, however, Hull's theoretical principles and experimental methodology were different from Skinner's.

Latent Learning and Behavior Analysis

The claim that behavioral theory was unable to explain latent learning is contradicted by an examination of the history of the experimental research and the resulting theoretical

debates. However, the question of the explanatory power of Skinner's (1950) theory relative to latent learning remains open, because there is no published research on latent learning conducted by behavior analysts. This is so in part because Skinner and his like-minded colleagues believed that the maze apparatus was not particularly useful for investigating basic learning processes (Sidman, 1960; Skinner, 1953). Another likely factor is the view that the focus on latent learning had proven fruitless. As Catania (1992) noted in his textbook on learning, "the language of latent learning seems to have led to a blind alley" (p. 86). Furthermore, this writer has as yet found no published systematic conceptual analyses of latent learning in terms of behavior-analytic principles. What follows is a modest attempt to remedy that situation.

Behavior analysis looks to likely variables in the species history (phylogenetic variables) and current behavior-environment contingencies (ontogenetic variables) of which changes in response frequencies might be shown to be a function. In addition, learning is taken to be a change in behavior rather than in correlated neurological changes (see, e.g., Hernandez, Sadeghian, & Kelley, 2002; Iacoboni, Woods, & Mazziota, 1996; Karni et al., 1998; Kennedy, Caruso, & Thompson, 2001) or a change in some hypothetical psychological event such as a drive, need, or habit. Ironically, it is to Tolman's own writings that we can turn to discover plausible phylogenetic and ontogenetic variables as they might relate to latent learning. As will be seen, phylogenetic variables are implicated in exploratory behaviors, and ontogenetic variables are part of the maze environment itself.

Looking once more at what the data in Figures 2 and 3 show for all three groups of rats, over the first 5 days of the Tolman and Honzik (1930b) study, we can see that learn-

ing is already taking place. The data show that there is an observable decrease in the frequency of one kind of a response (i.e., entering into a blind alley). For the next 6 days, the observed frequency continues to decline for only one of those groups, the group of rats that had from the beginning of the study found food at the end of its run. The observed frequency then declines sharply for a second group of rats, but only after they too have found food at the end of the run on the 11th day. The remaining rats in the control group show no further decline in response frequencies, although a reasonable prediction (confirmed by Blodgett, 1929) is that were they to find food at the end of their runs, they too would quickly make fewer entries into blind alleys. The issue, of course, is what has produced these changes in response frequencies.

Phylogenetic variables. Skinner (1969, 1975, 1989) long recognized the importance of the evolutionary history of a species for the development of behavior. He proposed that recurring behavior-environment relations across generations of a species could modify both behavior and corresponding genes. Such modifications would ensure the functionality of environmental events in relation to survival in successive generations of the species. Skinner analyzed such behavior in terms of phylogenetic contingencies or variables (see also Pear, 2001, chap. 16).

With regard to phylogenetic variables, Tolman (1925/1951) described the behavior of rats in mazes. He noted the "evidently *objective* descriptions of the innate nature, extent, and flexibility of the animals' initial exploratory tendencies" (p. 42) and that "The animal that persists at the same door over and over again obviously has an inferior set of exploratory tendencies ... to the one that tries other doors in between" (p. 42). In that same paper, Tolman described the "very pronounced ten-

Figure 3. Tolman and Honzik's (1930b) data showing the average number of seconds (with a constant multiplier) to complete the maze each day for rats receiving food at the end of the maze every day of the experiment (HR), rats not receiving food at the end of the maze for the entire experiment (HNR), and rats receiving food at the end of the maze beginning on Day 11 and every day thereafter through the end of the experiment (HNR-R). (Copyright 2003 by the University of California Press. Reprinted with permission.)

dency toward continuous and regular alternation—left, right, left, right, or right, left, right, left ... [indicating] that trial and error does not result merely because of failure, but that it is a positive tendency in and of itself" (p. 43; cf. Renner & Pierre, 1998). Finally, Tolman makes note of the "orientation" of the rat in the maze's alleys: "An opening straight ahead, for example, was entered much more frequently than one to the side, and on coming out of a blind, retracings were less frequent than goings-on" (p. 43).

None of what Tolman describes here is foreign to a behavior analyst. One would predict that the rat, rather than sitting stock still, would move out into the maze. Why? For two reasons. First, a sitting rat is a "sitting duck"—for predators. Second,

sitting still usually will not bring a food-deprived rat into contact with food. Behavioral variability is also a characteristic that has survival value in addition to being affected by ontogenic contingencies (Elsmore & McBride, 1994; Neuringer, 2002, 2004; Uster, Bättig, & Nägeli, 1976).

Finally, as noted above, Tolman states "and on coming out of a blind, retracings were less frequent than goings-on." Again, this is consistent with a view of the impact of phylogenetic history on rats: Food had not been found up to this point, so retracing is not likely to be strengthened over the species history of rats (Olton & Schlosberg, 1978).

Ontogenic variables. The variables described above have only gotten the rats in motion. When placed into the start box of the maze and the door

opens, the rat is going to move, alternate left-right or right-left, move straight on to the end of the alleys, and head for new territory rather than give old territory a second look. But we still have to deal with what appears ultimately to be a continuing change in response strength beyond that which occurred during the first 5 days of the experiment. This change occurs apparently despite the absence of food until the 11th day. That is, while the rats receiving food day in and day out from the 1st day enter fewer and fewer blind alleys with each passing trial, after an initial decline in errors the other rats seem to keep stumbling into the same old places. But then, when a subset of the unfed rats finally encounters food in the end box, it is as if there were no longer any blind alleys in the maze. On average, on the very next trial these newly fed rats surpass the performance of the rats fed from the 1st day.

Yet here we can find the intersection of two elements by which behavior-analytic principles can make sense of the data without appealing to the hypothetical and unobservable cognitive map. One of these elements is from Tolman himself and describes the environmental contingencies found in the maze. The other element is to be found in behavior-analytic principles, specifically, the process of stimulus control.

Contingencies in the maze environment. Tolman had made note of the fact that blind alleys in and of themselves affected behavior; that is, blind alleys, once experienced, were less likely to be reentered. In terms of basic behavioral processes, this looks much like a punishment contingency or perhaps extinction; that is, a response-environmental consequence relation that decreases the future probability of that response.

Entering into a blind alley describes a contingent relation: a turn left (or right) at the T leads to the termination of movement. If, when

the rat reaches that choice point on future occasions and is observed to turn in the direction opposite the blind alley with increasing frequency, one can reasonably conclude the effectiveness of the contingency between turning in a particular direction and the termination of forward movement.

Stimulus control. That the success of the rats can be attributed to variables within the behavior-analytic framework is also supported by investigations into the role of stimulus control in relation to effective responding in complex spatial environments.

Stimulus control refers to the effect of an antecedent stimulus on the probability of behavior that has in the past been reinforced in the presence of that antecedent stimulus. A number of studies have shown that the stimulus control exerted by visual stimuli is important for the acquisition and maintenance of competent maze responding by rats as well as for finding hidden objects in computer-generated virtual environments or natural environments by pigeons and humans. Research provides substantial evidence for the importance of "extra-maze" cues for such responding developmentally (Rudy, Stadler-Morris, & Albert, 1987) and for adults. Zoladek and Roberts (1978), for example, found "that rats were primarily dependent upon visual cues" (p. 80) in avoiding previously entered alleys on an eight-arm elevated radial maze. Similarly, Suzuki, Augerinos, and Black (1980) employed an eight-arm elevated radial maze to investigate the control exerted by visual stimuli when locating an arm leading to food. Suzuki et al. concluded that "the present experiments together with Olton and his associates ... indicate the priority of extra-maze cues over other kinds of cues in guiding radial maze performance" (p. 16).

Although the foregoing research suggests that the stimulus control

exerted by visual stimuli predominates, other sensory systems can also contribute to successful movement through a complex space. Zoladek and Roberts (1978) noted that rats in their study also responded to kinesthetic or vestibular stimulation in the development of successful maze running. Beritoff (1965) reported a similar reliance on vestibular cues by blindfolded dogs in solving spatial problems.

Recent research offers further support for the role of stimulus control in responding to spatial stimuli. A series of studies by Spetch and Cheng, along with their colleagues, demonstrated the nature of stimulus control for both pigeons and human participants (Cheng & Spetch, 1995; Spetch, Cheng, & MacDonald, 1996; Spetch et al., 1997). Whether the experiment involved pigeons or humans responding to virtual landmarks or behaving in three-dimensional space, results suggested that the determining relation was between antecedent visual stimuli and the responses of the pigeons and humans.

In addition, data from a number of studies (Jacobs, Thomas, Laurance, & Nadel, 1998; Richardson, Montello, & Hegarty, 1999; Waller, Loomis, Golledge, & Beall, 2000) suggest that people come to respond differentially to differing types of visual spatial stimuli. Looking over a map of a spatial environment, viewing a computer-generated version of that environment while moving through that environment via the keyboard, and navigating the actual environment appear to evoke differences in previously acquired responding to visual stimuli involved in distance estimations and locations of objects in an environment. Differences in the strength of the stimulus control exerted by the different discriminative stimuli (map, virtual space, actual space) offer the possibility of explaining these differences more directly than a hypothesized cognitive map.

The role of food. Stimulus control can reasonably be considered an important element in explaining the behavior of the rats in Tolman and Honzik's mazes, but there remains another variable to which Tolman urges us to attend. Tolman (1925/1951) wrote, "It must be emphasized that these object adjustments to the maze structure have meaning only with reference to the task of *getting to the food and getting there as quickly, both spatially and temporally, as possible*" (p. 44). As Tolman realized, the characteristics of the maze necessarily affected behavior, making some behavior more likely and other less likely. One can reasonably consider this relation between the maze characteristic and the performance to be ontogenic in nature. However, the fact that changes in the strength of some responses are not expressed until some other variable is introduced is not a reason then to attribute that expression to some unobservable variable (the cognitive map) when an observable variable will do just as well. The variable that Tolman identified is that of making food available to the deprived rat. And the function served by the food at the end of the maze is to demonstrate what effect the contingencies in the maze described above have on behavior in the maze.

An effect on behavior of the contingencies in the maze without food is evident from the Tolman and Honzik (1930b) data. The average error rate for all three groups of rats declined over the first 5 days. Because two groups of rats did not have food available at the end of the maze, the decline cannot be attributed to food. The fact that a comparable decline occurred for the group that received food leads to the conclusion that it is not necessarily the food that produced the initial "learning." That is, if food were the only determining variable, there would be either (a) no decline in the error rate in the unfed

Figure 4. Tolman and Honzik's (1930b) data showing the average number (with a constant multiplier) of blind alleys each day for rats receiving food at the end of the maze from the first day of the experiment (HR), rats not receiving food at the end of the maze for the entire experiment (HNR), and rats receiving food from the beginning of the experiment until Day 11 and then not receiving food at the end of the maze for the remainder of the experiment (hungry rewarded-nonrewarded, HR-NR). (Copyright 2003 by the University of California Press. Reprinted with permission.)

rats or (b) a much greater decline in the error rate in the fed rats over those first 5 days than was observed in the unfed rats.

The conclusion that the food served to demonstrate the functional effect of the maze contingencies on the rats' behavior is given further support by the Tolman and Honzik (1930b) study. Figure 4 includes error rates for rats that were fed from the beginning of the study but that then no longer found food at the end of the maze. As would be expected in the absence of food, error rates increased to match the level of the unfed rats. Alternatively, one might attempt to make a case for "latent forgetting."

In sum, the claim that behavioral principles suggested by Skinner cannot in principle explain the data from

Tolman and Honzik (1930b) on latent learning is clearly a mistaken one. Their findings are consistent with well-established behavior-analytic principles including punishment, stimulus control, and the importance of environmental contingencies, both phylogenetic and ontogenic.

The Empirical Status of the Cognitive Map

The main focus of the present paper is to advance the scholarship in introductory psychology textbooks in relation to claims regarding behavioral theories and latent learning, but the prevailing reliance on the cognitive map as an account warrants some examination as well. Thus, I will briefly examine issues related to its definition and the debate over its

current status as an explanatory variable.

Definitions of the cognitive map. Tolman (1948) conceived of the cognitive map explanation for behavior in spatial environments (i.e., mazes) as an alternative to the then-popular S-R theories of Hull and Guthrie, among a number of other distinguished learning psychologists. Tolman identified himself with a group he referred to as "field theorists" to distinguish themselves from the "telephone switchboard school" (p. 192) of Hull, Guthrie, and others. According to Tolman,

In the course of learning something like a field map of the environment gets established in the rat's brain. We agree with the other school that the rat in running a maze is exposed to stimuli and is finally led as a result of these stimuli to the responses which actually occur. ... The incoming impulses are usually worked over and elaborated in the central control room into a tentative, cognitive-like map of the environment. And it is this tentative map, indicating routes and paths and environmental relationships, which finally determines what responses, if any, the animal will finally release. (p. 192)

Subsequent researchers have theorized that the part of the brain in which the "field map of the environment gets established" is the hippocampus (O'Keefe & Nadel, 1978). Other research, however, offers experimental results that are inconsistent with some important assumptions of the O'Keefe and Nadel theory (Ellen, 1980).

There are a number of other definitions of the cognitive map in addition to those of Tolman (1948) and O'Keefe and Nadel (1978). Bennett (1996) has recently reviewed their various and often contradictory elements, including Thinus-Blanc's (1988) "allocentrically organized representation of environmental features," Gallistel's (1989) "record in the central nervous system of macroscopic geometric relations among surfaces in the environment used to plan movements through the envi-

ronment," and that of Downs and Stea (1973): "a process composed of a series of psychological transformations by which an individual acquires, codes, stores, recalls, and decodes information about the relative locations and attributes of phenomena in his everyday spatial environment" (all quotes from Bennett, pp. 220, 222).

Thus it appears that the intuitive appeal of a relatively simple metaphor has expanded to the search for simple or complex neural substrates, geometric relations (innate or acquired?), and the combining of metaphors (i.e., the map and the computer processor). Nonetheless, the viewpoint that has remained constant in the introductory psychology textbook is that of Tolman (1948).

Textbook authors typically relate the cognitive map explanation to the Tolman and Honzik (1930b) results. A look at introductory textbooks written by 10 different authors and published from 2000 to 2003 reveals that seven of those textbooks refer only to the Tolman and Honzik study when discussing the cognitive map. Yet the concept of the cognitive map actually did not surface until nearly two decades later (Tolman, 1948; Tolman, Ritchie, & Kalish, 1946). More significantly, the concept of the cognitive map was based on research that used an apparatus qualitatively different from that used in the Tolman and Honzik studies. The earlier studies utilized the T maze, but the cognitive map concept was derived from research that had rats running in a sunburst maze (Figure 5).

However, even when textbooks rely on Tolman's later research as the reference point for the cognitive map explanation, as did three of the authors whose textbooks I examined, a problem remains. In a review entitled "Mazes, Maps, and Memory," Olton (1979) first noted that the earlier experiments using T mazes had focused on behavioral stereoty-

Figure 5. A diagram of the sunburst maze used by Tolman, Ritchie, and Kalish (1946) to test for rats' "expectations." The results of this test served as an experimental foundation for the cognitive map. (Copyright 2003 by the American Psychological Association. Reprinted with permission.)

py. Later experiments focused on behavioral flexibility and variability through the use of sunburst, radial arm, and hexagonal mazes. It is this later research that gave rise to the cognitive map as an explanation. Olton points out, however, that "Although [Tolman et al., 1946] is often cited as a classic in support of cognitive mapping abilities in rats, such a choice is unfortunate. ... The results could not be replicated" (p. 589).

Thus, there are two concerns here. On the one hand, a close connection between a significant explanatory concept and the research method that gives rise to the concept is vitally important, especially to the introductory student. And on the other, if the cognitive map is going to be given

such great weight as the lynch-pin for the cognitive explanation, research supporting the concept that can be and has been replicated is a necessary foundation.

Current debate on the validity of the cognitive map. The history of the latent learning experiments indicates, first of all, that researchers were unable to provide the unequivocal results for an explanation for the latent learning phenomenon. Nonetheless, introductory psychology textbooks offer their readers an opposite conclusion.

At the same time, subsequent research has continued to promote skepticism regarding the explanatory robustness of the cognitive map. The results of some experiments have led researchers to conclude that a simpler

principle, such as adaptation, is sufficient (Haraway, Bailey, & Maples, 1971; Haraway, Grimmett, & Maples, 1977).

In addition, two important assumptions that underlie the "map in the head," one having to do with the integrated nature of the map and the other with reversibility of routes, have not been supported by experimental research. On the one hand, rather than existing as an integrated whole, Kuipers (1982) concluded that "spatial knowledge can fall into disconnected components, with little or no relation between the components." Furthermore, routes appear to be represented asymmetrically, and thus movement along the routes is irreversible.

Research with humans also calls into question the cognitive map as a basis for movement through complex spaces. Moeser (1988) has shown, for example, that even after 2 years of exposure to a complex space (a five-story hospital), what gets people from one place to another is apparently something other than a cognitive map that is somehow consulted. One would predict that for a cognitive map to function effectively it would be readily retrievable for consulting and would be an accurate map. Yet when adult subjects were asked to draw a map of four of the five floors of the hospital complex, none of the maps resembled the floor plans supplied by hospital administrators.

At the present time, accumulating research results suggest that there are explanations for the behavior of animals and humans in spatial environments that involve simpler processes than reading a cognitive map. As a result, some psychologists, cognitive neuroscientists, and biologists have concluded that the cognitive map is no longer a viable explanation for the successful navigation of animals and humans through spatially complex environments.

Benhamou (1996), for example, found that predictions from cognitive mapping theory of rats' navigation in a water maze were not supported. Benhamou's maze was designed of three elements. The first was a circular swimming pool, constructed of clear Plexiglas. The second element was an opaque-walled metallic cylinder with an opening on one side that was placed within the larger pool. Its diameter was less than that of the pool and, when placed in the pool, left a channel between the pool wall and the outside of the cylinder wall. The opening of the cylinder was reoriented from one trial to the next to prevent the opening itself from controlling the rat's responding. The third element was the goal, a small white cylinder located in the inner pool and slightly submerged beneath the water's surface. On each trial, the rat was released into the channel between the pool wall and the outer wall of the smaller cylinder at a location opposite the cylinder's opening into the water inside.

Results from the first two experiments showed that following the training trials with this apparatus, the rat's search behavior on the final test trial was random. The third experiment examined whether landmarks present during both the training and the test trials might be related to greater success in reaching the goal. The results of this third experiment led Benhamou (1996) to note that "the rats ... [although] not highly efficient during the test, searched for the platform in the vicinity of the correct location" (p. 209). That is, the presence of the same landmarks during both the training and test trials did indeed result in better performance during testing. In sum, the results of the critical tests led Benhamou to conclude, "rats are unable to rely on a true cognitive map (survey map) to resolve the navigational task" (p. 209).

A more recent study by D. M. Skinner et al. (2003) investigated rats' responding in both open field and T mazes. Skinner et al. examined the rats' success at finding and then consuming food hidden in a well. One group of rats had first to turn left (or right) before completing the search for the food. This response requirement was independent of the position of the maze in the room. A second group of rats went toward the experimental room's east or west wall, the direction also independent of the maze's position. For the third group of rats, the same location in the experimental room, independent of the maze position, determined the location of the hidden food. The three groups of rats were thus identified as "response," "direction," and "place" groups, respectively.

D. M. Skinner et al. (2003) found that with the open field and T mazes, place learning was difficult compared either (a) to the learning based on the right or left response requirement or (b) to the direction of travel to the food. Their interpretation of these findings centered on the importance of distinctive start points ("start point discriminations," p. 11) rather than a cognitive map. Indeed, Skinner et al. concluded by noting that "Blodgett et al.'s (1949) data and the present replication suggest that direction and response are much more readily used by the rat than place. The results of experiments with many species do not support the contention that animals are able to generate [and read] a map-like overview of their environment" (p. 12).

There is also the matter of the actual utilization of such a cognitive map. An examination of how humans actually behave with respect to a physical map reveals that a map evokes quite complex behavior such as talking and alternating looks at the map and searching the environment for landmarks, street signs, and other visual cues (Brown & Laurier, 2004). If one assumes that rats are engaging

in similar behaviors when consulting a cognitive map, one then is in the position of having to discover the conditions under which rats would have acquired such a complex repertoire. If, on the other hand, rats do not engage in such behavior (excluding talking), what is the means by which the cognitive map is consulted, read, compared to the spatial characteristics of the maze itself, and so on (Wittgenstein, 1958, Remark 653)?

Finally, Bennett (1996) reviewed experimental research investigating navigation to a goal and a number of researchers' differing definitions of a cognitive map. The experiments reviewed had included participants as disparate as desert ants, honeybees, rats, chimps, and humans. Bennett summarized his review as follows:

Owing to the repeated inability of experimenters to eliminate ... simpler explanations for at least 15 years, and the confusion caused by the numerous contradictory definitions of a cognitive map, I argue that the cognitive map is no longer a useful hypothesis for elucidating the spatial behaviour of animals and that use of the term should be avoided. (p. 219)

Thus, although the cognitive map metaphor might retain its appeal, a close examination of the metaphor as a scientific explanation for human and animal navigation through spatial environments exposes substantial weaknesses.

Conclusion

Many introductory psychology textbooks misinform students regarding the capabilities of behavioral theories for an explanation of latent learning, a psychological phenomenon that has been of importance to textbook authors for more than 40 years. The misinformation has extended across the S-R theories of Hull and Guthrie and at present targets the behavior-analytic principles of Skinner. For at least 25 of those years, introductory psychology

textbooks have told their readers, without further examination, that Skinner's behavior-analytic principles cannot explain this phenomenon.

Furthermore, many textbook authors ignore completely the history of the experimentation and corresponding theoretical debate that surrounded latent learning, as well as the demise of the theoretical debate due to the inability of experimentation to resolve the issues. In addition, these textbooks have, and continue to, misinform readers as to the methodology that gave rise to the accepted explanation for latent learning.

The introductory student who becomes a psychology major is often misinformed. This effect has implications for the disciplines of psychology and behavior analysis. The presence of the misinformation described above predicts that the attention of future researchers in psychology will be directed away from behavior analysis, a view supported by the alarming data of DeBell and Harless (1992), who found that compared to first-quarter freshmen, undergraduate and graduate psychology students were more likely to judge as true statements about Skinner's radical behaviorism that were in fact false.

There are likely to be new principles of behavior waiting to be discovered. Such discoveries are likely to be made sooner rather than later with larger numbers of knowledgeable investigators working singly and in collaboration. The continued misinforming of students contributes to the slow growth in the number of researchers schooled in radical behaviorism and behavior analysis.

Yet, the situation is not hopeless. Our students and nonbehaviorally inclined colleagues need to be informed of the inconsistencies between what they find in textbooks and in more accurate analyses. This strategy is important because our colleagues use introductory psychology textbooks for their courses, and some of

our students will become the next generation of professors.

We, our colleagues, and our students can alert textbook authors and publishers that content needs to be modified. Faculty can make these inconsistencies known to the publishers' representatives when they come to peddle the newest editions of introductory psychology textbooks. Point out to them why the information in their textbooks needs changing. Students and faculty can also write directly to the authors themselves. It can make a difference. In connection with this approach, faculty can and should take the opportunity to review up-and-coming newer editions of textbooks as well as textbooks that are in the process of publication for the first time.

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APPENDIX

Textbooks Reviewed

- (Page numbers in parentheses indicate discussions of latent learning; textbooks without page numbers do not discuss latent learning.)
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