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James Dougan, *Illinois Wesleyan University*

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GALLISTEL'S THE ORGANIZATION OF LEARNING: *THIS IS* NOT *CREATION SCIENCE*

JAMES D. DOUGAN

ILLINOIS WESLEYAN UNIVERSITY

> After almost a century and a half, evolution is still not widely understood. It is vigorously opposed by defenders of a creator. As a result, it is still impossible to teach biology properly in many American schools. A creation science has been proposed to be taught in its place. The role of variation and selection in the behavior of the individual suffers from the same opposition. Cognitive science is the creation science of psychology, as it struggles to maintain the position of a mind or self. (Skinner, 1990, p. 1209)

> One might expect that the experimental study of learning by psychologists would serve to clarify just what representations different animals can and cannot compute and something about the nature of the computations by which the animals derive these representations. By and large, one would be disappointed in this expectation because experimental psychologists do not generally view the phenomena of learning within a representational framework. It is my purpose to argue that they should. (Gallistel, 1990, pp. 2-3)

It is probably reasonable to predict that few behavior analysts (or evolutionary biologists, for that matter) spend time reading the creation science literature. The reasons are obvious: Creation "science" is in fact antiscientific and anti-intellectual, and there is little to gain from the study of such materials. I must confess a worry that the adamant Skinnerian will reject Gallistel's (1990) *The Organization of Learning* for similar reasons. The book is unapologetically cognitivist in its approach, and Skinner's final proclamations against cognitive science might lead the behavior analyst to reject the book before even reading it. This would be a mistake, because *The Organization of Learning* is not creation science. It is an intelligent and quite fascinating survey of nonhuman animal learning. At the very least, the reader will learn about diverse, interesting, and probably unfamiliar literatures. The careful reader may even gain new insights into learning processes—insights that could lead to new and profitable lines of research.

This is not to suggest that the behavior analyst should agree with everything Gallistel writes. On the contrary, the behavior analyst will probably want to disagree with a goodly portion of the book. More specifically, Gallistel's opinions on the definition and scope of learning, on the role of theory, and on the generality of behavioral principles will probably be disagreeable to many behavior analysts. We often learn more, however, from listening to people with whom we disagree, because such people challenge our preconceptions and force us to reevaluate our conceptual systems. Gallistel's book certainly accomplishes this.

The present review, therefore, will concentrate on those ideas that are most foreign to a behavior-analytic approach. First, however, it is necessary to outline Gallistel's theory of learning, because the theory forms the backbone around which the remainder of the book is organized.

The Basic Theory

> An animal's nervous system constructs representations of behavior-relevant aspects of the animal's environment as a means of adapting the animal's behavior to that environment. These constructions are representations in the mathematical sense of the term; there is a formal isomorphism between entities and processes within the nervous system and selected aspects of the external world. The isomorphism is not fortuitous; the entities and processes in the nervous system isomorphic to selected aspects of the external world play a causal role in generating behavior adapted to those same

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> external aspects. The isomorphism between the internal and external systems is the key to the success of the internal system in carrying out its function. (Gallistel, 1990, p. 475)

The Organization of Learning simultaneously operates on several levels. On one level, the book represents an attempt to summarize and organize a massive and diverse literature. On another level, the book serves as a program for future research, pointing to what Gallistel thinks are interesting and important areas of study. The third level is theoretical. Gallistel's theory of learning is both cognitivist and neurological. The theory is cognitivist because it invokes abstract representational systems as explanatory concepts. The theory is neurological because, unlike some cognitivist theories, the abstract representational systems are taken to represent actual neurological structures that in some cases have even been identified.

According to Gallistel, the basic process in learning is the formation of neurological representations of the environment. As noted in the above quote, the representations are assumed to be formally isomorphic with the external world. Here the term *isomorphism* is used in its purely mathematical sense. Specifically, an isomorphism

> exists when there is a procedure that maps entities, relations, and operations in the represented system into entities, relations, and operations in the representing system in such a way that two or more entities within the represented system are related in a given way *if and only if*. . . there is a corresponding relation between their representatives in the representing system. (Gallistel, 1990, p. 16)

To give an example, if Line A is twice the length of Line B in the external world, a neurological representation will be isomorphic (with respect to these types of entities, operations, and relations) if and only if some aspect of the neurological representation retains the relationship "twice the magnitude of."

As an animal experiences its environment, its nervous system forms representations of that environment. The process is taken to be automatic: No additional reinforcing event is necessary. In fact, "reinforcers" are taken to be just another aspect of the environment that the animal represents. Once formed, these representations are stored in the form of mathematical vectors (i.e., ordered strings of numbers).

Gallistel argues (on the basis of considerable data) that several types of representation are primitive and/or foundational. A primitive representation is one that cannot be reduced to any smaller, more primitive learning process or combination of such processes. A representation is considered foundational when it can be used in computations that produce higher order (secondary and tertiary) representations. Four types of primitive representations are proposed: spatial representation (a record of relative positions of points, lines, and surfaces in the environment), temporal representation (a record of the time of observation of environmental events), numerical representation (a record of the number of events that have occurred), and representation of the distal aspects of stimuli (records of characteristics such as reflectance, size, surface area, etc.).

Primary, foundational representations can be used in computing higher order representations. For example, the primary representation of time of occurrence can be used to compute a representation of temporal interval via the operation of subtraction. The learning of temporal intervals is thus a second-order learning process. The representation of temporal intervals may be combined with the representation of numerosity to calculate representations of rate. Rate is considered to be a tertiary representation because the calculations required (number of occurrences divided by the observation interval) include a secondary representation (temporal interval) as well as a primary representation (number).

In virtually every case, raw sensory data must be processed before representations can be stored. Gallistel thus spends considerable time discussing the types of calculations necessary to compute various types of representations. Because the types of calculation that are necessary vary with both sensory modality and the type of representation, descriptions of these constitute a set of special theories, relative to the general representational theory. In some cases, more than one set of calculations can be potentially effective, and in these cases Gallistel attempts to clarify the differences. For example, an animal's nervous system could represent its position in space by using either Cartesian or polar coordinates. Following a lengthy discussion of the calculations required in each system, Gallistel concludes that a Cartesian coordinate system is preferable because such a system minimizes error.

On the surface, it seems that Gallistel is invoking a "ghost in the machine" that is capable of performing sophisticated calculations—exactly the type of "internal originator" or "autonomous inner man" that Skinner (1971, 1990, and elsewhere) so vehemently argued against. Gallistel is careful to note, however, that no such ghost is required. Even very complex calculation processes have very simple analogues when represented in physical systems. For example, the volume of water in a bucket is a representation of the integral of inflow from the garden hose that fills it. The bucket literally "integrates" the hose with respect to time.

On a more neurological level, Gallistel notes that transmitter-controlled gene transcription within a neuron integrates that neuron's stimulation with respect to time. Because gene transcription results in accumulated biochemical products (proteins), the quantity of protein within a neuron represents the integral of stimulation with respect to time. Most important is the fact that transmitter-controlled transcription has been demonstrated within the nervous system (Greenburg, Ziff, & Greene, 1986). Using such a system, it is theoretically possible to construct a circuit that represents spatial position with respect to an arbitrary origin, using only six neurons (four neurons whose firing rate is proportional to the organism's velocity in each of the four cardinal directions, plus two neurons that integrate velocity along each of the two principal axes). Thus, apparently complex mathematical operations can be conducted by very simple physical systems.

The reader will note that, so far, nothing has been said about behavior. How are these various representations translated into behavior? It is on this point that Gallistel's theory is weakest. Gallistel argues that the various neurological representations are retrieved by various "readout" mechanisms that serve to adapt behavior relative to the representation (and thus relative to the isomorphic environment). He does not elaborate on their nature, simply stating that "suitable mechanisms may well be found if a search is made" and further that such processes "are seldom found (or at least recognized for what they are) unless they are looked for" (Gallistel, 1990, p. 595).

Clearly, Gallistel's theory is of a type that is foreign to most behavior analysts. The theory is cognitivist and neurological, and makes little contact with behavior. Further, as developed below, the theory challenges several conceptions that are widely held by behavior analysts. In each case, however, a careful consideration of these different ideas may be profitable.

The Role of Theory

> Theories—whether neural, mental, or conceptual—talk about intervening steps in . . . relationships. But instead of prompting us to search for and explore relevant variables, they frequently have quite the opposite effect. When we attribute behavior to a neural or mental event, real or conceptual, we are likely to forget that we still have the task of accounting for the neural or mental event. . . . We are likely to close our eyes to (the problem) and to use the theory to give us answers in place of answers we might find through further study. It might be argued that the principal function of learning theory to date has been, not to suggest appropriate research, but to create a false sense of security, and unwarranted satisfaction with the *status quo*. (Skinner, 1950, p. 194)

Behavior analysts are well schooled in Skinner's admonitions against theory. It is important to note that Skinner's warnings apply primarily to theories that invoke abstract, unobserved mediational states as explanatory mechanisms. Physiological theories, when well grounded in direct observation, are not objectionable (see Skinner, 1984, for clarification of this point). Because Gallistel's theory is both cognitivist and neurological, it is difficult to know if Skinner's admonitions apply. If a distinction must be made, however, Gallistel's theory resembles a purely cognitivist/mediational theory more than it does a purely physiological theory. Gallistel does devote a whole chapter to direct physiological evidence, but the majority of the theory is purely mediational because the physiological basis is entirely speculative. Therefore, criticisms of cognitivist/mediational theory apparently do apply.

If behavior analysts want to be (and I would argue we should be) in touch with substantive developments in mainstream psychology, they probably cannot avoid reading mediational theory altogether. Given this inevitability, behavior analysts are probably more comfortable with data-driven inductive theories than with hypothetico-deductive theories that often stray far from the data. Gallistel's theorizing is, for the most part, inductive and data driven. In numerous cases, his theory is developed as fol-

lows: (a) We know, from extensive data, that the animal behaves in a particular way. (b) There are a limited number of ways (and often only one way) that the animal can solve this problem, and (c) these solutions require the animal to respond to certain abstract attributes of stimuli in certain ways. Thus, (d) the animal's nervous system must be forming and using representations of those stimuli. The fourth statement is a logical necessity of the first three. If the first three statements are true (about which it is always possible to argue), then the fourth statement must be true. To deny this would be to deny that there is a neurological substrate for behavior (for an alternative view, see Costall, 1984).

A specific example should illustrate the point. The desert ant *Cataglyphis bicolor* lives underground in a nest that, from the surface of the desert, is identifiable only by a tiny hole in the ground (about 1 mm in diameter). As it forages for food, the ant moves across the desert in a winding, tortuous path, eventually traveling up to 100 m from the nest opening. Upon finding food, the ant turns and orients directly toward the distant (and visually undetectable) nest. It continues in a straight line and at a rapid rate until it is very close to the nest (i.e., within a few meters). How does the ant do this? There are several possibilities: It may be following a chemical trace that it laid down during its outward trip; it may be homing into an auditory or olfactory signal emanating from the nest; it may be "piloting" based on global landmarks; or it may be using "dead reckoning," in which it maintains a constant representation of its position relative to the nest and computes its homeward course on the basis of that representation.

These various possibilities may be eliminated experimentally. The first suggestion (retracing its steps by following a chemical trace) is obviously wrong because the ant does not retrace its outward path. Several other experiments have demonstrated that the ant is not using a beacon and is not piloting. For example, if the ant is taken away from its nest and released a few meters away, it does not orient directly toward the nest, but instead starts off in a random direction. In another experiment, ants were captured at a feeding station and transported to a second (identical) feeding station some distance away, where they were released. Ants left the second feeding station oriented in the direction that the nest would have been (had they not been displaced) and continued in a straight line until they were within 0.5 m of where the nest would have been. Both experiments clearly rule out the use of beacons or piloting by global landmarks. All of the data are consistent with the hypothesis that the ant is exhibiting a dead-reckoning system, in which it maintains and uses a representation of its spatial position relative to the nest, a representation that is updated as the ant moves under its own power but is not updated when the ant is manually displaced by the experimenter (note from the above discussion that the neural circuitry required for such a representational system is actually quite simple). Gallistel argues that a theory based on dead reckoning is the only one consistent with all of the data, and thus argues that animals (at least ants) form and maintain representations of their spatial position. If, as Gallistel asserts, all alternatives have been considered and eliminated, then his conclusions are correct. Even an ardent behavior analyst should find this type of data-driven, inductive theory interesting, if not compelling. It can certainly be considered more compelling than the brands of hypothetico-deductive theorizing more common in cognitivist circles.

In a strange way, such theorizing is quite consistent with the approach taken by behavior analysts. A science of behavior seeks to describe a functional relationship between behavior and environment, without making reference to causal entities that exist at different levels of analysis. The experiments on foraging ants do exactly that: They describe a functional relationship between the animal's spatial position and mode of locomotion (environmental variables) and the orientation and distance of its return trip (behavioral variables). Thus, Gallistel takes higher order invariances between behavior and environment, functionally analyzed, as the phenomena to be explained. Explanation at the behavioral level is therefore consistent in many respects with Gallistel's cognitivist/neurological theory.

A second example will serve to demonstrate this further. Myerson and Miezin (1980) formulated a familiar quantitative model of behavior in experiments using operant choice procedures. Gallistel is much taken by the model, and uses it as a basis for part of his theory. However, Gallistel notes that

> Myerson and Miezin do not present their model in the same spirit in which Gibbon, Church, and Meck present their models of timing and counting behavior, that is, as a model of the functional structure for the underlying causative process. Rather, they adopt a "Newtonian" or "black box" stance. They cast their model in the form of a set of mathematically formulated inductions about the laws of behavior. (Gallistel, 1990, p. 369)

Gallistel then goes on to translate the Myerson and Miezin model, essentially unchanged, into the language of representational theory. The ease by which the Myerson and Miezin model (a good example of a "pure" behavioral model) can be translated into a representational model is testimony to the fact that Gallistel's cognitivist/representational approach and the traditional behavioral approach are, in fact, parallel explanations of the same phenomena.

The next question, obviously, is "Why bother?" Skinner's points against theorizing are well taken. If behavioral and cognitivist/neurological approaches really offer parallel explanations of the same phenomena, what is gained by complex theorizing? A commonly offered answer is that theories can serve as a heuristic by which new research ideas are generated. By thinking in different ways about learning, we might conduct interesting experiments that otherwise might not have been conducted. This has already occurred in the area of matching. Gallistel's cognitivist/representational theory of matching has led him to conduct a series of extremely interesting matching experiments—experiments that have revealed important and surprising results (Mark & Gallistel, 1994). It is not possible to know whether these experiments would ever have been conducted in the absence of Gallistel's theory. What is clear is that theory-driven research *can* produce data that are not only relevant to behavior analysis but also provide a starting place for new behavioral analyses.

The danger lies in the fact that theory-driven research may be wasteful because it is often interesting only within the context of the theory that produced it. Skinner (1950) warned of this problem, which probably contributed to the ultimate downfall of the Hullian system (see Bolles, 1975, for an interesting discussion). A solution is to design experiments that will produce interesting empirical findings, regardless of the theory behind them. Although this may be easier said than done, Gallistel's recent matching work seems to have accomplished the task. The empirical results he has reported will remain of interest regardless of the ultimate fate of his theory.

In summary, Gallistel's approach uses a type of mediational theory that is foreign to most behavior analysts. The theory is inductive and data driven, however, and translation into behavioral terms is quite possible. A behavior analyst will do well to become familiar with experiments addressing such theories, which, if nothing else, can lead to new and interesting directions for behavioral analysis.

The Scope and Definition of Learning

> I wish to point out here simply that the observed data are merely changes in the strength of a reflex. As such they have no dimensions which distinguish them from changes in strength taking place during fatigue, facilitation, inhibition, or . . . changes in drive, emotion, and so on. The process of conditioning is distinguished by what is done to the organism to induce the change. (Skinner, 1938, p. 19)

> Learning is intimately connected to computational machinery that extracts information with a particular formal structure from particular sensory inputs, independent of the immediate utility the information may have and independent of the uses to which it may subsequently be put by diverse readout mechanisms. . . . The task in the analysis of learning is to figure out what is being extracted and stored—what it is about the external world that is represented by the stored code—and how this information is extracted—what kinds of computations are performed. (Gallistel, 1990, p. 88)

In a recent conversation, a colleague criticized *The Organization of Learning* because "it really doesn't have anything to do with learning." A reasonable criticism? Perhaps, depending upon one's definition of learning, and one's willingness to stretch that definition.

Skinner (1938) described two dynamic laws of conditioning, and ever since that time the behavioral analysis of learning has concentrated almost exclusively on operant and classical conditioning, with a heavy emphasis on the former. *The Organization of Learning* certainly concentrates on other things. In a book that is close to 600 pages in length, operant and classical conditioning are not even considered until page 351. When they are considered,

the treatment is relatively brief (just over 100 pages). Thus, a book claiming to be about learning dedicates only a about one sixth of its space to the topics that, to most behavior analysts, constitute the core of learning theory.

Instead of operant and classical conditioning, Gallistel chooses to emphasize what he considers to be the primary and foundational learning processes, processes that result in the representation of space, time, and numerosity. In fact, over half of the book is dedicated to these topics. Operant and classical conditioning receive less attention because they are based on the representation of rate, which, as discussed earlier, is taken to be a secondary or tertiary representation. Gallistel's deemphasis of operant and classical conditioning, therefore, is entirely consistent with his theoretical viewpoint.

The differences are deeper than a matter of emphasis, however. Gallistel offers a definition of learning that is fundamentally different from the definition to which behavior analysts are accustomed. In a Skinnerian system, "learning" or "conditioning" is defined empirically as a correlation between (as Skinner notes in the quote above) changes in reflex strength and the operations that produced that change. Learning exists entirely at the behavioral level, because the operations and observations that define learning are found at the behavioral level. In Gallistel's system, learning is the formation of representations. It occurs at the neurological level and need not be expressed at the behavioral level. Although changes in behavior must, of course, reflect the underlying neurological changes, learning is clearly something that occurs somewhere else, at some other level of analysis. Gallistel's failure to specify the nature of "readout" mechanisms exacerbates the problem, making it virtually impossible to predict how changes in representations will be reflected in behavior.

Despite such major differences in both emphasis and definition, Gallistel spends relatively little time discussing the relationship between his views and those of traditional behavioral analysis. When he does so, it is in a curious and almost self-contradictory way. In the process of developing his views on matching, Gallistel states that matching data (particularly data from Neuringer, 1967) "call attention to the shortcomings of a Skinnerian analysis of learning phenomena" (Gallistel, 1990, p. 364). This is particularly curious because it comes just a few pages before his very positive discussion of the Myerson and Miezin (1980) model that, as a "black box" approach, is fairly close to a true Skinnerian model.

The difficulty arises because Gallistel is confused over just what constitutes a Skinnerian analysis of learning, although his confusion is perhaps shared by a considerable number of behavior analysts. Apparently, Gallistel considers a Skinnerian analysis to be one in which reinforcement plays the primary (and perhaps only) causal role and in which rate of response is the primary (and perhaps only) dependent measure of interest. Such a position does not constitute a behavioral analysis. Behavioral analysis seeks the empirical description of functional relationships between environmental variables and behavioral variables. "Reinforcement" plays a fundamental role in a behavioral analysis of learning only to the extent that empirically defined reinforcers stand in an orderly functional relationship with behavior. Likewise, rate of response plays a fundamental role only to the extent that it varies in an orderly way as a function of environmental variables.

It is easy to understand the source of Gallistel's confusion, because it is easy to forget that Skinner initially proposed to "sketch what seems to me the most convenient formulation of the data *at the present time*" (Skinner, 1938, p. 5, emphasis mine). It is easy to forget because experiments examining various effects of reinforcement on response rate *have* dominated behavior analysis (although there is a trend in choice studies to treat rate in relative rather than absolute terms). It is easy to forget because in some of Skinner's own writings (e.g., Skinner, 1948) he seems to force a reinforcement analysis where other analyses might be more cogent. It is important for the behavior analyst to remember that the dominance of reinforcement and response rate is a matter of *emphasis,* justifiable only on empirical grounds. To believe otherwise is to be as dogmatic as the theoretical viewpoints behavior analysts have criticized as rigid.

Returning to Gallistel's analysis of matching, is he justified to reject a "Skinnerian" reinforcement analysis on *empirical* grounds? Gallistel's argument is based on data from Neuringer (1967) that show an inverse relationship between schedule preference and re-

inforcer probability, as well as on studies of choice on concurrent ratio schedules (Herrnstein & Loveland, 1975). Gallistel suggests as an alternative that the controlling variable in matching paradigms is not reinforcement but the *observed* (and hence internally represented) density of food. Gallistel is certainly correct that such data challenge the early empirical notions of reinforcers as universal strengtheners of behavior (Meehl, 1950; Skinner, 1938). However, more recent behavioral models (e.g., Baum, 1973; Herrnstein & Vaughan, 1980; Hinson & Staddon, 1983; Timberlake & Allison, 1974) have dealt with this problem while retaining the concept of reinforcement. Thus, at least for now, reinforcement remains an empirically justifiable concept. Nevertheless, it is probably worthwhile to consider Gallistel's alternative.

Unlike his treatment of "Skinnerian" analysis, Gallistel devotes many pages to an attack of traditional associative theory. This should bother behavior analysts little because associative theory is not particularly Skinnerian (see Williams, 1987), and Gallistel does not identify it as such. Most of Gallistel's argument should be familiar to behavior analysts: A variety of phenomena (blocking, overshadowing, background conditioning, latent inhibition, conditioned taste aversion, intertrial-interval effects, etc.) are inconsistent with simple conditioning models in which pairing of the conditional stimulus and the unconditional stimulus results in the formation of an associative link. As an alternative, Gallistel offers a nonassociative representational model that has the admirable characteristic of providing good fits to the data without the use of free parameters.

Gallistel's model of classical conditioning has some important implications for recent cellular/molecular models of learning (e.g., Gluck & Thompson, 1987; Hawkins & Kandel, 1984). Gallistel notes that these models are almost universally based on premodern concepts of association. If Gallistel is correct, and molecular theorists do not change their course, the so-called "decade of the brain" (see Cacioppo & Berntson, 1992) will end without any significant progress in identifying the neural substrate of learning.

In summary, Gallistel's views on the scope and definition of learning are quite different from those normally held by behavior analysts. Does this mean that behavior analysts should sell their pigeon boxes and begin studying dead reckoning in invertebrates? Certainly not. The behavior analyst is wise to remember, however, that the field's emphasis on reinforcement is justifiable on empirical grounds alone. There may exist other empirically reliable forms of learning that have nothing to do with reinforcement, and the behavior analyst would do well to educate him- or herself in these literatures. Gallistel's book can be an important component of that education.

On Comparative Psychology and General Laws

> We begin by choosing an organism—one which we hope will be representative but which is first merely convenient. (Skinner, 1957, p. 343)

> Similarly, when it comes to learning, we must expect to find different organs of learning (different computational mechanisms) for such fundamentally different problems as the problem of representing the structure of three-dimensional space and the problem of representing time and temporal intervals. Within one problem domain, we may expect to find only a few basic types of learning mechanism, with interesting variations that adapt a common basic solution to the spectrum of demands peculiar to a given species. (Gallistel, 1990, p. 582)

I have taken an informal survey of my colleagues over the past several years, and I think I have identified two subpopulations. The survey has only one question: Why do you study animal behavior? There are generally only two answers: One group (of which I consider myself a member) responds, somewhat defensively, "because I am interested in animal behavior!" A second group responds (a bit less defensively, perhaps), "because animals provide a model system through which I can understand human behavior." These two groups will, I suspect, have different reactions to *The Organization of Learning*. The first group will find it fascinating, the second might not. There are several reasons for this.

The first reason is based on emphasis and coverage. Virtually the entire book is based on the study of nonhuman animal behavior. On the rare occasions when Gallistel discusses data from human subjects, he seems almost apologetic (e.g., p. 528). The emphasis on animal behavior makes the book a gold mine for those whose primary interest is in animals. Many

of the topics come from biological literatures that a behavior analyst might seldom contact, giving the book a certain exotic flair. These topics include (to name a few) dead reckoning in ants, geese, and gerbils; sun-compass orientation in bees; stellar orientation in indigo buntings; distance estimation in insects, gerbils, chameleons, and toads; homing in pigeons and bats; ideal-free foraging in ducks; circadian foraging patterns in bees; the nursing behavior of hares; and echolocation in bats. At times, the book reads like a nature documentary, with each page revealing new and fascinating facts about the wonderful diversity of animal behavior.

Those behavior analysts who study animals as models of human behavior may find *The Organization of Learning* less satisfying. Much of the material simply does not apply to humans. A discussion of dead reckoning in ants might be viewed as intellectually interesting, but it might also be difficult to get excited about a type of problem that humans are notoriously poor at solving.

The behavior analyst who is primarily interested in animal models will also have other, more fundamental problems with Gallistel's book. Simply stated, Gallistel's theory implies that there may be no general laws of learning *at the behavioral level.* As developed above, Gallistel's theory places learning at the representational level; indeed, Gallistel argues that animals represent their environments in fundamentally similar ways (i.e., as mathematical vectors). However, such generalities necessarily exist at the *neural* level but do not necessarily exist at the *behavioral* level. The behavioral expression of learning is based in part on the way in which sensory and neural systems calculate quantities for representation, which in turn is a product of the type of stimuli the animal is likely to experience. Likewise, the behavioral expression of learning is based on the way in which specific (but as yet unspecified) readout mechanisms retrieve representations and adapt behavior. These readout mechanisms are also assumed to be a function of the type of environment in which the animal exists. The resulting implication is that generalities need not exist at the behavioral level. If these generalities do exist (and Gallistel argues that in some cases they probably do), they are the product of fortuitous evolutionary forces but are not necessitated by the general theory.

The debate over general laws of learning has a long and familiar history within behavior analysis, and behavior analysts have taken a fair amount of criticism (some justified, some not) for a failure to recognize species differences (e.g., Breland & Breland, 1961; Hall, 1987; Schwartz, 1989; Timberlake & Lucas, 1989). Despite some proclamations to the contrary, it is probably safe to say that many behavior analysts at least hope (and many fervently believe) that there are general behavioral laws of learning that apply, in a relatively universal way, across animal species.

Gallistel's thesis challenges a belief in general behavioral laws. What are the implications for behavior analysis if he is correct? At the most basic level, there are none. The question of whether laws of learning apply across species is entirely empirical, and there is no theoretical reason for the behavior analyst to believe or not to believe in such general laws. There is nothing fundamental in behavior analysis that predicts that general laws exist, just as there is nothing fundamental in behavior analysis that predicts that general laws do not exist. Skinner recognized that his initial choice of subjects was merely convenient and only hopefully representative, but this point is often forgotten.

At another level, however, Gallistel's thesis has strong implications for behavior analysis. If there are no general laws of learning, how do those of us who study animal behavior justify our activities to ourselves and to others? Those of us who study animal behavior primarily out of personal interest have no trouble with self-justification. But on what basis can we ethically claim scarce resources like faculty lines and grant funds? How can we confront the threat from the animal rights movement if humans stand to benefit little from behavioral research on animals?

These are difficult questions for which there are no obvious answers. Gallistel's thesis challenges us to consider the questions, and it is a good thing that we do. It is imperative that we consider such questions and formulate good answers. The future of the field may depend on it.

Different Questions, Different Answers

> As in all sciences, both laboratory practices and concepts and principles need to be constantly examined, but I see no point in *arguing* with

those who want to do things in a different way. (Skinner, 1987, p. 12)

> Because of its similarity to the vernacular, cognitive psychology was easy to understand and the so-called cognitive revolution was for a time successful. That may have accelerated the speed with which behavior analysts drew away from the psychological establishment, founding their own associations, holding their own meetings, publishing their own journals. They were accused of building their own ghetto, but they were simply accepting the fact that they had little to gain from the study of a creative mind. (Skinner, 1990, p. 1210)

Cognitivism is not behavioral and behaviorism is not cognitive (Schnaitter, 1987). Each approach asks its own questions and gets its own answers, with little or no required interaction with the other camp. A comparison of Gallistel's *The Organization of Learning* with traditional behavior analysis demonstrates the point nicely. Gallistel asks fundamentally different questions and gets fundamentally different answers. Gallistel is concerned with describing the nature of representations of the environment, whereas behavior analysts are concerned with describing functional relationships between behavior and environment. Although translations between the systems are clearly possible, each system could easily exist independent of the other.

The critical question is how to react to this difference. There are at least two possible strategies. One strategy, apparently that advocated by Skinner in his later years, is isolationism. Although the majority of behavior analysts are probably not isolationists, there is a substantial and vocal group representing this position (see also Coleman & Mehlman, 1992). Behavior analysts already represent a minority faction within psychology, but with our own journals, associations, and meetings, it really doesn't seem so lonely. A continued move toward intellectual isolation might not even be a noticeable difference. Behavior analysts choosing this route need not bother reading Gallistel's book.

Intellectual isolationism has its negative consequences, however. Intellectual isolationism breeds intellectual stagnation. If we surround ourselves with yes-persons, we can soon become so comfortable with our beliefs that we stagnate. We bask in "an orgy of self-adulation" (Nevin, 1991, p. 35) while we simultaneously cease to reevaluate. We cease to see creative solutions to problems because such solutions lie far outside our "mind-set." History shows us that homogeneity of thought inspires little progress, and isolationism could take behavior analysis into a new and darker age.

An alternative, also advocated by a large (but perhaps less vocal) group, is to view the present as an opportunity for a new liberalism. Rather than reflexively isolating him- or herself from new ideas, the behavior analyst could look at those new ideas without rigid precommitments. This is not to abandon the fundamental principles of behavior analysis; rather, it is to develop, as Neuringer (1991) suggests, a new sense of humility. It is to allow ourselves the intellectual challenge of considering ideas on their own merits instead of dogmatically and reflexively rejecting those ideas as "creation science." It is to choose a path toward intellectual growth.

Those behavior analysts who are interested in pursuing this more liberal path will find *The Organization of Learning* fascinating and intellectually stimulating, whether or not they find all of its steps to be in the right direction.

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