

system. His solution is a modification of the behaviorist position in that knowledge is assumed to be more complex than simple changes in response propensities. The prediction of the behavior of an animal requires the specification of its knowledge and its wants or goals. Gallistel assumes that motivation spreads from goal-related motivational units high in the hierarchy through the map to selectively potentiate action units at the level of telotaxes. Representations, like other units of action, consist of interconnected nodes interacting with lower levels.

While unification of knowledge and action systems, like the unification of motivation and action systems, has distinct advantages, such as leading to articulatory coding schemes, the available evidence appears to demand more complex representational systems containing dimensions orthogonal to the action system (Roitblat 1980; 1981). Hulse & Dorsky (1979), for example, present quite strong evidence that rats extract and represent information about a sequence of reinforcer values that reflects the overall rule structure of the pattern. Learning is faster with monotonic than with nonmonotonic patterns, and there is a positive transfer from one pattern to another employing the same rule. Despite arguments by Capaldi and his associates (Capaldi, Verry, & Davidson 1980) to the contrary, it is unlikely that this information could be stored in anything resembling an action hierarchy. This point is further illustrated by the work on the rotation of mental images done by Shepard and his associates (Shepard 1975; Cooper & Shepard 1979). Gallistel recognizes that the performance obtained with this task requires an elaboration of the hierarchical representational scheme to include dimensionally encoded vectors. Even this elaboration, however, is not sufficient to account for the finding that greater degrees of "mental rotation" take more time without the ad hoc assumption that values in the dimensional vector can only be adjusted in a stepwise fashion. These experiments appear to require a representational system that is orthogonal to the action system, not one "flowing" directly into it.

In summary, Gallistel does present a new synthesis of far-reaching importance. Whatever difficulties are present appear primarily to be the result of attempts to make the system operate as a theory of behavior. It is not a theory but more of a metatheory, suggesting how theories of action might be organized and providing a framework in which to cast theories of behavior. It will certainly require elaboration, including more kinds of higher-level structures and the specification of the particular laws that control the hierarchical interaction among units.

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The education of behaviorism and the nature of learning

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The education of behaviorism. At last! Someone in the mainstream of psychology has assembled in one book much of the historically important work on behavioral control, and attempted to integrate it into a general scheme that accounts for current research. The result is a breath of fresh air for teachers and researchers trying to develop a more adequate conception of behavior than that provided by the reflex, the operant, and neural pathways and centers. Most of us knew of other conceptions, but reading in secondary sources about taxes, oscillators, servomechanisms, and hierarchical control

did no justice to the expository powers of the original writers, or to the background of their contributions. Most important, these different conceptions of the functional units of behavior were difficult to integrate into a common scheme. They were largely based on unconnected pieces of research using (for psychologists) peculiar subjects and procedures, and the results were considered primarily relevant to issues of nature and nurture or to currently moot debates, such as whether the nervous system is spontaneously active.

Gallistel's great service was to point out the common themes in these classic works and to show how these themes are relevant to current research. There are arguable deficiencies in this book. Not everyone will be pleased by his choice of articles, or by the inconsistent editing style. There were important omissions, particularly in the analysis of organization of behavior at level 5 and above (e.g., McFarland 1974) and in his nearly exclusive focus on locomotion as an example of motor coordination and control. In addition, his conceptual framework will demand considerable work before it can be taken as adequate to account for current data. Gallistel's point that hierarchies are latticelike (that is, individual lower units can be controlled by many different higher units) is well taken, but his illustrations from the military and business suggest that hierarchies are shaped like pyramids, with fewer and fewer units at each successive level. It seems to me that hierarchies in an animal are as often shaped like hourglasses, diamonds, or even upside-down pyramids.

Another point: though Gallistel treats reflexes, oscillators, and servomechanisms primarily as low-level units, they strike me as representative functions and relations that are likely to be found at both high and low levels of a hierarchy. Further, I suspect that there are more complex units underlying behavior (Tinbergen 1951; Timberlake 1981), and that levels in a hierarchy may not be fixed but may actually reverse under different circumstances. Last, any assumption of ascending levels of control culminating in the neocortex makes me suspicious of a species bias. Gallistel's slightly cautious interpretation of the transection data provoked some wonder that invertebrates, even fish, reptiles, and birds, manage to get about in an orderly fashion.

I doubt that Gallistel would disagree that his scheme is provisional and incomplete. His attempt to include cognitive psychology is suggestive of the potential of a general structural approach, but it did not seem to me entirely successful. This may be more the fault of the dominant models in cognitive psychology than of a flaw in his approach. What he has shown is that we must treat behavior as an entity of complex causation and control. In so doing we may be forced to combine the varying conceptions underlying our research into a common final path, grounded in behavior but adequate to the complexities of information processing and decision making.

The nature of learning. It may be fervently hoped that one effect of Gallistel's integration will be to drag our conception of learning into the twentieth century. Until recently, the study of animal learning has suffered gravely from an impoverished model of the functioning organism, grounded in a simplistic and misinterpreted concept of the reflex that was common at the beginning of the twentieth century. (Gallistel's book is worth the price if only because it quotes enough of Sherrington to show that a functioning reflex was not a simple automatic unit of behavior, but a complexly controlled, integrated, and adaptive entity.)

In most traditional studies of learning, an animal is assumed to bring to the test chamber only basic sensory and motor equipment, a few reflexes, some regulatory tendencies, and its past learning. Even this simple equipment was denied in part by those who attempted to reduce regulatory tendencies to an associative basis (Hull 1935; see Timberlake 1980), and reflexes to learning occurring in ontogeny (see Kuo 1967).

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Such oversimplification focused all attention on the hypothetical strengthening process that presumably intervened between simple sensory reception and motor output to produce learned behavior. This focus isolated the study of learning from the animal's ecology (Timberlake 1981), its regulatory processes (Timberlake 1980), and the physiological coordination of its behavior.

In short, most of us have been studying learning using positivistic assertions that certain concepts exist (because we have developed traditional methods for measuring them), combined with outmoded and incorrect reflex physiology (or models of learning derived from that physiology). It is time to make the study of learning more continuous with the study of motor coordination and control. It is damaging to each approach to maintain sharp distinctions between them. The physiological units of motor coordination are vitally involved in learned behavior, and in many cases learning is no less involved in the control and integration of motor output. The investigation of phenomena such as bird-song learning, with its clear intertwining of physiological mechanisms and learning, may well be fundamental to the understanding of all learning.

A potential contribution of researchers in learning may be to develop more complex models of the structures an animal brings to the learning situation. I recently suggested such a model to account for the behavior of two rodent species toward conspecifics that predicted food (Timberlake 1981). Within this model, low-level sensorimotor units (such as reflexes, taxes, and oscillators) are combined into larger units of perceptual-motor organization called modules. Each module consists of responses that show statistical sequential/temporal relations and can be elicited, controlled, and terminated by particular stimuli. The modules are organized in a loosely hierarchical fashion into systems serving a common function (such as the feeding system). Potentiation consists of input at the system level, and depotentiation depends on the nature and temporal relation of controlling stimuli.

Learning is presumed to occur within these systems in the form of modification of the frequency, order, timing, integration, and elicitation of subunits, modules, and systems. Further, depending on the level and function, learning may occur as the result of response feedback, stimulus-response pairing, motor repetition, or stimulus presentation. The empirical realization of such a general model is a monumental task. It may well be no more than a heuristic guide to other models. One hopes that Gallistel's timely integration will facilitate the development of better models at the interface between research in motor coordination, control, and learning.

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Author's Response

Matters of principle: Hierarchies, representations, and action

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The two dominant themes in the *BBS* multiple review of my book are the adequacy of the principles I

propose for analyzing the action hierarchy and the nature of representations and how they are used by the action system. I am glad that these themes dominate, because they are the dominant themes of the book itself. I have grouped my responses under three headings: "Fundamental principles for analyzing the structure of action," "Learning and representations," and "Miscellanea." Under "Miscellanea" come the more or less inevitable claims that nothing new has been said and the equally inevitable complaints that I did not fry someone else's favorite fish. Also included in this category are responses that primarily raise interesting and pertinent questions to which I have no good answer or that voice sentiments with which I basically concur.

Fundamental principles for analyzing the structure of action

Bolles. I agree that the essence of the hierarchical notion is that there are units capable of carrying out certain functions on their own and then there are superordinate units that integrate these functions to carry out more complex functions. The "autonomy" of the lower units, the fact that they have a structure that enables them to execute certain operations (e.g., stepping a leg) in the absence of any intervention from above, is of primary importance. This is why I am inclined to doubt the view that in the higher animals every kind of operation is carried out in the cortex (cf. Hollerbach). The kinds of "feedforward" or anticipatory correction mechanisms that Bolles calls attention to in his hand-lifting example are clearly important. I did not discuss these because I am not aware of any principled analysis of how they are achieved. The few discussions I have seen in the AI (artificial intelligence) literature strike me as ad hoc, contrived solely for the purpose of explaining the example at hand, without any concern for generalizability or even testability. Perhaps, as Hollerbach seems to suggest, each scheme is unique to a particular case; if so, we are going to have a hard time creating our science. I am inclined to think that there must be some generalizable principles involved. The principles described by Dev seem to me very promising.

Chapple. The questions Chapple raises are the kinds that need to be asked again and again in the study of behavior. Is this particular piece of behavior an elementary or a complex unit of behavior? If elementary, what kind of elementary unit? Are there elementary units other than reflexes, oscillators, and servo-mechanisms? If so, what are their distinguishing characteristics? If we have to do with a complex unit, what are its constituent units and how do they interrelate?

Let us take the so-called scratch "reflex" as an example. Graham Brown (1910) showed that it has two constituents, a reflex raising of the paw to the point to be scratched and an oscillatory scratching motion. Therefore the scratch "reflex" is not an elementary unit of behavior. It is a complex unit, because it can be analyzed into at least two constituents, each of which has the essential features of a unit of behavior - initiators (the sensory receptors that drive the reflex