Motivating Muscles: The Problem of Action

C R Gallistel
The Organization of Action: A New Synthesis
Hillsdale, N.J.: Erlbaum, 1980. 432 pp.
$19.95

Review by
J. A. Scott Kelso *and*
Edward S. Reed

C. R. Gallistel is professor of psychology at the University of Pennsylvania. He is coauthor with Bochel Gelman of The Child's Understanding of Number. ■ *J. A. Scott Kelso is research scientist at the Haskins Laboratories in New Haven, Connecticut, and associate professor in the Departments of Psychology and Biobehavioral Sciences at the University of Connecticut. He is coauthor of chapters in G. E. Stelmach and J. Requin's* Tutorials in Motor Behavior. ■ *Edward S. Reed is a postdoctoral research trainee at the Center for Research in Human Learning of the University of Minnesota (Minneapolis). He is coeditor with R. K. Jones of* Reasons for Realism: Selected Essays of James J. Gibson *(in press).*

How do you get motives into muscles? Psychology has avoided this question like a plague. Theories of motive states, like the grand theories of biology (such as the molecular theory of the genetic code) are "just so" theories; a wave of the hand and sexual urges are translated into muscle potentials. But, as the physiological psychologist C. R. Gallistel is quick to point out, the story is not that simple. In fact, a major problem in modern psychology is the conceptual chasm between what we know about muscles and what we know about motivational processes. There is clearly a need for a theory of action.

According to Gallistel, the guts of the theory have been in the literature all the time just waiting to be organized in a way that would satisfy the palate of the modern psychologist. Gallistel's approach is, by his own admission, plagiaristic: He places in front of the reader some of the classic but infrequently cited papers, those that he believes provide a conceptual basis upon which to build a theory of action. These range from a chapter in Sherrington's *Integrative Action of the Nervous System* (1906) to von Holst's "Nature of Order in the Central Nervous System" (1938) to Weiss's insightful treatise on the problem of coordination (1941). Along the way he provides summaries and discussions of the newer data showing, more or less, how well recent findings fit the insights of these forerunners to modern neurobiology. Few would argue with Gallistel's selections, and he should be commended for bringing them together for students of movement.

Of course the intent of the book goes far beyond reminding us of the writings of Sherrington et al.—interesting though they are. By drawing concepts and examples from the neurobehavioral study of animal activity and linking them to some recent work on cognitive psychology (such as Cooper and Shepard's work on mental rotation) the author proposes—in recognition of its roots in behavioral neurobiology and ethology—a "neurethological theory of action" (p. 361). It is on the achievement of this admittedly lofty goal—not on the achievements of others—that one must evaluate this book. Gallistel's basic claim is that it is possible to bridge the chasm between motives and muscles by means of lessons learned in physiological psychology. In our opinion this may be somewhat premature. We suspect that the physiological psychologist's foundation for a theory of action is, as of now, more modest than the author thinks.

The basic building blocks of action?

Part of the problem in Gallistel's theory stems from his identification of the "elementary units of behavior." There are three of them in the author's view—reflexes, servomechanisms, and oscillators—all of which when combined in particular ways yield complex behaviors. The principle central to creating purposive actions is called *selective potentiation*. According to this principle, elementary units are not ordered *directly* by central programs, but rather subsets of them are "selectively potentiated" to fit prevailing circumstances. Selective potentiation, in a sense, specifies "viable options" and, in so doing, provides the animal with flexible control. As an example, at the highest level of a hierarchically structured system, central programs are thought to control the potential for action in lower level reflex arcs, ensuring that reflex action is consonant with certain specific environmental events. By merely controlling the potential for action, one can account for why the *same* stimulus—a tap to the paw of a locomoting cat—facilitates the flexion reflex during the swing phase and the extension reflex during the stance phase. Both are adaptive responses and "selective potentiation is the agent of behavioral harmony" (p. 279).

But why—we may ask—should a reflex or any other putative element constitute a building block of motivated behavior? And on what grounds would we select (or potentiate) one unit over another. Consider as a test case the work of Sherrington, which the author uses to promote the reflex unit. Sherrington's reflex hypothesis was an attempt to describe a type of mechanism to explain how the central nervous system accomplished some of its integrative function (see Swazey, 1969). However, Gallistel does not tell us about the reflex *hypothesis*; rather the reflex is characterized as one of the elementary *units* of behavior. Apparently the author agrees with Skinner (1938) that a "reflex is not, of course, a theory. It is a fact. It is an analytical unit which makes the investigation of behavior possible" (p. 9). This is odd, for Sherrington himself asserted that reflexes do not exist, except for a very few nonfunctional cases such as the patellar reflex. In fact, Gallistel's book contains the relevant quote: "The simple reflex is a convenient, if not probable fiction" (Sherrington, in Gallistel, p. 22). If reflexes are one of the units of behavior and if, as Gallistel claims, more complex behaviors are constructed out of them, then reflexes had better exist, for if the building blocks of something do not exist, then that something cannot exist. Of course the concepts of reflex, servomechanism, and oscillator have been, and probably will remain, useful for developing intuitions about the way motor systems work. But that is not to say they are the stuff out of which organisms construct actions, or out of which psychologists should construct theories of action.

A basic assumption behind the author's perspective is that the organization of action can be explained by physically realizable principles and processes (p. 6). Later on he castigates the information-processing approach to cognitive psychology, with its emphasis on computer metaphors, as failing to come to grips with the problem of action: "The structure of overt computer action bears little if any interesting resemblance to the structure of animal action" (p. 360). Gallistel is not alone in this view, but does he practice what he preaches? Not if his extensive use of computer terminology is anything to go by. "Central programs," for example, are "complex units of behavior" that figure heavily in Gallistel's explanations of purposive action. It is "the structure of these complex units of action and the structures that interconnect them [that] delimit the animal's behavioral options" (p. 391). There is not much internal consistency here: Programs constitute the language of formal symbol-manipulating machines (computers) not the language of physical principles. The failures of physiological connectionism are patched up with computer-metaphor connectionism; the old gap between muscles and motivation is simply replaced by a new gap between the physiologically irrelevant language of symbol manipulations and the physiologically embodied processes of action. Gallistel recognizes this problem, but his attempts to resolve it (as in his discussion of Deutsch's work) do not go far enough.

Any careful reading of the extensive experimental literature on the homing abilities of diverse animals is enough, I believe, to persuade a skeptic that many, many animals possess the capacity to form the quasi-Euclidean representation of how objects, scents, textures, are distributed within an experienced space. Such representations are then used to instruct the animal's orienting machinery on what cues to use for orienting and what orientation to adopt. The foraging ant has more in common with Proust's narrator than one might imagine.

Units of action vs units in action

It is interesting in this regard that physics, unlike biology and psychology, has largely abandoned the language of unitary mechanism and has replaced it with the concept of systems of interlocking dimensions. This is a necessary development, for what constitutes a unit at one level of analysis is merely a system of interrelated parts at finer grains of analysis. The concept of interlocking dimensions allows for physically realizable models that cut across several grains of analysis, whereas the units of action proposed by Gallistel are, at best, functional units of action at a single grain, losing their relevance at higher or lower levels of analysis. It is precisely this focus on understanding the systemic *relational dynamics* (to use Fentress's term) that motivated Bernstein (whose work is not discussed by Gallistel) and, later, Greene and Turvey (whose work is reviewed in chapter 12) to promote the idea of "coordinative structures" as functional groupings of muscles constrained to act in a unitary fashion. Unlike reflexes, servomechanisms, and the like, but like oscillatory systems, coordinative structures are units of action at any level of analysis, not merely units in actions. Evolution, development, and learning all play a role in economizing the tasks of the motor system via constraints that limit its operations to ranges of activity that can be behaviorally useful. In short, questions of mechanism (which Gallistel addresses) are not ontologically separate from questions of origin (which Gallistel, like most of psychology, chooses to ignore).

Much of Gallistel's synthesis of the locomotion literature fits the coordinative structure paradigm rather well, yet on the surface he is critical of the Bernsteinian approach as espoused by Greene and Turvey. On the one hand Greene's mathematical development of Bernstein's ideas is seen as "largely schematic," and Turvey's use of mathematical metaphors "opaque." On the other hand, the author recognizes that "the Turvey conceptualization has much in common with the one presented here" (p. 361). This is evident for all to see, and it is a pity that some of the derogatory remarks (as well as some of the confusion) could not have been avoided, as perhaps would have been the case had the author consulted some of Turvey and his colleagues' later work.

Toward the end of the book the author offers a self-indictment of his efforts that is perhaps too harsh: "I began," the author says, "by trumpeting my commitment to a physically realizable account of the principles that organize animal action. I end by babbling about my mental image of New York" (p. 388). But the oscillator concept elaborated in chapters 4, 5, and [illegible] is elegant and stimulating indeed, and it may touch base with physically realizable principles more closely than Gallistel recognizes. Thus, the newly emerging physical biology of Iberall and Yates recognizes living systems as composed of ensembles of coupled and mutually entrained oscillators. In this view, termed *homeokinetic* (cf. Iberall, 1978), the oscillatory behavior so common in biological systems is not owing to special mechanisms (like pacemaker neurons) but is a general physical property of systems undergoing energy flux. The beauty of an oscillatory design, of course, and its appeal to the theorist of action, is that a wide diversity of behavioral outputs (and kinematic detail) emerges from coupling processes, such as phase modulation, among interacting oscillators.

Since the link from physics to biology and psychology is still being forged (and resisted by some), one suspects that Gallistel's commitment to physical principles—admirable though it may be—will not be realized for a while. In fact, given psychology's rather limited efforts to actively develop any theory (never mind a theory) of action, it is not surprising that Gallistel's synthesis falls short of the mark. But, if this book motivates psychology to pick up the gauntlet, then Gallistel can claim no little success.

References

Iberall, A. S. Cybernetics offers a (hydrodynamic) thermodynamic view of brain activities. An alternative to reflexology. In F. Brambilla, P. K. Bridges, E. Endroczi, and G. Henser (Eds.), *Perspectives in endocrine psychobiology.* London: Wiley, 1978.

Skinner, B. F. *The behavior of organisms.* New York: Appleton-Century-Crofts, 1938.

Swazey, J. *Reflexes and motor integration: Sherrington's concept of motor integration.* Cambridge, Mass.: Harvard University Press, 1969.