

MAINSTREAMING MOVEMENT SCIENCE*

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The target article by Berkenblit, Fel'dman, and Fukson (in press) is a fine synthesis of a program of research that attacks many of the important issues facing movement science. Our view is that some of these issues--local though they may seem to the study of movement--can be usefully viewed within a larger scientific context, particularly, recent developments in nonlinear dynamical systems and theories of cooperative phenomena in physical, chemical, and biological systems. Thus, the multidegree of freedom movements of animals and people--whose principles continue to elude us--may be couched within, or be extensions of, the laws underlying order and regularity in other natural systems. Thereby, a minimum set of principles may emerge for understanding utterly diverse phenomena (Maxwell, 1877).

As others have long realized (e.g., Boylls, 1975; Greene, 1971, 1982; Turvey, 1977), the field of control and coordination of movement has a rich heritage, stemming from Bernstein's influential theories and empirical research. In a sense, in the West we are only beginning to appreciate Bernstein's legacy, an appreciation not only forced on us by emerging data on multidegree-of-freedom activities, but also as we get a better feel for the deep problems of biological motion. For example, recent work on human posture has demonstrated that rapid and flexible reactions occur in remote muscles when those activities are necessary to preserve function (e.g., stable posture when holding a cup of tea). Claims, however, that such effects "constitute a distinct and apparently new, class of motor reaction" (Marsden, Merton, & Morton, 1983, p. 645, emphasis ours) are myopic in light of this and previous Russian work, and may simply reflect a Western bias (see e.g., Gelfand, Gurfinkel, Tsetlin, & Shik, 1971). The old aphorism that one who is ignorant of history is destined to relive it, applies also, it seems, to insular attitudes in science.

It is interesting to note that Marsden et al.'s research on posture led them, by their own admission, to abandon an earlier influential servo-theory

*The author was invited by the Editor of The Behavioral and Brain Sciences to publish this article as a Continuing Commentary on the Berkenblit, Fel'dman, and Fukson (in press) article, but declined. The present paper was considered too long for inclusion in the original treatment, but was nevertheless forwarded to the authors, who have considered it in their Response to Commentators (Fel'dman, personal communication).

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Acknowledgment. Much of the research discussed herein was supported in part by NIH Grant NS-13617, BRS Grant RR-05596, and Contract No. N0014-83-C-0083 from the U. S. Office of Naval Research.

of stretch reflex function. Yet Berkenblit et al. hold tightly to the concept of reflex as the basis of volitional action, even though the frog's wiping behavior is far from the "machine-like fatality" envisaged by Sherrington or the "machine-like, inevitable reactions" of Pavlov (see Fearing, 1930/1970). In the first part of this comment, we advocate a requiem for the reflex, Sherrington's (1906) "likely, if not probable fiction" and "purely abstract conception." We take, along with much other evidence, the adaptive, context-sensitive and functionally-specific motor behavior of the spinal frog--beautifully shown in the experiments of Berkenblit et al.--as contributing for the reflex's death-knell. In a manner consistent with Bernstein, we claim that the functional units of action are not anything like reflexes: reflexes may be elemental, but they are not fundamental in the sense of affording an understanding of coherent action (for a discussion of the elemental-fundamental distinction in modern particle physics, see Buckley & Peat, 1979). The reflex is a vestige of Descartes and Newton, of a machine-view of animal action, and in our view it is time to discard it. The same could be said of explanations whose ontology rests on the formal machine concept, that is, the motor program and its neurally-based counterpart, the central pattern generator (CPG). But that issue has been addressed elsewhere as Berkenblit et al. note (Footnote 1, see also Kelso, 1981, in press; Selverston, 1980, and commentaries).

The second part of this commentary addresses two central issues lucidly demonstrated and discussed by Berkenblit et al.: (1) the capability of a tremendously complex system possessing a huge number of degrees of freedom to "simulate" a simple, knowable system like a mass-spring; and (2) relatedly, the system's capability to achieve the same macroscopic product (e.g., wiping) with a variety of different effectors, in the face of perturbations and changes in initial conditions, and through a (potentially infinite) number of trajectories. As Berkenblit et al. note, this "constancy" has parallels in perception and even in morphogenesis. The reproducibility of functional behavior in spite of much variability in the "reflex" itself and in the components that contribute to it is indicative of what the biologist would call structural stability, that is, a pronounced invariance in form and function against spatial or temporal deformations (e.g., Thom, 1975; Thompson, 1917; Weiss, 1969). These facts of action and perception (not principles, mark you--phenomena to be understood) can be brought under a common dynamical framework, although here we can only hint at its general features. To some extent, this involves linking the work of Berkenblit et al., with that of their colleagues who study regular and stochastic motion in simple and multidegree of freedom dissipative systems (e.g., Andronov & Chaikin, 1949; Arnol'd, 1978)--a field in which there is currently tremendous interest (e.g., Feigenbaum, 1980; Grassberger & Procaccia, 1983; Haken, 1975, 1983).

Requiem for the Reflex?

The idea that voluntary movement is constructed from reflexes (innate patterns) and ultimately effected by reflex parameterization is not new (cf. Fearing, 1930/1970), although the particular mechanism envisaged by Berkenblit et al. may be. Much confusion has arisen in physiology and psychology over the usage, meaning, and assumptions underlying the concept of reflex. It would not be too hard to document, in the fifty-five years following Fearing's brilliant historical and critical treatise on the reflex, the same conceptual pitfalls that he detailed.

A chief source of confusion rests on the assumption that simplicity of anatomy dictates simplicity of function, and vice-versa. The reflex arc is something with which we are all familiar, and the step toward understanding stimulus-response behavior must have seemed a natural one. In fairness, Sherrington (1906) was wary of such interpretative ease; near the end of his Silliman lectures, he stressed the importance of understanding the interaction of reflexes and volitional control. In our view, Sherrington was in a bind. On the one hand, he could map neurophysiologically the "reflex machinery," the wiring diagram, in certain simple cases (e.g., spinal preparations) and thus relate anatomy to function (e.g., the scratch reflex, the stepping reflex). On the other hand, as a self-professed Darwinian, he recognized that "the difficulty in assigning purpose to a particular reflex is hazardous and inversely proportional to the field covered by the reflex effect" (Sherrington, 1906, p. 239). In our view, the difficulty lay in Sherrington's belief that reflexes, by definition, were hard-wired entities.

Bernstein (1928/1967) took a very different tack from Sherrington. For Bernstein, movement was hypothesized to be "a living morphological object," not "chains of details but structures which are differentiated into details" (p. 67). The identity of movement with emerging form meant that changes in one single detail of a movement could lead to "a whole series of others which are sometimes very far removed from the former both in space and time" (p. 69). For Bernstein, a perturbation to the "motor field" was felt by the field as a whole in such a way as to preserve integrity of system function. It was the form or topology of action that was preserved. This is the essence of the coordinative structure construct (Berkenblit et al., in press, Section 3), by definition, an ensemble of neuromuscular components temporarily assembled as a functionally-specific unit. The remarkable adaptability and variability in the spinal frog's wiping behavior is characteristic of a coordinative structure, not a reflex--at least by any conventional definition.

A coordinative structure organization--as seen in the spinal frog--is apparent in many different activities attesting further to Berkenblit et al's intuition that nature operates with ancient themes. But in our view, it is not so much that higher levels exploit innate patterns as it is that coordinative structures are evident at every level of motor system description and across phyletic strata. This is because functions, not reflexes, are evolutionary primitives. For example, in the case of speech, a so-called "higher level" activity, an unexpected perturbation to the jaw during upward motion for final /b/ closure in the utterance /bæb/ reveals near-immediate changes in upper and lower lip muscles and movements (15-30 ms), but no changes in tongue muscle activity. The same perturbation applied during the utterance /bæz/ evokes rapid and increased tongue muscle activity for /z/ frication, but no active lip movement (Kelso, Tuller, & Fowler, 1982; Kelso, V. Bateson, & Fowler, 1984). Note that the form of interarticulator coordination is neither random nor hard-wired, but unique and specific to the phoneme produced. That a challenge to one member of a group of potentially independent articulators is met--on the very first perturbation experience--by remotely (but not, note, mechanically) linked members of the group, provides strong support for coordinative structures as the meaningful units of behavioral action, regardless of anatomical "level." Though such adaptive behavior could, because of its speed, be described as reflexive, its mutability speaks against any kind of reflex organization.

To recognize the coordinative structure as Greene's (1971) "significant informational unit" is not merely a plea for a change in terminology. It is to underscore the "soft," flexible nature of a unit of action, and to take us away from the hard-wired language of reflexes and CPGs or the hard-algorithmed language of computers (formal machines), which are the source of the motor program/CPG idea. In place of such machine metaphors, the coordinative structure construct emphasizes the analytic tools of qualitative (nonlinear) dynamics (e.g., Abraham & Shaw, 1982; Arnol'd, 1978) and the physical principles of cooperative phenomena in nonequilibrium, open systems (e.g., Haken, 1975). It is this "equipment" that may, on the one hand, provide a principled account of the phenomena discussed by Berkenblit et al. (in press) and, on the other, bring the study of biological motion into the mainstream of theoretical science.

Constancies, Motor Equivalence and Attractors

Berkenblit et al. (in press) ask: How do different movement trajectories, with different effectors and in the face of changing contextual conditions, manage to accomplish the same goal? Similarly, for the case of perceptual constancies, one can inquire: How do different retinal images yield the same percept? Note that in each case the number of microscopic degrees of freedom is enormous (e.g., the neurons, neuronal connections, muscle fibers involved in lifting a finger or the light rays to the eye, the retinal mosaic, and neural processing structures involved in perceiving an object). Yet somehow, this high dimensionality gets "compressed" into a lower dimensional subspace. How such compression is realized is the challenge faced, not only by students of action and perception, but in other realms of science as well. For example, chemistry asks how low-dimensional behavior, such as periodicity, arises in the Belousov-Zhabatinskii reaction even when thirty or more chemical species are present in the reaction vessel (e.g., Shaw, 1981). In the case of movement control and perception, a key may lie in the identity between the flow of a dynamical system (as reflecting, say, the self-equilibrating characteristic of a complex, multidegree of freedom motor system) and the flowing optic array described originally by Gibson (1950). In the former case, the flow is represented in the qualitative shapes or forms of motion observed in the system's phase portrait, that is, the totality of all possible phase plane trajectories generated by a particular dynamical system under a given parameterization. In the latter case, the visual flow is equivalent to optical structure (defined in terms of optical motion vectors rather than Euclidean images, see e.g., Johansson, 1977) that is lawfully generated by the environmental layout of surfaces and by the movements of animals (see e.g., Gibson, 1950, Chapter 7). In each case the relevant parameters are found to be macroscopic and low-dimensional.

Tasks like wiping off a noxious stimulus or reaching for a cup yield patterned forms of motion characteristic of point attractor dynamics, a generic category that denotes the fact that all trajectories on the phase portrait flow to an asymptotic equilibrium state (a basin of attraction). It is important to realize that multidegree-of-freedom systems whose trajectories converge to a stable position can also be described in the low-dimensional language of point attractor dynamics. In the context of movement, this is because the system is dissipative, that is, there is a contraction (not a conservation as in Hamiltonian systems) of phase space volume onto a surface of lower dimensionality than the original space. Other kinds of attractors corresponding to stable, steady-state motions in N-dimensional systems are

periodic attractors or limit cycles, which are capable of characterizing rhythmical tasks like chewing, locomotion and perhaps speaking (e.g., Kelso, V.-Bateson, Saltzman, & Kay, 1985). Moreover, given the presence of chaos even in simple deterministic dynamical systems (e.g., Feigenbaum, 1980), chaotic attractors in movement are not unlikely. Here we see the beginning of a way to conceptualize and model how an extremely complex system becomes controllable as low-dimensional dynamics.

This is obviously only a small part of a big story. Berkenblit et al. (in press) refer in several places to critical behavior (e.g., the critical hip phase angle for initiating the locomotory swing phase) and bifurcations (e.g., in terms of switching among trajectory subcomponents, abrupt modifications of movement pattern). As they note, although such phenomena are well known (if under-recognized) in movement, their lawful basis is not understood. Certain theoretical programs that deal explicitly with pattern formation and change (e.g., Haken, 1975; Nicolis & Prigogine, 1977) suggest a basis for understanding these and other phenomena. In synergetics, for example, near regions of instability (i.e., before qualitative shifts in pattern occur) the system's behavior can be completely specified by one or a few order parameters (Haken, 1975, 1983). Such order parameters are created by the cooperation among the individual components of a complex system, and they in turn govern the behavior of these components. They therefore afford, in principle, a linkage between macro- and microlevels of description. Using concepts of synergetics and nonlinear oscillator theory, Haken, Kelso, and Bunz (1985) have offered an explicit theoretical model of phase transitions in bimanual activity (Kelso, 1984) that should have general applicability to the kinds of critical phenomena and bifurcations discussed by Berkenblit et al. (in press). The theoretical strategy employed by Haken et al. may be worth noting. First, they specify the layout of attractor states characterizing the stable bimanual modes and show how, under the influence of continuous scaling on a control parameter, the layout changes--at a critical value--from one attractor to another. Then they derive this scenario and other features of the data (Kelso, 1984) from the equations of motion of each hand and a nonlinear coupling between them. Recently, Kelso and Scholz (1985) have verified several novel predictions of an extended version of the model (Schöner, Haken, & Kelso, 1986), including the existence of critical slowing down in order parameter behavior as the transition is approached, and enhanced fluctuations in order parameter behavior near the bifurcation region. Such predictions and results would hardly be expected from conventional motor program/CPG accounts of "switching" behavior, for example, gait changes (cf. Grillner, 1982, p. 224; Schmidt, 1982, p. 316).

The present framework may apply not just to biological motion, per se, but to the perception-action system as a total unit. Elaboration of Gibson's work on visual flow fields for example (see e.g., Lee, 1980; Lee & Reddish, 1981) reveals how the rate of dilation, $\tau(t)$ of a bounded region of optical structure specifies the time at which a moving object will contact a surface. (Note: the ratio of retinal expansion velocity and retinal size is equivalent to the inverse of τ). Flies, for example, have been demonstrated to begin to decelerate prior to surface contact at a critical value of the inverse of τ (Wagner, 1982). Thus, not only does the τ parameter and its rate of change, $\dot{\tau}$, provide continuous information for modulating ongoing activity, but at certain critical points, the system exhibits bifurcations to adaptive modes of behavior as well. In this view, then, information for the perception-action system is specified in the morphology of the flow field (Gibson, 1950, Ch. 7).

The flow field geometry is defined in terms of flow vectors to and from a focus of expansion, which can be conceived as basically an attractor or repeller. As the facts of motor equivalence/equifinality tell us, attractors and their layout must be defined in terms of their significance or meaningfulness for the perception-action system, (i.e., in Berkenblit et al., in press, the "reflex" is variable, but the goal achievement is not). Thus, a further consequence of the present framework, to which we can only allude here, is a dynamic information theory (see e.g., Haken, 1984)--one in which information is not viewed in the classical Shannonian sense, as a measure for scarcity of a message or ignorance regarding systemic states (i.e., as receiver-independent), but rather as carrying its own semantic content for the receiver.

In conclusion, there is reason to suppose that an understanding of the multidegree of freedom activities of animals and people falls squarely on the shoulders of an emerging theory of cooperativity and pattern formation in open, complex systems. If so, we conclude where we began, namely that many of the phenomena beautifully treated by Berkenblit et al. (in press) may not be "special" to movement science, and thus may not require "special" concepts beyond those developed from first principles. The view that we are pursuing is that biological motion is an important test field for the essential elaboration of these basically physical (but, mark, non-Newtonian and nonmechanical) ideas.

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