Beyond anatomical specificity

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I wish to address Berkinblit et al.'s treatment of movement plasticity with respect to two classical assumptions that figure prominently in their analysis: (1) the existence of elemental behaviors specific to anatomical structures and (2) the formation of complexity from simplicity. The first assumption can be introduced through the doctrine of specific nerve qualities advanced by Müller and Helmholtz (Boring 1942). According to this doctrine a given sensation (e.g., of brightness, hue, loudness) is specific to the activity in a given nerve. What causes the nerve to be active is immaterial; that the nerve is active is all that matters. Patently, any perception and any action must be specific to something. Generalizing the doctrine of specific nerve qualities leads to the traditional claim that something in question is a part of the body (more exactly, the states of some of its neurons). This is the notion of anatomical specificity that has enjoyed widespread acceptance since Descartes framed his doctrine of corporeal ideas (Reed 1982a). The study of the control and coordination of movement adverts to anatomical specificity in several ways. A host of responses conventionally termed "reflexes" are said to be specific to afferent–efferent linkages in the spinal cord. Oscillatory movements are said to be specific to particular single nuerons (pacemakers) or to circumscribed ensembles of neurons. Whole sequences of responses are said to be specific to command neurons.

Anatomical specificity implies invariable context-free movement units. For example, in the ordinary understanding of reflexes, if the anatomy can be effectively isolated, then when the appropriate afferent path is stimulated a fixed efferent outflow and skeletomuscular patterning should result. Movement plasticity gets defined, therefore, as the problem of how anatomically specific, context-free movement units are adjusted and combined. One can think of the problem in these terms: Successful activity in real environments requires that movements be variable and specific to tasks (the animal's goals or intentions with respect to the surrounding layout of surfaces, including its own); the movements in the basic repertoire, however, are assumed to be invariable and specific to anatomical structures. Attempts to resolve this predicament ordinarily invoke specialized devices mediating the inputs and outputs of the anatomical structures. In that such devices embody a degree of unexplained intelligence, their inclusion in the account of spinal capabilities has been, historically, a source of controversy (Fearing 1930/1970).

Now personally I doubt that there are, in fact, invariable anatomically specific movements. I think they owe their "existence" largely to a methodology that lifts isolated fragments out of context. As Bernstein (1967) underscored, instances of coordinated movement always involve two sets of forces, those provided muscularly and those provided nonmuscularly (reactive, Coriolis, frictional). An animal generates a set of forces that, in conjunction with the nonmuscular forces, realizes its aim. In more picturesque terms, an animal bends the (variable) force structure that is given to produce the force structure that is required (Fowler & Turvey 1978). Insofar as this would always have been true for the activity of any creature, from the smallest to the largest, it seems most unlikely that evolutionary pressure would have put a premium on "hard molded" context-free movements. What seem to be needed, in evolutionary perspective, are systems that can assemble task- or function-specific movements. Where the focus of anatomical specificity is the issue of how invariable units yield variable products, task- or functional-specificity focuses attention on reconciling invariance of a collective end state with variance in the detail of the component courses of events leading up to it. Outside of animal movements nature furnishes very many examples of multiple constellations of microevents yielding macroproducts of essentially the same standard type (Stevens 1974; Weiss 1967). The implication is that the phenomenon reflects self-organizing strategies of great generality. It is notable that morphogenetic processes (e.g., embryogenesis) persistently resist interpretation in terms of anatomical specificity. (Like Berkinblit et al., I believe that there are useful parallels, even convergences, to be drawn between coordinated movements and morphogenesis.) A mixture of isolated single cells, drawn randomly from an already functioning embryonic kidney and then scrambled, lumped, and suitably nourished, will assemble itself into a functionally adept miniature kidney. Such examples abound and are well-known (Weiss 1967). One important lesson they provide is that a cell's role is largely determined by the conditions prevailing at the cell's location. From locale to locale conditions vary depending on the configurational dynamics of the ensemble. Embryogenesis implicates a field perspective. Another important lesson of embryogenesis is that for functionally invariant macroproducts to arise through variable microevents, sources of stability and reproducibility (of function) must be realized in the evolving dynamics of the cell ensemble. Peter Kugler and I, working with a morphogenesis-like problem, have taken a leaf from Gibson (1966; 1979) and sought these sources in alternative descriptions (qualitative, macroscopic properties) of the dynamics (Kugler & Turvey, in press). These properties are close relatives of the "order parameters" of cooperative physical phenomena (e.g., Careri 1984; Haken 1978), and the equilibrium points of Berkinblit et al.'s λ theory may well be interpreted in similar fashion.

If the notion of anatomically specific movements is suspect, as I am suggesting (see also Reed 1982b), then the idea that there are classes of movements (termed "complex") built from simple stereotyped movement units must also be suspect. Thus, although the formation of complexity from simplicity is easily imagined I am inclined to think that it is an improper image. Activities such as the wiping response of focal concern to Berkinblit et al. are more properly construed, perhaps, as types of simplifications from complexity (Pattee 1972). Wiping in the frog is a simple dynamical regime as are the frog's forms of locomotion. Each regime is distilled out of an aggregation of many degrees of freedom at the muscle–joint level and very many more at the cellular level. The formation of these simple regimes from an extremely complex, functionally rich interior is to be understood as a collective or global activity contrasted with the locally specific organizational style of combining specific structures. Detailed accounts of the emergence of particular simplicities are hard to come by (cf. Kugler & Turvey, in press), but illustrations of the general process are widely documented (e.g., Haken 1978; 1981) Prigogine 1980).

By way of conclusion, I am taking the data provided by Berkinblit et al. on the variability and adaptability of the wiping response as evidence for a design principle of functional specificity (Fowler & Turvey 1978; Gibson 1966; Reed 1982b) rather than as evidence for the modifiability of anatomically specific units. And I am suggesting that functional specificity reflects, in part, an overarching set of physical biological strategies by which state spaces of high dimensionality are systematically compressed into state spaces of low dimensionality.

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