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Journal of Motor Behavior
1985, Vol. 17, No. 4, 493-498

The Role of Similarity Analysis in Understanding Movement

K. G. Munhall
J. A. S. Kelso
Haskins Laboratories
New Haven

In "Accepting the Null Hypothesis. . ." Corcos, Gottlieb, and Agarwal (1985) present an analysis of the conduct and interpretation of some recent motor control research (e.g., Brown & Cooke, 1981; Kelso & Holt, 1980). We find ourselves at odds with their presentation on several grounds. First, the review of the literature and statistical analysis is both inaccurate and incomplete. When one admonishes the field about its carefulness in hypothesis testing and data presentation, it behooves one to be both correct and comprehensive. Second, Corcos et al. (1985) advocate a research approach that may not be the most profitable one at this stage of our understanding of motor systems. Most problems in movement science, unlike those in a mechanics textbook, are not well posed. Discovery and understanding rest less on distinguishing hypotheses on some statistical basis as formulating the right questions to begin with. Finally and relatedly, we believe that determining the kind and degree of similarity that exists between conditions, individuals, and organisms is indeed a problem confronting movement science but it is not one that will be solved by setting arbitrary guidelines for statistical power. In fact, much larger questions are at issue than statistical procedures. We will address these three points briefly in turn.

1. Statistical Details

To begin, a few simple yet important pieces of information should be clarified. The subjects in Kelso and Holt (1980) produced 9 trials in each of the perturbed and non-perturbed conditions, not three as Corcos et al. indicate. (Subjects produced 3 trials at each of 3 perturbation loca-

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The authors' research is supported by NINCDS Program Project NS-13617, by U.S. Office of Naval Research (Psychological Sciences Division) Contract N00014-83-C-0083, and a Natural Sciences and Engineering Research Council of Canada postdoctoral fellowship to the first author. Elliot Saltzman and Pam Thompson made valuable comments on earlier versions of the manuscript.

The mailing address for the authors is Haskins Laboratories, 270 Crown St., New Haven, CT 06511-6695.

tions and the perturbed versus non-perturbed analysis was thus based on 9 trials per experimental condition.) As for the power of the Kelso and Holt (1980) experiments, there was sufficient information reported in the data presentation for Corcos et al. (1985) to calculate what size of difference could have been detected between conditions. For example, in Experiment 1 differences of 2.5° in absolute error could have been detected between the perturbed and non-perturbed conditions at the power level Corcos et al. (1985) propose. When the size of the deflection caused by the perturbations is considered it seems reasonable to suggest that the data reflected springlike behavior. Corcos et al. (1985) give no indication that they have made these calculations before levelling criticism.[1] In a similar fashion, insinuations are directed at the work of Brown and Cooke (1981) with no apparent evidence to substantiate them. This is certainly not constructive science in our view.

The overview of power analysis provided by Corcos et al. (1985) is in itself quite superficial. A voluminous and sophisticated literature exists on the topic and its experimental applications yet Corcos et al. (1985) seem to have sampled only some older texts on the subject. Cohen's (1977) work on power analysis is without doubt important but it is not the final word on the topic. Even a cursory search of recent issues of the *Psychological Bulletin* reveals that there is little consensus on such fundamental aspects of power analysis as the preferred relation between Type I and Type II errors (Ryan, 1985; Rosenthal & Rubin, 1985) and the relation between power and measurement reliability (Nicewander & Price, 1983). If a presentation on power analysis were to provide significant benefits to those of us studying motor systems (a possibility that we have some doubts about at present), we suspect that a more sophisticated tutorial than that provided by Corcos et al. (1985) will be required.

2. *Some Issues in Theory Testing*

At the beginning of their manuscript Corcos et al. (1985) suggest that the pulse-step and mass-spring "theories" are in conflict on specific findings. The implication is that the resolution of this conflict will allow some choice between theories, that is, that the veracity of the two "theories" is being tested by specific experiments. We believe that this analysis is wrong on a number of counts. The so-called pulse-step and mass-spring notions hardly deserve the status of "theory." They are, however, useful analogies for some aspects of movement control. Further, that the two approaches are not mutually exclusive nor in complete agreement in their areas of shared domain is not a situation that is unique to the study of motor control nor is it due to the lack of power in specific experiments. The field of theory comparison itself, has had to recognize that there are rival approaches whose commensurability is uncertain (Moberg, 1979).[2]

The choice between theoretical approaches seldom rests on a single criterion. Though empirical verification ranks as a chief way to check a theory's constructs, neither fact nor precision alone determine when a theory should be discarded. Simplicity often takes priority over precision in the establishment of natural law. Other important criteria are extensibility (Newton's laws of motion apply to apples and planets), stability of interpretation (one can't change the rules of the game at one's whim) and that vague word "elegance."[3]

Coming back to motor systems, should the mass spring model be discarded because "absolute" equifinality is not observed (Corcos et al., 1985) or because it is supported by null findings? We think not because of the heuristic value of the approach and its elegance in approximating a range of different dynamical behaviors. In fact, the advantages of taking a dynamical approach, as typified by the mass-spring model, can be specified in some detail (Kelso, in press; Saltzman & Kelso, in press). These include: (1) Generativity—an invariant dynamic structure can give rise to much surface kinematic variability; (2) No explicit representation of the system's planned trajectory need exist in such a dynamical system; (3) Different dynamic regimes can serve as a basis of categorization for different tasks as well as providing control structures for those tasks (e.g., point attractor dynamics for discrete reaching; periodic attractor dynamics for rhythmical tasks, etc.); and (4) Certain kinematic relations present in a large number of activities ranging from reaching to speaking (see Kelso & Kay, in press) can be rationalized. Yet in spite of such advantages the mass-spring model certainly does not account for all aspects of movement nor does it fit all its facts with equal precision. Motor systems are obviously not structurally equivalent to mass-spring systems. As the list above indicates, they do, however, share certain important *functional* equivalences (see *JMB* editorial, *14*, 3, 1982).[4]

Perhaps the most important reason for not becoming entrenched in a statistical theory-choice program at this time is our rudimentary understanding of motor systems. At this early stage in our efforts to characterize biological movement we feel that research must be guided more by our abilities to detect patterns in the data than by the direct comparison of models. As Feynman (1967) says in his "The Character of Physical Law;" "This kind of game of roughly guessing at family relationships . . . is illustrative of the kind of preliminary sparring which one does with nature before really discovering some deep and fundamental law" (p. 155).

3. *Some Proposed Strategies for Movement Science*

As we stated at the outset, we believe that there are important measurement issues confronting the study of movement: How constant should we expect biological patterns to be, and therefore what effect size should we specify in assessments of similarity across different experimental conditions? Perhaps more important, how might similarity in pattern be appropriately characterized? It is around issues such as these that we think a dialogue might fruitfully be opened in the movement community. It should be clear that nothing we have said up to this point obviates the difficulty that one encounters in distinguishing error, variation, and lawful behavior. But it should also be clear that power analysis alone will not allow us to solve these problems. While we do not pretend to possess all the answers we can suggest some strategies that might be explored.

The most obvious and perhaps most needed change is that we take seriously the often made suggestion that we systematically explore the stability of activities across their full working range (e.g., Lee [1984] and Yates [1983]). Recent experimental data and theoretical analysis indicates that certain patterns may be stable in a given region of parameter space, but in other regions instabilities occur and different stabilities arise (Haken, Kelso, & Bunz, 1985). It is clear that the phenomena we are studying must be characterized sufficiently before we can distinguish between theoretical accounts or know what range of data to expect from a "stable" system.

A second direction that warrants exploration is the use of the techniques and formalisms that have been developed in the study of pattern generation in other fields. At issue here is a fundamental one in science, that is, the question of genericity. The identification of generic patterns in nature has a long tradition (e.g., Thompson, 1917/1942) and has drawn considerable attention recently (e.g., Prigogine & Stengers, 1984; Haken, 1977; Feigenbaum, 1980). We believe that certain dynamic patterns in movement can be subjected to the same analytic treatment. The principle attraction is that a unified approach can be adopted in the understanding of structurally very different systems (e.g., Kelso, in press).

Finally, the use of intrinsic measures and, relatedly, dimensional analysis may establish natural units that uniquely characterize a system's stable operation (Rosen, 1985). That is, patterns of behavior may look very different among subjects on conventional units of measurement, but very similar when scaled appropriately to the dimensions of the organism and the activities that the organism is capable of performing (Kugler, Kelso, & Turvey, 1982). The reader is referred to Warren (1984) for a fuller application of this approach to a perception-action problem.

In closing, it should be noted that none of these suggestions remove the obligation to present our data and interpretations in sufficient detail so that the field may judge for itself the pattern's veracity. Power analysis may assist a given researcher in this judgement but it is not, in our opinion, the sole or most important information that should guide one's theoretical evaluations.

NOTES

1. The power issue is to some extent defused when one considers the replicability of the Kelso and Holt (1980) findings. The 13 subjects in each of the two experiment in Kelso, Holt, and Flatt (1980), the 12 and 8 subjects in the experiments in Kelso (1977) and the 12 and 6 subjects in the two experiments in Kelso and Holt (1980) amount to a considerable data pool as well as ample evidence of the stability of the finding. Moreover, in Kelso (1977) and Kelso et al. (1980) support for the mass-spring model rested more on the *highly significant differences* between distance and location conditions when subjects were deprived of feedback, than on the finding of *no* significant differences between normal and cuff movements.

2. This situation is due in part to the manifold ways in which theories can differ from each other. Most obviously they differ in what aspects of their domain they account for,

though even in areas of substantial theoretical overlap comparison is difficult. For example, theories differ in their use of terms whose semantic equivalence is often not easily assessed. Witness the controversy over the meaning of the term "mass" in Newtonian mechanics and special relativity (Field, 1973; Earman & Fine, 1977).

3. A certain mythology has grown up in the history of science about the verification of theories. A case in point is the testing of Einstein's predictions about the effect of gravitational forces on the path of light. It was this particular proposal and its test during eclipses that apparently greatly influenced Popper in his thinking on falsefiability. Yet the amount of refraction of light and its dependence on the proximity of the light's path to the sun has received far from complete corroboration (see Sciama [1969] for the complete eclipse data). As Bernstein (1973) has recently suggested, the variability in the refraction estimates would cause a certain sense of unease if they were the only bases of support for the theory. However, the elegance of the full theory and other independent sources of evidence argue in its favour.

4. The mass-spring analogy was first introduced as a model of the mechanism of muscle-load interaction at a single joint. In one view, muscles are represented by a pair of springs acting across a hinge in the agonist-antagonist configuration. Final equilibrium positions are established by selecting sets of length-tension properties in opposing muscles (e.g., Cooke, 1980; Kelso, 1977). This view, at best, may work for deafferented muscles, but it is inadequate for muscles in natural conditions (see Fel'dman & Latash, 1982). Further, it soon became clear that this mechanistic use of the analogy would have to be altered if the approach was to be useful in more general applications (i.e., complex, multivariable movements). The use of the analogy thus evolved in two significant ways. The model was extended from the consideration of simple linear springs to the consideration of the general field of dynamical systems. Second, applications of the analogy now stressed identities of behavioral function not physiological mechanism. For example, the phenomenon of motor equivalence can be instantiated by a wide variety of anatomical parts; all such structural realizations are related, however, by the same abstract functional organization (see Saltzman & Kelso, in press). Part of the confusion surrounding the mass-spring model is due to this shift in emphasis and the resulting semantic change of such terms as stiffness, dampening, etc.

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Submitted September 25, 1985