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COMMENTARIES

Hand Motor Control: Maturing an Immature Science

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In the target article Mark Latash has argued that there is but a single bona-fide theory for hand motor control (referent configuration theory). If this is true, and research is often phenomenological, then we must admit that the science of hand motor control is immature. While describing observations under varying conditions is a crucial (but early) stage of the science of any field, it is also true that the key to maturing any science is to vigorously subject extant theories and budding laws to critical experimentation. If competing theories are absent at the present time is it time for scientists to focus their efforts on maturing the science of hand motor control through critical testing of this long-standing theory (and related collections of knowledge such as the uncontrolled manifold)?

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In choosing the title "The hand: Shall we ever understand how it works?" Mark Latash at once conveys the inescapable conclusion that how the hand works is poorly understood. Someone who is unfamiliar with the body of scientific knowledge concerning hand function may think that the title serves merely as a rhetorical device for the author, but the question posed in the title is well formed and relevant. The nuanced title and opening section also intentionally invites the reader to consider the existing body of knowledge concerning prehension from the perspective of Popperian views of science. It is an invitation to assess the maturity of this body of knowledge with respect to our observational and descriptive knowledge (and our ability to generate hypotheses to test), the existence of 'laws' that the hand obeys without fail, and finally, scientific theories about hand function that are wide-ranging, predictive, and thus helpful in solving problems.

In this view, of course, an *hypothesis* is a proposition that can be tested (falsified) through experimentation or observation, whereas a *scientific law* is a description of how a natural system will behave under specific circumstances, based on repeated observation under those circumstances (understanding, explanation, or mechanism are welcome, but not required). *Scientific theories* are coherent explanations of behavior that are well-confirmed in a manner consistent with the scientific method, which can be falsified, and which lead to predictions about behaviors that have not

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been observed. Indeed, the confirmation of a theory (or at least its solidification to a degree) comes about through repeated attempts at falsification. As such, theories represent our most rigorous form of understanding, even as they become refined or even fundamentally changed through continual experimental challenge. In the interest of avoiding the dogmatism that often plagues science it is helpful to remember that multiple theories of the same natural system can coexist, firmly withstanding attempts at falsification, even though the mechanistic explanations they embody seem incompatible. This was true in the 1600s, with Christiann Huygens' proposition that light was a wave while Isaac Newton proposed a particle (corpuscular) theory. These opposing theories fueled 200 years of experimentation culminating in the proposition of wave-particle duality, not only for light but for matter (with de Broglie's extension of Einstein's photon theory of light, and the 1929 Nobel Prize for de Broglie).

And so, Mark Latash has asked us to take stock of the science of hand function, while already intimating through his title that this is not a mature science. It is difficult to argue with that assessment despite a long-standing scientific and medical interest in hand function. Indeed, are we even to the point of generating theory? He writes "For the author of this paper the purpose of motor control ("understanding") means discovering *laws of nature* that form the basis for the hand's function." Mark Latash proceeds to document and explain the rich research from his and his collaborators' laboratories that seem to show that the state of the science is not so dire, and we are defining laws of hand function as well as crossing the threshold (pun intended) from laws into a single budding theory ('RC-hypothesis', as the updated form of the equilibrium-point 'hypothesis' or lambda model).

Unfortunately, it seems quite true that the RC-hypothesis is the only coherent explanation of volitional motor behavior with the characteristics of a theory, particularly as it applies to hand function. The theory began with Anatol Feldman in the 1960s, and it would be a gross understatement to write that since then the RC-theory for posture and limb movement control has stimulated vigorous debate, yet only modest experimental testing beyond the proponent's laboratories. Feldman and Latash (2005), and Latash (2008) present cogent and sometimes candid accounts of the scientific evolution of the equilibrium-point hypothesis to the current RC-theory, and now its application to grasp (Latash et al., 2010; Pilon et al., 2007). The popular alternative approach has been EMG-force control (and excitation pulses), and its predictive computation. This computation now seems firmly attached in recent literature to the construct of the 'internal model', at least based on the frequency with which instances of predictive control are presumed, as a statement of fact, to reflect some form of an internal model. It is relatively common to read in the introductory statements of papers that the forces needed for grasp stabilization and manipulation occur through prediction (rather than reactive mechanisms), which is factual, and that therefore (in a lapse of logic) that an internal model is axiomatic.

The observation most-often wielded in casual support of the internal model view comes from grasp studies, in which it is clearly observed that the gripping and lifting forces are scaled to the probable mechanical properties of the object before the object is lifted. This observation of prediction doesn't necessitate a computational internal model. The more pertinent issue is the nature of the all-important control signals that lie at the heart of the difference between the RC-theory, and the EMG-force view. Do the control signals that finally yield EMG specify an intended change in force, or joint torque (or acceleration, etc.), in which case muscle length and velocity must be accounted for at some level of control. Or do the control signals specify a change in a parameter that defines mechanical interactions of a muscle (joint) with external forces, which as a statement of fact operate in force-length and force-velocity space (and its joint-level equivalents)? And therein lays the practical problem with designing definitive experiments that can falsify one view or the other. As Latash (2008) clearly notes, these control signals can only be inferred indirectly with current techniques. Finally, one should not confuse the RC-theory with the suggestion (Feldman 1986) that the control signal (lambda) is implemented via subthreshold depolarization of alpha-motoneurons. The latter, if disproved, does not falsify the RC-theory.

And so what is the current status of the RC-theory? From the impartial and unbiased view of the modern scientific method it is time to acknowledge that the RC-theory has not been falsified after 50 years. As for any theory, it still may be falsified, and probably will be modified. Moreover, as a theory it has made better predictions than EMG-force or 'excitation-pulse' strategies in studies jointly planned with leading critics of the RC-theory (Jaric et al., 1994; Ilic et al., 1996). Yet, there is a growing, tacit assumption that voluntary action is the result of EMG-force control via an internal model, despite the limited attempts to execute experiments that test essential predictions of this view, let alone critically describe those predictions. The application of the RC-theory to grasp is recent, and as such has not generated a vigorous debate. It seems time to marshal the intellectual resources needed to experimentally address the theory in ways that test its predictions under varied conditions, if for no other reason than the RC-theory is the most comprehensive and organized approach to how the hand is controlled for grasp. The same can be said for the accumulating knowledge around the application of the uncontrolled manifold (UCM) approach (Scholz & Schöner, 1999). Together, these approaches reflect a more mature science of hand motor control than the current alternatives and should command our attention for that intrinsic reason, and for their potential utility in solving the problems of the pure and applied science of motor control. The alternative is to bring to scientific maturity other views by attempting to recognize laws, and organizing those views into theory, and then proposing falsifiable explanations that can be tested. However we must ask if there is more to be gained by more rigorously testing the more mature body of knowledge represented by the RC-theory, and to some extent the UCM hypothesis as a way of organizing the redundant effectors in prehension.

Finally, looking ahead, a significant challenge to any theory of hand motor control will be the extent to which it remains useful (yields accurate predictions) in the face of neural damage or disease given the role of the CNS in generating the control signal. It seems as though the control of the hand is as challenging to the nervous system as understanding that control is to us. As Latash notes, besides the mechanical complexity and redundancy, and the relatively large cerebral cortical representations, hand control is too easily lost following damage or disease of the nervous system, and often in old age. As this science matures it will be reasonable to question whether extant theories of hand motor control remain valid under conditions of disease, particularly because the translation of motor control and of motor learning to rehabilitation will be an important use of accepted theory. For the RC-theory, we expect that damage may lead to error in the control signal and/or the mechanisms of its implementation, as may be the case in spasticity. However, with the recognition that the brain is in a continuous state of adaptive plasticity during daily life (as we move, as we learn new skills, and as we move less), and in response to damage and disease, we must remain open to the possibility that the very nature of the control signal adapts as well. As Mark Latash has noted (personal communication), studies of deafferented animals and "deafferented" patients show that movements can be produced rather accurately without reflexes. Obviously, the lambda model cannot work in these cases, which means other control signals produced these movements.

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