movement distance leads to: (1) an increase in peak velocity and (2) an increase in time delay before the antagonist burst starts. Presuming that the central antagonist λ shift is standard, the first factor leads to an increase in antagonist burst amplitude via reflex connections from the muscle spindles. The second factor leads to a decrease in the antagonist burst because of a longer time of "passive deceleration" due to the tonic stretch reflex arc action and viscous properties of the effector apparatus (cf. Lestienne 1979). The theoretical analysis shows that the first factor dominates for short movements; for long movements the second factor leads to a decrease in the antagonist EMG. In our experiments (Latash & Gottlieb, unpublished observations), the most frequently observed pattern included an increase in the antagonist burst for the movements of 18°–54°, and a decrease for longer movements (72°).

Space limitations do not let us discuss other consequences of the Ω-model and compare it to the pattern-imposing models. However, the predictive ability of the dynamic approach is not lower and it seems to provide a better basis for the "dual-strategy" idea.

Strategies for the control of studies of voluntary movements with one mechanical degree of freedom

Gerald E. Loeb

Bio-Medical Engineering Unit, Queen's University, Kingston, Ontario, Canada K7L 3N6

Motor psychologists presumably began studying experimentally constrained (as opposed to natural) movements because they believed that in them, they would discover elemental rules from which one could synthesize more complex, natural movements. No doubt the founders of this religion intended one day to reexamine this premise, but their disciples seem preoccupied with polishing the ornate brass candlesticks. Meanwhile, the other two churches in town, the Biomechanicians and the Physiologists, are dealing with reformist movements that are neither planned nor controlled at the single-joint level.

The phenomenology of movement in a living organism (as opposed to an inert object) consists of a complex interplay between voluntary strategies and physical laws. Some features of the observed trajectories are consequences of the physical system regardless of strategy, and some features of the strategies are constrained by physical laws and biological limits (if the strategies have any chance of achieving the prescribed goal); psychologists study the unconstrained leftovers to see whether they reflect fundamental principles of thought or, perhaps, a bit of whimsy from the brain. Highly constrained (and therefore artificial) paradigms pose two dangers: (1) Even more of the observable features than usual tend to be dominated by physical rather than mental properties of the system, and (2) the task may be so abnormal that the strategies expressed may have little relevance to natural motor behavior.

The first problem has been summed up by the joke about the psychologist who, upon observing the consistently parabolic trajectory of a rock tossed into the air, wondered at how such a simple organism could compute the inverse dynamics of the desired movement. Gottlieb et al. provide one of the more complete accounts of the physical processes that inexorably convert neural commands to motoneurons into EMG signals, cross-bridge attachments, tendon strains, joint torques, limb accelerations, velocities, and endpoints. The problem is that once one works backward from the phenomenology to the underlying physical causes, the conclusions are either painfully self-evident or cluttered with uncertainties that arose in the intervening relationships. For example, recording and integrating the EMG require somewhat arbitrary decisions about electrodes and time constants, decisions that may exert as much influence on the observed time course and peak amplitude of EMG as differences in neural command. Only the most robust "rules" can be seen through such a noisy filter and these tend, in retrospect, to be physically obvious (e.g., fast movements require more effort, slow movements take a long time).

Gottlieb et al. take this inherently noisy data set and subject it to three strategies of their own. First, they divide it into two (perhaps even three) putative behavioral strategies (not unlike the engineering process of piecewise linearization of messy functions). Second, they invoke special arguments to deal with some awkward details, noting, for example, that saturation may influence EMG amplitude (section 11.1a; actually unlikely for the movements studied), and that the onset of acceleration depends on how closely one examines the baseline. Third, they banish the occasional subject who "violates" the laws (section 14.1). Thus, it is not surprising that everything can be made to fit the authors' hypothesis. However, as Stein (1982) and Hasan and Karst (1987) have pointed out, the real question is whether one hypothesis fits so much better than all of the others that it seems to capture some universal strategy of the mind. Unfortunately, the results that can be obtained from the class of paradigms considered here are simply too noisy for this to happen, even if any particular hypothesis were to be completely correct.

One source of the noise may well be the "whimsy" of subjects asked to perform tasks that their brains perceive as awkward and foolish. At the biomechanical level (reviewed by Zajac 1989) and at the neural circuitry level (reviewed by McCrea 1986), it is becoming clear that natural movements are not planned or controlled at the single-joint level and cannot be. Natural movements require planning for intersegmental mechanical coupling, control of multiarticular muscles, and preparatory muscular activity at remote sites (which vitiates the latency arguments hinted at here). These are extraordinarily complicated problems that do not seem to be mechanically or neurophysiologically divisible into the "simpler movements" analyzed by Gottlieb et al. and legions of their predecessors. Certainly subjects can be cajoled into playing along with the experimenter and his ungainly apparatus, but the results may be akin to "the sound of one hand clapping." The fact that most of the subjects and their brains make similar guesses about how best to deal with vague and conflicting goals is not particularly reassuring.

Is psychology searching for hypotheses of motor planning in the wrong place, simply because "the light is better there"? Certainly the study of multiarticular movements requires vastly more complicated methodology and the dissection of the observed trajectories into passive and active mechanical factors requires vastly more complicated analytical techniques. Such complexity introduces a sort of "noise" which may obscure simple principles (if they exist at all), but nothing obscures a simple principle more than looking for it in the wrong place. There are the beginnings of general approaches to describing the mechanical states, goals, and strategies of complex motor systems such as limbs (e.g. Hogan 1985; Zajac 1989). Perhaps the braver among us will take these lights, however dim, and set off in search of reality.

Braking may be more critical than acceleration

William A. MacKay

Department of Physiology, University of Toronto, Toronto, Ontario, Canada M5S 1A8

Electronic mail: mackay%medacl@neat.ai.toronto.edu

Gottlieb, Corcos & Agarwal provide a rational, task-oriented approach to the selection of initial agonist burst parameters. They have greatly elaborated on the two-strategy hypothesis