environmental factors in adoptee antisocial personality. *European Archives of Psychiatry and Neurological Sciences* 239:231–40. [SFS]
Carey, G. & Gottesman, I.I. (1996) Genetics and antisocial behavior: Substance versus soundbites. *Politics and the Life Sciences* 15(1):88–90. [rLM]
Christiansen, K. O. (1977) A review of studies of criminality among twins. In: *Biosocial bases of criminal behavior,* ed. S. A. Mednick & K. O. Christiansen. Gardner Press. [rLM]
Cosmides, L. (1989) The logic of social exchange: Has natural selection shaped how humans reason? Studies with the Wason selection task. *Cognition* 31:187-276. [rLM]
Crowe, R. R. (1972) The adopted offspring of women criminal offenders: A study of their arrest records. *Archives of General Psychiatry* 27:600–3. [SFS]
(1974) An adoption study of anti-social personality. *Archives of General Psychiatry* 31:785–91. [SFS]
Crusio, W. E. (1995) The sociopathy of sociobiology. *Behavioral and Brain Sciences* 18 (3):552. [rLM, SFS]
Davis, M. & Stone, T. (1995) *Mental Simulation*. Blackwell. [JLHC]
Dawkins, R. (1980) Good strategy or evolutionarily stable strategy? In: *Sociobiology: Beyond the nature/nurture debate*, ed. G. W. Barlow & J. Silverberg. Westview. [rLM]
Dawkins, R. (1982) *The extended phenotype.* W. H. Freeman. [rLM]
Dodge, K. A. & Newman, J. P. (1981) Biased decision-making process in aggressive boys. *Journal of Abnormal Psychology* 90(4):375–79. [JLHC]
Emde, R. N., Plomin, R., Robinson, J., Corley, R., DeFries, J., Fulker, D. W., Reznick, J. S., Campos, J., Kagan, J. & Zahn-Waxler, C. (1992) Temperament, emotion and cognition at fourteen months: The MacArthur Longitudinal Twin Study. *Child Development* 63:1437–55. [SFS]
Goldman, A. I. (1989) Interpretation psychologized. *Mind and Language* 4(3):1–17. [JLHC]
(1992) In defense of the simulation theory. *Mind and Language* 7(1 & 2):104–19. [JLHC]
(1995) Simulation and interpersonal utility. *Ethics* 105:709–26. [JLHC]
Gopnik, A. (1993) How we know our minds: The illusion of first-person knowledge of intentionality. *Behavioral and Brain Sciences* 16:1–14. [JLHC]
Gopnik, A. & Wellman, H. M. (1992) Why the child's theory of mind really *is* a theory. *Mind and Language* 7(1 & 2):145–71. [JLHC]
Gordon, R. (1986) Folk psychology as simulation. *Mind and Language* 1(2):158–71. [JLHC]
Harris, R. (1989) *Children and emotion: The development of psychological understanding*. Basil Blackwell. [JLHC, rLM]
Heal, J. (1986) Replication and functionalism. In: *Language, mind and logic,* ed. J. Butterfield. Cambridge University Press. [JLHC]
Holcomb, H. (1995) Ethics: Adaptation or byproduct? Presented at the Annual Meeting of the Human Behavior and Evolution Society, Santa Barbara, CA, July 1995. [rLM]
(forthcoming) *All too human: How evolution rules our behavior.* University of Georgia. [rLM]
Kempthorne, O. (1978) Logical, epistemological and statistical aspects of nature-nurture data interpretation. *Biometrics* 34:1–23. [SFS]
Kosson, D. S. & Neuman, J. P. (1995) An evaluation of Mealey's hypotheses based on psychopathy checklist: Identified groups. *Behavioral and Brain Sciences* 18:562–63. [rLM]
Lakoff, G. (1987) *Women, fire, and dangerous things: What categories reveal about the mind.* University of Chicago Press. [DJS]
Leslie, A. (1994) Pretending and believing: Issues in the theory of ToMM. *Cognition* 50:211–38. [JLHC]
McGuire, T. R. & Hirsch, J. (1977) General intelligence (g) and heritability (H^2, h^2). In: *The structure of experience,* ed. I. C. Uzgiris & F. Weizmann. Plenum Press. [SFS]
Mealey, L. (1994) Experimentation in sociological research. In: *Magill's survey of social science: Sociology,* ed. H. L. Delgado. Salem Press. [rLM]
Mealey, L. (1995t) The sociobiology of sociopathy: An integrated evolutionary model. *Behavioral and Brain Sciences* 18(3):523–41. [JLHC, rLM, DJS, SFS]
(1995r) Primary sociopathy (psychopathy) is a type, secondary is not. *Behavioral and Brain Sciences* 18(3):579–87. [rLM]
Mealey, L., Daood, C. & Krage, M. (1996) Enhanced memory for faces of cheaters. *Ethology & Sociobiology* 17:119–28. [rLM]
Mednick, S. A. & & Finello, K. M. (1983) Biological factors and crime: Implications for forensic psychiatry. *International Journal of Law and Psychiatry* 6:1–15. [SFS]
Mednick, S. A., Gabrielli, W. F., Jr. & Hutchings, B. (1984) Genetic influences in criminal convictions: Evidence from an adoption cohort. *Science* 224:891–94. [SFS]
Mills, M. & Mealey, L. (forthcoming) *Sex differences: An evolutionary perspective.* Academic. [rLM]
Moore, C. & Rose, M. R. (1995) Adaptive and nonadaptive explanations of sociopathy. *Behavioral and Brain Sciences* 18:566–67. [rLM, DJS]
Nesse, R. M. & Williams, G. C. (1991) The dawn of Darwinian medicine. *Quarterly Review of Biology* 66:1–22. [rLM]
Nesse, R. M. & Williams, G. C. (1994) *Why we get sick: The new science of Darwinian medicine.* Random House. [rLM]
Perner, J., Leekam, S. & Wimmer, H. (1987) Three-year-old's difficulty understanding false belief: Cognitive limitation, lack of knowledge or pragmatic misunderstanding. *British Journal of Developmental Psychology* 5:125–37. [JLHC]
Price, J., Sloman, L., Gardner, R., Jr. & Rohde, P. (1994) The social competition hypothesis of depression. *British Journal of Psychiatry* 164:309–15. [rLM]
Raine, A. (1995) Psychopathy and violence: Arousal, temperament, birth complications, maternal rejection, and prefrontal dysfunction. *Behavioral and Brain Sciences* 18 (3):571–73. [rLM]
Rowe, D. C. (1983) Biometric genetical models of self-reported delinquent behavior: A twin study. *Behavior Genetics* 13(5):473–89. [rLM]
Stein, D. J. (1994) Is impulsive aggression a disorder of the individual or a social ill? A matter of metaphor. *Biological Psychiatry* 36:353–55. [DJS]
(1996) Philosophy of psychopathy. *Perspectives in Biology and Medicine* 39:569–80. [DJS]
Stoltenberg, S. F. (in press) Coming to terms with heritability. *Genetica*. [SFS]
Surbey, M. K. (1987) Anorexia nervosa, amenorrhea, and adaptation. *Ethology and Sociobiology* 8:47S–61S. [rLM]
Tversky, A. & Kahneman, D. (1982) Availability: A heuristic for judging frequency and probability. In: *Judgement under uncertainty: Heuristics and biases,* ed. D. Kahneman, P. Slovic & A. Tversky. Cambridge University Press. [rLM]
Wahlsten, D. (1979) A critique of the concepts of heritability and heredity in behavioral genetics. In: *Theoretical advances in behavior genetics,* ed. J. R. Royce & L. P. Mos. Sijthoff & Noordhoff. [SFS]
(1990) Insensitivity of the analysis of variance to heredity-environment interaction. *Behavioral and Brain Sciences* 13(1):109–61. [SFS]
(1994) The intelligence of heritability. *Canadian Psychology* 35:244–60. [SFS]
Wallman, J. (1994) Nature and nurture of myopia. *Nature* 371:20102. [rLM]
Wasserman, D. (1996) Research into genetics and crime: Consensus and controversy. *Politics and the Life Sciences* 15(1):107–09. [rLM]
Wellman, H. M. (1990, 1992) *The child's theory of mind*. MIT Press. [JLHC, rLM]
Wittgenstein, L. (1958) *Philosophical Investigations,* trans. G. E. M. Anscombe. Macmillan. [JLHC]

Commentary on **Mark L. Latash and J. Greg Anson (1996). What are "normal movements" in atypical populations? BBS 19:55–106.**

Abstract of the original article: Redundancy of the motor control system is an important feature that gives the central control structures options for solving everyday motor problems. The choice of particular control patterns is based on priorities (coordinative rules) that are presently unknown. Motor patterns observed in unimpaired young adults reflect these priorities. We hypothesize that under certain atypical conditions, which may include disorders in perception of the environment and in decision making, structural or biochemical changes within the central nervous system (CNS), and/or structural changes of the effectors, the central nervous system may reconsider its priorities. A new set of priorities will reflect the current state of the system and may lead to different patterns of voluntary movement. Under such conditions, changed motor patterns should be considered not pathological but rather adaptive to a primary disorder and may even be viewed as optimal for a given state of the system of movement production. Therapeutic approaches should not be directed toward restoring the motor patterns to as close to "normal" as possible but rather toward resolving the original underlying problem. We illustrate this approach using, as examples, movements in amputees, in patients with Parkinson's disease, in patients with dystonia, and in persons with Down syndrome.

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Process based functionalism instead of structural functionalism is needed

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Abstract: Latash & Anson's intention to describe only the regularities of motor behavior is compromised by the homunculus paradigm. Although we concur on the need to redefine "normal movements" in atypical populations, we contend that this enterprise requires a process based functionalism. We argue for accommodating movement control and perceptual processes with physical and task constraints in a natural setting.

Latash and Anson (1996) (L&A) provide alternatives to the premises of the common misconception of "normal movement." Agreeing with their criticism, we contend that to achieve their intended goal requires transcending these premises. This can be done by a careful consideration of theoretical and pragmatic issues.

L&A claim that there is no general theory of motor control and, at the same time, they acknowledge the assumption of hierarchical organization of motor control system with the homunculus at the top level. Although L&A's structuralist approach was criticized from functionalist and dynamic systems perspectives, L&A maintain that the CNS is a flexible, adaptive agent. Moreover, they argue that hierarchic and dynamic approaches are both correct, and they should be reconciled rather than contrasted. By using findings in biology, we argue for a process-based functionalism in which structure is derivative rather than complementary to dynamic processes.

Biologists traditionally assume that an organism is a relational complex (often hierarchical) structure of its parts. Even functionalist theories remain fundamentally dependent on structuralism because they conceive processes as functions of structural elements. Rashevsky (1954) recognized that in structuralism the very essence of life, the complex functional unity of an organism, is lost. He tried to elaborate a process-based functionalism instead. By looking at metabolic processes in various species Rashevsky found astonishing similarities providing the superiority of process-functionalism.

Using the example of structural metamorphosis of slime mold helps reconsider traditional structuralism. It is well-known that the decrease in nutritious substance forces the otherwise solitary slime mold cells to congregate and form a multicellular organism. In other words, chemical concentration provides a constraint on the metabolic process leading to the emergence of a complex structure. Although the morphological transformation is a relatively fast process, it demonstrates that constraints on metabolism are crucial in the emergence of species at the evolutionary time scale. In motile organisms, these structural changes provide constraints on the locomotory and perceptual processes that naturally lead to other structural changes in their movement control and perceptual systems (Gibson 1966), thus demonstrating that structure emerges from the constraints on fundamental processes. To consider processes with constraints as more fundamental than structure with processes has several advantages: (1) methods and findings of other scientific disciplines can be accommodated (Kugler & Turvey 1987); (2) unlike structural functionalism it does not imply intelligence (homunculus); and (3) placticity can be discussed more parsimoniously (see below).

Consequently, the task of human movement science in understanding normal and atypical movement is to describe movement and perception as processes with constraints. Under this view, structural components are derivative at specific time scales (e.g., anatomical structure at the evolutionary time scale, muscles and CNS structures at a developmental time scale) and they play the role of constraints on processes at shorter time scales. For instance, the skeleton is a hard (rigid) constraint and the CNS is a soft (flexible) constraint on the perceptual and movement control processes. While long term regular stressful activity can slowly lead to bone deformation, repeated activity over a short period of time usually results in deformation of the CNS (i.e., increase of the related active areas of the CNS). Besides these biological constraints of various degree of plasticity, there exist other constraints on movement control. The environment provides hard (physical) constraint (e.g., barriers, openings) and soft (psychological) constraints (goals as attractors, dangerous places as repellors) (Kadar 1996). In sum, under natural task conditions perceptual and movement control processes are closely linked and constrained by morphological, environmental, and intentional variables (Shaw et al. 1992). Thus, process based functionalism can be applied at various time scales. For instance, at the scale of the life time of a human movement control and motor learning can be understood as a result of various (e.g., biological, developmental, environmental, social, and psychological) constraints on dynamic processes of movement. In harmony with empirical findings, at birth, a minimal movement control system suffices to start with.

The proposed view offers a critical perspective on both theoretical and pragmatic issues regarding L&A's approach. Theoretically, perception as process and environment as constraint are undervalued in L&A's view because the experimental data to which they refer derive from abstractly defined tasks with limited dependence of action on perception (e.g., "move your arm as far as you can"). Evidence suggests that when the same movement is performed in an abstract and in a concrete task, one in which plentiful information about the environment is available to support the activity, children with cerebral palsy exhibit superior movement control in the concrete task (Van der Weel et al. 1991). It is observed that making a task concrete improves the smoothness and coordination of movements by hemiparetic and Parkinsonian patients (reviewed in Van der Weel et al. 1991). The profound influence of environmental support on walking ability of Parkinsonian patients is well known; markers on the floor match L-Dopa medication in facilitating the initiation and fluency of gait (Martin 1967). The pattern is helpful only if it can specify a step. To reiterate, where L&A suggest the need for a control agency, we suggest simple constraints that continuously affect action as it unfolds in a dynamic fashion.

Pragmatic consideration reinforce our theoretical stand. L&A are led to the conclusion that atypical movement patterns, as successful adaptations, are best left alone; on the contrary, the following examples suggest a positive role for therapy: (1) As noted, augmenting the environment with appropriate optical structure (e.g., painting wide stripes on the floor) can stabilize Parkinsonian gait, and (2) Gearing vision to stationary and moving objects and to limb segments is essential to coordination. In young infants with cerebral palsy the gearing is impaired, hindering the development of motor control. The deficiency could be lessened by exercises that change expropriospecific information while encouraging the detection of particular exterospecific information (turning the upright infant to and fro, with something of compelling interest in view; Lee et al. 1990; Van der Weel et al. 1991).

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Authors' Response

Does controlling movement require intelligence?

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Abstract: Motor control schemes should have an element of control and an element of coordination. The former is a source of initiative and a product of the brain's work (mind, intelligence, or "homunculus") while the latter can be viewed as a process with constraints emerging at a hierarchically lower, autonomous level. Limiting scientific analysis to an object smaller than the universe necessarily leads to a hierarchical (cybernetic) approach.

We are very grateful to **Kadar & Turvey** for their provocative commentary, which stands apart from the numerous papers that have promoted ecological psychology (as well as "dynamical systems" or "dynamic pattern generation") in motor control. Explicit statements are made about the underlying assumptions and philosophical principles, and direct formulation of these principles serves to clarify differences between two major competing ideologies evident in contemporary studies of motor control and coordination.

Kadar & Turvey's commentary contains several binary statements of an "either . . . or" or "this but not that" type. There seems to be no room for compromise. For example, Kadar & Turvey state that "the task of human movement science . . . is to describe movement and perception as processes with constraints." We suggest that the above should be construed as "a task" but not "the task" of human movement science. Similar views were expressed in an earlier commentary by Treffner & Kelso (1996), who stated that equations governing human movements are known and can be found in papers in the dynamical-systems approach. We have failed to find these equations (Anson & Latash 1996) and continue to adhere to our position that specific equations of motion of *internal variables* in the central nervous system are unknown. In our view, there is definitely room for more than one theory and philosophy.

In this reply, we address the following issues raised by Kadar & Turvey: (1) hierarchy vs. heterarchy, (2) the place of mind in motor control, (3) the origin and role of intentional variables, and (4) clinical examples.

R1. Hierarchy vs. heterarchy

Kadar & Turvey dismiss the idea of a hierarchical organization of the human motor control system as obsolete. We believe that the hierarchy vs. heterarchy debate is far from being over (cf. Meijer et al. 1988). As in any debate, it makes sense to start with premises and definitions. Let us identify two subsystems in a certain system based on some principle (Fig. R1). The subsystems exchange information. The informational input may exert symmetrical or asymmetrical effects on the subsystems. In Fig. R1(A) one of the subsystems (S2) cannot ignore the input from the other subsystem (S1), while S1 can use *or* ignore information coming from S2. In other words, S1 can decouple itself from the input originating from S2, whereas S2 does not have this property. We would call this case a hierarchy, and S1 a higher subsystem or level. In Fig. R1(B), neither S1 nor S2 can ignore the input from the other subsystem. This is an example of a heterarchy.

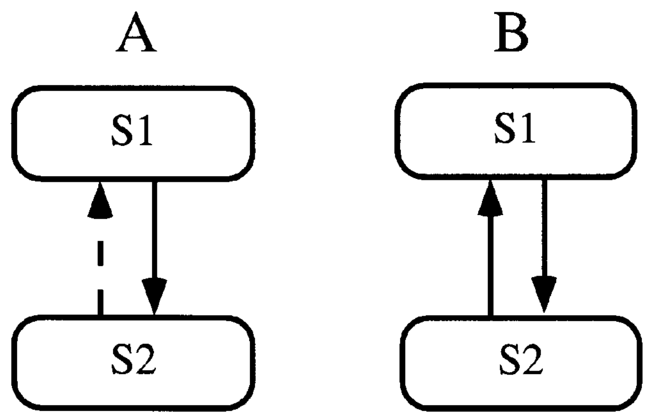

Figure R1. An illustration of a hierarchy (A) and of a heterarchy (B). Solid lines indicate obligatory informational inputs that cannot be ignored by the target subsystem. Dashed line indicates input that can be used or ignored by the target subsystem.

A large amount of information related to everyday observations and neurophysiological studies (e.g., studies of spinal locomotion and other movements, Shik & Orlovsky 1976; Berkinblit et al. 1986) suggests that within an animal or human body there is both hierarchical and heterarchical organization. In particular, the mind, which may be considered the work of the brain, is not necessarily driven by signals from the spinal cord or the environment. For example, while reading this paragraph, readers can, at any time, close their eyes and think about their dog or wiggle their big toe. To us, this simple observation points to a hierarchical organization of the system for thought and voluntary movement. On the other hand, motoneurons cannot ignore signals coming from receptors in their target muscles, and muscles cannot ignore signals coming from the motoneurons. This is an example of heterarchical organization.

We think that the first and probably the most important step in research is choosing an appropriate level of analysis and identifying functionally important variables relevant to that level. In a chosen level of analysis, an object of analysis is separated from outside factors. For example, if a person uses a tool, it makes sense to separate the physical properties of the tool from the person's brain. This represents a hierarchy because the tool is driven by the brain, whereas the brain is not necessarily driven by the tool. We think that the brain controls the body just as it controls external objects (tools).

In classical experiments by Kelso and his colleagues (Kelso et al. 1976) phase transitions were observed in bimanual tasks with an increase in the frequency of an oscillatory movement. The system bringing about the phase transitions was considered a heterarchy. But one might ask: Where do changes in the movement frequency come from? From the metronome! Who programmed the metronome? The experimenter! Who or what placed the idea of programming the metronome in this particular fashion into the experimenter's head? The progress of science in this particular area! Where did this progress come from? We could continue *ad absurdum* until our heterarchy includes the whole universe (and perhaps beyond, if our universe is dynamically coupled to some mega-dripping faucet). Alternatively, the heterarchy could assume an input from a homunculus. Why should the functional unity approach be limited by one's skin? This sounds rather egocentric. Why not the functional unity of the universe? In fact, separating the organism from the universe is already functional structuralism.

Kadar & Turvey argue that structure emerges from the constraints on fundamental processes. In this argument, the genetic code is a constraint of the organism. But how

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did it emerge? We can probably trace it back to the Big Bang. Indeed, what about the Big Bang? Would it be appropriate to consider it a consequence of an intelligent mind game? Apparently, one must stop somewhere and consider inputs to the selected level of analysis as being generated by a hierarchically higher system. For us, there is no problem in attributing intelligence to this system because it apparently encompasses all the experience of humankind.

R2. The place of mind in motor control

Kadar & Turvey specify three distinct advantages of their approach compared to "structural functionalism" (the cybernetic approach). Advantages 1 and 3 seem questionable. It is not clear to us why accepting the alternative view would prohibit one from accommodating methods and findings of other scientific disciplines (1) or from discussing plasticity in a parsimonious way (3). Advantage 2 surprised us: Kadar & Turvey suggest that their approach is better because it does not imply intelligence. Surely the most important difference between the movement of a rock rolling down a hill and coordinated human movement is exactly the element that can be called "intelligence." Human movements are typically purposeful and functionally appropriate and can hardly be conceived without an element of intelligence.

Returning to the hierarchy vs. heterarchy problem, we view mind or intelligence as a task carried out by the brain. For the purposes of motor control, the result of carrying out this task can be thought of as a complex variable produced by a hierarchically higher subsystem (a homunculus). We view the mind as the driving variable that initiates, monitors, and corrects all purposeful movements without necessarily prescribing all the details of peripheral motor patterns. In artificial laboratory experiments, a metronome or a moving visual field can be substituted for the brain's work so that the autonomous functioning of a lower subsystem can be studied. However, experimenters always rely on good will and cooperation from their subjects, hoping that they do not start using their minds in the middle of an experiment. Imagine, for example that subjects close their eyes or ignore the metronome beat and start to murmur a song with a different rhythm. Such feats of imagination (work of the brain) are not at all beyond the realms of possibility.

The major difference between the behavior of a dripping faucet and a human being (both are complex nonlinear dynamical systems) is the presence of mind that allows humans to ignore environmental factors or to use them at their discretion. In other words, mind gives us the ability to modulate the degree of coupling with perceptual variables between 0 and 100%.

N. A. Bernstein was the first scientist to claim that our movements were generated and controlled, not through reflexes, even very complex non-linear dynamic reflexes, but by initiative of the central nervous system, that is, by mind. Bernstein was the founder of the physiology of initiative (commonly translated incorrectly into English as "physiology of activity"; cf. Latash & Feigenberg 1996). We believe that depriving the motor control system of an element of intelligence represents a step backward, eventually leading to the Pavlovian scheme of behavior (a combination of inborn and conditioned reflexes) since no independent input signal from a "homunculus" (another word for mind or intelligence) is allowed. This does not seem to us to be much of an advantage.

With all due respect to the impressive success of the dynamical systems approach in movement science, it should be remembered that this approach is just a mathematical tool, and its application to human movements is an example of mathematical simulation using variables and parameters that frequently do not have a clear physical or physiological meaning. Until now, this approach has been very successful in describing regularities of *external movement patterns* (typically, trajectories of two joints/limbs/persons), not internal variables. Limiting movement science to this description seems to us appropriate for inanimate objects, but not for human behavior.

R3. The origins and role of intentional variables

Kadar & Turvey mention intentional variables once in their commentary but do not elaborate. To us, this is understandable because intentional variables are products of mind and absence of intelligence is claimed to be one of the advantages of the scheme championed by Kadar & Turvey. Recently, Turvey and Carello (1996) published a chapter with a figure illustrating their interpretation of the motor programming (hierarchical, cybernetic) approach and the alternative, dynamical systems approach. The first drawing contains an image of a fish whose peripheral organs are controlled, like a marionette, by another, "homunculus" fish. The second drawing does not have the homunculus; instead, the peripheral organs of the fish are linked to each other. We think both drawings are inadequate. The first drawing has an element of control (the homunculus) but no coordination, so each and every detail of movement must be prescribed centrally. The alternative drawing has coordination (links among effectors) but no control. This fish will never be able to change its behavior "on its own," only in response to environmental variables, that is, exactly along the lines advocated by the Pavlovian theory of conditioned reflexes.

We believe that a third drawing would be more helpful, one which combines coordinative links among the effectors and an independent input function reflecting an element of initiative (mind). This input need not control all the features of a planned movement. It can modify links among the effectors (we would describe these links as emerging at a lower hierarchical level) and provides general functional goals.

R4. Clinical examples

Kadar & Turvey mention two examples of motor disorders to support the practical applicability of their approach. One reference is to study by Van der Weel et al. (1991) on movements in cerebral palsy children. In the original paper, only a greater range of motion during the concrete version of the pronation-supination task is mentioned as a positive outcome. Apart from that, feedback conditions were different for the three tasks compared in the study; the feedback during the concrete task was specifically designed to encourage a larger range of motion. To interpret this as "superior movement control" (Kadar & Turvey) seems to us to encompass a substantial stretch of imagination.

Another example is Parkinson's disease (PD). It is well known that placing markers on the floor can improve walking initiation and gait in PD (e.g., Morris et al. 1995). However, we view this approach as potentially dangerous. Encouraging patients to make larger, "normal-looking" steps rather than using their typical shuffling gait may not be helpful. Problems in walking in PD may be secondary to problems in postural control. In the reproducible and friendly laboratory environment, a patient may be persuaded to take larger steps. However, making such steps in the much less predictable, real world may lead to harmful consequences like falling down because of the larger forces associated with "normal" stepping. So, attempts to provide sensory crutches should be carefully analyzed keeping in mind what the primary problem is and what the functional goals are.

To summarize, we do not see the homunculus paradigm as compromising our views in general or the target article in particular. Indeed, we do not see a viable alternative to having an element of intelligence in the general scheme of motor control. The only alternative is behavior based on a "stimulus-response" principle. We hope that Kadar & Turvey would agree that this principle is no longer viable (cf. Popper & Eccles 1983).

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References

Anson, J. G. & Latash, M. L. (1996) Towards peaceful coexistence of adaptive central strategies and medical professionals. *Behavioral and Brain Sciences* 19:94–106. [rMLL]

Berkinblit, M. B., Feldman, A. G. & Fukson, O. I. (1986) Adaptability of innate motor patterns and motor control mechanisms. *Behavioral and Brain Sciences* 9:585–638. [rMLL]

Bernstein, N. A. (1996) On dexterity and its development. In: *Dexterity and its development,* ed. M. L. Latash & M. T. Turvey. Erlbaum. [rMLL]

Gibson, J. J. (1966) *The senses considered as perceptual systems*. Houghton Mifflin. [EEK]

Kadar, E. E. (1996) *Field theoretic approach to the perceptual control of action.* Unpublished dissertation. University of Connecticut. [EEK]

Kelso, J. A. S., Southard, D. L. & Goodman, D. (1979) On the nature of human interlimb coordination. *Science* 203:1029–31. [rMLL]

Kugler, P. N. & Turvey, M. T. (1987) *Information, natural law, and the self-assembly of rhythmic movement*. Erlbaum. [EEK]

Latash, L. P. & Feigenberg, J. M. (1996) History of the physiology of activity: From a principle to regularities, to brain mechanisms, and beyond. Abstract of the International Conference. In: *Bernstein's traditions in motor control*. Penn State University Press. [rMLL]

Lee, D. N., Daniel, B. M., Turnbull, J. & Cook, M. L. (1990) Basic perceptuo-motor dysfunction in cerebral palsy. In: *Attention and performance XIII,* ed. M. Jeannerod. Erlbaum. [EEK]

Martin, J. P. (1967) *The basal ganglia and posture*. Pitman Medical Publishing. [EEK]

Meijer, O. G., Wagenaar, R. C. & Blankendaal, F. C. M. (1988) The hierarchy debate: Tema con variazioni. In: *Complex movement behavior: The motor-action controversy,* ed. O. G. Meijer & K. Roth. Elsevier. [rMLL]

Morris, M. E., Iansek, R., Summers, J. J. & Matyas, T. A. (1995) Motor control considerations for the rehabilitation of gait in Parkinson's disease. In: *Motor control and sensory motor integration. Issues and directions,* ed. D. J. Glencross & J. P. Piek. N.-Holland. [rMLL]

Popper, K. R. & Eccles, J. C. (1983) *The self and its brain*. Routledge & Kegan Paul. [rMLL]

Rashevsky, N. (1954) Topology and life: In search of general mathematical principles in biology and sociology. *Bulletin of Mathematical Biophysics* 16:317–48. [EEK]

Shaw, R. E., Kadar, E. E., Sim, M. & Repperger, D. W. (1992) The intentional spring: A strategy for modeling systems that learn to perform intentional acts. *Journal of Motor Behavior* 24:3–28. [EEK]

Shik, M. L. & Orlovsky, G. N. (1976) Neurophysiology of locomotor automatism. *Physiological Reviews* 56:465–501. [rMLL]

Treffner, P. J. & Kelso, J. A. S. (1996) Generic mechanism of coordination in special populations. *Behavioral and Brain Sciences* 19:89. [rMLL]

Turvey, M. T. & Carello, C. (1996) Dynamics of Bernstein's level of synergies. In: *Dexterity and its development,* ed. M. L. Latash & M. T. Turvey. Erlbaum. [rMLL]

Van der Weel, F. R., Van der Meer, A. L. H. & Lee, D. H. (1991) Effects of task on movement control in cerebral palsy: Implications for assessment and therapy. *Developmental Medicine and Child Neurology* 33:419–26. [rMLL, EEK]