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Short Communication

Mostly "Old wine in new bottles." Reply to commentaries

Michael G. Wade

School of Kinesiology, University of Minnesota, USA

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We critically reviewed the research literature on the possible cause of children diagnosed with developmental coordination disorder (DCD) employing the traditional information processing (IP) approach, and its reliance on metaphors and imaging studies. Information processing theory is a machine theory invoking a computer analogy. It embraces a reductionist perspective that seeks to reduce complex behaviors to a simple set of variables that offer the possibility of identifying a cause and an effect (i.e. reductionism is a form of determinism) (McLeod, 2008).

We contrast this approach with a dynamical systems theory (DS) that embraces Gibson's (1966) view that perception is direct, which argues that the coordination deficits identified in children with DCD have more to do with a degrading of perception–action coupling with respect to their movement abilities. We review and comment on the extant research and conclusions of both approaches and conclude that the data supporting an IP explanation, while substantial in terms of volume, remains less than convincing. Therefore, a reconsideration of the cause of DCD is in order. This is not to deny the importance of both the structural and functional role of the brain but to question the many inferences required to justify the neuroscientific status and the associated metaphors required of both earlier and more contemporary IP accounts of DCD, especially the internal model hypothesis (IMD).

We find ourselves in general agreement with the three main points outlined in Sugden's commentary. Regarding the validity of an IP account as a causal explanation of DCD, Sugden clearly shares our view that, to date, the empirical support for a contemporary IP explanation of DCD is less than compelling. We agree that the ability to "plan" is an important feature for effective motor action and that in this context executive function (EF) is essentially synonymous with what Gibson and Pick (2000) refer to as *prospectivity,* a component of *agency*–the intentionality of behavior. While this terminology infers similar behavior, the central question is where this anticipatory or planning fits into a theoretical structure. The EF studies we cite in Wade and Kazeck (2018) typically employed tasks that required little or no coordination and we question the generalization of such results to problems of poor coordination. Sugden's reference to Logie (2016), who suggests that the central executive notion be "offered a dignified retirement" would seem to support our position. Sugden correctly notes that we say little about 'development', (the first 'D' in the term DCD). Our focus was on *coordination*, i.e. the 'C' aspect, but we agree that the developmental trajectory of all children reflects the level of coordination as a consequence of physical growth, strength and experience, and that all are in a state of flux, changing at different rates over time. Finally, Sugden raises the notion of *non-linear pedagogy*, something we did not include in our article, but clearly reflects the differential rates of the overall development of the child.

The central theme of Geuze's commentary is the role of constraints, which is indeed crucial in the performance of all individuals across the lifespan. That said, it would seem that Geuze is less concerned with evaluating a particular theoretical perspective, taking

E-mail address: mwade@umn.edu.

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more of an 'individual difference' approach allowing theoretical explanations to fit a range of conclusions reached via the adopted empirical approach. This seems more akin to the 'hybrid' approach championed by Wilson et al., which we address below.

Geuze notes that, in his opinion, both information processing (IP) and dynamical systems (DS) theories are both 'black box theories.' We could not disagree more. The central departure point of our critique addressed, from the outset, the question of *how* the animal (i.e. human) perceives. The two views of perception are very different. Dynamical systems theory (DS), which embraces perception as direct (after Gibson, 1966 and 1979), assumes that environmental information is 'meaningful' to the observer. Thus, our senses *are* our perceptual systems (Gibson, 1966; cf. Runeson, 1977) and do not require an 'in the head' mechanism. Geuze regards the environment as the provider of task relevant or task irrelevant stimuli, yet fails to address how such stimuli are perceived.

Information processing theory is a theory of *machines* not of living systems which is central to our criticism of the machine metaphors that IP theory requires in order to delineate a possible cause of DCD. In earlier research, Geuze and colleague indicated the same: Volman and Geuze (1998), when describing rhythmic coordination, noted that "The differences in stability found between children with DCD and controls are difficult to explain by hierarchical timekeeper models " (p.55, 2nd paragraph, lines 12–13).

Geuze asks how the system "knows" and, if affordances *are* readily perceived at an early age, why must parents pay attention to the safety of their children? Having information available about opportunities for action does not imply that children are able to pick up this information and act safely on such knowledge. Lee, Young, and McLaughlin (1984) demonstrated that young children can accurately judge the speed of approaching vehicles and safely cross a simulated roadway, but this does not suggest that parents should allow their young children to safely exploit this ability! In closing, Geuze correctly notes that an IP model permits a decomposition of a system into functional testable parts. This, of course, is the scientific rationale for an IP approach, but also its 'Achilles heel.' In his recent essay (titled '*The Empty Brain*'), Epstein (2016) notes that the functional parts are all machine metaphors that are inferred with little or no basis in reality, arguing that we are born with senses, reflexes and learning mechanisms, and not with information, data, software, etc. In short, we are living organisms, not computers.

The central problem with IP theory as a causal explanation of DCD, a deficit or constraint in a living system, is not reflected in artificial machine metaphors.

2. Old wine in new bottles

The Wilson et al. commentary ('*Hybrid is not a dirty word*') is both lengthy (15 pages with 10 separate sub-headings) and detailed in its criticism. We, Wade and Kazeck (2018), challenged the underlying IP theory as a possible cause of DCD, and mounted a critical review of the extant empirical support for such a reductionist position.

In their commentary, Wilson et al. avoid this challenge arguing in favor of the IP view that perception is indirect and requires an internal mechanism to assign 'meaning' to sense data. They argue that a more contemporary view of IP models – plus more imaging studies and *meta*-analytical or other reviews – somehow strengthens their view. Their resulting commentary is essentially a list of sub-headings in the form of a series of promissory statements that may prove accurate at some future date.

We referenced the *meta*-analytical reviews by Wilson and colleagues (1998, 2012), and summary reviews of imaging studies by Hadders-Algra (2013a), Kashiwagi and Tamai (2013), and Brown-Lum and Zwicker (2015); none of these studies concluded anything definitive regarding the presence of a specific neurological deficit in children with DCD. It was apparent that a wide diffusion of brain activity was present, in the absence any specific neural location that might pinpoint a possible cause for DCD.

Wilson et al. make two points: First, that we ignore the increasing number of imaging studies conducted since their last *meta*-analysis (Wilson, Ruddock, Smits-Engelsman, Polatajko, & Blank, 2012); and second, that we cite only 5 studies (2 of which published from our laboratory). We, in fact, cite 20 associated references – both theoretical and empirical– questioning a reductionist approach to explain typical and atypical motor behavior. We would remind Wilson et al. about the conclusions of Brown-Lum and Zwicker (2015) that substantial neural variability is present when groups of typical and DCD participants are examined. Specifically, they noted:

> *"Although neuro-imaging studies have greatly informed our understanding of DCD, most studies are limited by small sample sizes and thus need to be replicated to confirm findings." (p. 138, c. 2, 2d par., lines 1–3)*

and

> *"Because most of the neuroimaging findings in DCD are related to differences in brain activation or are at the microstructure level, using MRI for diagnosis is not practical at this time; (…)" (p. 138, c. 2, 2d par., lines 13–16)*

The Wilson et al. commentary is essentially a defense of what they claim is a more contemporary version of "Mr. Schema" (beyond the narrow box and arrow models of traditional IP theory). Cognitive neuroscience has resulted in little fundamental change to IP's theoretical orientation. The internal model, first articulated by Francis and Wonham (1976), is no more 'mid-century' than Schmidt's (1975) Schema theory; and, at least Schmidt's (1975) theory afforded testable hypotheses, proposing a generalized motor program (GMP) and recall and recognition schemes that would be strengthened through variable practice. The internal model deficit (IMD) hypothesis as an explanation for the motor deficits in children with DCD was reviewed by Adams, Lust, Wilson, and Steenbergen (2014); their conclusions pointed to a diffuse set of findings, with many caveats, and once again the call for yet more imaging studies:

> *"Whether the processing issue is more a question of purely mapping the motor commands from perceptual input, or predicting perceptual outcomes based on a given set of motor commands, or both, remains an important question for future work that may be advanced by neuroimaging methods." (p. 241, section 4.5, lines 8–12).*