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The dynamics of Motor development:
Commentary on Wimmers and Vereijken

Before I comment specifically on the work of these young Dutch motor development researchers, I would like to step back and look somewhat more broadly on the role of theory, models, and data in developmental psychology. These are heady times in our field. After several decades of the post-Piagetian doldrums, a period of often somewhat aimless data-collection, we are now involved again in lively theoretical debate. Witness this meeting, the recent publication books such as Elman et al's (1996) *Rethinking Innateness*, revived interest in neo-Piagetians such as Kurt Fischer and Robbie Case, the influence of cognitive science and cognitive neuroscience, and other strong challenges to old theory or no theory.

There is no question in my mind that dynamic systems has been an important player in this revival of the theoretical debate. Although the bulk of the influence so far has been in motor development, we can see with the important writings of van Geert (1994), van der Maas and Molenaar (1992) and their colleagues, that the theoretical position is attractive for cognition and other areas in development as well. So I welcome this opportunity to offer a few thoughts on where we stand and where we are going.

In Defense of Metaphor

There has been considerable grumbling that developmental psychology does not need any more metaphorical or 'verbal' theories, and that without strict formalizations we can make little further progress. I beg to disagree. The point of the first part of my commentary will be to situate the role of metaphor and to vigorously defend it. I will argue that indeed *metaphor comes first* and that without a good and consistent metaphor, formalizations are less helpful.

Metaphor is a good thing. I recently read Michael Ondaatje's *The English Patient*. Ondaatje is a spare and elegant writer, whose poetry of narrative is evocative without causing indigestion. He uses explicit metaphor sparingly, but one was especially memorable. In describing the course of World War II battles in Italy, he said, 'The thermometer of blood moved up the country.' With that simple statement, he condensed a thousand images.

But more than literary window dressing, metaphor may be one way that we actually think. As Gibbs (1996) recently wrote: '..many concepts, especially abstract

ones, are structured and mentally represented in terms of metaphor.' I think the sense here is that we can make sense of difficult and abstract concepts only through the association of the here and now with what we have experienced in the past, and often with concrete bodily experiences. Some of these associations are more novel and more creative than others. If you are good at metaphor, like Ondaatje, you can win the Booker Prize for literature, but if you are too good, you will be offered large doses of thorazine.

Metaphor, explicit or not, is at the very core of psychological inquiry. Consider that mental life is very, very complicated. And it is also totally hidden. We can watch how people behave, but we never know what is *really* going on inside. How are we to understand the workings of the mind?

The only way to get an entree into the fabled black box is through asking how the processes of mental life *are like* something else whose processes are somewhat more transparent. So before we begin any foray into data collection, formal models, or verbal theory we adopt what has been called a 'root metaphor.' What is it that human mental life *is like*? How can we begin to even wrap our brains around our brains?

One pervasive root metaphor is that people think and behave *as if* they were machines or electric circuits of various complexity. Certainly the lineage includes Sherrington, Pavlov, Watson, much of information processing and a great deal of cognitive science. The contrasting metaphor is an organic one; biological systems have properties that are fundamentally, essentially different than those of machines. In this group I would put Darwin, Baldwin, Gesell, Waddington, Piaget, Edelman, and certainly Paul van Geert's work on dynamic systems.

Talking about this dichotomy may be old stuff, but I think it is still worth making explicit. When you think about how the system works ... what comes to mind? Do you begin to think of processing stages, boxes and arrows, or flowing streams and eddies? How does Steve Pinker, for example, image an innate language 'device'? In what form is the universal grammar stored? Is this fundamentally different from van Geert's organic language 'growers'?

I think that getting your metaphor explicit and straight is the essential first step in generating good theory and in collecting good data. Indeed, I will go so far as to say that we run into trouble theoretically precisely because we 'mix our metaphors.' Mixing metaphors is right up there with dangling participles on the list of sins in 9th grade English. What if Michael Ondaatge had said 'the thermometer of blood boiled over,' he would have mixed his metaphors-confusing the image of a thermometer with that of a pot.

Let us look at Piaget as a case in point. I believe there is a fundamental incompatibility between the very organic metaphor of equilibration through accommodation and assimilation and the much more mechanistic assumptions of formal logical structure. In the first case, the image is of consuming and digesting food. Piaget even uses the world 'aliment' to describe the input to the system, something to eat, digest and absorb until it becomes part of the organism (Piaget, 1952). The issue then is how a process so organic leads to a mind that works as a logical machine. Piaget's solution was to say in effect that the organism comes to reflect the logical structure of the world. What is the organic counterpart of formal logic? How do we express the two

concepts in a single compatible language and what is their common image? What does the system look like while it is part one and part another? How does something like a logical structure consume something?

Why is getting the metaphor straight important?

In Figure 1, I show the relationship among the root metaphor, the over-arching theory, the model and the data. In this scheme, the meta-phor/meta-theory determine both the data to be collected and the models generated. There are two pathways that can be followed in doing this. One is from Metaphor-to data- and then to the model. It is a data driven approach and the one that my colleagues and I have scrupulously followed. We are now just now getting to the formal models (e.g. Thelen, Schöner, Scheier, & Smith, 1997). But even if we never approached the formal models, I defend this approach as long as the metaphor organizes existing data, generates testable and falsiflable hypotheses, makes contact with other levels of organization and generally helps us understand how the system works. When the models are developed, they will grow organically from a long dialogue between the metaphor and the data.

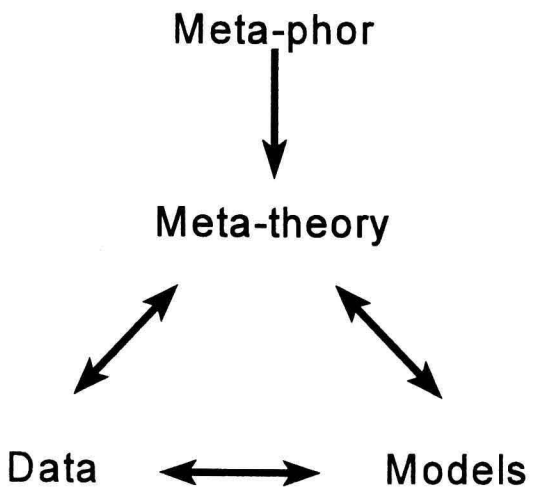

Fig. 1. Relationship between theory, model building and data.

I think this is why the work on bimanual coordination instituted by Kelso (1995) and his collaborators has been successful. These workers began with a phenomena, the spontaneous switching of coordination patterns, and quite thoroughly described that phenomena before they fit the data with an appropriate model, the Haken-Kelso-Bunz equations. The model generated predictions about the system that were tested experimentally. Presumably if the experiments had not confirmed the model

predictions, they would have changed the model, not looked for a different phenomenon.

The other pathway, from the metaphor to the model and then to the data, may be more problematic. First and most important, one can end up seeking a phenomenon that fits the model, a kind of search in data space for a fit to the formalism. The studies which use the cusp catastrophe model such as those involving conservation, as well as Raymond Wimmer's grasping study have this flavor: here is a model that looks like development, now let us find some behavior that we can fit the model to.

What is the developmental question in these cases? Is it: conservation or grasping is like a cusp catastrophe? What is at stake in the answer? If the model fits the data, does this mean that the developing organism is a dynamic system or like a dynamic system or like a dynamic system sometime, say with this particular task? If the model does not fit, do we then look for another phenomenon?

The real problem with this strategy, I believe, is when the model is inspired by one metaphor and the data have been collected by another. What do we make of data that have been collected to fit the assumptions of stage theory being massaged to fit the mathematics of dynamic systems? What kind of hybrid do we get? It is not to my taste to begin thinking about development with a shopping list of techniques, no matter how elegant.

So in this vein, my main question to Raymond Wimmers is 'What is at stake in showing that reaching with or without grasping can be modeled by a cusp catastrophe, *or by any other* model? What if the linear model had fit better — what would that mean for development? I have several other problems with the work, which I will only mention: First, the method that Wimmers used to determine whether infants reached without grasp or with grasp imposed an arbitrary criteria on what is a continuous behavior. Thus, there may have been intermediate forms (regions of inaccessibility in the cusp model) but we would never know it, because by definition the system must live in one mode or another. Second, even with this dichotomy imposed on the data, I am not convinced by any model fit that these are discontinuous transitions. In the second study, most of the babies are doing both forms of reaching and grasping in the same session. What are the implications of this finding? Perhaps that is what they actually do, and it is better to accept that they may not fit a model chosen for theoretical convenience.

I think, therefore, that Beatrix Vereijken's approach is inherently less problematic. She begins, as I believe we all should, with a developmental question, in this case, the role of cruising in the development of locomotion. She then maps the dynamics of the behaviors in question with careful continuous data variables. Hopefully, she will be able to model her results with some appropriate mathematics. But if that model does not materialize or the data are too noisy or non stationary or whatever, she will begin to answer the question of how the system might work. We would love the added precision of the model, but if that cannot be found, we may at least have good data.

I think model building is an important part of the triangle depicted in Figure 1. I know that the work that the Indiana group is doing with Gregor Schöner and Christian Schier on the A not B error has been enormously helpful in elevating the precision

of our thinking and in generating interesting experiments. But that is not an end in itself. Biology, for instance, has been enormously successful in understanding how organisms work largely without formal models, but with a consistent metaphor. We would be well-advised to seek similar levels of understanding by all the tools in our toolbox.

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