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Beyond the Metaphor ?

Review of:

A Dynamic Systems Approach to the Development: Applications (1993). Edited by Linda B. Smith and Esther Thelen. MIT press.

A Dynamic Systems Approach to the Development of Cognition and Action (1994) by Esther Thelen and Linda B. Smith. MIT press.

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Introduction

The recent interest in non-linear dynamic systems among, especially developmental, psychologists, has now cumulated in two impressive volumes by Esther Thelen and Linda B. Smith. This interest is justified. The study of non-linear dynamic systems has provided valuable new insights in disciplines like mathematics, physics, chemistry and biology. Furthermore, the theory of non-linear dynamic systems concerns a new conceptualization of change in complex self-organizing systems. Such a new theory is directly relevant to the study of psychological development.

The first volume, edited by Smith and Thelen, consists of 13 contributions of authors from diverse fields who apply the concepts and techniques of non-linear dynamic systems theory to specific research problems. In this format many articles have appeared in the last ten years. Each of these applications should be judged in relation to the models and techniques that are already used in the domain of application. In accordance with Thelen and Smith's terminology, I will call this the local application of non-linear dynamic systems theory.

The second volume on the other hand concerns the global application of non-linear dynamic systems theory. In this book Thelen and Smith propose a new theory of cognitive development, inspired by non-linear dynamic systems theory (mainly in the formulation of Haken, 1977) and theories of neural development (especially Edelman's (1987) Neural Darwinism). They explain their ideas in the context of several domains, like motor, cognitive and memory development and contrast their theory with both the Piagetian approach and the information processing approach. This book is the first attempt to formulate a general psychological developmental theory on the basis of non-linear dynamic systems theory.

In my judgement, the local applications of non-linear dynamic systems theory, as in Smith and Thelen (1993), though difficult, are often appropriate and successful. A careful review, however, would take many pages for each contribution. I focus here on the second book, not just for reasons of space but also because the second volume makes stronger and more

general claims. I think a strict analysis of the claims of Thelen and Smith is required.

Although there are many positive remarks possible, in this review I will pose some critical remarks. Eventually, my opinion is that Thelen and Smith's attempt to formulate a new general approach to the development of cognition and action is of great interest but not very successful.

To clarify my criticism I will first introduce the concepts of non-linear dynamic systems and neural development and give an impression of the progress in these fields. Second, I will summarize the approach of Thelen and Smith by discussing the questions they want to answer and the main points they make. Third, I will discuss my points of criticism which are organized in 7 arguments. Several of these points are also relevant to the local applications in the first book. In these cases I will refer to examples. Finally, in the last argument, I will focus on one of the local applications of Thelen and Smith themselves, concerning the famous A-not-B error.

Non-linear Dynamic systems theory

Over the last decade, non-linear dynamic systems theory (Jackson, 1989; Kelso, Ding, & Schöner, 1993) has received a lot of attention. It is, however, primarily a mathematical theory. Application to social science in general, where mostly non-mathematical models are used, is not straightforward.

Dynamic systems are systems changing over time. They are non-linear if their state depends in a non-linear fashion on their state at preceding time step(s). These time steps can be discrete or continuous as modeled by difference and differential equations, respectively. Non-linearity allows for the occurrence of radical phenomena, such as sudden phase shifts as a function of continuous changes in independent variables, deterministic but unpredictable chaotic behavior, and most interesting, self-organization or improbable spontaneous coherent behavior.

Phase shifts concern sudden qualitative changes in the equilibrium behavior of the system. Equilibrium behavior means that behavior is determined by attracting states, named, attractors. Equilibrium behavior depends on the type of attractor. Dynamic systems theory distinguishes for instance between point attractors, periodic attractors or limit cycles, and 'strange attractors' associated with chaos. Detailed information can be found in most introductory publications (Glass & Mackey, 1988; Kelso, Ding & Schöner, 1993; Prigogine & Stengers, 1984).

Phase shifts between attractor states have different forms, for instance Hopf bifurcations and a set of, so-called, catastrophes. One possibility, in the cusp catastrophe, is that behavior jumps to a new distant equilibrium when the old attractor disappears by smooth changes in the independent variables. One can distinguish three levels of application of, what is now called, bifurcation analysis. First, the mathematical analysis (Poston & Stewart, 1978) of the equations of the system involved, second, the modeling of bifurcations and, third, the empirical detection of bifurcations. The level of application depends on the knowledge of mathematical equations and state variables and the identification of control variables. Developmental psychologists can profit from the second and third level, the first level is almost always out of scope.

Deterministic chaos is another surprising discovery of non-linear dynamic systems theory. Deterministic non-linear dynamic systems can behave unpredictably, in fact in nearly random fashion. Sensitivity to initial conditions excludes long term prediction as a matter of principle. Theoretically this phenomenon is very important, yet, the empirical application of chaos theory is problematic. To distinguish deterministic chaos from noise requires long, dense and highly reliable time series. Robertson, Cohen, and Mayer-Kress (1993) discuss the technical problems of chaos detection in empirical time-series.

Self-organization is the ultimate promise of non-linear dynamic systems theory. It forms the core of Thelen and Smith's approach. Self-organization occurs in systems of many non-linear elements when macroscopic order emerges spontaneously, that is, without plan, algorithm or control structure. Famous examples are the Belousov-Zhabotinskii reaction and

the laser. In the first example, chemical reactions create macroscopic spatial structures like spiral waves. In the second, increased power leads to a sudden shift from random uncorrelated behavior to a phase locking of all atoms. In both examples novel structures are created without plan, blueprint or centralized commando.

In view of the old and common interest in novelty, creativity and insight, qualitative change, scientific revolutions, etc. this is quite a promise. If problems of homunculi and learning paradoxes can be solved by reference to self-organization, the theoretical advance is enormous. Thelen and Smith, among others, repeatedly refer to this possibility of dynamic systems theory. We are now at a point where we need evidence for this claim. A demonstration of functional self-organization in a psychologically relevant model (for instance, a neural network) would give significant and important support to this claim. In biology this step is made by, for instance, Boerlijst and Hogeweg (1991) who show how spiral waves deprive parasites in cellular automata models of hypercycles.

Neural Development

In addition to dynamic systems theory, Thelen and Smith base their approach on the theory of Edelman (1987, 1989). Edelman's theory concerns neural development. In this discipline strong progression has been made both in biological research and in computer simulation. The work of Edelman is one of the most interesting approaches to neural network modeling. Edelman's theory of neural group selection concerns selectionist principles applied to degenerate (redundant) and reentrant (intermodal) networks. The creation of these networks is based on embryological processes. Edelman's theoretical ideas are applied in computer models, called Darwin 2 and 3. Though Edelman's ideas are of great interest for developmental psychology, I hoped that Thelen and Smith would also refer to other neural network approaches.

It is important to have an overview of the broad field of computer simulation of developmental processes. The field of neural networks consists of many diverse approaches

(Grossberg, 1980; Rumelhart & McClelland, 1986; Kohonen, 1984; Hopfield, 1982; Rosenblatt, 1958, to mention a few well known). Both Edelman and Thelen and Smith reject the PDP approach of Rumelhart and McClelland (1986). As Smoliar (1991) points out, Edelman's (1989) argument is not convincing, "... while Edelman has been very concerned about biological support for his model, the computer models which he actually discusses are also, by his own admission, rather removed from their biological foundation " (1991, p. 315). It is true that Edelman's approach has very specific features and advantages, but an exclusive choice for one of the neural network approaches seems premature to me.

In fact the field of alternative, with respect to traditional artificial intelligence (AI), simulation approaches is much broader than neural network modeling. First, there are the 'modern' AI models, that with moderate success are able to learn (Michalski, 1987; Cohen & Feigenbaum, 1982, Ellman, 1989; Lenat & Brown, 1984). In developmental psychology the attempt of Klahr (1989) to construct self-modifying information processing models is well-known. Also, Boden (1990; 1994) gives an overview of computer models for creativity. Second, the field of genetic algorithms or evolutionary computation has made strong progression (Goldberg, 1989). This 'optimatization' technique simulates evolution of solutions (mostly sets of parameter values) for a large class of problems. In contrast to neural network approaches it can be very effective for typical AI tasks. It can be combined with other approaches to find, for instance, optimal neural networks (see Happel & Murre, 1994, for an overview). It is a selectionist approach by definition, and in combination with neural networks, very much in accord with Edelman's theoretical ideas. Third, there is the field of Artificial Life and Cellular Automata models. This biologically oriented approach is of interest, for instance because it concentrates on self-organizing functional structures, like macroscopic information exchange, in spatial parallel systems of low level simple units with local connections (Langton, 1990). There are no strict borders between these approaches. Some recent proposals, like Sims (1994), Holland, Holyoak, Nisbett, & Thagard (1989), Mitchell, & Hofstadter (1990), Brooks (1991), Ray (1991), and Minsky (1986) combine various features, or form new fields (like robot research).

This enormous variety of alternative approaches for the simulation of the development of perception, action and cognition, asks for criteria. How do we judge these models ? In cognitive developmental psychology a fundamental problem is the learning paradox of Fodor (1980). Based on Raijmakers & Molenaar (1994) I suggest four criteria for models that are meant a) to solve the learning paradox by self-organization (show qualitative development) b) to construct the bridge between the neural and the symbolic level. Each of these criteria is met by one or more simulation models but none of the current models can fulfill all criteria.

1) Self-organization is associated with the occurrence of phase transitions. Phase transitions can be tested by the criteria of catastrophe theory (van der Maas & Molenaar, 1994).

2) These phase transitions should have a constructive effect on performance. In most simulation models phase transitions are avoided because they generally destroy the functional behavior. It is difficult to show a self-organizing phase transition from incorrect to correct behavior though such examples are known for biological processes.

3) The model should be plausible from a biological perspective. This point is well discussed in, for instance, Thelen and Smith's book.

4) The model should be plausible from a cognitive psychological perspective as will be explained in the sections on symbols and higher cognitive functioning. In general, the simulation models should explain the data of cognitive developmental studies. The models should concern the learning of a cognitive task, for instance the Piagetian tasks or typical AI puzzles.

A good example of the application of the fourth criterion is found in Raijmakers, van Koten and Molenaar (1995). They propose to use a very simple, well studied, task: discrimination shift learning (Kendler, 1979). In this task the feedback is changed when after some trials two-dimensional stimuli are correctly discriminated. In the optional shift variant of the task subjects can make one of two switches, the extra-dimensional switch or the reversal shift. The first is in accord with stimulus-response (or behaviorist) models, the second is typical for concept-mediating (or cognitive) models. Young children as well as animals prefer

the extra-dimensional shift, whereas adults and children older than 6 years prefer the reversal shift. It is very easy to apply neural network models to this task. Raijmakers, van Koten and Molenaar (1995) have shown that the PDP type models fail this criterion (see also Kruschke, 1992). They make the extra-dimensional shift. How other simulation models behave on this task remains to be investigated. It would be very interesting to test how Edelman's models, Darwin 2 and 3, perform.

A Dynamic Systems Approach

Developmental psychologists are faced with rapid progress in disciplines that are, almost by definition, very relevant to developmental psychology. As an implication, an important question is how we can integrate the new results in these disciplines with our current work ? I agree with Thelen and Smith that we may not ignore this question. We can all learn a lot from their attempt to find a general new approach to the development of cognition and action.

What is it that Thelen and Smith propose ? They give a list of questions they want to answer. They want to understand the origins of novelty, to reconcile global regularities with local variability, complexity, and context-specificity, to integrate developmental data at many levels of explanation, to provide a biologically plausible yet non reductionist account of the development of behavior, to understand how local processes lead to global outcomes, and to establish a theoretical basis for generating and interpreting empirical research (1994, p. XVIII). From this list, and also from their title, we can safely conclude that Thelen and Smith want to propose a new theory of psychological development. They want to formulate a new alternative for the existing theories, especially structural theories.

Unfortunately, their alternative is not very concrete. Although non-linear dynamic systems theory concerns mainly mathematics, mathematical models are not discussed. And although the study of neural development results in concrete 'implementable' models, no such models are applied or simulated. The approach of Thelen and Smith can best be summarized by some statements: (1) knowledge is context dependent and acquired by interaction of perception and

action, (2) development and knowledge are dynamic and not static, (3) there are no rules, symbols and central processors, (4) domain specific variability is important, not domain general structures, (5) there is no end state, (6) the concept of competence is misleading, there is no context independent performance, (7) development is selection and not construction, (8) cognition is activity driven, reentrant and high-dimensional. According to Thelen and Smith their theory is related to the Vygotskian view, the organismic view and the ecological approach. It differs from the Piagetian theory with respect to the domain-specific variability and to the rejection of an end-state and structures. It differs importantly from the information processing approach because it denies symbols, rules, central processors and stresses development.

Methodologically, they propose (1) to look at high variance during transitions, (2) to perform intensive longitudinal experiments, (3) to apply perturbation studies to test the stability of states, and (4) to draw hypothetical trajectories in hypothetical state spaces. They explain their ideas and these techniques for a number of phenomena in early childhood development. I will review their model for the A-not-B error in more detail at the end of the article.

Metaphors and Explanations

The title of this review, beyond the metaphor, is adapted from the title of one of the chapters in the Smith and Thelen volume, written by Robertson, Cohen, and Mayer-Kress (1993). This chapter concerns behavioral chaos in cyclic motor activity. It contains an excellent discussion of the application of non-linear dynamic systems theory to psychological development. They explain their title as follows:

There is a danger, however, in this new fascination with dynamic systems theory. The danger lies in the temptation to naively adopt a new terminology or set of metaphors, to merely redescribe the phenomena we have been studying for so long, and then

conclude that we have explained them (Robertson, Cohen, and Mayer-Kress, 1993, p.119).

Robertson et al. (1993) suggest to focus on rigorous local applications in which we acknowledge the constraints of the level of analysis and the limitation of our measurements. " Ultimately, the goal should be to build a dynamic model of the mechanism controlling the observed phenomenon from which new observable properties can be deduced and subjected to experimental test" (Robertson et al., 1993, p. 120). This seems a very reasonable view. The first step is verbal redescription, that is the metaphorical level. The second step is a formal redescription (by mathematical, statistical and simulation models). But still a formal redescription is not enough. The final step concerns testing. The model must be falsifiable, implying non-trivial empirical predictions, and non-trivial fits to data.

At this moment we are somewhere between step one and two, between verbal and formal redescription. Some local applications generate testable predictions which distinguish non-linear dynamic models from other existing models for the phenomenon involved. An example is the prediction of hysteresis in transition research. In my opinion formal redescription in itself is very important. Theories in developmental psychology are mainly verbal, with all its consequences (what Thelen and Smith call a crisis in cognitive development). Currently, introductory books discuss the Piagetian theory, a number of neo-Piagetian theories, information-processing, contextual, ecological, nativistic and other theories. In general, they all lack testability and clarity. Of course there have always been researchers struggling to translate vague descriptions into testable formulations. They have focused on 'what can be said' on the basis of the current quality of our measurements and available statistical techniques. This point of view is essential (see Brainerd's (1978) criticism of the Piagetian theory). Therefore, application of non-linear dynamic models and techniques should take place on this technical level.

Thelen and Smith clearly do not work from this thrifty perspective. In their view a new metaphor needs a new verbal theory which adds one more to the list of current approaches. I will argue that we do not need a new verbal theory. The new metaphor is amazingly consistent

with the main theory, the Piagetian, especially where it concerns equilibration and stage transitions. The new metaphor is in agreement with many basic notions. Take for instance chaos and divergence. It has been intuitively clear to many psychologists that development can be strongly non-linear and diverging. By non-linear dynamic systems theory we can make this understandable (by the metaphor) but, I hope, also better described and tested (by models and new techniques).

My general judgement of Thelen and Smith's work is that it fails to make step two and three. I will further explain this judgement on the basis of my concrete points of criticism: the rejection of symbols, rules and central processors, the disregard of higher cognitive functioning, the rejection of the construction of structures, the absence of a point of view on the relation between deterministic dynamic systems and stochasticity, the absence of a real attempt to integrate dynamic systems theory with developmental theory, the omission of discussion of other attempts to apply non-linear dynamic systems theory, and the lack of mathematical or simulation models.

The Rejection of Symbols, Rules and Central Processors

Thelen and Smith categorically reject machine analogies of cognition and development and think that although behavior and development appear rule-driven, there are no rules (1994, xix). In concert with Edelman (who throws a computer out of the window to illustrate his point of view), Thelen and Smith reject the computer metaphor by creating a caricature of AI, sometimes called traditional AI. They see a biologically inspired theory of cognition as a radical departure from current cognitive theory.

However, traditional AI is not current AI. At this moment I do not know one author in developmental psychology who defends that cognition is static. The information processing theories in developmental psychology are different from this traditional mechanistic view (see Klahr, 1989, on self-modification). The point that biology is important to developmental

psychology is made by many authors, for instance in the discussion on connectionism and AI (e.g., Bates and Elman, 1993). The question now is how this point can be evidenced.

I can agree with the view that symbols correspond to or are related to attractor states in the neural networks (Amit, 1989). But I can not agree that they do not exist. Thelen and Smith state for instance that weaving consists of complex interactions across many neuronal groups and that thinking of weaving also consists of (other) interactions among neuronal groups in real time. In view of the current knowledge this is probably the most reasonable assumption, but it does not solve the problem of representation and symbolic thought.

Suppose I would make a plan to make money involving many activities of which one is weaving. In the computation of the loan I use weaving as a symbol in the typical human activity of reasoning. I then have the puzzling possibility of introspection and I suffer from a limited short term memory. According to Thelen and Smith current cognitive theories can not explain this without creating a ghost in the machine. But what Thelen and Smith offer as an alternative is not convincing either. Thelen and Smith say that introspection is a behavior like any behavior of this complex system. They never discuss short term memory and the neo-Piagetian developmental theories that are based on the idea that the increased capacity of this memory is the control parameter of many important developmental transitions. Their alternative is nothing more than the statement that knowing is emergent, which means only that we do not understand it (Aslin, 1993). In sum, higher cognitive functioning requires additional explanations. "Viewing cognition as all one kind, as all embodied, all distributed, all activity and all a complex event in time" (Thelen and Smith, p. 337, 1994), is not sufficient.

The Disregard of Higher Cognitive Functioning

The problem is that all present neural networks fail as models of higher cognitive functioning. Neural networks are of great interest because of their non-linear dynamics, distributed memory, parallel computation, correspondence to brain structures, and categorization and recognition ability. However, at present they can not replace AI. AI refers

to other types of functioning. It is important to notice that this other type of functioning, higher cognitive functioning, is what we are really interested in.

Thelen and Smith construct a theory of cognitive development based on dynamic systems and theories of neural development but do not apply their theory to 'real' cognition. Their examples concern early childhood action, perception and categorization. Indeed, these are the starting points for higher cognitive functioning, but they are not typically human. A theory of cognitive development should refer to more than the sensorimotor activities. It is too simplistic to reject symbols and rules when development of conservation or formal operational abilities are left out of the discussion.

Dynamic systems theory should not lead to a traditional behaviorist point of view. I'm sure Thelen and Smith do not want to propose such a theory. Yet most neural networks do probably not perform above the level of operant and classical conditioning (Raijmakers, van Koten & Molenaar, 1994). Simply stated, the problem is that rats have all the properties required according to Smith and Thelen (non-linear dynamic reentrant neural networks) but are unable to show higher cognitive behavior.

Interestingly, Pinker and Prince (1989) have given this same type of criticism in their analysis of the claims on language acquisition of Rumelhart and McClelland (1986). According to Rumelhart and McClelland their PDP model shows rule-like behavior without containing rules (but see Reese, 1989). McClelland and Jenkins (1991) make the same claim for their PDP model of balance scale learning. Although the model clearly lacks symbol-processing rules it appears to simulate the saltatory acquisition of the rule-systems proposed by Siegler (1981). These claims have been investigated with stronger criteria (Raijmakers, van Koten & Molenaar, 1995; Jansen & van der Maas, 1995). By use of the transition criteria of catastrophe theory the authors show that the shift from, for instance, rule 1 to rule 2 is not saltatory but continuous. By using latent class analysis, on both empirical data and simulation data of the PDP model, they show that the responses of the PDP model, in contrast to the responses of children, are not rule governed.

In my opinion one of the hardest problems is to bridge the neural level of modeling and the symbol-processing level. A simple rejection of the latter is not convincing. I fully agree with Pinker and Prince (1989) as they state:

Neuroscientists study firing rates, excitation, inhibition, and plasticity; cognitive scientists study rules, representations, and symbol systems. Although it is relatively easy to imagine ways to run cognitive symbol systems on digital computers, how they could be implemented in neural hardware remains obscure. Conversely, it is easy to get neural networks to execute simple forms of associative learning, but learning a language or engaging in logical reasoning is quite another thing. Any theory of the middle level faces a formidable set of criteria: it must satisfy the constraints of neurophysiology and neuroanatomy, yet supply the right kind of computational power to serve as a basis for cognition (p. 182).

The Rejection of the Construction of Structures

I have a similar problem with the rejection of structures and especially the rejection of the Piagetian concept of construction (p. 130, 1994). Piaget's theory is dynamic (equilibration), epigenetic (like Edelman's theory) and stresses the importance of the interaction of motor behavior and perception as a starting point for cognition. Thelen and Smith acknowledge this but stress that the Piagetian theory fails to capture the complexity and messiness of cognitive development in detail and incorrectly assumes underlying logicomathematical structures.

I'm not convinced that Piaget was correct on a global level and wrong on the local level, as Thelen and Smith put it. First, most authors agree that the 'décalages horizontales' constitute a major problem for the Piagetian theory (see for instance, Kreitler & Kreitler, 1989). These 'décalages horizontales' seem to be an aspect of the global organization of cognition (almost all children learn conservation of number before conservation of volume). Second, the concept of disequilibrium allows locally for variability and messiness (the transition phase within each domain of conservation learning). A comparison between current developmental theories and

aspects of non-linear dynamic systems theory is much more complex (Lewis, 1994; Beek, Hopkins, & Molenaar, 1993).

Indeed the logicomathematical structures have never met with much enthusiasm. Yet, structures of some kind, or at least an organization of knowledge, seem required to understand higher cognitive functioning. In part, development concerns generalization, transfer, etc.. Thelen and Smith introduce an alternative for the concept of structures. They state, "We differ, however, in our fundamental view of mental activity as a dynamic assembly rather than a hierarchy of structures, and in seeing the process of development as one of selection rather than construction " (1994, p. 130). On first sight, Thelen and Smith simply replace the concept of structure by the concept of assembly. It seems to me that assemblies may be built of lower order assemblies, hence assemblies can be hierarchically organized. That these assemblies are dynamic and emergent is not in contradiction with Piaget.

However, Smith and Thelen go further. These assemblies do not exist outside the task context (p.310). They say, "Solutions are always soft-assembled, and thus are both constrained by subjects' current intrinsic dynamics and potentially derailed or redirected by task conditions " (1994, p.311). I think this is an unsatisfactory view. Older children and adults simply do have context independent knowledge. They can do cognitive tasks, know that they can do the tasks and sometimes know how they do the task. If someone would ask me if I can weave, I do not need to try, I know I can't. I do not need to be in a specific task context to check it. It is simply not so easy to reject the competence performance distinction (assuming that this rejection is desirable, which I doubt).

Thelen and Smith (1993) have a second argument to distinguish between Piagetian structures and dynamic assemblies. In the first volume they say about their structures, "The structures are product of local processes, not the causes of them " (p. 162). In spite of their rejection of reductionism, to me this is a reductionist view. The rejection of reductionism is only correct if structures influence the local processes that created them and initiate other local processes (see for instance, Kaneko and Tsuda, 1994). This is what self-organization is about. The local processes are not more real than the emergent structures. One reason for this

statement is that the local processes themselves are macroscopic structures of lower order local processes.

The relation between the processes of construction and selection is very difficult to understand. First, self-organization, the promise of dynamic systems theory, seems more related to the Piagetian concept of construction than to Edelman's concept of selection. Second, in biology, the relation between selection and self-organization is known to be complex (Kauffman, 1993). According to Kauffman, selection is not sufficient to evolve complex organisms. Dynamic properties, like self-organization, are required too, which can cooperate but also compete with natural selection. I fully agree that selection is an important developmental mechanism but disagree that it is the only one. In Thelen and Smith's approach the role of dynamic processes is limited to the creation of variability on which selection can operate. However, to this end, random processes, such as mutation in biological evolution, may suffice.

All together, the point of view of Thelen and Smith is close to behaviorism. An approach based on learning by selection and on the rejection of structures, internal rules and symbols, bears strong resemblance to operant conditioning theories. For instance, Thelen and Smith completely ignore the possibility of learning by instruction. There are cases where someone tells me a logical rule and where I can solve a task on basis of this symbolic knowledge. A rejection of traditional AI must not lead to a rejection of cognition itself. Indeed, dynamics, biology, and development are important. Indeed, there is more than symbolic thought. In the end cognition is based on complex dynamic neural processes. But cognition exists. We can sometimes reason in abstract formal ways. We have organized structures of knowledge on, for instance, multiplication, which we can communicate about in symbols without actually performing multiplication itself.

In their book Thelen and Smith focus on early childhood development. The problem is that the 'cognitive' abilities they discuss are not typical human. Kicking mobiles, solving the A-not-B task, and object segregation may not need the construction of cognitive knowledge (structures) but this does not generalize to playing chess.

The Absence of Point of View on the Relation between Deterministic
Dynamic Systems and Stochasticity

A major question for dynamic system approaches is how to distinguish between measurement error and dynamic variability (Bogartz, 1994; van Geert, 1991). Dynamic systems theory concerns mainly deterministic systems. However current models allow for at least two stochastic elements. The first is stochasticity in the system or environment, the second is measurement error. Recognition of these elements have been important for the construction of statistical tests and statistical models. Translation of deterministic dynamic models to stochastic models is not straightforward although some progression has been made (see Cobb & Zacks, 1985; Hartelman, van der Maas, & Molenaar, 1995).

In my view Thelen and Smith are somewhat naive on this. They encourage readers to reach in their file cabinets where they store the studies they did not publish because their ANOVA's did not detect significant effects. Readers should think dynamically and use the variability in the data (p.342, 1994). It is true that noise can be chaos (Robertson, et al., 1993; Cooney & Troyer, 1994) and that high standard deviations are associated with critical fluctuation or anomalous variance near transition points. Yet in most cases noise is noise (or chaos + noise which is practically indistinguishable from noise) and error is measurement error.

More important, and perhaps as a consequence, Thelen and Smith do not discuss or apply any statistical methods. How do we statistically test for transitions and high variance. In the lessons from learning to walk (chapter 1, 1994) this is not explained. Individual growth curves are shown but not tested. The same is true for variance. With respect to critical fluctuation or anomalous variance, my guess is that we should fit quadratic regression lines to test whether variance is highest at transition points. Another example is the chapter of Goldfield (1993) in the first volume. Though the description of crawling in dynamic terms is very interesting, the final test is unsatisfactory. Goldfield (1993) applies a scalogram method to observed stage patterns. He extends the number of allowed patterns to acquire a fit. Yet a

test of homogeneity on the data (in table 2.4) shows that independence (the null hypothesis) holds ($\chi^2(11, N=90) = 12.05, p > .5$). In such a case we can hardly conclude that there are interesting individual paths or dynamic variability.

Furthermore, Thelen and Smith do not discuss how their treatment of longitudinal data relates to rather successful statistical developmental models based on Markov chains and covariance structure modelling. Bogartz (1994) discussed this point in more detail. A related issue is how we can detect attractors with our current statistical techniques. Attractors or modes in 'dynamic landscapes' relate to modes or peaks in multimodal statistical distributions. Therefore, I think the answer is found in techniques for clusters analysis, finite mixture analysis as found in Thomas (1989) and in applications of latent class analysis (Rindskopf, 1987). Combined with longitudinal techniques this means that an application of the mixed Markov latent class model (Langeheine, 1994; Van der Pol, Langeheine, de Jong, 1991) is very much in agreement with the dynamic approach. In my opinion, such a relation between dynamic concepts and statistical ones has a strong integrative effect (van der Maas, 1995).

The Absence of a Real Attempt to Integrate Dynamic System Theory with Developmental Theory

Thelen and Smith do not put much effort into trying to translate dynamic concepts to standard notions in developmental psychology. They say that their work relates to the Vygotskian and the ecological approach but don't show how. But there are other links too. In the view of neo-Piagetians and others cognition is dynamic, organismic, interactive, started from the interaction of action and perception, and so on. Furthermore, as Aslin (1993, p.387) notices, an emphasis on longitudinal analysis is hardly unique (see for instance, Hoppe-Graff (1989) and Siegler and Jenkins' (1989) argument for microgenetic studies). Another example is the stress on variability during transitions. In Piaget's theory there is disequilibrium and in his research the search for inconsistent verbal explanations, in Flavell and Wohlwill (1969) the search for oscillations, and more recently, in Breckinridge Church and Goldin-Meadow

(1986), the search for gesture-speech mismatches. Many more examples can be given. In my opinion this means that the concept of anomalous variance (or critical fluctuation) has been intuitively clear to many researchers. By the dynamic system approach this intuition can be made formal.

Aslin (p. 386, 1993) and Bogartz (1994) complain that proponents of dynamic systems theory rarely illustrate these historical links, thereby implying that the terminology and the underlying concepts are original. I agree on this subject. Aslin proposes three translations: attractor to stage of development, control parameter to independent variable, limit cycle to constraint. I may add that we already know the state variable as dependent variable, perturbation as counter suggestion or random feedback, critical slowing down as reaction time (see van Loocke, 1991) and instability as disequilibrium. Divergence occurs in processes like the self-fulfilling prophecy and we know reentrant simply as multivariate.

Take for instance the perturbation studies. The idea is that the stability of attractor states can be studied by perturbations which are especially informative if the system is near a transition point. According to Thelen and Smith, in research on motor development the application of perturbations is very successful (1994, p. 87). But the idea itself is not new. Piaget applied perturbations in his famous conservation studies in the form of countersuggestion. When a child gave the correct response he or she was faced with the incorrect argumentation. When the incorrect response was given, the correct argumentation was presented. In this way, Piaget tested the stability of the current state. Instability, e.g. sensitivity to countersuggestion, was used as an index for the transition state. As far as I can see this procedure is very consistent with the perturbation approach in dynamic systems theory.

The Omission of Discussion of Other Attempts to Apply

Non-linear Dynamic systems theory

A related point of criticism concerns the lack of references to other attempts to apply neural networks and non-linear dynamic systems theory to developmental psychology. I already mentioned some of the other neural network approaches. Some of these approaches are directly relevant to the work of Thelen and Smith (Bullock, Grossberg, & Guenther, 1993; Schuster & Wagner, 1990; McClelland & Jenkins, 1991). Thelen and Smith reject connectionism on failures that apply to all models including Edelman's theory and their own dynamic approach: there are no models that take all information on brain, biological processes, heterogeneity of development, and development seriously. What counts are not the intentions of the modeler but the model itself. Thelen and Smith do not discuss nor test the models of Edelman, Darwin 2 and 3, whereas an application of Edelman's theory requires simulation of these models.

In the last decade many authors have proposed to apply aspects of non-linear dynamic systems theory to psychological development (Thatcher, 1991; Keating, 1990; Lewis, 1994; Garcia, 1992; Molenaar, 1986). Thelen and Smith do not discuss the applications in the first volume. Take, for instance, the chapter of van Geert (1993). Van Geert proposes concrete non-linear difference equations for the growth of all kinds of cognitive abilities. How does this relate to Thelen and Smith's approach ?

Prigogine and Stengers' (1984) non-equilibrium thermodynamics, Haken's (1977) synergetics and Thom's (1975) catastrophe theory are related in various ways. Thelen and Smith are focused on Haken's synergetics. But what do they think of Garcia's (1993) application of Prigogine's ideas to the theory of Piaget ? What about catastrophe theory ?

Saari (1977) already made the first attempt to apply catastrophe theory to cognitive developmental transitions. The potential functions associated with the so-called cusp catastrophe are exactly equal to Thelen and Smith's dynamic landscapes. Catastrophe theory provides some additional criteria for the detection of transitions (e.g. hysteresis, critical slowing down), and shows that in some cases dynamic systems theory can be applied without knowledge of the formulas governing the system.

On the other hand, catastrophe theory is neutral with respect to the direction of change. Haken's theory concerns transitions to higher organized states, whereas catastrophe theory fits destructive transitions as well as constructive transitions. The advantage of catastrophe theory is that statistical theory has been developed (Cobb & Zacks, 1985). There are, for instance, tested cusp models for multimodal perception (Ta'eed, Ta'eed & Wright, 1988) that are directly relevant for the claims of Thelen and Smith. Our own work on catastrophe modeling of conservation development is an example of an application to cognitive development (van der Maas & Molenaar, 1992).

The Lack of Mathematical or Simulation Models

The last criticism concerns the vagueness of the approach of Thelen and Smith. I already complained about the lack of mathematical and simulation models. Although I agree with Thelen and Smith that self-organization in non-linear dynamic systems constitutes a new possibility to understand cognitive development, the promise remains a promise. We need concrete mathematical models of domain-specific self-organizing psychological systems, simulation results of related computer models and strong empirical evidence. The concrete models of Thelen and Smith are not convincing.

Thelen and Smith give 'local' models for various early childhood developmental phenomena, among them, the famous A-not-B error. The A-not-B error occurs when nine month old babies search for an object at location A, where it was hidden on the former trials, whereas they saw that the experimenter put the object at location B at the present trial. Babies younger than nine months do not search for hidden objects at all, whereas older babies successfully search at location B. This error is apparently a transitional property. Piaget asserted that children make the error because they do not represent the object as continuing in space and time independently of their own perceptions and actions (Thelen and Smith, 1994, p.280). According to Thelen and Smith, current explanations (the child knows that the object is at location B but can not inhibit the trained response of searching at location A), are

insufficient too. Reference to maturation of the frontal lobes and corresponding development of inhibitory mechanisms (e.g. Diamond, 1990) does not constitute a real explanation to them.

What then is the explanation of Thelen and Smith ? They provide a list of sequential events and translate these in terms of hypothetical trajectories in hypothetical three-dimensional state spaces. These trajectories are attractors that gain strength when they are repeated. In this case the idea is that when the interesting toy is hidden at location B, the trajectories intersect at the moment the child has to start the search, and is then pulled into the deeper A trajectory. The event that brings the trajectories together is the hand of the experimenter moving back from B to the center between A and B. One could doubt whether this is correct. Why does this event concern the hand whereas all other events refer to the toy ? Thelen and Smith assume that children look at movement, but this is an additional assumption. I put so much emphasis on this event because it is the only essential event in Thelen and Smith's model (it is the moment at which the child is going to make the error). The choice of the other events, the number and kind of dimensions, the choices of numerical values on these dimensions, are all arbitrary and unimportant.

That the dominant action, searching at A, attracts, is consistent with neural network approaches, behaviorist theories, and Diamond's approach. That memory, at least partly, concerns attracting states in network activities, is not a revolutionary point of view, and does not solve all problems. That children, if they are allowed to search for the toy directly without delay, perform better, is not explained in a new way by a statement as: "the representational momentum of the phase B trajectory is sufficient" (Thelen and Smith, 1994, p.293). This is merely a useless redescription of the memory explanation. That children will perform better if the hand of the experimenter stays at location B after the hiding of the toy, is not a unique prediction. To me it seems in agreement with common sense. That children perform better when there are multiple hiding locations because the other locations serve as a reminder, does not follow from the dynamic system approach.

I really do not see what is new on this type of explanation. Thelen and Smith are rather convinced that it is new. About their model they state:

In contrast, our dynamic systems account answers questions of process and cause. In our account 'inhibition' is realized in specific tasks when two attractors are separated by sufficiently high ridges so that a trajectory in one task context is distinct from the trajectory in another task context. In our account, there is not some internal ability called 'inhibition' that causes the performance; rather inhibition is the product of a system in a particular context with particular dynamic properties (Thelen and Smith, 1994, p. 308).

They are very critical on Diamond's (1990) approach, "there are both strong empirical and theoretical reasons to reject Diamond's defence of maturation "(Thelen and Smith, 1994, p.309). However, theoretically, maturation can be a control variable in a dynamic system description. Compare maturation with, for instance, the power of the laser of Haken, and with the input of reactants in case of the Belousov-Zhabotinskii reaction. Furthermore, their own model, particularly the hypothetical trajectories in state space can not be tested. How do we measure the relevant variables, how do we access the fit of data to the predicted trajectories ? These are questions not addressed by Thelen and Smith.

Finally, when Thelen and Smith reject Diamond's explanation they get close to Piaget's original explanation. Thelen and Smith conclude that there is no point in saying that children know something (competence) but are hampered by the task conditions. That is, knowing does not exist independent of the context specific actions and perceptions. Amazingly, this is exactly Piaget's explanation of the A-not-B error.

Thelen and Smith apply this idea of attracting trajectories also to a number of habituation paradigms in early childhood research. In the context of their model for object segregation they admit that, "This account is, of course, wholly fictional and begs the question why the solid rod generates a trajectory that is more similar to the habituating trajectory than the broken rod. ... The answer to this question requires specific investigations of the mechanisms of the hypothesized heterogeneous systems" (1994, p. 177). But at the same page they state,

"Indeed, although we have no empirical evidence or computational simulation of the plausibility of our 'account' of Kellman and Spelke's findings, we nonetheless believe that our account offers a better explanation than any other account" (1994, p. 177). To claim that a wholly fictional account without evidence or simulation offers the best account of such a well studied psychological phenomenon, might be on the tentative side.

A more modest attempt to integrate the various views on the A-not-B error and object segregation might be more fruitful. Without computer models (does Darwin 3 make the A-not-B error, do other network models make this error ?) it makes no sense to exchange one vague description for another.

Such an attempt would probably focus on other aspects of the A-not-B error. It demarcates a stage between less and more mature understanding of object permanence (1994, p. 280), therefore it might be a typical transitional property. An appropriate application of non-linear dynamic systems theory to the A-not-B error would be to model and test this transition (as Thelen did for walking). One could construct a cusp model wherein the likelihood of reaching to A versus B (or looking time in the habituation studies) is the state variable. Maturation, memory, inhibition, interest factor of the toy, ability to reach, distinctiveness of the locations, the search delay, and the number of habituation trials, are all candidates for control variables. To test whether the acquisition of object permanence constitutes a real transition we can apply the criteria of catastrophe theory (which include the criteria of synergetics).

One can look for sudden jumps, bimodality, inaccessibility, divergence, hysteresis, divergence of linear response, critical slowing down, and anomalous variance (Gilmore, 1981; van der Maas & Molenaar, 1992). Because most of these criteria require reliable measurements of the state variable, the first efforts need to concern measurement. Then the role of each of the possible control variables need to be investigated. Finally, a concrete cusp model can be constructed and tested by the statistical fit of stochastic cusp models (Cobb & Zacks, 1985). Such an account would mostly complement current theory and models. It leads

to a more formal and testable redescription of current ideas on the transition to object permanence.

Conclusion

Developmental psychology is faced with rapid progression in related fields, especially in the study of neural development and in the study of complex dynamic systems. We can not neglect these developments. But do we need a completely new theory ? Is it necessary to reject the information processing metaphor, the Piagetian approach and other structural theories, and start all over ?

I hope that this discussion of Thelen and Smith's work shows that such a step is premature. In my view, it is misleading to view dynamic systems theory as a new alternative psychological theory. A theory of dynamics should be integrated with current methodological and theoretical approaches. It might lead to a formal and testable redescription of, among others, the Piagetian account of equilibration, and thereby to a real explanation.

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