BEHAVIORAL AND BRAIN SCIENCES (1997) **20,** 351–369
Printed in the United States of America

Continuing Commentary

Commentary on **Annette Karmiloff-Smith (1994). Précis of *Beyond modularity: A developmental perspective on cognitive science*. BBS 17:693–745.**

Abstract of the original article: *Beyond modularity* attempts a synthesis of Fodor's anticonstructivist nativism and Piaget's antinativist constructivism. Contra Fodor, I argue that: (1) the study of cognitive development is essential to cognitive science, (2) the module/central processing dichotomy is too rigid, and (3) the mind does not begin with prespecified modules; rather, development involves a gradual process of "modularization." Contra Piaget, I argue that: (1) development rarely involves stagelike domain-general change and (2) domain-specific predispositions give development a small but significant kickstart by focusing the infant's attention on proprietary inputs. Development does not stop at efficient learning. A fundamental aspect of human development ("representational redescription") is the hypothesized process by which information that is *in* a cognitive system becomes progressively explicit knowledge *to* that system. Development thus involves two complementary processes of progressive modularization and progressive "explicitation." Empirical findings on the child as linguist, physicist, mathematician, psychologist, and notator are discussed in support of the theoretical framework. Each chapter concentrates first on the initial state of the infant mind/brain and on subsequent domain-specific learning in infancy and early childhood. It then goes on to explore data on older children's problem solving and theory building, with particular focus on evolving cognitive flexibility. Emphasis is placed throughout on the status of representations underlying different capacities and on the multiple levels at which knowledge is stored and accessible. Finally, consideration is given to the need for more formal developmental models, and a comparison is made between representational redescription and connectionist simulations of development. In conclusion, I consider what is special about human cognition by speculating on the status of representations underlying the structure of behavior in other species.

How far beyond modularity?

Luca Bonatti
Laboratoire des Sciences Cognitives et Psycholinguistique, 75006 Paris, France. **wca@cogito.lscp.ehess.fr**

Abstract: I question (1) whether Karmiloff-Smith's (1994a,r) criticisms of modularity hit the target and (2) how much better the representational redescription model is. In both cases, "the mystery of the cognitive clock" is problematic for her account.

Fodor's (1983; see also multiple book review of Fodor's "The modularity of mind" in *BBS* 8 (1) 1985) modularity thesis (M) is a central point of reference for cognitive scientists, but it is not dogma. There are at least two critical attitudes toward it. One is fairly simple: it consists in showing that M is false for some core domains. This can be done by providing evidence that, in an allegedly modular domain, information flows freely at any moment during processing. For example, if contextual information is always accessible on line during lexical access or syntactic processing, then M is false for these domains. Many people have raised doubts about the truth of M in specific cases; the debate is still open and fruitful.

Another, more ambitious, way is to offer an alternative or a more comprehensive framework that explains data and intuitions in favor of M and also covers phenomena that M leaves unexplained. Karmiloff-Smith (1992) in *Beyond modularity,* has taken the second route. It is not unimportant to determine whether her attempt is correct, because, as some have argued, if it were viable it might have substantial consequences in philosophy, in psychology (see Dartnall 1994; Losonsky 1994), and perhaps even in pedagogy (Estes 1994). Her attempt certainly deserves admiration; however, I find it unsuccessful.

To state my point briefly, I don't think *Beyond modularity* goes beyond modularity. Other commentators on Karmiloff-Smith's *précis* (1994t) have hinted at such a conclusion in one form or another (de Gelder 1994; Foster-Cohen 1994; Ohlsson 1994), but I don't think a fully explicit argument has been advanced and, necessarily, Karmiloff-Smith has only partially addressed those partial arguments (see Karmiloff-Smith 1994r, pp. 734–35). Briefly stating the argument can help in developing a brief and full answer, to everybody's satisfaction.

I think that Karmiloff-Smith's proposal is not an advance over M for at least two reasons. First, I do not see the precise nature of her criticisms of M; second, I am unclear about what gains the new framework would or could ensure. Both reservations are tied to what I will call the "mystery of the developmental clock": many cognitive abilities follow a strict order of development, both functional and temporal, regardless of the radical differences in inputs from child to child. For example, all normal neonates distinguish linguistic from acoustic nonlinguistic signals (Bertoncini et al. 1989) and can discriminate among different classes of languages (Mehler et al. 1988); however, they lose the ability to discriminate some contrasts irrelevant to their natural language at around 10 to 12 months (Werker & Tees 1984). Similarly, all normal children pass the "false belief task" at around 4 years of age, but not a few months earlier. Ten-month-olds seem unable to exploit properties of objects they nevertheless encode in order to draw sortal distinctions, but 12-month-olds succeed (Xu & Carey 1996). There are many cases like these. The mystery is not a real mystery, but only a problem, if one takes a strong nativist stand; the developmental clock is a biological clock, with the environment acting as a trigger. But then one must acknowledge that there is no real cognitive development "in the sense that developmental cognitive psychologists have in mind" (Fodor 1983). Karmiloff-Smith wants to resist this conclusion, but then she must eliminate the mystery and I do not think she can.

Does modularization go beyond modularity? Here is the first reason for my doubts. According to Karmiloff-Smith, there are two pieces of data one must account for: initial brain plasticity and final rigidity. For Karmiloff-Smith, M is consistent with final rigidity but at odds with initial brain plasticity. To encompass both aspects,

 0140-525X/97 $9.00+.10

she proposes to abandon strict M and to account for final rigidity through progressive modularization. Of course, if modularization were only the maturational unfolding of a genetic program plus or minus a bit, there would be no need to go beyond M: if nativism makes any sense, development in this sense is fully nativist. But for Karmiloff-Smith, modularization is something quite different, namely, the result of a true process of interaction with the environment (1992, p. 10; see also 1994a, p. 733). Notice that M and modularization do generate different explanations. Consider the example: Karmiloff-Smith thinks that at the outset language processing is not modular, but becomes modularized because, *inter alia,* it is processed very fast: "Attention biases and some innate predispositions could lead the child to focus on linguistically relevant input and, with time, to build up linguistic representations that are domain-specific. Since we process language very rapidly, the system might with time close itself off from other influences – i.e., become relatively modularized" (1992, p. 36). On the other hand, M would give an opposite explanation: we process language very rapidly because there is a language module. Hence it is important to see which paradigm is correct.

I see two problems in Karmiloff-Smith's claim that M should be substituted with modularization. First, even if M is compatible with brain localization of function at birth – for which there is indeed evidence (Bertoncini et al. 1989; Best 1988) – it does not require localization. M is a thesis about the functional organization of the mind, and not directly about the brain; hence, simple brain plasticity is no basis for rejecting it (as Karmiloff-Smith also recognizes, *contra* Quartz & Sejnowski 1994). M would indeed be in trouble if the initial state and the time course of development showed no functional modular architecture, but Karmiloff-Smith offers no evidence for that conclusion. Instead, there is much evidence (albeit inconclusive) that very specific signal processors are present at birth and are tuned for very specific features of the input (e.g., for syllables – Bijelac-Babic et al. 1993 – but not for morae – Bertoncini et al. 1995). Such specificity and its developmental schedule seem to be much more than the generic innate predispositions Karmiloff-Smith grants. Second, if her modularization is more than the genetic unfolding of a program in which the environment plays a triggering role, we would expect typical individual and group differences, determined by sharp differences in the experiential histories of the organisms, that we do not see. Of course, one can still pay lip service to development and insist that the unfolding of modules is not really M but a process of modularization, but then in such perspective the developmental clock becomes a mystery. Consider American Sign Language (ASL), for example. Because deaf children reach full linguistic mastery even if deprived of normal acoustic stimuli, Karmiloff-Smith concludes that ASL learning by the deaf child supports modularization against M. However, what is striking in the ASL case is not that deaf children learn a natural language: once again, this is not an argument against the existence of modules, but at most an indication of the level of abstractness of their inputs. What is striking is rather that the development of their linguistic abilities closely matches that of normal children regardless of the sharp difference in inputs (e.g., Petitto & Marentette 1991). This is a prediction for M, but it is only a lucky coincidence for modularization. The developmental clock makes modularization either highly implausible or just another name for M.

Can representational redescription be an alternative to nativism? The second reason for my doubt concerns Karmiloff-Smith's specific positive proposal, the representational redescription (RR) model. Deep down, Karmiloff-Smith proposes a modified Piagetianism integrated by "some innately specified, domain-specific predispositions that guide epigenesis" (1992, p. 172). There are other important differences between Piaget and Karmiloff-Smith, but in this context what counts is that both assume that a domain-general mechanism is responsible for cognitive development – RR in this case (1992, pp. 25 and 167). And the trouble is, RR is too Piagetian, both for the vagueness of its specification and for its centrality, and I feel that it is going to have the same problems as the Piagetian developmental theory. Let me state first what I think the real issue is, and then why I think RR fails in elucidating it.

Karmiloff-Smith raised a very interesting problem. During development, information implicit in the mind becomes knowledge to the mind, either explicitly or (sometimes) consciously: What is the role of these changes? RR is supposed to cast light on this issue. Now, a first reservation concerns what the real problem is exactly, and whether RR is really useful even to describe it. Notice that RR is proposed as a model of changes in the format of the internal representations, but it is not clear that the issue at hand really concerns representational *formats*. Suppose that a parser incorporates a grammar with explicit rules, which it can consult in the process of syntactic analysis. In that case, information is explicitly encoded in the parser, but may simply not be accessible to other cognitive departments. With time, we may imagine that some (but, crucially, not all) of the rules of the grammar are marked as accessible, hence open to the child's metareflection. Notice that in such a scenario no redescription or change in the format of representation is necessary, and yet the same phenomena Karmiloff-Smith mentions can be explained. In short, the accessibility of representations and their formats are orthogonal issues, and I don't see arguments for assuming that development engenders change in the latter. If I am right, then the important problem Karmiloff-Smith raises is not that the format of internal representations changes – it may well remain the same across the board – but that some piece of information can become explicit (at different levels) and some other piece is not and cannot. And then, under this reading, the interesting questions are detailed ones concerning what information becomes explicit, why, and what difference it makes (if any) for cognition. For example, 6-year-olds reach metalinguistic awareness of the category "word," and 10-year-olds of the possessive determiner system, but no child or untrained adult ever reaches conscious awareness of C-Command. Why, and what differences would it make if it were otherwise? Now, strictly speaking, RR cannot answer these types of questions because they are about representations changing their formats, and not about representations becoming more or less accessible.

One can easily modify RR and transform it into a taxonomy for different degrees of accessibility of representations (even if, in that case, it loses much of its "revolutionary" aspect). But then a second reservation comes to mind: in the absence of a specification of the mechanisms that should lead to the phases of redescription/increased accessibility postulated in the RR model, one is still unsure about its real explanatory role. In her response, Karmiloff-Smith remarks that nativists are no better off, since they often describe only scantily the mental operations that would embody the alleged innate structures in the child's mind. She surely makes a point here, but not one that is of much consolation for a theory such as hers, which is supposed to specifically vindicate *cognitive development*. Such a notion does not play any real role in a nativist framework, but it *is* the core of a constructivist's theory. Piaget already postulated something close to Karmiloff-Smith's redescription, the *abstraction réflechissante*. The trouble was that he never explained what that was. The best we can say is that one must wait to see whether RR will be empirically richer than Piaget's *abstraction réflechissante*.

I can grant Karmiloff-Smith that the above criticism is unfair and that it may be premature to ask from RR a detailed description of the mechanism of cognitive development. Then the crucial question for evaluating RR becomes: Can a more detailed version of it account for the phenomena of cognitive development any better than the current RR model? Even here, I doubt that it can be successful. My last reservation has to do with the structure of an RR-like model. We know that explaining cognitive development through unconstrained theory construction from unconstrained data is hopeless, even for developmental facts much less problematic than the cognitive clock. Now, RR is a domain-general mechanism, *just like theory construction*. How can it be any better? Karmiloff-Smith tries to make it different by conceding a

point to the nativist: RR does not start from a *tabula rasa,* but bootstraps from some initial innate attention biases. But notice that making room for selective attention does not render a model different from general theory construction. Even when we build a theory of, say, microphysics, we have selective attention, because we disregard data about ancient history. To construct a theory involves precisely the use of a domain-general mechanism applied to a domain-specific input. Add to it that the domain-specific initial biases granted by Karmiloff-Smith are really very poor: she concedes a "fairly limited amount of innately specified domain-specific predisposition" (1992, p. 4), or, as she writes in her response, only "some *minimal* predispositions" (1994, p. 733). So the tools she allows herself are a set of very poor domain-specific predispositions plus a domain-general mechanism, namely, RR. It looks like the available tools are even poorer than those of theory construction. But if that is all, how to square development with the uniqueness of its outcomes and its tight time course? Nobody expects people to come up with the same theories at the same time precisely because the looseness with which the elaboration of a theory is tied to its data eliminates the possibility of making such predictions. Likewise, we would expect "redescriptions" (as well as modularizations) to occur at any moment, almost unpredictably during cognitive development; we would expect broad variations in, say, phonological, semantic, or grammatical abilities that we do not find. And so the cognitive clock would still be a mystery.

Karmiloff-Smith is sensitive to this problem, so she adds another factor in order to explain the absence of variability. She writes: "The fact that development proceeds in similar ways across normal children does not necessarily mean that development must be innately specified in detail, because it is also true that all children evolve in species-typical environments. Thus, it is the interaction between similar innate constraints and similar environmental constraints that gives rise to common developmental paths" (1992, p. 172). For the sake of argument, let me grant this point as well. Now the question becomes: Are poor innate biases plus a domain general mechanism plus similar environmental constraints sufficient to cope with the cognitive clock? I doubt it. The notion of similar environmental conditions is too generic to cut any ice. What would it mean for language, for example? There are children who grow up in a monolingual environment and end up with the right grammar, the right phonology, the right semantics, and the same degree of metalinguistic awareness, roughly at the same ages. Other (indeed most) children grow up in multilingual environments right from the start, and yet never end up with mixed linguistic systems, or with wrong phonologies, or with higher or lower degrees of explicit grammatical knowledge. The linguistic environment of these two groups is surely similar in certain respects – for example, almost all children are exposed to motherese, and so on – but the trouble is that most of these factors are known to be irrelevant: the notion of "similar environment" is far too coarse-grained to account for children's regularities in the details.

In sum, I do not yet see how the RR model can offer an alternative to the nativist paradigm, in which general learning, theory construction, or epigenesis play a severely constrained role. If Karmiloff-Smith wants to pursue her project, she must find a way to remove the air of mystery from the developmental clock.

ACKNOWLEDGMENTS

Thanks to Jerry Fodor and Jacques Mehler for useful comments on a first version of this commentary. When not otherwise indicated, references are to Karmiloff-Smith's *Beyond modularity*.

Putting knowledge to work

Derek Browne

Department of Philosophy, University of Canterbury, Christchurch, New Zealand. **d.browne@phil.canterbury.ac.nz**

Abstract: Representational redescription (Karmiloff-Smith 1994a; 1994) translates implicit, procedural knowledge into explicit, declarative knowledge. Explicit knowledge is an enabling condition of cognitive flexibility. The articulation and inferential integration of knowledge are important in explaining flexibility. There is an interesting connection to the availability of knowledge for verbal report, but no clear explanatory work is done by the idea of knowledge that is available to consciousness.

Knowledge at the "entry level," Level I, is implicit, procedural, domain-specific, proprietary to particular skills, chunked into unarticulated wholes, and not transportable to other cognitive operations. Animals equipped only with this kind of knowledge are incapable of the cognitive, and so the behavioural flexibility that mature human beings display. To explain this versatility, we posit knowledge that is explicit, declarative, available across all domains, and articulated into elements that are independently transportable. Level-E3 knowledge is maximally explicit. It is available for verbal report. At the E2 level, it is (just) consciously accessible. It is minimally explicit when it is (just) available for multiple uses: that is, when it ceases to have a proprietary application. The process that translates implicit knowledge into explicit knowledge is "representational redescription" (Karmiloff-Smith 1994a; 1994). There are a lot of ideas here, some doing more explanatory work than others. I will argue that the articulation and inferential integration of knowledge are crucial to the development of cognitive flexibility. There is a plausible explanatory connection between cognitive flexibility and knowledge available for verbal report, which I identify as declarative knowledge in a strict sense. But the idea that some of our knowledge is consciously accessible does not do the required explanatory work.

1. Implicit/explicit. In a hierarchical cognitive system, knowledge that is explicit at one level is often hidden from operators at higher levels. In particular, knowledge that is fully explicit at the level of more elementary computational processes, and which is explicitly consulted by the microagents ("homunculi") who work at that level, is inaccessible at the personal level. The only reason for describing this knowledge as "implicit" is that it is personally inaccessible. The only reason for describing some other knowledge as "explicit" is that it is personally accessible. There is a different, Rylean sense in which knowledge might be described as implicit. In this sense, a skilled performance is executed *as if* the operator is following a written recipe, except that there is not literally a recipe to be read. But mere as-if knowledge cannot explain any actual activity. If Level I knowledge is to explain activity, it had better not be intrinsically nonexplicit. It had better just be hidden from higher-level operators.

2. Procedural/declarative. A variety of distinctions might be marked with these terms. I doubt the relevance of most of them. The useful distinction is this: knowledge is "procedural" if it is only available for use in the execution of a specific procedure, that is, if it is proprietary to that procedure. Knowledge is "declarative" if it is available for verbal report, that is, for declaration. Again, the important facts are facts about the access that cognitive operators have to bits of knowledge.

In the beginning, knowledge is distributed among microagents, who have proprietary use of it. Subsequent cognitive development produces both articulation and integration. Articulation produces finer-grained partitions in the mind's representation of things. Representational elements that are originally embedded in the control processes for independent skills, but which overlap semantically, are extracted from those processes and translated into more abstract formats that capture common (or overlapping) meanings. Articulation is necessary for the transportation of knowledge into new domains, for the development of the recombinatorial power that is at the core of higher thought and reasoning. Integration