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## COMMENTARY

# On the Cognition in Cognitive Development

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I argue that Demetriou and Raftopoulos have presented a complex and thought-provoking model of cognitive developmental change, but one that is based on a false and misleading assumption about the nature of representation. This assumption—roughly, that representation is encoding—cannot work and has induced serious problems in the model. An alternative model of representation is outlined, in part for contrast and in part to demonstrate that the aporia of encodingist assumptions *can* be avoided. The encodingist assumption about representation, however, is one that Demetriou and Raftopoulos share with the majority of psychologists, developmentalists included, so the stakes here are not limited to this model alone. © 1999 Academic Press

Demetriou and Raftopoulos (this issue) is a thought-provoking paper, though one that I have a number of disagreements with. Most basically, I will argue that Demetriou has bet on the wrong model of representation, and, therefore, on the wrong model of cognition, and, therefore, that his model of cognitive development lacks valid grounding. I will argue that models of representation—at least the alternative kinds of such models that I will discuss—make fundamental differences in how the dynamics of cognition are understood, and how the processes of development are understood. If my critique is correct, Demetriou has built false presuppositions about representation into the most basic aspects of his model of development. Furthermore, you cannot simply slide one model of representation out from under a model of cognition and cognitive development that has been built on it, and replace that model of representation with another: the way things work depends profoundly on what it is that is working and the nature of what it is working with.

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Demetriou has accepted a model of representation as being constituted as encoded correspondences with those things and properties that are being represented: He has accepted the assumptions about representation of standard computationalism. I have argued, and will again, that computationalism (and the wider encoding model of which it is a variant) is not only false, but that it is necessarily, logically, false. In its stead I will offer a model of representation as emergent out of certain forms of action and interaction systems—a roughly pragmatic, Peircean or Piagetian model. The two genres of representational models, encodingist and pragmatic, do yield profound differences in the models of cognition and development that can be constructed upon them.

## A CRITIQUE OF ENCODINGISM

The failures of standard models of representation are something of a scandal in the fields of artificial intelligence and cognitive science. They go by such terms as ‘‘the empty symbol problem’’ or ‘‘the symbol grounding problem.’’ There are, of course, numbers of people who assume that the problem is already solved, or at least that it is easy, but no one who has seriously thought about it would make such claims. The literature addressing these problems, in fact, is voluminous (e.g., Cummins, 1991, 1996; Dretske, 1981, 1988; Fodor, 1975, 1981, 1987, 1990a, 1990b, 1998; Loewer & Rey, 1991; Millikan, 1984, 1993).

There are multitudes of multifarious problems with standard models. I will not rehearse them all here, but instead will focus on a few that can have particularly pernicious influences on models of cognition that presuppose them—especially on models of cognitive development. Identifying such pernicious influences is important because those failed standard approaches continue not only to be *used*, but to be *assumed* in working with presumed properties of representation, properties that arguably are false for real representation. Presuming such false properties can distort and even render incoherent models of cognition that are constrained by them. For example, if you assume the phlogiston theory of fire and accept that phlogiston has negative weight (a consequence that some were willing to accept prior to Lavoisier’s discovery of oxygen, Butterfield, 1957), then your *further* theorizing will be badly wrong.

The core assumption of standard approaches to representation is that the representational relationship between something *X* that is a representation and something *Y* that it represents is some sort of a causal or lawful or informational *correspondence* between the two. The table reflects light that enters the retina and ultimately stimulates cortical activity that represents that table *by virtue of the causal connection with that table.* It is the later phrase that is critical because it is the later phrase that purports to explain how the cortical activity is a representation at all and how it is specified just what it represents. In other versions of this general approach, the crucial causal connection can be replaced by a lawful connection (e.g., the lawfulness of the light propagation) or by an informational connection (e.g., the light at the retina carries information about the table).

There is in fact one class of representations that do represent by virtue of such a special correspondence: encodings. If ''• • •'' encodes ''S'' in Morse code, that is by virtue of the conventional correspondences of the code. Or, for a non-conventional version, if the neutrino flux in this experiment ''encodes'' particular properties of fusion in the sun, that is also by virtue of a special, lawful, correspondence between the two.

What these two examples have in common is that, in order for the crucial epistemic ''encoding'' relation to exist, both ends of it plus the correspondence itself must already be known. We must know • • • and ''S'' and we must know ''neutrino fluxes'' and ''fusion processes in the sun'' *plus* the critical correspondences in order for the encodings to exist as encodings. There is no problem with this for purposes of using Morse code or testing hypotheses about fusion in the sun. But, for purposes of modeling the nature of representation in itself, it presupposes all of the knowledge, all of the representation, that we might want to account for in our models of representation. If we want to understand how we could possibly represent, say, ''S,'' in the first place, we cannot rely on just another encoding relationship on pain of circularity or an infinite regress. We can encode ''Z'' as ''Y'' and ''Y'' as ''X,'' and can continue such iterations, but only so long as we just assume that ''Z'' is already a representation. It we attempt to push the sequence below ''Z,'' where do we stop? And wherever we stop, how does that most fundamental level manage to represent anything at all? How is the ultimate atomic level of representation constituted as representational?

That foundational or grounding question is, in fact, the one we started with, so such encodings do not solve the problem. Encodings are *derivative* kinds of representation; they are changes in the *form or medium* of representation, and can be extremely useful as such (Morse code, computer code, and neutrino flux encodings illustrate the point), but they cannot model the basic nature or origin of representation. Nevertheless, such encodings are the only *genuine* representations that satisfy the ''representation by correspondence'' framework, so I have dubbed such approaches that, in effect, assume that *all* representations are encodings, with the ''ism'' of their assumption: *encodingism.*

*Representational error.* One central problem with such encoding approaches to foundational representation is that, in their simplest versions, they make representational error impossible. If the crucial correspondence exists, then the representation exists and it is correct. While if the crucial correspondence does not exist, then the representation does not exist, and so it cannot be *in*correct. It would be nice (perhaps) to have infallible representations, but there are none so far as we know, so this property of the models falsifies the models. A great deal of effort has been expended over the last fifteen or so years in attempting to account for representational error (e.g., Fodor, 1987, 1990a), but with no consensus, and, I argue, no success (Bickhard, 1993a, 1996; Bickhard & Terveen, 1995).

Lest it be thought that this is a purely philosophical issue, and, therefore, one to be left to the philosophers, note that if we cannot model the very possibility of representational error, then we cannot model error guided behavior or error guided learning. Or, if we do attempt to model such error guidance, then we have introduced a contradiction into our model: the model of representation makes such error *im*possible, but our further model—of learning or development, say—assumes that such error *is* possible.

*Representational emergence.* Another pernicious problem with encoding models of representation is that they cannot account for *new* foundational representations. This follows readily if they cannot account for *any* foundational representations, but the emphasis on ''new'' yields its own perspective and consequences. The only models we have of how new representations could be formed are as various sorts of combinations of units that are already representations. We can combine representations, but we cannot model the emergence of new *atomic* or *foundational* representations; we cannot model the emergence of representation out of parts or processes that are not already representational. One conclusion that might be drawn from this is that all of our basic representations must be innate, all cognitive development is just manipulations of these elements, and evolution is what created those basic innate representations (Fodor, 1991; Bickhard, 1991). Fortunately, this conclusion does not follow, even if its premises are accepted.

It does not follow because the problem is logical, not simply one of a lack of time or other resources (that evolution might have more of than development). There is no model, of any kind (so the argument goes), of how new representations can be created other than by combinations of already existing representations. That is, there is no model at all of how atomic representations can come into being, can emerge. Evolution is just as helpless in the face of this problem as is learning and development. If representation cannot emerge, then it cannot emerge by *any* process, including evolution, learning, and development alike.

Conversely, if we assume that somehow evolution *can* create emergent representations, then we have no argument that learning and/or development cannot do the same. If there is some way around the apparent logical problem, then there is no argument in existence that would restrict that possibility of emergence to evolutionary processes and deny that possibility to the individual.

It is clear, however, that representational emergence has occurred. There were no representations at the moment of the Big Bang, and there are now. Therefore, representation has emerged. Therefore, representational emergence is possible. Therefore, any model that makes such emergence impossible is false. Therefore, encodingism is false.

*Other problems.* I have outlined two basic problems with encodingist approaches to representation: they cannot account for error, and they cannot account for emergence. Since error and emergence both occur, or have occurred, encodingism cannot be correct. There are many additional problems (Bickhard, 1993a, 1996; Bickhard & Terveen, 1995), but I will stop for current purposes with these two. They suffice to demonstrate that encodingist approaches are false, or at least that they are in serious trouble, and they bear particular salience to issues of learning and development—the issues that Demetriou and Raftopoulos address.

## INTERACTIVISM

Before turning to more specific comments, I will adumbrate an alternative approach to modeling representation, one that makes it easy to account both for the possibility of representational error and for representational emergence. It is a generally pragmatist model in the sense that representation is a property of certain kinds of organization in interactive systems. That is, representation is emergent out of action, not out of the processing of inputs (Bickhard, 1993, 1998a).

In that respect, it is a cousin of Peirce's model of representation and of Piaget's. I do not think that either Peirce or Piaget got the details correct, but they introduced and helped to develop a massive shift in our ways of understanding the nature of mind and mental phenomena, especially such as representation (Joas, 1993; Mounce, 1997).

Note first that, if representation *is* emergent in certain kinds of interactive system organization, then the *problem* of emergence evaporates. Emergence of representation occurs whenever a new instance of such system organization occurs. In particular, whenever learning or development (or evolution) creates such new system organization, then it *ipso facto* creates emergent new representation. There is no *problem* at all.

The central idea in the interactive model is that representation emerges in the anticipatory processes that are involved in the selection of actions. Action selection, in complex organisms, occurs on the basis (in part) of the anticipated outcomes of those actions, should they be engaged in—select the actions and interactions that have desirable anticipated outcomes. Such anticipations have truth value. They may be true if the actual interaction with the actual environment does in fact yield (one of) the anticipated outcome(s). If not, then they are false. Such truth value is the fundamental emergent property of representation.

Further, note that, if such an interaction is engaged in and the anticipation is falsified, that error is functionally available to the system. Interactive representation provides a natural account not only of error, but of system detectable error—error that can guide behavior or can guide learning.

So, the two fatal problems for encodingism that I outlined, that of error and of emergence, are solved almost trivially by the interactive model. That doesn't imply that challenges cannot be made to the model, or that it is anywhere near complete in this drastically abbreviated outline, but it does show that interactivism makes short work of two problems that encodingism cannot solve at all.

One elaboration required of the interactive model is to show how the notion of ''anticipate'' can be rendered in terms that do not already presuppose mind and representation. That is, to show that there is no circularity involved in this intuitive usage. A similar requirement is to show that the outcomes being anticipated do not have to be themselves represented in order for that anticipation to occur: if representation were required, then this would be its own source of circularity. My purpose here is not to argue the details of the interactive model, however, but only to demonstrate that an alternative to encodingism does exist that does not render error and emergence impossible. To see how the interactive model meets these challenges, see Bickhard, 1993a, 1996; Bickhard & Terveen, 1995.

A third challenge to the interactive model would be to challenge its ultimate adequacy. It might seem that interactive representation could conceivably handle simple action possibility representations—perhaps those of worms or maybe even frogs—but what about more complex representations, such as those of objects, or, even more challenging, representations of abstractions, such as numbers? In at least a general way, Piaget has already answered those questions (e.g., Piaget, 1954) and I borrow freely from him. It is these contributions to a pragmatic model of representation that constitute Piaget's most important legacy, a legacy that all too often is distorted and dismissed because of a narrow misinterpretation of Piaget's stages (Bickhard, 1997; Chapman, 1988; Lourenço & Machado, 1996).

For this discussion, I take the interactive model to be at least a viable contender. More importantly for current purposes, I take representational error and representational emergence to have been shown to be quite possible, even trivially so—though not if you presuppose encodingism.

## DESIDERATA FOR A MODEL OF LEARNING

Models of representation can have important influences on models of learning and development. They can impose constraints—such as ''no emergent representation''—and can offer properties—such as ''all representation is individuated into manipulable units: symbols''—that may be false. Such misguidance can render a model of learning or development nugatory: when all psychological phenomena were thought to be comprised of associations, models of learning and development followed, and, correspondingly, they are of marginal scientific interest today.

Conversely, models of representation can veil genuine problems for models of learning and development. They can, for example, obscure the crucial roles of continuity and similarity in cognition by rendering all representation discrete and ad hoc: In contemporary encodingist approaches to analogy, issues of similarity are rendered in terms of encoded features (of course, there are many variations on the theme). One consequence is that all relevant features must be hand-coded into the models, and the learning or development of a *new* space of similarity relationships is, therefore, impossible to model (Bickhard & Campbell, 1996a; Morrison, 1997). Clearly, however, such learning does occur.

Such disconnections between the actual phenomena of learning and development and the constrained horizons of standard modeling approaches suggest that it would be useful to examine the general phenomena of learning and development in order to get a better sense of what such models ought to be held to account for. That is, it would be useful to create a list of desiderata for models of learning and development.

*Emergence.* I will not attempt a full list of such desiderata here, but instead will argue for just a few such properties as a framework for examining the model in Demetriou and Raftopoulos. The first is already clear: representational emergence. Such emergence has occurred and does occur and, therefore, any adequate model of learning and development must be able to account for it. The only alternative is to claim that all contemporary representations can be constructed out of those available at some point in history or phylogeny, and that the ones available at that magical point were created supernaturally. No one seriously proposes such a model (though see Chomsky and Fodor in Piattelli-Palmarini, 1980). No one proposes that representations of Riemann spaces or quarks can be constructed from those available at the time of Hammurabi, or has given the slightest hint of how such constructions could take place. Such representations have been created historically and in the lives and work of particular individuals, and, arguably, by each individual who learns them. (Note that an encodingist model could not account for such learning except by building in, hand-coding, a sufficient set of ad hoc atomic representations to begin with.)

*Heuristics.* A second characteristic of cognition that any ultimate model must account for is that of heuristic problem solving, including the generalizations that that involves: generalizations across similarity spaces of problems and of potential solutions. Furthermore, it must account for the learning and development of *new* such spaces. This turns out to be a very serious constraint, one that requires relatively novel architectures to be able to satisfy it (Bickhard & Campbell, 1996a). The discrete encodings of computationalism cannot even address it except in terms of hand coded features, which, among other problems, makes the learning or development of new such spaces impossible—that would require new such features, and encodingisms cannot model the creation of new basic encodings.

Even more problematic are *self-directed* heuristics, such as when self-directing one's learning of a new skill or realm of knowledge, and the *emergence* of self-directed heuristics out of more primitive learning processes. Such emergence is seriously aporetic within any computationalist perspective. Among other problems, the normative aspect of ‘‘what is to be learned’’ must be differentiated from learning that satisfies those normative criteria, and an account must be given of how that normative aspect can itself be learned at least partly independently of the learning that it then can guide (Christensen, Collier, Hooker, in preparation; Christensen & Hooker, in preparation).

There are other important desiderata for a model of learning and development, such as an account of how responsibility for error is determined when a complex system produces an error—where do you make changes in order to try to correct the error (this is a *very* serious problem)—or an account of phenomena of *self*-scaffolding of learning and development (an oxymoron on standard accounts of scaffolding; Bickhard, 1992a). But the necessity to account for representational emergence and for the learning and functioning of heuristics and the similarities that they involve will suffice for my purposes in this commentary.

## STRUCTURE AND CHANGE

Demetriou and Raftopoulos’ proposed model of cognitive change is embedded in a model of the overall architecture of cognition. That architecture consists of a first level of knowing, which is devoted to the environment and is modularized into ‘‘a set of specialized capacity systems that guide understanding of different reality and knowledge domains’’, a second level of knowing, a ‘‘hypercognitive system’’, that ‘‘monitors and controls the functioning of all other systems,’’ and a processing system that ‘‘defines the activation and the interaction between the two knowing levels.’’ Both levels of knowing and the processing system can be complexly and hierarchically organized. Each module in the first level of knowing ‘‘involves inbuilt structures that abstract specific types of representations’’ thereby providing ‘‘kernel elements’’, foundational symbols, for that module. This architecture is presented elsewhere, along with numerous supporting empirical studies, and is only outlined in this paper.

The basic process of change involves a source of variations and a process of selection together with criteria for selection. This appears to be a kind of a variation and selective retention model, though it is confusingly worded and references none of the long list of related literature (e.g., Campbell, D. T., 1974, 1990; Hahlweg & Hooker, 1989; Radnitzky & Bartley, 1987; Shimony & Nails, 1987; Wuketits, 1990). Variations are based on current structures, so ‘‘the products of developmental change . . . grow . . . as adjustments of present ‘tentative alternatives’ to core or prototype concepts.’’

These origins of change yield three different kinds of change: ‘‘(1) changes within individual structures, (2) changes in relations between structures within a hierarchical level, and (3) changes in the relations between hierarchical levels.’’ Changes in any of the three major architectural parts may, in turn, induce changes in either of the other two parts in a kind of ripple effect, and perhaps cycling back on the originating part. Change processes, then, can form dynamic loops within and among the parts of the cognitive architecture.

Cognitive change in this model is primarily change in mental units, where units are ultimately constructed on the basis of the ‘‘innate’’, ‘‘inbuilt,’’ kernel elements. Demetriou and Raftopoulos propose five basic mechanisms of change: bridging, interweaving, fusion, differentiation, and refinement. *Bridging* ‘‘refers to the construction of a new mental unit by establishing relations between units already available.’’ *Interweaving* is like bridging, but it also yields a preference for use of the new unit over the old. *Fusion* is also initially like bridging, but, in addition, it yields the elimination of the original units involved in the construction. *Differentiation* is ‘‘an improvement in the accuracy of the functioning of an already available unit.’’ *Refinement* is similar to differentiation, but with the addition of ‘‘the abandonment of strategies or skills involved in a mental unit when they are found irrelevant or redundant to the unit’s field of application.’’

In addition to these basic mechanisms for constructing new units out of old, there are also two mechanisms for the construction of new units relative to old units: metarepresentation and symbolic individuation. Metarepresentation ‘‘is a process which looks for, codifies, and typifies similarities between representations to enhance understanding and problem-solving efficiency.’’ Symbolic individuation ‘‘is a process which pairs newly generated ideas with specific symbols.’’

On the basis of this general model of cognitive change, Demetriou and Raftopoulos propose several general characterizations of development and undertake comparisons with several other theories. I will adumbrate these characterizations and comparisons in the context of the comments that I will offer about them.

## GENERAL CRITIQUE

My most basic critique is already clear: Demetriou and Raftopoulos have presupposed a false model of the nature of representation. That presupposition, in turn, has influenced their broader model in unfortunate ways. I will offer some observations about some of these basic influences, and then turn to more specific criticisms.

The commitment to an encoding model is clear: The specialized capacity systems, the modules in knowing level one, are ‘‘symbolic systems’’; The classic symbol system hypothesis of Newell and Simon together with its related problem space hypothesis are endorsed; a Fodorian innatism, which derives from encodingist assumptions (Bickhard, 1991), is also specifically endorsed, as is a similarly motivated Fodorian informational encapsulation of the modules; They outline computational models when writing, for example, of requiring ‘‘different symbol systems to represent their domains and sustain their computational functioning.’’ And so on.

Immediately, however, it must also be pointed out that the levels and the processing system also involve ''actions,'' ''processing skills,'' ''outputs,'' ''computational skills and strategies,'' and so on. Each level, each module, seems to a computational system in its own right, complete with dedicated symbolic resources *and* computational resources. There is no problem in combining representation and processing in modules in this manner—none, that is, beyond the fundamental problems of encodingism per se—but this combining of representational and dynamic properties does become problematic with the kernal elements in each module. There too we find both representational and processing properties, but their relationships within a kernel are not articulated. An action based model of the nature of representation would give one kind of approach to such an integration, but the strong distinction between representation and process in the computationalist approach makes this a very difficult issue to even conceptualize: Foundational encoding representations are atomic elements. They can be manipulated, combined with others, moved, and so on, but they are themselves inherently inert. Their representational power is constituted in the atemporal encoding relationship with whatever they are supposed to represent. Computationalism is founded on this absolute distinction between encoded representation and computational dynamics. Demetriou and Raftopoulos seem to want to have it both ways, but they have provided no model of how that could be possible. Furthermore, numerous other positions that they take, such as endorsing Newell and Simon or Fodor, also commit them to the computationalist diremption between representation and process: it is not open to them to attempt an action based, a pragmatic, notion of representation at the level of the kernel elements because that would invalidate the logics underlying these numerous other endorsements they have made. Demetriou and Raftopoulos are stuck with their computationalism, but they have violated it with the inconsistent characterizations of representation and computation in the same elements.

Given this computationalism, it makes historical sense to make the sorts of endorsements that they do, such as of the problem space hypothesis or Fodor's innatism. But, as indicated above, even here the logic is not valid. Fodor's innatism does not solve the logical problem that supposedly leads to it (Bickhard, 1991), and even Fodor doesn't really endorse it (e.g., Piattelli-Palmarini, 1980, p. 268; Fodor, 1990b, p. 190; Fodor, 1998). The problem space hypothesis, in its turn, cannot account for the generation of *new* problem spaces, the learning or development of new problem spaces—these require new encodings, and there is no account for new encodings within the computationalist framework (Bickhard & Terveen, 1995). Demetriou and Raftopoulos are attempting to construct a model of learning and development within a framework that makes genuine learning and development impossible (Fodor, 1975, 1981; Piattelli-Palmarini, 1980).

It might seem to be open to Demetriou and Raftopoulos to simply posit that some kernels are symbolic and some (others) are computational, but the processes of generating new symbols and of generating new computational programs are vastly different, and no such difference is accommodated in their models of cognitive change. This contradiction between representational and dynamic properties permeates further into the specifics of the kinds of change proposed. *Differentiation* of a cognitive unit, for example, makes good sense from an external functional perspective, and could probably be given some more specific model for a computational unit, but makes no sense whatsoever for an atomic symbolic encoding. How can you differentiate the representational power within an atomic representational unit? If it were possible, it wouldn't be atomic.

The entire program in Demetriou and Raftopoulos' model of generating new units requires some sort of program writer for the computational aspects of units, but is restricted to the same sort of strict combinatorics of basic atomic representational units as are all other encodingist models. It is not possible for fundamentally new symbolic encodings to emerge. At best, you can posit, as does Fodor (1975), that the innate set is combinatorically adequate to all concepts, all representations, that any human being ever had or ever will have. This consequence is one of the many reductios of the encodingist assumption.

This same impossibility of emergence shows up with special force in Demetriou and Raftopoulos' notion of metarepresentation. What are the foundational units of representation at this meta-level? None are discussed, but how do Demetriou and Raftopoulos propose to escape the impossibility of emergent new such representations? Or, conversely, what could possibly be the combinatorically adequate set of innate representations here? Their very posit of a metarepresentation process contradicts their underlying computationalism.

Further, Demetriou and Raftopoulos propose that metarepresentation is based on some sort of analogical reasoning applied to other representations. Setting aside issues of the adequacy of that proposal for metarepresentation in a general sense, they encounter here still another manifestation of the limits of encodingism. Computationalist models of analogy, metaphor, and similarity are based on encoded representations associated with encoded features (Bickhard & Campbell, 1996a). There are many technical details involved in the design and evaluation of these models, but something which none of them attempts to address, nor can they address, is the problem of learning or developing new such analogical or similarity spaces. That requires new encodings and new encoded features, and that is impossible within the encodingist, computationalist, framework (Bickhard, in press; Bickhard & Terveen, 1995; Bickhard & Campbell, 1996a; Morrison, 1997). Encoding based analogical reasoning cannot do what real analogical reasoning does—it cannot capture in a non-ad-hoc manner the actual dynamics, creativity, the reciprocal changes in both source and target of an analogy, and so on and on (Dietrich, in press; Gentner & Grudin, 1985; Gentner &

Jeziorski, 1993; Gentner & Markman, 1995; Gentner & Ratterman, 1991; Gentner & Wolff, in press; Hofstadter, 1995; Medin, Goldstone & Gentner, 1993; Tversky, 1977; Tversky & Gati, 1978). So appeals to analogy from within a computationalist framework, as in Demetriou and Raftopoulos' model, illicitly draws on the genuine creativity of analogy and metaphor (e.g., Lakoff & Johnson, 1980) while making that creativity impossible within the framework that is presupposed (see the discussion of Lenat's CYC in Bickhard & Terveen, 1995).

Clearly the computationalism assumed in this model deeply affects the details and the plausibility of the model. Demetriou and Raftopoulos are certainly not alone in making such computationalist presuppositions (in fact, it is obviously my position that is the minority position here), but theirs is a very good illustration of the consequences—some direct, some perniciously indirect and subtle—of accepting the premises that they share with Fodor but not accepting his conclusion that genuine development is impossible (Bickhard, 1991; Piattelli-Palmarini, 1980). Fodor's reasoning is not invalid; Demetriou and Raftopoulos, along with many others, are just as committed to his conclusions as is Fodor himself. The problem with Fodor's reasoning is that it is valid but unsound: it is based on a false premise: it is based on a premise of the encoding nature of representation.

## OTHER ISSUES

There are a number of additional issues I would like to address at least briefly. Some are also related to the encodingist assumptions; and some are not, but they are, in one way or another, not as focal (at least in this paper) as the encoding issues discussed above. Nevertheless, they can have major importance in their own right and especially in their own contexts.

*Architectural support.* The paper that I am commenting on does not focus on the extensive empirical support that Demetriou and colleagues have gathered for the basic model of cognitive architecture. Nevertheless, I would like to offer a couple of comments about this issue. My primary comment is that the evidence, massive as it is, is largely confirmatory in nature—it does not test Demetriou's model against any other. This research style is extremely common in Psychology, but it does not succeed in providing strong support for a model or theory, no matter how much data is accumulated, because it is not clear what alternative explanations, models, hypotheses, and so on are ruled out (Bickhard, 1993b). In the extreme, if the data are consistent with the favored model, but are also consistent with every other model in the literature, then the data are worthless. Whether or not such an extreme is actually the case, or what less extreme rational inference situation might hold, cannot be determined without examination, but if studies are not *designed* to select among alternatives, it is rare that the data will in the end be useful for selecting among alternatives. The simple inductivism that underlies this sort of confirmatory research is part of the naive positivism that still dominates the implicit and explicit philosophy of science in Psychology. It has been rejected within the philosophy of science for half a century (Bickhard, 1992b).

As a more specific example, consider the transfer studies that Demetriou mentions. If training within a module generalizes better to other tasks within the module than to tasks in a different module, then that data is consistent with the positing of the modules. But it is also consistent with any other model of differential similarity, in which generalization to more similar tasks is greater than to less similar tasks, and for which there are no module boundaries at all.

Demetriou and Raftopoulos also mention a formal analysis in which the modules postulated are argued for on formal grounds, not just on empirical grounds (Kargopoulos & Demetriou, 1998). This is a form of argument that is of fundamental importance in other sciences, such as physics, but is almost unknown in psychology. I have elsewhere congratulated Demetriou for the recognition that such a kind of argument is possible, powerful, and, for some kinds of claims, necessary (Bickhard, 1998b). There are very few in psychology who have seen these points. But I also pointed out that this form of argument too can be in error, and I argued that Demetriou's particular argument is valid, if at all, only on the assumption of computationalism. With regard to the computationalist versus pragmatist issue, then, it presupposes precisely what is at issue.

*Variation and selective retention.* I applaud Demetriou and Raftopoulos' positing of a variation and selection process for the generation of new knowledge. Without foreknowledge of some sort, such an evolutionary epistemology is the only possibility (Bickhard & Campbell, 1996a, 1996b), though many would still hold to some sort of induction or transduction. But Demetriou and Raftopoulos' reliance on computationalism posits precisely the foundationalisms that evolutionary epistemology can escape, thus vitiating at least part of the strength of the position (Brown, 1988; Radnitzky & Bartley, 1987). The variation and selection process also requires that representational error can be detected, but their encodingism makes that impossible. Still further, most learning and development is *not* without foreknowledge; most learning and development is based on heuristic knowledge that guides and preselects the variations. Demetriou and Raftopoulos make a major point about such ''constrained constructivism'' (see below for additional comments on this point), but, here again, their encodingist presuppositions make such guidance extremely difficult to model. In particular, such heuristics depend on making use of similarities among problems and solutions, and such similarities can only be modeled with encoded features within the computationalist framework. As mentioned before, such encodings have difficulty handling the actual dynamics of similarity processing, whether judgements, analogies, or metaphor (Bickhard & Campbell, 1996a; Dietrich & Markman, in press), but have no chance whatsoever of modeling the learning or development of new such similarity spaces: the features are hand-coded in the models—they are not generated within the models—and encodingism precludes the possibility of generating genuinely new such features (Bickhard, 1993; Bickhard & Terveen, 1995; Bickhard & Campbell, 1996a; Morrison, 1997).

*Comparison?* At one point, Demetriou and Raftopoulos compare the model in this paper with several others which also address ''the self-oriented level of the mental architecture.'' Their comment is that ''these models do not elaborate on the organization and functioning of the other two levels of the mental architecture, that is the processing system and the environment-oriented systems.'' Since one of the models compared to is Campbell and Bickhard (1986), in which I have a mild interest, I thought I might respond on behalf of that model.

First, the knowing levels model of Campbell and Bickhard (1986) most certainly does have a level directed to the environment—level one. That, in fact, is definitional of level one. Presumably, in commenting that there is no elaboration ''on the organization and functioning of the other two levels of the mental architecture'' they are referring in this case to the fact that there is no set of innate modules in the Campbell and Bickhard model. But I would argue that the evidence for such *innate* modules is in fact much weaker than Demetriou and Raftopoulos assume (Bickhard, 1995, 1997), and that modularization of the cognitive organizations can nevertheless occur as a natural process during development (Campbell & Bickhard, 1992). A criticism that there are no innate modules in the model is question begging in that it simply presupposes that there are in fact such innate modules, a presupposition that I contend is false. Similarly, I would contend that the ''processing system'' is not a functionally distinct system at all, so to challenge its absence is similarly question begging.

To illustrate this in the opposite direction, let me point out that the Campbell and Bickhard (1986) model posits a process of *reflective abstraction* as central to development, and postulates knowing levels three (and further) as having important functions, at least for some. Level *three*, interestingly, is necessary, according to this model, for the development of self identity, and it is self-oriented aspects that Demetriou and Raftopoulos are willing to credit this model with addressing. So, I could criticize Demetriou and Raftopoulos for not having any model of reflective abstraction nor of any levels of knowing higher than two (the hierarchies that they posit in their levels one and two are not of the right sort to constitute knowing levels; such hierarchies are discussed in Campbell and Bickhard, 1986). Clearly, the force of any such criticisms depends on the warrant that is credited to the posits of reflective abstraction and higher knowing levels. If no such phenomena occur, then the criticism is of no weight, but if such phenomena do occur, then it has relevant weight. To advance a criticism simply because Campbell and

Bickhard (1986) have such aspects in their model while Demetriou and Raftopoulos do not would be question begging.

Demetriou and Raftopoulos can reply that ''We argued here, however, that none of the three levels can be satisfactorily understood without satisfactory understanding of the others'' and that that constitutes non-question begging warrant. They did so argue, but that argument, even accepting its *validity* (not soundness) at face value, is premised on computationalism, with all of its assumptions that Campbell and Bickhard (1986) explicitly reject and argue strongly against. Again, a question begging stance: Demetriou and Raftopoulos do not argue for their encodingist computationalism at all.

*Connectionism.* Demetriou and Raftopoulos present an extended comparison of their model with connectionism in general and with Elman, et al. (1996). I have several responses to this discussion.

It is of interest to me, first of all, that there is no discussion of the manifestly difficult aspects of connectionist models, such as the unrealistic process of back-propagation for ''learning'', or the requirement of an external tutor for such a process to occur at all. This is of relevance not only because back-propagation cannot be correct at the neural level, and probably not at any functional brain level, but also because they advert to connectionism for their metarepresentational function to ''codify, and typify similarities between representations.'' Again, this is not neurally realistic, but, even worse, the partitioning of an activation space that occurs in a trained connectionist net is the product of an external tutor imposing that partitioning on the net. There is no such tutor, nor is one possible, for mental metarepresentation.

Still further, connectionist ''representations'' are just as encodingist as are computationalist ''representations''. They are trained instead of designed in engineered transducers, but they yield informational correspondences between activation vectors and input patterns, and *that is all.* The assumption that such informational correspondences constitute representation is precisely the encodingist assumption. Informational correspondences occur ubiquitously throughout the universe—every instance of a causal correspondence, for example, is also an instance of an informational correspondence—and they are in general *not* representations. Connectionism, no more than other versions of encodingism, has made good on explaining how their particular version solves the problems of representation—of error, for example, or of any of the myriads of other problems (Bickhard, 1993; Bickhard & Terveen, 1995).

One critical difference between connectionist models and computationalist models is that the strong distinction between representational elements and computational processes is eliminated in connectionist modeling. The trajectory of the activation vector in its space is simultaneously representation and computation (at least in the terms of the model). This elimination of separate representational elements is taken by some to be a source of great power for connectionist models and by others to be a fatal limitation, but, whichever evaluative stance is adopted, the difference remains. It would seem to be a lacuna in Demetriou and Raftopoulos' discussion that they appeal both to computationalist properties and to connectionist properties without addressing issues of their incompatibility.

I am also puzzled that there is no discussion of recurrent nets, which have their own interesting powers (Elman, 1990, 1991; Pollack, 1990, 1991; Tani & Nolfi, 1998).

At a more detailed level, in the suggested connectionist model of *bridging*, an activation space is repartitioned so that ''a new area is formed'' but the space ''still contains the areas that correspond to the combined operations''—''the new mental unit is superimposed on those that have been combined to generate it.'' I think I understand functionally what is intended here, but an activation vector cannot exist in more than one attractor region at a time. Unless Demetriou and Raftopoulos are using the term ''partition'' in a very nonstandard (and unexplained) way, these claims are simply impossible. Something like what they describe here could be engineered into a connectionist framework but it would require more machinery than they have introduced.

*Wild constructivism.* Demetriou and Raftopoulos end their paper with a call for constrained constructivism instead of the ''myth of wild constructivism'' of such as Piaget and Vygotsky. They do not define wild constructivism, but presumably it means a constructivism that does not have the constraints similar to what they posit on variations in a variation and selection construction process. There may be such wild constructivists, though I cannot think of any off-hand (at least not in developmental psychology), but to construe Piaget this way is seriously mistaken.

Piaget's constructions are *highly* constrained (Piaget, 1971, 1985). In fact, Piaget did not countenance variation and selection construction at all—he didn't deny its existence, but did deny its power for explaining development. Piaget's constructivism is a strongly constrained, almost teleological, constructivism that depends on active ''groping'' in exploration of the world to construct the mental structures that he posited, always on the foundation of and in the context of already extant constructions (Bickhard, 1988). In comparison, it is Demetriou and Raftopoulos who posit a relatively unconstrained variation and selection constructive model. In fact, any model that has variations generated out of prior constructions (Campbell & Bickhard, 1992) will manifest the sort of ''constraint'' that Demetriou and Raftopoulos advocate. Wild constructivism would have to posit free constructions that have no historicity at all. There are positions in evolutionary biology that make the mistake of presupposing such a model of evolutionary constructions (though even there no one that I know of who explicitly advocates such a position), but, as mentioned, I know of none in developmental psychology.

In any case, Piaget is most certainly not an example of any kind of ‘‘wild constructivism.’’

## CONCLUSION

Demetriou and Raftopoulos (this issue) is not only thought provoking, but also quite complex. I have commented on only a slice of issues that they address—clearly there are many others that also deserve attention. But I will bring this commentary to a close here.

My differences with Demetriou and Raftopoulos are deep and ramify throughout the complexities of the model they propose. In the most perspicacious sense, they are the differences of two fundamental orientations to the nature of representation, one millennia old—and still dominant—and one originated only a century ago by Peirce. Sorting out these differences and arguing their merits will obviously take much more time and attention than is afforded in this paper. Nevertheless, most of theoretical psychology depends on the outcome of this rivalry, developmental psychology included—issues about representation permeate everywhere.

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