Dynamics or Representational Epicycles?

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Review of Dietrich, E., & Markman, A. B. (Eds.) (2000). *Cognitive dynamics: Conceptual and representational change in humans and machines*. Mahwah, NJ: Lawrence Erlbaum.

The stated objective of *Cognitive Dynamics* is to defend the representational/ computational theory (R/CT) in cognitive science against various criticisms. Starting from the view that R/CT is the best theory we have, the book attempts to meet the challenges posed by cognitive dynamics, embodied cognition, and situated action to convince the reader that these issues can be handled by R/CT. The authors' various conceptions of representation do just that. Representations can be dynamic, embodied, and sensitive to context (good examples appear in Dietrich [Chapter 10], Prinz & Barsalou [3], and Burgess & Lund [6], respectively). However, there is little in the way of a common thread among these solutions, other than the commitment to mediation as the defining property of representations. Also, there are reasons to question the virtue of these conceptualizations. They are admittedly ad hoc (Dietrich [10]); no consensus has emerged about which representations are correct; and most of the problems addressed grew out of work by scientists looking at cognition from other points of view. So we may ask whether the work reported in the book represents an advance in theory or simply interesting puzzle solving within a dominant scientific paradigm. .

We cannot review the entire book in detail. Otherwise, we would have many positive things to say about the accomplishments of the authors. However, the positive story is relatively well known. Consequently, we focus on a few claims that overstate the case for R/CT, and we discuss representation more generally.

Marcus [4] argues for the necessity of representations involving rules and variables and claims that connectionist models cannot realize these properties. As an example, he cites his own work showing that infants can learn patterns of aab and aba and can discriminate new instances. Therefore, infants must have rules that include variables, like a and b. Actually, the finding only shows that infants can recognize identity and patterns of repeating elements. Is this proof of rules and variables (cf. Gomez & Gerken, 2000)? Several connectionist models without rules and variables have been able to reproduce the result (e.g., Altmann & Dienes, 1999; Seidenberg & Elman, 1999a, 1999b; Shultz & Bale, in press), but Marcus does not mention these. The issue of recognizing identity as particularly a problem for network models is also raised by Holyoak and Hummel [9], but, again, identity can be handled by recurrent network models so why no discussion of these possibilities?

If representations are essential to cognitive theory, it seems reasonable to ask just what discovery methods will be used to individuate them and to determine what these representations are. The experience in experimental cognitive psychology does not provide much encouragement. Many of the major issues over the last half-century have resisted empirical resolution. The nature of categories and concepts provides an example of particular relevance to the role of representations in cognition. Researchers still debate which mental representations underlie categories. Are they exemplars (e.g., Kruschke, 1992), prototypes and family resemblance structures (e.g., Rosch & Mervis, 1975), or abstract entities (e.g., Marcus [4]). Remez's [5] detailed empirical review clarifies how representation of spoken language resists all these conventional characterizations. What if conventional issues of representation cannot be resolved empirically? Consider the arguments about the identifiability of cognitive mechanisms advanced by Townsend (1972), Anderson (1978), and Uttal (1990). Perhaps we have been asking the wrong questions. Issues about the particular form of mental representations appear to be particularly difficult to resolve.

D&M argue that representation is essential to theory in cognitive science because representation is essentially mediation, and cognition obviously involves mediation. If representation is mediation, then why not speak of mediation rather than representation? Why not frame the problem as the reliable individuation of mediated effects? The reasons may have to do with a restricted view of computation as Turing computation, which requires representations. Once alternative metaphors (such as complex systems) are more fully explored, an expanded notion of computation may be realized in which conventional representations play no role (see Solé & Goodwin, 2000, for a survey of the archetypes that motivate complexity theory).

In our view, representation involves more than mediation. It is informative to recall the triadic formulation of the theory of signs developed by C. S. Peirce in the late 19^{th} Century (Buchler, 1940, p. 99). In Peirce's words, "a sign, or representamen, is something which stands to somebody for something in some respect or capacity." For Peirce, the triadic character of signs was irreducible. In a representation, all three elements (the sign, the signified, and the interpretant) and the relations among them must be involved. Peirce's analysis should encourage us to pay more attention to the interpretations of representations. In particular, in understanding a system, it is important to distinguish between *intrinsic representations* where interpretations are made by the system as opposed to *attributed representations* where interpretations are made by an outside observer. We think that considerable conceptual confusion can accompany failures to make this distinction.

What constitutes interpretation? A simple proposal is that interpretation requires explicitly treating the vehicle of the representation as a stand-in for the referent. Interestingly this requirement is one of the defining characteristics of intentionality. Thoughts have intentionality in that they are *about* something. Peirce's notion of the interpretant appears to be closely related to two essential properties of intentionality, aboutness and aspect (Lyons, 1995). For instance, when we think about an *apple*, we form a specific conception of it from a specific, first-hand, point-of-view. The aspectual view changes from one context to another and always entails a specific interpretation. Thus, Hummel's [7] critique of view-based perception, while effective against particular view-based models, fails to accommodate pervasive intuitions about intentionality. Arguably, first-hand intuitions about intentionality are more compelling than intuitions about mediation (Gibbs, 1999), but intentionality is not successfully addressed by R/CT (Juarrero, 1999). It would be ironic, indeed, if the proper role of representation in cognitive theory turns out to be in the analysis of intentionality (cf. Searle, 1992).

Only intrinsic representations should be included in a representational account of the workings of a system. Otherwise we run the risk of attributing properties to the system that it does not possess. The debate about representation in James Watt's steamengine governor may usefully be seen in light of the distinction between intrinsic and attributed representations. It is easy to *attribute* representation to the governor and/or to James Watt (Bechtel, 1998; Prinz & Barsalou [3]), but it is difficult to see an *intrinsically* interpreted representation of speed in the governor itself (van Gelder, 1997).

D&M [1] apparently do not agree. They argue that what is important is that a system can be *interpreted* (by a theoretician?) as carrying out computations (p. 11). This formulation appears to take priority over the facts of the matter to favor developing R/CT interpretations, and it conflates success in simulation with an understanding of the actual workings of a system. We propose that deeper understanding of a system can be obtained with a causal account of its workings rather than simply a simulation of its behavior. If intrinsic representations (such as beliefs and intentions) enter into a causal account (see Juarrero, 1999 and Searle, 1994 for example), so be it. But, deciding that a system uses representations and computations, because it can be described in such terms, is claiming too much. Would we say that weather is a R/CT system because it can be simulated by symbolic computations?

Bickard [2] discusses the crucial question of how representations attain their content. Most other chapters do not address this issue. Instead, representations are proposed with little regard for how they achieve their status. D&M [1] acknowledge Bickard's (1998, [2]) argument that representations emerge from a system's nonrepresentational interactions with its environment (p. 19). Their emphasis is on the representations that result, but if Bickard's view is correct, we must recognize the priority of nonrepresentational processes. Once admitted, how are we to anticipate, before adequate investigation, the limits of such processes? May they not occupy a central role in cognition that is not to be understood in R/CT terms? The importance of such "background" in cognition is increasingly recognized as a central problem for cognitive science (Fodor, 2000; Polanyi, 1964; Searle, 1992).

Bickard's [2] discussion of action and interaction with the environment, invariances, and potentialities (cf. affordances), and his use of the perception-action cycle (p. 41) runs strikingly parallel to proposals developed by ecological psychologists (e.g., Gibson, 1979) who manage without representations. We are also puzzled by Bickard's attempt to localize representation in the brain when emergence is found at the macrolevel of an acting body in a world. How does one get beneath the epistemological barriers inherent in emergent properties? If emergence lies in the coupling of perception and action, what is the sense in localizing such effects in representations?

Overall, no real debate is found in the book. No advocates of alternative views are present (with the possible exceptions of Bickard [2], Remez [5], and Burgess & Lund [6]). This lack is unfortunate because there is genuine doubt about the adequacy of the R/CT approach to mind, in particular, and to cognitive science, in general. Critics of R/CT are found in scholars interested in consciousness (e.g., Chalmers,1996; Velmans, 2000), creativity (e.g., Fodor, 2000; Searle, 1992; Shanon, 1993), embodiment (e.g., Johnson, 1987; Varela, Thompson, & Rosch, 1991), neuroscience (e.g., Edelman, 1992; Freeman, 1995), and dynamic systems (e.g., Juarrero, 1999; Port & van Gelder, 1995). Let's briefly consider a few of these perspectives.

A long-time leading proponent of R/CT, Jerry Fodor (2000), argues that, at best, the theory is incomplete. Fodor provides a clear analysis of the entailments of R/CT, and he holds that there are serious limitations to the theory (e.g., the frame problem, inferences to the best explanation). Essentially, R/CT cannot accommodate phenomena that implicate global constraints. Advocates of R/CT often ignore such problems in their optimistic prognostications about the prospects for the future of the theory.

Alicia Juarrero (1999) offers some fascinating ideas about causality and intentional action from the perspective of dynamic systems theory. An account of intentional action has previously evaded adequate philosophical analysis. Juarrero provides a new candidate for such an account by characterizing an intention to perform an act as a high-level constraint in a self-organizing system. Along the way, she discusses how the notion of self-cause is sensible within the framework of complex dynamical systems.

Presently, however, the dynamical approach faces the same challenge that has plagued R/CT – the necessity of coherent, rigorous empirical tests of hypotheses. As Bickard acknowledges, none of the illustrative phenomena in his chapter actually constitute tests of dynamical hypotheses. Contrast this empirical gap with work in motor control where successful critical tests have been conducted (e.g., Kelso, 1995), or cognitive studies that corroborate long-range correlations in the variability found in performance data (Gilden, 2001) as expected from complexity theory. Cognitive performances may be better described under different basic assumptions (Van Orden, Moreno, & Holden, in press). This work is only beginning, but it appears worthy of further development.

There are certainly reasons to continue to pursue the solution of problems within the R/CT paradigm. For instance, many applied problems can benefit from such an analysis. The book successfully demonstrates how R/CT can work in specific applications. However, as we have discussed, it is not clear that substantial theoretical progress is to be found in these endeavors. At this juncture in the development of theory in cognitive science, serious consideration of alternatives may be more valuable than simply pressing forward with the view that has dominated cognitive science throughout its 50-year history.

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