Useful Theories Make Predictions

Andrew Howes

Manchester Business School, University of Manchester

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Abstract

Stephen and Van Orden (this issue) propose that there is a complex system approach to cognitive science, and collectively the authors of the papers presented in this issue believe that this approach provides the means to drive a revolution in the science of the mind. Unfortunately, however illuminating, this explanation is absent and hyperbole is all too extensive. In contrast, I argue (1) that dynamic systems theory is not new to cognitive science and does not provide a basis for a revolution, (2) it is not necessary to reject cognitive science in order to explain the constraints imposed by the body and the environment, (3) it is not necessary, as Silberstein and Chemero (this issue) appear to do, to reject cognitive science in order to explain consciousness, and (4) our understanding of pragmatics is not advanced by Gibbs and Van Orden’s (this issue) “self-organized criticality”. Any debate about the future of cognitive science could usefully focus on predictive adequacy. Unfortunately, this is not the approach taken by the authors of this issue.

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Stephen and Van Orden (this issue) propose that there is a complex system approach to cognitive science, and collectively the authors of the papers presented in this special issue of *Topics in Cognitive Science* believe that this approach provides the means to drive a revolution in the science of the mind. Unfortunately, however, as is sometimes the case with attempts at the polemical essay, illuminating explanation is absent and hyperbole is all too extensive. We are told, for example, that cognitive science has reached an “impasse” and also that some “ultra-fast” responses appear to be so fast that it is as though the brain did “nothing in between.” We are told that “representationalism” must be rejected, and that there are “paradoxes” of cognition. The paradoxes turn out to be nothing of the sort. The papers are rich with jargon and allusions to scientific formalism but, sadly, contain few empirical facts and no new formal approach to cognitive science.

Correspondence should be send to Andrew Howes, Manchester Business School, University of Manchester, Booth Street West, Manchester M15 6PB. E-mail: andrew.howes@mbs.ac.uk
To be sure, the authors set themselves a hard challenge. It cannot be easy to construct a sound argument for a paradigm shift in the study of a set of phenomena that have been studied so extensively by so many. Below, I describe why the arguments presented by Stephen and Van Orden (this issue), Gibson, Holden, Mirman, and Stephen (this issue), Silberstein and Chemero (this issue), Riley, Shockley, and Van Orden (this issue), and Gibbs and Van Orden (this issue) do not provide a basis for a revolution in cognitive science.

First, dynamic systems theory is not new to cognitive science and does not provide a basis for a revolution. Writing some 10 years ago, Busemeyer (2002, p. 1049) summarized an article on the contribution of dynamic systems to cognitive science: ‘‘cognitive scientists are making broad use of this approach, especially in applications of connectionist and artificial neural network models of cognition.’’ Busemeyer provides examples of how dynamic systems approaches have influenced theories of perceptual-motor behavior, child development, speech and language, and artificial intelligence. Busemeyer also provides rigorous models (computational and mathematical) of how inputs and outputs from mental processing systems change over time. The article is tutorial, modest, and elegantly written. If you want an introduction to understand how dynamic systems theory can be used to build predictive models concerning cognitive phenomena, then start with Busemeyer.

Second, it is not necessary to reject cognitive science in order to explain the constraints imposed by the body and the environment. Silberstein and Chemero (this issue) propose an approach to explaining subjective conscious experience that the authors call extended phenomenological-cognitive systems (EPCS), but they fail to realize that embodiment and computation are not mutually exclusive alternatives. There are many examples of work that takes the empirical basis of embodiment seriously, for example, Connell and Lynott (2010). There are also many examples of work that take embodiment seriously without throwing out computational approaches to cognition (Gray, Sims, Fu, & Schoelles, 2006; Howes, Lewis, & Vera, 2009; Rothkopf & Ballard, 2010). These theories are intrinsically about an embodied cognition that interacts with a constraining and resource-laden external world. These contributions remain firmly within the cognitive science tradition and provide rigorous empirical and computational explanations for the role of embodiment constraints, including variance. They demonstrate the predictive adequacy of complex computational theories of cognition.

Third, it is not necessary, as Silberstein and Chemero (this issue) appear to do, to reject cognitive science in order to explain consciousness. Heterophenomenology (Dennett, 1991) offers a rigorous approach to the scientific investigation of subjective experience and therefore of verbal reports of consciousness. There is no reason presented in these articles for cognitive scientists to indulge poorly specified pre-scientific concepts such as qualia. Dennett (1991) explains how the mind, including phenomena associated with verbal reports of consciousness, can be viewed as a computational process physically instantiated within the neuronal structures of the brain and therefore within the body. An informal combination of phenomenology, embodiment, and complex systems ideas is a poor match for the simple scientific rigor offered by Dennett’s approach to consciousness.

Of course, it makes sense to describe systems that extend beyond an individual, but there is much existing theoretical and empirical work that already does this. Agent-based systems,
for example, are computational systems that offer theories of information processing that include multiple individuals (e.g., Nowak & Sigmund, 2005). Such systems demonstrate self-organizing, emergent behavior. I see no reason that developing a science of systems that include an individual as one element means that we need to redefine basic terms such as cognition, mind, and consciousness to encompass a broader set of physical entities.

Fourth, our understanding of pragmatics is not advanced by Gibbs and Van Orden’s (this issue) “self-organized criticality.” Self-organized criticality is described in terms of five broad assertions. All of these assertions seem reasonable, but they do not make contact with computational or mathematical theory and it is unclear that they are a radical departure from mainstream views in cognitive science. For example, Gibbs and Van Orden write, “the choices available to pragmatics are motivated by the requirements of interpersonal communication,” but this seems nothing more than a definition of conversational pragmatics. These assertions may well provide a starting point for the development of theory, but it appears that it is early days.

What is important for cognitive science is the usefulness of a theory, that is, how reliable it predicts empirical observations. Unfortunately, the papers presented in this issue leave the reader none the wiser about the predictive adequacy of the proposed approach.

References