The Complex Systems Approach: Rhetoric or Revolution

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Abstract

The complex systems approach (CSA) to characterizing cognitive function is purported to underlie a conceptual and methodological revolution by its proponents. I examine one central claim from each of the contributed papers and argue that the provided examples do not justify calls for radical change in how we do cognitive science. Instead, I note how currently available approaches in “standard” cognitive science are adequate (or even more appropriate) for understanding the CSA provided examples.

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There is revolution on the horizon! It comes in the form of complexity theory mixed with dynamical systems theory, coupled to the observation that cognitive systems are extended brain–body–environment systems resulting in emergent power-law relationships identifiable by multifractality. The authors of the four target articles insist it is time to “move beyond the formalism of information exchange” (Dixon et al.) so we can succeed in “circumventing ... paradoxes” (Riley et al.) that plague cognitive science. The problems with current approaches are deep, but luckily this new approach can “shed new light” (Gibbs & van Orden) on such paradoxes, as it “promises conceptual and methodological advances” (Silverstein & Chemero) not otherwise available. I think I have it right.

But is it right? Does cognitive science need a fundamental shift in its approach to cognition? Or more accurately, does the complex systems approach (CSA) provide a needed fundamental shift to cognitive science? Proponents clearly think so. The quotes in the previous paragraph are taken only from the abstracts of these papers. Many other, far more immoderate claims can be found in the papers themselves. And what is in these papers is all that is left after the many reviewer complaints of rhetorical excesses in the original drafts.

When challenged on some of the more contentious claims during the review process, some authors provided clarifications like “We mean ‘paradox’ in the broader sense of defy-
ing intuitions’’ (Riley et al., personal communication). This may seem a minor semantic issue, but paradox means ‘‘a seemingly absurd or self-contradictory statement’’ (Canadian Oxford Dictionary, 2nd ed.) not ‘‘surprising.’’ Paradoxes might be surprising, but surprising things are not thereby paradoxes. The authors themselves refer to these ‘‘paradoxes’’ as ‘‘inconsistencies in fact and theory of cognition’’ (p. 1) in the current paper, so they seem to have the stronger definition in mind. Worse yet, example paradoxes, such as rapid (ultrafast) perception, are not inconsistent with, nor do they obviously challenge, current perceptual theories (see, e.g., Osadchy, LeCun, & Miller, 2007; Riesenhuber & Poggio, 1999). So, they are not even surprising.

Why does this matter? Because I believe it is symptomatic of a deeper problem with many discussions surrounding CSA. This symptom is seen many times over in the four papers under discussion. The symptom is rhetorical excess; the problem is a lack of useful novelty.

To be clear, I think we have many things to learn by keeping in mind the dynamics of cognitive systems, the environment of cognitive systems, and the complex interactions within and between cognitive systems and their surrounds. Arguments for the importance of these issues have been made since the 1990s at least (Port & van Gelder, 1995; and since much earlier; cf. Gibson, 1966). But then, as now, the case was over-stated and underspecified.

It was over-stated because it simply was not true that contemporary approaches to cognitive science ignored time (environment, etc.). Even paragons of the symbolicist paradigm, such as Newell, worried greatly about the dynamics of cognitive systems (Newell, 1990).

It was underspecified because there were no systematic, quantitative alternatives provided. Yes, there were models that used differential equations (e.g., Busemeyer & Townsend, 1993), but these were piece-meal, vague in their relation to measurable properties of the system, and typically not very cognitive (see Eliasmith, 1996, 2001, 2009, for further discussion). Furthermore, we already had models that were sets of differential equations, which fit perfectly well with talk of computation, representation, and information processing (Hopfield, 1982; Olshausen, Anderson, & van Essen, 1993; Rao & Ballard, 1997), some of the main targets of CSA approaches.

Perhaps things are different now. Perhaps the old terminology really ought to be replaced with new terminology so we can rid ourselves of the constraining, misleading notions of ‘‘standard’’ cognitive science: notions like ‘‘representation’’ and ‘‘computation.’’ Of course, we should not take doing so lightly. No doubt our notion of ‘‘cell’’ is much different in biology now than it was 100 years ago. But it is still a central theoretical element of our biology. This is true of many theoretical terms: mass, heat, neuron, species, atom, the list goes on. But sometimes notions do get thrown out, like ‘‘phlogiston,’’ because they are not part of a theory that captures as much data as a competitor. Is ‘‘representation’’ more like ‘‘cell’’ or ‘‘phlogiston’’?

In short, only if CSA provides a theory that captures phenomena that cannot be captured given the theoretical resources of the ‘‘standard’’ views should we take the calls for revolution seriously. In the rest of this commentary, I suggest that it does not so we should not. That is, there is little, if anything, in the examples provided by the four papers that is not
consistent with current approaches. To make this case, I will consider one central claim from each of the four papers.

I have already briefly considered the Riley et al. discussion of “ultrafast” perception. So how about we consider “ultrafast” action. The claim is that we need “unconventional solutions” to this serious “paradox.” Apparently, a central aspect of the correct solution is motor synergies. Oddly, the notion of a motor synergy has been around since the 1900s (Hall, 1893), long before most elements of the CSA. For the authors, synergies are “compensatory, low-dimensional relations in the dynamic activities of neuromuscular components” and clearly not “static representational structures such as motor programs.” Perhaps less driven by a particular theoretical view, synergies are generally considered “a set of muscles recruited by a single neural command signal” (Torres-Oviedo, Macpherson, & Ting, 2006, p. 1530). Regardless of which definition we pick, the means of incorporating motor synergies into a “standard” description of motor control is to notice that representations do not need to be static.

Recent work in combining optimal motor control and neurobiological constraints adopts exactly the view that the motor system is a computational system that traffics in dynamic neural representations defined over synergies (Dewolf, 2010). The proposed model of the motor system is able to compute optimal trajectories, even under perturbation, in real time by manipulation of representations at many levels of a neural hierarchy simultaneously. There is, in short, nothing odd or untoward in thinking of motor synergies as time-varying representations picked by a computational process realized in a neural substrate. The ideas, methods, and results are all perfectly understandable from a “standard” cognitive science perspective.

Furthermore, in this work the notions of “representation,” “computation,” and their mapping to underlying dynamical neural systems are mathematically defined (Eliasmith & Anderson, 2003). These definitions underwrite detailed simulations of motor behavior. It is unclear to me how suggestions that CSA terminology better describes the phenomena are supposed to convince these authors to change their chosen, well-specified terminology when neither competing models nor unexplained phenomena are on offer.

Turning to the Dixon et al. contribution, they take their discussion to address the “fundamental issue” of “Does cognition arise from the activity of insular components or the multiplicative interactions among many nested structures?” I think this is a good, interesting question for cognitive scientists to ask. I am also unsure why we need CSA to ask it.

In fact, I am not sure what Dixon et al. need CSA for. They are, for instance, comfortable with representations: “The sudden, spontaneous discovery of a new relationship, alternation, marks the emergence of a new cognitive structure, by all conventional accounts, a new representation of the problem (Dixon & Bangert, 2002).” So internal representations are at least permissible. However, one concern with their account is that the story stops there. That is, it suggests evidence for a new representational structure, but it says nothing of what that structure is, how it enters into processing, what mechanisms give rise to it, and so on. These are the crucial questions for cognitive science.

So, the observation that changes in power-law exponents correlate with new cognitive structure convinces me that the brain is a physical system sharing organizational principles
with other highly coupled nonlinear physical systems. That is good to know, but it does not depend on CSA. This exact essay could well be cast as an *internal* critique in “standard” cognitive science: a critique, that is, of models that do not have enough interaction among their components or assume additive white noise. Surely not all, probably not even a majority, of “standard” cognitive science is captured by the assumption that “cognitive architectures comprise functionally independent components.” That is simply a straw man characterization of the field.

In the concluding paragraph to the article, Dixon et al. suggest that “in this light, the cognitive system shows itself to be a physical system built of components that interact similarly at very many scales.” This seems a fine conclusion that their data and arguments support, which is perfectly understandable, characterizable, and relevant without CSA. The very last sentence: “The multifractal dynamics of the fluctuations in the cognitive system provide complex systems approaches to cognitive science with new leverage on solving the problem of how a cognitive system evolves;” however, seems an unconvincing attempt to make something out of nothing. What leverage does nonlinear coupled interactions provide CSA that is not available to others in cognitive science? Surely, CSA does not have a monopoly on the ability to include such features in a model or theory. What specific models does CSA provide that explain the evolution of such a system in a way able to match data or provide explanations not otherwise available? There are no provided examples in this paper (or the others, which is, of course, my main thesis).

Let me turn to a third contribution, the Silberstein and Chemero paper. In this work, they suggest that in “dynamical systems explanation, one adopts the mathematical methods of nonlinear dynamical systems theory, employing differential equations rather than computation as the primary explanatory tool.” The difficulty here, as I have been at pains to point out elsewhere (Eliasmith, 1996, 1998, 2009), is that such claims are simply a false dichotomy. We do not need to pick between computation and differential equations. The CSA has no monopoly on “nonlinear” or “dynamical” or “differential equation” or “interaction dominant” systems, as I observed earlier. Sure, brains are dynamical physical systems, but so are digital computers. We can describe computers as computational systems, and as I mentioned earlier, detailed mathematical theories of how brains are computational systems have been on offer for over two decades (Abbott & Sejnowksi, 1999; Anderson & Essen, 1995; Bialek & Zee, 1990; Eliasmith & Anderson, 1999; Eliasmith & Anderson, 2003).

Why proponents of CSA feel that these suggestions are unwelcome or somehow illegitimate characterizations of brain function needs to be addressed. Pointing to a kind of mathematics is not the same as identifying conceptual differences. Especially, when many kinds of mathematics have been used to characterize cognitive systems in “standard” cognitive science.

On a separate but related note, the kinds of conceptual change that are suggested are often simply counter-productive: “In extended cognitive science, nonlinearly coupled animal-environment systems are taken to form just one unified system. This removes the pressure to treat one portion of the system as representing other portions of the system—at least for many cognitive acts. That is, if the animal-environment system is just one system, the animal portion of the system need not represent the environment portion of the system to main-
tain its connection with it. There is no separation between animal and environment that must be bridged by representations.” Treating something as “one” system does not remove representation. Surely, a computer-controlled production line can be treated as one system. This does not mean that representation and computation disappear. After all, systems have sub-systems. Choices about where system boundaries (and sub-boundaries) are can be important for the ways we talk about them. Nevertheless, simply pretending there is no boundary at some level of analysis does not settle the matter.

The final contribution, from Gibbs and van Orden, looks like it should be answering my challenge. That is, there are strong claims that new, specific explanations are available if we adopt CSA: “Our purpose in this article is to advance a view of pragmatics based on complexity theory, which specifically explains the pragmatic choices speakers make in conversations.” I take the phrase of crucial importance here to be “specifically explains the pragmatic choices.” This seems to suggest that if we provide a specific conversational setup, CSA can determine what specific choice someone will make in that setup.

But this does not turn out to be the case. Quite the opposite in fact: “We cannot know sufficient contingent details to predict with certainty the proximal causes of saying...” or “Instability of critical states plus unpredictable contingencies also explains why speakers often utter words that are not necessarily consistent with one another.” In other words, pragmatic choices are so complicated, and dependent on detail, that we cannot get specific explanations. Here is another example: “All the available sources of constraint, in the moment before saying any word or making any verbal sound, contribute constraints to self-organize the anticipatory critical state.” In short, everything matters. So it seems a CSA analysis suggests specific explanations are not available. I do not think we need CSA to point out that pragmatics is difficult and complicated. Beyond that, there is definitely no evidence in this paper that CSA “specifically explains the pragmatic choices” of a speaker. Maybe it explains why CSA doesn’t explain the pragmatic choices.Personally, I am more hopeful that we can build detailed neural models that will be able to predict, at least, distributions of phrase choice, or perhaps the classes of linguistic act that are chosen under different circumstances.

But of course, we are not yet there. And I do not see any suggestions in this work of how CSA is more likely to get us close to such a goal than other methods in “standard” cognitive science. Do we really need CSA to get to the conclusion of this article: “What is certain, today, however, is this: No known factor stands alone to produce human performance”?

In conclusion, I would like to again emphasize that I take none of my considerations here to suggest, in any way, that dynamical, nonlinear, interaction dependent models are undesirable. Quite the opposite, in fact. I take most of my own work to be of precisely this nature (Eliasmith, 2005; Rasmussen & Eliasmith, 2011; Singh & Eliasmith, 2006). The point is simply that calls for revolution in cognitive science are vastly overstated. Currently, it is rhetoric, not revolution, that is on offer. Surely cognitive science has many challenges ahead of it, but it is not in a state of crisis as proponents of CSA would have us believe. And, furthermore, it is unclear that CSA has a useful conceptual revolution to offer. So it seems that notions like “representation,” “computation,” and “information processing” may change, may need reworking, may need fine-tuning—but they do not need throwing out.
References


