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Response to Commentaries on "Representation and Radical Empiricism"

Teed ROCKWELL*

My paper *Representation and Radical Empiricism (*Rockwell 2013, this journal) was an attempt to broker something like a peace deal between Anthony Chemero's Radical Embodied Cognitive Science (RECS) and the Representational Theory of Mind (RTM). My basic strategy was to divide representations into two categories, which I called *analog representations* and *computational representations.* As Chemero had already made a similar distinction in the 3rd chapter of his 2009 book, I saw our differences as being rather minor. My two main objections were 1) Occasional passages where he would deny that certain cognitive process involved representations, without clearly saying what was doing the theoretical work ordinarily done by representations. I referred to these kinds of explanations as "magical", because they seemed to substitute hand-waving for actual explanation. 2) Although RECS gave us good reasons for rejecting computational representation, I argued that it would be advisable for RECS to continue to use explanations that relied on some (perhaps metaphorical) concept of analog representation.

The commentaries published with that paper contained objections from both sides of the controversy. Winters and Swan (W&S, 2013) objected to my claim that classifying Mind/World isomorphisms as analog representations was problematic and metaphorical. Martin and Chemero (M&C, 2013) restate and refine the assertion that representations are unnecessary for certain kinds of cognitive science, and misinterpret my accusation that some of Chemero's explanations are magical. This misinterpretation is almost the exact opposite of what I actually believe. Nevertheless, it is an understandable extrapolation from some carelessly chosen words of mine, particularly since there are many people who do believe the position M&C are attributing to me.

My accusation about magical explanations is much narrower than M&C think it is. I do not believe that "RECS requires magic" (M& C 2013, p.255). On the contrary, I think avoiding this kind of magical rhetoric is fairly easy. One merely needs to stop using locutions like "one can simply see […] how fast something is moving without computing it." (Chemero, 2009, p. 122). As there is nothing simple about the dynamic processes that Chemero is describing in this context, whatever it is that is allegedly simple seems inexplicable and mysterious. The only thing we are told is that this process is not representational, and the lack of positive content makes this explanation appear magical. I also have problems with the phrase "direct perception" as used by ecological psychologists like J.J. Gibson, when it is paraphrased with locutions like "the information of the light just *is* this relation between the light and the environment" (Chemero, 2009, p. 108; italics in the original). The default assumption that most of us make is that if anything in the organism is causally responsible for the occurrence of the perception, it is no longer direct,

* Philosophy Dept., Sonoma State University, Berkeley CA, 94710. teedrockwell<at>gmail.com.

and thus to say that a perception is direct is to say it happens by magic. I think my redefinition of "direct perception" in Rockwell (2013) gives enough content to the concept that even if my definition is wrong, it is no longer empty. However, as they are currently used, phrases like "just see" and "direct perception" imply that one is going to simply duck the question of how those processes actually work, and have resulted in RECS acquiring many unnecessary enemies.

M&C interpreted my accusation as applying not just to locutions of this sort, but to any kind of explanation that relies on dynamic systems theory. "Rockwell thinks that RECS requires magic because he thinks dynamical and ecological explanations of cognition and perception are themselves legitimated only by being given a mechanistic interpretation." (p. 255). It appears that Chemero inferred my alleged commitment to mechanism from the fact that I wanted to replace computational models of cognition with connectionist ones. According to Chemero, connectionism "is committed to explaining cognitive capacities in terms of mechanisms, whether those mechanisms are composed of idealized connectionist nodes or real neural populations." (*ibid*). Many people believe this, but I don't. In Rockwell (2005a), I argue that the fundamental cognitive unit in the nervous system is not the neuron or neurode, but an attractor space in a dynamic system. In chapter 10 of Rockwell (2005), my goal is to "redescribe the principles of cognitive vector transformation in ways that make no specific reference to neurons" because "neural networks are only one species of dynamic system that can be described by these generic principles when they are formulated at the appropriate level of abstraction." (p. 191-192).

As far as I can see, a mechanical explanation is a relatively crude lower-level physical explanation and a dynamical explanation is a sophisticated higher-level physical explanation. The mind, being a very sophisticated machine, will require the most sophisticated physical explanations we can formulate, which I believe are the dynamic systems theories used by RECS. I have, in fact, argued in Rockwell (2008) against the mechanistic view that we can understand a system only by analyzing it into its component parts, because I believe that higher level systems have their own causal powers that cannot be reduced to the causal powers of their components. Relatively few people agree with me on this, but Chemero is, or should be, one of them. M&C seem to be arguing that we should accept something like this as an heuristic assumption, even if they do not explicitly embrace it as a metaphysical or ontological principle.

So if I agree with M&C about all of this, do we actually have any other areas of disagreement? To answer this question, I need to make a third distinction that puts M&C's distinction into a single category. Both mechanical and dynamic explanations are *physical* explanations, which are both importantly different from *functional* explanations. The concept of representation is a functional concept, as are all the concepts that distinguish engineering from physics, and biology from organic chemistry. Consequently, Chemero's detailed argument that dynamic explanations are superior to mechanical ones misses my point. Neither mechanical nor dynamic explanations can substitute for the Representational Theory of Mind, because RTM is a functional theory, not a physical one. To say that a process is not

representational because it is dynamic is a kind of category mistake, rather like saying "this can't be the letter 'A' because it's green."

Cognitive science is a branch of engineering whose goal is to reverse-engineer those biological processes we call cognitive. No biological process can be fully understood by a complete physical description, regardless of whether that description is either dynamic or mechanical. A complete chemical analysis of the protein of the heart, and the fluid dynamics of blood flow, cannot substitute for a medical or biological description of the heart. A biological explanation accounts for the purpose and function of pumping blood, whose function in turn is to provide oxygen for the rest of the body, etc. As Ruth Millikan points out, "representation", like "heart", is a biological category, which can only be understood by articulating its proper function. The dynamic explanations of the two examples in M&C don't appear to require any reference to representation, and they seem to be complete in themselves. This doesn't prove anything, however, because this alleged completeness is an illusion produced by sidestepping the fundamental goal of cognitive science--explaining the functional significance of those physical processes. There would be nothing "missing" in a description of the heart that contained nothing but the physical and chemical facts about the heart. But that's just because it is not the job of physics to explain biological functions.

I am not saying that these functional descriptions are autonomous with respect to physics, nor am I saying that physical discoveries cannot lead to the reduction and elimination of our currently used functional concepts. But a new theory cannot reduce or eliminate an old theory simply by ignoring it. It must explain why the old theory appeared to be right about many things, and why the new theory is better at resolving the puzzles and crises that couldn't be resolved by the old one. I think that in this case, it is not enough to replace talk about representations with talk about attractor spaces in dynamic systems. We must also talk about the way that our traditional ideas about representations both resemble and are different from attractor spaces in dynamic systems.

M&C believe that RECS has dispensed with the need to talk about representations, or any other functional concepts, because it relies entirely on the language of physics. In fact, RECS still relies on metaphorical extrapolations from the concept of analog representation, even in examples that M&C consider to be paradigmatic. They argue, I think correctly, that their example of the Sussex robot (M&C, 2013, pp. 256-258) is more accurately explained by dynamic principles then by mechanical ones. However, their dynamic explanation relies heavily on the concept of *phase portraits* within the basin of an attractor space. Two points are relevant here: 1) A portrait is a kind of picture i.e. an analog representation. 2) A phase portrait is not literally a portrait the way the Mona Lisa is a portrait. It is a metaphorical extrapolation from the concept of analog representation, of precisely the sort that I was defending in my original paper. Chemero could say this is a moment of weakness, like our expression that "the sun rises" to indicate that the earth has rotated. I think, on the contrary, that it is expressions like these that connect these dynamical models to actual cognition, and thus save RECS from being nothing but physics.

In the long run, I think that RECS will develop a concept of representation that differs from the common sense concepts of "word" and "picture" as much as modern atomic particles differ from billiard balls. I also think, however, that we can only get there by starting with the common sense concepts, or even better, tracing the common sense concepts back to their roots. That was why I argued that the prototypical concept of representation derives from the human activity Dewey calls inquiry, and that the use of that concept becomes metaphorical and problematic when applied anywhere else. One thing that does get preserved in these metaphorical extrapolations, is that the concept of inquiry remains irreducibly functional. This is why W&S miss the point when they say that if an organism maintains any sort of relationship with its environment, that relationship is "harmonious insofar as the organism is not destroyed" (p. 246). Organisms have goals and purposes other than survival – they prefer certain food and mates over others, they compose music and write philosophy etc. – and the relationship between organism and environment can be evaluated as harmonious only by making reference to those goals and purposes.

I am not claiming that inquiry is ontologically or metaphysically prior to representation, as W&S seem to think. I am also not trying to provide a precise definition of representation, because I don't think there is one. I am offering something more like an Anthropological genealogy, that will help us understand how we came to use the concept and how we are using it now. The representational theory of mind was born when cognitive scientists transplanted the embodied human practice of Inquiry into the brain, and put the label "representation" on the isomorphisms between the brain and the world. This was a plausible transplant, because isomorphism is a necessary characteristic of representations, even if it is not sufficient to define them. But neurological and dynamic research has forced us to stretch the concept almost to the point of unrecognizability.

The structures that make logical inferences from directly perceived sense data are nowhere to be found in the dynamic neurocomputations we have actually discovered in biological organisms. Thus even though no experiments have been done to show the non-existence of sense data, the distributive nature of connectionist systems is inconsistent with what W&S call "the acquisition of knowledge that happens over time, layer by layer."(p. 249) In a connectionist system, the information cannot be separated into discrete layers, because, as Walter Freeman discovered, every new piece of information effects all of the old information in the system (Rockwell, 2005, pp. 199-201). It can always be maintained as an article of faith that some day we will discover digital processes in our brains. But even though we obviously can manipulate the discrete bits of information used by digital information systems. there seems no reason at this point to believe that we do this by means of digital systems inside our heads. This is really no more surprising than the fact that we can play tennis even though we don't have a tennis court inside our heads. The analog processes in our minds utilize scaffolding in our environments to manipulate digital representations. We are, in other words, analog computers that know how to build and use digital computers.

There are also problems with seeing all analog isomorphisms as portraits or pictures. Organisms use isomorphisms between themselves and the environment to achieve their goals, but not all such isomorphisms are representations, and we still don't have precise criteria for identifying representations. W&S's attempts to distinguish representations from other isomorphisms are unsatisfactory. They define mental representations as "specific repeatable patterns of neural activity that uniquely represent certain features of the environment." (p. 250), "which is circular because it basically says "representations are things that represent." The fact that my example of diapers and beer may be a correlation, rather than a causal relationship, is also irrelevant, because most causal isomorphisms are not representations. If I cut my finger, the knife causes it to bleed, but no one believes that either the bleeding or the knife represent each other.

Nevertheless, until RECS come up with an alternative functional category, it has no real alternative but to stretch and fiddle with the problematically technical concept of "representation" that it has inherited from the Representational Theory of Mind. The best RECS work is already doing this (viz. my above discussion of the phase portraits in the Sussex Robot) and should continue to do so. I think this will eventually lead to a new technical concept of representation that can serve as a scientific reduction of both the RTM and common sense concepts. If the most sophisticated cognitive systems are non-linear, the RECS functional categories might even eliminate the concept of representation altogether. Although isomorphism is not sufficient to define representations, it is necessary. It appears that in a non-linear system, there are no strict isomorphisms between an organism and its environment, even when the organism is behaving with great cognitive skill. Without isomorphisms, there would be no representation of the environment by the organism, as far as I can see, and RECS would have delivered on its promise of a genuinely non-representational cognitive system. That is why I believe Chemero's work with non-linear systems is his most important, at least philosophically.

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