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Avant 3/2, 18-27

2012

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With the most profound misgivings Interview with Anthony P. Chemero

Dawid Lubiszewski, Witold Wachowski

Anthony P. Chemero

Departments of Philosophy and Psychology, University of Cincinnati, USA
anthony.chemero@uc.edu

Realized March 2012; published online 24 December 2012.

What is a thing? Is this something that contains and discloses truth about our environment, or rather truth about us, observers?

“An affordance cuts across the dichotomy of subjective-objective and helps us understand its inadequacy”¹². It seemed to be a somewhat unclear but significant statement. Does it still?

I'm not sure I have a general answer to the question about what a thing is. It seems to me that there are many different kinds of things, and there can be a different answer for each of them. I have written mostly about a specific variety of things, what Gibson called 'affordances'. Affordances are the entities that animals perceive and act upon. I agree with Gibson that affordances cut across the subjective-objective distinction, and also that they cut across the mental-physical distinction. This is because affordances are intelligible only in terms of both the abilities of animals, including perceptual abilities, and the features of the environment. So, they really are both mental and physical. So, as you put it, the theory of affordances discloses truths about both observers and environments. The theory of affordances, I also believe, fails to disclose truths about either observers or environments separately. Gibson (and later Gibsonian psychologists) have put this by saying that psychology studies animal-environment systems.

What about other sorts of things? I should point out here that the conception of affordances is (non-viciously) circular. Affordances are only intelligible in terms of abilities, and vice versa. So abilities also cut across this subjective-objective dichotomy. One could also define all sorts of other things in terms of affordances, thereby making affordances some sort of ontological primitive. Doing so would make every kind of thing disclosing of truths about both observers and environments. I have, for example, written about defining perceivable events in terms of affordances. Beyond that,

¹² (Gibson 1979: 129)

however, I'm not sure that generalizing from the funny subjective-objective character of affordances is a good idea. That is, I doubt that everything is both subjective and objective like affordances are.

Certainly, it was not Gibson's intention to claim that everything was an affordance. Rather, he wanted to argue that affordances existed in the environment along with the trees and monkeys. That is the sense in which Gibson was a realist. The things we perceive are not constructed in our heads, and projected onto the world; instead, the things we perceive exist in the world. I feel confident that he was right about this.

For the short answer: Why is Gibsonian theory the best theory of the nature of animal environment systems for radical embodied cognitive science?

Philosopher Martin Heidegger in your account seems to be very... ecological-psychology-hungry. ("Heidegger in the Lab: When tools break down" – an excellent title!) What has inspired you in his work most of all?

I got very interested in Gibson via reading phenomenology, really. I happened to be reading both Merleau-Ponty's *The Phenomenology of Perception* and Gibson's *The Ecological Approach to Visual Perception* at the same time while I was in grad school. I was struck by the similarities. Prior to that, I had been hanging around Tim van Gelder, and reading lots of Heidegger, along with lots of robotics and dynamical modeling. The set of ideas that I eventually started calling 'radical embodied cognitive science' (that name is stolen from Andy Clark, by the way) started there. When I read Gibson, I thought that he had outlined the way that phenomenologists should do scientific psychology. Reading work by later Gibsonians, especially Mike Turvey and Bill Warren, only made me more convinced of this. Radical embodied cognitive science is, in large part, a philosophy of science for the science that folks like Turvey and Warren do.

That was a genealogical answer to a conceptual question. So here is the conceptual answer, which I wrote about in one of my first publications. When people talk about mental representation, they typically have in mind an organism confronting a independent, structured environment, and the organism having some structures on its inside that stand in for the structures of the environment. So, if you want to be an anti-representationalist, you can say that there is nothing on the inside of the organism that is the right sort of structure. Alternatively, you could say that the environmental structures are not independent of the organism in the right way. It seemed to me that the Gibson was making both of these claims: representations inside of organisms are the wrong way to understand perception, and that the things that organisms perceive are not fully independent of the organisms themselves. Furthermore, Gibson's ideas—unlike Heidegger's and Merleau-Ponty's—have already been the engine for lots of scientific discovery.

To what extent have you retired from Gibson?

Over the last several years, Rob Withagen and I¹³ have been developing some of Gibson's ideas in ways that most ecological psychologists do not like, precisely because they contradict some of the things Gibson wrote. This gets pretty technical, but the main area of disagreement is over what is required for some pattern in light (or vibrating air, etc.) to carry information about the environment. Basically, Gibson and later Gibsonians (especially, Mike Turvey, Bill Mace, and Bob Shaw) argued that in order to carry information, a pattern in light (etc.) has to be 1:1 correlated with a particular environmental event. Rob and I think this is far too strict, and that we guide our behavior by lots of patterns in light (etc.) that are only probabilistically related to environmental events. Here's an example. Suppose you are in your office with the light on. I could, from outside the building, see that your light is on (a particular pattern) and guide my behavior as if you are in your office. We would say that the light being on carries information about your presence in your office. But, sometimes your office light is on when you are not, in fact, in your office. Gibson (and later Gibsonians) would say that the possibility that you are not in your office when the light is on means that the light being on can never carry information that you are in your office. Rob and I disagree.

This molehill can be made into a mountain, because it implies different conceptions of affordances and direct perception, which Rob and I have also developed. In doing so, though, we don't think of ourselves as abandoning Gibson's ideas, but as fine-tuning them. Many Gibsonians think we are abandoning Gibson.

There are many definitions of emergence and self-organization. Both phenomena are connected especially in ecological approach. Furthermore, it is not so clear what lower(micro) level and higher(global) level are. How do you understand it?

I typically try very hard not to use the 'E-word' because it has so many connotations, and is taken by some people to signal fuzzy headedness. I have, however, tried to say in some detail what self-organization is¹⁴, mostly in work collaborating with Mike Turvey. Before I talk about that, I should say something about why ecological psychologists care so much about self-organization, because it's not necessarily obvious why they would. In the ecological approach, perception of the world is not a matter of adding information to sensory representations; instead it is a matter of keeping in touch with the environment. Because perception doesn't end with representations of the environment, action cannot come from developing plans by manipulating representations. So the ecological approach requires an understanding of action that doesn't require plans. As Gibson put it, action needs to be regular without being regulated. There is no internal agency doing the driving. Scott Kelso, Peter Kugler, and Mike Turvey realized that a good way to think about action as regular but not regulated is to

¹³ (Withagen & Chemero 2009, 2012)

¹⁴ (Chemero 2008)

think of it as self-organizing. That was more than 30 years ago, and there is now lots and lots of evidence that they were right.

OK, now to the actual questions being asked: what is the right way to understand self-organization, and how should one understand the relation between levels? Unfortunately, there are lots of names for self-organization, even though they all point to the same basic phenomenon, and most descriptions of that phenomenon are highly technical and/or mathematical. The basic idea is pretty simple, though. You can see self-organization every time you flush your toilet, in the whirlpool that forms as the water flows out. This whirlpool is a self-organizing pattern of activity. It is made up of a constantly changing collection of water molecules. When the water molecules are in the whirlpool, their activity is constrained by their being in the whirlpool. Although whirlpools and other instances of self-organization are ubiquitous in nature, they look very strange from the point of view of physicalism and mechanism. The whirlpool is not identical to any collection of water molecules, but it is not something in addition to the water molecules. The whirlpool, which is made up of water molecules, changes the behavior of the water molecules. So, if we want to put this in terms of levels, we have a macro-level whirlpool shaping the behavior of the micro-level molecules that make it—the macro-level whirlpool—up. Notice that this is a pretty dramatic departure from the ways philosophers of mind and philosophers of science typically discuss levels, in that the macro-levels cannot supervene on the micro-levels because they causally interact with them. That is, we have “downward causation” in exactly the sense that is supposedly impossible according to mechanism and physicalism. Michael Silberstein¹⁵, a frequent collaborator, often puts this by saying that physicalism isn’t even true in physics. And Michael and I¹⁶ have been writing a lot about consciousness in terms of the sort of micro-macro relations we see in self-organizing systems. (See below.)

The theory of self-organization and emergence is important in ecological psychology because some of its important aspects like affordances are described in terms originating from those theories. It is said that affordances are emergent results of self-organized interaction between agent and environment. It also means that behaviour is such an emergent property. In many studies scientist are looking on how certain behaviours like grasping or walking emerge. One object of such studies may be robots. Do you think that other, more sophisticated behaviours like moral actions will emerge in robots the same way as walking and grasping?

I can’t make a good prediction about whether moral behavior will be emergent in robots. For now, I must admit, though, that I’m not confident that it will. Mostly, I believe this because I think that there is too much at stake with the sort of robots that are on the horizon (i.e., military robots) to allow ethical rules to be anything but explicitly built in by engineers, under guidance from policy makers.

¹⁵ (e. g. : Silberstein 2002)

¹⁶ (e. g. & Chemero & Silberstein 2008)

My shaky predictions about the future of robot moral behavior do not reflect my views about moral behavior in animals. I believe that moral actions in animals work in exactly the same way as other actions: they are emergent in animal-environment systems. A few years ago, I published a paper about this with Eranda Jayawickreme¹⁷, a former student who now teaches at Wake Forest University. Eranda and I argued that moral virtues should be understood as a kind of ability to act. Just as abilities to walk and grasp are only intelligible in relation to affordances for walking and grasping, we argued that virtues are only intelligible in terms of moral affordances, i.e., opportunities to act morally. If this is right, moral action would emerge in animal-environment systems in exactly the way that walking and grasping do. The paper was just a sketch, and there is a lot more work to do on it. I haven't had time, unfortunately, but Eranda has carried this a little further. He's been using the ideas of virtues as abilities and moral affordances to understand heroism.

“There is no need to posit representations of the environment inside the animal (or computations thereupon) because animals and environments are taken, both in theory and models, to be coupled”; “Dynamical systems theory can also provide nonrepresentational explanations of internal brain processes”¹⁸. We have an untoward inquiry: To what extent can one talk reasonably about consciousness and mind without relating to neuroscience, head, brain in a vat?

I don't think we can explain consciousness without neuroscience, and things in the head will surely be part of the explanation of consciousness. But I feel very confident that we won't be able to tell the whole story about consciousness in terms of brains. Saying this alone is enough to indicate that I give absolutely no credence to the idea of consciousness in a brain in a vat. First, on the brains in vats, everyone should read the paper by Diego Cosmelli and Evan Thompson in the 2011 collection *Enaction*. They consider in some detail exactly how brains work, and in so doing pretty definitively crush the very idea of a brain in a vat. I won't spoil their punch line here. Even more strongly, though, than rejecting brains in vats, I reject the idea of neural correlates of consciousness. There are no correlates of consciousness because consciousness, like thinking more generally, happens in brain-body-environment systems. There's a small discussion of this in my book, and a longer discussion in a recent paper I've written with Michael Silberstein.

Claiming that consciousness doesn't happen in brains alone might strike many people reading this as crazy, even though similar claims have also been made by Evan Thompson and Alva Noë. In today's brain-centric intellectual climate, the claim is undeniably counterintuitive. But Michael and I argue that the current problem space for discussions of consciousness is a dead end, essentially forcing you to be a reductionist or a dualist—there really aren't other stable and convincing positions. The advantage of rejecting the idea of neural correlates of consciousness is that it gets you out of this dilemma. That is, it makes it possible to claim, to adapt a phrase from Ryle, that con-

¹⁷ (Jayawickreme & Chemero 2008)

¹⁸ (Chemero 2009)

sciousness is neither nothing but brain activity, nor is it something else in addition to brain activity. Michael and I argue that consciousness is best understood as the activity of nonlinearly coupled brain-body-environment systems. Maybe this is crazy, but at least it has the advantage of pushing us out of the current dead end arguments.

Are there any dangers threatening cognitive science? And are there any dangers threatening ecological psychology?

I think that both cognitive science and ecological psychology are chugging along reasonably well at the moment. To the extent that they are under threat, both are under threat from the same source: neuroscientific reductionism. There is a way of interpreting research in neuroscience, quite common among neuroscientists that I have met and often reported in the popular press, that takes neuroscience as a replacement for psychology. That is, many neuroscientists think that, eventually, the psychology and cognitive science departments will go away. At the moment, it seems to me that this sort of reductionism is winning the public relations battle. In optimistic moods, I think that this is a temporary phase.

When you commented on “Information, Perception, and Action” by Michaels¹⁹, you pointed out her misinterpretation of empirical evidences²⁰. Do you see many examples of misinterpretation of empirical studies?

Let me start by saying that I don't think that Claire Michaels *mis*interpreted her evidence in that study. The main thing I did was to suggest an alternative interpretation, one which seemed to me to have happier consequences than her interpretation.

OK, that aside, I would say that there is always selective interpretation of data. This is inevitable. People are bound to design experiments and interpret findings in light of their theoretical assumptions. And sometimes, those theoretical assumptions will make them blind to important causal factors. I've been thinking a lot about this lately, actually, inspired by the work of philosophers of science such as Bas van Fraassen and Isabelle Peschard.

Peschard looks at some of the experiments I've done with Charles Heyser²¹. Charles and I think that neuroscientists who use the object exploration methodology to study rodents aren't careful enough about the objects they allow animals to explore. We showed, in a series of experiments, that mice preferentially explore objects that are climbable. We concluded from this that neuroscientists need to take an explicitly embodied approach, and focus not just on neurotransmitters, but on neurotransmitters-in-brains-in-bodies-in-environments. Peschard points out that the disagreement is really over what is taken to be *relevant* in explaining the behavior. We are saying that features of bodies and the environment are not just causally active in determining

¹⁹ (Michaels 2000)

²⁰ (Chemero 2001)

²¹ (Chemero & Heyser 2009)

behavior, but also that they are relevant in explaining mouse behavior. No neuroscientist who uses the object exploration methodology doubts that bodies and environments are causally active in mouse behavior; they just didn't think they are relevant. They could respond to our experiments by admitting that bodies and environments are relevant, or they could take features of environments as something to be controlled for in designing experiments. That is, a neuroscientist who does not share our commitment to an embodied, ecological approach can admit that features of objects are causally active in determining behavior, but are nonetheless irrelevant in the scientific explanation of behavior.

One more example, very briefly: Another recent set of experiments I've been working on with students and former students (Dobri Dotov, Lin Nie, Kevin Wojcik) focuses on some claims derived from Heidegger's phenomenology. (Here's a link to an open-access publication:

<http://www.plosone.org/article/info:doi/10.1371/journal.pone.0009433>.) In our experiments, the primary thing we measure are the hand-mouse movements of a person playing a simple video game. What we are interested in about the hand movements is their variability, and nothing about their central tendency (i.e., their average). That is, what we are interested in primarily is exactly what is generally thrown out as noise in most other experiments. From our point of view in these studies, the noise is the primary piece of data.

This difference over what is taken to be relevant is probably the biggest difference between "paradigms" in cognitive science, and the most important thing that leads to differing interpretations of data. Neuroscientists agree that the body and the environment are causally important in determining behavior; they just doubt they are relevant. Gibsonians agree that happenings in brains are causally important in determining behavior; they just doubt they are relevant.

"I have not shown that radical embodied cognitive science is the one true story about the mind or cognition or even perception-action. No clever philosophical argument can do that"²². You promote the idea of explanatory pluralism. You claim that we can be both situated, embodied cognitive scientists and realists. Do you believe in these ideas gaining acceptance in academic circle?

I have actually been surprised at the resistance to explanatory pluralism. It seems to me to be an acknowledgement of the complexity of nature, along with our limitations as investigators. Ian Hacking puts this really nicely when he says that the world is so rich and multifarious that no one story could ever be true of the world as a whole. I would say the same thing about the mind. No one theory will be able to explain perception, action, reasoning, social interactions, creativity, and so on. Many people are less modest about the theories they believe. There are two basic reasons for this. First, many people believe that it should be the goal of the sciences to eventually have unified theories. Second, many people feel that their particular theory of the mind or

²² (Chemero 2009: 208)

explanatory style will be able account for every phenomenon. These two objections to pluralism can both be admirable. Unification is a worthwhile goal, of course, and good explanations should unify apparently disparate phenomena. It is a good thing as well for scientists to attempt to apply their theories to as many phenomena as possible, if only to determine their limits. But these admirable impulses are too often turned into pieces of metaphysical or normative dogma. For example, some mechanist philosophers of science insist that *only* mechanistic explanation is legitimate.

As for the embodiment and realism, I'm not sure many people care enough to agree or disagree. One exception, I suppose, is Tom Ziemke²³, who reviewed my book and devoted a lot of the discussion to my discussion of realism. He politely disagreed with my conclusions.

It has not been a long time since you published your last book *Radical Embodied Cognitive Science*, but you are a very active researcher. Is there anything that you would like to change, add or remove from the book?

What is the most problematic question for you in your work recently?

If you had to attack Anthony Chemero's approach, what would you choose as a blind-spot?

Of course, there are many, many things I wish I'd done differently in my book. There are things I should have said differently, and things I wish I had written more about. One mistake I made was to focus on Gibson and dynamics too much, and in so doing failed to reflect the strength of the influence of phenomenologists on the ideas in the book. I must admit that this was partly strategic: I thought that the ideas in the book would strike as being too strange already, and I didn't want to give mainstream cognitive scientists and philosophers of cognitive science yet another reason to dismiss the book without reading. Colin Klein, another former student, read a draft of the book and said something like "All the Gibson and William James is weird enough, but Feyerabend? That just goes too far." Imagine if I had also added lots of Heidegger and Merleau-Ponty. Though it might have scared off some readers, I think that the book would have been better if I had allowed more of the phenomenology in. I make up for this in my next book, I guess, which is co-authored with my colleague Stephan Käufer and is actually about phenomenology. Stephan and I argue that the heirs of the phenomenological tradition that begins with Husserl, Heidegger, and Merleau-Ponty are scientists, not philosophers and literary theorists. In particular, we argue that beginning in the 1960s the tradition of phenomenology is taken up by several groups of cognitive scientists and neuroscientists: the practitioners of ecological psychology, enactivist cognitive science, neurodynamics, cognitive linguistics, and Heideggerian artificial intelligence and robotics. So the legacy of phenomenological philosophy is not post-modernist literary theory; rather, it is research in the cognitive sciences that attempts to explain lived, human experience.

²³ (Ziemke 2001)

The second major thing I think is really wrong with the book is in the discussion of dynamics. There are two main problems with it. The first is that I had focused too much on research closely related to the Haken-Kelso-Bunz (HKB) model. Focusing so much on HKB was rhetorically useful in a lot of ways: it is easy to understand and already familiar to a lot of readers; it allowed me to make a lot of points about how dynamical cognitive science works; and it allowed me to show how dynamical models could provide guides to discovery; how it explains both by making predictions concerning novel phenomena and by unifying apparently disparate psychological and neural phenomena; etc. But... it is hardly the state of the art in dynamical modeling, and I wish I had focused more on some newer varieties of dynamical modeling. The second problem is that I underestimated the popularity of the “dynamics doesn’t explain, it only describes” objection to dynamical modeling. Much to my surprise, I hear that dismissal of dynamical cognitive science and neuroscience all the time. Had I known that *anyone* still believed that this was an effective objection, I would have addressed it in the book. Now that I realize that *lots of people*, including people I respect like Bill Bechtel, believe this objection, I am working with Michael Silberstein on a new theory of dynamical explanation. We’ve just submitted the first of what will be a series of papers on this.

Preface and beginning of the first chapter of your book was a sort of manifesto for our academic association, from which the AVANT journal originated. Were you, so to speak, asking for trouble writing this text, or a declaration like this?

One of the central tenets of my academic worldview is that things are supposed to be fun. It should be fun to write the things you write, and people should enjoy reading them. This is especially true in philosophy, where the stakes are pretty low. I haven’t always succeeded in writing things that are fun to read, but I think that I did succeed with several chunks of my book, including the beginning. I wouldn’t say, exactly, that I was asking for trouble, at least not at first. But when I saw the perplexed way some (especially, older) philosophers reacted to talks about this material, I thought it would be fun to push it farther. Let’s not just compare Chomsky and Fodor to Hegel; let’s also compare them to intelligent design theorists!

It is important to realize, though, that Chomsky and Fodor are among my heroes. I sent a copy of my book to Fodor. He sent me a kind letter, saying he “looked forward to reading it with the most profound misgivings.” The letter is hanging on the wall of my office.

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