THE HOPE OF A RADICALLY EMBODIED SCIENCE

Alan Costall  
University of Portsmouth


Students of psychology are taught to regard the Representational Theory of Mind as a relatively new invention, attached to the rise of modern computer technologies. Yet, as Jerry Fodor—for once—rightly pointed out, “insofar as the Representational Theory of Mind is the content of the computer metaphor, the computer metaphor predates the computer by about three hundred years” (Fodor, 1981, p. 140).

Fundamental difficulties with the Representational Theory of Mind were identified long before the new cognitivism. Two of the main problems continue to be remembered intermittently, only to be forgotten almost immediately. The first is what has come to be known as the symbol-grounding problem: How do mental representations ever come to connect meaningfully to the world (Harnad, 1990)? To put it bluntly, where do they come from? The second concerns the application of mental rules and representations: How can rules and representations be applied intelligently to the specifics of any particular situation (Polanyi, 1969)? In other words, how do we know when and how to apply any particular rule or representation? By appeal to yet more rules and representations? (For an excellent recent discussion of this second problem see Shaw, 2003.)

Since the rise of modern cognitivism there have been many important reactions and alternatives. Unfortunately, Anthony Chemero hardly attempts to situate his new book, Radical Embodied Cognitive Science, within this much wider context. There are no references, for example, to Jean Lave’s Cognition in Practice (1988), Lucy Suchman’s Plans and Situated Actions (1987), Susan Oyama’s Ontogeny as Information (2000), or to the extensive work within ethnomethodology and discursive psychology (e.g., Coulter, 1983; Edwards, 1997). There are only fleeting references to Ed Hutchins’ Cognition in the Wild (1995) and other landmark texts including those of Edward Reed, Chemero’s former colleague at Franklin and Marshall College. Given that Chemero does not situate his exact position in relation to some of the big issues on this wider anti-cognitivist agenda, it is difficult to tell exactly where he stands, not least with regard to

AUTHOR’S NOTE: I am grateful to Harry Heft and James Good for their comments.  
Please address correspondence to Alan Costall, Department of Psychology, University of Portsmouth, Portsmouth, PO1 2DY, UK; Email: alan.costall@port.ac.uk

345
situatedness, and, indeed, to other prominent approaches (e.g., sociological approaches) to embodiment.¹

Chemero’s aim, in this spirited book, is to promote a coalition of just two of the existing alternatives to cognitivism: James Gibson’s ecological psychology and dynamic systems theory. One of the glowing testimonials on the cover of the book even claims that Chemero “weds” these two frameworks, but, if so, the marriage was long overdue. The couple had already been living together in Connecticut (with Michael Turvey and Bob Shaw) for a considerable time. As long ago as 1981, Claire Michaels and Claudia Carello presented an engaging celebration of this partnership in their book *Direct Perception*.

**Getting Radical**

Chemero is rightly suspicious of the radical claims of some of the supposed alternatives to cognitivism. These “alternatives” often invoke embodiment or situatedness— or both—as a supplement to cognitivism rather than a true alternative. Some of these alternatives even treat these developments as providing the very “bootstrap” that cognitivism had long been waiting for! Chemero’s radical stance involves the complete rejection of “representationalism and computationalism” (p. 29).

I am at one with Chemero in his rejection of computationalism and, indeed, formalism (if that really is also his ultimate target). Psychologists fail to take note of the fundamental limits that the pioneers of computationalism themselves placed upon formalism. Turing is especially intriguing in this respect. After all, his work on diffusion demonstrated “how patterns in nature can emerge without any programmer at all” (Kelso, 1995, p. 4).

I also agree with Chemero that representationalist theorists have played fast and loose with the concept of representation. As Gibson’s teacher, Edwin Holt, put it, “if anybody has ever assented to the representative theory of knowledge it is only because he has not examined the concept of representation” (Holt, 1914, p. 142).

Unfortunately, Chemero does not situate representationalism historically. In fact, in his first chapter he makes a point of not “taking on” representationalism at all, arguing instead for pluralism. In a sense, he may be right. For representationalism in itself is not the problem. The real problem is the many different problems and mainly pseudo-problems it pretends to solve. In fact, there are other “mediationist” solutions to these problems (e.g., social constructionism) that probably would not count as “representationalist” in Chemero’s sense (see Costall, 2007).

Of course, rejecting representationalism does not make representation itself go away as a conspicuous issue within human psychology. People are involved in a whole host of representational practices based, for example, on symbol systems,

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¹ For discussions of the situated approach considered from a broadly ecological perspective, see the special issue of *Ecological Psychology* edited by Costall & Leudar (1996).
pictures, and, now, computer technologies, and these technologies, in turn, transform us psychologically. Indeed, a crucial motivation of Gibson’s own project was to get a proper grip on the problem of representation by identifying its conditions of possibility (see Reed, 1991).

**Affordances**

Chemero promotes and extends Gibson’s concept of affordances, an attempt to undermine the traditional dualism of the objective and subjective. On the face of it, Gibson’s definition seems relatively straightforward:

> An affordance is not what we call a “subjective” quality of a thing. But neither is it what we call an “objective” property of a thing if by that we mean that a physical object has no reference to any animal. An affordance cuts across the dichotomy of subjective–objective and helps us to understand its inadequacy. The affordances of the environment are facts of the environment, not appearances. But they are not, on the other hand, facts at the level of physics concerned only with matter and energy with animals left out. (Gibson, 1977, pp. 69-70; emphasis added)

In fact, Gibson’s own presentation of this concept was not only sketchy but self-contradictory, and so it is hardly surprising that it had already given rise to a good deal of critical discussion before Chemero himself entered the fray. In particular, Chemero emphasizes the relational status of affordances, but this issue had long been the subject of debate. What does seem to be new is Chemero’s definition of that relation in terms of features rather than properties. To explain the distinction, he gives the example of the “taller-than” relation between two people:

> The taller-than relation is not inherent in either of them, but depends on both of them for its existence. [Affords-Φ (environment, organism), where Φ is a behavior] is like taller-than in this respect: it is neither of the person, nor of the environment, but rather of their combination. Second, the affordance is not an extra thing in any of the usual senses of “thing.” (p. 142)

This move looks very promising to me, but I am puzzled that Chemero does not also apply this approach to Gibson’s concept of information. He seems to stay close to Gibson’s own definition, where, unlike affordances, information is not regarded as relational. It does not imply an agent to be informed:

> Ontologically speaking, information is a relation between energy in the environment (light, vibrations, etc.) and the substances and surfaces in the environment. (p. 109)

Although Chemero does not make the connection, his “de-thinging” of affordances seems to chime in well with John Shotter’s emphasis upon the open-ended, fluid nature of affordances:
. . .the beings in Gibson’s world are depicted merely as observers, not as actors, i.e. not as beings able to provide for themselves, by their own actions, conditions appropriate to support their action’s continuation. They may move about, but they do not act; thus rather than “makers”, they are presented merely as “finders” of what already exists. Such a view, I would argue, fails to recognize the peculiar form-producing character of activity in a biological and social world; it fails to assign a proper role to time and to processes of growth and development. (Shotter, 1983, p. 20)

According to this dynamic, historical view, “an affordance is only completely specified as the affordance it is when the activity it affords is complete” (Shotter, 1983, p. 27; emphasis added). On the one hand, therefore, this is a crucial corrective to what Bill Noble has called Gibson’s “fallacy of objectification,” the treatment of affordances as though they are lodged within things (Noble, 1981, 1991). On the other hand, it is also a seriously misleading over-statement. What I miss in Chemero’s account (along with those of Shotter, Noble, and Gibson) is a recognition of the specific circumstances within which the meanings of things do indeed become objectified as “canonical affordances” and embody intentions (Costall, 1995; Costall & Richards, in press). A chair is for sitting on, whether or not anyone happens to be sitting on it, standing on it to change a light bulb, or brandishing it to ward off an intruder. Canonical affordances imply not simply “the person” in the singular, but people in relation to one another entering into normative practices.

Chemero’s strategy of radicalism is a valuable counter to the intellectualism of cognitivist theorizing, with its perpetual appeal to “mental gymnastics,” to use his own nice term (p. 109). But, in his hands, the strategy can also end up as a rather blunt tool, obscuring rather than revealing special cases, not least those of canonical affordances and representation.

**GATTS Revisited**

I want to return to Chemero’s celebration of the coalition of Gibson’s ecological psychology and dynamic systems theory (GATTS: Gibson-according-to-Turvey-and-Shaw). Back in the 1980s it never occurred to many of the rest of us who have long been interested in James Gibson’s work (i.e., the Non-Connecticut Non-School of Ecological Psychology) that this was the obvious direction for ecological psychology to take. After all, there is little hint in Gibson’s writings of any commitment to dynamic systems theory (as distinct from systems theory).

In terms of the amount of quality research output, GATTS has certainly provided the basis for a most highly productive development within ecological psychology—but there are limits and there have been costs. The first is that the research conducted within this context has not only remained bound almost exclusively to the laboratory, it has also focused upon minimal, non-ecological tasks such as finger wagging and leg swinging. The further big drawback is that the attractive formalism of dynamic systems theory does not travel beyond such
limited domains. Attempts to extend the formal terminology beyond those domains can end up as diverting, yet superficial, exercises in hand waving.

Chemero himself eventually concedes that there are limits, though of a different kind:

> It is quite easy to draw a lot of intelligent behavior under the coordination umbrella. *Now for the obvious*: there is a lot of cognition that will be getting wet. (Because it’s not under the metaphorical coordination umbrella.) Not everything that cognitive science wants to explain is coordinated activity. (p. 97; emphasis added)

Furthermore, according to Chemero, “the dynamical stance must be instrumentalist to avoid representationalism, and *qua* instrumentalist it cannot tell us what cognition is” (p. 97). It falls to ecological psychology, rather than the dynamic stance, to provide us with “a *theory* of cognition” (p. 97; more on this later).

As I have said, the dynamic systems approach, despite its emphasis upon embodiment and embeddedness, does seem to have had a curiously Ebbinghausian attraction towards artificial and simplified situations. The hope, of course, is that we will thereby identify *fundamental* laws that will then generalize to life, the universe, and everything. Now this is an old scientific trick and it has, of course, sometimes worked splendidly. Yet, as proponents of “situated action” (along with Gibson and Bartlett before them) have insisted, complexity is not just an inconvenient complication for tidy-minded researchers, but also a *resource* for the very people they are studying.

In my view, the biggest cost of the coupling of ecological psychology and dynamic systems theory has been the self-enclosure of ecological psychology not only within psychology, but also, almost exclusively, within the psychological laboratory. This self-enclosure stands in stark contrast to the early meetings of The International Society for Ecological Psychology (ISEP), which were inspiring interdisciplinary, attracting misfits from many different subject areas, each reacting against the representationalism and formalism rife within his or her own fields of research.

**The Place of the Social–Cultural**

Many of the alternatives to cognitivism, such as situated action, ethnomethodology, neo-Vygotskian approaches, and sociological approaches to embodiment, place a central emphasis upon the socio-cultural. So where does Chemero’s wedding of ecological psychology and dynamical systems theory stand on this point?

There is a widespread view that Gibson’s ecological psychology has very little to do with the socio-cultural. For example, George Lakoff complained that “the Gibsonian environment is monolithic and self-consistent and the same for all people” (Lakoff, 1987, p. 216). On this view, the Gibsonian project is, therefore, about sorting out the fundamental principles—*before* “taking on” the social. I
remember Michael Turvey making this point forcefully at one of the early conferences of the ISEP and even claiming that this fundamental work might well take much longer than our own lifetimes.

But is the social really a “terminal addition,” i.e., something tagged on at the end of a developmental process and having no real impact upon the supposed preceding “stages” of development?²

James Gibson, himself, had a long-term interest in social psychology, and, as Eleanor Gibson once told me, this was the one course that he taught throughout his career. Gibson did not regard the social as a mere “after-thought”:

Social learning is inevitably moral, in an elementary sense of the term, and it is probably a mistake first to construct a behavior theory without reference to social interaction, and then to attach it only at the end. (Gibson, 1950, pp. 154-155; emphasis added).

When I first read Chemero’s book I thought he had found no place for the socio-cultural in his approach. On second reading, however, I came across his approving reference to “a great profusion of work on social coupling” (p. 96). The research to which he refers is certainly interesting, but it also reflects the way that methodology can itself powerfully constrain a domain of inquiry. The “profusion of work” to which Chemero refers is largely restricted to studies of people jointly wiggling their fingers or swinging their legs, in or out of phase.

**Psychology and Philosophy**

The role of philosophers, especially at Oxford and Cambridge, in relation to psychology was once very clear: to expose this hope of a science as hopelessly misguided. By the 1970s, however, several very clever people, most notably Fodor and Dennett, achieved fame as camp followers of the new cognitivism. Given that they were indeed clever, I could never believe they were really serious. Dennett had even studied with Gilbert Ryle!³

In *Radically Embodied Cognitive Science* Chemero makes a point of identifying himself as a philosopher:

Philosophers of science should be more appreciative when practicing experimentalists take time to produce theoretical work as careful and detailed as that done by Turvey, Shaw, Mace, and, more recently, Warren. *It makes our jobs much easier.* (p. 216)

But what is supposed to transform being “careful and detailed” about theory into philosophy rather than science? I do not understand on what basis Chemero, other than his academic background, believes his contribution (including even his

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² The term “terminal addition” comes from nineteenth-century recapitulation theory.
³ For Ryle’s forthright opinion of Fodor and his more muted reaction to Dennett’s incipient defection, see: http://ejap.louisiana.edu/EJAP/2002/RyleLett.html
discussion of “metaphysics,” the topic of his final chapter) counts as philosophy as opposed to an intrinsic, if neglected, part of the psychologist’s own business.

Cognition

As we have seen, Chemero looks to ecological psychology to provide us with “a theory of cognition” (p. 97). The term “cognition” has had a long-established and reasonably definite meaning within the tradition of intellectualism:

The prevailing doctrine. . . holds (1) that Intelligence is a special faculty, the exercises of which are those specific internal acts which are called acts of thinking, namely, the operations of considering propositions; (2) that practical activities merit their titles ‘intelligent’, ‘clever’, and the rest only because they are accompanied by some such internal acts of considering propositions (and particularly ‘regulative’ propositions). That is to say, doing things is never itself an exercise of intelligence, but is, at best, a process introduced and somehow steered by some ulterior act of theorising. (Ryle, 1946/1971, p. 212)

Chemero’s book presents a sustained challenge to the intellectualist notion of “cognition” that still prevails within mainstream psychology. However, as far as I can see, Chemero treats “cognition” itself as a self-evident and unitary topic that can simply lend itself to radically different theoretical formulations. Given not only its extremely long history within Western philosophy but also its relatively brief career within psychology, it might be time for psychologists to find some alternative terms (please note the plural) to replace “cognition.” New theories of cognition can sound as unpromising as new theories of phlogiston. Ryle made precisely this point in his letter to Dennett.

Locked in the Laboratory

Unfortunately, although revision of terminology (e.g., dropping the term “cognition”) would help, it will not be enough. For example, fundamental assumptions of neo-behaviorism are themselves embodied in the experimental paradigms of modern cognitivism, not least the stimulus–response formula and the “bodily” passivity of the experimental subject, whose task is to “respond” to the imposed “conditions” rather than transform or escape them.

As the historian of psychology, Kurt Danziger, has been brilliantly demonstrating, we need to look beyond the language of psychological theory. The very “objects” of psychological discourse are themselves shaped by our current research practices, and these usually serve “to fit the Procrustean bed of a very limited range of allowable procedures” (Danziger, 1996, p. 17). Chemero’s book does not situate the language of psychological theory within the practice of actual research. Yet, this move would not only be consistent with the wider “discursive turn” within postmodernism, but also with Wittgenstein’s emphasis on “forms of life.” More to the point, it would be in line, of course, with the emphasis upon “the primacy of praxis” within several important alternatives to cognitivism.
Chemero’s hopes for a radical embodied cognitive science are admirable, but throughout his book I could not help but notice a radical mismatch between his ambitions and the limited kinds of research he relies upon to make his case.

References


