

Radical Embodied Cognitive Science

Anthony Chemero

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Preface: In Praise of Dr. Fodor

Jerry Fodor is my favorite philosopher.

I think that Jerry Fodor is wrong about nearly everything.

Knowing these two facts about me should be helpful for those who wish to understand what this book is all about. My goal is that this book is for non-representational, embodied, ecological psychology what Fodor's *The Language of Thought* (1975) was for rationalist, computational psychology. *The Language of Thought* was a true landmark in (the philosophy of) cognitive science. It set out in great detail just what it is to do computational psychology, what some of the benefits to doing computational psychology are, what some of the results of computational psychology (c. 1975) were, and what the philosophical consequences of the computational approach are. It is admirably clear and rigorous, and also very funny. (See, for example, his discussion of the dispositional properties of Wheaties.) I would argue that *The Language of Thought* is the very best work ever done in the philosophy of cognitive science.

I warn you in advance that this book is not *that* good. I can live with that. I have set lofty goals for this book, fully aware that I would not reach them. My assumption was that if you aim for the stars, you might end up in low orbit, or in a deluxe apartment in the sky. So, although not as good, this book really is intended as a counterpart to Fodor's. Like his book, I describe a way one might pursue the scientific study of cognition and lay out the philosophical consequences of studying cognition this way. Like Fodor's book, the purpose is primarily to say what this way of doing cognitive science is, warts and all. Only secondarily do I try to convince you that what I describe is the *right* way to pursue the science of the mind. I am more than happy to accept the following reaction: "If that is what non-representational, embodied, ecological cognitive science is all about, I'll stick with computationalism. Maybe I'll become a pastry chef instead." In

other words, it could be that this book is one big *modus tollens*. If so, so be it. As Fodor himself puts it, “Hate me, hate my dog” (1990, xii).

The main way that this book is different from Fodor’s, apart from the already-apologized-for difference in quality, is the nature of the cognitive science it describes. I suspect that the approach described and defended here, which I call *radical embodied cognitive science*,¹ would make Fodor gag.² In defending radical embodied cognitive science, I embrace many of the things that Fodor has railed against (direct perception, American naturalism, connectionist networks, teleological theories of content) and reject many of the things near and dear to him (especially mental representation). Another way that this book differs from Fodor’s is in its attitude toward competing approaches in cognitive science. The hilarious and biting first section of *The Language of Thought* is devoted to dismantling behaviorist approaches to cognition in order to make space for his positive story. There is no such section in this book. Indeed, I think that this felt need to “make space” for a new scientific approach by showing that all other approaches are faulty or doomed to fail is a peculiar philosophical malady, and one that desperately needs curing.³ This is the point of chapter 1. In it, I argue that primarily conceptual arguments against scientific approaches should be taken with a grain of salt, and never as dispositive. This is true in the case of arguments against the computational approach (Dreyfus 1964, 1972; Searle 1980) as well as arguments against radical embodied cognitive science (Clark 1997, 2008; Markman and Dietrich 2000a,b). Having argued against space-making arguments, I do not argue against other theories to argue for radical embodied cognitive science.

In chapter 2, I describe radical embodied cognitive science very broadly, comparing it to plain old embodied cognitive science, and outline a few historical antecedents and factors that make it attractive. Radical embodied cognitive science, very roughly, is the thesis that cognition is to be described in terms of agent-environment dynamics, and not in terms of computation and representation. The point of these chapters is to show that radical embodied cognitive science deserves a place at the cognitive science table, alongside more traditional computational approaches.

In the second part of the book, I explain just what it takes to embrace radical embodied cognitive science. One of the things I try to make clear is that it is actually very difficult to reject internal representations, and that radical embodied cognitive science must be more radical than most of its proponents realize. Representationalists can, and do, claim that things in agents are representations, even when they have few or none of the trappings of classical representations and even when calling them such plays

no explanatory role. (See, e.g., Markman and Dietrich 2000a,b; Wheeler 2005.) The way to avoid this problem is to argue for a particular explanatory stance toward cognitive systems and models of them. To defend radical embodied cognitive science, one must take up what I call the *dynamical stance*, a methodological commitment to explaining perception, action, and cognition dynamically and without referring to representations. A strategy similar to this has been employed with considerable success throughout the cognitive sciences, including in studies of perception, motor control, speech, and development. Despite this success, and the promise of considerable future success, there is a particular problem for dynamical cognitive science that is not faced by computational and representational explanation: the problem of discovery.

The problem of discovery is not a new problem: it first sprang up in a debate between Mach and Boltzmann at the beginning of the twentieth century and has been discussed extensively in the philosophy of science. (See, e.g., Hanson 1958.) The problem boils down to the way in which new hypotheses are generated for testing. Since dynamical cognitive science is a commitment to methodology, it is instrumentalist. That is, it has no necessary connection to any particular posit about what its subject matter is like. The dynamical stance, like Dennett's stances on which it is based, is blissfully metaphysics-free. Computationalism and representationalism, though, are not: they are tied to the posit that the mind (or brain) is a computer and full of representations being acted upon by algorithms. This background assumption has been extraordinarily productive in the generation of new hypotheses for testing (as well as productive for papers published and new journals). Without such a set of background assumptions, it might seem that the dynamical stance is without a guide to discovery, without a method of systematically generating new hypotheses. This is, I take it, a serious disadvantage, one that might lead some sympathizers back to representationalism and computationalism. (I am psychoanalyzing Andy Clark here.) In short, radical embodied cognitive science has a methodology in dynamical modeling; it also needs a background theory, a theory of what its objects of study are.

In the third part of the book, I propose that Gibsonian ecological psychology is just the right theory. Gibson's assumptions—that perception is direct, constitutively linked to action, of affordances—are fully compatible with radical embodied cognitive science and with dynamical methodology. Indeed, for the sociologically inclined, much of the dynamical research in the cognitive sciences today can be traced back to two related, Gibson-sympathizing institutions in Connecticut: Haskins Labs in New Haven

and the Center for the Ecological Study of Perception and Action at the University of Connecticut. The main problem with the ecological approach as the background theory for radical embodied cognitive science and dynamical modeling is that the key concepts of Gibson's approach are, to be frank, obscure. There is much disagreement, even among ecologically oriented psychologists, over just what affordances are supposed to be, and how they relate to animals, information, and events. The bulk of the third part of the book, therefore, is devoted to setting out a relatively faithful, conceptually sound theory of the main concepts of Gibsonian ecological psychology. Think of this as Gibson for philosophers, or for psychologists who (understandably) are a bit confused about what Gibson was on about.

The first three parts outline a theoretical orientation to cognition (radical embodied cognitive science), a methodology (the dynamical stance), and a background metaphysics and epistemology (shored-up ecological psychology). The union of these three things is what I'm recommending as the way to do cognitive science. Since (as noted above) I don't argue against other views on what is the right way to do cognitive science, you might wonder why you should accept my advice. After all, you'll have to relearn calculus after all that effort learning computability theory. The true test of an approach in any science is how well it answers the questions we want answered with empirical results. Though some empirical results and promises for more are outlined at various places throughout the book, these results are mostly not in. (Though I think that the computational approach has appallingly little to show considering the time and money that have been devoted to applying it.) How, then, to make the proposed reorientation appealing? In the last section of the book I look at a few traditional philosophical problems through the lens of radical embodied cognitive science. The comparative ease with which these problems (reductionism, epistemological skepticism, metaphysical realism, consciousness) are solved or dissolved constitutes, along with the sketched and promised empirical results, a fairly strong recommendation. My hope is that they will convince impressionable, young cognitive scientists that radical embodied cognitive science is worth some of their time and effort.