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# Anthropology's Disenchantment With the Cognitive Revolution<sup>1</sup>

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## Abstract

Beller, Bender, and Medin should be congratulated for their generous attempt at expressive academic therapy for troubled interdisciplinary relationships. In this essay, I suggest that a negative answer to the central question (“Should anthropology be part of cognitive science?”) is not necessarily distressing, that in retrospect the breakup seems fairly predictable, and that disenchantment with the cognitive revolution is nothing new.

*Keywords:* Cognitive revolution; Behaviorism; Anthropology; Jerome Bruner; Roy D’Andrade; Clifford Geertz; Roger Shepard

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Some of the leaders of the cognitive revolution of the late 1950s and 1960s began parting from the cause almost as soon as it triumphed. Jerome Bruner, for example, who always enjoyed writing essays for both the left hand and the right hand, turned to hermeneutics, the study of law, and the interpretation of narratives (see Bruner, 1979, 1990). Even in the early days of the rebellion Bruner was attentive to language pragmatics, which may be one reason he named his 1960s big tent interdisciplinary center at Harvard University the “Center for Cognitive Studies” and not the “Center for Cognitive Science.” Bruner had just as much interest in the humanistic writings of E. H. Gombrich, Nelson Goodman, and Clifford Geertz as in the latest claims about basic/fundamental/universal cognitive processes coming out of experimental labs situated in Cambridge, London, or Geneva.

For other defectors, it took some time to realize that the ideals they harbored for their intellectual movement were not necessarily the common currency of the uprising. Sieghard Beller, Andrea Bender, and Doug Medin (BBM) appear to be experiencing that realization now. By their lights, cognitive researchers ought to be able to understand and explain the behavior of intelligent organisms by reference to the actual content of their goals, values, and pictures of the world. They wonder why contemporary cognitive

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science has “embarrassingly little” to say about such issues as inter-group conflict and environmental decision making. BBM pose the normative question: Should anthropology be part of cognitive science? Their answer, floated tentatively, with a sigh and a sense of regret, is “no.” I don’t necessarily disagree with them, although I certainly would have answered “of course” or with an enthusiastic “yes” (as I am confident they would) had the question been posed this way: “Should anthropologists be part of any interdisciplinary research enterprise rigorously and critically investigating similarities and differences in human mentalities across cultural groups?” BBM clearly have a scholarly interest in the real problems facing real people in the real world, in cultural and language diversity, and in the variety of (and potential conflicts between) different time- and place-dependent human mentalities. Their essay thus makes me wonder whether they themselves are getting ready to defect, given the contemporary institutional form of the cognitive revolution and its reincarnation as cognitive science.

### 1. Disenchantment with the cognitive revolution is nothing new

Disenchantment with the drift of the cognitive revolution is nothing new. Many cultural anthropologists took part in the movement in the late 1950s and throughout the 1960s. Early on, two of them, A. Kimball Romney and Roy G. D’Andrade, organized an influential interdisciplinary conference of psychologists, linguists, and anthropologists, which was published as a special issue of the *American Anthropologist* under the title “Transcultural Studies in Cognition” (Romney & D’Andrade, 1964).

Now fast-forward a couple of decades. By 1980 Roy D’Andrade was a major voice (arguably *the* major voice) for cognitive studies within anthropology (see D’Andrade, 1995). Yet on March 14, 1980 during a planning meeting for an interdisciplinary conference on the “symbols and meanings” approach to the study of culture I observed him reflect on the cognitive revolution this way:

When I was a graduate student, one imagined people *in* a culture; 10 years later culture was all in their heads. The thing went from something out there and very large to something that got placed inside. Culture became a branch of cognitive psychology. We went from “Let’s try to look at behavior and describe it” to “let’s try to look at ideas.” Now, how you were to look at ideas was a bit of a problem — and some people said, “Well, look at language.” That notion, that you look at idea systems, was extremely general in the social sciences. On, I think, the same afternoon in 1957 you have papers by Chomsky and Miller and in anthropology, Ward Goodenough. All signal an end to the era of “Let’s look at people’s behavior and see what they do.” Before 1957 the definition of culture was primarily a behavioral one — culture was patterns of behavior, actions, and customs. The same behavioral emphasis was there in linguistics and psychology. The idea that cognition is where its at struck all three fields at the same time — was a slightly different trajectory in each discipline — whether you do experiments or whether you look for intuitions or whether you talk to informants. I think it was a nice replacement. But the

thing is now breaking — that force set in motion in the late fifties. And I feel it is breaking in psychology, it's breaking in linguistics, and it's breaking in anthropology and we each have different ideas about how it's breaking up. (quoted in Shweder, 1984, p. 7)

Ironically, Roy D'Andrade's observation in 1980 that the cognitive revolution was breaking up was offered the very year that the Alfred P. Sloan Foundation kept the revolution going by providing substantial funding for training programs and research centers. I participated in a joint University of Chicago–University of Michigan training program that was a hotbed for colloquies on diverse topics bringing together linguists, psychologists, and (some) anthropologists. Nevertheless, the actual character of the movement as an intellectual undertaking remained nominal: With foundation support the cognitive revolution became institutionalized and honored itself with the title “Cognitive Science,” a society and journal got formed, and occasional united-we-stand utopian programmatic statements were issued calling for team research and the integration of knowledge across disciplines and levels of analysis.

## **2. Fundamentalism in cognitive psychology**

BBM have smart and insightful things to say about some of this, and I am not sure I have much to add to their brilliant and comprehensive account of the reasons for the subsequent takeover of the Cognitive Science society *per se* (and its journal) by ecumenists and fundamental process oriented experimental psychologists. Except to say that I am not entirely surprised that, given all the defections from the movement over the past 55 years, the surviving legal heirs to the cognitive revolution — those who today might describe themselves and be recognized by others as card-carrying cognitive scientists — are mainly experimental psychologists (plus perhaps some brain scientists, artificial intelligence researchers, and a few mathematicians)

Those psychological fundamentalists, unlike most anthropologists, place their highest priority on the search for highly general (and thus often quite abstract or even mathematical) laws of mental functioning; and consequently, unlike most anthropologists, are not especially interested in the study of cultural and linguistic diversity; or in the way the content of thought (what you think about) can be decisive for how you think; or in the parochial aspects of human mentalities in particular social groups.

When it comes to the study of mind, most (although certainly not all) cultural anthropologists have a somewhat different intellectual aim. In general, it is not their goal to become a branch of cognitive psychology; and given the fate of the discipline of cognitive psychology over the past 20 years it is probably not their goal to become an extension of the neurosciences either. The anthropologist Clifford Geertz, who was arguably the most famous American cultural anthropologist since the onset of the cognitive revolution in the late 1950s, once remarked: “I have never been able to understand why such comments as ‘your conclusions, such as they are, only cover 2 million people [Bali], or 15 million [Morocco], or 65 million [Java], and only over some years or centuries’ are supposed to be criticism.” Indeed, during the 1960s and 1970s, as the cognitive revolution was becoming increasingly

unsure about the unity of its aims, Geertz famously publicized the “thick description” of “local knowledge” as a royal road to understanding. By “thick description” he meant the interpretation of real-world behavior (including communicative behavior) by reference to socially transmitted, time- and place-dependent beliefs and desires made manifest in the ordinary or taken-for-granted actions of members of a social group (Geertz, 1973) — the Balinese cockfight, for example. And it is worth noting that right from the start, Geertz made a substantial contribution to our understanding of such things as nationalism, cultural collisions, and inter-group conflict and the real challenges faced by real people in places like Java, Bali, and Morocco (see, e.g., Geertz, 1963, 1986, 1998; also Shweder, 2010, where I discuss what I called “Geertz’s Challenge,” as well as the implications of his version of robust cultural pluralism for the political shape of the “New World Order”).

Nevertheless, whether the descriptions one seeks for why people say the things they say and do the things they do are thick or thin, for the ecumenists and fundamentalists who are the heirs of the cognitive revolution merely pointing out the time- and place-bound character of that type of anthropological study of mind *is* criticism of a sort. This is not because contemporary cognitive scientists have no personal curiosity about the distinctive aspects of Balinese versus Moroccan versus Javanese thought; or because as a breed they are so unworldly as to think that human minds are in all respects the same wherever you go; or because they are so dogmatic as to deny the existence of boundary conditions on the activation of (what they view as) fundamental cognitive processes; or because they are so narrow-minded as to overlook the reality of situated effects on the products of thought (related to the context, purpose, and content of any cognitive act).

The main reason it is criticism of a sort when a cognitive scientist says, “That’s mere content” or retorts, “Your findings ‘such as they are’ are geographically limited in scope and are culture-bound” is that the search for those aspects of the human mind that are invariant (fundamental, basic, deep, universal) is what defines high seriousness of purpose for most (although certainly not all) cognitive scientists. As BBM duly note (and in their essay they take note of just about everything), it is the discovery of such universal laws of thought that has become the measure of prestige in the cognitive sciences as an academic guild. Still, given that aim of the trade, it is not hard to understand why cognitive scientists don’t typically embrace travel to the Atlas Mountains of Morocco or a paddy field in Bali or a rain forest in Brazil as a royal road for getting at what they view as the basic or fundamental cognitive structures or processes enabling human beings to have a mental life at all.

When it comes to the study of the human mind, different aims lead to different judgments of research value (and academic prestige). This leads me to suggest that if the aims of two disciplines diverge (and if what seems “deep” or “fundamental” to one discipline seems “thin” or empty to the other; and, conversely, if what seems “thick” and reality binding to the other discipline seems fleeting or superficial to the first), so be it! Why should they be united in a single movement? And it also leads me to confess I am not particularly distressed by a negative answer to the question “Should anthropology be part of cognitive science?” I hope at the end of the day BBM are not too distressed either.

In any case, “divided we stand” is not necessarily a bad principle; and in this instance, the divergence of aims may be just as real as BBM suggest. Long ago, experimental

psychologists developed a set of research strategies to aid in their search for highly general laws of mental functioning that transcend time and place, which of necessity directs them to control for or withdraw their attention from all the things that are variable in the mental life of human beings—beliefs, values, content, context, culture, language, etc. BBM say as much (and much more) about why anthropologists are rarely seen at Cognitive Science Society meetings and also about why experimental cognitive psychologists are so prevalent.

### 3. The contrast between Clifford Geertz and Roger Shepard: Relax and enjoy it!

Contrast, for example, the ambition of Clifford Geertz (to observe, document, and render intelligible a local way of life) with the ambition of the cognitive science eminence Roger Shepard, who sought to discover a universal law of generalization inherent in all categorization behavior, regardless of species or stimulus domain. I point to Shepard's quest as an illustration of a high prestige research agenda among today's heirs to the cognitive revolution (Shepard, 1987; see Shweder, 1991 for a fuller discussion).

Shepard's universal law (which he proudly likens to Newton's law of gravitational attraction) is an abstract representation of an exponential decay function for stimulus generalization likelihoods for pairs of stimuli, showing the probability that a response learned to any one stimulus within some given domain (indeed, any domain—consonant phonemes, color chips, triangles of different sizes and shapes, presumably social categories as well) will generalize to any other stimulus within that domain. His aim is to discover something fundamental, basic, and deep about thought processes. Notably, Shepard (1987) acknowledges that from a strictly empirical point of view, his proposed fundamental and universal law is truly descriptive of stimulus generalization behavior *only* when "generalization is tested immediately after a single learning trial with a novel stimulus" (p. 1322). To my critical and interpretive anthropological eye that hardly seems like a minor disclaimer, but it does help me make the relevant point about divergence in scholarly aims.

The relevant point is that in order to get at what he genuinely aims to discover—a basic psychological process inherent in human categorization behavior—Shepard deliberately (and with his eyes wide open) chooses to limit his investigation of the effects of stimuli on similarity and difference judgments by focusing only on the reactions of subjects to unfamiliar stimuli encountered in one-trial learning environments. In other words, on principled grounds he turns his attention away from several levels of reality that he himself knows play a major part in normal human classificatory behavior. He withdraws his attention from those levels of reality because he knows they will produce variable or diverse (he calls them "noisy") results that are not universal across species or stimulus domains.

Thus, for example, he seeks to move his investigation beyond any observations or analyses of the objectively describable similarities and differences in the stimulus materials being studied. For as he notes, it has been shown—he views the relevant findings as "troublesome" and "discouraging"—that there exists no universal mathematical function for predicting the probability of a generalization response from the measurable physical characteristics of stimuli; those mathematical functions seem to vary by stimulus domain (Shepard,

1987, p. 1317). He is aware that the mathematic function for stimulus generalization for the color domain may differ from the function for tonal scales; and that each of these may differ by individual or by species; and that within a particular stimulus domain (the color domain, for example) a response to a color chip may generalize to a distant hue at the opposite end of the spectrum (for example, red and green might be associated together as “Christmas colors”). Given that his cognitive science is going on a quest to discover a universal law of generalization underlying all categorization behavior, he has good reason to suspect that there can be no universal law of the stimulus environment and that any truly fundamental and universal process must be a purely psychological function and not a psycho-physical function (Shepard, 1987, p. 1318).

Then he seeks to move his investigation beyond the observation and analysis of the influence of any and all possible learning processes. This is because Shepard understands very well that his proposed universal law is unlikely to describe generalization behavior under multiple learning trials because differential reinforcement could shape the generalization function and contours around a particular stimulus into a wide variety of forms (Shepard, 1987, p. 1322).

Finally, he seeks to move his investigation beyond the observation and analysis of any process involving long-term memory and its capacity to mentally re-cognize or imaginatively reshape the prior experience of a stimulus event. He takes this step because he is fully aware that the proposed universal law is not descriptive of generalization behavior when learning trials are delayed. He interprets that type of failure of validation of the universal law as “‘noise’ due to the internal representation of the stimuli” (Shepard, 1987, p. 1322).

It is crucial to notice that Roger Shepard is not in the business of denying the existence of variability in human classificatory behavior or in discouraging others from studying time-bound or place-bound or stimulus-bound mental processes or events. He is just doing his own business. His primary aim—the thing he cares about most as a cognitive scientist—is to move his research beyond all the “noisy” diversity (the shadows in the cave) in a search for pure psychological forms and invariant laws of thought. I do not find this particularly distressing; quite the contrary, it seems like an appealing (and potentially productive) application of the principle “live and let live.” As BBM suggest toward the end of their essay, this is one way to relax and enjoy their tentative negative answer to the question “Should anthropology be part of cognitive science?”

The relaxed recognition of cross-purposes can be quite revealing, too. I suspect the cultural psychologists Michael Cole and John Gay (1972, p. 1066) will never forget one particular critical comment they received (in this case from a cultural anthropologist) when they first made some claims about cross-cultural differences in thought processes based on results from their cognitive experimental research in West Africa (among the Kpelle people of Liberia). In paraphrase and with a bit of elaboration (and poetic license), the critical comment went roughly as follows: “Thank you very much for your fascinating presentation, but the thought processes of the Kpelle do not differ from our own; only their beliefs, values, and classifications differ; which is why they perform so differently on psychological tests.” For most anthropologists the study of cognition is primarily about those beliefs, values, and classifications; and in general the discipline of anthropology does not place any special

value on getting beyond those beliefs, values, and classifications or transcending their particularities.

#### **4. The breakup was predictable: Liberation from behaviorism is not enough to sustain an academic marriage**

In sum, if I have anything at all to add to BBM's generous and loving attempt at expressive academic therapy for troubled interdisciplinary relationships, it is to suggest that disenchantment with the cognitive revolution is not all that new, and to reinforce the notion that division is not necessarily a bad thing.

In retrospect, the breakup seems pretty predictable. The cognitive revolution began as a general interdisciplinary reaction against something: namely, behaviorism. A major premise of behaviorism was the view that anything that could not in principle be observed (such as human mental states and processes) was unreal or, if one was willing (most behaviorists were not) to be a dualist with regard to the mind-body problem and actually grant the reality status of unphysical things (such as beliefs and desires and all other "intentional states"), then at the very least the study of such things was methodologically beyond the scope of empirical/objective science and should be left to the humanists and to folk psychology.

The revolution turned out to be largely a declaration of negative liberty, freeing a group of strange bedfellows from such strictures. But as soon as a positive program had to be spelled out for the study of human mental states and processes the revolution fractured into diverse sub-disciples or schools of thought along familiar philosophical fault lines. Unavoidable ontological and epistemological questions got raised about the difference between an objective versus subjective point of view and about the nature and true reality of unobservable mental things. Diverse schools of thought arose defined by their answers to questions about the true causal efficacy (vs. epiphenomenal status) of mental states, about the meaning of cognitive or representational mental functioning per se — knowing something or seeing something—and its relationship to non-cognitive or non-representational mental functioning—wanting something or feeling something. Some cognitive researchers turned out to be closet behaviorists who wanted to know: If one rejected the strictures of behaviorism, what did one put in their place? Deep and difficult issues got raised about how one should go about studying the unobservable mental life of intelligent agents. In what sense, if any, are intelligent agents merely objects for study whose behavior is fully determined by potentially observable physical particles in fields of force (neuronal firings, electrical activity in the brain)? Is the very idea of mental causation an illusion of folk psychology? If by definition the aim of any empirical science is to provide us with knowledge based exclusively on observation via some reliable sensing device together with whatever purely logical deductions follow from those observations, can empirical science provide us with a complete account of human mental life, including (for example) the study of meaning? If there are limits to what can be studied from an objective point of view, precisely where does empirical science end and interpretive studies begin? Can there even be a science of meaning?

When it comes to the study of the human mind, such questions are deep and fundamental; yet (both before and after 1957) the answers given to those basic questions have never been universal. Perhaps, in part, that is why Shepard is Shepard and Geertz is Geertz; why cognitive science is not cognitive studies; and why anthropologists don't show up at cognitive science society meetings. I am confident, however, that even if our authors decide to defect from the cognitive science movement they won't lack for anthropological company in their interdisciplinary explorations of cultural cognition.

## Note

1. Cultural anthropology, of course, is a vast and not necessarily unified profession. When I speak of anthropology or cultural anthropologists in this essay, I have in mind those in the discipline who have had a professional interest in the study of mental things (what people know, think, feel, want, and value as good or bad) as one way of understanding what people and peoples habitually or customarily do within and across cultural groups. For a discussion of some of the camps within the discipline of cultural anthropology, including the skeptical postmodern and identity politics movements, see the Introduction in Shweder (2003).

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