Cultural Models, Consensus Analysis, and the Social Organization of Knowledge

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Abstract

The introductory essay to this collection correctly observes that there are many ''challenges for rapprochement'' between anthropology and (the rest of) cognitive science. Still, the possibilities of fruitful interchanges provide some hope for the parties getting back together, at least on an intermittent basis. This response offers some views concerning the ''incompatibility'' of psychology and anthropology, reviews why cognitive anthropology drifted away from cognitive science, and notes two areas of contemporary interest within cognitive anthropology that may lead to a re-engagement.

Keywords: Anthropology; Cognitive science; Cultural models; Cultural consensus analysis; Socially distributed knowledge

This response to the Beller, Bender, and Medin's introductory essay begins with their Challenge 3: the notion that cognitive science (principally, psychology) and anthropology are incompatible, both in terms of perspectives and methods. Second, I note why cognitive anthropology was compatible with cognitive science's agenda in the first place and how internal-to-anthropology shifts of interest since the early years of cognitive anthropology have led to diminishing engagement. This historical review is relevant to Challenge 1 (why many anthropologists think cognitive science is not on the right track). Finally, I note two areas of interest among contemporary cognitive anthropologists that may hold the key to any future rapprochement with cognitive science, two research areas where re-engagement might be mutually beneficial.

1. Perspectives and methods: Vive la différence!

Research projects in the social and behavioral sciences typically have a number of specific purposes and goals; however, they can be accommodated in three general categories:

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exploration, description, or explanation (Babbie, 2010, pp. 92–94). And different research methods are appropriate for these different purposes. Ethnographic methods are most suitable for exploratory work, surveys using probability sampling are *de rigueur* when the goal is to describe variations in a population, and experiments are the best way to test causal hypotheses.

Most anthropological research is exploratory in Babbie's sense, and only rarely descriptive or explanatory. Ethnographic fieldwork involves a wide variety of data collections — mapping, photographing, counting, observing, chatting, open-ended interviewing, group discussions, systematic questioning, etc. — with the overall goal being to identify patterns in the natives' behaviors and belief systems, to see interconnections (Boster, 2011). But, because anthropologists very rarely select their informants, households, or villages using probability sampling, their ethnographic accounts typically consist of qualitative generalizations from small samples to an unspecifiable population (*the* Bongo-Bongo think-believe-do X). By ignoring sampling issues, anthropologists seldom have sufficient data to estimate population parameters the way a well-designed descriptive study can. Likewise, unless specific hypotheses were formulated in advance, anthropologists seldom have the time or inclination to design and conduct experiments on the fly while in the field. The closest they might come would be systematic comparisons of naturally occurring groups, for example, elicit standardized data from men versus women, young versus old, neighboring ethnic groups, and so on. But quasi-experimental studies of this sort seldom use probability sampling to recruit participants. Indeed, the strong suit of exploratory, inductive, ethnographic research lies not so much in definitively answering pre-formulated questions, but rather in raising interesting questions, connections, and hypotheses for further study.

By contrast, most psychological research is explanatory in Babbie's sense, and only rarely exploratory or descriptive. The focus is on deductively testing specific causal hypotheses through controlled laboratory experiments. Oddly, the one thing psychologists have in common with anthropologists is a general lack of concern with sampling issues. For psychologists, this is more understandable, since the standard experimental designs use random assignment to treatment conditions as the mechanism to achieve *ceteris paribus*. But sampling issues do underlie other academics' consternation about psychologists' proclivity to generalize so readily from college sophomores to humanity at large. Are college students really a ''representative'' sample of humanity? Yes, for some research questions, and definitely not for others (Henrich, Heine, & Norenzayan, 2010).

The larger point is that anthropology and psychology differ in their stereotypical research goals and methods, but there is no real incompatibility. The natural progression in scientific research — whether conducted by an individual or community of scholars — is to go from inductive exploratory research to increasingly accurate descriptions to deductive testing of causal hypotheses, and then repeat the cycle again and again. How convenient that anthropologists, sociologists, and psychologists are already spread out along this continuum. What a serendipitous division of labor.

One additional observation relevant to Challenge 3b (that cognitive science is interested in general accounts, whereas anthropology is interested in specifics) is that anthropology has long-standing affinities with both history and natural history. This historical perspective

makes anthropologists very skeptical of generalizations about all of humankind. They are interested in such species-wide generalizations, to be sure, but aware that generalizations discovered through comparisons of naturally occurring variations may well apply only to particular types of societies, to particular kinds of people acting within particular contexts. Indeed, as Kluckhohn and Murray (1953) succinctly described the nature of human variation more than a half century ago:

Every man is in certain respects:
1. like all other men.
2. like some other men.
3. like no other man. (p. 53)

Thus, although psychologists like to generalize about human nature and anthropologists like to identify differences among cultural groups, these disciplinary proclivities are not really competing perspectives so much as complementary agendas. Both pursuits are necessary to explicate human variability. Indeed, both camps need the other as a corrective: anthropologists to catch psychological over-generalizations, and psychologists to catch anthropological myopias.

2. Anthropology and cognitive science: Growing apart

The key features of cognitive science, as identified by Gardner (1985), consist of five guiding principles:
1. Representations as a separate level of analysis.
2. Computers as both model (syntactic engine, computational mind) and tool (platform for simulations).
3. De-emphasis on affect, context, culture, and history.
4. Belief in interdisciplinary studies.
5. Rootedness in classical philosophical problems.

The first, second, fourth, and fifth of these descriptive generalizations were and are compatible with the worldview and interests of anthropological research. The third principle, however, is definitely not. To the extent that cognitive science has remained focused on studying the ''individual-in-isolation,'' it is *not* interested in the social organization nor the social transmission of knowledge through time, yet both of these above-the-individual processes are central to anthropology. Nonetheless, and for rather quirky historical reasons, during the 1950s through 1960s, some anthropologists were doing research in which affect, context, culture, and history were not the main concerns.

Borrowing heavily from linguistics, the focus of these early cognitive anthropologists was developing formal models of word meanings within rather narrowly defined semantic domains. Rather than translating one word at a time, however, these researchers focused on how words within a lexical set *contrast* with one another, and initially the domain of interest was kinship terminology. Their new approach for understanding terminology systems was

called ''componental analysis,'' because the objective was to identify the separate components of word meaning that underlay contrast sets (e.g., Goodenough, 1956; Lounsbury, 1951; Romney & D'Andrade, 1964; Wallace & Atkins, 1960). As the lexical domains being studied expanded beyond kin terms, researchers developed more and more explicit methods of data elicitation (e.g., Conklin, 1969; Frake, 1964; Metzger & Williams, 1966), and this style of research became known as ''distinctive feature analysis,'' ''semantic feature analysis,'' or just ''ethnosemantics'' (see D'Andrade, 1995; and Goodenough, 1970, for more complete overviews).

The key point for present purposes is that these early ethnosemantic studies were not concerned with and did not take into account intracultural variation. Generalizing from a few key informants, anthropologists translated selected exotic lexicons as if every Bongo-Bongo were in complete agreement about the meanings of these words. In this sense, they were compatible with the individual-in-isolation framework, and, not surprisingly, many of the anthropologists doing this kind of work were involved with the new interdisciplinary effort that became cognitive science.

By the mid-1960s and 1970s, three problems with componential analyses became clear. An important criticism, levied by non-practitioners, was that componential analyses of isolated semantic domains (e.g., firewood, beer categories, etc.) were seldom relevant to the truly important beliefs and behaviors of people living in the societies being studied. The charge was that ethnoscientists had retreated into their methods and lost sight of what should be studied (Berreman, 1966). Agreeing with this criticism, mainstream cultural anthropologists began regarding these ''cognitive'' studies as rather trivial undertakings. Practitioners themselves came to realize two different sorts of issues. The first was how to decide among alternative componential analyses of the same lexical set. This came to be known as the ''psychological reality'' problem (Burling, 1964; Wallace, 1965), and it was quickly realized that additional, psychological data were required to make such a determination. The second issue noted by practitioners was that distinctive feature analysis is geared to closed lexical sets that have relatively few named categories and are organized paradigmatically rather than hierarchically. That is, while componential analysis works pretty well for kin terms or personal pronoun system, it is not particularly useful for elucidating the folk classification systems of more complicated domains, such as kinds of trees, illnesses, or religious beliefs.

There were two noteworthy consequences of these criticisms and methodological issues. First, because of its (perceived) lack of relevance to important human issues, cognitive anthropology remained a rather small club within anthropology as a whole. Second, the research interests of cognitive anthropologists themselves shifted from revealing the *defining* features of words within a small contrast set to identifying the *salient* features that organize more complex domains. And, in order to identify the salient features of domains, data collections shifted from working with single informants to administering specific ''tasks'' (e.g., free-listing, pile-sorts, triadic comparisons, analogy-frames) to *samples* of informants and aggregating the results (D'Andrade, 1995). The increased use of samples of informants forces one to take seriously the fact that cultural knowledge is not uniformly replicated among individuals, which in turn makes the individual-in-isolation viewpoint less and less

palatable. On the other hand, the increased reliance on specific tasks to elicit data is very compatible with standard practice in psychology. Thus, through the 1970s and into the early 1980s, some cognitive anthropologists continued to see their work as relevant to and even an integral part of cognitive science.

Since the mid-1980s, however, there have been two additional developments within cognitive anthropology, neither of which has much in common with the overall agenda of mainstream cognitive science. For the past quarter century or so, the primary foci of research in cognitive anthropology have been (a) ferreting out ''cultural models'' from the way people naturally talk about things; and/or (b) using ''cultural consensus analysis'' to study the social distribution of knowledge.[1] I would suggest these more recent shifts of interest are both, for different reasons, responsible for cognitive anthropologists being less interested in cognitive science now than they were some years ago.

3. Two (potential) areas for rapprochement

The cultural model approach (e.g., D'Andrade & Strauss, 1992; Holland & Quinn, 1987; Quinn, 2005; Strauss & Quinn, 1997) has many antecedents within anthropology. For example, as an offshoot of his ''trouble-case'' method for studying the law-ways of people who have no explicitly codified laws, Hoebel (1954) was among the first to condense his ethnographic understanding into a list of basic postulates and corollaries. But, whereas Hoebel developed his postulates intuitively, drawing upon his overall understanding of Cheyenne or Ifugao or Comanche culture, today's researchers construct their cultural models through detailed textual analyses of interviews with informants, that is, their models are grounded in the ways particular individuals talk about particular topics in particular contexts.

The concept of cultural models is rooted in a general cognitive perspective and proposes that internalized versions of socially transmitted knowledge are what individuals use to interpret the world and to act within it. In his ''ontology of cultural forms,'' D'Andrade (1995) uses three psychological criteria to distinguish cultural models from other mental entities: (a) cultural models are fairly high-level configurations, meaning they are composed of too many schemas to fit into short-term memory all at once; (b) they are mostly implicit rather than explicit; and (c) they take a relatively long time to learn and to modify, which implies they may involve connectionist learning rather than serial symbolic processes.

There are cultural models of American marriage, disease, home heat control, the mind, American society, the blue crab fishery, and romantic love, to name just a few. The concept has evolved somewhat over the years, but two key features persist: (a) individuals are not passive receivers of cultural models, rather they actively use and interpret these for their own purposes; and (b) only shared (socially transmitted) mental models qualify to be called cultural models. Of course, the notion of sharing immediately raises two questions — shared by whom, and shared to what extent? Proponents of the cultural model approach are aware that sharing is a complex notion and always a matter of degree (e.g., Strauss & Quinn, 1997,

pp. 122–134, provide a very nice discussion of the conformity-inducing and variation-inducing factors at play in society). Still, the cultural model approach does not *require* researchers to study inter-individual variation, and few do. By contrast, this is the stock-in-trade of cultural consensus theory.

Cultural consensus theory developed in response to the problem of describing a cultural system in the face of informant disagreement. When different informants do not share the same folk classification of, say, birds, whose culture should the anthropologist describe? More generally, given the ''variable participation of individuals'' (Linton, 1936) in a culture's ''information pool'' (Roberts, 1964), how can the anthropologist decide if there is a common culture or just individual differences? Boster's (1980, 1985) study of Aguaruna manioc identification pioneered a solution to this sort of question. The key to the approach lies in taking seriously the fact that no one knows all of his or her group's culture and that agreement is a matter of degree. By examining the patterning of agreement among informants, Boster suggested one could detect whether individuals' understandings of a particular domain are uniform, variable in the form of expertise gradients, variable by sub-group affiliation, or just randomly idiosyncratic.

Romney, Weller, and Batchelder (1986) developed this insight into cultural consensus theory. Drawing on psychometric testing theory, consensus theory assumes that ''the correspondence between the answers of any two informants is a function of the extent to which each is correlated with the truth'' (1986, p. 316). When people know the same ''cultural truth,'' they will converge on the same answer. When they do not know, they will guess. The formal consensus model (designed for nominal-scale data) begins by computing a respondent-by-respondent agreement matrix for a battery of similarly formatted questions. Each cell represents the proportion of answers that a pair of respondents agreed upon. After correcting this raw agreement matrix for guessing, the matrix is factor analyzed using the minimum residual technique. If the three assumptions of the formal model are met (there is a single common culture, respondents answer independently of one another, and the questions are of equal difficulty), it follows by mathematical necessity that the eigenvalue of the first factor will be substantially larger than the eigenvalue of the second factor. Furthermore, the mean first factor loading will be fairly large, because each respondent's first factor loading provides a condensed measure of how well that individual represents the entire sample (and the numeric value closely approximates the fraction of questions ''known''). Whether this group-representativeness variable is associated with other characteristics of individuals, such as age, sex, education, shoe size, or hair color, can be checked using standard statistical tests. Likewise, the second factor — indicative of the second-largest dimension of variation in the sample — can be quite revealing of subcultural differences.

In short, consensus analysis provides an inductive, Q-mode, data-reduction technique whereby one can determine the degree of cultural consensus among individuals for given sets of similarly formatted questions. The key research questions addressed by consensus analysis are as follows: (a) How much knowledge is shared? (the mean first factor loading); and (b) How is the knowledge socially distributed? (the distributional pattern, such as uniform, expertise gradient, subcultures, perspectival diversities, or random).

The cultural models and cultural consensus approaches developed almost independently of one another and involve rather different views concerning the nature of knowledge. At the same time, they have almost reciprocal strengths and weaknesses. Used in conjunction, however, they provide a culture-sensitive twist to the individual-in-isolation paradigm underlying most cognitive science research. And, because the components of cultural models can generally be translated into propositions (which can then be used in systematic data collections), the procedure for combining the approaches is fairly straightforward (Gatewood & Lowe, 2008).

Within this culture-sensitive cognitive framework, humans are regarded as very much enmeshed in a social world, where most of an individual's knowledge, perspectives, attitudes, and opinions have careers in time and space extending beyond the individual per se and derive from semi-conscious enculturation and socialization processes. That is, most of the interpretive schemes one uses in day-to-day life have social origins and function for the individual as ready-made heuristics (Moscovici, 1984, 2001). Even the material culture in one's environment embodies a great deal of mnemonics that people use when thinking through routinely encountered problems (Hutchins, 1995). At the same time, learning is a creative and fallible process accomplished by individuals one at a time, such that the personal meanings of collective representations are inherently variable to some degree (e.g., Gatewood, 1983, 2011; Strauss & Quinn, 1997). Further, individuals use their understandings creatively in day-to-day living — modifying, embellishing, and re-working them throughout their lives (Gatewood, 1985; Keller & Keller, 1996). This last point is particularly the case with respect to topics that are subject to discussion, whether in the context of interpersonal conversations or the public media. Indeed, a person's own understanding can be greatly influenced by his or her perception of other people's views (Strauss, 2005, concept of ''cultural standing'').

One area in which anthropologists could use some help from cognitive scientists is in sorting out the diverse mental entities currently lumped together as ''cultural models'' into a more realistic typology. For instance, which of the following criteria are genuinely important, which are not important, and what additional criteria should be used?

Cognitive Properties	Social-Distributional Properties
Temporal scale	Degree of elaboration across individuals
Time to become activated	Components learned separately or as package
Duration of activation	Core components widely shared but variable with respect to peripheral components, or just idiosyncratic variation
Inertial characteristics	Patterns of ''sharing'' across individuals
Time to learn/develop	Uniformly and widely shared, subcultural differences, expertise gradients, perspectival diversity, or free variation
Time to unlearn/modify	Degree to which X is a topic of discussion and, hence, subject to sociallyinduced conformity and/or polarization pressures

Functional integrity
  Number of component parts
  Degree of integration among the
   components (e.g., all activated at once,
   all activated but separately, or some
   components can be activated without
   activating others)
Generative capacity
Motivational force
Degree of implicitness/ease
 of communication

Conversely, cultural consensus analysis is something that a variety of cognitive scientists, from social psychologists to computer scientists, may want to use in their own research.

4. Conclusion

Should anthropology be part of cognitive science? Yes, definitely.

Will anthropologists re-engage (and be welcomed back)? Maybe, maybe not.

There is certainly the potential for mutually beneficial interchanges, but individuals one at a time decide how and with whom they want to spend their time and energies. Here, I hope to have sufficiently piqued the interest of cognitive scientists regarding cultural consensus analysis and tweaked anthropologists about their cognitive naivety to provoke at least some trial reunions.

Note

1. There are other important research interests (e.g., decision-making, value systems, reasoning), but more anthropological ink is being used on cultural models and consensus analysis.

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