COGNITION AS A RESIDUAL CATEGORY IN ANTHROPOLOGY

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INTRODUCTION

From time to time in the history of anthropology, there have been rumors to the effect that psychologists know something about how people think, which if it were known to anthropologists, would assist them in their descriptions and explanations of culturally organized behavior. However, following Boas (9), anthropologists have, in the main, preferred to assume that "the functions of the human mind are common to the whole of humanity" and to take as their task the discovery of the rules underlying the diversity of human organization that this common mind is capable of producing.

Nonetheless, vocabulary referring to mental events has never completely disappeared from anthropology. This is natural enough, since it is difficult to describe the actions of people in everyday language without employing terms whose referents can be taken to be events internal to the actors being described. Terminological ambiguities have sometimes led to arguments and (generally justified) accusations of pseudo-social explanations masquerading as psychological explanations.

It is the purpose of this review to determine the senses in which recent anthropological and psychological studies of human cognition are mutually

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relevant. In order to do this, we will first turn to the source of rumors about the relevance of psychology to anthropology. Then we will review several bodies of anthropological literature which routinely use cognitive language, gauging each enterprise against theoretical psychological desiderata. Our review will be highly selective, relying on two excellent and more extensive discussions of individual topics by Black (8) and Needham (54) and an important bibliography by Conklin (16). Finally, we will reconsider the communality between psychological and ethnographic studies of intellectual activity in the light of recent research.

What's Cognitive About Cognitive Psychology?

If we are to assess successfully the rumor that cognitive psychology has something to offer the anthropologist, at least one of the problems we face is to determine what it is that cognitive psychologists know, *qua* psychologists, about cognition.

The recent work within anthropology that employs cognitive terminology (variously, "modes of thought," "models in the head," "decision rules," "cognitive processes") coincides with a period in psychology in which dissatisfaction with theories of behavior led to a theoretical revolution. During the 1930s and 1940s, anthropologists interested in what psychologists could tell them about thinking did not have a rich store of relevant data and theory to draw on. The dominant psychology of learning in this era attempted to apply principles derived from the study of conditioned reflexes. An exception to this generalization, Bartlett (2, 3), was not taken up by psychology at large until long after his work was published, although his work on remembering entered into anthropological theory and field study almost immediately [cf Bateson (4), Nadel (52)].

Several publications mark a change in theoretical language and a concomitant change in empirical practice in the late 1950s. In an influential book entitled *A Study of Thinking* (12), Bruner, Goodnow & Austin introduced their empirical work with an hypothetical, everyday problem that their experiments are supposed to mirror; the decision that we have to make when newcomers arrive in our neighborhood about "what kind of people they are," in short, a problem in everyday classification. In the same period, Bruner (11) published a paper in which he defined cognition as "going beyond the information given," illustrating the need for this concept in such ubiquitous problems as formation of equivalence classes, learning about redundancy, and learning coding schemes and building theories (which requires deployment of all three of the just-mentioned abilities and more). In a watershed effort, Miller, Galanter & Pribram (51) adopted mentalistic terminology to describe the results of a variety of lines of psychological

research (including, importantly, language) which theories based upon elaborations of stimulus-response concepts could not account for, or could account for only with great difficulty.

Neisser (55), in an extremely influential book, emphasized that cognition is an active accomplishment requiring formative, constructive behaviors. The "cognitive revolution" in psychology was well under way. Research flourished in such widely diverse areas as memory, classification, problem solving, and particularly the study of language and language-related phenomena which were readily recognizable by anthropologists as phenomena they pondered in their studies of exotic peoples.

Unfortunately, a point that seems to have been lost in the rush to represent humankind in all its psychological complexity is that the rules of evidence for warranting technical statements about the operation of psychological processes did not change when psychologists ceased talking about reflexes and began to talk about plans and decisions. Loose talk is loose talk, and psychologists are as guilty of this professional hazard as any group of social scientists. But behind the loose talk of any discipline is a set of procedures which can be pointed to when the analyst is speaking technically. Psychologists have such procedures when they speak about the operation of one or another cognitive process, and these procedures universally rely on the observations of behavior at some point in the process.

To begin with, the psychologist must have a well-defined task which serves as the environment (or context) within which an informant's behavior can be framed. The tasks studied by cognitive psychologists are varied and it is no easy matter to determine what makes a task cognitive. Bartlett (3) made the useful suggestion that one posits and studies "thinking" whenever there is a gap in the necessary information available to an informant working on some task. The emphasis on extrapolation and transformation of information is also encountered often in the work of Bruner, Neisser, and others.

To be well defined, a task ought to yield at least information about the goal of the activity, the initial conditions confronting the informant, and the set of elements in the task environment that the informant confronts at any time. In other words, a well-defined task specifies all of the possible stimuli which the subject might have to attend to in the course of proceeding from one point in the solution of a task to another.

Next the cognitive psychologist requires a circumscribed and predetermined set of behaviors that are allowable within the task environment. If behaviors that are *not* a part of the analytic system have to be taken into account, it is not possible to specify the probability of one event (a response) relating to other events (stimuli) because the sample spaces for both sides of the function are of indeterminant size.

Finally, one must have a model which specifies the relationship between various states of the task environment (the stimuli) and the various "moves" (the behavior) of the informant within the task environment. "Cognitive process" then refers only to the model-generated function relating behaviors in the task environment to its different states.

It is rare to meet all these specifications completely, so analysts rely heavily on a fourth requirement: the specification of relations between behavior and task for systematic (parametric) variations in a range of principally similar task environments. In practice, this means giving subjects more than one problem of the same type or using problems which have enough states so that various "subgoals" can serve as "problems of the same type." "Remembering," for example, requires at a minimum that one have a presentation phase and a remembering phase of a task. A theory that specifies the remembering activities must always be predicated on the analyst's ability to specify performance differences for two or more states of the task environment (two or more retention intervals, kinds of stimulus materials, etc). In fact, most theory testing in cognitive psychology never makes the hypothetical processes available directly to the analyst. Rather, processes are inferred from differences between two or more task environments that differ in a model-relevant way (67). Insofar as this is true, cognitive psychologists do not study "what is in people's heads." They study differences in what people do in two or more highly similar task environments where behavior is severely restricted. Moreover, they rarely if ever base their theories on the behavior of individuals, but rather on the aggregated differences of behavior between theoretically differing tasks.

In the best described tasks in cognitive psychology, as in the work of Simon and his associates, thinking and its various subcategories (comparing, searching, retrieving, etc) are embodied in "precisely stated (computer) programs and the data structures" (69, p. 148). Like many cognitive theorists, Simon and Newell develop computer programs which simulate aspects of the subjects' behavior. Assuming that the analyst can show proper correspondence between what the program and the informant do in a specified task environment, the ability to program behavior becomes the guarantee for the reality of the psychological processes said to be inside the head.

However, the theory need not be embodied in a computer program. Equally explicit theories have been written in terms of an axiom system whose implications are worked out algebraically (22), and the psychological literature is saturated with more prosaic theories of thinking based on extensive research in circumscribed sets of task environments. As the theories come to be couched in less specific and formalizable terms, the warrant

for talking about particular cognitive processes in operation becomes progressively weaker. As we will try to make clear in the following sections, most anthropological statements on thinking fall at the extreme end of weakly defined operations for the specification of cognitive processes.

Implications for Cognitive Anthropology

Despite the antiseptic way in which we have presented the goals and ideal methods of cognitive psychological research, important similarities between the psychological enterprise-as-described and anthropological writings on the topic of thinking should be discernible.

In both disciplines we find practitioners referring to thinking as a process that goes on "inside the head." Consider the following statements from Simon & Newell (69, p. 147), "An explanation of the processes involved in human thinking requires reference to things going on inside the head"; and from Frake (27, p. 133), "If we want to account for behavior by relating it to the conditions in which it normally occurs, we require procedures for discovering what people attend to, what information they are processing, when they reach decisions which lead to culturally appropriate behavior. We must get inside our subjects' heads."

Both disciplines seek careful specification of the tasks that they describe. We have already made the case for this goal in cognitive psychology. Much of the argument concerning statements said to reflect primitive mentality [e.g. the Nuer classic, "Twins are birds" (23)] are rooted in disagreement about the definition of what tasks people are working on when they say such a thing. Similarly, the prime aim of the "new ethnography" was the careful description of behavior as a display of knowledge in carefully constructed eliciting contexts, preferably as such contexts normally occurred in the lives of the people queried. Black (8, p. 529) put the point very clearly: "If all objects and behaviors observed by a field ethnographer are responses to some stimuli or answers to some unstated question, it follows that his business is to discover the stimuli or questions to which the actions are responses." This characterization fits perfectly Frake's statement of methods: "What we want to do then is to discover how a person . . . finds out from one of his fellows what he knows" (27, p. 133). Both psychologists and anthropologists interested in native thinking are concerned lest the analyst misconstrue the task as it is being dealt with by the informant, which by definition disables the analytic enterprise of specifying thought processes.

Despite these similarities, there are also some fundamental differences. Anthropologists seek out tasks that systematically represent the organization of activities across tasks within a community, whereas psychologists

want to generalize across individuals within a task as a first step. The psychologist seeks conclusions about the within-informant workings of the human mind (as they can be specified for a given task environment) while the anthropologist is groping for conclusions about culture. For example, Geertz (29) provides us with a relatively detailed analysis of a Balinese cockfight (certainly a complex task) because it offers a "metasocial commentary" upon status relations within a community. Psychologists are less interested in the specification of a task within the system of tasks which mark the intellectual life of a community. The cognitive psychologist seeks maximal analysability of the way in which cognitive processes are operating in the task at hand.

The remaining differences can be seen to follow from this fundamental difference in orientation. The psychologist and anthropologist differ quite visibly in the kinds of tasks they study precisely because of the differences in the generalizations they want to make. Often the psychologist resorts to task environments which are impoverished from the point of view of everyday practice, precisely because without this impoverishment, analytic power is lost. The anthropologist generally does not try to create, and certainly would not rely solely on the use of a model environment for delineating the contexts for native behavior. The anthropologist would not carefully coach informants to restrict their attention to the problem-as-given and their behaviors to those allowed. Instead, the informant's task must be *inductively* specified, through discovery procedures that gradually make clear what is going on. This inductive approach means that the ethnographer must discover both the "task environment" and the "behaviors." Again, to quote Black (8, p. 529), "The point is that we don't know much about a culture until we know what question is being answered; an act is not meaningful until one knows the context or stimulus."

As pointed out by Cole and Scribner (14, 65), by choosing different ways of proceeding in the delimitation of task environments, psychologists and anthropologists differentially restrict their conclusions. Psychologists are more efficient at modeling thought processes in specific task environments, but consequently have difficulty generalizing their results to adequate statements about the thinking anyone might do in other settings. Anthropologists aim their inquiries to a larger range of phenomena, but it is unclear how much they are able to specify about thinking.

MODES OF THOUGHT

One of the striking developments in recent studies of culturally organized belief systems, especially in England and on the Continent, is a revival of interest in the nineteenth century notion that "primitive" and "civilized"

cultures are distinguishable, among other things, by a characteristic called their *modes of thought.*

The confusion that has suffused this topic from its inception has several sources which are our major concern here. The first involves the term "modes of thought" (or "mentality") as it appears in this discussion. In principle, anthropologists have followed Durkheim in maintaining a distinction between the formal properties of cultural belief systems ("collective representations") and the mental operations of individuals when thinking about (or "with") these belief systems. In practice, however, the distinction is difficult to maintain, as shown in the writings of Levy-Bruhl (46, 47). "Collective representations are social phenomena, like the institutions for which they account; . . . social phenomena have their own laws, and laws which analysis of the individual *qua* individual could never reveal" (46, p. 63). This statement is followed by a multitude of assertions that are virtually impossible to interpret as being about other than individual thought processes: for example, "The preconnections, preperceptions, and preconclusions which play so great a part in the mentality of uncivilized peoples do not involve any logical activity; they are simply committed to memory" (46, p. 93).

There is little wonder that Boas and others could conclude that Levy-Bruhl was making statements about the operation of individual thought processes and admonish him with his own principle that there is no justification in concluding illogicality of individual thought from the falseness of cultural belief systems!

Perhaps because our language relies too heavily on terminology that attributes traits to individuals, the confusion over "modes of thought" remains in current discussions. In their introduction to *Modes of Thought* (43), Horton & Finnegan never resolve the issue of the referent(s) of the phrase, nor do the contributors to their book. "Modes of thought" is applied variously to how people process information in well-defined tasks, the logic of beliefs, the logic of the activity in which the belief is used, cultural ideals embodying rules for thinking, and the "idiom" in which theory is cast. There is no agreement on whether it is the activities of individuals or cultural groups that are being referred to.

In the course of the discussion, a great deal of dispute turns on the second problematic area in this discussion: in those settings which furnish the data concerning primitive thinking, what precisely are the tasks in which people are engaged? Horton (41, 42) and others in the "intellectualist" tradition assert (in our terminology) that the "task" confronting the traditional and scientific thinker alike is to explain the world by building theory. The common purpose of theory in the settings in question is to place explanations in a broader context because the narrower context of common sense

won't suffice. In our reading of Horton, it is the explanatory power of "modes of thought" as institutionalized systems of explanation, rather than as internal "mental operations," that distinguish traditional and scientific thought.

We read some of the more interesting critics (62, 70) of the intellectualist position as claiming that preliterate people who exhibit the characteristics of primitive thinking are not, in fact, engaged in the task of theoretical explanation. Tambiah, for example, maintains that ritual acts (which he claims statements reflecting primitive mentality to be) are "like illocutionary and 'performative' acts . . . [they] have consequences, effect changes, structure situations, not in the idiom of 'Western Sciences' and 'rationality' but in terms of convention and normative judgment, and as solutions of existential problems and intellectual puzzles" (70, p. 226). Tambiah goes on to admit that magical activities may be directed toward curing a physical problem as well as toward maintaining the normative order. In effect, he is claiming that more than one task is involved in the instances cited, and hence more than one interpretation of the behavior.

From a cognitive-psychological perspective, the failure to find grounds for agreeing on task and behavior in the work reviewed is crippling to the effort to decide the status of claims about thinking offered by the various analysts. Nor should anyone rest content with those varieties of the discussion which are concerned with the logic of belief systems as reflective of either the logic or the thought processes of individuals. Cognitive psychologists currently treat "logical thinking" as a culturally elaborated, institutionalized procedure for drawing inferences; "logic," "deduction," and inference represent hypothetical processes compounded of elementary terms used as theoretical entities in explaining behavior in highly constrained environments of a special sort (64) and are not considered generic terms in the study of thinking.

Fortunately, it is not psychologists alone who take this view, which has been put to excellent use by Goody (39, 40) in a challenging set of essays on *The Domestication of the Savage Mind.* Goody suggests that many of the valid aspects of the primitive-civilized thought dichotomy "can be reduced to changes in the mode of communication, especially the introduction of various forms of writings" (39, p. 16). In particular, Goody states that

The specific proposition is that writing, and more especially alphabetic literacy, make it possible to scrutinise discourse in a different kind of way by giving oral communication a semi-permanent form; this scrutiny favoured the increase in scope of critical activity, and hence of rationality, scepticism, and logic to resurrect memories of those questionable dichotomies. . . . ; the human mind was freed [by literacy] to study static "text" (rather than be limited by participation in the dynamic "utterance"), a process that enabled man to stand back from his creation and examine it in a more abstract, generalized, and "rational" way (p. 37).

The gradual increase in the sophistication of literate technology is seen as both cause and effect of increases in cognitive power in Goody's dialectical treatment of the interaction between task, technology, and actor. Where Goody points to a change in "mental technology" as a key differentiator of modes of thought, he makes contact with psychological discussions of cognition by attempting to specify changes in cognitive tasks and the behavior which they permit.

A similar line of argument, but one which literacy enters into only in passing, is presented in a provocative article by Shweder (66), who claims that "magical thinking is an expression of a cognitive-processing limitation of the human mind" (p. 637). Shweder's argument, which applies equally to "them" and "us" ["most of us have a 'savage' mentality most of the time" (p. 637)] is based upon an ingenious extrapolation of data showing that even college-educated American adults are not as likely to remember the nonoccurrence of an event as effectively as its occurrence. A consequence of this "memory limitation" is that the frequency of the co-occurrence of two events, rather than the correlational information about these events (which includes the combinations of their co-occurrence and nonoccurrences) is used to estimate the likelihood that one event will be followed by the other. When this memory limitation is combined with conceptual similarity between the events in question, conceptual similarity replaces the real covariation as the information relied on (in such statements as "fowl's excrement cures ringworm").

From our current perspective, Shweder is claiming that, unassisted, people everywhere face limitations on the kinds of information they have available to make causal inferences. They are prey to common confusions arising from the universal capacity to apprehend the similarity between objects and events in a variety of ways, and as a result, they make common errors of inference which are labeled "magical thinking" when they give rise to seemingly bizarre statements of the kind "twins are birds."

An interesting link between Goody's claims about the consequences of literacy and Shweder's observations about confusion of causality with co-occurrence and similarity is that both point to ways in which literacy can change the structure of the information that people consider, thereby reducing the chances of error. The only conditions which Shweder reports as effective in getting people to consider true correlations is to have the information summarized in a contingency table, which (in effect) totally eliminates their dependence on memory and puts before them simultaneously all of the information necessary to draw the correct inference.

These new lines of thinking concerning the problems touched on in the modes of thought literature seem promising insofar as they make explicit the link between structure of the task environments, in terms of which

people are doing their thinking, and the nature of the cognitive products. However, with Tambiah and others, we think it useful to remember that in many of the contexts where "magical thinking" is exhibited, it is very reasonable to assume that the informants are not engaged in the enterprise of doing theory, but in exerting social control.

COGNITIVE ANTHROPOLOGY

In the mid-1950s, a small group of young anthropologists concerned with how to do better ethnographies borrowed some techniques from structural linguistics which seemed so impressively scientific at the time. They started a movement known variously as ethnographic semantics, cognitive anthropology, ethnoscience, and even the New Ethnography. Its basic concern for adequate description, well stated by Conklin (15) and Frake (26), for example, was obscured in the controversy surrounding its methods and its rhetoric.

Perhaps the first principle of cognitive anthropology was the quite sensible notion that people must somehow communicate about whatever is most important to them. Accordingly, a record of what it is that they talk about and name can offer an interesting gloss on what it is that they know. To paraphrase Frake's most cognitive sounding programmatic paper (25), such a record will not capture all of a cognitive system, but it will certainly represent an important part of it, namely, the part that people use in getting each other organized to attend to certain issues in everyday life. The definition of contrast and inclusion across native terms became the measure of native concern and knowledge (15, 25, 36).

This approach produced some quick victories, particularly in the area of kinship terminology (50), and also in the description of the use of native knowledge in the organization of subsistence (18) and social interactional systems (26, 38). The important thing to note about most of the advances is that they were embedded in long-term ethnographic research. The new ethnography offered some new data gathering and data presentation techniques, but it was successful to the extent that it was embedded in traditional ethnography by participant observation.

In the excitement of new advances, methods and claims began to outrun their original purposes, and the specifics of the taxonomic representation of people's knowledge began to replace a concern for the people and the uses they might make of their knowledge. Although Frake (25) was clear to warn that *use* had to be the primary starting point of analysis and behavior the ultimate criterion for success in any analysis, we have had to wait until recent years to get a systematic body of accounts of the situational variability in the meaning of terms [e.g. Casson (13), Rosaldo (59)]. Between its

ethnographic beginning and most recent developments, cognitive an-
thropology earned its name as an attempt to understand the thought pat-
terns of peoples in different cultures. There are three strands to this effort
for us to review for their statements about cognition.

Testing for Psychological Reality

Once ethnographic accounts of various cultural domains began to record
conflicting taxonomies or domains too fuzzy to put within the confines of
a taxonomic chart, one immediate response was to probe deeper into native
naming behavior to uncover a more real (in the sense of "psychologically
real") representation of native knowledge. Although the effort was unfail-
ingly ethnographic in the sense that analysts were still interested in the best
way to represent the specifics of the knowledge system of a particular
people, the techniques used in these accounts of psychological reality (triads
tests, for example) were quite divorced from participation in the everyday
life of the people under analysis. This is an intriguing development in that
it marks a move to more careful constraining of task environments and
shows that some anthropologists shared the psychologists' belief that to
make statements about the psychological relevance of any description, the
task the subject works on must be well defined (if only in terms of con-
straints arranged by the analyst).

   With the advent of multidimensional scaling, the dimensions of native
classificatory systems came to be represented by a delineation across a large
data field of "what-goes-with-what" (58). A basic assumption of this ap-
proach (particulars of the statistical aggregating assumptions aside) is that
objects are located in relation to each other conceptually on the basis of the
conjunction of a limited number of their common attributes or features.
These features are discovered in the "dimensions" which emerge from the
data-analytic technique.

   While these techniques may be a useful way economically to display
regular relations in a body of data, we agree with D'Andrade (20) that "The
multidimensional techniques gave us one representation about what people
believe, but not a representation of how people go about believing" (p. 155).
D'Andrade's "implicational analysis" adds to the richness of the descrip-
tions of the relations between concepts that have heretofore been studied
by multidimensional scaling techniques. In a discussion of this work, D'An-
drade tells us that ". . . thinking consists of more than classification. Think-
ing involves inferences, and an effective structure for making inferences
requires at least the use of relations" (p. 179). This statement is undoubtedly
true, but it should not be construed as a claim on D'Andrade's part that
he is discovering "how people think" in a cognitive psychologist's sense
owing to his use of a new data analysis technique. Rather, he is continuing

his search for ways to make explicit the operations by which analysts come to make statements about the concepts and relations that constitute belief systems.

Principles of Folk Classification

A second literature within anthropology which has been given to cognitive claims has focused on the semantics of folk classifications of objects (particularly living ones) in the natural world and their colors. This literature differs from the literature actively engaged in testing the psychological reality of taxonomies by concerning itself with the principles of classification across persons, domains, and even cultures rather than with the particulars of a taxonomy as it is elaborated by a particular people for particular purposes. This tradition strays far from the ethnographic base from which it emerged in order to make broader claims about the human mind and its evolution, a diversion which causes considerable problems in the interpretation of the taxonomic data it has to work with, as they are not constrained by their normal contexts of occurrence in a natural community (17, 63).

The major event in this literature is Berlin & Kay's (6) book on *Basic Color Terms,* which displayed an apparently universal organization of options in the arrangement of color terms in the world's languages and suggested an evolutionary progression in the organization of color terms across cultures of differing complexity. In moving to other domains of classification, similar results have been accumulating; folk taxonomies of different domains appear to be organized according to similar structural principles (5, 10, 59). Although this work has produced some rhetoric about the structure of the human mind (for example, because there is evidence that taxonomies of things appear to have a ceiling of five hierarchic levels), few practitioners of the research try to claim that such folk taxonomies (or alternative representations) actually represent how the world is perceived and known by particular persons (e.g. 44). Most investigators appear content to live with the less powerful claim that the folk taxonomies represent only the categories available to people if and when they do some thinking about the domains in question.

In view of their data-gathering techniques, students of folk classification are well advised to avoid psychological claims. Formal interviews on the similarities and differences in named objects or colors, and dictionary forays with the same ends in mind, cannot give strong data on how people are processing the information in question. In the terms of this paper, the task is ill defined from the points of view of either analysts or natives. When the distinctions the people make fall into a pattern, something interesting must be going on, but it is hard to know about it in any detail.

One of the most interesting statements about the current status and the future course of this effort has been offered recently by Rosch (60, 61).

Although her work has often been interpreted as defining a competence model for the kinds of conceptual structuring systems people have in their heads, Rosch has recently stated that: "the issues in categorization with which we are primarily concerned have to do with explaining the categories found in a culture and coded by the language of that culture" (61, p. 2). She specifically rejects the idea that her research is designed to specify how the categories are processed. Before more detailed psychological statements can be made, the objects specified in the classifications would have to be detailed in terms of their use "in the events of everyday life" (p. 25). In line with this objective, Rosch has initiated a study of people's classification of events and objects in events. If it is the case that "events stand at the interface between an analysis of social structure and culture and analysis of individual psychology" (p. 27), then her future work will be of special interest to anthropologists.

Models of Native Decision Making

A third development in the literature growing out of ethnographic semantics has focused on decision making in everyday life. In assuming that members of a society do not simply have knowledge about cultural principles, but act in accord with them, decision theorists appear to be making a number of claims about psychological processes, but the exact status of cognitive psychological claims is often difficult to pin down, a situation which is mirrored in studies of social decision making by psychologists (68).

In Goodenough's (37) classic discussion of Trukese residence rules, psychological claims are almost incidental. His concern was to show that the ethnographer must develop a theory of the choices available to the members of the particular culture. He suggests that the categories of residence and criteria of choice can be validated by using them to predict where actual married couples would choose to live or to predict where hypothetical couples would live and seeing if these predictions "would agree with those which members of society would also make for such hypothetical marriages" (p. 29).

Similar to Goodenough's discussion of residence choices are Keesing's model of fosterage (45) and Fjellman's (24) and Ebihara's (21) accounts of residence decisions. While each of these authors infers that individuals have the cultural knowledge needed to make decisions, how the decisions are made is not necessarily attributed to individual actors and no claims are made concerning psychological process.

In contrast to the work of Goodenough, Geoghegan's studies on the use of address terms (30) and residential decision making (31) among the Eastern Samal of the Philippines contain explicit psychological claims. In the residence study he promises "a formal model of the cognitive process underlying a native actor's decisions about residence" (p. 1). Geoghegan derives

his model of residence rules from interviews of one principal informant, "and the resultant model is intended to represent a portion of the cognitive organization of only this one informant." The model is claimed to have generality for both the individual informant and "native actor competence in general." The test of this claim is to use residence and status information for members of the population as input; their actual residence mode is then predicted within a small margin of error.

C. Gladwin (32) modeled Mfantse fish sellers' decisions of whether to take their fish to market, and if so, where. She developed a model on the basis of interviews with fish sellers and output data from a large number of actual trips to market. The model, which consists of a series of steps that Gladwin assumed "a fish seller mentally goes through" (p. 99), accounts for 90% of the variance in market trip data. The model assumes that individual fish sellers calculate the probability of a particular market being good given its condition the day before. Although the Mfantse do not talk about such calculations, the model assumes that over many years of observing market conditions the fish sellers have as part of their knowledge something equivalent to a contingency table for making this calculation. Validation for this model does not come directly from informants' verbal reports but from the success of the model for predicting the data on when the traders went to market.

In a study of fish sellers from a village neighboring the one studied by Gladwin, Quinn (57) has argued strongly in favor of the importance of verbal reports. Based on interview data alone, she addressed Gladwin's assumption that fish sellers make probability estimates. Quinn suggests a model of fish sellers' decisions which offers a "cognitively plausible" alternative to the assumption "that individual decision makers can and do construct probability distributions against which to assess the riskiness of uncertain decisions" (p. 3). Quinn does not test her alternative model against actual outcomes as did Gladwin, although she acknowledges the usefulness of such a test. Her argument is intended to demonstrate how verbal reports can be used in the initial modeling of economic choices. Quinn's argument that sellers do not construct probability distributions is based on her failure to find any mention of such a procedure by any of the individual sellers. Whether it better predicts economic behavior is left unclear because Quinn shifts the criterion, defending the new model on the grounds that it is more consistent with accounts of how decisions are made.

In order to evaluate the psychological claims in any of these studies of decision making, we must examine the suitability of interview data for making statements about cognitive processes. Verbal reports can say a great deal about the kinds of things it is possible to know about in a culture, and cognitive anthropologists have gone to great lengths to show how this can

be done carefully (1), but considerable data have been reported recently about the limits of treating what people say in interviews as literal data about thought processes. D'Andrade (19) has reported that interview data about remembered events are particularly unreliable because they are strongly skewed in the direction of semantic relations between the descriptive terms used. Nisbett & Wilson (56) have shown that subjects asked to report on their own decision making consistently answer on the basis of their own a priori theories about how certain stimuli and plausible responses go together rather than on the basis of veridical introspective recall. Bilmes has argued and demonstrated on the basis of some Thai data (7) that misinformation and ambiguity are essential elements in the social organization of verbal interaction.

An important example of the dangers of drawing cognitive conclusions from interview data and decontextualized cognitive tests is available in the growing literature on navigational knowledge in Micronesia. For two decades now, T. Gladwin has been producing excellent descriptions of canoe voyages by Micronesians crossing hundreds of miles of open sea. He has also been using their navigational system to reach conclusions about their thinking. In the 1950s, before he began to study navigation, his conclusion was that Micronesians could not think abstractly (35). In the 1960s, after his initial research on navigation, the conclusion changed to allow Micronesians abstract thought but an inability to plan (33). Finally, in *East is a Big Bird* (34), in many ways an excellent book, the last chapter discusses the thinking of the Micronesian, arguing now that navigators can think abstractly and clearly. They must organize elaborate plans for their voyages, but they do no more conceptual problem solving than they have to do; they don't think "heuristically." After extensive interviews, and a few small voyages, Gladwin concluded that navigation on Pulawat was a closed rote system of knowledge about sailing under local conditions. This fact, in conjunction with the results of some tests in which the navigators performed at the level of preadolescent minors, was the basis for Gladwin's claim that the navigators were nonheuristic thinkers who were given to solving intellectual problems only when there was no other choice.

Fortunately, Gladwin's analysis was followed by the work of Lewis, who increased the range of observation by going with the navigators for long trips to strange places under their guidance. In his exciting book, *We the Navigators* (48), Lewis shows that the Micronesian navigational system is not as closed as Gladwin reports. Taken far off course, the navigators were consistently able to reconstruct their location and the way home by an intensive study of the stars and waves about them in ways that Gladwin would characterize as reflecting heuristic thinking. Recently these skills were further displayed by a Micronesian navigating a canoe thousands of

miles across Polynesia from Hawaii to Tahiti (49). This entire enterprise shows how the typical range of verbal reports, from open interviews to constrained psychological experiments, proves inadequate if it is not embedded in data gained from a more systematic analysis of the actual doing of the task in question.

CONCLUSION

In light of the many difficulties facing the ethnographer who seeks to make inferences about the cognitive processes of the people he studies, it is tempting to counsel retreat; the rumor that psychologists have been learning something recently that ethnographers need to know is false. Insofar as this conclusion depends upon the hope that psychologists have techniques which can determine "what goes on inside people's heads," such a retreat is more than warranted by the evidence.

However, in our opinion, the relevant contribution from psychology should not be sought in presumed privileged access to people's thought processes because it is not to be found there; rather the virtue of cognitive psychology is to be found in its procedures for limiting uncontrolled speculation about thinking. These procedures (the specification of task environment, behavior, and their relations) can yield plausible, warranted statements about the activities that organize the behavior we observe as long as the constraints on behavior are well described, by either experimental manipulation or intensive behavioral analysis. But with few exceptions, the theoretical statements which are cognitive processes for the psychologist are so limited in scope and predictive value that unless there is a special ethnographic interest in the task in question, the anthropologist is unlikely to wish to follow the psychologist in the quest for process specification.

In our view, the very extremes to which the psychologist is pushed are resources for the anthropologist. We believe that the ethnographer's question, "What is going on here?" is not different in kind from the psychologist's question, "What thought processes are involved in this task?" Rather, it differs in the level of context which is being questioned. In each of the substantive areas that we reviewed, crucial disagreement among anthropologists has turned exactly on this point—disagreement about what it is that people are doing in the task under study. In the modes of thought discussion, disagreement about task takes the form of arguments between "intellectualist, neo-Tylorians," and those who claim that the native task is not to explain the world in causal terms, but to exert social control; literacy is seen as a technology which fundamentally changes the structure of the information provided by the environment for thinking. In cognitive ethnography, the focus on native questions as a key to locating native tasks

is a close analog to the psychologist's desire for task specification. Unfortu-
nately, as Frake (28, p. 3) has pointed out,

> ... The notion that answers are there, that the job is to find the questions, while often
> cited, did not seem really to take hold. Frames began to be called eliciting frames, to be
> thought of not as contexts for behavior but as prods to behavior. The ethnographer rather
> than the informant (became) the questioner.

With the aim of substantiating cognitive claims, some anthropologists have
increased the constraints on their tasks, but have fallen short of the critieria
applied by psychology. In such cases, neither psychological nor ethno-
graphic inferences benefit; the result is indeterminacy.

In our view, each of the lines of work reviewed here makes important
contributions to our understanding of human thought. Most often, how-
ever, it is not thought-as-process but the content of thought which is the
topic of inquiry. Although there is no room to discuss the matter here, we
would claim that all but the most exacting of psychological cognitive re-
search fits (albeit imperfectly) the same characterization.

In concluding we can do no better than cite Nadel's (53) wise comment

> ... unless the relations between social and psychological enquiry are precisely stated,
> certain dangers, all-too-evident in the anthropological and psychological literature, will
> never be banished. Psychologists will overstate their claims and produce, by valid psy-
> chological methods, spurious sociological explanations; or the student of society, while
> officially disregarding psychology, will smuggle it in by the backdoor; or he may assign
> to psychology merely the residue of his enquiry—all the facts with which his own
> methods seem incapable of dealing (p. 289).

And so it goes.

Literature Cited

1. Agar, M. 1975. Selecting a dealer. *Am. Ethnol.* 2:47–60
2. Bartlett, F. C. 1932. *Remembering.* London: Cambridge Univ. Press. 317 pp.
3. Bartlett, F. C. 1958. *Thinking.* New York: Basic Books. 203 pp.
4. Bateson, G. 1958. *Naven.* Stanford: Stanford Univ. Press. 312 pp.
5. Berlin, B., Breedlove, D. E., Raven, P. H. 1973. General principles of class-ification and nomenclature in folk biology. *Am. Anthropol.* 75:214–42
6. Berlin, B., Kay, P. 1969. *Basic Color Terms: Their Universality and Evolu-tion.* Berkeley: Univ. California Press
7. Bilmes, J. 1975. Misinformation and ambiguity in verbal interaction. *Int. J. Sociol. Lang.* 5:63–75
8. Black, M. B. 1974. Belief systems. In *Handbook of Social and Cultural An-thropology,* ed. J. J. Honigmann, pp. 509–77. New York: Rand McNally
9. Boas, F. 1911. *The Mind of Primitive Man.* New York: Free Press. 254 pp.
10. Brown, C. H. 1976. General principles of human anatomical partonomy and speculations on the growth of parto-nomic nomenclature. *Am. Ethnol.* 3: 400–24
11. Bruner, J. S. 1957. Going beyond the information given. In *Contemporary Approaches to Cognition: A Symposium held at The University of Colorado,* pp. 41–69. Cambridge, MA: Harvard Univ. Press
12. Bruner, J. S., Goodnow, J., Austin, G. A. 1956. *A Study of Thinking.* New York: Wiley
13. Casson, R. 1975. The semantics of kin term usage. *Am. Ethnol.* 2:229–38

14. Cole, M., Scribner, S. 1975. Theorizing about socialization of the intellect. *Ethos* 3:249–68

15. Conklin, H. C. 1964. Ethnogeneological method. In *Explorations in Cultural Anthropology,* ed. W. Goodenough, pp. 25–56. New York: McGraw Hill

16. Conklin, H. C. 1972. *Folk Classification: A Topically Arranged Bibliography.* Yale Univ. Dep. Anthropol. 501 pp.

17. Conklin, H. C. 1973. Color categorization. *Am. Anthropol.* 75:931–42

18. Conklin, H. C. 1975. *Hanuoo Agriculture: A Report on an Integral System of Shifting Cultivation in the Philippines.* Northford, CT: Elliot's. 209 pp. Reprinted ed.

19. D'Andrade, R. G. 1974. Memory and the assessment of behavior. In *Measurement in the Social Sciences,* ed. T. Blalock, pp. 159–85. Chicago:Aldine-Atherton

20. D'Andrade, R. G. 1976. A propositional analysis of U.S. American beliefs about illness. In *Meaning in Anthropology,* ed. K. H. Basso, H. A. Selby, pp. 155–80 Albuquerque: Univ. New Mexico Press

21. Ebihara, M. 1977. Residence pattern in a Khmer peasant village. *Ann. NY Acad. Sci.* 243:51–68

22. Estes, W. K. 1975. The locus of inferential and perceptual processes in letter identification. *J. Exp. Psychol. Gen.* 104:122–45

23. Evans-Pritchard, E. E. 1956. *Nuer Religion,* London: Oxford Univ. Press

24. Fjellman, S. M. 1976. Talking about talking about residence. *Am. Ethnol.* 3:671–82

25. Frake, C. O. 1962. The ethnographic study of cognitive systems. In *Anthropology and Human Behavior,* ed. T. Gladwin, W. C. Sturtevant, pp. 72–85. Anthropol. Soc. Washington. 214 pp.

26. Frake, C. O. 1964. A structural description of Subanun 'religious' behavior. See Ref. 15, pp. 111–29

27. Frake, C. O. 1964. Notes on queries in ethnography. *Am. Anthropol.* 66:132–45

28. Frake, C. O. 1977. Plying frames can be dangerous: some reflections on methodology in cognitive anthropology. *Q. Newsl. Inst. Comp. Hum. Dev.* 1(3):1–7

29. Geertz, C. 1972. Deep play: notes on the Balinese cockfight. *Daedalus* 101:1–37

30. Geoghegan, W. H. 1973. Natural information processing rules: Formal theory and applications to ethnography. *Monogr. Lang. Behav. Res. Lab.* No. 3. Univ. California. 446 pp.

31. Geoghegan, W. H. 1978. Decision making and residence on Tagtabon Island. *J. Anthropol. Res.* In press

32. Gladwin, C. 1975. A model of the supply of smoked fish from Cape Coast to Kumasi. In *Formal Methods in Economic Anthropology,* ed. S. Plattner, pp. 77–127 Washington DC: Am. Anthropol. Assoc.

33. Gladwin, T. 1964. Culture and logical process. See Ref. 15, pp. 167–78

34. Gladwin, T. 1970. *East is a Big Bird: Navigation and Logic on Puluwat Atoll.* Cambridge, MA: Harvard Univ. Press. 241 pp.

35. Gladwin, T., Sarason, S. 1953. *Truk: Man in Paradise.* Viking Fund Publ. Anthropol. No. 20. New York: Wenner-Gren Found. Anthropol. Res.

36. Goodenough, W. 1956. Componential analysis and the study of meaning. *Language* 32:195–216

37. Goodenough, W. 1956. Residence rules. *Southwest. J. Anthropol.* 12:22–37

38. Goodenough. W. 1965. Rethinking 'status' and 'role'. In *The Relevance of Models in Social Anthropology,* ed. M. Banton, pp. 1–24. London: Tavistock

39. Goody, J. 1977. *The Domestication of the Savage Mind.* Cambridge Univ. Press. 178 pp.

40. Goody, J. 1977. Memoire et appretissage dans les societes avec et sans ecriture. *L'Homme* 17:29–52

41. Horton, R. 1968. Neo-Tylorianism: sound sense or sinister prejudice? *Man* 3:625–34

42. Horton, R. 1973. Paradox and explanation: a reply to Mr. Skorupski I, II. *Philos. Soc. Sci.* 3:231–56, 289–312

43. Horton, R., Finnegan, R., eds. 1973. *Modes of Thought.* London: Faber & Faber. 399 pp.

44. Hunn, E. 1976. Toward a perceptual model of folk biological classification. *Am. Ethnol.* 3:508–24

45. Keesing, R. 1970. Kwaio fosterage. *Am. Anthropol.* 72:991–1019

46. Levy-Bruhl, L. 1966. *How Natives Think.* New York: Washington Square Press. 355 pp.

47. Levy-Bruhl, L. et al. 1923. La mentalite primitive. Report of a meeting held Feb. 15, 1923. *Bull. Soc. Fr. Philos.* 18:2–48

48. Lewis, D. 1972. *We the Navigators.* Honolulu: Univ. Hawaii Press. 335 pp.

49. Lewis, D. 1977. Mau Piailug's navigation of Hokule'a from Hawaii to Tahiti. *Topics in Cultural Learning 5.* Honolulu: East-West Center

50. Lounsbury, F. 1964. The structural analysis of kinship semantics. *Proc. 9th*

Int. Congr. Ling., pp. 1073–93. The Hague: Mouton

51. Miller, G., Galanter, E., Pribram, K. 1960. *Plans and Structure of Behavior.* New York:Holt, Rinehart & Winston. 226 pp.
52. Nadel, S. F. 1937. A field experiment in racial psychology. *Br. J. Psychol.* 28: 195–211
53. Nadel, S. F. 1951. *The Foundations of Social Anthropology.* London: Cohen & West. 426 pp.
54. Needham, R. 1972. *Belief, Language and Experience.* Univ. Chicago Press. 269 pp.
55. Neisser, U. 1968. *Cognitive Psychology.* New York: McGraw-Hill. 351 pp.
56. Nisbett, R. E., Wilson, T. 1977. Telling more than we can know: Verbal reports on mental processes. *Psychol Rev.* 84: 231–59
57. Quinn, N. 1978. Do Mfantse fish sellers estimate probabilities in their heads? *Am. Ethnol.* In press
58. Romney, A. K., Shepard, R. N., Nerlove, S. B., eds. 1972. *Multidimensional Scaling,* Vol. 2. New York: Seminar. 323 pp.
59. Rosaldo, M. Z. 1972. Metaphors and folk classification. *Southwest. J. Anthropol.* 27:83–99
60. Rosch, E. 1975. Cognitive representation of semantic categories. *J. Exp. Psychol. Gen.* 104:192–233
61. Rosch, E. 1978. Principles of categorization. In *Cognition and Categorization,* ed. B. Lloyd, E. Rosch. Hillsdale, NJ: Erlbaum. In press
62. Ross, G. 1971. Neo-Tylorianism: a reassessment. *Man* 6:105–16
63. Sahlins, M. 1976. Colors and cultures. *Semiotica* 16:1–22
64. Scribner, S. 1978. Modes of thinking and ways of speaking. In *Discourse Production and Comprehension,* ed. R. O. Freedle. Hillsdale, NJ: Erlbaum. In press
65. Scribner, S. 1975. Situating the experiment in cross-cultural research. In *The Developing Individual in a Changing World; Historical and Cultural Issues,* ed. K. F. Riegel, J. A. Meacham, 1:310–21. The Hague: Mouton
66. Shweder, R. A. 1977. Likeness and likelihood in everyday thought: Magical thinking in judgments about personality. *Curr. Anthropol.* 18:637–58
67. Simon, H. A. 1975. The functional equivalence of problem solving skills. *Cogn. Psychol.* 7:268–88
68. Simon, H. A. 1976. Discussion: Cognition and social behavior. In *Cognition and Social Behavior,* ed. J. S. Carroll, J. W. Payne, pp. 253–67. Hillsdale, NJ: Erlbaum
69. Simon, H. A., Newell, A. 1971. Human problem solving: the state of the theory in 1970. *Am. Psychol.* 26:145–59
70. Tambiah, S. J. 1973. Form and meaning of magical acts. See Ref. 43, pp. 199–229