THE DEVELOPMENT OF COGNITIVE ANTHROPOLOGY

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Preface

I wrote this book with two major goals in mind. At present, there is no single book to which outsiders can go to find out about cognitive anthropology. Work in cognitive anthropology has been published in articles spread across a range of journals and edited collections. One goal, then, is to bring some thirty years of work together in one place.

The second reason, related to the first, is that many social and cognitive scientists do not know about recent work in cognitive anthropology. The common view of the field was set in place by work done in the 1960s on kin terms and plant taxonomies. Research during the 1980s and 1990s on cultural models, reasoning, consensus, emotion, memory, motivation, and distributed cognition is less well known. While I have not been able to do a complete review, this book is intended to provide reasonable coverage of current research and thinking.

This book is not a standard textbook; it is too particular in its perspective and involved with current controversy. Nor is it a history of the sort historians of science write, since I have been more concerned with the presentation of ideas than with the intricacies of chronology and first authorship. However, it does try to show how the field of cognitive anthropology developed over time. I wrote it to tell what I believe is an interesting story about a fascinating problem.

What is the problem? The problem is the nature of human culture. One can conceive of a society's culture, in Ward Goodenough's famous phrase, as "whatever it is one has to know or believe in order to operate in a manner acceptable to its members." Certainly humans do learn an enormous amount of cultural knowledge. The problem comes when one tries to understand what that knowledge *is*. Is it lists of propositions? Organized structures of contrasting attributes? A storehouse of images? A collection of taxonomies? A set of computer-like programs? Is it totally language based, or does it include images and physical skills?

Along with these questions about the character of cultural knowledge comes a related set of questions about how other mental processes might effect how that knowledge is organized and used, such as the limitations of short-term memory or degree to which knowledge is necessarily involved with emotion

and motivation. In turn, these questions lead to other questions about how what is learned affects other mental processes, such as long-term memory and reasoning. And underlying all of this is the crucial issue: how can one formulate these questions so that they can be investigated?

The field of cognitive anthropology has "grown up" with the other cognitive sciences. There has been a constant exchange of ideas across fields, although the practitioners in each discipline are often unaware of the parallel development of the fields or believe that these parallel developments are due to borrowing from their own field. In my experience, I have found that a good idea appears almost everywhere at once across the cognitive sciences, although how seriously and effectively a particular idea is pursued may differ greatly by field.

Overall, the agenda of cognitive anthropology has held to the idea that the study of cognition should be more than a series of propositions which are based solely on laboratory experiments. There is nothing wrong with a laboratory; a great many questions can only be answered through experimentation. However, a general goal of anthropology is to understand the natural world of human life, as it is found. What the anthropologist needs is a theory which will help in understanding how ordinary people normally organize and use knowledge.

Along with the idea that cognitive anthropology should try to understand the way knowledge is used in ordinary life, there is also the notion that cognitive anthropology should study the way in which knowledge is conventionalized into *culture*. Human knowledge is much too precious a thing to be carelessly discarded each generation with the hope that it will be rediscovered in the next. Human knowledge is carefully preserved and passed from one generation to another. Most of what any human ever thinks has been thought before, and most of what any human ever thinks has been learned from other humans. Or, to put it another way, most of what anyone knows is *cultural* knowledge. Cognitive anthropology investigates cultural knowledge, knowledge which is embedded in words, in stories, and artifacts, and which is learned from and shared with other humans.

The number of anthropologists who have been involved in creating the field of cognitive anthropology has not been large. At various times the number of anthropologists and linguists involved in cognitive research may have reached two hundred or so. About a hundred and fifty anthropologists and linguists are cited in the following chapters, along with about a hundred psychologists. Most of the work has been carried out by a shifting core which has never been larger than about thirty persons. Within this group there has been a remarkable spirit of collaboration and good will; the personal attacks and exaggerated claims to precedence and prominence typical of much writing in the social sciences have been notably absent from the cognitive anthropology literature.

This book is dedicated to those who set the agenda of cognitive anthropology: Floyd Lounsbury, Ward Goodenough, Anthony Wallace, Harold Conklin, and A. Kimball Romney. And to the fine folks who helped with this manuscript, many thanks.

1 Background

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1 Background

Cognitive anthropology is the study of the relation between human society and human thought. The cognitive anthropologist studies how people in social groups conceive of and think about the objects and events which make up their world – including everything from physical objects like wild plants to abstract events like social justice. Such a project is closely linked to psychology because the study of how particular social groups categorize and reason inevitably leads to questions about the basic nature of such cognitive processes.

Early history

The story of cognitive anthropology begins in the late 1950s. To make comprehensible what was happening at this time requires understanding certain aspects of the history of anthropology. Anthropology started as a professional field of study in the late nineteenth century. The original charter of anthropology was to fill in a missing piece of human history – or, more accurately, human "prehistory," the period of time before written history and the rise of the classic civilizations. Part of the motivation for this agenda was western society's discovery of the native peoples of the Americas, the Pacific, and the far Orient. European savants speculated with great interest and imagination about where these people might have come from and what their history might have been. Obtaining facts to resolve these questions rapidly grew into a recognized field of scholarly endeavor in the late nineteenth century.

This "prehistoric" agenda of anthropology had three different methods of investigation. One involved the direct investigation of the past through exploration of the physical remains. This became the field of *archaeology*, which started with the professionalization of the techniques that had been developed by gentlemen scholars interested in ancient Greek, Roman, and Egyptian antiquities. Methods of careful excavation were developed to work out from the stratigraphy of materials buried in the earth the chronology of early peoples. Interest spread from the study of the chronology of early Middle Eastern and European civilizations to the prehistory of North and South

American Indians, and eventually to the general study of the prehistory of humanity.

By the 1950s an enormous amount had been learned about human prehistory. A detailed chronology had been worked out beginning with the evolution of early hominids several million years ago. This chronology includes the development of hunting and gathering technologies in the paleolithic, the shift to food cultivation in the neolithic 8,000 to 10,000 years ago, and the rise of the six great independent centers of civilization over the past 5,000 years in Egypt, Mesopotamia, the Indus, the Yangtze, Mesoamerica, and the Peruvian coast.

A second method of investigation developed at the end of the nineteenth century was called *ethnography* – the observational study of the ways of life of primitive peoples. By obtaining and comparing objective accounts of the social and cultural institutions of primitive people around the world, it was thought that the historical connections and course of evolutionary development could be worked out, complementing the results obtained by the archaeologists. According to Radcliffe-Brown in 1909: "A meeting of teachers from Oxford, Cambridge and London was held to discuss the terminology of our subject. We agreed to use 'ethnography' as the term for descriptive accounts of non-literate peoples. *The hypothetical reconstruction of 'history' of such peoples was accepted as the task of ethnography and prehistoric archaeology*" (italics added).[1]

Early ethnographers were interested in the way in which particular cultural traits diffused from one society to another, and the way in which simple societies could be grouped on the basis of overall similarity into geographic clusters of societies, called *culture areas*. They were strongly divided on whether or not societies *evolved* in a series of stages from simple hunting people to complex urban civilization or were simply involved in non-evolutionary, non-directional, multiple process of *change* – an argument that is still not entirely resolved.

The third method was the investigation of human physical types. Unfortunately, this work became contaminated with the racist ideas common in western societies in the nineteenth century. However, the basic project was reasonable. This project was to collect data on physical similarities and differences between human groups so that patterns of migration and historical relations between groups could be determined, and special environmental adaptations discovered. With modern techniques of direct genetic comparison there is some hope that this agenda can now be undertaken without falling into racist typologizing.

Thus anthropology began with three fields – ethnography, archaeology, and what is now called "biological anthropology." These three fields are still found

[1] Quoted in Kuper (1983:2). Radcliffe-Brown's statement goes on to contrast "ethnology" with "social anthropology" which was defined as the *comparative* study of the institutions of primitive societies.

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in most modern departments of anthropology. It is interesting that while the general project of working out the history of early civilizations and primitive peoples has long since retreated to a minor place in both the fields of ethnography and biological anthropology, the coalition of the three fields remains fixed in the institutional framework of university and college departments.

The field of linguistics has also played a part in the general development of anthropology. From the very first it was recognized that similarities and differences in languages gave crucial information about historical relationships. Since languages change slowly, historical relationships and connections between very different societies can sometimes be discovered by linguistic comparison. While there was still controversy about the grouping of some of the major stocks, by the fifties most of the languages of the world had been classified and described in some detail (Ruhlen 1987).

Another tie between anthropology and linguistics is based on the practicalities of learning unwritten languages. To carry out ethnographic research it is a great help to know the language of the people being studied. And to transcribe and learn to speak unwritten languages, one needs to know how to transcribe exotic sounds and to know how to analyze rules of word formation and syntax. Thus linguistics became part of the field training curriculum of many departments of anthropology. Although today most universities have separate departments of linguistics, linguists are often still found in a variety of departments from anthropology to Slavic literature and cognitive science.

It might seem from the description given above that anthropology is primarily a kind of historical study. Certainly part of it – *archaeology* – is, and certainly all the fields of anthropology have contributed to our general understanding of what has taken place in human prehistory. Ethnography, however, drastically changed its goals. This change in goals is an example of an interesting phenomenon in anthropology and the social sciences which I call *agenda hopping*. Agenda hopping is different from a *paradigm shift*, a process made famous by Thomas Kuhn in his book *The Structure of Scientific Revolutions*.

According to Kuhn, at any particular time a science will have a number of examples of what is excellent science – "examples which include law, theory, application, and instrumentation together – [which] provide models from which spring particular coherent traditions of scientific research" (1970:10). Working within an established scientific paradigm is called *normal science* and involves a kind of puzzle solving activity in which the major problem is to fit new pieces of information into an already known pattern. However, there comes a time at which more and more pieces of information are found which do not fit into the pattern. Anomalies accumulate. At some point maverick scientists break out of the old paradigm and try to develop a new conceptual framework which can account for these anomalies. Such times are periods of intense controversy. Reinterpretation of the old facts into the framework of the

new paradigm is often a matter of intense debate. The shift from the Newtonian mechanics to quantum mechanics is an often cited example of such a paradigm shift.

Agenda hopping is quite a different process. What happens in agenda hopping is that a given agenda of research reaches a point at which nothing new or exciting is emerging from the work of even the best practitioners. It is not that the old agenda is completed, or that too many anomalies have accumulated to proceed with equanimity. Rather, what has happened is that as more and more has been learned the practitioners have come to understand that the phenomena being investigated are quite complex. Greater and greater effort is required to produce anything new, and whatever is found seems to be of less and less interest. When this happens, a number of practitioners may defect to another agenda – a new direction of work in which there is some hope of finding something really interesting. Note that in agenda hopping there is no reinterpretation of the old findings into a new framework as there is in a paradigm shift. Rather, there is simple abandonment of the old venture in favor of a new set of problems.

Although the old agenda may still be a reasonable endeavor (except for its dullness and difficulty), the defectors to the new agenda will usually attack the old agenda with great vehemence. They may say the old agenda simply cannot be done (since they could not do much with it), and therefore is unscientific or irrelevant, or that it is just too incomplete (since it does not cover the phenomena they are now interested in), or more simply and brutally that it is "old-fashioned" and "out-of-date." These attacks on old agendas are unfortunate, since they often denigrate a record of considerable accomplishment.

Agenda hopping often begins quite early in the history of a field. By the time of World War I a number of ethnographers had already begun to abandon the historical agenda for the study of simple societies. Adam Kuper describes the situation as follows:

> But if one were to characterize the mood of British anthropology in the first decades of this century one would have to stress the over-riding concern with the accumulation of data. The ultimate goal might still be the reconstruction of culture history, or evolutionist generalizations, but these interests were overlaid by a strong resurgence of British empiricism. There was a feeling that the facts which were increasingly becoming available made *facile evolutionist and diffusionist schemes look rather silly.* (Italics added) (1983:5)

These are the perceptions of scientists about to abandon an old agenda for a new one. The old agenda had become entangled in problems as data was collected. Rather than modifying the evolutionist and evolutions schemes so that they would *not* look "silly," the decision was to move on to a new agenda in the name of empiricism. Bronislaw Malinowski, the great ethnographer of the Trobriand Islands, developed a series of intricate arguments for why historical

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study was irrelevant to the study of primitive societies.[2] Radcliffe-Brown, who had done pioneering ethnographic work among the Andaman Islanders and Australian aborigines in the first decade of the twentieth century, also made his contempt for "conjectural history" quite clear. Although Franz Boas, who had founded the first department of anthropology in the United States at Columbia University and trained the first elite cadre of professional anthropologists in America, remained affiliated with the old historical agenda of anthropology, by the 1930s most American ethnographers were primarily engaged in the new agenda.

It should be stressed that there is nothing wrong with agenda hopping: indeed it is a good thing if the new agenda has scientific potential. And the new agenda of the early twentieth-century ethnographers proved to have real potential. This new agenda was focused on *the detailed examination of how the institutions of society are integrated together to make society function*. According to the new agenda, the institutions of a society are not just a jumble of traits, but rather a set of *learned and prescribed activities which are coordinated with one another to bring about a satisfactory way of life and maintain social order*.

While they wrote about their data using the general term "society," these anthropologists were in fact exclusively interested in the functioning of simple, kinship-based, non-literate societies – an inheritance from the previous "pre-historical" agenda. To carry out such a detailed examination required extensive field work, with the ethnographer spending months and sometimes years living intimately with the people being studied, observing and participating in the ordinary routines of life. A special aspect of this kind of field work is that the anthropologist *learns* a significant part of the culture – an anthropologist knows he or she understands a kinship system, for example, when he or she can classify kin and anticipate what kin will do the same way a native of the culture can.

The result of field work was expected to be one or more lengthy monographs – *ethnographies* – which would describe in a series of chapters the technology and techniques of providing for material needs, the composition of the village or local group, the composition and roles of the family and extended kinship grouping, the organization of politics and leadership, as well as the nature of magic, religion, witchcraft, and other native systems of belief. For cultural and social anthropology, ethnography – published in books, monographs, and articles – is the basic data of the discipline.

This agenda remained in force as the dominant project in social and cultural anthropology until the 1950s. Central work on this agenda was done by British social anthropologists who completed a series of outstanding ethnographies which became the exemplars for the entire field. Australian aborigines, African pastoral and horticultural groups, Pacific Islanders, Burmese tribal peoples, all

[2] See Ernest Gellner's delightful paper "Zeno of Cracow *or* Revolution at Nemi *or* the Polish revenge: A Drama in Three Acts" (1987).

were described in meticulous detail and presented in a way that made the organization of these societies vividly apparent. In the thirties there was a shift from a "functional" to a more "structural" approach, that is, from an emphasis on how institutional activities related to individual and social needs to an emphasis on how such institutions were organized into an encompassing structure through the kinship system and political activity. Overall, however, there was considerable continuity from the functional to the structural periods; both emphasized the detailed description of institutional forms of activity which made society possible. An excellent account of the development of this new agenda, which had become a full-fledged scientific paradigm by the late 1930s, can be found in Adam Kuper's *Anthropology and Anthropologists: The Modern British School*.

For those who have never read an ethnography, the account given above gives little sense of the real accomplishment of this work. Today, the standard ethnographies written for undergraduate classes follow almost exactly the course laid out by these pioneers (for example, Napoleon Chagnon's *Yanomamö* or Richard Lee's *Dobe!Kung*). Good ethnography has the ability to immerse one in a strange and different world, which, while exotic, nevertheless is comprehensible.

While the central core of the classic ethnographic paradigm was developed by British social anthropologists, the Americans constructed a number of variations around this core. The Americans did not abandon the historical agenda as completely as the British. One American school, lead by Leslie White and Elmer Service, emphasized the process of social evolution. The social evolutionists argued that human societies have evolved from band-based hunting and gathering societies to simple tribal forms of organization, and then to more and more powerful chiefdoms, and eventually to the development of the state. The "motor" for evolutionary advancement to more complex forms of organization has been generally thought to be technology and economy, especially the technical means by which energy is captured and put to human use. This school is still flourishing, and its practitioners have made common cause with archaeologists to build a sound "non-conjectural" prehistory of human societies.[3]

Another American variant within the ethnographic agenda was the *culture and personality* school. The distinctive characteristic of this school was an emphasis on the way in which socialization practices shape the personality of the members of a society, making them more likely as adults to behave in certain distinctive ways and more likely to adopt certain cultural institutions. The culture and personality school was split into two camps. One camp, lead by Ruth Benedict and Margaret Mead, emphasized the way in which each society is marked by a particular *ethos* – a common emotional and characterological way of responding to the world which could be seen throughout the

[3] See Johnson and Earle 1987.

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[4] See Benedict 1934 and Mead 1950.
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range of cultural activities performed by members of a society.[4] According to Benedict and Mead culture and personality are basically the same thing; culture can be seen as group personality "writ large."[5] The members of this camp, most of whom were students of Boas, used as evidence detailed ethnographic materials to show that a particular culture is infused with a particular emotional ethos.

The other camp, initiated by Abraham Kardiner, a psychoanalyst, and Ralph Linton, an anthropologist, took as their task working out the ways in which particular child rearing practices give rise to particular personality problems which are then expressed in specific cultural activities and beliefs.[6] This group relied more on the analysis of comparative and cross-cultural data and has been more specific about the psychological mechanisms by which socialization practices are linked to cultural activities and beliefs than the Mead and Benedict camp. John and Beatrice Whiting's *Children of Six Cultures* is a good exemplar of this field. A good historical review of the entire culture and personality field can be found in Phillip Bock's *Continuities in Psychological Anthropology*.

While each of these schools had a different explanations of how social life was organized, these were differences *within* a general paradigm. Overall, the task was agreed upon – *to find out how institutionalized systems of action are organized*. The means to carry out the task was also agreed upon – intensive ethnographic research. While Malinowski held that much of the organization was based on the satisfaction of human needs, and Radcliffe-Brown held that much of the organization was based on the requirements of the functioning of society, and Mead and Kardiner held that much of the organization was based on personality as formed by early experience, and White and Service held that much of the organization was formed by the means and modes of production, these differences were primarily matters of emphasis.

Each of these schools agreed on the centrality of kinship and face-to-face relationships in understanding "primitive" society. Even more basically, they agreed – without needing to say it – that *the basic unit for scientific analysis consisted of learned and prescribed systems of action*, variously called "customs," or "traits," or "institutions." By the 40s George Murdock and his collaborators had developed an *Outline of Cultural Materials* containing a listing of over 500 categories of institutions classified under eighty-eight general headings like "agriculture," "family," "religious practices," etc.

By the early 1950s, this kind of ethnography had become "normal science." A good social or cultural anthropology graduate student could be expected to return from a year's field work with a solid description of the institutions which comprised the technology, economy, kinship, politics, religion, and magical practices of the people studied, and could be expected to put these facts together

[4] See Benedict 1934 and Mead 1950.
[5] See LeVine 1973 for a devastating methodological critique of this assumption.
[6] Kardiner and Linton 1949.

into an argument about how these facts were organized by functional or structural or economic or personality factors. From 1920 to 1950 excellent ethnographies were written on a large number of societies spanning the entire world. Murdock's *Social Structure*, a cross-cultural study published in 1949, lists 250 societies on which information was complete enough to determine the major social institutions. By 1957 Murdock's "World Ethnographic Sample" included 564 societies, and probably included less than a quarter of the societies on which a reasonable amount of ethnographic material had been published.[7]

Success, however, has its cost. As more and more ethnographies appeared, the value of each new one decreased. As more and more facts became known, the idea that any one school would be able demonstrate that its central concept was truly *the* primary organizing factor became more and more unlikely. It was not that the questions were settled and nothing new remained to be discovered. It was just that adding anything really *new* had become increasing difficult. The time for agenda hopping had again arrived.

A new agenda in anthropology and the great paradigm shift

As the agenda for the study of the organization of social institutions was reaching a point of exhaustion, a genuine scientific revolution was taking place in psychology and related fields. By the 1920s *behaviorism* had replaced "introspectionism" as the dominant paradigm in psychology. The basic assumption of the behaviorist paradigm was that psychology should be the study of the *observable*. One can observe the responses and the stimuli which impinge on an animal; therefore the task of psychology is to work out how *stimuli* are lawfully related to *responses*. "S–R" or "stimulus–response" theory retained its hegemony for more than thirty years. A basic principle of the behaviorists was that no theoretical construct about what goes on "inside the mind" should be introduced into psychological theory unless it could be *tightly* connected to measurable external events. Thus behaviorists could talk about "drive" as a state of the "organism" because it could be measured directly from external events – typically the number of hours of deprivation (of food, water, exercise, etc.) the animal had experienced. Similarly, one could talk about the "reinforcing" or rewarding properties of certain stimuli because reinforcement simply referred to the fact that an animal was more likely to perform a response if it was immediately followed by the presentation of a certain class of stimuli. A pellet of food, presented to a rat after pressing a lever, when the rat had not eaten in twenty-four hours, was a reinforcement because the rat was now more likely to press the lever than it had been before the pellet was presented.

[7] Today it would probably be possible to give an account of the major economic, social, political, and cultural institutions of over 2,000 societies based on professional ethnographic descriptions. *The Encyclopedia of World Cultures*, edited by David Levinson, for example, presents summaries of the cultural institutions of more than 1,500 societies.

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While all the behaviorists agreed on the necessity for this kind of tight connection between observables and theoretical terms, there was a split between the purists, such as B. F. Skinner, the eminent psychologist at Harvard, and those willing to use "hypothetical constructs" such as Clark Hull, the eminent psychologist at Yale. According to Skinner, even constructs like "drive" were unnecessary; all that one should talk about was number of hours of deprivation. Nothing was gained, according to Skinner, by postulating states of the organism one could not *observe* directly. Those with less pure inclinations, however, maintained that the use of *hypothetical constructs*, such as "drive," "habit," "frustration," "anxiety," "expectancy," etc., properly tied tightly to observable events, allowed the development of a more powerful theory.

Although behaviorism had begun as a healthy corrective for the subjectivism of the introspectionist schools of psychology, by the fifties a large number of anomalies had made the tight constraints of the behaviorists seem like an intellectual prison. Tolman, an experimental psychologist at Berkeley, had performed careful experiments with rats that demonstrated beyond any reasonable degree of doubt that even as simple a creature as the common rat had in its mind a complex *map* of its environment which it could use to make decisions, and that this map could not be reduced to some complex of S–R connections. Jean Piaget, in Switzerland, had been doing interesting work on the intellectual development of young children, showing that as children develop they construct more and more complex models of the world around them. Jerome Bruner, a cognitive psychologist at Harvard, had shown how college students use a variety of strategies in concept attainment tasks.[8] None of these findings could be easily assimilated to the behaviorist paradigm.

In 1957, when I went to graduate school, the arguments on both sides of the behaviorist dispute were well known. The counter argument of the behaviorists to the growing list of anomalous findings was that while it was true that there were lots of things one could not account for with the behaviorist paradigm *at this point*, nevertheless the thing a good scientist should do is stick with what can be *really* understood, and gradually work one's way to the more complex phenomena. Various findings might seem anomalous now, but would most probably prove to be nothing more than some special combination of simple S–R processes *when the truth was known*.

Despite such arguments, the behaviorist paradigm collapsed quickly. In my view, a major factor in the rapidity of the death of the behaviorist paradigm was the influence of the modern digital computer. In the late 1950s computers were becoming a major part of the university scene. With the development of higher level programing languages it became possible to write computer programs ranging from simple statistical analyses to programs capable of playing checkers and chess. These game playing programs had memory, could plan ahead,

[8] See Bruner, Goodnow, and Austin 1956.

and could even be constructed to make good guesses about what would happen in situations too complex to calculate exact answers.

Since computers gave a mechanical example of how a mind *could* work, they began to serve as a model for how the human mind *did* work. By 1958 Newell, Shaw, and Simon had extended what had been learned about solving problems with computers to human problem solving.[9] George Miller, one of the pioneer psychologists in the cognitive revolution, said in an interview about this period of time:

> My thinking was influenced by computers perhaps earlier than most people's. Even while I felt that I should be behavioristic, I was willing to play around with other ideas. The generation before me felt that *you couldn't use a term without having a physical instantiation of it*. And on that criterion, *we now have physical instantiation, by means of computers, of fabulous things!* Things that they had never dreamed of. So, just accepting that as your license to talk . . . you could talk about memory, syntactic rules, plans, schemata, and the like. We didn't believe that computers were giant brains, but *we could see the similarities*. (Italics added) (Baars 1986:205)

The cognitive revolution was not limited to psychology. Prior to 1956 most theory in linguistics was also behavioristic in approach. The ideal was one in which the linguist transcribed into a phonetic alphabet the speech of a competent speaker of some language, and once transcribed, analyzed these written symbols in a relatively mechanical, algorithmic way, with minimal reference to meanings, to discover the various levels of structure of the language. Little or nothing was assumed about the mind, or about anything psychological. Noam Chomsky's *Syntactic Structures*, published in 1957, changed all this. This book had an enormous impact on both the field of linguistics and the field of psychology. What Chomsky was able to demonstrate was that *one could not learn a language like English by just learning what words can follow other words*. As George Miller put it in a personal interview:

> As I thought about Chomsky's arguments, it occurred to me that if you try to learn English using purely statistical approximations to English – by learning transitional probabilities between words – then when you look at the size of the set of sentences 20 words long, it turns out that you have to learn an astronomical number of connections in order to generate just exactly the set of English sentences and no others. I think it works out that the average number of possible transitions following any word in a sentence is on the order of 10 – that is, at any point in a sentence there is an average of about 10 words that can follow that word. So, in sentences about 20 words long – which is not very long, that's about the average length of sentences in the *Reader's Digest* – that would lead to 10 to the 20th power number of sentences. And there are less than 10 to the 10th seconds in a century.
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> So if you imagine that you have been learning one transitional probability per second since your were born, you would not have had enough time to learn more than a tiny fraction of all the sentences you can in fact produce and understand. (Baars 1986:208)

[9] See Newell, Shaw, and Simon 1958.

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In summary, *to be able to speak a language a speaker must learn a grammar – a relatively small set of rules that will generate all and only those sentences which are grammatical in that language*. This grammar is a mental object. In *Syntactic Structures* Chomsky presented a rigorous formal proof that such a grammar could not be described or accounted for in stimulus–response terms.

The paradigm shift from behaviorism to cognition has been widely commented on and written about. Howard Gardner's *The Minds New Science* is an accessible and wide-ranging account of the development of the cognitive sciences, covering cognitive developments in psychology, artificial intelligence, linguistics, anthropology, and neuroscience. Bernard Baars' *The Cognitive Revolution in Psychology* gives a good description of the behaviorist paradigm and includes a series of interviews with major figures in psychology in both camps. If there is any controversy about the cognitive revolution, it is not about whether such a shift occurred, or who was involved, or what the change in the general conceptual framework consisted of, *but whether or not to call what happened a true paradigm shift*.

The issue here, of course, is how the word "paradigm" is to be defined. According to Kuhn, one does not have real paradigms in the social sciences because the examples, research strategies, instruments, etc., which provide the models from which a tradition springs are not *completely and firmly shared* – as they are in the physical and natural sciences. Speaking of "pre-paradigmatic" science, Kuhn says:

> fundamental disagreements characterized, for example, the study of motion before Aristotle and of statics before Archimedes, the study of heat before Black, of chemistry before Boyle and Boerhaave, and of historical geology before Hutton. In parts of biology – the study of heredity, for example – the first universally received paradigms are still more recent: *and it remains an open question what parts of social science have yet acquired such paradigms at all*. History suggests that the road to *a firm research consensus* is extraordinarily arduous. (Italics added) (1970:15)

So, from the Kuhnian viewpoint, what I have been calling "paradigms" in the history of anthropology and psychology should really be called something else – perhaps "pre-paradigmatic traditions" or "quasi-paradigms." However, what we find in psychology or anthropology also consists of "accepted examples of scientific practice" which "provide models from which spring particular coherent traditions of scientific research." What is different about them is that *the paradigms in the social sciences are less widely accepted than the paradigms in the physical and natural sciences*. This is easy to understand – the physical and natural science paradigms are *better* than the paradigms in psychology and anthropology. That is, the paradigms in physics, chemistry, and biology fit a broader range of facts, are more precisely stated, and give more effective predictions than the paradigms in the social sciences and psychology. The high degree of sharing and firmness of commitment of the physical scientists to their

conceptual schemes, exemplars, research strategies, etc., is not due to anything special about the intrinsic character of their conceptual schemes, exemplars, and research strategies as such, but to their superiority in explaining the world. In my view it would be a mistake to make a *high degree of sharing* the criterion by which one decides what is or is not a paradigm. It is like refusing to call someone a "runner" unless they win a race.[10] But in any case, whatever we call it, the revolution occurred.

The cognitive revolution was more indirect in its effect on anthropology than in its effect on psychology and linguistics. Anthropology had been less behavioristic in its orientation than psychology and linguistics, and so the revolutionaries had less to change. However, anthropology had arrived at a point where the dominant agenda was reaching exhaustion. In fact, by the time the cognitive revolution hit psychology, anthropology had already begun to move towards more ideational, mental, and cognitive concerns – the study of ideas, beliefs, values, and cosmologies. In 1955, J. Beattie, a respected British social anthropologist, wrote a paper titled "Contemporary Trends in British Social Anthropology." In this paper he says:

> Evans-Pritchard suggests that *the full understanding of human societies requires that they be studied as moral or symbolic systems*, not simply as "natural" systems . . . This general broadening of theoretical approach . . . has led to a marked advance in the study of beliefs and ideas . . . Recent studies, therefore, have undertaken the study of *systems of ideas and beliefs* not exclusively from the functional point of view, but also *as systems in their own right*. (Italics added) (1955:12)

The shift from the study of institutional behavior – "natural systems" – to the ethnographic study of "idea systems" or "symbolic systems" appears to have been a very general trend. George Mandler has suggested that a major cause of this shift was the tremendous expansion of the importance of communication and information technology throughout industrial societies by mid-century.[11] The dependence of society on the growth, organization, and retrieval of information was becoming clearly apparent in the development of telephone, radio, television, phonograph, and film industries. The computer was not the cause of the cognitive revolution in psychology, but rather the new piece of technology that symbolized in physical form the power of information manipulation.

This trend in the social sciences towards the study of idea systems occurred in Britain, France, and the United States, although each country had its own particular direction. In the United States Clyde Kluckhohn had moved from the functionalist perspective found in his 1949 classic work on *Navaho Witchcraft*

[10] In the second edition of *The Structure of Scientific Revolutions*, Kuhn explicitly recognizes this distinction between "cognitive content" as paradigm versus the "consensus of the scientific community" as paradigm, a distinction he admits is conflated in the first edition, but which he does not completely renounce in the second edition. See pp.174-190. [11] Personal communication.

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   a. Knowledge is power.
   b. The quest is for harmony.
   c. Harmony can be restored by or
   d. One price of disorder, in huma
2. The universe tends to be personal
3. The universe is full of dangers.
4. Evil and good are complementary
5. Morality is conceived in tradition
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to the study of Navaho concepts and values. One of his first publications in this area was an attempt to lay out the basic philosophic ideas of the Navahos. Below is Kluckhohn's summarization of the Navaho concept of the universe:

1. The universe is orderly: all events are caused and interrelated.
   a. Knowledge is power.
   b. The quest is for harmony.
   c. Harmony can be restored by orderly procedures.
   d. One price of disorder, in human terms, is illness.
2. The universe tends to be personalized.
3. The universe is full of dangers.
4. Evil and good are complementary, and both are ever present.
5. Morality is conceived in traditionalistic and situational terms rather than in terms of abstract absolutes.
6. Human relations are premised upon familistic individualism. (1949b)

Despite Kluckhohn's great knowledge of Navaho culture, these results are not impressive. The propositions are too abstract, too general, almost disembodied. Many tribal groups could be described in these terms. It is unclear to what degree the selection of these particular propositions depended on Kluckhohn's special interests. Kluckhohn's pioneering attempt to state Navaho philosophic assumptions did not appear to be the best way to get at cultural "idea systems'.

Shifting from very general "conceptions of the universe" Kluckhohn turned to the study of *values*. He defined "values" as "conceptions of the desirable" – that is, a special class of socially shared *ideas* about what is "good". In the early fifties he organized a project in the American Southwest in which the cultural values of five different societies – Zuni, Navaho, Morman, Texan, and Spanish American – were to be analyzed and related to ethnographic descriptions of social, economic, and religious institutions in each of these societies. This work aroused considerable interest in anthropology and the social sciences in general.[12] However, the results were generally agreed to be disappointing.[13] The major problem seems to have involved the *identification* of values. If a universal classification system was used, like Florence Kluckhohn's universal framework for the analysis of values, specific cultural values were left undescribed and unanalyzed.[14] But no procedures had been developed to determine specific cultural values. By the late fifties Clyde Kluckhohn had lost interest in the study of values and shifted his interest to new work in communication and linguistic anthropology.

There were other examples of this new tendency to change the "ethnographic object" – that which is to be described – in the direction of idea systems. Meyer Fortes, an outstanding British social anthropologist, wrote a monograph titled

[12] For a summary of this work see Vogt and Albert 1966.
[13] A good review of Clyde Kluckhohn's work on values is Edmunson 1973.
[14] See Kluckhohn and Strodtbeck 1961.

Oedipus and Job in West African Religion, which was first published in 1959. It gave an outline of religious concepts which define the relationship of the individual to Tallensi society in great ethnographic detail. Robin Horton, in an introductory essay to a second publication of *Oedipus and Job*, argues that Fortes was attempting to look at a religious system as a kind of folk "social psychology" in which supernatural forces can be understood as internalized psychological forces.

Another more controversial example is Edward Banfield's *The Moral Basis of a Backward Society*, published in 1958. Banfield gives a brief ethnographic description of Montegrano, a small and then desperately poor town in the province of Potenza in southern Italy. As a political scientist, Banfield begins by pointing to the lack of any effective local political organization in Montegrano along with the lack of any sense of shared community for which people are willing to make some sacrifice. His book is an attempt to answer the question "what accounts for the political incapacity of the village?" The answer Banfield found was that the usual explanations of poverty, a history of oppression, and class antagonisms were insufficient – that basically the resources for political action were present, but stopped by a *value system* of "amoral familism." The idea that it was a set of ideas, rather than structural or material conditions, that prevented social progress was intensely debated. Roy Miller, who did a study of "amoral familism" in a village near Montegrano, cites an "almost acrimonious exchange" between Banfield and Robert Redfield, an eminent American anthropology, during a seminar at the University of Chicago (1974). Given Redfield's legendary good humor and kindliness, the thesis that a bad life might be due to bad values must have provoked Redfield greatly.

Perhaps the most challenging new agenda in anthropology in the late fifties was an amalgam of ethnoscience with linguistic anthropology. Ethnoscience had long been a minor sub-field of ethnography concerned with the study of what native peoples knew about biology, zoology, astronomy, and related topics. This interest had always been a part of Boazian ethnography, which had from its inception been concerned with the rich variety of cultural knowledge to be found in any society. In the Yale department of anthropology, influenced by George Murdock, a strong program in ethnography had developed. The anthropologists trained at Yale had been concerned to raise the standards of ethnography to the kind of precision that linguists were able to obtain in their descriptions of native languages. In this endeavor they were supported and inspired by Floyd Lounsbury, an anthropological linguist at Yale who had worked with American Indian languages and was specially interested in kinship systems.

Lounsbury and Ward Goodenough published a pair of papers in 1956 on the semantic analysis of kinship terms. Lounsbury analyzed an American Indian kin term system, Pawnee, which had a complex kind of generational skewing (to be described later), while Goodenough analyzed a kin term system from

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To understand why these papers by Lounsbury and Goodenough had the effect they had, and understand how these papers came to form the basis of the early theories of cognitive anthropology, we must leave our historical overflight and enter into the more detailed world of ethnographic description.