A Conversation on Cognitive Anthropology with Charles O. Frake
at the Laboratory of Comparative Human Cognition
(16 June 1977)

Mike and Jean,

This text has gone from talk to a messy transcript, then through a buggy MSW version, and penultimately back to a plain text (which erased a few layers of multi-color Track Changes now incorporated invisibly into the text). With dozens of still really small changes added on, I think we are getting closer to a final copy: informal but readable, prescient but not precious. I have tried to alter only fluency bumps (sentence initial ticks, hedges, many some of phrases, a little repetition, etc.) and to leave all content alone. Where I have found the transcript unclear or in need of comment from either of you, the text is in red or green. I might occasionally have changed too much. Keep your eyes open for mistakes and things you might want to say more precisely in your 1977 voice.

Interlocutors:

Michael Cole, John Dore, Jean Lave, David Roth, Sylvia Scribner

Cole: We want to ask Chuck about what people were doing in the 1950s and subsequently that has come to be called cognitive anthropology in the context of our own concerns about how you should talk about cultural differences in thinking.

Frake: Let me go back to where anthropology was in the mid-fifties, where at least for me, most of this started. You realize when you ask a native to give a history of his own group—that's what we call ethnohistory in anthropology—there are some principles for what happens. First, people who tell ethnohistories try to make their own group look good. Second, you can't replicate ethnohistories. If you have another person from my group, let's say Roy D'Andrade, doing the same thing, he would give a different account. That doesn't mean we're lying or can't see what's really there. It means there really isn't anything out there called cognitive anthropology that one can find and bring in here to describe to you. It's a matter of interpreting different people doing different things. Cognitive anthropology is one of several labels that have been used for various purposes by various people, so I could talk about how that label has been used, a sort of a sociolinguistic account of that, but not today. What I would rather do is to recapture issues we were facing at that time and to give a sense of how the areas I've worked on got going and have come to be known as cognitive anthropology.

Of course you realize that anthropologists, in general, concern themselves with what they call culture, rather than, let's say, individual behavior or abilities. This means we can't define culture in a laboratory. As part of our ideology, we have to go out in the world and see people behaving and doing things as a manifestation of their culture, and it's their culture we're trying to describe.

In the mid-fifties, there were two major streams of anthropology in this country. One was called social or social structural anthropology. It was heavily influenced by British anthropology and Chicago was the heartland of that tradition with people like Fred Eggan and Robert Redfield. The other was the culture and personality tradition, and Harvard was the center of that kind of work. I was at Yale where we were left out of both traditions, but what we had that they didn't have was linguistics. Yale at that time was the center of American structural linguistics. For many of us in anthropology, linguistics was the most exciting thing going on intellectually at Yale. Psychology at Yale at that time didn't seem interesting; even people in culture and personality didn't look to Yale psychology.

So a number of us, mostly students, got interested in linguistics. We looked upon linguistics as a model for the things we should be doing. First of all, linguistics took as its task the description of languages and took as a serious theoretical task the job of description. We thought that ethnography — the job of going out and describing cultures — ought to be taken as a serious theoretical task in its own right. Ethnography was more than just a matter of collecting data which then a theoretician could assemble and put in 2x2 tables and make generalizations, which was a tradition of work actually associated with Yale at that time, often called cross-cultural work. George Peter Murdock developed files at Yale, the Human Relations Area Files, which I'm sure you heard of, where you tabulated all the data under categories for all the cultures on record. If you wanted to find out if, say, kinship terminology went with descent, you looked up all the societies in the world and figured out which ones had various systems of descent tied to various systems of kinship terminology. Then you reached the conclusions given by the correlations.

We were reacting strongly against that easy comparative tradition. We preferred the argument coming out of linguistics, most importantly, that we can't categorize things in cultures *a priori*. You have to go out and describe each culture uniquely just as you have to describe each language. You can't impose the traditional categories of Latin grammar on new languages. You have to find out what their categories are. We were trying to use that argument for what you have to do in anthropology, an argument that makes ethnography more problematic than if you think you can just write down what the people are doing or even the things they have. You have to discover what they think they're doing.

That's how cognition became a focus. As soon as we started thinking that we had to look at the world from their point of view and find out what their categories are, the word cognitive crept in. This happened slowly because linguistics at Yale at that time was behaviorist in its ideology. They argued that you have to, for example, discover the sounds of a given language in that language, that you can't impose them beforehand. The idea was that various sounds were sort of out there to be heard, but you had to discover them. It wasn't thought of as some kind of conceptual end, in that you weren't doing anything psychological when you were doing linguistics. I think that's a fundamental misunderstanding of the early history of cognitive anthropology. It did not start out as a cognitive, mentalist enterprise at all. Some still associated with cognitive anthropology are still behavioristic in the way they talk about human behavior.

In fact, one of our dissatisfactions with linguistics at that time was that they weren't interested in meaning at all. Meaning was considered mentalistic. You had to talk about meaning without using words like concepts and thoughts. Linguists at that time thought that the job of the linguist was to discover the distinctive elements in the language and then describe the rules of their co-occurrence or distribution. A main problem in ethnographic fieldwork is to find out what people are talking about. There comes a time when a fieldworker has to confront meaning. So we began worrying about how to do semantics, how to find out about meaning in an objective, replicable, scientific way. People started doing that with easy things like kinship, at least they seemed easy at the time, where you can use the analogy with the way linguistics operates on sounds, and borrow the distinctive features of sounds and import such analyses into the study of kinship categories. Kinship is an old favorite subject in anthropology, a good domain to start out with. This has been a continuing tradition, and I think one of the great successes of the field has been that we've really made important theoretical advances in the study of how kinship systems work.

Several of us decided that if we can do it with kinship we can do it with other things as well. A number of people at Yale were interested in ecology, biological adaptations, and agricultural systems. Part of their concern came out of the argument that to do ethnography, you have to find out what people are doing; you can't start with a preconceived notion of what's important. You must try to account for what people are doing. So if you go to the field and people are continually involved with plants and talking about plants, you don't just say, "Shut up. I want to learn about your kinship system." Rather, you'd say, "Well, maybe plants are really important in their life," and you'd try to find out about it (Frake, 1961, 1962, 1964b).

Since many of us worked in places where plants were important, we got interested in folk biology. How do people classify and think about plants? Just what is their system of knowledge about the natural world? Again the argument is that you can't pick up a plant, get a scientific identification, and say that's what the plants are. You have to find out how the people classify and identify plants. That turns out to be much more complicated than dealing with kinship terms. There are many more plants, and plants have many more attributes. And you have the problem perception. You can't see a kinsman in the sense that there's nothing perceptual about an uncle, but there is about plants.

That was one strand of work that began at Yale, and ethnography was the flag we were waving. We started dealing with systems of folk biology, of ethnobiology, and Harold Conklin, who is one of the pioneers in this, started teaching a course where he wanted to talk about ethnobiology, ethnobotany, ethnozoology. He called that course ethnoscience, and I did the same thing when I started teaching at Harvard. The label got picked up as a general label for the field, and that's how the word ethnoscience crept in. We justified it by saying that what we really mean by science is knowledge, and ethnoscience is just the study of the native's knowledge. That was our *post hoc* explanation of the word.

In the meantime, at Harvard, there was a reaction, and you need someone like D'Andrade or Kim Romney to explain what was going on there, but I think a number of the students there were reacting against culture and personality work especially as it was applied by people like John Whiting in a cross-cultural statistical way. The way Murdock was doing with social structure at Yale, Whiting was doing with culture and personality work at Harvard. I think some of his students, like Romney and D'Andrade, reacted to that under the influence of the cognitive psychology that was going on at Harvard under Roger Brown and George Miller. There was an exciting atmosphere in terms of psychology at Harvard, unlike at Yale, where we didn't see anything going on. I went to Harvard after my experience at Yale and through these people got interested in cognitive psychology. It seemed to me that what I was doing when I worried about how people classify things had some relation to what others, like Jerome Bruner, were doing when they talked about concepts and categories, and attributes and features, and so on.

It seemed to me there was some connection there, even though there was a big difference: the psychologists were interested in how people form concepts and different ways of doing that, and they used artificial procedures to find out; in contrast, our concern was with how you can go into the world and find the concepts people have, and how they define them and distinguish one from another. Despite the differences in method, there seemed to be a sustained mutual interest tying cognitive anthropology and cognitive psychology.

Another thing that happened at Harvard at that time was that Harold Garfinkel was there on a post-doc.

Cole: I anticipated that when you defined ethnoscience as the study of cultural knowledge, since Garfinkel was going to get interested in method and he wanted to get member's definitions, the connection between ethnoscience and ethnomethodology seems pre-determined.

Frake: He was in a seminar with me in 1958-59. That's when I decided ethnoscience was a way of studying people's cognitive systems, a way of studying their knowledge about the natural world. Then I realized you could study religion with the same point of view. In fact, it is easier to study gods and other invisible things, because you don't have to worry about what they really look like. Anyway, ethnoscience just became the study of any kind of knowledge. Some psychologists—at least Bruner and Miller—were picking up the word at that time. Garfinkel got into that meaning of the word and decided that, since he was studying the methods that the natives use, he would call it ethnomethodology. He had an influence on a number of us at Harvard at that time.

People like Romney and D'Andrade were influenced by psychology and psychological paradigms in much the same way those of us at Yale were influenced by linguistics. They were more sympathetic to experimental methods and to worrying about individual differences in cognitive abilities and cognitive processes than the more linguistically inclined, who like me, were more concerned with trying to discover systems of knowledge rather than worrying about processes like memory and so on. The two strands came together a little when I went to Harvard. Then a group of us (Frake, D'Andrade, Romney) went to Stanford, and we had students who had both linguistic and psychological interests in various combinations.

One of the common links was a concern with methodology and that is one thing I got out of linguistics. At that time the notion was that linguistics had a really good methodology. It was objective, it was replicable, their data were out there, they could show you where they came from, and I think the psychological types felt that psychology also gave them some methodological tools, or at least an ideology of methodology, which anthropology lacked at that time. We used to say, "Well, anthropologists just write essays, and we need some kind of systematic life psychology where you can see what is going on."

I think if you look at some of my earlier papers (1962, 1964b), I talk about operational procedures. It was important to us at that time, but it got undermined a little bit in the Chomsky revolution. We had been appealing to the linguistic paradigms saying, "Oh! Look how great linguistics is, they can do all this, and they've solved all these problems." On the one hand, Chomsky really helped us out, because already at that time we maintained that we were studying people's knowledge. We were getting into people's heads in that sense. The Chomsky revolution in linguistics made mentalism respectable in linguistics and eventually led to linguists being interested in things like meaning, although not initially. Chomsky also vindicated our argument that description is a theoretically interesting task. He showed that you could get good theoretical ideas by worrying about how you describe something. If you've got a method for describing a language, then you've got a theory of language.

On the other hand, this appeal got pulled out from under us with Chomsky's argument that all languages in which a linguist might be interested are essentially alike. If one language is as good as another, you might as well work with English, which is accessible. This worked to the detriment of anthropological interests because, whatever the truth of the matter, anthropologists still have to go into the field and cope with different languages. No matter how alike languages may be if you go deep enough, they're still very different on the surface, and they are still just as hard to learn as they ever were.

This is hard to describe. In the field, language is noise and you've got to have some method of organizing that noise. I don't think there's any better method, frankly, than the old methods of structural linguistics as a practical method for facing that problem. So we still had to train people to cope with different languages, but linguists stopped doing that and stopped being interested in describing different languages. They stopped being interested in operational procedures, because Chomsky argued that it doesn't matter how you get your data or where it comes from as long as you can show that the final thing works. This may be a good enough argument, but it's not too useful when you're faced with the practical task of coping with different languages. You can't send anyone out to get any data they want and say, "If it works, then it'll be O.K." There are discovery procedures to learn about how to do it. So in that respect the transformational revolution was a setback. Students didn't get trained in linguistics the way they used to and still don't.

The other thing that some of us were trying to do was to go beyond worrying about the semantics of words. We wanted to look upon culture as like a grammar, but we used the metaphor that culture is an elaborate code that enables people to interpret what they are doing with each other. I was trying to find ways of watching people behave and to discover major chunks of their culture. Anthropologists in their papers and monographs usually have a section on religion and social structure and economics and politics. I wanted to figure out ways of getting at those major kinds of divisions, but without depending on predefined operational definitions of religion or economy. The strategy for doing this was to look at people behaving in particular situations and to find out how they define that situation inside a classification of kinds of situations. If you have a bunch of situations that are similar and a whole cultural enterprise producing those situations, and if the situations seem to be like religion, then you could label that set of things "religion" (Frake, 1964c). Your definition of religion is specific to that cultural system, and then you can compare "religion" among different cultures. That's how a number of us got interested in situations and contexts and trying to find ways of figuring out what people are doing. I guess now I am at the point where that is my major interest, and my major argument is that in order to understand what human beings are doing, you really need to have an understanding of what is real for them, what they are relating to in a given situation, and what it is they think they're doing.

Cole: I have a question. To pick up on the religion discussion, one of the things that gets said about cognitive anthropology that we have to be concerned with given our own focus on getting people's definitions of what they're doing so we can ask, "Well, how is what one person doing different from what another person is doing, or how is what one person doing on one occasion different from what they're doing on another occasion." It seems ethnographers have worked hard at getting, say, a definition of religion in people's own terms, including an account of how they organize what they're doing. But the second thing, the comparative step, doesn't seem to get done so much, so that you might have, in New York, somebody's activities that constitute religion for them, but the people who do that don't then get concerned with the problem of how that's different or the same as how others practice religion.

There seems to be a division, an ideologically correlated division of labor, so that those people who do the generalizing don't go to all the trouble of establishing indigenous terms for what the activity is, and those people who worry about description don't worry enough about generalizing.

Frake: I don't think that's really true. This argument is always raised against cognitive anthropology: that it's too relativistic. If you describe everything in your own terms, how on earth can you ever compare or generalize anything. If I define religion uniquely for this culture and someone does it somewhere else, then how do we know they are the same. My argument is that <u>you can't legislate comparability</u>. To define arbitrarily what religion is, like religion is to believe in supernaturals and I know what a supernatural is, and to go around the world and checking off people's beliefs in that way, I think it's the wrong procedure. <u>Comparability is something to be discovered.</u> Also, and I think this has been born out, if you do try to describe cultures in their own terms and then compare them, you find out that in fact they're comparable in many ways you hadn't expected. I think there really is something like religion. I think anthropological intuitions that they're doing the same thing when they go around the world describing something called religion is in some yet undefined way sound. I think there is something behind it. We have to really find out empirically what that is. I think universal is a big word now, and you can even say that some cognitive anthropologists are going overboard on that (again we're being influenced by Chomsky), but we're certainly finding a lot of things across cultures by describing each one in its own terms.

Color categories are an example. I don't know how far along you want to go with what Berlin and Kay (1969) did on the universality of color terms, but they argue it was only by using ethnoscience and cognitive anthropology that they could make universalist claims. It takes quite a bit of fieldwork for someone to discover these kinds of universals. Before Berlin and Kay, color was a classic example of cultural relativity, but there certainly is a regularity across cultures in how they conceptualize and label colors. Whether Berlin and Kay have gotten all the subtleties is arguable, but there certainly is something there. It's not just randomly arbitrary or completely relative (Conklin, 1955, 1973; Frake, 2007a).[i]

How does that work? Take a different domain, because my next dirty question was to move beyond color terminology, about which we know only a little, to the logic of kinship systems, since there has been much work there. We're interested partly in the steps of how it works, because you get something that looks complicated, with a lot of complicated relations you can point to. Then you get a second example, and that too is probably not enough; so maybe you need at least two more, and eventually you can show how they share more some features, that something about the system is more alike. Jean [Lave] has done kinship stuff like this, too.

Let me say something about kinship. It's a good example, because it would be acknowledged as relevant by all anthropologists. A.L. Kroeber, way back in the 1909, proposed the basic dimensions of all kinship systems much like the universal dimensions of phonology had been described by linguists. Although he was a leading anthropological linguist at the time, he didn't use linguistics at all when focused on kinship. He laid out a set of dimensions for kinship arguing that, because kinship systems are all combinations of using the universal dimensions, kinship was not of much sociological interest. It was a psychological phenomenon. That was Kroeber's argument. It was not a warranted conclusion from the data, but at least he laid out the dimensions. So the tradition of looking at kinship this way and defining kinship systems uniquely in cultures predates cognitive anthropology. It was one of our models for doing the same thing in other ways. But since that time, and work in kinship by people like Floyd Lounsbury (1956, 1964, 1965) have shown that not only are there certain universal dimensions that seem to be used, but there are certain rules that generate systems, and you can order kinship systems in terms of the number of rules they put in play. There are some rules that all systems have, and there are some that only a few have and so forth. You can make an implicational series much as Berlin and Kay made for color. The only difference is that we've come at the beginning of the kinship line so we don't consider the kinship series as evolutionary, but the color series comes at the end of an evolutionary sequence. The logic is the same. By this logic, there are merging rules, they can have constraints on them, and then finally there are skewing rules. If you have skewing rules, you have all the other rules, but only a few systems have skewing rules.

One thing that ought to be said is that the data that had been used for all of the work on formal analysis of kinship systems is interview data without regard for the actual conditions of use. Up until recently, all the claims have been focused on what is in individual heads, and the data are always clean: simple sets of one-to-one agreed upon associations between kin type and a kin-term. If you actually ask individuals for data and map their use of the kin terms onto the kin types of their relatives, you find much variation. It's not at all clean. It's messy stuff, and it's bothered me. I think it's been unclear to people.

One problem is that eliciting genealogy is a specific context that is not that common in some societies, although it is a really common event in others. Many have been trying to take things like kinship terms and see how they're actually used in social contexts as words. This is a much more interesting kind of enterprise eventually, but in the old kinship tradition, I think a real accomplishment was made. One secure accomplishment of anthropology is the description of how kinship concepts are defined in terms of an ideal focal type, by which: there's one uncle who everyone will agree is an uncle and there's no problem, and then there are others that are debatable. For many kinds of concepts, this seems the way they get defined. There are good members and not so good members of many ordinary categories of people used in everyday life. This is a fact they have to face up to. But you want to talk about something like law?

Cole: Yeah, I'm interested in that, how that goes. Partly that's interesting because people argue when they do law and arguments are something spoken and public.

Frake: Because law is mostly talk, one way to go at it is to look at it as part of the domain of different kinds of ways people talk to each other. Then you try to sort out in a given culture what's an argument and what's a discussion. Like now, we're not really arguing; we're discussing. We're not negotiating, because we're not trying to settle a basic issue. I think in most societies there are situations where you have two opposing parties trying to reach an agreement over a conflict, and I think in most societies, not all, there are formalized ways of handling situations when they really get tough—enough for an arbitrator to mediate the action. So we can look in a given society for the ways they organize such situations and what they see themselves doing.

Cole: But how do you do that? In a way it sounds like you start into that with something that you didn't pick up from the people you went to work with. You introduced notions like people have a conflict about something. Presumably, that's something you'd have to do. I'm not arguing you wouldn't find it, but it is a beginning place you might need.

Frake: Yeah, I have a notion that the people have conflicts and they fight, but I don't go into a society and look around for a conflict. I let them tell me when they're having a conflict. It may be that people throwing things at each other is a sign of friendship. I have that kind of misunderstanding in New York all the time, when I think people hate each other or are killing each other, and they actually love each other. You have to find out what people interpret. It's not so much asking people to tell me if it's a conflict or a legal case, but to have them interpret a given situation for each other. If two people are doing something, there are people looking on and asking, "Is this what you'd expect in this situation?" Do they say they're being angry, insulting, or whatever. You get people's interpretations of what's going on to find out what the conventional rules of that situation are.

Of course, one of the things people are always doing is violating conventions. That's one of the strategies of ordinary everyday behavior. The way to be funny is to do something a little bit unexpected, not too much, or else you'll be crazy. But in every society people try to be funny. People express anger and hostility in subtle ways. They do it against the background of what you'd ordinarily expect given that definition of the situation. So the way to find out what the definition of the situation is not just to watch what people are doing. Because they're usually doing something just a little different than what their expectations are, you have to find out what their interpretations are.

Cole: We have read some of the work of Naomi Quinn, who has worked on legal stuff, and others who are doing what are called decision models. What is that as an enterprise as different from something that you would do?

Frake: That's really not my field, the decision thing. I'm interested in it, but somehow, compared to the linguistic model I appeal to, the decision model seems off the mark, at least for what I'm interested in. Decision models of behavior try to figure out why a person decided to do what he's doing. This seems to me less interesting than knowing how, after people do something, to account for other people's interpretations of what has just happened. To generate a decision model that leads you to account for why I'm choosing the particular words I'm using right now, would be interesting if it could be done, but I'm more interested in understanding how you're interpreting what I'm saying as serious, provocative, dumb, funny, or whatever.

Cole: You're saying, if it could be done, implies that it's harder to do than what you're doing.

Frake: Yeah, I think it is. Whether they're succeeding in doing what they think they're doing, I don't know.

Cole: Why do you doubt it?

Frake: You'll have to get one of them to explain what they think, whether they think they're really getting actual decision making processes in the head, or getting more at the rules of how people account for what they have done. Hugh Gladwin (1975) and Christina Gladwin (1975) have offered decision models of how Kumasi fish-sellers decide to go to one fish market or another. Does that model account for which market the people decide to go to, or does it account only for the way people account for where they go. Someone always asks, "Why did you go to that market?" Do people give their real reasons, or do they just present arguments to make it seem reasonable that they went where they did. Decision modelers talk to people; this is how they get decision models. Rather than modeling what is going on in individual heads, they're really getting people's accounts of what, given certain circumstances, would be a reasonable thing to do. I think this is an interesting way to think about what they do.

Lave: I think there is a third alternative for what they are doing. They're really trying to account for the distribution of outcomes, for how many folks show up at the market. The notion is that if you understood the decision process you'd understand why they did what they did. I think that's another way of talking about the same thing: that the decision model stuff is the reaction against formal analysis, against a deterministic set of rules for a whole culture. Decision theorists were beginning to feel that life was more complicated than just following rules, and they wanted to represent the complexity somehow. One way to do that was to keep the formality of the models, but to account formally for the variation. Then you say, "Variation in what?" and my feeling is that regardless of whether decision theorists think they have described a particular version of taking account, whether or not they're trying to account for variation in people's heads, what they're actually doing is trying to account for variation in the distribution of particular kinds of houses, or fish sellers, in one market rather than another, etc.

Frake: They certainly have the notion that the test of the stated decision rules is whether they can in fact show that people distribute the way the rules would predict - like living in the houses that the rules would predict.

Cole: From a psychological perspective, that's a strange way to talk about being in people's heads.

Cole:
I was thinking about statistical models of electrons bouncing around. If you were an electron and you had a strong theory that every one of the elementary things of matter were like you were, you could generate your hypothesis by predicting what was happening on the basis of your intuition about where you would go if you were an electron. You might develop a model that did a pretty good job of distributing stuff. We have a belief that that would be bologna, that somehow, however the electron generated the hypothesis, it brought its intuition into line with where the other electrons went. It's the question of how seriously you want to take reality, whether or not you want to treat the outcome of something like a residence model or a fish-sellers model as really strong evidence of what individuals are doing to generate decisions, or whether, wherever the ideas came from, that's a good description of outcomes given specifiable circumstances. When there's black box psychology and you take the cognitive position????, then your statements got to be inside very seriously. So the conservative way to treat

this is that it has nothing to do, not necessarily, with what is in the head. I mean it may have to do with attributions of things in people's heads, but what it's really doing is organizing a distribution of behavior. You could do that in a behavioristic way, without really losing anything. Now, what then would you want to do additionally to make it seems like you were forced to be more psychological? I don't think that psychologists are all that psychological either. A lot of psychology is exactly the same.

Frake: Of course, my argument would be that the distribution—let's take the residence example where you have a decision model that predicts who's going to live in what kind of house according to what kind of rule—the actual accounting for the distribution on the ground per se to me is not as interesting as accounting for the interpretations of the people involved. Who are the guys who are following the rules and who are the guys who aren't? You can have maybe 50% of the people breaking the rule and considered weird, deviant, crazy or something. That's what I want to know, not just the axial distribution of things that may be important for some investigations. What makes the enterprise cognitive is the argument that we have to find the native's own perspectives on what they're doing.

Cole: What makes that more cognitive?

Frake: We're using that label as a reaction against people who say you can just go out there and look. Cognitive for us is sort of a metaphor, symbol, or flag to indicate that you have to find out things from the natives. The stuff isn't out there to see. You have to ask people about it. That's different from what you mean by cognitive. That's our battle. Let me give you a good example of this to show what you can get from ethnography.

There's a nice study of two little villages in the Italian Alps by John Cole and Eric Wolf (1974). The two villages are in the Tyrol. Before WWI it was part of Austria and after WWI this little section went to Italy. One village speaks the dialect of Tyrol German and the other speaks a Romance language, Romanche. Since they became part of Italy, the German speaking group—I call them German speaking even though what they speak is quite different from standard German—they've identified themselves very strongly as German. They used standard German as the language in schools, and they were strong supporters of Germany in WWII. The other village, even though they don't speak Italian—they speak another Romance language spoken in the Alps—nevertheless identify themselves as Italian. They've become strongly Italian.

There are two great pictures in the book that illustrates this. The towns in the Tyrol have volunteer fire departments, and they have a men's organization that organizes dances, parties, parades, and marches. One picture shows a volunteer fire department in the German-speaking town, the other shows the Italian equivalent. The German parade displays an explicit order with everyone marching as if to the same beat. The Italian parade is more of a meander with most of the participants looking around; the line functions as only a suggestion, a mere remnant of what everyone might attend to at some point. They are acting out the stereotype of what they should be. So they conceive of themselves as different kinds of people. On the other hand, they are living in an ecologically constraining and demanding environment. In the Alps, there are just a few things to do to make a living. Basically, it's farming, and of course one of the problems in any kind of farming community where there is limited land is how to divide up the land. Are all the sons going to get it or only one son?

Well, it's part of the German tradition throughout the Alps that only one son gets the land and the other sons are just left out. They either work almost like servants for the eldest son, or they go off fighting in the war or something. The German ideal is that land is not divided. The estate must stay together. The Italian ideal is that all the kids get something. They're all equal and everyone gets something. So they divide up the estate; that's their rule. But because of the ecological situation, if each group follows its own rule all the time, the Italians would be cultivating postage stamps and most of the Germans would be starving. So both groups violate the rule about 50% of the time. This means that they are both doing exactly the same thing.

An objective observer who didn't listen to anyone would just look at what they were doing, and he'd say they were doing the same thing. But both groups managed to make doing the same thing somehow different. They managed to make themselves out as doing exactly the opposite kind of thing. Because they have opposite rules, they can interpret a given instance of doing the same thing as something different. Let's say they are dividing up an estate. The Italians say, "Great, that's the way it should be," and the Germans say, "Given this situation, what else can we do." The whole thing can be reversed as well. So there are some anti-cognitive wings in anthropology that want to say, "O.K. that just shows that you shouldn't listen to informants. No one knows what people are really doing. These people are really doing the same thing, and it's ecologically determined, and that's all you've got to know." Marvin Harris might come to mind. What I'm interested in is how human beings can construe and interpret doing things as fundamentally different. They make boundaries and say they're there. They can say we're one kind of people and they're another kind of people. They have that rule and we have this rule. That's for me an example of what we mean by going into the informant's head and finding out what they think they're doing. You have to know their interpretations of what they're doing.

Cole: That's a super duper example.

Frake: I'm exaggerating a little bit. Don't go read the book now—just take my word.

Cole: It seems to me that the argument for asking for people's interpretations can be justified by the range of phenomena that can be encompassed in what you can say as a result. While it's true if you only examine the distribution of land holdings (let's assume it's true), then asking people what they think about it may not be useful at all. In fact, it may mislead you, if that was all that you paid attention to. But if you wanted to account for that and for the different languages spoken, and for the way they walk, and just look at the land holdings, it would give you no explanation whatsoever for why Italians walk around like Italians and Germans walk around like Germans. It seems you don't want to claim that either of these things is all of cultural reality. If you just went and asked people and never looked at what they were doing, then when you looked at what they were doing, you'd be flabbergasted and vice versa. There is no exclusion rule.

John Dore?? Or Sylvia??? Ray votes JD: It seems to me that what you're trying to do is find a really accurate way to categorize a culture, to get in and elicit a people's belief systems, and to describe how that type of culture looks, but I don't hear you really making claims about the way people's brains operate, about what actual cognitive processes are going on other than to say that you need to hear how people are talking.

Frake: I'm certainly not going to make that claim to this audience. I don't really know what cognitive processes are and that's the kind of claim I'd make in this audience.

Scribner: Is there another story like the Cole and Wolf book that would be a good illustration of a piece of work in anthropology that gets close to psychology or to what's going on in people's heads? Is there one piece of research that you like?

Roth: I have a nosey question about methodology. It seems to me that your methodology has changed from early ethnoscience to your more recent papers, where, rather than looking at rules that people apply in an action, you are looking more at how people negotiate the rules. You don't look at what's going on in people's heads, but how they objectively interact. Because you mentioned Garfinkel, and because some ethnomethodologists—I'm talking about D. Lawrence Wieder (1970)—have directly critiqued description in ethnoscience as logically incomplete, did that have any impact on your work? Am I correct you've been under the influence of this critique?

Frake: Not so much because of his critique. I have certainly been influenced by writings in ethnomethodology all along, from the beginning. My idea of methodology has not really changed the way I work as much as it has altered my hopes for trying to have–and to explain–some sort of really rigorous methodology. I have always been interested in methods by which I can say, "Well, here are my data and here's what I did with them." I think there's much misunderstanding, partly my fault in some of my earlier methodological articles, that seemed to say, "Here's a set of methods that you can take out in the field and plug in and get an ethnography." They weren't meant so much as cookbooks. There are always more ways to go. However you get your data, in ethnography, it's just got to be loose. There is no easy way to do it. I never want to tell people how to get their data as much as I want to say, "Here are some ways of organizing data so they can be tested." This is what you do when you do ethnography. You get ideas and data coming at you from all directions, and you try to put them in an order that informs what to check and what specifically to ask about.

That was, in retrospect, what I was talking about. It is the way I actually worked in the field. I don't just go in and sit a person down and go through things like how many kinds of this and how many kinds of that. But after you get some ideas about what's important, then you can sit down and say, "You were talking the other day about this." Then you can check things out and you can organize them. The basic idea is that you can organize a description in a formal way that highlights the kinds of questions that you can ask to check data. The questioning itself is a problematic. The questions can be used in many different ways in different situations.

A pure information eliciting situation is an unusual thing – not that common in life. So I'm less interested now in going around eliciting things out of people's heads. Now I want to see how people are actually interpreting ongoing social life. I think it is harder, but I've been doing it for a long time. It's not a recent revelation. There are different strains of cognitive anthropology. I guess there are many anthropologists who still believe that people have a cognitive map in their head and that such a map is a miniature model of the whole culture. By this model, the ethnographer must elicit the cognitive map from people by asking them the right kinds of questions. I think that was one of the early models that I never did strongly subscribe to, and I certainly never worked that way in the field. I certainly don't believe it now.

Dore: Do they know when they're making a mistake?

Frake: Well that's a real problem. They often can't state the rule. Let's say I make a grammatical mistake. Even if I can interpret it as a mistake, somebody else might be able to point out a rule that would somehow simulate what I somehow might have known.

Dore: So then the kinds of rules you want to write about – covering what a person knows about in a situation and how to proceed – would be formulatable by someone. What you need to understand and to compare a person's behavior is how they make use of that rule. I'm wondering about what the form of the claim is, because you are always talking about how you are interested in people in terms of what's going on, and how they are orienting to something real. You also say that what they are doing might be inexplicit, like the background conventions they're operating on, and I'm wondering about the possibilities for formulating that complexity with clear-cut statements about what they're doing. What do you do in those cases?

Frake: To me a rule is a thing that people appeal to account for what they're doing. It's not something that explains what they're doing. You want to know what a rule is going to look like? Or how you handle the problem of some rules that people can talk about and others can't?

Dore: How are you going to state that someone interpreted something in such and such a way?

Cole: I think that the pun has moved from in people's heads to interpretation. Let me put out a hypothesis and see how it works. Chuck is interested in what people say about what other people are doing, and that's what he means by interpret. He's not talking about psychological interpretations. If he is talking about a psychological process, then it's incidental to what he thinks he's talking about which is a description of what people say. I'm using what they say as evidence for something—as evidence for the fact they have some knowledge of conventions. I hate to use the words, "in their heads," but it has some currency in the sense that the people have learned something; so I guess something is in there, only I don't know where it is. That is what I want to find out. What have they learned? I'm arguing that what they've learned is not just simply some rules that generate their behavior. What they've learned are ways of making and finding meaning while making sense out of what people do. I don't mean just what a few people do, but what the whole world is doing around them. You're saying they are interpreting the situation in this or that way. What kind of statement comes after that?

This is the way they're interpreting the situation. They're doing something, here is what they said, and you take that as your data. You say, here is what they said about the situation, and then you go on to make a claim beyond their behavior – away from their language. You say that they interpreted the situation in such and such a way, and then you make a statement. What would one aim for?

Frake: Take a given situation. And look at different interpretations or performances within it. I did a thing on how to enter a house. Sometimes when people enter a house, it's considered a hostile act and other times people laugh and still other times nothing much happens. So then I suggest that there is a convention or rule about entering a house whereby violating certain ways is funny and violating other ways is hostile. It's a violation or not depending on the details of how people behave and others read their behavior. So you look, you have to have some kind of comparative context, different interpretations of what is considered the same situation or context. With different interpretations of performances within that, you form a kind of rule.

It's as if you have a statement of conventions. This is a convention that's operating for these people and then you have two kinds of evidence, this is what they say and this is what they are actually doing apart from what they say. And then somehow you can say what they are doing.

Well, when people are saying things, they are actually doing something too. The relation between what people say and what they really do bothers me. I think for me the more problematic situation is when they appeal explicitly to a rule – they say, for example, that's funny because we do it this way. Or that's funny; it's just funny. Sometimes people can appeal explicitly to rules, or things that look like rules. Other times they don't. If I make a grammatical mistake, I can't always appeal to the grammatical rule of English that really accounts for the mistake. I might [1] to something I learned from my English teacher but what relation that has to the way it usually works is different. The status of rules—in terms of rules the investigator infers from interpretations and rules the people have appealed to–and appealing to a rule explicitly is a kind of behavior that you want to look for. Someone else may say, that's not really the rule, that the rule is really this. So there is a rule for appealing rules.

Cole: One of the things you've reinforced for me at least, in what you've said so far, and it's part of a discussion we had during the winter, concerns what is psychological about Chomsky's grammar. We dragged George Miller in and tried to get him to talk about the sense in which linguists can talk about things that are going on in people's heads, that is, in the psychological status of linguistic rules. I'm not sure what the psychological state of linguistic rules is. I'm not sure of the psychological status of the kind of rules you're talking about. But I think that they are somehow similar to each other in that the fact that you're coming out - you're talking about linguistics as a model for description, sort of reinforces that to me - that is maybe one place we ought to worry about some of these issues, that we ought to think about them in terms of the issue of what linguistic rules say about psychology and the claims of certain types that Chomsky would want to make. George seems to have different ideas; he might accept part of that agenda and also have added criteria that he is interested in. I'm sort of answering John Dore. I'm just pointing to something that looks to me like it's reinforcing the notion that somehow the status of the kinds of things that Chuck is talking about are like the status of those kinds of things in linguistics.

[1] Other cultural systems Frake worked on invite comparisons at the level of detail: legal argumentation (1963, 1964a, 1969), disease and diagnosis (1961, 1980), ecological utilization (1955, 1962), navigation (1985, 1989, 1994b), ethic border negotiations (1996b, 1996c, 1998, 2011, 2012), and formality (1983).

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[i] Other cultural systems Frake worked on also invite comparisons at the level of detail: legal argumentation (1963, 1964a, 1969, 1980), disease and diagnosis (1961, 1980), ecological utilization (1955, 1962), navigation (1985, 1989, 1994b), ethic border negotiations (1996b, 1996c, 1998, 2011, 2012), and formality (1983).