

problems (Cole et al., 1971, Chap. 6). Overall, only 33 percent of the problems were answered correctly, and a great majority of the correct responses were for the wrong reason. In a follow-up study, responses of traditional adults were compared with those of a group of young adult high school students: 90 percent of student responses were correct, and their answers were very much in the Western mold. Next, groups of 10- to 14-year-old children with varying degrees of schooling were studied. The children who had never attended school responded like the traditional adults; children who had reached the third grade responded more like the high-schoolers. Hence it appears that some experiences involved in Western-style education change the "set" taken to these problems so that the logical relations that they express, rather than their factual content, constrain the conclusions reached.

Up to this point, we have the beginnings of a descriptive account of how traditional people handle verbal logical problems. But we are not even this far advanced in understanding *why* logical relations seldom govern subjects' conclusions. Do the relations among verbal propositions constitute a specific source of difficulty? Or is it the case that the subject processes the relational information but chooses to ignore it when it conflicts with his personal experience?

In an effort to get at some of these problems, Scribner (1973b) followed up on an observation of Luria's. Luria had observed that when subjects tried to repeat a question, they frequently dropped the relational terms and rendered the major and minor premises as two isolated statements. Scribner somewhat modified the procedure to incorporate a requirement that the subject recall the problem at two points in the experiment. First, the subject answered the problem, stated his reason, and was asked to recall the problem; then the experimenter slowly reread the problem and the subject was asked to recall it immediately. If the subject omitted the question at the end of the problem in either of his recall attempts, the experimenter prompted him by inquiring, "What question did I ask you?" The first recall, given after the subject had wrestled with the problem and come up with an answer, was often fragmentary and contained transformations of the kind already discussed. But a striking and unexpected finding was that the second recall, given immediately after the subject reheard the

problem, was often no more complete nor accurate than the first! A second prominent feature of the recalls was that the question contained in the problem was frequently stated as a conclusion—a statement of fact. Here are a few examples:

Example 1

Problem: The chief's brother either gave him a goat or he gave him a chicken.

The chief's brother did not give him a goat. Did he give him a chicken?

Subject's answer and reason: Yes. I know he gave it to him.

Subject's first recall: The chief's brother will give him a goat. If he does not give him a goat, he will give him a chicken.

Experimenter: What question did I ask?

Subject: You asked me, is the chief's brother going to give him a goat?

Experimenter: (Reads the problem again.)

Subject's second recall: Yes. That is what you told me. The chief's brother will give him a goat. If he does not give him a goat, he will give him a chicken.

Experimenter: What question did I ask you?

Subject: You asked me, the chief's brother will give him a goat. If he does not give him a goat, will he give him a chicken?

If we inspect this protocol carefully, we will observe that at no time during the entire procedure does the subject reproduce all the information that is necessary to reach a conclusion; the information that the brother did not give the chief a goat has dropped out entirely. The several attempts to recall the problem, moreover, indicate that without this information, the subject had difficulty keeping the problem question in mind—again, necessary information for the correct solution. Lacking evidence, our respondent solved his special problem of acquitting himself in the experimental task by giving an essentially arbitrary answer: "I know he gave it to him."

Example 2

Problem: Some kwi (Western) people are wealthy.

All wealthy people are powerful. Are some kwi people powerful?

Subject's answer and reason: Yes. It is because some kwi are wealthy and they have power.

Subject's first recall: Some kwi in this town are wealthy. They have power.

Experimenter: What question did I ask you?

Subject: Do some kwi have power?

Experimenter: (Reads the problem again.)

Subject's second recall: Some kwi are wealthy. They have power. Do many wealthy men have power?

In this example we observe the disappearance of the generalizing proposition, "All wealthy people are powerful," which is an essential condition for a correct conclusion and the transformation of the question into a piece of evidence. To the extent that the subject's reproduction of the problem accurately represents the one he was in fact attempting to solve (and for many reasons, which we will not take up here, we cannot be entirely sure of this), the examples we have quoted seem to be instances in which subjects were disposing of the experimental task by giving essentially arbitrary answers or by answering on the basis of general knowledge (in Liberia, kwi people *are* both wealthy and powerful). These were perhaps sensible ways of handling the task, but they represent a different mode of problem solution from the examples of creative constructions of appropriate (factually congruent) problems we encountered in the first interviews.

The techniques we have relied on to help us get at the actual thinking processes of the subjects, and away from inferences based entirely on the correctness or incorrectness of answers, are still new and in need of refinement and testing before they can give us reliable information: They have been useful, however, in helping us reformulate the questions with which we are dealing. It is quite clear that we cannot draw conclusions about reasoning processes from the *answers* people give to logic problems. We have first to ask: "What is their understanding of the task? How do they encode the information presented to them? What transformations does the information undergo, and what factors control these?"

Before going on to conclude something about the ability of nonliterate peoples to reason verbally, a different form of reasoning problem must be described. In this study conducted by John Gay (1971) the material was presented in a familiar folk-tale context. The problem involved a judgment based on a comparison of relations between elements expressed in the form of verbal

logical connectives (*and, or, if . . . then*). The story in one of its forms is as follows:

This is an old matter. You must listen carefully and think about the answer, so that you can answer the questions in a sensible way.

Two men named Flumo and Yakpalo were wanting to marry, and so they went on the road to find beautiful girls. They came to a man's house and found that the man had a beautiful daughter. Each one brought gifts for the marriage. The gifts were money and sickness. They told the man, "if you do not marry your daughter to one of us and take the gifts he offers, we will kill you."

Flumo said, "You must take money *and* sickness."

Yakpalo said, "You must take money *or* sickness."

Which one did the man give his daughter to and why?

In other forms of the story there were systematic variations in the gifts offered and in the logical connectives used between the gifts in each man's statement. In some cases both gifts were good, in some only one was good, and in some both were bad. The person interviewed was required to choose the most advantageous or least harmful combination of gifts. Each subject was scored as to whether his answer was correct, incorrect, or irrelevant; an example of an irrelevant answer was that the respondent would rather die than accept either suitor, if both offered bad gifts.

Gay presented the problems to three groups of Kpelle subjects, two of them nonliterate (one of 18- to 25-year-olds, the other of 40- to 50-year-olds) and the third consisting of 18- to 25-year-old high school students. In addition, 36 American college students were given the same set of problems. The most interesting result of this study is that the American college students and all the Kpelle groups had the same percentage of wrong answers—there were no group differences in errors. But the American college students exceeded all three Kpelle groups in the percentage of *correct* answers. This seeming paradox is accounted for by the fact that fully one-fifth of the Kpelle replies (in each of the experimental groups) was irrelevant. And irrelevant answers were given almost exclusively when a decision had to be made between two bad choices; under these circumstances, the Kpelle tended to avoid the issue, while the Americans sought to make the best of a bad bargain. It would appear that the Kpelle subjects were as able to avoid errors as the American college students, but they were much

more likely to deviate from the solution pattern set up by the experimenter in order to give a more socially appropriate response.

What can we conclude from this series of studies? For one thing, with all the varied materials and procedures that have been used, we have encountered no examples of thinking that violate the logical law of contradiction. On the contrary, our preliminary analysis shows that the major sources of error in Kpelle performance are of the same kinds as those involved in the reasoning of American college students (Henle, 1962). What we have encountered among people in traditional societies is a refusal to remain within the boundaries of the problem presented by the experimenter. In the case of the more standard experimental material (syllogistic reasoning), the terms of the problem were often not accepted or were modified; additional information was supplied in order to bring the statements and their implications into closer conformity with the factual world of experience. In the folk-tale problem, subjects tended to reject the restricted set of possible solutions if the outcome violated some standard of social truth. We know, too, that when traditional people have some schooling (as in our studies in Liberia) or become involved in complex acts of social planning (Luria's data), verbal problems of this kind are accepted, and reasoning is constrained by the structure of the problem. Why this switchover occurs is a challenging problem for investigation. And equally challenging is the task of adapting traditional procedures so that they yield a detailed account of how, in fact, traditional people do reason when they are presented with hypothetical verbal problems.

Summary

A real irony is embodied in this chapter. For more than a hundred years, speculation about the relation between culture and cognition has centered on the issues we have just been reviewing. Yet so few psychological studies of problem solving in differing cultural settings have been conducted that a summary of this work can have little content.

We can identify three reasons for the discontinuity between interest and accomplishment. First, as we attempted to make

clear in the opening pages of this chapter, there is an important sense in which psychologists and other social scientists are not talking about the same topic when they refer to culture, logic, and problem solving. There is no way to test Levy-Bruhl's assertions about primitive mind by referring to the amount of water in two glasses. He explicitly excluded such activities from consideration. Yet psychology has developed no generally agreed upon techniques for studying the cognitive mechanisms at work in the domain of beliefs.

Second, with the exception of the Piagetian research (which we have only sampled here), there really is no solid body of research on culture and problem solving using techniques and problems that psychologists view as legitimate. One reason for this is the paucity of problem-solving research anywhere in psychology in the last thirty years. There are a variety of interesting problem-solving situations that could be studied to good purpose in cross-cultural contexts, however, and some techniques are available for tackling them. For example, one of the few statements to grow out of Levi-Strauss' work that could be readily studied by psychologists is his characterization of the primitive problem solver as a *bricoleur* (see Chapter 2, pp. 26-27). A *bricoleur* is a jack-of-all-trades, a man who makes flexible use of his instruments, a man who does not fixate on a single attribute of objects and so can use them in a variety of ways. This suggests that primitive people should be less subject to *functional fixedness* (the inability to use an object in a nonstandard way to solve a novel problem) than are those in more technologically developed societies. No studies have followed up this hypothesis.

At the moment, the greatest source of suggestions for problem-solving studies is the anthropological literature, although psychologists will have to turn such work to their own uses. For example, Gladwin (1969) provides fascinating examples of complex navigation skills developed by nonliterate sailing people in the South Pacific.

In his well-known work on Yacui Indian sorcery, Carlos Castenada gives many examples of Don Juan's thinking that seem reminiscent of Levy-Bruhl's descriptions of "primitive mentality." At the same time, Don Juan is presented as a man who, in his interpersonal relations, demonstrates problem-solving techniques compelling enough to greatly influence Castenada himself (1968).

Bringing such observations together with psychological research is a giant order, and an issue that we will take up explicitly in the next chapter.

We would speculate that the third reason for the paucity of problem-solving research is that studies on this topic are so difficult to interpret, even within familiar cultural contexts. The key here is that problem solving, in ways that are pervasive and compelling, is always seen as a component of a larger behavioral network in which perception, memory, classification, and all other cognitive processes play a role. It is virtually impossible to isolate problem solving as a "thing."

Here cross-cultural research seems to be in a position to make some modest contribution to theories about the development of thinking. In a dramatic way, it highlights the need to analyze *all* the components involved in problem-solving tasks before inferences are made about reasoning processes or logical structures *per se*. Secondly, cross-cultural research might make a contribution to the development of a general theory of thinking. There are indications that a number of specific experiential factors play important roles in performance on classical psychological problem-solving tasks: familiarity with materials, opportunities presented by the environment for exploring spatial relationships, social contact with urbanized people, attendance at Western-type schools—all have been implicated as factors in the performance of one or more tasks. Yet there is no theory of thinking that seems at the present time to handle these varied constellations of experience within a single coherent framework.

But what can we say about problem solving among traditional people? The most firmly based, and perhaps the most important, conclusion we can reach at the present time is that thus far there is no evidence for different *kinds* of reasoning processes such as the old classic theories alleged—we have no evidence for a "primitive" logic. To go beyond this to a positive characterization of *how* traditional people think will require a host of new techniques and a great deal of imaginative thinking on the part of psychologists.

chapter 8 Culture and Cognition: trees in search of a forest

In the preceding pages we have reviewed a large number of studies whose subjects and subject matters have ranged across the globe and across a broad spectrum of the problems involved in the study of the role of culture in cognition. In looking back over the material covered, one cannot avoid the feeling that somewhere along the line important questions have been sidetracked. Our somewhat cautious and circumspect introduction is now easier to understand—the accomplishments of psychologists seeking to understand the relation between culture and cognition have been modest indeed. We can now plainly see that phrases such as "the mind of the primitive" have no clear referents in the real world and generate questions that have no answers, such as: "Does the mind of the primitive differ from the mind of technological man?" But the questions that have been posed in place of these

general and unresolvable phrases seem to be not nearly as significant: the question of whether or not education influences concept formation gets translated into studies of whether or not children make particular kinds of judgments about the consequences of pouring water from one glass to another.

This sequence, from grand speculation to narrowly specified experimental conditions is not, of course, restricted to cross-cultural psychological research. But we have been dealing with an area of human knowledge that has evoked enormously broad speculation; the gap between the kinds of statements we would like to make and the statements actually warranted by empirical evidence is very wide indeed.

The key problem, as we have been emphasizing throughout this book, is that any fact, or small set of facts, is open to a wide variety of interpretations. So long as we are only concerned with demonstrating that human cultural groups differ enormously in their beliefs and theories about the world and in their art products and technological accomplishments, there can be no question: there are marked and multitudinous cultural differences. But are these differences the result of differences in basic *cognitive processes*, or are they merely the expressions of the many products that a universal human mind can manufacture, given wide variations in conditions of life and culturally valued activities?

Our review has not answered this question. But it has suggested that obstacles to asking (and beginning to answer) this and other central questions about culture and cognition arise from weaknesses in both theory and empirical investigation. Such a conclusion might seem fatuous; yet only because attempts have been made to put theories to the test have the ambiguities and weaknesses of global, undifferentiated concepts about Mind and Man become apparent. Conversely, efforts to pull together and interpret facts accumulated from a variety of disconnected experiments have succeeded in revealing the lack of an integrating theory, without which unambiguous interpretation is impossible. What we have learned from the pioneer thinkers and investigators in the field is a long list of how-not-to's—how not to ask questions, and how not to go about investigating them. We also have a shorter, more tentative list of how-to's. We will try to summarize some of these observations and their implications in this chapter. If at times our emphasis seems one-sidedly negative, our

justification is that in the long run it will be more productive to direct our criticism at the deficiencies of our science rather than at the alleged deficiencies of the people we study.

To begin with, we have seen more than ample evidence of how *not* to ask questions about culture and cognition. If we were to restrict ourselves to the many studies that have been conducted because the experimenter wanted to “see what the X's would do when presented this task” (IQ test, visual illusion, conservation problem), no reasonable specification of the relation between culture and cognitive processes would be forthcoming. There is simply no way to evaluate the sources of variation when aborigines do not respond to an IQ test in the same way Cambridge undergraduates do. This seemingly simple fact is widely acknowledged and just as widely ignored.

But even when we turn to more sophisticated research—studies involving variations in instructions, motivational conditions, and populations—we find serious problems of conception and interpretation. Among the problems we have emphasized, the following are most pervasive:

1. There is a great readiness to assume that particular kinds of tests or experimental situations are diagnostic of particular cognitive capacities or processes.
2. Psychological processes are treated as “entities,” which a person “has” or “does not have” as a property of the person independent of the problem situation. They are also considered to operate independently of each other.
3. Closely related to (1) and (2) is a readiness to believe that poor performance on a particular test is reflective of a deficiency in, or lack of, “the” process that the test is said to measure.
4. Evidence from other disciplines (especially anthropology and linguistics) is usually not taken into account in making inferences about the cognitive processes which a given cultural group has or uses.
5. The complexity of the cultural groups and institutions studied is very often grossly oversimplified.

It will be helpful to keep these general lines of criticism in mind in the discussion that follows, because they represent a kind of catalogue of negative virtues that we believe the psychological study of culture and cognition should not have. By implication, they suggest an approach to future research. These criticisms raise questions about basic concepts and research strategies applicable to the entire field of psychology as well as

about those unique to cross-cultural endeavors, but we will deal with them only in the latter context. In developing these criticisms, we will begin where we initially began—with points of view about cognition and how it can be tested. We will then, by way of illustration, present a particular theoretical problem, assess the limitations of the evidence brought to bear on it to date, and outline what we see as the necessary ingredients of a research program to resolve some of the issues involved. Finally, we will double back to the original question animating this entire area of research: Are there *really* cognitive differences among different peoples, or are observed differences in behavior and belief merely varied expressions of a universal human mind?

Are Tests "Diagnostic" of Cognitive Capacities?

As we have seen, many cross-cultural studies set out to determine whether or not some particular group described by anthropologists "has," or "has more," or "has less," of some cognitive capacity considered characteristic of normal psychological functioning in industrialized Western societies. Do rural Africans have *3-D perception* of pictorial material? Do they show *abstract thought*? Have they complex processes of *perceptual analysis*? The *logical structure* of conservation? Questions of this kind imply that each of these terms designates some psychological entity, which is the property of a person and which is measurable by a specifiable—and limited—set of operations. With these assumptions in mind and a measuring instrument (test or experimental paradigm) in hand, the psychologist journeys forth to explore the relation between culture and Entity X. To make the dilemmas he encounters more concrete, let us first simplify the case and consider the problems a cross-cultural investigator would have if he wanted to explain the effect of culture on some nonpsychological characteristic—body temperature, for example. He would, of course, be careful to select people who were suffering no known illness, and he could include, as variables for study, the age of the people, the social structure that characterizes their society, their language, and a host of other factors. Although it is counterfactual, imagine for a moment that he found group differences in temperature. He would then face the problem of *explaining* the source of these differences. He would, of course, want to check

the hypothesis that what he was observing were not cultural differences, but physiological differences (much as Berry, 1971, was led to reassess a cultural interpretation of susceptibility to the Muller-Lyer illusion in terms of skin pigmentation). He might also want to check for the possibility that physical factors (altitude, rainfall) are associated with cultural variations, so that he could rule out the possibility that some factor like altitude is responsible for the observed group differences (a not-impossible factor—several anthropologists have sought explanations for cultural differences in variations in the physical ecology).

In short, he will face all of the problems that the psychologist faces when he moves from the documentation of group differences to the explanation of them.

But as a culture-temperature theorist, he will not face a central problem that the psychologist does face. He might have to be cautious about the influence of extremely high altitudes on temperature measurement, but at least he knows that thermometers *measure temperature*.

We do not have analogous information about psychological tests. In a very important sense, we do not know what they measure. Consider the best known of all psychological tests, the IQ test. So long as IQ tests are treated *solely* in terms of their ability to predict a child's school performance, arguments about the nature of the test need not arise. But as soon as we ask, What do IQ tests *really* measure? we enter an area of seemingly endless arguments and ambiguity; there are almost as many definitions of what intelligence *really* is as there are psychologists giving tests. And note that these problems arise before we get to the question: What determines the "amount" of intelligence in a particular child or cultural group? The problems arise, of course, because intelligence is not a property of individuals in the same sense that temperature is. To treat intelligence as if it could be measured like temperature leads us into a variety of absurdities, not the least of which is that we treat an intelligence test like a thermometer.

Similar remarks apply to virtually all psychological tests when they are treated as instruments measuring a fixed capacity. Research on memory conducted prior to 1906 gave us procedures for the study of memory that are still widely used today. These procedures embody our commonsense notion that if we can recall at a later time some material we previously had learned, we can

be said to have "remembered" it. But does the Ebbinghaus procedure (or any other) measure our memory? Is memory for nonsense syllables the same as memory for childhood events or memory for popular songs? Put this way, it becomes clear that we cannot treat psychological functions as if they are unitary entities to which we can apply a "thermometer."

All of this simply argues for taking seriously the notion that in cognition, as in other areas of psychological functioning, we are dealing with processes, not with properties. We gain understanding of cultural variations in memory or thinking or concept formation when we can specify the operations that go to make them up in given situations and how these operations and situations differ from one population to another. In the cross-cultural context, this means that we want to ask questions about how a particular group goes about interpreting pictorial material, learning a discrimination problem, classifying geometric stimuli, and the like.

We know, too, that in almost every area of research reviewed in this book, the nature of the operations subjects use has been shown to be sensitive to a whole host of factors connected with the particular problem situation: the specific demands of the task (giving a verbal description of a picture, selecting a match, or "modeling" it), the task material (whether it is familiar or strange, represented by objects or pictures), the semantic content of the problem (factually true or factually false syllogisms), the response mode (adjusting a rod in a frame or finding a hidden picture). Because little attention has been paid to them, we have altogether neglected motivational, attitudinal, and other factors that also may affect how the person goes about the experimental task. Again these considerations argue against the practice of treating tests or experiments that deal with performance in some common area (verbal recall, for example) as though, in fact, they are all getting at the *same thing*.

Finally, if we agree that we are studying operations, not entities, and that these operations are "shifty" and may work differently in different circumstances, then it follows that experiments are unlikely to allow us to rank different people in terms of the "existence" or "amount" of any particular cognitive process. Since this point is central to all cross-cultural research, we will illustrate how this approach to what cognition is and what experiments measure can help us thread our way through the

paradoxes encountered in testing a particular hypothesis about cultural effects on cognition.

A Cross-Cultural Hypothesis

For this purpose, we shall pick a problem that has not been dealt with in the previous chapters.

A number of anthropologists writing about the learning processes of nonliterate peoples have remarked on the fact that learning and teaching are almost always an integral part of ongoing activity such as hunting or a round of household chores (Fortes, 1938; Mead, 1964). Children are said to learn by observing.

Observational learning is usually contrasted with learning that is acquired primarily through the medium of language. Mead points out, for example, that in traditional societies adults rarely formulate a particular practice in words or rules; instead, they demonstrate what is to be done. Fortes observes that traditional children (he worked with the Tale people of Ghana) were rarely heard to ask *why* questions. He concludes that such questions are rare because so much of the child's learning occurs in real-life situations where the meaning is intrinsic to the context.

If these anthropological observations and speculations are correct, we might hypothesize two cognitive consequences of a reliance on learning by observation. First, we might expect to find that people who have a lot of practice in learning by observation will be good at it—they will learn quickly if given the chance to learn by observing. Second, these same people ought to experience special difficulties if they are asked to teach or learn something when the teacher and student are not engaged in a common, ongoing activity.

Our next problem is to turn these speculations into observations that are appropriate for experimental, psychological analysis.

Is there an experimental situation that has been developed to tap learning and teaching skills when learner and teacher do not have a shared, meaningful context for carrying out their tasks? There is. It is referred to in the research literature as the "communication experiment." Let us turn to a description of it as it was used by Cole, Gay, and Glick (1969) to study learning and teaching skills among the Kpelle.

A Communication Experiment

The example we have chosen to discuss involves an experimental situation appropriate to testing learning and teaching skills out of context.

Two men are seated at a table. The men are Kpelle rice farmers from central Liberia. Every year since they were small boys they have gone into the jungle to clear patches of land where upland rice is grown. They know the forest and its vegetation well; they work there almost every day; it gives them food, building materials, tools, and medicines.

On the table in front of them are 10 pairs of sticks (pieces of wood of different kinds) divided into two piles, each pile having one member of every pair. One pile is in front of each man.

A barrier is then placed between the men so that they can neither see each other nor each other's sticks. The experimenter, who is sitting where both men can see him, picks a stick from the speaker's pile and lays it on the table at the speaker's left. The speaker is told to describe the stick so that his partner (the listener) can pick its mate out of his pile.

After hearing the description, the listener tries to select the appropriate stick from his pile. The experimenter then picks out a second stick, places it next to the first stick on the speaker's side, and asks the speaker to describe it so the listener can find the mate in his pile and put it in place. This procedure is continued until all 10 sticks have been described by the speaker and laid out in a row in front of him, and the listener has tried to duplicate these activities.

At this point, the barrier is lifted, and the men are asked to compare the two rows of sticks to see whether they have correctly matched pairs. Errors are described and discussed, and the whole process is then repeated for a second trial.

Descriptions of a set of sticks as we might give them and as they were given by a Kpelle speaker on two trials of an experiment session are listed in Table 8-1.

What is striking about this man's performance (and it is representative of the performance of the many traditional Kpelle rice farmers who participated in this study) is that he is failing to include in his description features that must be communicated if the message is to be received unambiguously.

Table 8-1.

English Description	Kpelle Description (First Trial)	Kpelle Description ^a (Second Trial)
thickest straight wood	one of the sticks	one of the sticks
medium straight wood	not a large one	one of the sticks
hook	one of the sticks	stick with a fork
forked stick	one of the sticks	one of the sticks
thin curved bamboo	piece of bamboo	curved bamboo
thin curved wood	one stick	one of the sticks
thin straight bamboo	one piece of bamboo	small bamboo
long fat bamboo	one of the bamboo	large bamboo
short thorny	one of the thorny	has a thorn
long thorny	one of the thorny sticks	has a thorn

^aNote that actual order of presentation on Trial 2 was different from that of Trial 1.

It seems a fair description of our result to say that traditional rice farmers are poor communicators in a task where teacher and student (speaker and listener) do not share a common field of vision.

How are we to interpret this "fact"?

Interpreting Failures to Communicate

We can begin by noting that this communication-teaching situation is similar in many respects to experiments aimed at assessing the growth of cognitive development in children. The original motivation for this line of work came from studies by Piaget (1926) suggesting that young children have difficulty in understanding differences in points of view between themselves and people with whom they are talking (that is, they are egocentric).

In recent years, many investigators have extended Piaget's original observations to include procedures very similar to those we have just described. In one series of studies, Krauss and his associates found that as middle-class American children grow older, they produce shorter, more adequate descriptions of nonsense shapes. For example, a young child might say that a strangely shaped block "looks like my mother's hat," giving his listener no reliable clues to the identity of the object. Older chil-

dren mention specific features of the object they are describing, which permit their listeners to make correct selections. (For a summary of this research see Glucksberg, Krauss, and Higgins, in press.)

This same technique has been used to explore population differences in the development of communicative skills. Krauss and Rotter (1968), Heider (1971), and several other investigators have rather consistently found performance differences among children from different socioeconomic and ethnic groups; children from lower socioeconomic groups perform more poorly than wealthier groups of children.

Although the exact theoretical terminology changes from one investigator to another, virtually everyone working in this field conceptualizes the source of the communicator's inadequacies as the result of a *failure to take into consideration the information the listener needs in order to understand the message*. This egocentrism of the child is most clearly seen, according to Piaget,

when one child tries to explain something to another or in discussions among children. In both situations one sees the systematic difficulty children have in taking the point of view of the other, in making him grasp the desired information. . . . It is only after long training that the child reaches the point . . . where he speaks no longer for himself, but from the point of view of the other (Piaget and Inhelder, 1969, p. 122).

In terms of Piaget's theory, group differences in communicative performance reflect differences in the level of cognitive development achieved by the children in those groups. Piaget was, of course, speaking of groups defined by the children's age, but those who have studied ethnic and socioeconomic group differences have applied the same line of inference. In the early work of Bernstein (1970) and Krauss and Rotter (1968) it is hypothesized that lower-class children experience difficulties in such communication tasks (respond egocentrically) because of minimal interaction between child and adult and because lower-class speech patterns fail to make meanings explicit.

Summarizing this interpretative approach, we might want to conclude that for some reason (the nature of their language? the lack of formal schooling?) Kpelle adults are "egocentric"—that is, they have failed to develop the capacity to take a listener's point of view. Such an interpretation would represent an applica-

tion of the experiment-as-thermometer point of view: the study measures egocentrism and Kpelle adults "have it."

Although this interpretation may seem plausible when applied to children 4, 5, or even 7 years old, is it reasonable to claim that the average Kpelle adult is no more developed cognitively than a Genevan first-grader, or that Kpelle speech patterns are inadequate for purposes of communication?

Our doubts about the reasonableness of this interpretation are quickly reinforced as soon as we step outside of the experimental situation—at just about the time our two subjects have talked us into buying them a bottle of beer! Our own, real-life, non-laboratory observations and the more controlled observations of many anthropologists attest to the fact that there are no *generalized* problems of communication among traditional people.

Evans-Pritchard (1963), for example, describes the way in which the Zande people exploit the potential for ambiguity in speech in order to protect themselves against their supposedly hostile tribesmen. Here is a striking example of this form of indirect speech, called *sanza*:

A man says in the presence of his wife to his friend, "friend, those swallows, how they flit about there." He is speaking about the flightiness of his wife and in case she should understand the allusion, he covers himself by looking up at the swallows as he makes his seemingly innocent remark. His friend understands what he means and replies, "yes, sir, do not talk to me about those swallows, how they come here, sir!" (What you say is only too true.) His wife also understands what he means and says tartly, "yes, sir, you leave that she (wife) to take a good she (wife), sir, since you married a swallow, sir!" (Marry someone else if that is the way you feel about it.) The husband looks surprised and pained that his wife would take umbrage at a harmless remark about swallows. He says to her, "does one get touchy about what is above (swallows), madam?" She replies, "Ai, sir. Deceiving me is not agreeable to me. You speak about me. You will fall from my tree." The sense of this reply is, "you are a fool to try and deceive me in my presence. It is me you speak about and you are always going at me. I will run away and something will happen to you when you try and follow me" (p. 211).

Evans-Pritchard's formulation for a successful *sanza* is as follows: "The great thing is to keep under cover and to keep open a line of retreat should the sufferer from your malice take offense and try to make trouble."

In order to be successful at this practice, the speaker must be very finely tuned in to the meaning ascribed to his remarks by the person he is addressing. If he cannot accurately assess the listener's point of view, he will be unable to carry the *sanza* through to its desired outcome.

Moreover, the speaker must take into consideration, not only the point of view of the listener, but also the point of view of others who may (intentionally) overhear part of the conversation. He must also consider various avenues of response open to the target of his malice and the culturally accepted rules that will protect him from such retaliation. In spite of these complex and difficult features of *sanza*, Evans-Pritchard find its use so effective and so ubiquitous in everyday Zande speech that our renowned Oxonian colleague is led to lament at the end of his article:

It [*sanza*] adds greatly to the difficulties of anthropological inquiry. Eventually the anthropologist's sense of security is also undermined, his confidence shaken. He learns the language, can say what he wants to say in it, and can understand what he hears; but then he begins to wonder whether he has really understood . . . he cannot be sure, and even they [the Zande] cannot be sure, whether the words do have a nuance or someone imagines that they do.

He closes by quoting the Zande proverb, "Can one look into a person as one looks into an open-wove basket?" (P. 228.)

It is important to mention that while the particular form of ambiguous speech that Evans-Pritchard describes may have special features among the Zande, the use of rhetorical skills as a vehicle for controlling one's social environment is a very general feature of both nonliterate and literate societies (Albert, 1964; Labov, 1970).

Assuming the existence of such skills among the Kpelle—and there is good evidence for this assumption (Bellman, 1968)—the anthropological data on language usage seriously call into question the egocentrism interpretation of our Kpelle rice farmer's communication difficulties.

This interpretation is also challenged by observations of other cultural phenomena. Here a broader look at the developmental hypothesis first suggested by Piaget is useful; egocentric communication was not seen as an isolated phenomenon by Piaget, but as one manifestation of the intellectual organization of the young child.

In a recent summary, Piaget and Inhelder (1969) tell us that children who manifest egocentrism in their communication behavior also manifest it in the way they play games. When adults play games,

there is common observance of the rules, which are known to the players; mutual surveillance to make sure the rules are observed; and above all, a collective spirit of honest competition, so that some win and others lose according to accepted rules (p. 119).

Young children play games quite differently:

Everyone plays the game as he understands it, without much concern for or checking up on what the others are doing. . . . [M]ost significant, nobody loses and everybody wins at the same time, for the purpose is to have fun by playing for oneself while being stimulated by the group. . . . There is, then, a total lack of differentiation between social behavior and concentration on individual action (p. 119).

If Kpelle game-playing is supposed to reflect the same level of cognitive organization as their communication performance, we should look for childlike patterns of game play. Quite the opposite conclusions are suggested by the ethnographic literature on the Kpelle. As part of general descriptions of Kpelle life, several authors have presented evidence that the Kpelle play a variety of rule-governed games, adhering to modes of behavior that fit Piaget and Inhelder's characterization of *adult* gaming. Kulah (1973) describes a verbal game played according to strict rules, even by 6- to 7-year-old children. Cole and his associates (1971) went so far as to hold a tournament to determine the best adult players of a traditional Kpelle board game with complex rules. Some of the successful competitors were among the men who manifested inadequate responses in the communication task. Piaget and Inhelder also link communicative egocentrism to "children's initial difficulty in finding (even in seeking) modes of collaboration, as if collaboration did not constitute a specific end that must be pursued for its own sake" (p. 120).

Do the Kpelle generally experience difficulties in finding modes of cooperation? All the evidence we have (e.g., Gibbs, 1965) tells us just the opposite. Kpelle society has evolved a variety of institutions for ensuring cooperation among its members, for minimizing conflicts, and for maximizing the corporate good. Just one example of cooperative effort occurs in farming and housebuild-

ing. For these enterprises, which require a lot of time and effort, people organize themselves into cooperative work groups, called *kuus*. As described by Bellman (1968), the operation of the *kuu* somewhat resembles a nineteenth-century barn raising. The common labor is accompanied by shared palm wine and shared music to make the work less oppressive. There may be competition involved (for example, to see who can cut the low bush most rapidly), but it is competition in the service of the corporate good.

In this, as in many other aspects of their lives, Kpelle adults seem to represent the antithesis of a Piagetian child.

From Negative to Positive: A Program of Research

Let us recapitulate what we have learned thus far.

1. Starting from anthropological observations that traditional, non-literate peoples do most of their learning and teaching in the context of the objects being discussed, we hypothesized that such people would experience problems if they had to communicate about objects not viewed in common.

2. We arranged a study that embodied our assumptions and found that difficulties in communication did in fact occur. Specifically, speakers did not tell listeners enough to permit unambiguous choices among objects.

3. We noted that when similar behavior is observed in young European and American children, it is attributed to their egocentrism—their inability to take another's point of view.

4. We pointed out that in other areas of cross-cultural research, psychological interpretations developed in Western cultures to explain experimental findings have often been uncritically accepted as explanations of similar findings in other cultures. In this vein, we asked whether the egocentrism interpretation of poor performance in the communication experiment could help us understand the outcome of the Kpelle study.

5. Turning to anthropological accounts of verbal behavior among the Kpelle and another tribal African group, we found these traditional people behaving in ways that we can interpret only by assuming that they do consider their listener's knowledge, and in very subtle ways. In behavior domains where the Piagetian theory tells us to expect further evidence of egocentrism, quite the opposite situation prevails.

It seems, in summary, as if our initial guess about communication difficulties was correct, but we are not much closer to specifying

ing the mechanisms at work. Although we may agree that the research described thus far has been inadequate, the question remains: *Are there any research strategies that will serve the purpose of helping us identify the process underlying poor (or competent) cognitive performance?*

In order to increase our understanding of cultural influences on communication, three complementary courses of action suggest themselves.

First, we can systematically inquire into the task-specific sources of difficulty that the Kpelle speakers experience in the formal *experimental situation*. This calls for a research program in which we manipulate various features of the experiment so as to uncover the component processes involved in poor communication and to determine what particular conditions regulate which specific processes.

Second, we can follow up anthropological accounts with systematic investigation of the *situations in everyday life* in which the Kpelle show themselves to be good communicators. What distinguishes these situations from our laboratory (experimental) situation?

Third, we can return to the experiment to test specific hypotheses of what makes for good performance in naturally occurring situations. Through this back-and-forth process—from observation in natural settings to experiment in artificial or laboratory settings—we may be able to make progress in understanding the complex relations among cultural factors and communication.

In short, we are proposing that we tackle our problem through the twin methods of *experiment* (the psychologist's stock-in-trade) and *observation* (the anthropologist's specialty). We believe, and we hope to show, that these two methods, frequently considered unrelated if not actually antagonistic modes of inquiry, are instead complementary and mutually enriching research approaches. Let us consider each aspect of this visionary research program in turn.

Analyzing the Experiment

Some of the difficulties that may confront the Kpelle speaker in the communication experiment seem self-evident. For example, it is obvious that he must perceive the differences between the objects he is asked to describe and other similar objects in the

array before him. If a speaker does not *see* or does not *notice* the differences among, say, the three bamboo sticks, he is not going to encode these differences for himself, let alone communicate them accurately.

A closely related hypothesis is that the necessary distinctions may be difficult to describe in the Kpelle language. Some basis for such speculation is provided by Gay and Cole (1967), who report the paucity of measurement terms that can be applied to materials like those used here.

Both these hypotheses are susceptible to experimental test, and indeed we have evidence concerning them. The possibility of linguistic deficiencies was ruled out in the 10-stick study just described, from which the example in Table 8-1 was drawn (Cole et al., 1969). This was accomplished by including a condition in which a college-educated Kpelle experimenter acted as the speaker. When this speaker described the sticks, his listeners made few errors in picking the correct alternative. Each of the necessary modifiers exists in Kpelle (long, short, thorny, and so forth) and when properly applied, they produced effective messages. This result also tells us that the Kpelle rice farmers *can* make the necessary perceptual distinctions among the sticks.

However, we still need to consider the possibility that our speakers, although capable of making the necessary perceptual discriminations, failed to do so because they did not *notice* the differences.

Several techniques suggest themselves as means of clarifying this issue. For example, we might begin the study by a preparatory session with the speaker in which we present him with pairs of sticks (thin curved bamboo and thin straight bamboo) and require him to tell us the difference between the two. In this way, we could assess each speaker's attention to, and encoding of, the discriminating attributes of the sticks.

We might get at the same set of issues by speculating along the following lines: Although Kpelle farmers are familiar with the sticks they are describing, their use of them rarely requires the kinds of discriminations required in this task. If we picked objects of equal complexity, but ones that embody culturally meaningful attributes, the speakers would spontaneously note them. For example, similar objects differing in their *functional* attributes might be more easily communicated (a hunting knife and a small

knife for cutting vegetables). Perhaps objects with similar functions but of different manufacture would be more readily coded (a country-smelted cutlass and a store-bought, steel cutlass). A great many possibilities of this kind exist, all of which pursue the influence exerted on communication by the nature of the objects being communicated about.

This section would not be complete if we did not discuss the problems surrounding instructions to the subjects. This is a tricky issue because there are two interpretations of the claim that "the subjects did not know what to do": (1) The subjects were unclear about the goals and procedures of the experiment (what we ordinarily mean by instructions). And (2) they were not told what to do in order to accomplish the goal (which is usually the object of the study and not usually part of the instructions).

In the present communication study, a good deal of care was taken to make the instructions (the goals and procedures) clear. Preliminary instructions were tested with college-educated assistants, who prepared translations. The procedures were pilot-tested with Kpelle speakers who had not encountered the situation previously, and everyone *seemed* to understand what to do.

But the matter cannot be settled by any one "correct" set of instructions; the possibility of misunderstanding still exists. The only reasonable course is to conduct a study that incorporates instructional variations that eliminate specific sources of misunderstanding.

For example, in the 10-sticks experiment, instead of laying the sticks in two piles and simply telling the subjects that pairs of sticks are supposed to be matched, the experimenter might lay out the sticks as they should appear when matched, so that subjects could see the desired outcome.

A second instructional approach is to give the subjects repeated practice with one or more sets of materials. In the study we have been discussing, subjects were shown the outcome of their choices after the first trial, and the speaker did improve his messages, as Table 8-1 indicates. From our point of view, the repeated trials constitute practice, but the nature of this practice is instructional in that it has the effect of making clear to the speaker just what is required of him.

This practice, or training, approach is also directly relevant to evaluating alternative hypotheses about the source of a speaker's

inadequacies. According to the egocentrism hypothesis (even if it is applicable only in this limited context), the speaker *cannot* take the point of view of his listener. Therefore, if practice in the task leads to improvement, the egocentrism hypothesis has to be weakened, at least to the extent of claiming that the speaker *can* take into account the listener's point of view, even though he does not.

Studies in the United States (summarized in Glucksberg et al., in press) do *not* find practice effects in children younger than 8 years, but they have found practice effects in children over 8, thus supporting the notion that the younger children are *unable* to handle the task. In Table 8-1 we see clear improvement in the performance of the speaker (although it is by no means perfect). This finding supports the idea that more precise instructions (here embodied in the correction procedure) will reveal communicative competence where we had thought it absent.

Investigating Naturally Occurring Situations

A series of studies modeled along the lines described in the previous section would certainly increase our understanding of the conditions under which traditional Kpelle rice farmers will or will not produce adequate messages for unseen listeners in a particular species of experimental situation. But to restrict our inquiry in this way is to lose sight of our original research goal, which is to specify the relation between culturally determined experiences and communicative (teaching and learning) behavior.

A necessary complementary approach is suggested by the anthropological observations we have summarized above.

To begin with, can we pinpoint, in those communication situations where anthropological evidence suggests that speakers *do* seem to take into account their listener's point of view, any factor or factors that distinguish those situations from our experimental situation, where the speakers do not consider the listener's point of view?

The obvious first candidate is exactly the variable we picked as the focus of our interest—whether or not the speaker and listener can make face-to-face contact with each other.

Starting again from this point, we might be tempted to choose a commonly occurring face-to-face communication situation, just to make sure that “anthropological wisdom” about the nonverbal

nature of the teaching in such societies is correct. Among the Kpelle, for instance, we might ask a skilled basket maker to teach a novice how to weave a basket and simply record the proceedings. John Gay reports, in a personal communication, that he has made such observations in an informal way, and he confirms that the teacher relies heavily on demonstration (“you do it like this”). Moreover, the few teachers sampled seemed to have difficulty in describing the total process when explicitly asked to do so.

We might now be led to wonder what would happen if someone were asked to teach about an object for which no long-standing teaching techniques had been established.

For example, many people in Kpelle-land now carry flashlights, yet few know how to take a flashlight apart, and put it back together. What would happen if we selected a group of Kpelle adults and gave them an elementary knowledge of flashlights—the names of their components, how they are put together, and how to determine the source of malfunctions? Then, in individual sessions, each teacher could be asked to teach another adult what he had learned. Motivation for good performance could be insured by rewarding successful teachers and students with flashlights. The question then becomes: Will teachers apply a traditional demonstration teaching technique, or will they provide verbal descriptions and elaborations of the sort they themselves received?

Extensions of this line of inquiry would lead us to ask people to instruct friends or strangers about the directions to a distant town, in order to see whether the speakers modified their instructions to suit the knowledge of the listener. We might ask people to describe themselves so that someone from another village could pick them out from a group of their friends, to see whether they would pick adequate descriptions. In general, we could devise a variety of naturalistic and quasi-experimental observations that would tell us a good deal more about habitual teaching techniques as well as about the conditions under which people seem to take a listener's point of view into account.

Integrating Experiment and Observation

Based on what we learn, we could try to test specific hypotheses about conditions for effective communication in the formalized setting of the experiment. We might begin by devising a situation

like the communication experiment, with a speaker who describes to a listener each of a set of objects, like sticks. We would, however, preserve the natural feature of social communication by allowing the two participants to be in face-to-face contact.

Although this is a simple enough proposal, there are many practical hurdles to overcome, especially in primitive field situations.

Consider what would happen if we simply removed the barrier between the two men. When the experimenter selected a stick from the pile and laid it down, the speaker would not have to say a word; the listener could simply look over his pile of sticks and pick the matching one.

A slightly more plausible idea might be to conduct the study as follows: The speaker and listener sit across a table from each other, but only the *listener* has a pile of sticks in front of him. The matching 10 sticks are held by the experimenter, who is seated behind the listener, where the speaker can see him but the listener cannot. The experimenter holds up one stick at a time, and the speaker must verbally instruct the listener so that he will pick out the corresponding stick.

This procedure could produce a variety of outcomes, depending on whether the listener is allowed to ask questions, on whether the speaker is permitted to modify his directions if he sees the listener making an incorrect choice, and also on the kinds of verbal instructions that are allowable. Even posing the problem in this manner suggests some of the factors operating in real-life communication which we did not permit in our original experiment. Suppose that a speaker says, "the bamboo one," and the listener dutifully picks one of the three bamboo sticks but not the correct one. If allowed to respond in any way he deems appropriate, the speaker might point to the correct one, or say, "no, not that one, the other one over there." But supposing that we do not permit the speaker to point, a variety of nondescriptive responses could still be used to guide the listener.

Several versions of letting things run their natural course (perhaps even to the extent of allowing pointing) would be a reasonable first step, but would not capture the spirit of the experimental communication task. So we could add increasingly stringent restrictions, such as "you can't point," "you can only give one message for each stick," and so on. In this way we could

determine exactly which components are crucial to adequate communication.

It would be possible to continue the list of relevant experiments on the problem of culture and communication, but it is not our intent to display the extent of our experimental imaginations or to try the reader's endurance. We have gone into some detail in the previous sections in order to suggest the kind of experimental program that is necessary if we are to provide a rich and convincing account of the relation between cultural variations and variations in communicative performance. Assuming that still other questions need to be asked if we are to reach something approaching real understanding, we want to return now to the principles underlying the approach to culture and cognition that this example illustrates.

Cognitive Differences

The example of a research program on communication skills and their cultural variations has pitted two points of view against each other—the more standard view that cultural differences in cognition are composed largely of differences in the existence or amount of some hypothetical psychological capacity (egocentrism, for example) and the view that such differences reside in the way particular processes are brought to bear on the problem at hand. At the beginning of this chapter, we used a theoretical argument to assert that it is not useful to conceive of cognition in terms of capacities or properties or characteristics. The communication experiment example demonstrates that, in *practice*, such a view leads to unresolvable ambiguities and paradoxes in the interpretation of experimental data and their integration with the research findings of anthropologists. We suggested that conceptualizing cognition in terms of *processes* or *operations* might help us develop research that would be more fruitful in locating the specific sources of observed differences in performance.

The communication experiment and our analysis of it suggest that a variety of related cognitive processes have to operate together in the experimental situation for effective communication to occur. Successful formulation of a message was seen to require the speaker to *perceive* differences among the sticks, to *attend* to

distinctive, distinguishing features of the stimuli, to *assign* appropriate descriptive terms from his vocabulary, to *remember* descriptions already used, and to *understand* the task at hand. Virtually every area of cognition described in this book plays some part in successful communication performance. And we have not yet listed the ability to take the point of view of the listener!

It seems clear to us that a failure in any one of these processes, either because the person "does not have it" or because he fails to apply it, can yield poor performance.

This is not a new point of view in psychology, although it is one that has not received much attention in Western European and American writing. However, in the early 1920s a very similar position was advanced by L. S. Vygotsky. Speaking in the context of the cortical representation of complex cognitive processes, A. R. Luria, Vygotsky's student, has repeatedly asserted that "higher mental functions are complex, organized *functional systems*," the components of which are represented in different areas of the brain and combined in different constellations depending on the task at hand (see Luria, 1966, pp. 23ff). Furthermore, Luria emphasizes that neither the components nor the functional relations into which they enter are already formed at birth. Rather, they are formed in the course of each individual's development and depend very closely on the social experiences of the child.

As an illustration of a functional system, we can consider the set of experiments on free recall discussed in Chapter 6. In those studies, it was consistently found that educated and noneducated subjects performed differently. In tracing the source of the differences, we saw that under some circumstances, both groups showed efficient, organized recall. However, under standard experimental situations, the groups without schooling did not recall well, did not improve much with repeated practice, and did not organize their recall categorially. Yet, as the work of Scribner (unpublished) demonstrates, taxonomic categorization entered into the performance of both educated and noneducated subjects. It did not, however, play the same role in the recall of the two groups. For the noneducated groups it was present (as seen in the way items were grouped prior to recall) but did not play a controlling role in the recall process itself. For the educated groups, taxonomic categorization was both the dominant mode of sorting the

stimuli and the control mechanism in recall. In the language of functional systems, we would say that the same component has entered into two different functional systems (those characteristic of the educated and noneducated groups for this task) and it is the *functional systems* that differ.

Another way to look at this is to say that the same component process may play different roles, depending on the organized functional system of which it is a part. In the case of the educated subjects, their categorization of the material *led* their recall, while with the villagers it played an *auxiliary* role (which we are not yet in a position to characterize more precisely). For certain tasks, one functional system may be more effective—that is, may produce better performance—than another. In some situations, however, equal levels of performance might be achieved by different functional systems. The recall of a set of objects that has been sorted into groups might be mediated by visual representation of the objects, by the names of individual items or names of the groups into which they have been sorted ("things we hunt with"), or by different combinations of these representational processes—all of which might produce the same outcome. *Whether or not the outcome is the same*, the important research problem for the psychologist is to identify the processes actually employed by different subjects and to determine how these processes were coordinated to handle the task at hand. We are conceiving of functional systems, then, as flexible and variable organizations of cognitive processes directed toward some fixed end.

A major implication of this view for cross-cultural work is that *we are unlikely to find cultural differences in basic component cognitive processes*. While we cannot completely rule out this possibility, there is no evidence, in any line of investigation that we have reviewed, that any cultural group wholly lacks a basic process such as abstraction, or inferential reasoning, or categorization. Rather, the data have left us to wrestle with the problem of why it is that some procedures suggest that a given process is involved in the performance and some suggest it is not. The concept of functional system is helpful here. We might start with the hypothesis that sociocultural factors play an important role in influencing which of possible alternative processes (visual or verbal representation, for example) are evoked in a given situation and what role they play in the total performance (is verbal en-

coding used in the active process of rehearsal in a memory task or not?). To illustrate again with our communication example: it may be that Kpelle adults in the domain of social intercourse adopt the orientation of the other person but that this orientation is not activated when the task at hand is transmitting information about impersonal, technical matters such as, in the experimental case, object properties. If cultural differences are assumed to be reflected in the way functional systems are organized for various purposes, then a double line of research becomes important: the first is to uncover the culturally determined experiential factors that give rise to different dominant functional systems (is formal schooling the critical experience for the development of techniques of categorized recall in the free-recall experiment?); the second is to determine which situational features—content domain, task requirements—call out which functional organizations.

Nothing we have said so far can be considered a theory of culture and cognition—not even a primitive one! But we think the concept of *functional cognitive systems*, which may vary with cultural variations, may be a most useful approach to guide future research and may at some point offer the possibility of an eventual integration of theory and fact in this field.

From a research point of view, this approach highlights several needs. Greater attention needs to be paid to the possible range of cultural features that may be implicated in the development of certain modal functional systems (we will return to this point later). We also need to isolate general dimensions of task situations that are related to differences in behavior. Demonstrating that there are variations in the situations within which people manifest a particular cognitive process may be a good beginning, but it is not psychologically meaningful unless we can specify the rules underlying the patterns of behavior that we see in different situations. Clearly a part of any situation-dependent theory is going to have to include a theory of situations. No such theory exists, and none is likely to be developed without the close participation of cultural anthropologists.

From the functional systems point of view, which we have been espousing, it readily follows that our insistence on a variety of approaches to studying a particular kind of cognitive performance is neither a caprice nor an effort to garner a large number of publications. It is an absolute requirement, dictated by our conception of the origin and organization of cognitive processes.

This idea, that single experiments are inadequate for the evaluation of culture-cognition hypotheses, is widely recognized even by those who tend to theorize in very different terms. Many authors have written on this subject, but the classic statement was made by Donald Campbell (1961):

We who are interested in using such [cross-cultural] data for delineating process rather than exhaustively describing single instances must accept this rule: *No comparison of a single pair of natural objects is interpretable . . .* (p. 344).

However, if there are multiple indicators which vary in their irrelevant attributes, and if these all agree as to the direction of the difference on the theoretically intended aspects, then the number of rival explanations becomes greatly reduced and the confirmation of theory more nearly certain (p. 345).

Looking back at the research programs described in previous chapters, we can see that the most compelling lines of research followed Campbell's prescription, at least in part. For example, the extensive series of studies by Dawson and Berry on cultural variations in field-dependence would have little plausibility if they had been restricted to two cultural groups and a single experimental task (say, Temme versus Scots on the embedded figures test). The hypothesis that child-rearing and ecological factors combine to influence the degree of field-independence gained considerable plausibility from the fact that the pattern of relationships appeared over several different cultural groups and two or more seemingly unrelated tasks, such as the embedded figures test and Hudson's test of three-dimensional picture perception (as described in the work of Berry, 1971).

However, our requirements for a successful research program go beyond the recommendations of Campbell, and far beyond the practice of Berry and others, in the range and number of observations that we ordinarily think of as necessary in order to confirm a cross-cultural hypothesis. Although Campbell would almost certainly agree to the usefulness of the experimental program that we laid out in connection with our hypothesis about culture and communication, his remarks have almost always been applied within the context of rather narrowly defined experimental operations; nowhere are there suggestions for making observations akin to watching traditional teachers at work or analyzing the content of an exchange of verbal insults. Where anthropological variables do enter into the experimental programs we have been

reviewing, they have most often occurred in the search for interesting populations among whom a test or experiment should be tried. This is the strategy used in the Berry research just mentioned.

We have seen, however, that it is enormously difficult to interpret any set of experimental data, no matter how well conceived and elaborated the experimental program, without taking into account knowledge about the culture and the behavior of the people gained from the work of anthropologists, linguists, and other social scientists. While it is important to integrate general knowledge from the various intellectual disciplines, we would go further in suggesting that the *methods* of these relevant fields of endeavor need to be integrated for the purpose of generating information on particular hypotheses. Field and laboratory, anthropological observation and psychological experimentation, can yield knowledge from different perspectives about the same function. In outlining our model research program on communication, for example, we showed that naturalistic observations of traditional teaching methods could be important in generating specific hypotheses about communication behavior and in helping us pinpoint what there is about the experimental situation that disrupts effective communication patterns. The idea that different disciplines should cooperate in an integrated research program is certainly not new. A number of cooperative endeavors have been carried out in studies of culture and personality and of cultural variations in socialization practices. But, as we have seen, such examples are unhappily still rare in cross-cultural investigations of cognition.

Another research implication of the functional-systems view is that wherever possible in the design of cross-cultural experiments (as opposed to other types of observation) all kinds of performance ought to be readily interpretable in terms of what the person *is* doing. This point follows directly from our criticisms of the way that psychologists interpret lack of performance, but it is by no means easy to implement. Still it is worth mentioning because cleverly designed experiments can shorten the string of observations necessary to make decisions about a hypothesis.

As a positive example of what we mean, we can mention the experiment by Deregowski (see Chapter 4) in which he was evaluating the role of the perspective of the viewer and the per-

spective of the photographer in situations where the two did not coincide. This was not, strictly speaking, a cross-cultural experiment, since Deregowski was trying to track down the source of errors in the perceptual performance of a particular African nonliterate group. But the example is useful because he did not pose his hypothesis in terms of success or failure, but in terms of subjects' adherence to one of two viewing perspectives. It could have turned out, of course, that his subjects adopted neither perspective and the pattern of results could not have been directly interpretable, but the chance of such an outcome was minimized by the nature of his experimental design. Since he was able to interpret performance in terms of what subjects *did*, he could then follow up with a meaningful question about cultural influences by asking, "And how does Group X respond to this task?" In a more limited way the studies of free recall have proved useful because hypotheses about different memory strategies (rote recapitulation versus meaningful reorganization) could be tested directly from the data. Unhappily, that series of experiments is notable for the fact that subjects failed to respond consistently with either hypothesis, necessitating the long and still-incomplete series of experiments that followed.

Cultural Differences

At numerous points in this volume we have commented on the fact that the overwhelming majority of cross-cultural psychological experiments consist essentially of finding two populations that contrast in some theoretically interesting way, and then running a standardized test on the two groups to see whether there is a difference in performance.

Yet it may be noticed that in our extended discussion of hypotheses about communication among the Kpelle, no mention was made of studies contrasting children of different ages, school-children and nonliterate children, adults involved in special activities, or any of a number of seemingly interesting contrasts of this general type.

Of course, we did have, in the back of our minds, a general contrast between traditional Kpelle farmers and *someone* (what else could cross-cultural mean?), and we did mention the fact

that American third-graders did not experience difficulties with the experimental task we presented the farmers. But the key point is that an entire experimental program was generated without intergroup comparisons at its center.

How could this be?

We think it arises from two characteristics of our view toward psychological research on cognition.

Partly as a result of believing in our own criticism of the research we have been reviewing, and partly as a result of adopting a functional-systems approach to the study of culture and cognition, we have come to a new view of the role of intergroup comparisons: Instead of a useful way to *test* cross-cultural hypotheses, we find that intergroup comparisons of the sort typically encountered (educated and noneducated, middle class and lower class, hunters and farmers, indulgent upbringers and disciplinarians) seem to function more as *hypothesis generators*. In every chapter of this book, a study that was intended to test a hypothesis about culture and cognition gave rise to much deeper speculations about the actual mechanisms involved in the particular performance *in any culture*. We were led to speculate about the many things a person needs to learn in order to interpret or copy a picture, the many stages between presentation and recall of a list of unconnected words, the factors that control whether someone puts two and two together to make an apparently simple inference, and so on. In each of these cases, the fact that one group performs well while another group seems to experience severe difficulties becomes a stimulus to the investigator to re-examine his ideas of what good performance entails—so long as he does not conclude that poor performance implies a corresponding lack of process. In a very real sense, cross-cultural experimentation conducted in this spirit can add as much to our knowledge of ourselves as it adds to our knowledge of “them.”

Intergroup comparisons, however, can and should be made to help illuminate the factors that lead to the *development* of different organizations of cognitive functions. The kind of intergroup comparisons that are likely to be most helpful are comparisons of groups *within the same culture*. As we have become familiar with the details of research programs, we cannot fail to have been impressed by the fact that the old simplistic notion of some generic entity called “primitive culture” has given way to an appreciation

of the diversity of traditional *cultures*. Beyond this, we have seen that the population within a *single* traditional culture—even one with a low technological level and little differentiation of labor—does not constitute a homogenous mass such that one can talk about *the* Temne or *the* Kpelle for all purposes without taking into consideration the fact that some Temne are hunters, some not; that some Kpelle are farmers, others are blacksmiths, and in greatly expanding numbers some today are factory workers. Populations within cultures may be differentiated by all the characteristics popularly used in psychological research in the United States: age, sex, and the like. New and exciting research opportunities present themselves in third-world countries, however; many of the societies are undergoing rapid change, and these changing circumstances are affecting different segments of the population. Thus it becomes possible to investigate the effects of schooling on apparently comparable groups of children from the same village (as Greenfield did), or the effects of modernization of village life on adults still involved in traditional occupations (as Scribner did).

Traditional cultures in transition would thus seem to offer an important natural laboratory in which to explore the historical factors (from a societal point of view) and the developmental factors (from an individual point of view) which contribute to specific cognitive organizations. To pursue such questions would call upon still another research strategy—one rarely made use of in cross-cultural research. That is the longitudinal research design, which follows one group of people through time to see what changes in life experiences may lead to changes in cognitive skills. Same-group comparisons at two points of time within a culture might usefully complement intergroup comparisons at one point of time. Neither inter-group, nor same-group, nor two-or-more-culture comparisons have special powers, however; each is but one tool in the psychologist's kit, a tool that is not useful taken by itself, but only when used in conjunction with others.

By now it must be evident that our aim in this book is not to mark out a new *field* of psychology. Rather, we want to encourage a new *approach* to the study of the role of culture in psychological development. We have touched briefly on the main features of this approach in the present chapter and they have been evident in the questions we raised and conclusions we drew

from the research reviewed throughout the book. At the heart of this approach is a commitment to pursue a program of research that attempts to integrate in theory and in practice the knowledge and methods of *both* anthropology and psychology. It implies that the truly challenging questions about human thought and its development will only yield to inquiry when investigators bring to bear on them all the tools that the separate sciences have developed for studying man-in-his-culture.

A Final Word: Experimenters and Their Points of View

Discussions of scientific method in psychology emphasize the need to put psychological theory and experimentation on a rigorous, scientific plane. A good deal of attention is devoted to *method*—the rules by which experiments are designed, subjects are selected, stimuli are equated, and data are analyzed. This book has contributed its fair share to such a discussion.

Nonetheless, we hope it does not escape the reader's attention that in this last chapter, extraordinary weight has been placed on the role of the experimenter's point of view in insuring the success of the scientific enterprise. The really influential psychological theories dominating cognitive psychology today (Piaget's theory is a good example) have not gained their influence through the prediction of one or two unusual phenomena, or the specification of a single, unanticipated relationship. Rather, they have convinced us by repeated successes in a wide variety of situations within our own culture. But carrying such theories overseas without some awareness of their cultural roots and their very real limitations, even in the cultures in which they arose, carries with it the risk of experimental egocentrism—mistaking as universals the particular organizations of cognitive skills that have arisen in the historical circumstances of our own society, and interpreting their absence in other cultures as “deficiency.” Perhaps this risk may never be entirely overcome until psychological science in non-Western countries becomes further advanced and generates its own theories and research methods—which can be tested on us!

It is well to remember that all of the processes and problems that we encounter in the people we study apply to us as experi-

menters. The future of the study of culture and cognition will depend upon our abilities to organize our own functional systems to give a comprehensive and coherent account of the intellectual behaviors of those whom we so provincially refer to as “our subjects.”

I know how to begin the old mat pattern but I do not know how to begin the new.

An old Kpelle proverb

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