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MICHAEL COLE and SYLVIA SCRIBNER

INTRODUCTION

Speculation and disagreement about the influence of cultural environment on the development of the mind has characterized the social sciences since their inception in the nineteenth century. Within both anthropology and psychology, it has been possible to find defenders of the idea that children raised in nontechnological societies fail to develop "higher order" mental skills; it is just as easy to find adherents of the position that all apparent differences mask underlying, universal equivilences (see Boas 1911, LeVine 1970, Mead 1964, Scribner and Cole 1973).

While we have also engaged in such discussions (Cole and Bruner 1971, Cole and Scribner 1974) we have grown increasingly uneasy about the nature of the debate. When asked to summarize our current knowledge about the "socialization of the intellect" by editor Schwartz, we found that conceptual problems surrounding the na-

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ture of data and its relation to theory prevented us from providing more than a catalogue of facts for which there was no agreed-upon interpretation by anthropologists and psychologists currently working on this question. Consequently, we have decided to examine the presuppositions that anthropologists and psychologists bring to the enterprise, the observations they are led to make, and the kinds of inferences about "socialization of the intellect" that have become accepted within each discipline. We point out some weaknesses inherent in current formulations, and offer a framework designed to produce a common ground for future explorations of this question.

As an illustration of the difficulties that concern us, we have chosen a brief interchange between Margaret Mead and Jerome Kagan which took place at the convention of the American Association for the Advancement of Science in the fall of 1972.

Professor Kagan had just completed a report on psychological research in Guatemala. Working with young infants on a task that assessed their responses to stimulus novelty, Kagan had found that rural Guatemalan children were several months retarded in the "cognitive function of activation of hypotheses" at around one year of age. Working with older children, using recall of familiar objects, recognition memory, an embedded figure test, and other standard procedures for assessing cognitive development in the United States, Kagan asserted that there were only minimal differences between Guatemalan and American children by the age of 8 or 9 years. Taken together, the pattern of results led Kagan to conclude that "infant retardation seems to be partially reversible and cognitive development during the early years more resilient than had been supposed (Kagan and Klein 1973:957)."

Following Professor Kagan's presentation, Professor Mead took the floor to comment (we quote from memory): "Why has it taken psychologists so long to discover what anthropologists have known all along?" To which Kagan replied, "I guess we're a little slow."

It seemed to us at the time, and seems so still, that in the laughter which followed this charming interchange, an important point was overlooked. In a very basic sense, Professors Mead and Kagan were talking past each other. Not only had Kagan failed to discover what Mead already knew, they did not (as anthropologist and psychologist respectively) know the same things. They were only, on this occasion, expressing their shared opinion that Guatemalan peasants are mentally competent. Unexplored were possible, even probable disagreements about the suitability of the data for such a conclusion and the implications of other research showing that performance among Guatemalan or similar peoples were not up to "American standards" on tasks very like the ones that Kagan used. The crucial problem arises just from the fact that data do exist which seem to point to conclusions antithetical to Mead's and Kagan's. Such data have been gathered by both anthropologists (Gladwin 1970) and psychologists (Greenfield and Bruner 1969) who conclude that there exist fundamental differences in the thought processes of people socialized into different cultures, differences that are related directly to such theoretical constructs as "level of cognitive development." How are such data and conclusions to be interpreted?

We believe the general failure of anthropologists and psychologists to share the same definitions, facts, and theoretical constructs is a fundamental impediment to our understanding of the relation between culture and the development of cognitive processes; all the more so because this failure often goes unnoticed. Because they share a common interest and a common terminology, psychologists and anthropologists tend to make the assumption that they share a common topic of inquiry—each in his own way pursues the link between social experiences and cognition. We believe this assumption is unfounded on both sides of the equation: anthropologists and psychologists do not mean the same thing when they speak of cognitive "consequences"; they do not agree on the characteristics of culture that are potential "antecedents"; and they distrust each other's method for discovering the links between the two. As long as these underlying differences remain "unnoticed," there is little hope that they will be overcome. In this paper, we examine some of the basic differences between the two disciplines as they have been expressed in theory and practice and consider whether and how they might be resolved. While we are concerned with the differing interpretations of both culture and cognition, our major attention is directed to the way in which the two disciplines treat cognition. For reasons that we hope become clear in the course of the discussion, we believe that unless there is some agreement on what "cognitive consequences" we are studying, there are no guide-lines for deciding what aspects of culture are relevant to the search for "critical socializing" experiences.

The disagreements between anthropologists and psychologists on the nature of cognition and how it is to be studied are interrelated in many complex ways, but we find it useful to consider them in terms of three dichotomies: (1) emphasis on content or process in defining cognition; (2) choice of naturally occurring or contrived situations as contexts for data collection; and (3) reliance on observational or manipulative research techniques. Some investigators cross over from one pole to another, but, in general, anthropologists emphasize content, natural occurrences, and observation, while psychologists stress process, contrived situations, and experimental control. As we shall see, the way in which the investigator defines cognition influences his choice of research method and, in turn, his choice of method determines the data he collects and thus the inferences he can make about the nature of cognition. Although we are presenting these choices as dichotomies because they are often posed that way in partisan discussion, we hope to show that the oppositions are more apparent than real and that both approaches can be integrated in the research enterprise. In the final section we describe and criticize our early attempts at an anthropological-psychological integration and suggest directions that might be taken in future research.

AN EXAMPLE

How the differing approaches work out in practice can be illustrated by the hypothetical responses of psychologists and anthropologists to an actual piece of cross-cultural research on cognition. We have chosen for this example a recently published study by Daniel Wagner (1974) in Mexico entitled, "The Development of Short-term and Incidental Memory: A Cross-Cultural Study."

Wagner's task required his subjects to recall the position of one of 7 familiar items in a linear array. The location of the to-be-recalled item varied from trial to trial. The items (pictures on cards) were shown one at a time for two seconds each, and then turned face down. After all 7 had been presented, a duplicate of one item in the row was shown to the subject and he (she) had to point to its location. At two seconds per item, and taking into account the time required to turn over the cards, the longest interval between presentation and recall test was about 20 seconds, the shortest about 3 seconds.

We selected this work for several reasons. First, it is a technically fine piece of research (Cole was present when the work was begun and knows how carefully and painstakingly Wagner chose his stimulus material, worked on his instruction, trained his assistant, and observed each data collection session). Second, the research was carried out among Mayan people living in Yucatan not far from Kagan's site in Guatemala and it investigated memory, one of the "basic cognitive functions" Kagan studied.[1] But Wagner's conclusions seem diametrically opposed to Kagan's:

> Higher-level mnemonic strategies in memory may do more than "lag" by several years—the present data indicate that without formal schooling, such skills may not develop at all (Wagner 1974:395).

This conclusion is based on several facts, two of which suffice for this discussion. (1) Overall recall of the target item improved almost entirely as a function of number of years of education, not as a function of age (subjects ranged in educational experience from 1 to 15 years, in age from 7 to 35 years). (2) Analysis of the function relating time-of-delay between presentation and test to amount recalled showed the pattern of results associated with application of rehearsal strategies (Wagner's referent for "higher level mnemonic strategies") *only* for those subjects (adults or children) who attained more than the fifth grade in school.

INTELLECT: CONTENT VS. PROCESS

Developmental psychologists might find Wagner's results interesting for a variety of reasons. Age and educational experience are hopelessly confounded in the United States. Wagner provides evidence that the traditional developmental function seen in the work

1. While Kagan's own research might seem the most appropriate vehicle for this discussion, several factors, including those given in the text, argue against making his data the focal point of the presentation. Paramount is that while we share Kagan's belief in mental competence of Guatemalan peasants, we do not believe that this conclusion follows from the published reports (Kagan and Klein 1973, Kagan, Haith, and Morrison 1973). Since classification of our interpretation would require a lengthy discussion located almost entirely within the domain of psychological theory and data analysis, we have chosen a less controversial study to illustrates paradoxes that are interdisciplinary in scope.

of Hagan (1971) and Flavell (1970) on mnemonic development may be more of an "educational development" than a maturational one.

Cross-cultural psychologists would add Wagner's results to the growing list of instances where formal schooling seems to affect intellectual performance (cf. Cole and Scribner 1974, for a review). They would also look carefully at the procedures and groups used to derive hypotheses as to why formal education makes a difference in Wagner's work and not Kagan's. Kagan did not vary independently the age, educational level, and urbanization of his subjects. Does his finding of "no difference" in memory skills have to do with marked effects of schooling? Is the difference to be associated with the age of his subjects (his oldest children were 11–12 years old while Wagner's effect occurred at the age/education level that averaged 14–15 years and 8 years of education)? Do performance differences have to do with the different tasks Kagan and Wagner used to diagnose memory skills,[2] or differences in the cultures of Yucatecan and Guatemalan Maya?

There are clearly a great many questions psychologists might raise about this research and its interpretation. But there is one question they are unlikely to deal with: no matter how different their theoretical persuasions, most psychologists will take it for granted that this research is, more or less adequately, "measuring" population variations in memory *processes*. They will share the basic assumption that it is possible, in principle and in fact, to examine memory processes across diverse social groups with relatively little concern for the content of what is being remembered.

We do not mean to imply that psychologists do not put a good deal of time and care into selecting their experimental material. Especially in recent years, stung by accusations of ethnocentrism and cultural bias, investigators have exercised considerable ingenuity in devising what they consider to be "culturally fair" materials and in assuring that materials are equally "familiar" to the cultural groups being compared (see Glick 1975 and Berry 1969, for interesting discussions on comparability in cross-cultural experimenta-

2. It is unlikely that the nature of the tasks is the locus of apparent differences in these authors' conclusions. We have recently completed a series of studies in Yucatan, including a free recall study quite similar to the one used by Kagan, but found education-dependent results parallel to those obtained by Wagner (Sharp and Cole 1974).

tion). But this very effort contains within it the notion that the content of the intellectual task can be neutralized and in some sense held "consant" across groups so that performance differences can be assumed to reflect differences in "pure process."

To be sure, present-day psychological investigators are showing that for many intellectual tasks there is a relationship between the nature of the task material and the operations that are brought to bear on it (Price-Williams, Gordon, and Ramirez 1969, Irwin and McLaughlin 1970, Cole *et al.* 1971). In *practice,* however, most psychologists still tend to interpret performance with a given set of materials as revealing some fixed set of "content-free" processes within the subject population (cf. Berry and Dasen 1974). This is what they *mean* when they talk about cultural variations in cognition.

But psychologists are by no means alone in finding it difficult to deal with content-process relationships in cognition. Anthropological theorizing about cognition in some ways presents a mirror image to what we have just described. Many anthropologists will criticize the Wagner experiments because Wagner attempts to come to conclusions about the memory of Yucatec Mayans using material that is artificial, meaningless or of no "interest" to the Mayans (despite his use of materials intended to have the opposite characteristics). A fair test of memory, they may argue, requires that each cultural group be tested with materials and tasks that are meaningfully organized within that society. While this argument recognizes the legitimacy of investigating process when content is taken into account, in *practice,* most anthropologists tend to ignore possible process variations and attribute to differences in "content" all observed cultural group differences. If the Iatmul display a prodigious memory for totemic names (Bateson 1958) and the Swazi for cattle transactions (Bartlett 1932), they make the assumption that the "memories" of Iatmul and Swazi are the same; they just remember different things. What the majority of anthropologists mean by cultural variations in cognition are differences in thinking or memory *content.*

Levy-Bruhl remains the classic example of the ambiguity with which terms referring to cognition—terms like "thought" and "perception"—are handled in anthropological literature. What are we to conclude when he asserts that:

primitives perceive nothing in the same way we do. The social milieu which surrounds them is different, the external world they perceive differs from that which we apprehend (1966:30).

or

primitives perceive with eyes like ours, but they do not perceive with the same minds (1966:31).

Here, and in many places in his writing, Levy-Bruhl oscillates between an insistence that he is talking only about "collective representations" (culture-wide belief systems or thinking *content,* in the context of this discussion), and examples that imply that he is talking about specific cognitive *processes* operating within individuals.

This slipping from content-to-process was roundly criticized by Boas (1911) many years ago; he pointed out that one cannot legitimately infer psychological processes directly from an examination of culture-wide beliefs and attitudes. Because Levy-Bruhl's conclusions have been widely repudiated, it might appear that Boas's caution was taken to heart by succeeding generations of anthropologists and that we are beating a dead dog. But consider this statement from a leading contemporary anthropologist whose views on cognition appear to be the direct antithesis of Levy-Bruhl's: "Both science and magic require the same sort of mental operations and they differ not so much in kind as in the different types of phenomena to which they are applied" (Levi-Strauss 1966:13). Here again, what is stressed are differences in content between two thought systems ("phenomena to which they are applied") and the same unwarranted leap is made from an analysis of *cultural* systems to a generalization about individual mental processes. We agree that Levy-Bruhl's conclusions about mental processes are unfounded; but so are Levi-Strauss's.

The problem arises in part from the limitation of dealing with thought as content. But further problems are posed for psychological and anthropological theorizing because the focus on content or process influences choices for our two remaining dichotomies.

CRITICAL SITUATIONS: NATURAL VS. CONTRIVED?

Let us return to our example and the discussion of what the Wagner experiment tells us about memory development. Just as psychologists assume that this experiment taps some underlying process, so they tend to assume that this *same* process is operative outside of the experimental situation. The motivation for the research, after all, is to shed light on the role of various experiences (school, literacy, technology, culture) on the development of memory skills of Yucatecans or Guatemalans in general—not just on the skills of individuals serving as subjects in a specialized experiment. We think it is safe to say, however, that attempts to reach general conclusions from this experiment represent the kind of psychological theorizing that would come under attack by anthropologists.

Robert Edgerton succinctly summarized the position of many anthropologists by saying that the roots of anthropology's anti-experimental convictions

> are deep in anthropological history. . . . At heart, anthropologists are naturalists whose commitment is to the phenomena themselves. Anthropologists have always believed that human phenomena can best be understood by procedures that are primarily sensitive to context, be it situational, social or cultural (1974:63–64).

With this commitment to phenomena as they naturally occur, it is easy to understand why anthropologists might challenge the generalizability of the Wagner findings. They may be inclined to interpret the results as reflecting only differences in the way various people respond to the demands of artificially contrived situations.

For example, focusing on the performance difference between Wagner's subjects who went to school and those who did not, the anthropologist might conclude that going to school helps people to interpret the demands of nonnatural experimental tasks. In effect, this line of analysis would lead to a conclusion such as: The less educated subjects were sufficiently unfamiliar with such tasks that the instructions failed to communicate the task demands clearly. The difference in performance would then be considered a reflection of comprehension of the task, not of short-term memory skills.

This analysis might be bolstered by reference to numerous everyday, naturally occurring activities in which uneducated subjects demonstrate adequate short-term memory. For example, it might be pointed out that interpreting complex grammatical phrases with embedded clauses or carrying on a normal conversation are both contexts that require and produce short-term recall many times a day in all Yucatecan adults. Knowledge of Yucatecan culture might lead to suggestions of other contexts in which short-term recall must be at work, such as the production of complicated patterns in hammock weaving. How is the evidence from such naturally occurring situations to be squared with experimental evidence? What do we conclude if the two sources of evidence seem to conflict? Before attempting to answer those questions, we need to consider our third dichotomy.

OBSERVATION OR EXPERIMENT

In addition to stressing the need to investigate cognition in context, the anthropologist values what Edgerton describes as "unobtrusive" methods of research:

> Our methods are primarily unobtrusive, non-reactive ones; we observe, we participate, we learn, hopefully we understand. . . . This is our unspoken paradigm and it is directly at odds with the discovery of truth by experimentation (1974:63–64).

Here is yet another source of anthropological attack on the generalizability of experimental results: group differences found in experiments simply reflect differences in the readiness with which individuals of differing cultural, social, and educational backgrounds enter into the "subject role" and the behaviors appropriate to this particular social encounter. The experiment, by its very nature, changes the phenomenon under investigation.

Keeping these anthropological criticisms of the experimental method in mind, let us reverse the case and consider the shortcomings of naturalistic observation as a research methodology. We will not argue here the validity of Edgerton's characterization of anthropological method, although it should be recognized that he is expressing the ideal, or perhaps the ideology, not the reality of anthropological research. Intensive interviewing of selected informants is not unobtrusive and is rarely nonreactive. Our purpose, however, is to illustrate the restrictions of the unstructured observational method for drawing inferences about individual cognitive processes. These restrictions are recognized and analyzed with unusual clarity by Bateson (1958) in his discussion of memory skills among the Iatmul. Bateson picks memory as his topic because his ethnographic work revealed that learned men among the Iatmul are veritable storehouses of totems and names that are used in debating. Adding the number of name songs belonging to each clan, the number of names per song, and the songs from other clans that some men knew, Bateson estimated that such people must carry ten to twenty thousand names around in their heads. He takes this as prima facie evidence of highly developed memory capacities.

So far, Bateson's discussion could have come straight out of Levy-Bruhl who, along with many others, claimed exceptional memory capacities for nonliterate peoples. Bateson, however, was a field worker conversant with work in the experimental psychology of memory of the period, so his analysis did not stop here.

Bateson went on to provide an early test of a *psychologically derived* hypothesis about the relation between culture and memory. Specifically, he provided convincing evidence against Bartlett's hypothesis (1932) that preliterate peoples remember by a rote process. He did this by recording the order in which informants offer mythical names on different occasions and by observing that, when asked about past events, the Iatmul do not have to describe a series of chronologically related events to give a meaningful reply. He also studied the techniques of debating and easily rejected the notion that people call in their store of names in any rote fashion. He even provided evidence about the deliberate use of mnemonic devices in debating.

But then Bateson was stuck, which he clearly recognized:

> though we may with fair certainty say that rote memory is not the principal process stimulated in Iatmul erudition, it is not possible to say which of the higher processes is chiefly involved (1958:224).

He goes on to offer some hypotheses about plausible memory mechanisms underlying the Iatmul achievements, but these he cannot be sure of on the basis of ethnographic data alone. As he puts it:

> I have little material which would demonstrate the methods of thought of individual natives, and therefore depend almost entirely upon the details of the culture, deducing therefrom the patterns of thought of the individuals. Ideally it should be possible to trace the same processes in the utterances of informants and in individual behavior in experimental conditions as well as in the norms of the culture (1958:229).

Bateson's work clearly illustrates the importance of ethnographic inquiry in exploring culture-thought relations. It illustrates the way in which strategic observations can rule out a hypothesized process, and it suggests situations in which remembering is an important activity. It just as clearly illustrates that a purely observational approach encounters specific limits in accounting for the way in which cultural demands influence thought processes.

As we examine these oppositions between anthropological and psychological approaches to intellect, the limitations of each approach taken by itself become apparent. It is painfully obvious that each discipline rests on a very narrow and specialized data base from which it makes overly broad and often improper generalizations. The dangers in this position were clearly specified by Nadel:

> unless the relations between social and psychological enquiry are precisely stated, certain dangers, all-too-evident in anthropological and psychological literature, will never be banished. Psychologists will overstate their claims and produce, by valid psychological methods, spurious sociological explanations; or the student of society, while officially disregarding psychology, will smuggle it in by the back door; or he may assign to psychology merely the residue of his enquiry—all the facts with which his own methods seem incapable of dealing (1951:289).

The case for substantive collaboration between psychologists and anthropologists could not be made more clearly. Both groups want to extend the power and range of their theories about the intellectual consequences of differing sociocultural experiences. If each has a limited view of the problem and a limited range of techniques, some combination of resources is needed to accomplish the goal.

But can this be more than a prescription to "do good?" We think so. The dichotomies we have described are traditions that have grown up in practice but are not intrinsic characteristics of the scientific enterprise.

People in both disciplines have pointed to the weaknesses of this dichotomous thinking and have argued that there is no inherent incompatibility between content and process, observation and experiment—and by extension, anthropology and psychology.

Since we have been considering culture and memory, it is sobering to recall that the psychologist Sir Frederick Bartlett (1932), a pioneer in this field, investigated both the content and process of memory in studies conducted a half century ago. He showed how instrumental the "socially dominant interests" of the culture are in determining what individual members of the culture remember. It is no accident that Swazi have "good" memory for cattle transactions and the Iatmul for totemic names, and psychologists wishing to understand memory cannot proceed as if these relationships are arbitrary. Nadel (1951:292) made a similar point in a more general vein. In practice, he said, it is often difficult if not impossible to separate thinking-as-content from thinking-as-process. The psychologist examining any mental mechanism is of necessity examining a mechanism normally operating with material given in society and culture, and he cannot get away from such "living contents" even in the artificial isolation of an experiment. Similarly, if anthropologists are concerned with how "living contents" come into existence and change over history, they need to understand what operations ("processes") individuals bring to the material that is culturally given.

There has also been growing recognition that the dichotomy between observation and experiment is unfounded. Bateson's study of Iatmul memory skills is a fine example of the complementary nature of the two techniques in anthropological practice. The case for a complementary relation between experiment and natural observation within psychology has been argued by a distinguished investigator of comparative behavior whose own research elegantly combined the two research approaches. Schneirla (1972) urged investigators to think of field and laboratory research as basically similar, each making different aspects of behavior available for analysis. "Field work may be thought of as furnishing opportunities for investigation not initially available under laboratory conditions, to be gained through access to the complete natural phenomena . . . the laboratory may be considered as a limited and controllable field in which isolation and quantitative measurement of selected aspects of behavior can be made. Properly speaking, in terms of the logic of science, there is really no experimental method as distinct from observation" (1972:3–4).

Even after clearing away "differences in principle" we are still left with the problem of how integration can be achieved in practice. It would be helpful at this point if we could present a piece of research showing what "ethnographic psychology" actually looks like. Unfortunately we have not been able to find such a model. Our own work on culture and memory was an early attempt to combine anthropological and psychological approaches but it suffered from many of the same shortcomings we have documented here. Although this work has been described in detail elsewhere (Cole *et al.* 1971, Scribner 1974) we present it briefly to show the problems that arose as we struggled to interpret our results and the modifications in research strategy that we developed as a result.[3]

Our studies of memory among the Kpelle began with the expectation based upon anthropological folklore that nonliterates would perform better on a memory task (have more highly developed memory skills) than literates. We not only derived our hypothesis from anthropological literature but we took great care to develop our experimental materials (we were using word lists) from verbal responses given by a representative sample of Kpelle men and women on standard linguistic elicitation tasks. Even with the use of such materials, our initial studies of free recall, using the standard experimental techniques, failed to confirm the notion of "superior memory" among nonliterates. Quite the reverse. The free recall performance of Kpelle rice farmers was such that were we to make simple performance-process inferences, we would have concluded that they were virtually retarded. Furthermore, we might have been led to the conclusion that without schooling to a point where literacy is achieved, higher mnemonic skills do not develop. This would have followed because Kpelle children exposed to about eight years of school, like Mayan children, exhibited recall performances that are associated with "higher mnemonic abilities" in the psychological literature.

We did not jump to such a conclusion. We were disturbed by the discrepancy between our experimental results and the anthropological folklore which motivated them. We were inclined to share the anthropologist's skepticism about the representativeness of performance in an experimental situation and became convinced that no reasonable conclusions about group differences could be made on the basis of results of a single experimental performance. It seemed

3. A detailed example of a combined naturalistic and experimental approach to the study of cognitive processes is presented in the final chapter of Cole and Scribner *Culture and Thought: A Psychological Introduction* (1974).

to us that our task, rather than our subjects' lack of ability, might be the source of poor performance. At the time we thought the solution lay in substituting for the single experiment a series of experiments in which the nature of the task requirements was systematically modified.

So we set out to modify our initial free recall task in such a way that "normal performance" (e.g. the performance we had come to expect from college sophomores) was achieved. From the psychological literature on free recall, we borrowed such manipulations as presenting concrete objects, emphasizing the organizing principles inherent in the materials, varying those principles along both taxonomic and functional lines, paying people to do well, and many others.

Although these studies produced statistically significant variations in some cases, we were far from our goal of observing really good, let alone remarkable recall. In addition to casting about for alternative experimental procedures, we began to ask ourselves about occasions when Kpelle people would be likely to have to recall lists or sets of things as a more-or-less isolated activity. We imagined a wife going off to market who, being illiterate, could not prepare a shopping list. We discovered a Kpelle game where children had quickly to recall the name of many leaves. At some point we recognized that the folk stories we had been collecting for several years were remembered products, albeit not lists of isolated "things." Our seat-of-the-pants "ethnography" of Kpelle remembering, when combined with experimentally derived results, began to produce some rather dramatic changes in performance; under a variety of conditions we began to observe organized, and in some cases, high levels of recall among noneducated Kpelle.

The set of circumstances which changed performance was quite heterogeneous: embedding to-be-recalled items in pseudofolk stories quite clearly indicated that recall was influenced by the organization of the story; associating items with concrete objects (chairs) increased organization and recall as did requiring people to recall one specified category at a time; paired associate learning was clearly influenced by the list structure to a much greater degree than free recall of the same list.

These findings led to a reformulation of the factors underlying recall performance in a range of tasks of which ours were a subsample. We were led to hypothesize that the tasks that produced good performance shared the characteristic that the potential structure of the set of materials was made explicit and the task itself induced the subject to make use of this structure.

Because of the time limitations on research projects (ours actually lasted four years, longer than most) we were not really able to test our inductive hypothesis about the locus of performance differences between educated and noneducated Kpelle. Our ideas about "providing structure" have been shown in the interim to dovetail neatly with a variety of developmental analyses of recall (Brown 1974) but we only vaguely grasped the issue when we had to "close down shop." Fortunately, one study following up these ideas has been completed by Scribner (1974). Her subjects were first required to arrive at a *stable* structuring of to-be-recalled items. Although different populations of subjects structured the materials in different ways, recall was generally good and organized in a manner consistent with the structure provided by the subjects themselves.

Our work then had led us to the point where we could specify with some confidence what features of the task and material controlled good performance. But as soon as we attempted to account for differences in performance among the Kpelle or between Kpelle adults and those of industrialized cultures, we were at a loss. Why did "schooled subjects" among the Kpelle perform so differently from those individuals who had never gone to school? Why did our devices making the structure of the material explicit work so well with traditional farmers? No matter how we analyzed our experimental tasks, we could not get from them to any understanding of the culturally determined experiences that might account for the different deployment of memory skills that we observed among the Kpelle. Nor were we much closer to bridging the gap between anthropological reports of everyday memory feats and our experimental findings. It now seems apparent to us that we cannot account for performance in our experimental tasks until we learn a great deal more about the kind of memory-requiring tasks or situations that Kpelle people (or any people) normally encounter and how the demands of the experimental task compare to the demands imposed by these everyday situations (see Scribner 1975).

For example, suppose that we sought to pursue the line of reasoning that arose during our work on free recall among the Kpelle where we imagined a nonliterate Kpelle wife going off to market. Aside from what our imaginations can tell us, we really know next to nothing about this mundane remembering occasion.

Let us suppose that Kpelle women really do check their larders and then set off for the marketplace. Do they commit the needed items to memory before leaving? Or do they wait until browsing through the seller's stalls to "be reminded" of what they need? Our psychological analysis emphasizes the difference between actively rehearsing to-be-recalled materials and using ready-made recall cues. Which activity does "remembering what groceries to buy" really entail for the Kpelle housewife?

If we are to get beneath such global variables as "urbanization" and "literacy," many more questions such as these need to be posed for a variety of situations where people seem to have to rely on "remembering" in a relatively well-defined observable manner. We need, in effect, an ethnography of a specific cognitive activity, the implications of which are then tested by a variety of observations, including experimental ones. It might turn out that the prominent economic and social activities that traditional Kpelle engage in rarely, if ever, require deliberate, before-the-fact remembering. Or it might turn out that only certain specialists, or all people only on special occasions need engage in such activities. Whichever the case, we would have to be certain to use our analysis of indigenous occasions for remembering as a point of contact between psychological and ethnographic descriptions. We only began such work in our research, and as a result, our study of culture and memory shares the limitations of other cross-cultural research of its kind.

Relatively early in our thinking on this problem we were led to remark that we found it useful to treat experiments as specially contrived situations for the manifestation of cognitive skills. In the light of our subsequent experiences and Schneirla's formulation, it seems to us now that the term cognitive *skills* was gratuitous and that experiments are best seen as specially contrived occasions for cognitive *activity*—a subset of occasions provided in every society for the development and manifestation of intellectual capacities.

In this view both the anthropologist and the psychologist are dealing with the same "stuff"—cognitive activities—and naturalistic observations and experimental observations are both part of the single enterprise of analyzing how this activity is shaped and organized by the features of the particular situation in which it occurs. For the psychologist, this poses the somewhat awesome problem of developing new techniques for studying cognitive activities as they unfold in daily life. But it poses a challenge to the anthropologist as well. There is precious little in the anthropological literature to guide a psychologist who was convinced of the importance of studying cognition in "real-life" situations. Neither an analysis of belief systems nor a sophisticated componential analysis of kinship terms is likely to carry us very far. But if the ethnographer took as his task the analysis of cognition as a *specific set of activities* engaged in on *specifiable occasions* for reasons deducible from his *social* theory, a real rapprochement is possible. If such a reciprocal approach were worked out between two scholars, or within the head of one, their *common concern would be cognitive activity in a variety of settings analyzed in varying degrees of detail.* "Cultural differences in memory" would then refer to cultural variations in the organization of different kinds of remembering tasks, the intellectual activities that these tasks require, and consequently, the kinds of "memory skills" that members of different cultural groups, or specialists within each group, could be expected to develop.

At the present time, we have only flawed or partial demonstrations of how a combined ethnographic-psychology of cognition might look. But we think that a sharp awareness of our current limitations, augmented by a clearer vision of our goal, can bridge "East" and "West," leaving both richer in the process.

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