

Mind and social practice

Selected writings of Sylvia Scribner

Edited by

ETHEL TOBACH

American Museum of Natural History

RACHEL JOFFE FALMAGNE

Clark University

MARY B. PARLEE

Massachusetts Institute of Technology

LAURA M. W. MARTIN

Arizona Museum of Science and Technology

AGGIE SCRIBNER KAPELMAN

National Labor Relations Board



CAMBRIDGE
UNIVERSITY PRESS

10 Situating the experiment in cross-cultural research

Of the many methodological problems in cross-cultural research I have selected the experiment as the point of emphasis because it seems to me that the role of the experiment needs to be clarified if we hope to resolve a central dilemma in the field of culture and cognition.

In a sense, the experiment has created the dilemma. In the last several decades, there has been a substantial increase in the number of cross-cultural psychological studies of cognition in which the principal research tool has been the experiment or a task derived from an experiment. In the same period, there has been an upsurge of interest in cognitive phenomena among anthropologists and the initiation of new lines of research based principally on the methods of field observation and interview.

Ordinarily, this shared interest and intensive research effort by two disciplines should promote a more rapid growth of knowledge and understanding. This seems to have been the case in the field of culture and personality, which also arose as a specialized domain of inquiry sitting astride the two disciplines of psychology and anthropology. But in culture and cognition, the multiplication of psychological and anthropological studies has not yet resulted in an integrated body of data or in a set of unifying constructs. On the contrary, it has brought sharply into focus the discontinuities in the evidence each of these sciences presents of cultural variations in cognition.

This essay originally appeared in *The Developing Individual in a Changing World*, vol. 1, *Historical and Cultural Issues*, ed. K. F. Riegel and J. A. Meacham (Berlin: Walter de Gruyter, 1976), pp. 310–21. Reprinted with the permission of Walter de Gruyter.

Discontinuities between anthropology and psychology

I will illustrate the problem with a few sketchy and admittedly oversimplified examples. Many carefully conducted experiments using Piagetian tasks have found a considerable number of *adults* in nontechnical societies failing to show behavior associated with the possession of logical structures of intelligence assumed to be characteristic of 8 to 12-year-old children in technological societies (Dasen 1972 reviews many of these studies). Yet anthropologists by means of new analytic techniques are identifying complex logical structures underlying conceptual systems within these cultures (Wallace 1962, for example). Moreover, ethnographic studies reveal that individuals within these cultures engage in elegant processes of inferential reasoning as they go about the everyday business of settling disputes (Gibbs 1962), or the more exotic business of bargaining on the terms of their participation in some Western-inspired research project (Kulah 1973).

Evans-Pritchard (1963), Albert (1964), Bellman (1968), and other anthropologists have documented the complex communication skills involved in patterns of verbal exchange among the Zande, Burundi, and Kpelle peoples of Africa. Cole and his associates, on the other hand (1969), found that Kpelle adults performed poorly in an experimental situation that was specifically designed to tap communication skills.

As a final example, anthropological reports of feats of memory on the part of nonliterate people in traditional societies date back to as early as the seventeenth century (Evreux 1864). But ever since the 1920s, psychologists using methods and procedures developed in the laboratory to test memory performance have failed to confirm these generalizations about extraordinary mnemonic powers.

These examples indicate that the divergences in data and generalizations between anthropology and psychology on various topics of cognition generally run in the same direction; contemporary anthropological evidence highlights the commonality in the cognitive skills of populations in technological and nontechnological societies; psychological evidence, for the most part, emphasizes either the absence of certain skills or the lower levels of skill of nontechnological peoples. The problem and dilemma is how to reconcile these two sets of data and interpretations.

One response to this problem has been the denial that there is any need for reconciliation because the two research approaches and two sets of evidence really speak to different questions. Thus, some psychologists feel that ethnographic descriptions of performance in naturally occurring situations are useful for many purposes but have little to contribute to an understanding of the basic psychological processes underlying performance in different cultures.

They feel that such knowledge can only be generated by the laboratory experiment which permits the isolation and systematic manipulation of various components of the performance.

Critiques of experimental method

Some anthropologists, on the other hand, question whether the laboratory situation yields findings that have any trans-situational generality at all. One objection is that experimental materials, tasks, and procedures developed in industrial societies are ethnocentric and culturally biased. Others go beyond this in asserting that the experiment itself, as a context for the elicitation of behavior, has no ecological validity in the cultures to which it has been transported. Whatever limitations are imposed by the artificiality of the experiment in the societies in which it originated, they argue, are magnified many times over in traditional societies whose people lack experience with test-like situations. Granted that experimental methods make it possible to analyze processes underlying performance. But if the performance itself is nonrepresentative and distorted, what can be learned from such an analysis that has any relevance for the understanding of cultural determinants of behavior?

This position, which is essentially an attempt to draw a line between what anthropology and psychology can tell us about cognition, does not seem to be a very fruitful way of handling the problem. Psychologists would hardly be willing to accept the conclusion that experiments can do little to illuminate the problem of cultural influences on cognition. And anthropologists, I am sure, would be equally resistant to the notion that evidence of cultural skills is not relevant to an understanding of individual cognitive processes. But even if, as psychologists, we were ready to ply our narrow trade, we would still have to take into account the questions that have been raised about the use of the experiment as a tool in cross-cultural research. It certainly seems precarious to pursue ambitious investigations that seek to compare cognitive processes among populations of *different* cultures, if we cannot reconcile the comparative evidence of psychological and anthropological studies of cognitive processes within the *same* culture.

To meet some of these criticisms, cross-cultural psychologists have devoted considerable attention in recent years to reducing sources of cultural bias in the experiment. The idea that an experiment consists of a fixed set of materials and operations that can be taken abroad like a piece of luggage has been replaced by an emphasis on the need to adapt features of the experiment to the culture in which the research is being carried out. The contemporary view, as Glick (1975) puts it, is that 'The logic of comparative study involves the testing of people in a *comparable* (note, not *identical*) manner'. Lloyd (1972) agrees that the investigator's concern is not to duplicate the original experiment but to 'ensure that it will produce data in the new setting which can be compared

with that collected in the original Euro-American situation' (p. 21). Frijda and Jahoda (1966), Berry (1969), and others have made important contributions toward solution of problems of comparability in materials, procedures, experimenter-subject communication, motivations, etc.

While these are important, they leave untouched the perhaps more fundamental criticism that the experiment, by its very nature, rather than by this or that feature of it, cannot be considered an equivalent or comparable performance situation in all societies. To handle this criticism, we have to go beyond the consideration of specific features of the experiment and explore what the experiment represents as a context for the manifestation of cognitive skills within the traditional cultures to which we carry it. What are the naturally occurring contexts in the culture in which these same skills are elicited? How does the experimental paradigm compare to these naturally occurring situations? Are there situations similar to the experimental situation (such as test-taking in school, for example) that individuals encounter in some cultures and not in others? (See Scribner and Cole, 1973.) These are some of the questions we would want to ask simply to meet the criterion of establishing comparability between experimental investigations in one culture and another.

The experiment as an unnatural situation

But this is essentially the same set of questions that arises when we confront the problem of comparing and integrating data from psychological experiments with data for field research within *one* culture. To relate the two sets of data to each other we are led to ask questions about the contexts in which the behavior we are investigating was elicited. When we observe a Kpelle child trying to memorize word lists in a free recall experiment and when we observe him trying to memorize the names of nine leaves in a singing game on the road behind his house, we are in each case studying the act of memorizing as it occurs in a given situation with a given set of features. Looking upon the experiment this way it makes sense to ask about the similarities and differences between these situations and, most particularly, about the similarities and differences in the *cognitive demands* they make upon the child. Can these differences be characterized in any generalized or formal way? If we can identify dimensions along which the experimental situation can be compared to the naturally occurring situation we will have a better possibility of achieving some integrated interpretation of performance in the two situations.

This approach suggests that it might be valuable to make the experiment itself an object of cross-cultural inquiry. Our aim would then be to identify certain distinctive features of the experimental situation as a context for cognitive behavior and to fit it into the range of situations in the culture in which this behavior is manifested.

This is a very general statement and an ambitious manifesto. I have no

blueprint to propose and no developed line of investigation to use as a model. But some contemporary lines of research suggest certain techniques that might be useful in helping us understand what is going on in the experiment when we are investigating cognitive phenomena in other cultures. I will pick up on these and draw them out to show that this line of inquiry is a feasible one.

Investigating subject's understanding of the experiment

A number of years ago Webb, Campbell, and their colleagues (Webb, Campbell, Schwartz and Sechrest 1966) analyzed the special problems involved in drawing inferences from experimental data which stem from the fact that the experiment is a reactive situation. By this term, they emphasized that the performance outcome in an experiment is determined not merely by the conditions the investigator establishes but by the subject's awareness that he is an object of study.

Orne (1970) has systematically investigated the contribution this awareness makes to a variety of behavioral responses in experimental situations, ranging from hypnotic phenomena to galvanic skin responses. He identified as significant variables such factors as the subject's construction of the hypothesis under investigation — what he thinks the experimental question really is, what he identifies as the relevant variables, and what he thinks constitutes appropriate behavior in the experimental situation. Orne calls these the 'demand characteristics' of the experiment — the information the experimental situation conveys to the subject over and beyond what the experimenter tells him. One of the most interesting features of Orne's work is that, in spite of the investment of a great deal of effort and ingenuity, he found it impossible to design an experiment *without* demand characteristics — that is, an experiment that was totally meaningless to his subjects!

If, as Orne has demonstrated, it is important for the experimenter to take the subject's definition of the experiment into account, even when he is working with a familiar and relatively homogenous subject population, how much more crucial this is when an experimenter is working in an unfamiliar culture with subjects for whom the experiment is an alien situation. Yet, to my knowledge, there has been no systematic attempt to study demand characteristics in a cross-cultural setting. There is some anecdotal material, however, that suggests how this might be done.

Glick (1969), for example, was investigating what attributes of objects traditional Kpelle rice farmers use in classification tasks. His experimental procedure was the standard one in which the subject is presented with an array of familiar objects and told to put together those that belong together. He found that the great majority of subjects made groupings that were based on functional or perceptual relations between items rather than on their common member-

ship in a taxonomic category. Other investigators have interpreted similar findings as an indication that individuals displaying this behavior are deficient in conceptual thinking. Glick, however, asked his subjects why they grouped the items in the way they did. Many answered that this was the clever way to do it, the way that made 'Kpelle sense'. This reply suggested to him that subjects were construing his request to group the items as a test of their cleverness and were responding according to the culturally accepted view of what cleverness is. Glick followed up his hunch, asking a subject to group the items again, this time as a stupid person might do it. Interestingly, under these instructions, he secured perfect taxonomic grouping!

This can be construed as a role-playing approach and many modifications come to mind: Villagers might be asked to group the objects as students attending school might do it, as Westerners might do it, or as elders might do it. Another manipulation might be to vary the role of the experimenter instead of the subject: Are different task expectations conveyed by an experimenter identified with traditional ways and one identified with foreign ways?

In doing pilot work among Kpelle villagers in West Africa on solution of verbal syllogisms, I tried another technique for eliciting information on subject's perception of the task. I was asking individuals to answer classical syllogisms of the following type: All stores in Kpelleland are in a town. Mr. Ukatu has a store in Kpelleland. It is in a town? Earlier research by Cole and his colleagues (Cole *et al.* 1971) showed that traditional Kpelle villagers handled these problems on no better than a chance basis while young Kpelle adults attending high school performed in a manner comparable to that of American students. I was interested in finding out what features might account for the poor performance of the villagers.

One hypothesis was that they were failing to grasp the nature of the task as one that involved reasoning to reach a conclusion. It seemed from other evidence that they might be conceiving of the problem as a test of their knowledge of facts. So, working with expert translators, we prepared a set of instructions carefully explaining the hypothetical nature of the problems. We also gave a series of practice problems in which we helped the subject arrive at the right answer and demonstrated the peculiarities of the syllogism – how the answer can be derived simply from the information contained in the premises of the problem without any knowledge of the factual situation to which the premises refer.

After the series of test problems, we asked the subject some questions about the experiment and then requested him to give us a problem just like the ones we had given him. This was our test of how the subject construed the experimental task. Here is a typical problem offered by a village tailor: 'Suppose you see your son climbing up in a palm tree and start cutting nuts. You go and begin cooking for him. You hear a sound. How can you find out whether the palm nut fell down or your son?'

This problem and others like it are very instructive. First it tells us that the tailor had correctly grasped the purpose of the psychological game we were playing – his problem, indeed, is one that involves a reasoning process. But it also tells us that he did not grasp the *distinctive* features of the verbal syllogism. An important characteristic of the tailor's problem is that it has a number of correct answers. Among several possibilities, you can find out what has happened to your son by going to the palm tree and looking for him or by staying home near the cooking fire and listening for another sound. The information given in the problem does not in any way dictate the choice of a particular alternative. We know from previous ethnographic research that this problem is similar to a whole class of Kpelle riddle problems that furnish the material for verbal battles of wit in the villages. These problems do not have a single right answer, nor is there necessarily a social consensus as to which answer is the best one; honors go to the participant who delivers the most persuasive and unshakeable argument for the answer he chooses to give. In this respect, traditional Kpelle reasoning problems stand in sharp contrast to the verbal syllogisms we were using in our experiment. The defining attribute of a syllogism is that the answer or conclusion is a necessary one, whether or not it is reasonable, sensible, or clever.

We also learn from the problems given us about the limitations of verbal instructions and brief practice procedures. Our instructions seemed to meet all formal requirements in the sense that they covered the essential features of the task, and they seemed to meet all linguistic requirements as well – they simply failed to communicate what we thought we were communicating, and that was the special nature of the problem material.

The repertoire of problems we secured from our subjects also helps us in interpreting their performance on our test problems. We have the suggestion that one of the factors leading to poor performance might be the assimilation of the syllogism to the traditional riddle problem. If this were the case, subjects may have considered the choice of a Yes or No answer relatively unimportant in comparison to the clever reason they could construct to support it.

Certain testable hypotheses open up from this line of reasoning. One is that villagers might do better when the content of verbal syllogisms is made as unfamiliar as possible since this might counter their tendency to assimilate syllogisms to the traditional problem form. This would be an interesting hypothesis to test because it implies that achieving equal familiarity of problem content in two cultures or in two population groups within a culture does not ensure comparable task difficulty; the dimension of familiarity may be an irrelevant dimension for one group, a facilitating dimension for a second, and a disruptive dimension for a third.

In addition to suggesting modifications in the experiment, this hypothesis suggests a new line of ethnographic research which might help us link the investigation of reasoning processes in the laboratory with those occurring in

everyday life. Is there an analog of the Western logic problem in the language games of the Kpelle – that is, a language game in which the response is determined by the formal or structural features of the material and not by its content? If so, we might have more suitable material for experimental purposes. If not, we might want to identify individuals renowned for their skill at traditional riddle-problems to see how they do on our syllogistic problems. Do we observe negative transfer from one class of problems to another or do we observe a generalized verbal problem-solving skill? Through an interweaving of experimental and ethnographic research, we should make progress toward identifying the characteristics of problems and problem-solving situations that influence how reasoning processes are manifested.

Experimenting with the experiment

A second strategy for studying the cognitive demands of a particular experimental paradigm, proposed by Cole (Cole *et al.* 1971), is to subject it to systematic variation until the investigator achieves equal levels of performance among populations that may have initially differed in performance. This research strategy shifts the principal class of independent variables under investigation from those related to characteristics of populations to those related to characteristics of experiments. Instead of carrying one fixed paradigm to many different cultures, the researcher works with many different variations of a single paradigm within one culture. This approach is exemplified by a series of free recall studies conducted by Cole, Gay, Glick and Sharp (1971). These began with the standard free recall paradigm in which the experimenter read a list of disconnected words naming objects belonging to four Kpelle language semantic categories (food, tools, clothes, utensils). In the United States, when lists of this kind are presented in random order, school children from the upper elementary grades on, and middle-class adults, typically reorder the list and recall words clustered together by category rather than in their original order. The amount of clustering in recall has been found to be positively associated with number of words recalled. In the first studies, Kpelle villagers showed little learning of the list and little evidence of clustering.

Cole and his colleagues, however, did not terminate the experimentation at this point. They raised the question: What does it take in the way of experimental procedure to secure clustering and recall performance among the Kpelle villagers comparable to that of educated populations? After failing with some experimental manipulations, they hit on three tasks that dramatically shifted performance. In one, the objects to be remembered were associated with external cues, such as chairs; in another, to-be-remembered words were embedded in narrative stories of a traditional style; in the third successful manipulation, subjects were asked at recall to give back the items of one category at a time – that is, the experimenter instructed the subject to recall all the *foods*, then all the

clothes, and so on. In all these situations, there was not only an increase in the amount recalled but an analysis of the order in which the items were recalled showed that villagers were engaging in grouping or categorizing operations.

Under these special conditions, the retrieval processes of nonliterate Kpelle farmers seemed very much like those of American or Kpelle students: both intracultural and intercultural differences were greatly reduced. Cole (1972) offers the following interpretation of these findings:

'This series of experiments taken as a unit certainly seems to bear out the dictum that people will be able to perform well at tasks they find normal and which they often encounter. As such, it confirms anthropological doctrine. But . . . it specifies somewhat more closely than usual what "normal" conditions are. And it turns out that "normal" cannot be simply equated with "encounter often". Some of the experimental situations eliciting fine recall were *abnormal* in the sense of infrequently encountered . . . What the successful conditions seem to share with "frequently encountered" situations is a lot of structure. Where life or the experimental procedures do not structure the memory task, the traditional person has great difficulty. "Normal" in this case refers to the presence of certain structural features.'

This work is interesting from our present point of view because it identifies a specific cognitive demand present in the experimental situation that is presumed absent from naturally occurring situations. The argument is that the free recall paradigm, unlike situations in everyday life, fails to provide external cues or structure for recall and requires the subject to produce internal cues or structure to support the mnemonic performance. How well he does this, or whether he adopts this strategy at all, may depend on how often his culture confronts him with a similar cognitive demand. This leads to a specific hypothesis about how cultural circumstances may contribute to differences in memory performance – that is, the hypothesis that a member of a traditional society will rarely encounter situations in everyday life that require him to make his own retrieval plan.

To confirm that this is the case among the Kpelle requires an extensive program of field research to identify the contexts in which Kpelle need to learn, store, and retrieve masses of information. The leader of a cooperative work group must remember the work days, hours, and places put in by every one of the twenty or more individuals who constitute the group. Does he have any specific devices for doing this? What are the memory demands required by other activities, such as ritual ceremonies or instruction of the young in bush schools? Can we identify any devices built into these contexts that may serve as retrieval cues?

I am not suggesting that this kind of research will yield analytic knowledge of component processes of recall. But that is not its purpose. Its purpose is to tell us something about how situations vary in their cognitive demands and how the particular experimental paradigm we are using fits into this spectrum.

Studying naturally occurring and quasi-experimental situations

We have seen in all these examples how questions arising in experimental research lead to questions that can best be explored in field research, and the other way around. In closing, I should like to take this approach one step further and suggest the value of a research strategy that seeks from the outset to investigate some particular cognitive phenomenon in a range of situations, from the naturally occurring to the experimental. This strategy requires that we go beyond the use of ethnographic data to set a performance baseline for experimental findings and beyond their use as a source of hypotheses to be tested in experimental research. It means employing a full range of research techniques – both those of anthropology and those of psychology – to study a *single question* concerning cognitive performance.

Without trying to push the parallel, this strategy has been fruitfully employed in the comparative study of animal behavior, principally by Schneirla and his associates (Aronson, Tobach, Rosenblatt & Lehrman, 1970). Schneirla's own studies of ant behavior show the complementary nature of observation in the field and experimentation in the laboratory. Field work gives the investigator access to the complete natural phenomenon; selected aspects of this phenomenon can then be isolated and studied quantitatively in the laboratory. One of Schneirla's contributions that has a special relevance for our topic is his emphasis on the possibility of intervention in the field – that is, introducing some experimental manipulation in the naturally occurring situation to test a specific hypothesis about conditions controlling the behavior in question (Aronson, Tobach, Rosenblatt & Lehrman, 1971). I have borrowed the term *quasi-experiment* (Campbell and Stanley 1963) to designate this manipulation of conditions in the field.

Again, to keep the discussion concrete, let me work out a specific example. Dr. Akki Kulah, a Kpelle colleague, has described a game called *kolon* (1973), widely played by young and old, whose function seems to be that of teaching young children proverbs. *Kolon* is a competitive game played by two opposing teams whose members vary in age from six to adulthood. The game begins when the leader of one team calls out a phrase to the youngest member of the opposing team who must respond with the 'answer' which is a particular proverb. If the child fails to respond correctly the turn passes to the next older team member.

Kulah has recorded a number of *kolon* games and is now analyzing this material to stipulate the rules of the game and to develop some hypotheses about the relations between the stimulus material and the proverb responses. This analysis will not in itself tell us much about component learning and memory processes of the individual players. Since we do not know the history of the participants, we cannot tell when a child fails to respond correctly whether

he has lost the association between the stimulus and the proverb, whether he has forgotten the proverb, or whether he never knew it. When we fully understand the structure of the game, however, we can intervene in it, turning it into a quasi-experimental situation. We might introduce new material to be learned in a format similar to the customary one so that rounds of the game are equivalent to learning trials. We might then begin to manipulate features of the game to see how learning and memory are affected – what happens when we change the structure of the material, that is, vary the relations between the stimulus and response members? What is the influence of the social structure of the game – are there memory cues in the interrelationships of game participants? What happens when it is converted into an individual learning situation? At this point we might return to the laboratory and set up a formal paired associate learning experiment and then gradually reintroduce features of the *kolon* game.

This strategy will clearly not be equally useful for the study of all cognitive phenomena, and for some it may be inapplicable. But it seems feasible and appropriate for pursuing many controversial issues in memory, problem-solving, classification, learning, communication, and related areas. At the least, the systematic study of a given phenomenon in a range of situations, including the quasi-experimental, should help us use the experiment to greater advantage in cross-cultural research. At the best it will move us along toward identifying the formal features of situations that affect cognitive performance. As we develop a framework which relates cognitive processes to their contexts, we may overcome the old dichotomies that have stood in the way of our fuller understanding of the interrelations between culture and cognition.

Note

The views here expressed have developed in the course of collaborative work with Michael Cole and owe much to his formulations on cross-cultural experimentation. Preparation of this paper was supported by a grant from the Carnegie foundation to Michael Cole and U.S.P.H.S. Grant GM 16735 from the Institute of General Medical Sciences to C. Pfaffman.

References

- Albert, E. M. (1964). 'Rhetoric', 'logic', and 'poetics' in Burundi culture patterning of speech behavior. *American Anthropologist*, 66, 35–54.
- Aronson, L. R., Tobach, E., Rosenblatt, J. S., & Lehrman, D. S. (Eds.) (1970). *Development and evolution of behavior: Essays in memory of T. C. Schneirla*. San Francisco: W. H. Freeman Press.
- Aronson, L. R., Tobach, E., Lehrman, D. S., & Rosenblatt, J. S. (Eds.) (1971). *Selected writings of T. C. Schneirla*. San Francisco: W. H. Freeman Press.
- Bellman, B. L. (1968). Unpublished field notes.

- Berry, J. (1969). On cross-cultural comparability. *International Journal of Psychology*, 4, 119-128.
- Campbell, D. T., & Stanley, J. C. (1963). *Experimental and quasi-experimental designs for research*. Chicago: Rand McNally.
- Cole, M. (1972, April). *Toward an experimental anthropology of thinking*. Paper presented at the joint meeting of the American Ethnological Society Council on Anthropology and Education, Montreal.
- Cole, M., Gay, J., & Glick, J. (1969, March). *Communication skills among the Kpelle of Liberia*. Paper presented at the Society for Research in Child Development Meeting, Santa Monica, CA.
- Cole, M., Gay, J., Glick, J., & Sharp, D. W. (1971). *The cultural context of learning and thinking*. New York: Basic Books.
- Dasen, P. (1972). Cross-cultural Piagetian research: A summary. *Journal of Cross-Cultural Psychology*, 3, 23-39.
- Evans-Pritchard, E. E. (1963). Sanza, a characteristic feature of Zande language and thought. In Evans-Pritchard (Ed.), *Essays in social anthropology*. New York: Free Press.
- Evreux, Y. (1864). Voyage dans le nord du Bresil fait durant les annees 1613 et 1614. Paris and Leipzig, Frank, 1864. (New Haven Human Relations Area Files.)
- Frijda, N., & Jahoda, G. (1966). On the scope and methods of cross-cultural research. *International Journal of Psychology*, 1, 110-127.
- Gibbs, J. (1962). Poro values and courtroom procedures in Kpelle chiefdom. *South-western Journal of Anthropology*, 18, 341-350.
- Glick, J. (1969, November). *Culture and cognition: Some theoretical and methodological concerns*. Paper presented at the American Anthropological Association meetings, New Orleans.
- Glick, J. (1975). Cognitive development in cross-cultural perspective. In J. Horowitz (Ed.), *Review of child development research*, vol. 4, pp. 595-654.
- Kulah, A. A. (1973). *The organization and learning of proverbs among the Kpelle of Liberia*. Unpublished doctoral dissertation, Harvard University.
- Lloyd, B. B. (1972). *Perception and cognition: A cross-cultural perspective*. Middlesex, England: Penguin Books.
- Orne, M. T. (1970). Hypnosis, motivation, and the ecological validity of the psychological experiment. In W. J. Arnold, & M. M. Page (Eds.), *Nebraska symposium on motivation* (pp. 187-265). Lincoln: University of Nebraska Press.
- Scribner, S., & Cole, M. (1973). The cognitive consequences of formal and informal education. *Science*, 182, 553-559.
- Wallace, A. F. C. (1962). Culture and cognition. *Science*, 135, 351-357.
- Webb, E. J., Campbell, D. T., Schwartz, R. D., & Sechrest, L. (1966). *Unobtrusive measures: Non-reactive research in the social sciences*. Chicago: Rand McNally.