Problem Solving in a Human Relationship: The Interactional Accomplishment of a "Zone of Proximal Development" During Therapy*

Lawrence M. Lope
High Scope Foundation
Ypsilanti, Michigan

Although problem solving has long been studied in the cognitive sciences, we still know little about how it is accomplished in everyday life. Perhaps, as Simon (1976) has suggested, we will only need to add some variables to existing models to apply experimental analyses to everyday life scenes. An alternative formulation by Cole, Hood, and McDermott (1978) suggests that problem solving in the social world is structured in ways that render existing experimental analyses inapplicable. In either event, the cognitive social sciences need to develop techniques that can provide principled descriptions of everyday-life problem solving. These descriptions could stand as an empirical base from which we might begin to assess the adequacy of models derived in the various ways presently popular in the field (Carroll & Payne, 1976). The kind of background Anzai and Simon (1979) have given us for an experimental interaction between a task environment and a problem-solver is even more essential on the social level, for which descriptive work is sparse and experimental results disparate.

Excellent conditions for examining social problem solving exist in those very institutions that are set up and financed for that purpose. In this paper, a family therapy session is analyzed. A couple with rather severe social problems (drunkeness and depression) come in search of help to a therapist who has only words as a resource (as different, for example, from job opportunities, exercise programs, drug therapies or forced confinement). The task confronting the couple and the therapist is similar to the one we are faced with in more standard cognitive studies. Together they must agree

upon the problems before them, define the resources they have for solving these problems, develop the "insight" to apply their resources on the most appropriate occasions, and thus organize environments in which their problems are dimished.

This paper will briefly describe some social task environments that occurred in therapy and the resources the trio used for dealing with them. A special emphasis is placed on the conditions for moments of "insight," from whence possibilities for change are developed. In other words, I want to describe my theory of therapeutic insight as a learning experience that occurs as a result of a social-cognitive-interactional reorganization of the therapeutic environment. In his role as a mediator of the interaction between the couple in therapy, the therapist creates a form of zone of proximal development in which system changes of a special sort are made momentarily possible.

The Couple and The Therapy

The couple in therapy arrived at the first session with a common sense theory that explained their problem to each other, to the therapist, and to the world. I call this theory the "personal defect" theory. During the course of therapy, a second theory explaining the couple's problem was introduced or developed among the participants. I call this a "systems focused" theory. I am the therapist.

The couple's initial, common sense theory is reflected in a form of talk that locates their problem in the individual and his or her personal defect (drinking, depression, running around with other women). This form of talk was generally introduced by the couple. The therapist's theory is reflected in a form of talk that locates the problem in the couple's well integrated interactional pattern (Jackson's marital quid-pro-quo, Jackson, 1965), a pattern of interaction developed and sustained over their last fifteen years together.

The couple re-entered therapy after a three month "recess" prior to the session under analysis. Before termination, the couple expressed the belief that the therapy sessions had been a rewarding experience. I agreed that some progress had been made, but noted that the couple had become comfortable in the absence of a crisis. The wife, Nancy, stated that her future plans included travel and she expressed interest in becoming more physically active. The husband, Frank, revealed that he was interested in taking the state licensing examination and looking for a better paying job. The sessions ended with the therapist indicating to the couple that if Frank decided to take the state exami-

nations the couple might well consider coming back into therapy. This was suggested based upon the therapist's theory of the couple's marital quid-pro-quo. Early in their relationship the couple had developed a pattern of complementarity, reflecting aspects of dependency and independency, or helping and being helped. The unstated question in the mind of the therapist at that point was: Would Frank's move to take the examination be a violation of this well "institutionalized" pattern of behavior? If Frank passed the test, for example, he would potentially be much less dependent on his wife, and a recalibration of the system would be necessary.

At the time of the session described below, Frank had recently quit his job of nine years to study full time for his state board examination, and the couple had re-entered therapy in crisis. As I had hypothesized, the marital quid-pro-quo had become an important issue, as the couple and I discussed the constraints that the pattern of dependency/independency imposed on their relationship. Frank's decision to take the examination was framed as a *crisis* by me in terms of its potential consequences for the couple's relationship.

The opposing theories for talking about the couple's problems are available in the discourse during the videotape session. If we turn to the text:

(1) Therapist: Ya, you want to remember when you came back in in what I thought was a crisis, my statement was not so much passing.

(2) Husband: okay,

(3) Therapist: was were you going to take the test, that was the big thing. You talked about not studying and you really were concerned whether you were going to have it together well enough to just go in there and take the test. . . .

(4) Husband: Ya

(5) Therapist: Right

(6) Husband: Well, well the thing is let me say this. I stopped drinking about two weeks ago something like that? Two and a half weeks approximately?

(7) Therapist: It's about three weeks now.

(8) Husband: It's about three weeks I stopped drinking completely now. Okay, the fact is (sigh) that I could have passed this test a long time ago but that drinking held me back. . . .

In the struggle to define the problem the couple is to solve, I offer an interactional interpretation that suggests that Frank's move to take the examination was a potential crisis in terms of the couple's relationship. Note the contrast in the interpretation of the problem as illustrated by the husbands explanation. When I say, "have it all together to go in there and take the test...." (line 3), I am interrupted by Frank (lines 6 and 8), who goes on to talk about his individual character defect and how drinking has held him back. As I introduce my theory, Frank appears a little uncomfortable. His fractured speech is suggestive of his discomfort and is in sharp contrast to the smooth, rehearsed-sounding narrative that generally accompanies his talk about his personal shortcomings (line 8).

Two important interactional configurations emerge during the therapy hour. These configurations can be referred to in the analysis as family homeostasis during which the couple struggles to maintain the defect logic of their relationship (Jackson, 1957), and therapy homeostasis, during which the couple seeks to understand their relationship, rather than their personal defects, as the source of their problem (Lopes, 1979). During the former, the couple provides an elegant narrative claiming that their problems are related to the husband's drinking or running around, or the wife's drinking or depression. I was particularly struck by the husband who continually used the nagging phrase, "I feel strongly about that." I interpreted this recurrent phrase to be an important linguistic marker which the husband used to introduce the narrative that helped maintain the couple's family homeostasis. This homeostasis is reflected in the kind of talk (first person pronoun usage followed by a statement about his/her defect--drinking, depression, etc.), in their interactional pattern (they talk through the therapist and not to each other), and in their body position which is clearly closed. During the second interactional configuration, connected with relational interpretations, the kind of talk changes (the pronoun usage shifts to "we," "our," and their problem is now spoken of in terms of "this relationship"), the interactional patterns change (they talk to each other), and they shift into an open body position (For details see Lopes, 1979).

The session is marked by an interesting series of interactional segments. These segments are described here because they appear so dramatically different from the *flow of interaction* typical of the hour session. These segments are characterized by a six second silence accompanied by a lack of any body movement. So uncanny are these segments that, when I first showed them to colleagues, I was asked if the tape had been spliced at that point; a "technological modification" seemed to be the only explanation. The segments described correspond to my introduction of statements concerning the effect that the marital quid-pro-quo is having on their relationship. When I first introduced this concept, the couple became motionless and silent for six seconds. On a second occasion, the husband, with some modification, takes my statement and uses it to talk about their relationship. "I, I want to be realistic, okay, I don't want to lose her but I want to be freer." This is met by another six second silence and an absence of any body movement. I then restated the concept in a different way, and again the couple sits, silently and motionlessly, for another six seconds.

Shortly thereafter the couple shifts into the second interactional configuration (therapy homeostasis) and for the first time in four months of therapy adopt the therapist's systems-focused view of their problems. In this way, they redefine their problems in relational terms and momentarily see each other as potential solutions rather than as causes of their problems. These are insightful moments, and a description of their organization is important.

Insight As (pause) Cognition

Problem solving requires thinking. The struggle to reframe a problem in a new light requires an extended mental effort over time. Peak moments within that process, when a problem becomes suddenly penetrable and open to solution, are moments of insight. How shall we

understand how such moments are made possible? In his famous work on Sultan the ape, the pioneer Gestalt psychologist, Wolfgang Kohler, took on this problem, and his description of Sultan's behavior seems to be remarkably parallel to my description of the therapy session. After milking his analysis for similarities, I go on to offer how a more social description than he had to offer can organize us to appreciate Vygotsky's (1978) notion of a zone of proximal development as a critical element in any theory of social problem solving.

Kohler was interested in how the ape dealt with new problems, the solution of which required that the animal use the artifacts in his environment in a new system of activities to obtain an inaccessible goal. Sultan was required to get a banana that lay just outside his cage. Initially he could reach the banana unaided. But then the banana was removed from reach. Sticks inside the cage were not long enough to reach the banana. But two sticks, one of which could be inserted into the end of the other did the job. Sultan, of course had to discover this for himself. In Kohler's discussion of chance and insight he states:

> He (Sultan) is seldom seen to attempt anything which would have been considered accidental in relation to the situation....As long as his efforts are directed to the objective, all distinguishable stages of his behavior (as with human beings in similar situations), tend to appear as complete attempts at solution, none of which appears as the product of accidentally arrayed parts. This is true, most of all, of the solution which is finally successful. Certainly, it often follows upon a period of perplexity or quietness (often a period of survey), but in real and convincing cases and never appears in a disorder of blind impulses. (Kohler, p. 191)

What made an impression on Kohler was the way Sultan and other apes would pause just before the problem was resolved. He commented:

> I have noticed from myself and others, that what is particularly enlightening as to the ape's behavior are the pauses mentioned above. A local colleague convinced, like most students, of the general value of the chance theory for animal psychology, came to see the anthropods. I chose Sultan for the demonstration. He made one attempt at solution, then a second, and a third; but nothing made so great an impression on the visitor as the pause after that during which Sultan slowly scratched his head and moved nothing but his eyes and head gently, while he most carefully eyed the whole situation. (Kohler, p. 191)

These observations led Kohler to define insight as, "the appearance of a complete solution with reference to the whole layout of the field" (Kohler, p. 191).

The parallel between Kohler's description and my therapeutic example should be clear, right down to and including the very marked pauses. The structure of the 'personal-defect theory' represents the initial "layout of the field" and the 'systems interpretation' is the final layout. The marked speech and motor pauses, differentiate the two layouts analogous to Sultan's pause. After a pause to think, Frank and Nancy are ready to take a step in a new life. Of course, there are some important differences in the two descriptions. The couple face a much more complex problem than Sultan, and their solution is necessarily more tenuous. This complexity need not discourage our effort to describe therapeutic insight. In fact it is possible to use this complexity as a resource in the analysis; their insight must be constantly renewed if the couple is to move into a different way of being alive for each other. The very complexity of their situation, and the work they do to overcome it may help us to a more profound description of what the "pause for thought" might include.

For example, it seems that once Kohler's ape had an "insight" (e.g., a changed layout of the field), change was accomplished. In Kohler's description, Sultan immediately ran to the limits of the cage to reach for the banana with the implement made from the two sticks found in his cage. In the example from the therapy session a single "insight episode" was not enough. In all three examples of a pause in the couple's interaction, the couple attempted to return to their initial layout of the field (family homeostasis), while the therapist continued to create an environment that prevented their return. Turning to the text:

(1) Wife: (to Husband) Once you pass your exam you shouldn't have any worries either.

(2) Therapist: All right, two things come to mind. . . .

(3) Therapist: One is (Husband places hand over mouth) is. . asked about some negative reasons for passing the examination. . . .

(4) Husband: Okay.

(5) Therapist: You just gave me another one.

(6) Husband: Right, ah

(7) Therapist: One was you were spending a lot of time away from home.

(8) Husband: Okay.

(9) Therapist: And two was (pause) maybe your wife might leave you (Both the husband and wife are frozen at this time and no one is moving for 6 seconds).

(10) Husband: Yea, she'll probably leave me (pointing with his finger) the first ah (pause) for the first, ah (pause) statement that I made, ah (pause) staying away from home for a long time. . . .

The interactional reference is made by me (line 9). Frank is asked what will keep Nancy from leaving him once he has passed his examination. There is a six second pause in all interaction. Frank recovers but with a great deal of difficulty. He starts, pauses, starts again, pauses, (swallows) and finally gets on track by using his finger to orchestrate the familiar narrative. (line 9) The brief struggle is over, the rhythm is restored temporarily.

A second pause is intiated when Frank takes my statement about Nancy leaving and reinterprets it:

(1) Husband: Ya, but the thing is, if I finish that contract, okay, I can't throw it out of the house.

(2) Wife: Why not?

(3) Husband: Well, the thing is (pause) that (pause) ah (pause) I feel (pause) no, I'm trying to be realistic okay (pause) in other words, I do not want her away and yet I want to be freer. (There is a six second pause and both are motionless)

(4) Wife: Well, I would think that if you want to be freer, all you would have to do would be just like any courteous person would be, if you're not going to be home for dinner.

(5) Husband: Okay.

(6) Wife: Things like that, but then, you did that once before, you said and I believed you but then.

(7) Therapist: Let's not worry about what he did before.

Frank's statement (line 3) is a reformulation of what I had said earlier. My claim is that a "systems insight" into the marital quid-pro-quo can be seen here. Frank attempts to use my systems formulation but his individual "insight" is not enough and Nancy moves to return to the original layout of the field. The silence is broken by Nancy (line 4) attempting to recite the defect narrative. She is interrupted by me (line 7), before she can successfully get into the narrative about Frank's staying out late and running around. I follow Frank's statement with a restatement of my original claim that contours Nancy's view:

(1) Therapist: You've made an interesting step. You've moved, you moved to do that. You moved for what you call your independence.

(2) Husband: Ya. (head nod)

(3) Therapist: This has been a relationship where it really hasn't allowed for the two of you to be independent at the same time. Either you're falling down drunk, acting all crazy on the weekend (head nod) or your wife's depressed.

Again the couple is motionless, frozen in position. The six second pause is broken by Frank who says, "Well, you see there's no other women in my life, okay." Here the therapist and Frank both make statements that have significant consequences for the couple's relationship. In all three cases, the interpretation is immediately followed by a six second silence accompanied by a freezing in body position. In each case either the husband or the wife attempts to restore the original "layout of the field" as the therapist continues to redirect (reframe) those attempts. By reframing these precise points in the interaction, the therapist plays an active role in establishing and maintaining a "new layout of the field"--a layout that they can not easily maintain on their own, at least initially. This is perhaps, because the insight itself is not an individual matter; it is located in the interaction of the couple. What is interesting here is that as Frank begins to reorganize his thinking under the therapist's direction, he now becomes part of the field being reorganized for Nancy.

This example of therapeutic insight, because of its complexity, offers some information about the conditions of change that Kohler's work could not. It is in considering the requirements for producing and sustaining change that Vygotsky's (1978) idea of a "zone of proximal development" becomes relevant.

Insight As An Interactional Accomplishment

It now appears that the concept of a zone of proximal development, within which the couple gain insight into their problem, can be applied to the period of time when the couple moves from the first interactional configuration to the second. This application is not much different from that applied to observations made in learning environments (Cazden, 1979). Vygotsky (1978) discussed his concept of the zone of proximal development as follows:

It is the distance between the actual developmental level as determined by independent problem solving and the level of potential development as determined through problem solving under adult guidance or in collaboration with peers (p. 86).

What I am claiming is that the couple came to therapy with an independent solution to their problem, a theory which supposes that if only Frank could stop his drinking and running around or if Nancy could just stop being depressed the two of them could get on with the business of living. Frank could pass the state examination and get a better paying job, and Nancy could travel and start to enjoy life. The therapist has suggested an alternate theory of the couple's problem, a theory generated from his training. This theory suggests the level of potential development which can be reached through problem solving under the therapist's guidance. The theory suggest that the problem is *not* solely in the personal defects defined by the couple, but is embedded in a pattern of interaction that has supported Frank's dependency in their relationship for fifteen years. It also suggests that Frank's move to take the state examination is now threatening that pattern.

In the three segments described above, we get a progression in the therapist's attempt to get the couple to talk about their problem in a different way. The three segments represent a form of "systems insight" as described by Kohler. But what is interesting is the fact that the potential new problem solving framework does not appear to come from the individual, as it did with Sultan, but from the social interaction between the couple. I contradict Frank and the couple is stunned. Frank reformulates my statement as he moves toward my view of the problem, but Nancy attempts to reintroduce the old problem-solving pattern. I contradict her by keeping my interpretation on the floor, thus preventing her from re-introducing the familiar narrative. Shortly thereafter, the couple begins to use my problem-solving strategy to talk about their problem. It is such facts that compel me to say that the reorganization of their field, their "therapeutic insight" is thus an interactional accomplishment.

The four-minutes immediately following the silent segments is significantly different in content, affect, and form of interaction than the previous twenty-eight minutes. The couple adopted my view of their problem and begin to reveal their individual fears about the relationship and the consequences of the relation in terms of Frank's move to take the examination. The adoption of the therapist's problem-solving strategy is brief, however, and after four minutes the couple reinstitutes the old problem-solving strategy as the session continues very much like the first twenty-four minutes.

The ultimate question, is: for therapeutic problem solving to what extent do the couple adopt the new strategy to deal with their problem *outside the therapeutic environment?* Unfortunately, that information is beyond the scope of this analysis. What is clear, however, is that now the couple is able to employ the new strategy to discuss their problem during the hour session in a way that produces the most dramatic interaction in the four months of therapy. Problem solving in human relationships is difficult. So is its description. Both are worth the effort.

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Performance Before Competence: Assistance to Child Discourse in the Zone of Proximal Development*

Courtney B. Cazden
School of Education
Harvard University

It is the distance between the actual developmental level as determined by independent problem solving and the level of potential development as determined through problem solving under adult guidance or in collaboration with peers (1978, p.86).[1]

This is how Vygotsky described what he called the "zone of proximal development." The concept of a zone within which a child can accomplish with help what later can be accomplished alone can be useful in helping us understand the child's acquisition of discourse.

*This paper is a revised version of the third part of Cazden, 1979.

[1] Perhaps because the adjective, "proximal," is infrequently used in English, some writers use the term "zone of potential development." But the two are not synonyms. The trouble with *potential* is that it has no boundaries; potentially, any child is capable of learning anything at some future time. By contrast, the zone of *proximal* development is much narrower. Instruction in this zone leads to development by aiming at the "ripening" function; by being just a little ahead, not out of sight.

If we substitute "speaking" for "problem-solving" in Vygotsky's definition, then the zone of proximal development for speaking,

is the distance between the actual developmental level as determined by independent speaking and the level of potential development as determined through speaking under adult guidance or in collaboration with peers.

What kind of assistance to discourse development through "speaking under adult guidance" do children get in school? It is my impression, from both personal experience as a teacher and from the research literature, that children get help in answering teacher questions, and, more rarely, they get help in participating in the discourse structure typical of classroom lessons. An example of each in turn:

Help with Particular Questions

It's important to distinguish between help that somehow gets a child to produce the right answer, and help from which the child might learn how to answer similar questions in the future. Only the latter is of educational interest. All teachers sometimes have to get the answer said somehow in order to keep the lesson going for the sake of social order, what Mehan empathetically calls "getting through" (1979, pp. 111-114); but one cannot defend the value of such sequences to the individual child. If, for example, when a child cannot read the word *bus* on a word card, the teacher prompts the answer with the question, "What do you ride to school on?" the child may correctly now say "bus." But that is not a prompt that the child could give to herself the next time, because the prompt depends on the very knowledge of the word that it is supposed to cue. We are looking for assistance that at least has the possibility of helping children learn how to answer, even if we lack evidence that it in fact does.

Here are two examples from reading lessons with first-grade children analyzed by Mehan (1979). From one theoretical perspective, these are excellent examples of "negotiated interactions" or "interactional accomplishments": the children get the teacher to give them the clues they need to find the answer. (I am grateful to Peg Griffin for this observation.) But, speaking for the teacher, I want to suggest another non-contradictory perspective: that the teacher was providing implicit information about how to answer such questions--information that is applicable, and, I hope, eventually transferred, beyond the particular instance.

Examples of Question Sequences
(From Mehan, 1979)

(1) T.	OK, what's the name of this story?
	(points to title of story)
Ss.	(no response)
T.	Who remembers, what's the name, what's the story about?
Ss.	(no response)
T.	Is it about taking a bath?
Ss.	No.
T.	Is it about the sunshine?
Ss.	No.
T.	Edward, what's it about?
Edward.	The map.

T.	The map. That's right. This says 'the map.'
(2) T.	What else, what else, Edward, what do you think we could put there that starts with an M?
Edward.	(No answer)
T.	Somebody in your family, Edward. . . .
Audrey.	I know. I know.
T.	What?
Audrey.	Man
T.	Man, good for you, Audrey.

In the first example, the implicit message is about the meaning of the question "What's the story about?" The form of simplification here is from the initial wh-question to yes/no alternatives, a sequence common in adult talk to children (J. B. Gleason, personal communication). The specific alternatives are deliberately absurd members of the category of things that could answer that question.[2] In the second example, the general wh-question about "What else to think of a word, a useful heuristic is to narrow the field to a small set you can run through in your mind.

Help with Participation in Lessons

There are various reasons why help with particular questions may not be enough, why children may need help with known-answer questions, in general, and the lesson structure in which they are embedded. They may be newcomers to school; they may need to get used to particular lesson structures in particular classrooms; they may come from a cultural background that may lead to sociolinguistic interference between the discourse patterns of home and school.

Even in the now growing literature on cultural differences between home and school, there are few descriptions of what can be called "second discourse teaching." More attention has been given by researchers to how the teacher can adapt to children's preferred ways of interacting than to how teachers can help children adapt to the school, even though Philips (1972) presented both alternatives in the concluding section of her influential paper. One important exception is Heath's (in press) work with teachers in a Black community in the southeast U.S., which she calls Trackton. When the teachers complained that children did not participate in lessons, Heath helped them understand what she had learned from five years of ethnographic field work in the Trackton community. For example, the

children were not used to known-answer questions about the labels and attributes of objects and events; as one third grade boy complained, "Ain't nobody can talk about things being about theirselves." Heath then worked with the teachers to try out changes in their classrooms. Because Heath's work is such an imaginative and rare example of assistance to children's discourse development in school, I quote at some length:

For some portions of the curriculum, teachers adapted some teaching materials and techniques in accordance with what they had learned about questions in Trackton. For example, in early units on social studies, which taught about "our community," teachers began to use photographs of sections of different local communities, public buildings of the town, and scenes from the nearby countryside. Teachers then asked not for the identification of specific objects or attributes of the objects in these photographs, but questions such as:

> What's happening here?
> Have you ever been here?
> Tell me what you did when you were there.
> What's this like? (pointing to a scene, or item in a scene)

Responses of children were far different than those given in usual social studies lessons. Trackton children talked, actively and aggressively became involved in the lesson, and offered useful information about their past experiences. For specific lessons, responses of children were taped; after class, teachers then added to the tapes specific questions and statements identifying objects, attributes, etc. Answers to these questions were provided by children adept at responding to these types of questions. Class members then used these tapes in learning centers. Trackton students were particularly drawn to these, presumably because they could hear themselves in responses similar in type to those used in their own community. In addition, they benefitted from hearing the kinds of questions and answers teachers used when talking about things. On the tapes, they heard appropriate classroom discourse strategies. Learning these strategies from tapes was less threatening than acquiring them in actual classroom activities where the facility of other students with recall questions enabled them to dominate teacher-student interactions. Gradually, teachers asked specific Trackton students to work with them in preparing recall questions and answers to add to the tapes. Trackton students then began to hear *themselves* in successful classroom responses to questions such as "What is that?" "What kind of community helper works there?"

In addition to using the tapes, teachers openly discussed different types of questions with students, and the class talked about the kinds of answers called for by certain questions. For example, *who, when,* and *what* questions could often be answered orally by single words; other kinds of questions were often answered with many words which made up sentences and paragraphs when put into writing. (Heath, in press)

Help With Other Discourse Forms

In the San Diego classroom described by Mehan, we created one special speech situation that we called an instructional chain (IC) (Cazden et al., 1979), and I want to describe it briefly here as an example of the possible benefits of non-lesson discourse. Briefly, in each IC the teacher taught a lesson to one child who then taught the same lesson to one or more peers. Leola, a Black third grader, was asked to learn and then

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[2]As this article was going to press, I found a relevant analysis by Churchill (1978) of disconfirming answers to yes/no ('specific proposal') questions. His "generalized invitation maxim" says in part: "If you are asked a specific proposal question. . . . your answer is the disconfirming one, either give the disconfirming answer and then give the correct answer or give the correct answer only (p. 48). Presumably the teacher would have welcomed a one-turn answer that followed this maxim:

> Is it about taking a bath?
> No, it's about the map.

But that did not happen. Whether for developmental or situational reasons, the children answered only "no"; and a separate teacher turn was necessary to elicit the correct answer.

teach a language arts task. Here are the first three items on her worksheet in completed form.

1. new   1. Y б̷ ʎ̷ o д̷ u   2. t ʏ̷ ʇ̷ o l ʃ̷ d   3. m б̷ ñ̷ e
2. no
3. off   *You*   *told*   *me*

Table 1 gives a skeletal version, minus repetitions, corrections, etc., of the teacher's directions as she talked Leola through the first two items on the task, and the full transcript of Leola's subsequent directions first back to the teacher as a rehearsal, and then in actual instruction of her peers. Note in passing that the teacher's questions serve to talk Leola through the task until she can do it herself. That such aid does help Leola work independently is shown by a comparison of the teacher's instructions for the first and second items. The first three parts are repeated, but then a much vaguer and incomplete question "Now what are you going to--" is sufficient, and Leola takes off on her own.

Table 1

Teacher's Instructions to Leola

ITEM 1

Teacher	Leola
OK, now number one here says *new*. What's the opposite of *new*?	Old.
Old. How would you spell *old*?	O-L-D
OK, in the letters that are on this paper, cross out the letters you just used for spelling *old*.	(L. does it)
Good. What word is left?	Y-O-U
What does that spell?	You.
OK, and down here you'll write *you*.	

ITEM 2

Teacher	Leola
OK, now number 2 here says--	No.
No. What's the opposite of *no*?	Yes.
OK, how do you spell *yes*?	Y-E-S
All right, now what are you going--	(L. crosses out the letters Y-E-S) Told.

Leola's Versions of the Instructions

in rehearsal to the teacher:

L.   Spell these letters, and then put out that letter, and then have another letter left.

(later, after T. goes over the instructions again)
To do the opposite of this. You got to write old. I'm gonna tell 'em: you gotta write old, cross old out and you have another letter left.

in actual instruction of her peers:

(1)   [Goes to get pencils, then returns to work desk and sits down]
It is hard, . . . You gotta write--what's the opposite of "new" is "old."

So you got--so you gotta cross O-L and D, and you have a letter left, and you--you put the letter left in these words.

(2)   You cross it--you see, you got to do the opposite of "n-no" i--"no" is "yes" on number two. "No"--"no" is "yes", so you gotta write Y-E-S. And you have a "told"

(3)   left, so you write T-O-L-D. See, d-do the op--the op--the opposite of ah--uh--"off" is "on", so you gotta cross, on number three, you gotta cross "on" off. O-N. And you--it is "me" left, M-E.

The important aspect of this IC for thinking about discourse development at school is the increased articulateness and precision in Leola's instructions from her first rehearsal to the teacher:

Spell these letters, and then put out that letter, and then have another letter left.

to the most elaborated version in item 3. Here it is without the hesitations and self-repairs:

The opposite of *off* is *on*, so on number 3, you gotta cross *on* off. O-N. And it is *me* left, M-E.

This is a good example of what Wertsch, following the Soviet psychologists, calls microgenesis--that is, development within an observable time period, and it is a kind of development that Leola seemed to need. In the nine lessons analyzed by Mehan, some three hours of talk in all, she spoke four times, and only twice more than one word. This is not to say that she was in any way non-verbal; but it is to suggest that she could benefit from challenges to talk about academic topics, not just in response to questions.

Discussion

Earlier I pointed out the obvious difference between helping a child somehow get a particular answer, and helping a child gain some conceptual understanding from which answers to similar questions can be generated alone at a future time. We can think about this distinction more generally as different relationships between performance and competence.

Child discourse under adult guidance that is more advanced than what the child can speak alone can be called "performance without competence." Gleason and Weintraub (1976) first pointed out the existence of such performance in early social routines like *bye-bye, thank you,* and *trick-or-treat.* I am generalizing her description to other kinds of adult-assisted talk. In the school examples, the teacher assumes--with Vygotsky--that the assisted performance is not just performance *without* competence, but performance *before* competence--that the assisted performance does indeed contribute to subsequent development. Our task as researchers is to find out if and how that happens.

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Mathematics Learning Difficulties in African Children: A Clinical Interview Study*

Herbert P. Ginsburg
Robert L. Russell
*Graduate School of Education
and Human Development
University of Rochester*

Jill K. Posner
*Developmental Psychology Program
Graduate Center
City University of New York*

Many African countries have introduced Western curricula as part of the process of modernization. Thus, in the Ivory Coast, the mathematics curriculum has both a "new math" flavor involving set theory notions and notation, and a Piagetian component involving an emphasis on manipulation of real objects and the gradual development of abstraction. Little is known about the effects of such schooling. One may ask: What do children from traditional societies learn from Western schooling, and what kinds of difficulties do they experience in learning mathematics? The answers to these

questions have implications for understanding the cognitive processes of traditional peoples.

An old view held that there is a "primitive mentality" which is "pre-logical," and generally incapable of abstractions and of rational thought (Levy-Bruhl, 1966, quoted in Cole and Scribner, 1974). Presumably such a primitive mentality would severely handicap traditional African children in their efforts to profit from the abstractions inculcated by Western schooling; it might produce a failure to appreciate abstractions and a tendency to perform on an extremely concrete level.

Recent research presents a markedly different view of traditional peoples' abilities in three areas--general cognitive processes, informal mathematics, and schooled mathematics--and leads to different predictions concerning their learning difficulties. First, modern cross-cultural research attests to the overall similarity of basic cognitive processes in various cultural groups (Cole and Scribner, 1974). Moreover, traditional peoples can perform such abstractions as inventing codified symbolism, as in the case of the unschooled West African Vai, who have developed their own system for reading and writing (Scribner and Cole, 1978). Second, in the area of informal mathematical thinking, Posner (in process) has shown that unschooled children in two West African groups, the Baoulé and Dioula, possess basic mathematical concepts of equivalence, inequality, and understanding of the operations of addition, and in these respects are similar to American children. Similarly, Ginsburg, Posner, and Russell (in press, b) have found that unschooled Dioula children and adults use fairly elaborate and clever strategies, involving counting, regrouping, and addition facts, to solve mental addition problems, and eventually achieve levels of accuracy as high as educated Americans'. Third, academic knowledge takes roughly the same form in the Baoulé and Dioula as it does among American students. Ginsburg, Posner, and Russell (in press, a) present data showing that African and American elementary school children display commonalities in such areas of academic knowledge as: (a) Technical skills used for reading and writing numbers, elementary calculation, and remembered number facts; (b) The understanding of fundamental principles concerning arithmetic, for example in the area of place value; and (c) "Invented procedures" (Groen and Resnick, 1977), that is, calculational techniques at least partly invented by the child. (e.g., he solves a written addition problem partly by counting on the fingers and partly by "carrying" as taught in school.)

Since the literature suggests that Americans and Africans are characterized by similar general cognitive processes, informal mathematics, and academic knowledge, we advance the hypothesis that difficulties in the understanding of arithmetic, specifically elementary addition, also take similar forms in the two groups. Africans experiencing serious difficulty in school arithmetic should exhibit the following phenomena shown by research, mainly involving case studies (Ginsburg, 1977), to be characteristic of American children. First consider some "strengths":

1. *Standard algorithms.* Sometimes, even children experiencing difficulty are capable of simple algorithms for addition. Often, they can use these only under limited circumstances, as when no carrying is required.

2. *Number facts.* Similarly, these children are in possession of some elementary number facts.

3. *Invented procedures.* While these children often cannot accurately employ the standard algorithms, they may be able to solve written problems by means of "invented" procedures. An example is combining column addition with finger counting.

4. *Informal procedures.* While some of these children are unable to deal with a given type of written problem (e.g., column addition), they may be able to solve essentially the same problem when concrete objects or spoken numbers (and no written representation) are involved.

5. *Understanding principles.* Despite computational difficulties, children sometimes comprehend basic principles like comutativity.

Consider next some "weaknesses":

6. *Error strategies.* Often these children produce consistent errors as the result of a systematic rule. Examples are, (a) Reading or writing numbers as they sound. The child writes "403" for "forty-three," and (b) Diagonal addition.

7. *Wrong number fact.* Occasionally the child is incorrect because he or she has the number facts wrong, as in $2 + 2 = 5$.

8. *Counting errors.* Occasionally, the child miscounts, which can have deleterious effects on informal or invented strategies.

9. *Arbitrariness.* Sometimes these children believe that mathematical procedures are completely arbitrary, bearing no relation to reality. Hence they are willing to accept absurd results $(1 + 100 = 200)$ or contradictions between incorrect written calculations and correct informal calculations because: "That's the way you do it."

10. *Wild guess.* Occasionally these children make wild guesses, apparently without any calculation or thought.

11. *Misunderstanding of principle.* Children may misunderstand basic principles on an explicit, verbal level. A common example involves place value.

It is also possible to characterize these children's cognition in terms of a kind of "deep structure," that is, relations among the elements already described. Several such structures have been observed:

12. *Invented-written gap.* Some children, while capable of clever invented strategies for the solution of written problems, nevertheless cannot solve these problems by the standard algorithms.

13. *Informal-written gap.* Some children, while capable of calculation with concrete objects, cannot use any form of written symbolism, conventional or invented, to solve essentially the same problems.

14. *Overall weakness.* Some children lack all three major procedures for dealing with calculational problems: informal, invented, and conventional.

Methods

This paper presents brief statistical data concerning four African children experiencing serious difficulty in school arithmetic. The children were from two cultural groups within the Ivory Coast, the Baoulé and the Dioula. Traditionally, the Baoulé have supported themselves through subsistence farming, employing very primitive procedures and tools. The Baoulé are an animist group, and live both in traditional villages and, more recently, in growing urban centers. Two of the four subjects involved in the present study were drawn from this latter group. The Dioula are a Moslem group and less homogenous ethnically than the Baoulé. The Dioula have traditionally engaged in mercantile activities, and are scattered throughout West Africa. Like the Baoulé, many Dioula are now settled either temporarily or permanently in urban centers. Our Dioula

subjects were drawn from those maintaining relatively permanent residence in a small urban center. All subjects can be considered newly urbanized individuals, whose families still maintain close links to the village culture. All subjects were in the third grade. The two Dioulas were NC, a boy, 8-10, and AL, a girl, 9-3. The Baoulés were ND, a boy, 10-1, and AA, a girl, 9-3. These children were first encountered when they served as subjects in the Ginsburg, Posner, and Russell studies. The interviewer (JKP), consulting with the teachers, asked for children experiencing severe difficulty in school mathematics. The teacher suggested these children, as well as a few others, who were eliminated from this study because they refused or were unable to communicate with the examiner. For further information concerning the nature of the schools, see Ginsburg, Posner, and Russell (in press, a).

The children were interviewed in French, the language of the school. The interview was designed to center around common difficulties in the calculation of elementary addition problems. After a few fairly standard questions were asked, the interviewer was free to pursue the child's responses wherever they led, in the manner of the Piagetian clinical interview. Similar interviews are presented in Ginsburg (1977, chapter 7). As the interviews were given, an African assistant was available to ask questions in the child's native language. Occasionally, when the French seemed to be misunderstood, this procedure was used. The interviews were tape-recorded, and later transcribed and translated for further analysis, which was conducted as follows.

First, one of the authors (HPG) read each interview and identified relevant segments for further analysis. The only parts of the transcripts which were omitted from consideration involved those dealing with completely extraneous material, or with attempts by the interviewer to teach a certain concept once a difficulty had been identified. Next, the analyst coded each identified segment into one or more of the 14 categories described in the introduction to this paper. In addition, the coder determined whether the behavior in question was similar to behaviors observed in American children. To assess reliability, a second analyst (RLR) independently coded the previously identified segments. In addition, he reviewed the protocols to determine whether other relevant material had been omitted from consideration, and found that none had been omitted. The first and second analysts agreed on 75% of the judgements concerning NC (15 of 20 judgements); 82% of the judgements concerning ND (13 of 16 judgements); 76% of the judgements concerning AA (19 of 25 judgements); and 80% of the judgements concerning AL (16 of 20 judgements). The overall agreement was 79%. In general, the judges seldom disagreed. Discrepancies in scoring usually resulted from one judge failing to notice a behavior scored by the other judge, or from ambiguities in the code.

Results

The results showed that every behavior exhibited by the four subjects is similar to American children's behavior. (Both analysts agreed *in all cases* that the similarity was present). Thus, our first finding is that not one behavior of the African children was considered

different from American children's behavior.

Table 1 shows the frequencies of categorized behaviors in each of the four subjects. Several points are worth noting. First, the African subjects exhibit various strengths. They occasionally use the standard algorithm and number facts. More importantly, they are capable of invented procedures and informal procedures, both of which are developed, at least in part, independently of schooling. Table 1 shows that all subjects use either invented strategies or informal procedures. Two subjects use both. Similar strengths are typical of American children experiencing difficulty in school arithmetic. Second, the most common weakness among African children is the error strategy. As Table 1 shows, these behaviors are more frequent than any others, and include common error strategies like reading and writing numbers by sound, and diagonal addition. Third, the African subjects show no evidence of explicit, verbal understanding of principle, again a common phenomenon among children experiencing mathematics difficulties. Fourth, one type of "deep structure" appears consistently, namely the gap between informal procedure and written work. All of the subjects show this pattern: while capable of sound informal procedures (involving, for example, mental calculation with money)

they cannot successfully execute the analogous written procedures. Subjects do not exhibit any other type of "deep structure." (Full interpreted transcripts for each subject are available from the senior author).

Discussion

The results support the hypothesis that African children experiencing difficulty in school mathematics exhibit cognitive processes like those of their American peers. These processes include informal strengths, like informal procedures, as well as weaknesses like error strategies and misunderstanding of principles. The Africans exhibit a gap between faulty academic knowledge and surprisingly powerful informal and invented procedures. These results reinforce the findings of our previous study (Ginsburg, Posner, & Russell, in press, a) that Africans' academic knowledge is quite similar to Americans' despite differences in culture and to a lesser extent, in schooling.

The results reveal several key aspects of cognitive processes in the cultural groups studied. First, our results agree with other findings in showing that traditional peoples may process informal mathematical skills for use in non-written tasks (like the mental addition of money). Second, our results show that traditional people may assimilate what is taught in school into their informal knowledge. The result is the "invented procedure." This is not the rote learning of school material but a "creative act." Third, our results on error strategies make a similar point. These strategies are systematic procedures (leading however to error), not random responses. Traditional people's mathematical behavior is generated by abstract rule; the errors are not always, or even frequently, the result of rote responding. The errors, as well as many of the successful responses, derive from a creative activity.

Further research on these issues is required since the present study suffers from two shortcomings. One is the small number of subjects. The other is the fact that subjects were newly urbanized individuals. While it seems unlikely that a brief period of urbanization could overcome the effects of culture, future research might investigate learning difficulties in larger numbers of traditional peoples in non-urban settings.

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Table 1
Frequency of Categorized Behaviors in Each of the Four Subjects

| | Subject | | | |
	AA	AL	NC	ND
Strengths				
Standard algorithm	0	1	2	0
Number facts	1	4	2	1
Invented procedures	4	0	0	5
Informal procedures	2	2	4	2
Understanding of principles	0	0	0	0
Weaknesses				
Error strategies				
Reading or writing numbers by sound	2	2	0	2
Misalignment	2	3	1	1
Carrying problems	3	0	2	1
Diagonal or sideways addition	1	1	1	0
Zero makes zero rule $(3+0=0)$	1	0	2	1
Other	0	2	0	2
Total error strategies	9	8	6	7
Wrong number fact	0	0	1	0
Counting error	2	0	0	0
Arbitrariness	0	1	1	0
Wild guess	0	0	0	0
Misunderstanding of principle	1	0	1	1
Deep Structure				
Invented-written gap	0	0	0	0
Informal-written gap	3	3	4	1
Overall weakness	0	0	0	0
Total	22	19	21	17

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Psychological and Anthropological Approaches to Cognition

Roy Goodwin D'Andrade
University of California, San Diego

A. Kimball Romney
University of California, Irvine

EDITORS' NOTE: The following article is reprinted from a special issue of the *American Anthropologist* published in June, 1964 (Volume 66, Number 3, Part 2). It is a statement by two of this country's leading cognitive anthropologists of major issues dividing psychologists and anthropologists interested in relationships between culture and cognition. We present this material because it summarizes in especially precise ways, theoretical and methodological problems that are as central to the understanding of intellect in 1981 as they were in 1964.

In this chapter we shall attempt to report the major issues that arose during the discussion of the papers. In general, the same issues arose again and again during discussion and had less to do with particular papers than with the differences in basic assumptions among the disciplines. Therefore, we have organized the comments around issues rather than around the papers.

Before presenting excerpts from the discussion, we have attempted to clarify the differences in basic assumptions among the three disciplines represented at the conference. It seems to us that the assumptions, theories, and methods used by members of a scientific discipline are strongly affected by the way in which the phenomena under study are defined. Thus, differences in approach among disciplines should be related to differences in viewpoint about the nature of the phenomena being studied. What different aspects of human behavior are being studied by anthropologists (including linguists) and psychologists who are interested in cognition? We believe that the anthropologist focuses upon the study of learned codes shared by groups of individuals, while the psychologist focuses

upon the intellective processes of the individual.

We use the term "code" to refer to the set of rules for the appropriate construction and interpretation of a message; so a code is part of the process of communication in which an addresser sends information within some context to an addressee, using some physical channel to transmit signals. Thus, for example, in a game of chess (the context), a player (addresser) may make a move (send information) according to the rules of the game (the code) by mailing to his opponent (addressee) a postcard on which is written (channel), "p-k4" (the signals). Notice that the same code is used to send different information and that the same information may be sent using different signals in different channels. The signals constitute the actual behavior, while the code consists of the rules or shared understandings by which the signals are transformed into messages carrying information.

Codes may form complex hierarchies of representation. The phrase "hierarchy of representation" refers to the use of information in one code to stand for or represent signals in another code. In chess, for example, the player must learn at least three codes; one code in which physical pieces or written letters constitute signals that must be transformed into information about pieces, a second code in which each piece constitutes a signal that must be transformed into information about potential movements, and a third code in which various combinations of potential movements constitute a signal that certain outcomes have occurred, such as *check, checkmate, capture,* etc. Language may similarly be segmented in a hierarchy of codes consisting of phonemic, morphemic, lexemic, and sememic strata. A speech utterance may also serve as a signal in a code concerning social relationships, as in the use of the *tu* and *vous* pronouns in French which serve to signal information about relationships of power and solidarity.

In saying that the primary interest of anthropologists who are studying cognition consists of socially learned codes, we are not implying that codes are the primary type of data for all anthropologists. However, if "culture" were to be defined within the communication vocabulary, perhaps "code," rather than "signal" or "information," describes most accurately what most anthropologists intuitively feel is the proper object of study. Kluckhohn, for example, consistently held that culture is not behavior, but rather a construct which makes behavior intelligible. Similarly, Levi-Strauss and Needham hold that a social structure does not consist of actual interpersonal behaviors but of the organization of relationships being *expressed* in these behaviors. Generally these positions appear to treat behavior as signals, the anthropologist's task being to investigate the code that makes these signals intelligible.

In labeling the primary phenomena of cognitive psychology as intellective processes, we are referring to the psychologist's interest in a very general sequence of external events and human responses which can account for the behavioral performance of individuals. Intellective processes that have interested psychologists include categorization, inference, discrimination, and generalization, as well as more complex activities such as synesthesia, insight, and styles of problem solving. The relationship between the codes an individual learns and the intellective processes of the individual is

apparently quite complex. Such processes as categorization and inference, for example, appear to be built into codes, providing the individual with a ready-made set of categories and inferences for use. However, to allow the individual to use these cognitive maps which are built into codes also demands the exercise of other complex intellective processes.

In order to illustrate how the study of intellective processes *vs.* codes might have affected the theories, methods, and assumptions of anthropology and psychology, imagine that these disciplines were limited to investigating ordinary game-playing behavior in such games as chess, checkers, patolli, etc. The psychologist's material would then consist primarily of the intellective processes of the people playing games, while the anthropologist's major material would consist of working out the codes or rules of the games employed by game-playing societies around the world. Each discipline would develop its own methods and theories to accomplish these different goals. The anthropologist, for example, would quickly discover that standing behind trees and surreptitiously observing games is not nearly as efficient a technique of decoding the players' signals as having someone try to teach him the game. He would thereby come, in time, to define his role as that of a "participant observer," in which he is expected to know how one should play a game, even if he cannot do it very well.

Second, the ethnographer would learn not to try to force the players to behave in unusual ways, since the players might then decide this must be some new game and abandon the effort of trying to utilize and explain the original code.

Third, the ethnographer would probably learn to distrust what his informants say are the rules of the game, because they will often leave out some rules or explain things in ways which may not cause the ethnographer to act appropriately. He will rely more on corrections and interpretations made during a game than on what the informant says are the rules before the game starts. For example, in chess, an informant's statement that a king can move one space in any direction might have to be revised when the ethnographer tries to move his king next to the opponent's queen. However, if the ethnographer must choose between watching a game, without being able to ask for interpretations or comments, and interviewing an informant not actually playing a game, he will probably find that interviewing is somewhat more informative than observing, provided he knows something about how to ask appropriate questions.

Fourth, the ethnographer's investigations would probably convince him that while each game has a *shared* code, not all behaviors which are shared by the players are part of the code. For example, the ethnographer may notice that, in chess, players make their moves rapidly at the beginning of the game and slow down considerably as the game develops. This shared behavior, however, may not be part of the rules of the game. Rather it may be a by-product of the kinds of rules and piece arrangements found in chess in conjunction with the individual's reaction to the complexity of the intellective processes demanded by the opening *vs.* middle game.

Fifth, the ethnographer would probably come to distrust generalizations made about human behavior across all classes of games, since for him most behavior is "determined" by the code in use, which varies by game.

Sixth, ethnographers would eventually develop a series of formalized procedures and constructs by which games could be adequately described. This conceptual apparatus used by the ethnographer would constitute a descriptive theory of games-in-general, to be tested in each new variety of the game.

Finally, some ethnographers might develop the argument that there are no rules, as such, in the minds of the players and that the so-called rules are really inventions of the ethnographer, created as a convenient device for efficient description. However, other ethnographers would argue that the rules are psychologically real, despite the fact that their description can only be an approximation of the true rules held by the players.

In contrast to the traditions of the anthropologist, the psychologist, investigating the intellective processes involved in games, would have elaborated a different set of methods and theories. In order to investigate these processes, the psychologist might initially approach the problem by trying to discover what abilities differentiate winning players from losing players. For this problem, the ethnographic technique of participant observation would be found to be less efficient than the use of special testing procedures. For example, the psychologist might hypothesize that one important intellective process in chess playing is visualization. In order to test this hypothesis, the psychologist would first have to develop a test of visualization. Unlike the ethnographer, who tries to remain with the natural situation, the psychologist would be likely to create a novel and simplified task for his test in order to cancel out the effects of previous learning and to insure that only one specific process was being required.

In order to test the validity of his hypothesis, the psychologist would use procedures which are different from those of the ethnographer. The ethnographer tests the adequacy of his formulation of the code by finding out if this formulation is able to regenerate the behavior with which he is concerned. The psychologist tests the adequacy of his hypothesis by finding out if results in the testing situation can actually predict the results in *other* situations. For example, the psychologist might construct a simple test of visualization by asking subjects to tell him, without using a board or pieces, how many moves it will take to move eight knights placed in a row on one side of a board to corresponding positions on the other side of the board. If the results of this test can predict which players are most likely to win in chess matches, the psychologist would feel that his hypothesis has been supported.

Further contrasts with the ethnographic approach would also follow from the psychologist's avenue of investigation. The psychologist would be likely to consider that shared behavior in response to a situation indicates that some general process is at work. Thus, if almost all his subjects can visualize the movements of rooks more easily than the movements of knights, the psychologist will suspect that this regularity reflects something about a general process concerning the ease of remembering and reordering few *vs.* many bits of information. The ethnographer, on the other hand, would be more likely to leave out of his formulation shared behavior that did not appear to be part of any

code. Again, the psychologist, if he finds that visualization is related to how well a subject can play chess, will expect that even in different kinds of games, the better visualizer will still win most frequently. The ethnographer, however, will be more likely to suspect and, where possible, find exceptions to such cross-code generalizations. Finally, the psychologist's theory, rooted in processes, would be more likely to specify the relationship between antecedent and consequent events, in contrast to the ethnographer's more classificatory theory.

Both the psychologists and the anthropologists, however, might develop similar controversies within their own disciplines concerning whether the events referred to in their theories actually occur or are only convenient fictions. Thus, some psychologists might argue that since the process of visualization of chess pieces cannot be directly seen and measured, it should be considered as a fictional event, dubiously useful only in prediction. Such an argument would be similar to that among anthropologists as to whether or not the code or rules of the game are ethnographic fictions without the status of ontological reality.

The analogy of psychologists and anthropologists each studying different aspects of game-playing has been presented in some detail. This analogy should prove helpful in understanding what lies behind the discussion material presented below. The notion on which this analogy is based, concerning the two different conceptions of cognition, was formulated after the conference. While listening to the tapes and transcribing quotations of the various participants, it began to appear as though many of the issues could be reduced to one: whether the transcultural study of cognition is primarily a study of codes or of intellective processes.

The issues are presented in approximately the order in which each topic arose. An early topic of discussion concerned the use of *etic* vs. *emic* categories. After some clarification of what was meant by this distinction, Hymes, Stefflre, Frake, and Sturtevant argued that it is impossible to know *a priori* the content of any code. They began with the example of residence categories, such as "patrilocal," "avunculocal," etc., pointing out that the actual decisions made in a society might not conform to any previous set of categories.

Hymes: The point of this discussion on residence is that the domain of residence is involved in the *total* culture, and people may differ from one group to the other in the functions of residence choice. In the same way, the function of kinship terms, so called, may differ. For example, kinship terms which may be translated as if their primary function were to classify biological relatives may actually function in a very different way. They may, for example, be primarily role designating terms in the culture, and it may not be possible to understand how the system really works if it is thought of as a classificatory system applying to biological kin types. . . .

Stefflre: One way of saying this is that first, the domains may not be the same in color or kinship and second, the features that are used in differentiating objects in the domain may also be different; they *may* be hue, saturation, and brightness, or the kin type notation, *or* they may be something completely different. Anthropologists sometimes become uneasy when they hear that psychologists use hue, saturation, and brightness in testing color discrimination in other cultures because they think other

dimensions actually may be in use. However, anthropologists do exactly the same thing in their own analysis of kinship. You have both problems--one, the domain, and two, the dimensions. These have to be developed internally for certain kinds of purposes.

Frake: I think that brings up an important point. You can convince anthropologists, and perhaps psychologists, of the fact that people may classify an uncle, or the spectrum, differently, but when you try to talk about large domains like kinship, the fact that you have to define these as well in terms of the culture--that you can't define beforehand that a kinsman is anyone who can be defined by a kin type--this is a little harder to get across.

To this general position, Osgood noted that much of the data given by the anthropologists concerned the use of language. The discussion then shifted to a consideration of the relation between language and culture.

Osgood: I've been thinking about these examples of Trukese residence and Hannoo color classification. To illustrate one of the very basic questions here, let's suppose the entire issue as to whether residence is patrilocal or matrilocal on Truk is completely irrelevant--suppose there are other factors below the level of awareness of the Trukese which determine where they live. An anthropologist might then make an analysis which is irrelevant and yet statistically significant. Now if this is true, there is a basic question involved.

In psychology we have had the argument for many years, "Is language a mirror of thought or an adequate mirror of cognition?" The answer usually given is, "No there are many aspects of thinking below the level of verbalization." Now the questions raised by many of these papers is whether language is an adequate mirror of *culture*. That is, can one have an ethnoscience if its fundamental way of getting at (culture) is language categories? In a sense, are you not already predetermining an answer that says, "My view of culture, of ethnography, will be that which is presented by language"?

To rephrase this question within the framework of the game analogy, Osgood appears to be asking if all the cognitive operations involved in a game can be discovered from the way in which people use words concerning the game. In answer to this, Stefflre proposes that knowledge of the way in which pieces are labeled will, at least, help predict which pieces will be used in similar ways.

Stefflre: I think the ethnologists make an assumption that things people respond to in the same way nonverbally they will respond to in the same way verbally. There are different levels of sensitivity and subtlety you can use in such an analysis--you can use free association as a clinician might, etc. Ethnographers pick the lexicon and take that as their unit, assuming that things which are given the same word are going to function similarly in the culture. Now if they were trying to make a sensitive analysis of an individual who may not have developed a vocabulary of words, this would be absurd to do. On the other hand, in a culture where people make certain distinctions with high frequency, it becomes easier, I think, for them to label their distinctions and rote learn a label than to generate circumlocutions. It seems to me, while one can easily find examples where behavioral invariances aren't reflected in the lexicon, a good first approximation for an ethnographer is to assume that things classified the same way lexically will function similarly in a culture.

Frake continues the discussion by pointing out that an ethnographer will do more than find out how people talk.

Frake: Most ethnographers do look at behavior other than

speech; we don't just sit down with an (informant) in a room and talk to him. I do not believe an adequate ethnography can be produced solely from a record of what people say. One often needs to look at nonverbal performances and their physical settings in order to know what people are talking about. Also, one cannot *emically* segment and interpret nonverbal behavior and physical objects as artifacts of culture without some reference to the way people talk about them. To me, the important distinction is not between verbal and nonverbal behavior, nor between language and culture, but between observable behavior, including speech, on the one hand, and the socially transmitted system of knowledge--or culture, which includes language--on the other. The ethnographer's task is to account successfully for the observable behavior by constructing a theory of the nature of this socially transmitted system of knowledge.

Hymes: I think it's clear that cultures and societies differ on the functions of language. For example, in an article by J. B. Adams on Egyptian village politics in the *American Anthropologist,* there is a discussion of methods of communication in this village. Most of the things which villagers say to each other in the course of the day are stereotyped--the lexical and syntactic content is predictable. What gives information is the *way* they say it. This is expressed very subtly, and attended to very closely.

What I'm arguing here for, briefly, is some empirical ethnographic studies of the uses to which language is put and the way in which its use is organized in different societies. I think that there are some indications that a number of variables affect the way this language is used and also its acquisition. For example, in our own society there have been studies of language acquisition which indicate that it varies with the number of children in the family and their age. This could be looked into cross-culturally. It might be that different kinds of social structure or family structure would affect this. I think it's clear different groups differ in the way they conceive the child's acquisition of language, and the way in which they respond to the child as a linguistic performer, and this might affect the way in which language skills are acquired or what kind. I've been very impressed by the fact that its apparently the case that a lot of adult skills, in some societies at least, are learned with very little intervention of language.

In other words, I think that we sometimes have to ask how *unimportant is language* in order to understand properly its actual place in a given society. I don't think that it has exactly the same functions from one society to another. It doesn't enter into experience and cognitive activity in exactly the same way. This is raising questions I can't answer, but I think this is where anthropology--also joined nowadays by sociologists and psychologists who get around the world about as much as we do--has a great deal to offer to our understanding of language.

Another problem relates back to the discussion of emic *vs.* etic descriptions. The issue concerns the problem of comparability. That is, if each culture is described "in its own terms," then how can cultures be compared?

Kessen: The general issue which we have discussed today concerns the hope of being able to describe a culture in its own terms--being able to describe a culture from within-- of evolving a general *a priori* theory of description that applies to all cultures. I think this is an impressive and ambitious scheme. I also think it is possible to construct by *a priori* methods a theory for accounting for behavior in each culture. One can have a theory of Chiapas Indians and a theory of Chicago slum people which are independent of one another. If they are truly independent, though, it seems to me that they are then incomparable. The anthropologist seems to be in the tension between wanting to be a comparable anthropologist and wanting to

do internal structure construction. This seems to me *too* ambitious, for to the extent these internal structures are independent, they are incomparable. Also, to fully describe a culture in this way is a tremendous job, perhaps an impossible job.

Sturtevant: There is a descriptive point of view and a comparative one. You say that if cultures are different, then they are entirely independent and not comparable. There is an answer to this. I think the standard one is, "Well, really we are after those things which *are* comparable. Therefore, we control to see what are the true correspondences between cultures."

Kessen: That's really the heart of the matter, isn't it?

Sturtevant: If Frake can discover how the Subanun think in terms of phrase linkages, and if Berlin and Romney can discover how Tzeltals classify, and so on, then what is interesting and possible to compare is the *principles* by which they think, rather than the particular content.

Stefflre: I find the contrast set nomothetic *vs.* ideographic an absurd one. Descriptions of different cultures are not independent and incomparable. The only way you can describe a culture, if you describe it in its own terms, is by applying certain procedures and then applying these same procedures elsewhere. What you are going to come up with are different *systems,* but the descriptive techniques are *universal,* and they are very comparable. They're not substantively comparable, but in the procedures used.

Frake: The methodology itself might not depend on any particular theory, but if the methodology is successful, that methodology could imply a general theory.

Osgood: And isn't it true the only way you can communicate the uniqueness of the particular pattern or structure is by virtue of having a methodology which provides a bridge of communication? Otherwise, one could only use intuition to understand what the data mean. What I'm trying to say is even to comprehend oneself as a scientist, to understand what is unique in the pattern or structure, in a sense implies a comparative framework within which you make these discriminations. . . .

Frake: The real weakness of anthropology is not that we don't get comparable results, but that we don't have an explicit methodology.

Kessen: The question is, "What kinds of generalizations are transcultural?" I still feel there is a tension which remains unresolved between the descriptive and comparative approaches.

While the issue here is apparently simple, nevertheless it contains hidden ambiguities. First, if the term "culture," as it occurs here, is taken to mean shared behavior, then to say that the substantive content of different cultures cannot be compared is to deny that behavior manifests *any* cross-cultural regularities. Clearly, this would be an unreasonable statement, equivalent to saying that the cognitive processes in playing chess cannot be compared to cognitive processes in playing checkers.

However, if the term "culture" is taken to mean the codes involved in communicating, then comparison between cultures becomes difficult. We cannot meaningfully compare chess and checkers by asking how many round pieces each game contains; congruence in the physical signal does not imply congruence in the message being sent. The only possible way to compare these two games is to find congruences in the structure of the code; e.g., both games use "capture" or the removal of the opponent's pieces, although one game captures by "jumping," the other by "replacement."

The point here is that the term "culture," to the degree it is used in both senses, creates considerable confusion, since what is true of "culture" as code is not true of "culture" as shared behavior.

Another issue which was discussed concerns the reality of anthropological and psychological descriptions.

Sturtevant: Frake makes a statement in his paper--"We must get inside our subjects' heads. This should not be an impossible feat: our subjects themselves accomplished the same thing when they learned their culture and became 'native actors.' They had no mysterious avenues of perception not available to us as investigators." If I understand this correctly, I think this statement is perhaps put a bit too strongly. I agree as to the aim, but I'm less convinced of the practical possibility of doing this since an outside observer can never become a full participant in a culture, and surely he's not going to learn the culture in the same way that the participants did; he's not enculturated, socialized, beginning as a child without any culture. I think this is related to what Wallace and Atkins have discussed as the difference between a psychologically real and a structurally real description. Wallace and Atkins say social structure reality can be achieved, psychological reality can only be approximated. If I understand this correctly, it seems to me a reasonable statement.

Frake: I'd like to address myself to the issue which Sturtevant has raised. Frankly I've never understood the distinction between *psychologically real* vs. *structurally real* descriptions. I just don't know what the difference is. It seems to me that a description is good to the extent that it accounts for something we are trying to account for (in this case, the kind of behavior that the Subanun would consider appropriate). And the better description is the one which accounts for more things. This is to my mind about the only criterion we have for what's psychologically real.

Brown: I think that's right. Perhaps the form the problem of "psychological reality" takes in current psychology is chiefly the question of whether or not simulation models of cognitive processes are psychologically real. For example, one can write a program that will reproduce many of the features of nonsense syllable serial learning. Then the question is, "Yes, but do the operations which were put into that program have corresponding operations in the human head?" I think the answer to this question has to be, "I will feel more confident that it is psychologically real, the greater the range of demonstrable human phenomena that it reproduces." For example, we have done some work in writing grammars for children, trying to generate their sentences. Initially we were in the position of wanting to say there were an enormous number of grammars that would fit the facts we had. The trouble was, we were not requiring enough of those grammars; we were just requiring them to turn out utterances. If we required them to represent the speaker's sense of what are sub-whole within sentences or constituents, that greatly reduces the number of conceivable grammars. It seems to me the question of psychological reality is never settled by test, instead what happens is that as you require more and more, there are fewer and fewer competing descriptions until at last it is such an enormous job to find an alternative model for the total range of data that nobody ever does and there really isn't any contest.

One point should be emphasized with respect to the problem of psychological reality. This is that the test of the psychological reality of a description seems to involve not only the ability of the description to encompass a wide range of a particular kind of behavior, but also to make predictions about new and different classes of behavior. Thus, in Roger Brown's example of testing

the psychological reality of a grammar, part of this test involves asking subjects to perform the novel task of breaking sentences into subunits. This task elicits a different kind of behavior than being asked to generate utterances would. In one sense, then, testing for the psychological reality of a description is to require the description to do more than just describe. It should also predict behavior which is different from the behavior on which the description is based.

During the discussion of Osgood's paper, several methodological objections were raised by the anthropologists. The objection which received the most attention involved the use of "natural" *vs.* "forced" questions in cross-cultural investigation.

Sturtevant: In using the semantic differential, people are forced to evaluate objects, and apparently there is consistency in the way in which they respond. I don't understand what the meaning of this consistency is in terms of the culture in which it occurred. And until I understand it, I don't see any point in comparing across cultures. You say that these factors are related to "visceral reactions"-- then perhaps what this consistency means is that cultural differences have been wiped out and only a common animal reaction remains.

Osgood: No, just the reverse. We are trying to find a framework of similarities within which we can observe more meaningfully and more rigorously real differences. Then we can be sure that these are real differences and not just artifacts of the language being used.

Strodtbeck: If you did investigate which adjectives went together in a language and culture which you know, do you doubt seriously that you would find three dimensions similar to evaluation, potency, and activity?

Sturtevant: I don't know. And the reason it bothers me is that it seems so artificial to force people to tell you whether "Wednesday" is "good" or "bad." And I don't understand what significance any consistency you may get in forced tasks such as this has for understanding a culture.

Osgood: Why is it more arbitrary to employ the kinds of probes used in the semantic differential than it is to employ the "kind of" and "part of" questions Frake uses? What is there unique about one kind of questioning which makes it arbitrary?

Frake: I think we're doing the same things for different reasons. We are both asking people questions and getting answers. But one difference is that if people laugh at one of my questions, I throw away the question. If people laugh at what you ask, you force an answer and say, "Come on, boys, now this is serious."

What I would like to know is, "How much force is legitimate, and is the amount of force a variable to be taken into consideration?"

Osgood: This is the narrow line we have tried hard to walk. That is, in order to get comparability, we have had to use enough control over the general context in which the data is elicited to obtain some reasonable security of equivalence. The problem, then, is to make sure that the kind of control which is exercised for that purpose can in no way influence the substantive relationships within the material. . . .

However, I want to go back to this business of laughter, because I think this may be being overdone. And perhaps in illustrating this, we may get a little tighter grasp on the problem. Let me give the illustration of a comparative study we did involving Americans, Navaho, Japanese, and Spanish-speaking Mexicans. We used a set of cards, each one divided in half. On each half there was

a simple visual comparison. For example, there was a black circle vs. a white circle, an upward pointed arrow vs. a downward pointed arrow, a jagged line vs. a wavy line, etc. We took sets of words from each language and asked our subjects to point to the side of each of these cards which best represented that word. For the word "happy," for example, it would typically be the white circle, the smooth line, the upward pointed arrow, etc. Now, the point is, whether they laughed or not, they came up with consistent responses. That is, in taking those items for which, within each culture, they agreed clearly in which direction the word should go, and then making comparisons for these items across cultures, we found that cultures agreed with each other on the direction. For Japanese and American items, 99% of all comparisons went in the same direction. The most divergent culture was the Navaho, and they agreed in about 90% of the items with other cultures. My question is, "Can't I say there is generality in verbal synesthesis, even though they may have laughed when we asked them whether the white or the black circle is 'happy'?"

Stefflre: When people laugh at such questions, what they are saying is that the dimensions you are using aren't those they use for these objects. However, they may agree on how these dimensions *could* be used for these objects.

Frake: I am not saying one can't use normally inappropriate questions. But what I want to know are simple things. The assumption is that there is a set of questions in a language and that these questions have different ranges of appropriateness or applicability. You can't appropriately ask any question about anything. I want to know what the acceptable range of a question is in order to find out what are considered the salient features or relationships between objects. Now if I'm going to do this, I have to have some criteria for determining the range of the question. Laughter is one criterion for finding out if the question is appropriate. A more common response to an inappropriate question might be for people to remark, "We don't say that."

Sturtevant: It seems to me that the first step is to find out what the folk classification is and then to see how people can generalize this structure in novel ways, using techniques like the semantic differential.

Hymes: From my view of the goals of linguistic theory, there can be no conflict between the semantic differential approach and the ethnoscience approach because, as Osgood's work has made so abundantly clear, people *do* have the ability to do the things he asks them to do. My view of a completely adequate linguistic theory has to account for the fact they can do it. If they couldn't do it, that would be another thing; but he hasn't come here telling us about all the troubles he has had trying to get people to do this. He's telling us one of the amazing things about it is that people find it so easy to do, and therefore this is in some sense a linguistic ability people have which needs to be part of the data when linguistic skills are described.

Also, it should be mentioned that when people laugh, one doesn't necessarily know what it means because laughter is a communicative act; and it has to be understood in terms of the structure of communicative acts in a particular culture.

In this discussion, certain of the differences between traditions stand out sharply. What seems to lie behind the divergence in viewpoints is the previously discussed assumption by anthropologists that "cognition" is equivalent to the code held by a group of individuals, in contrast to the psychologists' assumption that "cognition" is equivalent to mental capacities or intellective

processes. Given the anthropologists' assumption, asking inappropriate questions would be an inefficient strategy, since in order to investigate the nature of the coding system, informants must be kept within the system. It should be made clear that here the term "inappropriate" really means "outside the coding system," rather than "evaluated negatively" (although many performances which are outside the code will be evaluated negatively). For example, in terms of chess games, the question, "which can move *faster,* a queen or a pawn?" is somewhat inappropriate. The more appropriate phrasing is, "which can move *further,* a queen or a pawn?" That is, in chess, pieces move through positions, and what is criterial is what moves are possible for a specific piece. (However, since a queen can move further, she can get to distant positions in less moves, and in this sense, can travel faster.) The question, "which can move faster," is not inappropriate because it is in bad taste, but because under the rules of chess, pieces do not, in making a single move, travel "faster" than others, only "further" or "differently."

In contrast to the investigation of codes, the study of cognitive operations is not dependent upon asking questions or requesting performance within any learned code. We have argued that in order to test for such capacities, the psychologist will usually have to request performance outside any code, so that he can construct a test which is not influenced by previous learning and which measures only one specific capacity. This means that the psychologist will typically "force" subjects to do unusual tasks. From this point of view, using the ethnographic criterion of "appropriateness" appears to be an arbitrary device, since any kind of shared behavior, no matter how bizarre, may be the result of an interesting intellectual process.

One substantive stipulation needs to be made, however. That is, while Osgood's example of testing for synesthesia might be said to be an example of requesting performance outside the normal coding boundaries, the testing procedures for establishing the adjective factors reported in Osgood's paper in this volume are carefully constructed to keep within the coding boundaries of each culture. The "adjectives" which were selected through natural linguistic frames have a wide range of applicability across semantic domains. Also the scale procedure for rating allows the individual to indicate which adjectives are not relevant modifiers of particular substantives. And finally, the principle of grouping adjectives used in factor analysis, despite its statistical complexity, is basically that of co-distribution, which is also a basic principle of linguistic and cultural analysis. One piece of evidence presented by Osgood at the conference supports this view that the factors of evaluation, *et al.,* are part of the semantic code. In the English frame, "He is _____ but _____," where "but" is used contrastively, people will often fill the frame with a pair of terms which have positions on opposite sides of the semantic differential factor space. For example, in the phrase, "He is *intelligent* but *lazy,*" the term "intelligent" is high on the factors of evaluation, potency, and activity, while "lazy" is low on all three. The fact that English speakers use such contrasts would appear to indicate that something like the semantic differential factors are actual dimensions in the code by which contradictions in expectation may be expressed. (It is

interesting that a number of the anthropologists felt that if Osgood had more of this kind of evidence, they would be more convinced of the universal psychological reality of three factors. One anthropologist stated that the statistical treatment was less than meaningful for him, but that a demonstration that the factors were actually used by people was impressive.)

The final topic of discussion selected concerns the relationship between codes and the degree to which behavior is shared. This topic came up during the last sessions of the conference and helped make explicit the basic differences between the psychological and anthropological traditions in the study of cognition.

Strodtbeck: One of the things I have noticed is that when the point is made that certain methods of linguistic theory involve the exploration of systems that are so overlearned that any one respondent can tell you about them, we don't move on in the discussion except to say that there must be a whole range of other things which are not so overlearned. Those of us who have been trained primarily in sampling methods don't know how to work with these overlearned kinds of behavior, and those who have been trained to work with the overlearned kinds of behavior begin to feel uncomfortable when the sampling problem is raised.

Stefflre: In a way, it's not as bad as it looks since the use of certain kinds of information which small numbers of people can give often predicts regularities in the behavior of very large numbers of people. And these are the kinds of information which ethnographers know how to obtain.

Strodtbeck: I agree, but knowing how class-specific much drinking behavior is, I have reservations about the sampling universe. However, when we get into problems such as connotative meaning of the word Truman, which we were discussing, where the meaning is different for Republicans and Democrats--when you get into this kind of problem in which one cross-break of the sample will change the parameters of a response, then we could use the methods of survey research. Methods for organizing both the responses and the respondents simultaneously, as well as various scaling techniques, are undoubtedly going to be used when linguists and ethnoscientists begin to explore behavior which does not have this overlearned characteristic.

Stefflre: The only point I'm trying to make is that anthropologists frequently get criticized for saying, "The Chamba walk down paths one at a time--at least the one I saw did," and generalizing this instance as a statement of regularities of behavior in the culture. Now I was very surprised to find for certain things about object-object relations--not the preference distributions, but in what's similar to what--you can get verbal judgments from a small number of people which will predict how large numbers of people will respond nonverbally to these objects. However, it's indeed true they should be a representative sample of the larger population.

Osgood: I'm not sure what the difference is between these two statements. The more you have sharing of the same meanings, the fewer the number of people you need to work with to estimate what these meanings are. In phonology, for example, you can get the phonology from one informant. Why? Because it has to be so commonly shared in order for the group to communicate.

Stefflre: But if you investigate the actual phonology, you find a great deal of variation. Everybody pronounces things to some degree differently. Yet it is also the case that each speaker can communicate with every other. There is real variability in phonology, and yet everybody talks to one another. There is real variability in what people like, but there is tremendous consensus about what's like what--I don't understand why the world is like this, but it appears to be.

Brown: I share your bewilderment.

Osgood: Must one be absolute, can't one be statistical in setting up language classes? Let me give an illustration in the area of pause phenomena. If you look at filled pauses, such as "ah," versus unfilled pauses or long silences--if you look at the distribution of these two types of pauses, there is a highly significant statistical difference in where they occur. Filled pauses tend to occur at syntactical junctures or phrase junctures, and unfilled pauses tend to fall before high information value words. However, you can find some filled pauses before high information words, and you can find some unfilled pauses at phrase junctures. Now is there any reason why one must say that because there is some overlap, these are not in complementary distribution?

Lamb: This is what I take to be a question of the use of discrete or continuous phenomena. You say, "Don't we also have continuous phenomena (in language)?" I would say no. Certainly there is a statistical distribution, but these pauses are not part of the linguistic structure. In general, you can't take everything a person says--all the features of sound--and say it's part of the linguistic structure. It's essential to separate out those which you call structural and those which you call nonstructural.

Osgood: It shouldn't be that you make the decision about whether a phenomenon belongs in or outside the structure on the basis of your method.

Lamb: I see your point, but it's always necessary to do this. This is a philosophical point. One can't get anywhere in linguistic analysis unless the normal is distinguished from the abnormal, the usual from the contrived. There is a structure there, and to find it needs care.

Postdoctoral Fellowships in Comparative Human Cognition

The Laboratory of Comparative Human Cognition announces the availability of three postdoctoral fellowships beginning approximately September 1981. Fellowship periods will be for 1 or 2 years. Candidates should be able to demonstrate prior experience living in or working in one or more cultural groups other than that of their own family backgrounds. Research training will emphasize the study of cultural diversity in cognition. One one-year fellowship at the predoctoral dissertation level is also available.

Applicants should send a resume, brief statement of interests, names of three references (and may include any other useful evaluation material). All correspondence should be addressed to: *LCHC, D-003, University of California, San Diego, La Jolla, California 92093.* The University of California, San Diego is an Affirmative Action/Equal Opportunity Employer.

ANNOTATED BIBLIOGRAPHIES

Bowen, Elenore Smith. *Return to Laughter.* Natural History Library, New York: Doubleday, 1964.

Last week I set out to find 10 copies of *Return to Laughter,* a classic, fictionalized account of Laura Bohannon's field work in Nigeria when she was beginning her anthropological career. I intended to use it as the major reading for my part in a graduate serminate seminar on basic issues of cognitive psychology. Obtaining ten copies turned out to be a chore, because the book is currently out of print. So, the first message of this review is clear: go out and find a copy of *Return to Laughter* while there is still one to be found!

The second message concerns the relevance of a piece of fictional ethnography to the study of cognition. It requires a little more elaboration than the first.

Return to Laughter begins as Ms. Bohannon is about to climb into a truck which will carry her to the path nearest to the area where she has decided to do field work. It chronicles her day by day experiences over many months during which she comes to know the local language to a level where she can engage in complex disputes and common gossip, understand something of the complex ties that bind people to each other and their surroundings, and find her very notions of self put under the mundane, but terrifying pressure, of being a stranger in a land where such selves are incomprehensible.

Return to Laughter has long been a classic among cultural anthropologists, but that is not sufficient reason to explain its relevance to a psychological seminar on cognition. To explain this connection, I need to digress again.

During the past several decades there has been a rapidly growing literature relating cognition and its development to the nature of the culture in which the cognizing and developing are done. In so far as this interest comes from people trained as psychologists, the means for treating culture-cognition relations is to use some test or experimental technique devised to study cognition according to some existing theory. So, for example, Piaget's theory of development motivates the use of various conservation tasks in cultures around the world. "Culture" in this enterprise is pretty sparsely represented! As a rule, a standard ethnography is cited for those interested in more detail, and the author provides enough description of the culture to motivate the particular contrasts that s/he wants to make.

One major value of Bohannon's book to people interested in cultural influences on learning and thinking is the many rich examples of everyday thinking and problem solving that it contains. It is especially rich in hints about the many contrasts between the way that learning is organized in schools, (which finds its apex in Ms. Bohannon's techniques for rendering systematic her knowledge of native customs) and the way learning is

organized when she is engaged in village life. One of the more amusing episodes in the book relates Ms. Bohannon's inability to learn rapidly the names of native vegetation, in part because the discriminations required of her were unfamiliar, and in part because her use of writing as a technique for remembering contrasts so strongly with the way that the native children around her came to master this same material in the course of their everyday lives.

There are many fine examples of the nature of social reasoning. They provide some hint of the structure of the problems that concern people in their everyday struggle to coordinate with each other in order to survive. They also carry the deep lesson that sophistication of problem solving behaviors, knowledge of a language, and social understanding, are deeply interconnected. Characteristically, the misunderstandings provoked by incomplete understanding are important resources for learning (by analogy, I believe, with the fact that we learn about the nature of our language when we detect a linguistic error--the error tells us something about the structure of interactions that has been disturbed).

For example, on one occasion Ms. Bohannon agreed that some village women would honor her with a dance. In agreeing to this, it turned out that she unwittingly agreed to provide those who honored her with a very large gift in return. Failing to realize the obligation she had incurred, she announced that she would not purchase a ram, her traditional contribution, in return:

> It was Accident who made me see the difficulty. As he talked, I again realized that learning the language and learning the culture were mutually dependent. I had misunderstood because I did not know the full social implications of the words. . . . "It isn't right not to give Poorgbilin's women a ram. They are bringing you new yams and all manner of vegetables for sauces, and firewood and eggs and chickens."
>
> "Then why didn't they tell me so?" I too was protesting. "They did tell you. They must have told you, for I myself have heard you tell other people that they were coming to honor you with a dance."
>
> (Back home Bohannon complained that her housekeeping crew, whom she depended upon to save her from social faux pas, had failed in their task): They could not grasp the nature of my difficulty. Like everyone else, they assumed that if I used a word at all, I must be aware of all that it implied. (p. 110-111)

Here we see the full pathos of the limits of intercultural understanding and a central puzzle for cognitive psychology in a single example. Our experimental methods come out of a technological culture and the information processing devices (human "cognitive strategies," and high speed computers are two that come readily to mind) that it fosters. In seeking to understand how culture influences thought, we must learn to deal with the full range of human interactions that are the content of human thinking. Bohannon shows how difficult this process is and how ready we must be to

recognize the limitations of our own experience, no matter how superior the technology of that culture may appear to us.

And she does more. She shows us in human terms that behind all of the surface features that separate human beings, behind the web of habits and assumptions that are our separate designs for living, are universal predicaments that technology can never entirely mask.

Michael Cole
Laboratory of Comparative Human Cognition
University of California, San Diego

Langer, E.J. Rethinking the role of thought in social interaction. In J. Harvey, W. Ickes, & R. Kidd (Eds.), *New directions in attribution theory, Vol. 2.* Hillsdale, N.J.: Erlbaum, 1978.

Currently popular social psychological theories of attribution, cognitive consistency, social comparison, and equity theories are based on the assumption that humans strive to be fully aware of the details of the present situation in order to decide how to behave. In contrast, Langer proposes that many of our everyday social interactions do not require such full awareness, but instead proceed through script enactment. These scripts develop from our processing of regular, repeated situations and they represent the essence of a social situation. When situational cues elicit a script, the individual can enact a sequence of behavior without spending a great deal of thought or attention on present details.

Langer contends that script enactment can be an adaptive strategy which frees our minds from attending to the details of very routine interactions. But script enactment can also be maladaptive when the diminished attention blinds us to situational changes, leading us to make scripted responses when the situation demands an unscripted, thoughtful response. Langer offers research findings in support of the hypothesis that individuals enacting a script are less sensitive to novel goal-relevant information than individuals not relying on a script.

She raises a number of questions concerning the role of scripts in social interaction. How much do we rely on scripts in social interaction? What events are necessary to signal that a particular script may be inappropriate to the present situation? Do certain environments call for a greater amount of scripted behavior than others? If some environments elicit a great deal of scripted behavior, are these environments viewed by occupants as boring and monotonous? Could such negative effects be reversed by the creation of new, unscripted situations?

Her challenge to active processing assumptions also calls into question the generality of laboratory analyses of social interaction. She contends that the laboratory often calls for a heightened awareness uncharacteristic of many real life situations. Therefore, laboratory analyses of interaction may only generalize to unscripted

aspects of everyday interactions. Her argument suggests that psychologists must change their approach to research in order to capture the functioning of everyday cognition.

Barbara B. Brown
Department of Psychology
University of Utah

"like the practice of free speech, free thought and free reading, the act of immersion in a wholly foreign culture demands the will and the ability to think out the consequences."

Laura Bohannon

Postdoctoral Fellowships
in Cognitive Science/Cognitive Psychology

The Center for Human Information Processing at the University of California, San Diego invites applications for its visiting scholar program in Cognitive Science and postdoctoral program in Cognitive Psychology.

1. The Program in Cognitive Science seeks candidates who have strong substantive interests in Cognitive Science with training in such areas as Cognitive Psychology, Linguistics, Sociolinguists, Artificial Intelligence, Neuroscience and related disciplines.

2. The Program in Cognitive Psychology seeks candidates whose prior training has been primarily in areas of psychology other than cognitive psychology.

A flexible training program includes opportunities in interdisciplinary work, computer modeling, cross-cultural research and participation in a variety of active research programs. Fellowship stipends are approximately $16,500 per year. Some of the appointments will be NIMH postdoctoral fellowships with supplementation. Admission to the two programs is coordinated as is the training program, so that only one application for the programs is required. The exact dates and duration (6 to 12 months) are arranged by mutual agreement.

Applicants should send a resume, brief statement of interests, names of three references (and any other useful evaluation material) to: *Program in Cognitive Science, Center for Human Information Processing, C-015, University of California, San Diego, La Jolla, CA 92093.* The University of California, San Diego is an Affirmative Action/Equal Opportunity Employer.

SUBMISSION OF MANUSCRIPTS: If your work has important implications for characterizing the way people use their minds and organize their lives, we would like to encourage you to submit a brief (6 to 15 pages) article for consideration. As a newsletter rather than a journal, this publication provides a forum for discussing issues that are difficult to discuss in typical journal outlets. It is a good place to try out new ideas or report new techniques; authors often get feedback from other subscribers. Please keep in mind when preparing a manuscript that our readership is unusually broad (anthropologists, psychologists, linguists, sociologists, educators, and public policy people are all among our subscribers) and avoid jargon that is familiar only to researchers in one field. Also try to keep references to a minimum; it is the ideas, not the scholarly pedigree, that concerns us.

We would also like to encourage you to contribute items to our annotated bibliography section on an ad hoc basis. Any book or article that you have read recently (old or new) that you are enthused about and want to share with others is a likely candidate.

Please send three copies of all submissions and use the style suggested by the American Psychological Association for your references. All figures and illustrations must be camera-ready originals.

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