



THE QUARTERLY NEWSLETTER OF THE  
*Institute for Comparative Human Development*

THE ROCKEFELLER UNIVERSITY

June 1977 Volume 1, Number 3

Plying Frames Can Be Dangerous:  
Some Reflections on Methodology  
in Cognitive Anthropology<sup>1</sup>

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What is written here is not so much a "paper" addressed to a reader as it is a representation of a "talk" presented to an audience. Some of the discussion refers to the audience's shared experience as listeners to the talk. It would be helpful if the reader could imagine that he or she were a member of that audience. Accordingly, he or she will henceforth be addressed in the second person. Remarks addressed to you as a "reader" rather than as an "audience member" will be set off by dots and placed in parentheses.

Cognitive anthropologists, sometimes known as "ethnoscience," are said to be people who listen to what the natives have to say. I do not claim to be one of the experts among cognitive anthropologists, but I do claim to be a native. This occasion, then, can be taken as an opportunity to assess the interest of listening to a native's point of view about the activities of his own group.

The research tradition that has come to be known as "cognitive anthropology," like most labeled schools of thought, includes such a diversity of approaches and perspectives that it is difficult to find much, apart from the label itself (and, maybe, the attacks of Marvin Harris), that holds the tradition together. Probably the most apparent common theme has been a concern for methodology, inspired by an admiration for the supposedly greater rigor of sister sciences, either psychology or linguistics. The value of the various methods proposed may be debatable, but the effort of pursuing them has succeeded in giving the field of cognitive anthropology a bag-of-tricks image among both adherents and critics. Some adherents seem to feel that the problems of the field, problems of lack of coherent theory or of substantial descriptive accounts, can be solved by evermore diligent pursuit of new methods. Somehow, with tighter frames, more dimensional scales, and more flawless flow charts, the cognitive maps of our informants will be brought into focus. Critics, on the other hand,

have pointed to the danger of this kind of excessive methodological tinkering. The scope of the data narrows to accommodate the methods. A pursuit of methods that work for something—anything—replaces the search for a theory that explicates what we want to know.

I am not about to suggest that cognitive anthropology abandon its methodological concerns. I do not recommend following the route of some linguists, freeing oneself for theoretical flights by cutting off ties with empirically grounded data. Nor do I advocate joining the hermeneutic circle, burying data under repetitive interpretations of "what it means (to me)," a tactic that produces thick books, but does not necessarily deepen understanding.

Methodology, some theoretically motivated notions of *what to do* when faced with the real world, is as necessary in science as it is in everyday life. Methods link data—what we construe to be observations of some particular reality—with theory, our proposals for understanding reality in general. When methods fail, the answer may be not only to tinker some more with the methods, but also to rethink the theory. My purpose here is to reflect upon some of the methodological successes and failures of cognitive anthropology in terms of their implications for general conceptions of the relations among behavior, verbal descriptions of behavior, cognition, and culture.

I will focus on what is certainly one of the best-known items in the cognitive anthropologist's bag of tricks: the frame. This methodological device was lifted out of the distributional model of structural linguistics, and shares kinship with similar notions of the same ancestry: paradigmatic/syntagmatic, slot-filler, contrast/contiguity, alternation/co-occurrence. Some element, A, is specified by its contextual constraints, X—Y, and by its relation to other elements, B, that can occur in the same context,  $\begin{matrix} X & A & Y \\ \times & \times & \times \end{matrix}$ .

The unique, and still poorly appreciated, contribution that the cognitive anthropologist made to this contextual model was that the context was not limited to portions of single, isolated sentences. A frame was construed as an inquiry matched with a set of responses. The unit of analysis was a question-answer sequence, a conversational exchange.

This extension of the range of linguistic context beyond single speakers uttering isolated sentences was made in an effort to find a context that would frame semantic, rather than grammatical, relations (the latter being the sole concern of both structural and transformational linguists at that time). Inquiries specify informational contexts, constraining the semantic domain of the response. Speakers of language were seen as question askers and answerers, not simply as sentence producers. This pursuit of meaning by relating sentences produced by different speakers together as part of a discourse was an advance over the sentence-bound semantics practiced until recently by linguists. (Compare the analysis of "bachelor" in Katz and Fodor's much-heralded 1963 paper on linguistic semantics with what was being done at that time in ethnographic semantics. Katz and Fodor's analysis would have allowed a married man to answer a woman's question "Are you a bachelor?" by "Yes," on the grounds that he was (1) a holder of a BA degree, or (2) a seal.)

The notion of frames proved to be a powerful and useful methodological tool. It provided ways of obtaining and organizing certain kinds of data so that they made certain kinds of sense in convincing ways. But, as critics have been quick to point out, not all kinds of data proved to be equally tractable and, more significantly, the results, while they may have made some kind of sense, often did not seem to answer very interesting or important questions. In contrast to the essays of symbolic anthropologists, not so hampered by methodological constraints, the output of cognitive anthropology often seemed compartmentalized and trivial. One might counter that one person's trivia is another's eureka, and, moreover, a secure little truth is as useful as a wobbly grand theory. Nevertheless, cognitive anthropology ought to aspire to bigger truths, to go beyond offering tiny fragments of cognitive maps from here and there, to offer an overall view of the landscape.

More upsetting to cognitive anthropologists than the triviality issue have been problems of inducing people to verbalize in consistent question-response fashion about many topics of interest to the investigator and of obvious relevance to the people being studied. Why is it easier to get a taxonomy of birds than of social roles?

The other problem that has arisen from applying frames and other, more experimental, methods in cognitive anthropology has been the high degree of informant variability that is so often manifest. This result, to my mind, reveals a strength, not a weakness, of the methods. It reflects the way the world is, a reality less methodologically oriented approaches in anthropology have obscured. The last thing we should do is to flee from this reality or to tinker with our methods to eliminate it. But there remains the

question of how to account for variability. The traditional use of the frame as a question-response device leads to what I think is the wrong answer: that we each go around with unique cognitive ideolects in our heads, each of which must be separately described and somehow summed up to equal culture.

These methodological difficulties have arisen from a failure to exploit fully the interactive aspects of the frame model, to widen the frame so as to capture a context that more fully specifies how human behavior comes to have meaning. Instead, attention was focused on questions and responses as chunks of verbiage isolated from their settings and their speakers. The specter of the stimulus-response model of behavior hung over many early programmatic statements. Some investigators emphasized that the idea was to *discover* the questions (stimuli) that evoked the answers (responses) we were trying to describe. But this notion that the answers are there, that the job is to find the questions, while often cited, did not seem really to take hold. Frames began to be called *eliciting* frames, to be thought of not as contexts for behavior but as prods to behavior. The ethnographer, rather than the informant, thus becomes the questioner.

Of course, one tries to elicit the questions from the informant, but this process can amount to little more than finding out how to translate into the informant's language the questions the ethnographer wants to ask. Both the prevalence and the hopelessness of this procedure has become apparent to me in classroom informant-eliciting exercises, both those I have staged and those I have witnessed. In cases where I had some knowledge of the language and culture (and I quickly restricted this game to such cases), it was clear that the only way to discover useful questions was to specify inquiry contexts within which such questions could be asked. Doing this in English without a knowledge of the culture is nearly impossible for most domains. But it does show, more clearly than actual ethnography, where the context is more likely to be taken for granted because it is there; that questions have to be related to larger contexts.

Apart from the distortion of frames into probes, there are certain technical difficulties with the notion of a "question." An inquiry for information (a query) is a kind of speech act that must be distinguished from a question, a grammatical interrogative. An interrogative can, and perhaps most often does, represent such speech acts as summons and greetings, which are not queries. The ethnographer of American disease who goes around our society asking "How are you?" is not likely to elicit a very large inventory of disease terms. In Yakan (a Philippine language), a frequent question is "Who is your companion there?," an ideal question, one might think,

for eliciting terms of social identity. Yet, the question is most appropriately posed to someone who is alone. The only appropriate answer is "nobody." This question is, in most contexts, a greeting. The Yakan question "What are you carrying there?" is typically a greeting if what is being carried is easily visible, a query if it is not (Frake, 1975). The status of a question as a query is dependent on the context in which it was uttered. Formal eliciting—so-called white-room ethnography—is an attempt to circumvent this problem by removing all previously relevant context, training the informant to see the white room as an interrogation chamber. This is an excellent methodological strategy, if what we want to know is how people behave in white rooms.

Even when we have a context in which we know that a given question is a query, we still can't be certain what query the question represents. A single question (a given surface-structure form) can represent a variety of queries for different kinds of information. The form of the question constrains the grammatical form of the response, but it does not, in itself, necessarily constrain the semantic domain of information. In Yakan, the common question "X is Y's what?" represents any query, the answer to which can be given in the genitive (surface) case: X is Y's grandfather; X is Y's rice field; X is Y's roof; X is Y's fate; etc. The answer to this kind of problem is not to search for more specific and necessarily highly artificial questions that, it is hoped, will sort out these different semantic relations, but to attend to the wider contexts of questioning that accomplish this sorting for the Yakan.

If one takes seriously the admonition to go out into the real world and look for queries, to seek "query-rich settings," as I once put it, one finds that people talk all the time and ask each other a lot of questions, but disappointingly few of the questions represent queries about the overt topics of the questions. Even children, the champion questioners, use this grammatical form in subtle ways. The child's stock question, "Mommy, you know what?" is not a request for information, but a clever use of sociolinguistic rules to acquire speaking rights (Sacks, 1972).

Perhaps instead of trying to devise provocative questions and other instruments to persuade people to talk about things they do not ordinarily talk about in that way, we should take as a serious topic of investigation what people in fact talk about, or, better, what they are in fact doing when they talk. When we look at talk, we find that people do not so much ask and answer inquiries; they propose, defend, and negotiate interpretations of what is happening. Because what is happening is what we are interested in explicating, these interpretations provide the key to understanding. Viewing informants not just as ques-

tion-answerers, but also as interpreters of their lives, provides not only a sounder perspective for handling problems of informant variability and reticence, but also a more realistic notion of the relation of cognitive systems to behavior.

It is not so much that some things are hard to talk about. People can and do talk about anything. But some questions, if taken seriously as inquiries, are hard to answer. What kinds of sounds are there in your language? This is obviously a ridiculous question to pose to an informant if what you want to know are his phonological concepts; yet it can be answered, not by asking it, but by attending to interpretations of sounds made by speakers of the language. The problem with verbalized interpretations is not a difficulty in eliciting them but in locating what cues are being responded to in formulating a particular interpretation. Cues of sound, appearance, expression, body stance, and movement often cannot easily be explicitly identified by those who use them. Careful observation of the behavior, object, or event being interpreted is required. Simply recording what people say about things is no more adequate than simply recording what one sees. The informant's interpretations must be linked with the investigator's observations.

Attending to interpretations will not eliminate variation, but it will help to explicate it. Of course, people vary in behavior because they have different life experiences, different childhood traumas, different mental capacities, different hormonal balances, and so on. But this is only part of the story. Informants vary in what they say and do because interpretation itself is problematic. (It can be especially problematic when an informant is confronted with an ethnographer across a tape recorder.). An interpretation is not an answer to a question automatically produced in the mind by a cultural computer program as a result of proper input. It is a proposal, a theory to be tested, tested not only against the reality it covers, but (like scientific theory) also against its reception by one's fellows (or by the ethnographer).

Construing talk about things (including responses to the investigator's queries and tests) as proposals for interpreting not only what is being talked about, but also what is going on *now*, makes variability in verbal responses much more understandable. Where we must seek underlying cultural constants is not in the content of the talk, but in the principles for formulating interpretations, for making sense of life. It is when things do not make sense that you know you have wandered off the edge of your cognitive map.

My arguments thus far all point to the necessity of expanding our frames to encompass the wider social context that makes interpretation possible. Calls for considering wider context, for defining behavior in

terms of the situations in which it occurs, are certainly not new. Malinowski made them, ethnoscientists and ethnomethodologists have made them, sociolinguists have made them. Even straight linguists have begun to make some moves in this direction. Appeals are made, but it is rarely very clear how one specifies and delimits relevant context. All that is clear is that specification of relevant context is problematic, not only for investigators, but also for natives. It is itself, as ethnomethodologists are fond of telling us, a matter of interpretation. Context is not there to be seen. Its specification is a social accomplishment.

One way to begin a search for the units by which the specification of context is accomplished is to track a bit of meaningful behavior through a variety of native interpretations. Take one of the more secure findings of cognitive anthropology: that the English word for *mother* is "mother" (sometimes pronounced "muh-thuh"). Cognitive anthropologists have learned this by asking a query equivalent to "How are you related to so-and-so?," where the so-and-so's are named individuals in the informant's social world. This frame serves to sort out a domain of "relatives" ("We're not related; we're just friends"), and also to distinguish real "mothers" from such metaphorical mothers as mother superiors, mother tongues, and mother nature—the kinds of mothers who attract symbolic anthropologists.

Componential analysis permits us to define real kinship mothers as female, first ascending generation, lineal ( $\varphi G^{+1}L$ ). Whenever you encounter a  $\varphi G^{+1}L$ , you have found yourself a real mother. But even real  $\varphi G^{+1}L$  kinship mothers can suffer a variety of interpretive fates. Here are some recorded comments on mothers, made by a native—an American-English speaking informant of Irish-Catholic background. The informant begins by describing his home community.

(In the oral presentation of this talk, I play here a few excerpts from a phonograph record by the comedian George Carlin [1973]. He does a routine on his neighborhood, "White Harlem" [Morningside Heights], then one on verbal dueling, "slip-fights," in the parlance of his group. He notes that some groups have a rule for slip-fights: "No mothers, man; no mothers." His group didn't have that rule. They started right in with mothers: "Hey, where'd yuh go last night?"

"I was out with yuh muh-thuh, man."

He then notes that it is a cause of some embarrassment if the mother of the addressee turns out to be dead:

"I forgot, man."

Carlin goes on to acknowledge the origin of slip-fights in such Black street games as "the dozens." He recites an example of the dozens, in which the reference to what the speaker does with the addressee's mother is rather more graphic than in the white, Irish-Catholic slip-fight example.

The recording includes the laughter of Carlin's audience.)

The slip-fight exchange is ostensibly rather innocent. It is not difficult to imagine contexts in which it could so be taken: if the respondent, for example, were the questioner's father. There could also easily be contexts in which this exchange could be a grievous insult: if, for example, there were good reason to believe that the respondent really had been out with the questioner's mother. But what we have is not a real insult, but a ritual insult as part of a game of verbal dueling. But if the target's mother happens to be dead, then the insult can no longer be taken ritually. An apology is called for. So, even though participants clearly take the depicted event as mythical, it is a real kinship mother, not a mythical or metaphorical mother, who is being referred to.

But, of course, what *you* (the member of the audience) heard was not the mention of "mother" in a slip-fight. Nor did you really hear an informant describing a slip-fight for the enlightenment of an ethnographer. I am sure you quickly saw through that fabrication and realized you were hearing a performance intended to entertain an audience. So you knew you were not being insulted, not even ritually. But you were not being entertained, either. For you are not an audience of a "show" but of a "talk." And, although the audience of a talk is allowed to laugh, too, we would all agree that reference to mothers in this talk would be a failure if it could not somehow be interpreted as a reasonably apt illustration of some serious, scholarly point.

The point here is not so much to characterize varieties of such speech acts as insults and invitations. This is an active enterprise now in several scholarly fields. Nor is the real point even that of Goffmanesque frames (Goffman, 1974): how to distinguish real insults from ritual ones in slip-fights; real slip-fight ritual insults from performances of them in a show; and real show performances of ritual insults from illustrative use of them in a talk. These matters are very relevant. But my focus here is not on the kinds of acts that can be discerned within contextual frames, nor on the human capacity to reframe reality repeatedly, but rather on the shape of the contextual frames themselves, frames within which people organize their conceptions of what, basically, is happening at a given time.

It is easy to argue for the indeterminability of specifying what is really happening at any given here and now. What is the spatial extent of here? What is the time span of now? Which of the multitude of detectable motions and changes surrounding us and within us constitute what is really happening here and now? Are we scratching our heads? Feeling hungry? Worrying about taxes? Watching a fly on the wall? Breathing in our neighbor's cold germs?

the ordinary conception—namely, that a speaker constructs his turn, stops, and then the next speaker begins—while it may describe what the “text” looks like once it is produced, is a distorted picture of the process of the text’s production. Instead, each particular turn is constructed by the speaker so that a possible completion point can be “projected” by the listener. The listener, in taking up the option of starting a turn at a point of possible completion, participates in determining the size of the previous turn. That speaker and listener simultaneously appreciate a turn’s ongoing projectability is an important aspect of the recipient design of turn-taking in conversation.

Sacks et al. observe that conversationalists commonly accomplish conversation with no gap and no overlap between turns. Even when overlaps occur, they rarely represent listener interruptions. For example, a speaker may come to a point of possible turn-completion and go on to add a tag question; the listener recognizes the possible completion point and starts to talk simultaneously with the tag. The facility with which parties exchange turns is the evidence that parties know and share turn-taking procedures which operate only at possible turn-transition points and allocate only the next turns (Sacks et al., 1974). Such a “local management system,” based on the recipient design of the turn-being-constructed, puts considerable demand on the listener’s attention. Their formalization of this system shows how the attention of both speaker and listener to each other’s concerns can succeed in accomplishing each particular transition smoothly.

Very generally, the model is composed of two components and a set of rules. The components account for turn-construction and turn-allocation, and the rules specify the ordering of options for speaker selection at potential turn-transition points. For example, the rules specify that for each possible transition point, if the speaker has not selected the next speaker (e.g., with an addressed question) then another party may self-select, and the first starter acquires rights to the turn. Turns are made up of units from the turn-constructional component, and the speaker is entitled initially to only one. Sacks et al. make what amounts to a psycholinguistic claim that turn-constructional units are syntactically defined—words, phrases, clauses, sentences. During the course of the speaker’s turn-construction, listeners know, by some sort of syntactic analysis of the ongoing turn, which utterance junctures permit turn-negotiations. Although Sacks et al. do not provide psycholinguistic documentation for this aspect of recipient design, the important claim is that turns are internally projectable from their onset.

The recipient design of adult conversation can be contrasted with Piaget’s (1955) early descriptions of

young children’s speech as “egocentric.” Piaget claimed that not only is children’s speech typically not addressed or otherwise adapted to the needs of a listener, but listeners feel no obligation to attend critically or to respond to the speaker. But recent studies indicate that children’s conversations are quite like adult conversations in describable ways, suggesting that children do design some aspects of their talk for their recipients. Experimental investigations have shown that children can adjust the complexity of their speech to the needs of the listener—for example, if the listener is a toddler (Shatz and Gelman, 1973). Observational studies of peer interactions, for which video-tape was used, have shown that nursery-school-age children attend to one another and produce dialogues which, although constructed differently from adult dialogues, are descriptably coherent (Garvey, 1974; Keenan, 1974). Recent research leads us to the proposal that there is a wide variety of separable social skills, understandings, and information-processing strategies that characterize participants’ knowledge about social interaction, and that their careful differentiation provides bases for adult-child comparisons (Gearhart, 1976). The turn-taking model of Sacks et al. provides a framework for re-examining some recent findings concerned with children’s verbal interactions, and suggests some further questions for developmental research.

Keenan’s (1974) descriptions of the early-morning conversations of her twin boys (at about age three) show that they can achieve a coherent dialogue and one in which the speaker expects the listener to give a relevant response. Relevance is displayed in these dialogues primarily by repeating the previous turn or some constituent of it with some inventive variation or elaboration. Whereas Piaget had considered repetition essentially egocentric, Keenan has demonstrated its social function. Others have extended her finding. McTear (1976), for example, describes speech-act functions of repetition in adult-child interaction. Newman (1977) demonstrates that the repetitive dialogue between two children can display for outsiders not only that the two are engaged together, but that certain ownership rights are in effect. It has become clear that children are displaying their mutual engagement and attentiveness through repetition. Although their procedures are different from those of adults, their displays are, in essence, public and thus visible to the investigator.

Consider now the implications for children’s talk of a particular aspect of recipient design mentioned earlier, namely, the ongoing projectability of the current turn. Sacks et al. suggest that this is central to adult conversation, in that it provides for the interactive determination of the turn as a unit. It can be asked how young children determine that it is time

for the next turn at talk. There are reasons to suggest that children use procedures for turn-construction that are not like those which Sacks et al. describe for adults. Garvey (1974) describes the rhythmic quality of some repetitive exchanges between children in play. In such a sequence, child A could determine the end of child B's turn, not from the internal structure of B's turn, but from its relationship to A's own previous turn. Here, precise timing of turn-exchange would depend on recognition of repetitiveness, a turn-taking procedure of limited applicability. Our own informal observations of the materials with which we have been working (videotapes of seven three-year-olds and their nursery-school teacher) give us the impressions that overlaps of turns indicate competitive refusals to attend, rather than cooperative turn-construction, and that gaps between turns during attentive dialogue seem to be considerably larger than for adults. It is possible, then, that (in the absence of rhythmic, repetitive exchanges) turn-construction and exchange work as follows: child A constructs a turn at talk, stops, then B responds. This, of course, is just the paradigm of turn-construction that Sacks et al. are arguing against for adults. It appears that a "conversational turn" for a nursery-school child is not the same kind of social unit that Sacks et al. describe for adults.

To determine how children acquire the procedures which adults display, more observational research will be necessary. Of particular importance will be the examination of naturally occurring occasions of three (or more) party exchanges and adult-child exchanges. For three (or more) participants, simply stopping a turn and expecting a listener to respond is not adequate for allocating the next turn, because multiple listeners must have some additional means for negotiating who talks next. A listener who is able to project the turn's completion will have the advantage for invoking the "first-starter rule" and claiming next turn. How might the child learn turn-projectability? In adult-child exchanges, the child will find his own turns being projected and terminated by his listener. Thus, the child may learn, for turn-projectability, the consequences of the listener's intentional activities, in much the way in which Ryan (1974) suggests that parents' expansions of infants' utterances display to infants how utterances are interpreted in the culture. The child's growing abilities to project turn-completion should be reflected in the acquired usage of "pre-starters" (e.g., "well," "but," "look") and "post-completers" (e.g., tag questions), devices which, as Sacks et al. show, comprise the overwhelming majority of adult overlaps and which accomplish turn-claiming and turn-allocation without jeopardizing perception and comprehension of the turn's central contents.

The complexity of the acquisition of the turn-taking system may be reflected in research like that reported by Selman and Byrne (Byrne, 1973; Selman and Byrne, 1974) from children's analyses of socio-moral dilemma stories. Subjects younger than 12 were unable to understand "that both self and other can consider each party's point of view simultaneously and mutually." Although story comprehension and conversations are obviously very different tasks, we have seen that cooperative production of conversational turns makes a similar requirement—that is, the simultaneous appreciation of a turn's possible completion point by both speaker and listener. The acquisition of fully adultlike procedures for turn-taking might be a fairly late phenomenon.

#### REFERENCES

- BYRNE, D. F. 1973. The development of role-taking in adolescence. Unpublished doctoral dissertation, Harvard University.
- GARFINKEL, H., and SACKS, H. 1970. On Formal Structures of Practical Actions. In: J. S. McKinney and E. A. Tiryakian (Eds.), *Theoretical Sociology*. New York: Appleton Century-Crofts, pp. 337-366.
- GARVEY, C. 1974. Some properties of social play. *Merrill-Palmer Quarterly*, 20: 163-180.
- GEARHART, M. 1976. Social interaction in a nursery-school classroom: How three-year-olds accomplish beginnings. Unpublished.
- KEENAN, E. O. 1974. Conversational competence in children. *Journal of Child Language*, 1: 163-183.
- MCTEAR, M. F. 1976. Repetition in Child Language: Imitation or Creation? Paper presented at Psychology of Language Conference, Stirling University, Stirling, Scotland, June 1976.
- NEWMAN, D. 1977. Ownership and permission among nursery-school children. In: J. Glick and A. Clarke-Stewart (Eds.), *Studies in Social and Cognitive Development*. New York: Gardner, in press.
- PIAGET, J. 1955. *Language and Thought of the Child*. New York: World.
- RYAN, J. 1974. Early Language Development: Towards a Communicational Analysis. In: M. P. M. Richards (Ed.), *The Integration of a Child into a Social World*. Cambridge: Cambridge University Press, pp. 185-213.
- SACKS, H., SCHEGLOFF, E. A., and JEFFERSON, G. 1974. A simplest systematics for the organization of turn-taking in conversation. *Language*, 50: 696-735.
- SELMAN, R. L., and BYRNE, D. F. 1974. A structural-developmental analysis of levels of role-taking in middle childhood. *Child Development*, 45: 803-806.
- SHATZ, M., and GELMAN, R. 1973. The Development of Communication Skills: Modifications in the Speech of Young Children as a Function of the Listener. *Monographs of the Society for Research in Child Development*, 38 (5, Serial No. 152): 1-38.

# The Social Organization of a Successful and an Unsuccessful School Performance

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An inventive member of the administrative staff in a Harlem school recently created a "debating club" for about one-dozen fifth graders who have posed such severe academic and/or behavior problems that their teacher can no longer work with them in the classroom. The moderator brings them together to discuss issues that face the children in both the school and the community. The club meets twice a week after school and is successful in the sense that the children look forward to the discussion, seldom demonstrate the misbehavior problems that mark their lives in the classroom, and articulate well-organized positions on different issues.

This brief account of how the children and the staff member achieve their success suggests that it is difficult to understand a child's performance without looking carefully at the social-organizational work that directs the attention of the children and the adults to certain problems and their solutions. In the same vein, we will contrast this successful discussion with an unsuccessful attempt by a different staff member in the same setting with the same children. The description should raise doubts about any attempts to characterize the ways in which (or the efficiency with which) children perform without paying careful attention to the social contexts in which that performance takes place. This is an important point, in that most evaluations of children, be they by tests, behavioral rating scales, or the adult native eye, proceed without an analysis of the children's social contexts.

There are two components to the group moderator's efforts to create an environment in which the children can achieve social cohesion and intellectual clarity in their discussion.

First, by constantly insisting on a precise formulation of the topic, the moderator helps to give the children something to listen to, something in terms of which they can achieve a clarity of focus. For example, she can often be heard to say, "He is saying that the . . ., isn't that right?" And children are encouraged to respond with either affirmatives or reformulations. Occasionally, a child will abandon a topic and begin to tell an unrelated story. Immediately, the moderator stops the child and asks for a reformulation. For example, she says:

T: What are we talking about? What was the question I asked you?

C1: "What are you doing?"

T: No. What was the original question I asked?

C2: About the tests.

T: What about the tests?

C2: "Are you getting ready for the tests?"

T: All right. Now we are still on that subject. All right, Gregory, can you talk to that one thing? What are you doing for your tests? How do you feel about them?

In this way, everyone in the group is given a framework for listening to the others, and most of the children apparently are able to follow the arguments most of the time.

Although the moderator works to keep a topic clearly well formulated, she is careful not to force a topic on the children. If a subject is not picked up and elaborated, the topic is changed. Thus, the formulations of topics are interactionally negotiated by the children and the moderator together. At one point, she might ask the children if they have anything to say about classroom events during the day; if they respond in unison "no," a new topic is suggested.

Second, the moderator is sensitive to the interactional structure of the group in order to insure that any speaker has a complete audience. Often she can be heard insisting "talk to your group, not just me." But more than that, the moderator is sensitive to how the children position themselves vis-a-vis each other. She seems to read these positionings as barometers of how the listeners are performing as debating society members. When a topic begins to disintegrate, and before switching to a new topic, she instructs the group to "sit back so we can all see each other." By repositioning the group in this way, a group tonus is established for the negotiation and discussion of a new topic. (On the notion of people positioning each other in order to achieve a working consensus on what they are doing with each other, see Goffman, 1974, and Scheflen, 1973.)

So far, the analysis of the group's behavior has centered on the moderator, but the children's competence to participate in the discussion is no less impressive. The moderator does not have to force her organization on the children. Most often, they turn to a new topic at the same time the moderator does, and eagerly achieve new positionings in the interactional arena. The children generally speak on topic, incorporate information from other speakers, and import information that is relevant and interesting. We note that they generally "follow the rules" and that they do so with each other's help. Interactionally, they keep each other informed of what they should be doing and, with the moderator's help, they usually manage to organize in ways conducive to a successful debating club.

This intellectual and interactional achievement is perhaps most stunning in contrast with the rest of

the children's experiences in school. In lieu of an account of their classroom life, we can offer an account of how the same children in the same setting with a different moderator failed to achieve equivalent success. The situation was quite different with the substitute moderator at the reins, although the children know her well and are subject to her disciplinary authority during the school day.

This session starts off the same. The children act appropriately as they sit down around the table and the substitute moderator begins with the same question with which the regular moderator successfully started another session, namely, "Why do children fight?" The first child addresses the issue with a story, the details of which appear rather uninteresting to both the substitute and the other children, who begin to squirm. The substitute exercises none of the formulating activities of the moderator. A second child is called on, and he takes a similar direction with a narrative which addresses the issue at hand only slightly. The listeners' attention wanders and some of them begin to talk with each other as the substitute looks on with growing concern. The third speaker takes up the conversation and also speaks off the topic. Now more than half the children are talking with each other. Finally, the substitute invokes the rules by scolding one of the children for not paying attention; the boy complains that everyone else is talking. However, the group is still able to organize well enough to allow for the beginning of the next turn. Again, the substitute exercises no frame control of the speaker's topic or the listeners' attention. Again, the children stop listening, and the substitute scolds one of the talkers. Again the accused protests.

At about this time, the situation seems to go through a marked transition. The members shift around in their seats and never after this point is the group's attention focused on one person. Rather, the group appears to split up into many coalitions, which take turns breaking the rules of the debating society. The new situation is apparently something like "Let's not let anyone else talk the way they would talk in debating club." Occasionally, a child will attempt to present an argument concerning a particular issue, and the rest of the children then take turns trying to keep that person from developing the argument as it might have been developed if the regular moderator had been running the session. To the casual observer, the intellectual and interactional work of the children runs amuck, for the children's behavior is regressive in terms of both the debating society and their relations with the substitute and the school's disciplinary code that she represents.

In a case such as this, when the framework for producing a successful school performance breaks down, the intellectual work accomplished shifts markedly. We do not mean here to demean the intel-

lectual quality of the talk of the children while they are trying their best to destroy the possibility of a person making an argument. There is order in their behavior. The situation is in no way chaotic in the sense of being without behavioral regularities. After the debating breaks down and the children have redefined the situation, their behavior can be understood as orderly in terms of the new definition of the situation. Their jokes are clever. Their well-timed barbs and funny gestures display a knowledge of their social order. But in order to understand their performance, we must locate their definition of the situation; we must locate their sense of the intellectual and interactional problems they are trying to solve.

We have presented this information on how people achieve success or failure in the debating club in order to make some methodological points: 1) a person's or group's performance of any task must be understood in terms of how they define the situation at hand; and 2) the situation is defined in terms of people's interactions with each other and their identification for each other of the tasks to be worked on. Not only is it difficult to assume that every person has some general capacities which can be measured by certain tasks on different evaluation scales; it is often difficult to discover what the person is trying to do without a detailed analysis of that person's social circumstances.

#### REFERENCES

- GOFFMAN, E. 1974. *Frame Analysis*. New York: Harper and Row.  
SCHEFLEN, A. 1973. *Communicational Structure*. Bloomington: Indiana University Press.

## Strategies for Investigating Intelligence in its Cultural Context

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In recent years, a number of researchers have questioned the applicability of basic constructs in Western psychology as a means of explaining behavior in non-Western cultural contexts. The rationale for this metatheoretical question has been illustrated by analogy with the linguistic distinction between phonetics and phonemics (Pike, 1967). Are concepts such as schizophrenia, need for achievement, field-dependence, or intelligence part of an "etic" system of categories, which impartially describes phenomena universal to all cultures from an outsider's point of view, just as the system of phonetics claims to provide a code for the sounds of all languages? Or are



they "emic" categories, which, like the linguist's phonemes, specify just those salient distinctions that carry meaning relative to a given culture from an insider's point of view (Serpell, 1976)? In this essay, I shall compare a number of different strategies for investigating the issue in relation to the concept of intelligence.

### TALK ABOUT WORDS

Wober (1974) set out from an explicitly relativistic perspective to investigate the nature of "the Kiganda concept of intelligence." His data were obtained by asking five groups of adults in Uganda to locate either the English word "intelligence" or the Lu-Ganda word *obugezi* on semantic differential scales, extending from the values 1 through 9, between 22 pairs of opposing words, like modern-traditional, rare-common, private-public, yielding-obdurate, cold-hot, hurried-delayed, careful-hasty. Three groups did the test with Lu-Ganda words: rural villagers, rural schoolteachers, and educated urban élites. Another group of villagers did the test in Lu-Toro, a different Ugandan language, and a group of medical students of miscellaneous ethnic origin did the test in English.

The tabulated results for the three groups tested in Lu-Ganda do not show any cases in which the two most contrasting groups (the urban élites and the villagers) locate the word *obugezi* on opposite sides of the mid-point of the scale. There are, however, significant differences among the groups, from which Wober infers that

Teachers no longer think of intelligence as so slow, or stable, healthy, careful, active, or hot, as do the villagers; nor do teachers so strongly associate intelligence with the idea "friendly." (p. 270)

The problem with this inference is that the data do not provide evidence of what the groups think about intelligence, but only of what they think about *obugezi*. The distinction is far from trivial, because clearly it is inappropriate to place the results from the medical students, who responded to English words, on the same continuum. Yet this is just what Wober does: "The results are yet more marked when comparing the villagers' ideas with those of the medical students" (p. 270). The ambiguous status of this comparison is highlighted by the observation that it was clear even from the dictionary, before conducting the experiment, that "the word *obugezi* has a meaning that includes the English referents of wisdom, as well as of intelligence" (p. 277). The problem of translation equivalence is insuperable in comparative research across different groups if the data are restricted to verbal responses to words.

If we confine our attention to the differences across Wober's groups tested in the same language, a fur-

ther limitation of this research strategy emerges. The differences in response between the villagers and the rural teachers are, for the most part, further accentuated in the contrast between villagers and urban élites. Therefore, it is tempting to conclude that the respondents' conception of *obugezi*, reflected in their constellation of associated Lu-Ganda words, is subject to an acculturative influence that increases as a function of exposure to Western values. But what if the subjects' values are not at issue in the task? It seems quite possible that respondents interpret this task as requiring them to explain the meaning of a word to the experimenter, rather than to express their personal values. If this is so, there is a strong possibility that the bilingual teachers and urban élites tend to confuse the connotations of the English word intelligence with those of the Lu-Ganda word *obugezi*. This kind of cross-language semantic interference is an interesting phenomenon in its own right, but it does not necessarily reflect an underlying change of values.

Talk about words in a culture's indigenous language illuminates the culture's emic network of concepts, as the well-known discussions by Miles and Ryle on the English word intelligence attest. If we juxtapose these networks for different languages, a provocative contrastive analysis of the type undertaken by B. L. Whorf may be attempted. But the major significance for psychology of any cross-cultural differences in the definition of intelligence would be not in the uses of words, but in the criteria by which people evaluate the actual behavior of other people. In the bicultural communities of the Third World, discrepancies between the modes of evaluating children's behavior by home- and school-based socializing agents may have far-reaching social consequences (Serpell, 1972), as well as important theoretical implications (Scribner and Cole, 1973). If we wish to draw inferences about how different cultures evaluate intelligence, we need a research paradigm that examines the relation between terminology (one of the culture's institutions) and observed behavior on the one hand, and between terminology and evaluation on the other.

### INDIGENOUS ASSESSMENT OF BEHAVIOR BY A TRANSLATED CRITERION

One way of examining the relation between terminology and behavior is to pick a given word and ask indigenous members of the culture to rate children's behavior by that criterion. Klein, Freeman, and Millett (1973) formed the impression from ethnographic interviews with adult members of an isolated, rural, Guatemalan village that, within their culture, "the most descriptive indigenous term for intelligence is *listura*" (p. 220). They then proceeded

to ask a number of villagers to rank seven-year-old boys with whom they were familiar in terms of *listura*. The average *listura* ranks were compared with scores on a number of mental tests adapted from batteries standardized on urban children in the United States. Correlations ranged from .11 to .75, the highest value being statistically reliable. The authors interpret their results as preliminary evidence of "uniform characteristics of cognitive competence across cultures" (p. 222).

A double-bind arises in interpreting these types of data. On the one hand, it is easy to dismiss high correlations as arising from a bias in the procedure for deciding on the indigenous criterion of intelligence. The greater the ethnographer's success in understanding the culture, the more precisely will he or she be able to identify that concept in the indigenous culture which corresponds most closely to the Western concept of intelligence for which the tests were developed. Indeed, part of what the investigator's bilingual informants were doing when they helped to choose the "best translation" of intelligence probably was to guess which word local people would use to describe success on Western tests! Equally troubling is the variety of options available for interpreting low correlations in this design. There is no reliable way to decide between attributing the "error" to (1) discrepancy between the meaning of the English and the indigenous word; (2) poor rating by the judges; and (3) discrepancy between the tests and indigenous conceptions of intelligence.

#### ETIC ANALYSIS OF BEHAVIOR

Given the difficulties I have described of getting an inside view of a foreign culture's interpretation of behavior, it is tempting for cross-cultural researchers to try to by-pass the problem altogether. If a precise enough description of behavioral events could be developed, it might be used across many cultures as an objective measuring device analogous to the International Phonetic Alphabet. Working in Guatemala with the same community as Klein et al. (1973), Nerlove et al. (1975) made 20 spot-check observations of boys and girls in their normal environment over a period of eight weeks. They classified the behavior observed on each occasion according to the presence or absence of two qualitative characteristics: "self-managed sequences" of behavior, and "voluntary social activities." Frequency of occurrence scores on each of these indices were then compared to scores on adapted Western tests. Self-managed sequence frequency was reliably correlated with scores on the visual tests of "analytic ability" and, in the case of girls, with tests of "language facility," whereas lower, but reliable, correlations obtained between voluntary social activity frequency and language facility scores.

These authors refer to the indices derived from their spot observations as "natural indicators of cognitive development," noting that although "the observers are part of the basic culture" of the children, "the coding of the activities rests on their structural features rather than on situationally specific cultural content" (p. 281). It is in this sense that I interpret their strategy as proposing an etic formulation to transcend the specifics of any cultural system. Indeed, they predict from their results that "further study of natural indicators should lead to the discovery of tests valid across many cultures" (p. 292). Yet the only criterion of this "validity" they present is correlation with adapted Western tests. Each of their measures of behavior is thus apparently underwriting the validity of the other, although neither appeals to any valuation internal to the culture itself. It would seem to be quite possible to develop, in this fashion, naturalistic observation codings and formal tests of ability which would correlate more and more closely with each other and yet would bear no meaningful relation to successful adaptation within the community.

Nerlove and her associates are aware of the possibility that "the older members of a community make effective use of the talent available to them" by guiding those talents selectively into different specialized occupations. And they suggest that, in nonliterate societies, "such a 'track' system of education . . . to some extent . . . must rest on the correct assessment of natural indicators" (p. 293). Here, as with the promise of cross-culturally valid tests, they seem to conceive of self-managed sequences and voluntary social activities as indices of cognitive competence. But there is an alternative meaning for these behavioral categories—that of causal influences on cognitive development. The authors imply at several points that the correlations between the "natural indicators" and the "test performance" are mediated by such a causal relationship, voluntary social activity promotes verbal facility, and self-managed sequences provide practice in analytic skills. In fact, frequency of occurrence seems more appropriate as a measure of opportunities for learning, whereas the assessment of cognitive competence calls for a measure of proficiency in each of these behavioral domains. Proficiency, however, can be assessed only by reference to standards of excellence. Thus, if we are to refine our observations of how adults evaluate children's cognitive development as it is displayed in a natural setting, we need to know not only which domains of behavior they observe, but also on what scales they assess what they see.

#### SEARCHING FOR RELEVANT DIMENSIONS WITHIN THE CULTURE

In our research in Zambia since 1972, we have

been attempting to address a number of the same issues as did the studies described above, but from a somewhat different angle. We started by assuming that the defining properties of intelligence within a culture are partially constrained by two factors accessible to external observation: (1) the range of occasions for adults to evaluate child behavior; and (2) the range of opportunities for children to acquire skills. It is evident that there is a substantial difference in both of these ranges between the Western cultures that have generated currently available tests of intelligence and the culture of a rural African community. One of our interests was in the criteria proposed by adult members of such a community for choosing among actual children in locally appropriate situations. Another was in the range of performance on tests designed to measure skills which rural Zambian children can acquire outside the context of school. We were also interested to discover whether there was any congruence between adult assessments by their own criteria and the relative scores of the children on our tests.

An essential principle in our method (Serpell, 1974) was to direct the attention of our adult informants away from the culture of the Western-type schools that prevail in Zambia, and onto the indigenous traditional culture. We asked the adults to select one among a group of about five village children of similar age for each of several specific hypothetical tasks, such as going to fetch help in an emergency or explaining news of an unusual local event. These operational definitions of the behavioral domain under discussion helped to secure our cross-cultural communication with elderly villagers who had had no experience of Western schools. Our finding that they cited qualities of cooperation and obedience nearly as often as mental abilities constitutes evidence, not of the meaning of a verbal concept, but of how these people evaluate a certain range of behavior.

Prior to the collection of this evidence, our test development was guided by a combination of naturalistic observation and intuition. The skills we sought to measure were carefully screened to include only cognitive operations we knew were fostered by indigenous preschool activities. And all of our test materials were designed to be familiar to this population. Scores improved on all the tests with chronological age and were unaffected (within the narrow range we sampled) by formal education. But the correlations were low between the test scores and the aggregate assessments by the informants who knew the children in their home environments.

Although a failure to find reliable correlation between two measures is always open to various explanations, note that in our research strategy neither variable is conceived as a criterion of validity for the

other. One possible interpretation of our results is that our tests did not accurately sample the children's skills, which the adult informants had assessed under a wider range of conditions. It is quite possible, however, that had we asked the villagers to predict the performances of different children on our tests, they would have done so reliably. Another interpretation could be that the adults did not address the appropriate task when they used criteria of social cooperation to rank the children. This, however, misses the point that we consulted these informants, not to assess their efficiency in applying a predetermined etic criterion of intelligence, but in order to learn from them about the emic system for evaluating child behavior within that particular culture.

My own preference is to treat our data as evidence of a divergence between the criteria favored by this traditional African community and those set up by most Western-trained psychologists for assessing intelligence. Adaptability in a small, closely-knit community, which has limited technology, may well be more dependent on social cooperation than does adaptability in an urban, industrial society, which places a high premium on independent initiative. The implications of such a divergence for future test development would take us beyond the scope of this paper. It may be that the larger village communities of Guatemala evaluate intelligence in a manner more consonant with the modern American ethos than do the village communities of Zambia. But the differences in methodology among the studies described above preclude any definite inference to that effect. My concern in this discussion has been to highlight some of the difficulties inherent in the project of eliciting from members of a foreign community a clear account of their own value system without inadvertently imposing criteria derived from the researcher's exogenous cultural intuitions.

#### ACKNOWLEDGMENT

This discussion was presented at a colloquium held in the Laboratory of Comparative Human Cognition at The Rockefeller University on November 19, 1976. The author wishes to acknowledge the support of The Rockefeller University during its preparation.

#### REFERENCES

- KLEIN, R. D., FREEMAN, H. E., and MILLETT, R. 1973. Psychological test performance and indigenous conceptions of intelligence. *Journal of Psychology*, 84: 219-222.
- NERLOVE, S. B., ROBERTS, J. M., KLEIN, R. E., YARBROUGH, C., and HABICHT, J. P. 1975. Natural indicators of cognitive development: An observational study of rural Guatemalan children. *Ethos*, 3: 265-295.
- PIKE, K. L. 1967. *Language in Relation to a Unified Theory*

- of the *Structure of Human Behavior*. The Hague: Mouton.
- SCRIBNER, S., and COLE, M. 1973. Cognitive consequences of formal and informal education. *Science*, 182: 553-559.
- SERPELL, R. 1972. Intelligence tests in the Third World. *Race Today*, 4: 46-49.
- SERPELL, R. 1974. Estimates of intelligence in a rural community of Eastern Zambia. *Human Development Research Unit Reports*, Vol. 25. Lusaka: University of Zambia (mimeographed).
- SERPELL, R. 1976. *Culture's Influence on Behavior*. London: Methuen.
- WOBER, M. 1974. Towards an understanding of the Kiganda concept of intelligence. In: J. W. Berry and P. R. Dasen (Eds.), *Culture and Cognition: Readings in Cross-cultural Psychology*. London: Methuen, pp. 261-280.

- GALLIMORE, RONALD, and HOWARD, ALAN (Eds.). 1968. *Studies in a Hawaiian Community: Na Makamaka o Nanakuli*. Pacific Anthropological Record Number 1, Honolulu: B. P. Bishop Museum.
- HOWARD, ALAN. 1974. *Ain't No Big Thing: Coping Strategies in a Hawaiian-American Community*. Honolulu: The University Press of Hawaii.
- GALLIMORE, RONALD, BOGGS, JOAN W., and JORDAN, CATHIE. 1974. *Culture, Behavior and Education: A Study of Hawaiian-Americans*. Volume 11. Sage Library of Social Research, Beverly Hills: Sage Publications.

In the mid-1960s, an interdisciplinary, five-year research project was undertaken to investigate the social and educational difficulties experienced by Hawaiian-Americans. Working in a community in rural Oahu, the team of anthropologists and psychologists employed a variety of research strategies, including participant-observation in the community and classroom, interviews with adolescents and adults, and social-psychological experimentation and psychological testing. Gallimore and Howard's is an ethnographic report, and presents the preliminary findings concerning Hawaiian-American behavior patterns in a variety of cultural domains, from diet and health to family and school. The processes by which Hawaiian-Americans learn these basic shared behavior patterns are described in Howard's *Ain't No Big Thing*. He discusses both the characteristics of Hawaiian-American culture and the factors that produce intra-community differences in adult strategies for risk-taking, confrontation avoidance, treatment of illness, and child rearing. The issue of education in the cultural context, a concern in all three books, is the central focus of *Culture, Behavior and Education*. Here Gallimore, Boggs, and Jordan agree that the educational difficulties that Hawaiian-American children experience are due in large part to culture conflict in the classroom. To clarify the processes by which problems in the classroom result from culture conflict, the researchers conducted experiments on help-seeking, responsiveness, and achievement-oriented behavior among children. This research project in the Hawaiian-American community provides a good model for the study of education in the cultural context because of its combination of extensive ethnographic description and controlled social-psychological experimentation.

PAULA F. LEVIN

- ZISTERER, SYLVIA. 1975. Probleme der phylogenetischen Sprachentstehung-Ansaetze zu einer Entwicklungsgeschichte menschlicher Sprache. (Issues in Language Origins—Approaches to a Developmental History of Human Language). In: A. Leist (Ed.), *Ansaetze zur materialistischen Sprachtheorie* Kronberg/Ts. (West Germany).

This paper presents a concise, integrative model of gestural language as the basis of linguistic development. Homination is depicted as a triadic process including language, labor, and social organization.

Communication in subhuman species (primates) relies on the visual modality and functions as a control device for interaction. Similarly, early stages of cooperation in

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## ANNOTATED BIBLIOGRAPHY

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- CICOUREL, AARON V. 1974. Interviewing and Memory. In: Colin Cherry (Ed.), *Pragmatic Aspects of Human Communication*. Dordrecht: D. Riedel.
- CICOUREL, AARON V. 1975. Discourse and Text. To appear in *Versus*.
- SHUY, ROGER W. 1972. Sociolinguistics and the Medical History. Paper presented to Third International Conference of Applied Linguistics.
- SHUY, ROGER W. 1976. The medical interview: Problems in communication. *Primary Care*, 3 (3): 365-386.

These authors find the transmittal of medical information between doctor and patient to be a problematic communication process, not a simple assembly of medical facts. Shuy suggests that medical communication breakdowns start from a mismatch between the language and culture of doctors and patients, are intensified by patients who come to doctors in highly emotional states, and are further vitiated by the doctor's use of highly specialized medical vocabularies. Cicourel reports communication problems which occur when doctors try to assess a patient's memory by techniques that run across the grain. He describes the information-gathering procedures when medical histories are taken, and when medical information is transformed from oral discourse to written text.

These studies are important because they examine language use and information processing in the medical institution. Because their data base is tapes and transcripts, their findings provide a basis of comparison with similar processes in educational and legal settings, thereby laying the groundwork for a comparative view of language and cognitive practices in institutional settings.

SUE FISHER

human groups may have been suitably controlled by gestural communication. Ostensive gestures organized participation in the cooperative process, dance rites represented complex situations (hunting). The simplicity of tools did not require verbal transmission of knowledge. However, major changes in the societal organization (status and role become independent from the situation at hand) made an integration of cognitive and communicational accomplishments necessary: symbols with stable meanings had to mediate between subject and environment, and represented a stable social structure. The development of grammar parallels the increasing complexity of the labor process.

JURGEN STREECK

HABERMAS, JURGEN. 1976. *Universalpragmatische Hinweise auf das System der Ich-Abgrenzungen*. In: Auwärter, M., Kirsch, E., and Schröter, M. (Eds.), *Seminar: Kommunikation, Interaktion, Identität*. Frankfurt/M. (West Germany).

In this paper, Habermas applies his theory of "communicative competence" (Universalpragmatik) to a conceptual model of the development of the self and shows how basic aspects of self-identity are reflected in and mediated by universal features of speech.

Standard speech acts make four validity claims: truth (objectivity of reported experiences); appropriateness (of the social rules the speaker invokes); sincerity (of the speaker's expression of his intentions); and intelligibility (of the semantic content of the utterance). Validity claims relate to four "regions" constantly addressed in speech: objective nature, society, inner nature, and language itself. Types of linguistic universals can be ordered according to this scheme: reference and classification devices index the objective world, the normative reality of society is reflected in systems of personal pronouns and speech acts, self-expression of subjectivity is made possible by intentional expressions; intelligibility is achieved by the meaning function of linguistic units which provides for the construction of an intersubjective universe of discourse.

Idealized developmental stages of language acquisition can be systematized in terms of this framework: at the first stage (symbolic interaction), the semantic content is inseparably embedded in action, propositional content and performance of the speech act are indistinguishable. They become separated at the second stage of "concretely linguistic communication"; here, cognition becomes independent from communication. At the final stage of "formal linguistic communication," speech potentially becomes an activity in its own right, all four aspects being translatable into topics. Hence, essence and appearance, "is" and "ought," sign and meaning can be distinguished, reality can be addressed in terms of modality, and the self can be fully separated from the world and other selves.

JURGEN STREECK

OEVERMANN, ULRICH, et al. 1976. *Beobachtungen zur Struktur der sozialisatorischen Interaktion* (Notes on the Structure of Socializing Interaction). In: Auwärter,

M., Kirsch, E., and Schröter, M. (Eds.), *Seminar: Kommunikation, Interaktion, Identität*. Frankfurt/M. (West Germany).

The central thesis of this outline of a sociological conception of the social constitution of ontogenetic developmental processes is that the structure of socializing interaction is constituted relatively independent of participants' intentions as an objective "latent meaning structure." This structure provides the child with "exceedingly" structured experiences transcending his interpretation capacity, which only at a later developmental stage can become interpreted consciously.

The authors discuss three transcripts of parent-child interaction revealing hidden (oedipal) meaning patterns. They present a method of "objective hermeneutics" aiming at an explication and reconstruction of objective meanings of documented talk. This method has to exhibit all possible readings of the text, independent of the participants' subjective interpretations. Moreover, discrepancies of objective meanings and participants' interpretations provide data for diagnostic case studies.

JURGEN STREECK

RICHMAN, CHARLES L., NIDA, STEVE, and PITTMAN, LESLIE. 1976. Effects of meaningfulness on child free-recall learning. *Developmental Psychology*, 12: 460-465.

At first blush, this short article appears to be just another study of children's memory in the verbal learning tradition of the Ebbinghaus Empire. The details of the study will be of particular interest only to psychologists engaged in conventional laboratory memory research. However, consideration of this experiment in a broader context shows its important relevance to the general study of comparative cognitive development. The study addressed the crucial question of the appropriateness of the task for the experimental groups tested. The authors prepared lists of words to be remembered which varied in "meaningfulness," as defined in terms of the average number of free associations to trigrams given by children of different ages. Examples were: COY (low meaningfulness), and CAR (high meaningfulness). When a single list of words was given to children in grades K, 2, and 6 (thus increasing meaningfulness as age increased), the usual developmental result was found: memory performance increased as a function of age. However, when different lists which were equated for meaningfulness for children at each of the age levels were used, there was no developmental increase in memory performance. This article, thus, represents one of a growing number of studies in which conventional developmental effects are minimized or even eliminated. Such studies use manipulations that adjust some aspect of the experimental situation to fit certain cognitive or social characteristics of the subjects in the various groups compared. The particular manipulation in this experiment, that of equating the meaningfulness of the items to be remembered for subjects in different age groups, would seem to have potential for use in studies of memory and information processing which make ethnic group or cross-cultural comparisons.

PATRICIA WORDEN