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**EDITORS' NOTE:** During the next few months the future of this Newsletter will be decided. *The issue is money.* Until now the Newsletter has been almost entirely funded by outside agencies. This help is now drying up and we are going to have to become totally, or at least primarily, self-supporting. As a University publication we are unable to accept advertising. Our only source of support is our readership. We have taken the first steps toward self-support; we have streamlined our operation and raised our subscription rates. Now we need you to cooperate by sending in your renewal fees and by encouraging other people to subscribe. If we are not operating in the black by the end of 1980 we will have to discontinue the Newsletter.

## Rhetorical Devices in Black English: Some Psycholinguistic and Educational Observations

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Although the last few years have seen a surge of interest in the attention devoted to figurative, and especially metaphorical, uses of language from disciplines such as linguistics, philosophy, and psychology (see, for example, Ortony, 1979a), most of this interest has been focused on rather theoretical issues. Nevertheless, the topic has many practical facets. One practical issue concerns the relationship between the comprehension of nonliteral language and linguistic performance. Since nonliteralness is an integral part of our language system and since facility in language is a primary element of success in school, the relationship between the comprehension of nonliteral language and linguistic performance, both oral and written, is a potentially important one for educators. This relationship, especially as it applies to metaphorical uses of language, may hold special significance for those interested in the problems and prospects of lower-class, Black children.

Many reasons have been proposed to account for the low achievement of Black children in today's school system. Explanations have included the notions of low motivation (Zigler & Butterfield, 1968), low self-concept (Clark & Clark, 1939; Asher & Allen, 1969; McAdoo, 1977), inherent intelligence deficits (Jensen, 1969; Jones, 1973), negative teacher attitude (Rist, 1970; Covington, 1975), and differences in social acculturation (Silverstein & Krute, 1975). However, by far the greatest attention in recent years has been devoted to explanations which are rooted in language behavior, i.e., an impoverished language (Bereiter & Engemann, 1966; Kagan, 1968), and differences in communicative environment (Hall & Tirre, 1979). Even so, the role of metaphorical uses of language in day-to-day communication has been essentially ignored in these discussions, in spite of the fact that it seems to be the locus of interesting differences in the language of Black and White children.

The possibility that children are facile in producing and understanding metaphorical uses of language by the age of 8 or 9 is contrary to the findings of a number of developmental psychologists who claim that children cannot handle metaphorical uses of language until early adolescence. However, much of this research can be challenged on the ground that it confounds children's abilities to produce and understand metaphors with various other uncontrolled variables such as knowledge of the world and context dependence (see, Ortony, Reynolds, & Arter, 1978). Furthermore, Cazden (1972) cites evidence suggesting that lower-class children are more likely to use metaphorical descriptions of abstract figures than middle-class children. This would be an extraordinary finding if it were indeed true that the use and comprehension of metaphorical language is governed by late-to-develop cognitive capacities.

Research reported in Reynolds and Ortony (1979) suggests that an important reason for the difficulties that younger children appear to have is that they often do not know what they are supposed to do with something that is, at least on the surface, a non sequitur. This implies that with suitable exposure to specific kinds of usage children would be able to handle metaphorical uses that they previously appeared unable to deal with.

In this paper we shall concentrate on three forms of nonliteralness prevalent in, and principally found in,

Black language communities: "signifying," "marking," and "playing the dozens." Each of these communicative devices involves metaphor in one form or another and is employed by children, adolescents, and adults from both urban and rural environments. Most of our examples will be drawn from two papers in a collection by Kochman (1972b), one by Claudia Mitchell-Kernan and the other by William Labov.

### Signifying

In standard English the term "signify" can either refer to an explicitly stated relationship between a meaning and an act ("yes" can be signified by, for example, raising the hand), or to an implicit relationship which stems from conventional associations (tattered clothes can signify poverty). In Black communities signifying is more like the latter in that the relationship is often implicit although the association is seldom conventional. The term is used to refer either to a tactic employed in verbal dueling or to a way of encoding messages or meanings in natural conversations with elements of indirection (Mitchell-Kernan, 1972). Signifying is used to imply, goad, beg, or boast by indirect verbal or gestural means. It can be done to stir up trouble for the sake of amusement or for vindictive reasons, or it can be done to simply transmit a message (often negative) without being direct or blunt. Signifying is a way of encoding messages or meanings in natural conversation via indirection, innuendo, and insinuation. The listener cannot rely solely on the dictionary meaning of words to get the full import of the message. Interpretation is shaped and guided by the listener's background knowledge. The situational context and knowledge about the speaker's background, in addition to expectations brought into the interchange, all come into play. Of course, this is true of human communication generally, but Black speakers make special use of indirection and innuendo with this verbal manipulation. A double entendre is often implied or hinted at but the responsibility for message interpretation lies with the hearer, often together with an audience of listeners. Signifying allows the speaker to maintain control of the interchange by reserving the right to insist on a harmless interpretation if the listener indeed shows signs of challenging the speaker's motives or intent. For example, B had been visiting A for quite some time when A asks:

A: What time is it?

B: Are you asking me to leave?

A: Naw man, I just want to know what time it is.

A's tone and expression played a large role in B's detection of the underlying meaning. So that in fact B did "get the message," despite A's denial, and left shortly thereafter.

Signifying, although used for specific purposes, may spring up in any natural, free-flowing conversation, with signaling cues which are often subtle and/or ambig-

uous. Inflection of the voice, eye gaze, cutting of the eyes and facial expression, are just some of the paralinguistic hints that influence interpretation. Although signifying can be used as a diplomatic way to communicate a fault, this is not necessarily the case. Sometimes it is amusing or treated as a joke. Mitchell-Kernan reports the following example:

Grace: After I had my little boy, I swore I was not having any more babies. I thought four kids was a nice-sized family. But it didn't turn out that way. I was a little bit disgusted and didn't tell anybody when I discovered I was pregnant. My sister came over one day and I had started to show by that time.

Rochelle: Girl, you sure do need to join the Metrecal for lunch bunch.

Grace: (non-committally) Yea, I guess I am putting on a little weight.

Rochelle: Now look here, girl, we both standing here soaking wet and you still trying to tell me it ain't raining. (p. 323)

Rochelle was letting Grace know in no uncertain terms that she knew Grace was pregnant and that there was no need to act as though she was not. In this example, an entire sentence, "we both standing here getting soaking wet and you still trying to tell me it ain't raining," requires a metaphorical interpretation.

The primary element of signifying is indirection, indirection almost wholly transmitted stylistically. The context constrains interpretation; markers of tone, dialect (Vernacular Black English), and nonverbal signals set the stage for the artistic display. A slight change in any aspect of the stage may change identical comments from one of play to one of aggression. It is recognized by the audience and the participants that it takes skill to correctly decode the message by manipulating the components of the speech act, and clever manipulation to produce a worthy retort.

### Marking

"Marking" is a narrative device commonly used in Black communities in the telling of folk tales or in the description of a scene witnessed by the speaker. In addition to reproducing the actual words of the original speaker(s), the narrator may adopt the voice, the speaking peculiarities, and the behavioral mannerisms, often inserting new content to gain specific expressive value. The replayed scene appears to be more of a caricature or parody than a direct imitation, but by portraying every observed nuance and idiosyncrasy, while overplaying notable features of the speaker, the narrator provides for the audience the full impact of what the narrator has perceived. Mitchell-Kernan uses the following example to illustrate these points.

The individuals here, with the exception of S<sub>1</sub>, had recently attended the convention of a large corporation and had been part of a group which had been meeting

prior to the convention to develop some strategy for putting pressure on the corporation to hire more blacks in executive positions. They had planned to bring the matter up at a general meeting of delegates, but before they had an opportunity to do so, a black company man spoke before the entire body. S<sub>2</sub> said, "After he spoke our whole strategy was undermined, there was no way to get around his impact on the whites."

S<sub>1</sub>: What did he say?

S<sub>2</sub>: (drawing) He said, "Ah'm so-o-o happy to be here today. First of all, ah want to thank all you good white folks for creatin so many opportunities for us niggers and ya'll can be sho that as soon as we can git ourselves qualified we gon be filin our applications. Ya'll done done what we been waiting for a long time. Ya'll done give a colored man a good job with the company."

S<sub>1</sub>: Did he really say that?

S<sub>3</sub>: Um hm, yes he said it. Girl, where have you been. (Put down by intimating S<sub>1</sub> was being literal.)

S<sub>1</sub>: Yeah, I understand, but what did he really say?

S<sub>4</sub>: He said, "This is a moment of great personal pride for me. My very presence here is a tribute to the civil rights movement. We now have ample evidence of the good faith of the company and we must now begin to prepare ourselves to handle more responsible positions. This is a major step forward on the part of the company. The next step is up to us." In other words, he said just what S<sub>2</sub> said he said. He sold us out by accepting that kind of tokenism. (p. 334-35)

In this example the entire passage spoken by speaker 2 requires a figurative interpretation. The narrator is implicitly identifying the speaker at the convention with a stereotypic portrayal of an "Uncle Tom," and he expects the audience to understand the full import of his message. In fact, a member of the audience is mildly ridiculed for even asking if that was in fact what occurred. Notice, also, that the last speaker's statement of what was "really said" could be taken as a figurative reinterpretation of S<sub>2</sub>'s original parody.

An examination of marking can lead to interesting insights into attitudinal correlates of language and nonverbal communication. By noting which linguistic and behavioral traits are selected for exaggeration, whether consciously or unconsciously, it is possible to gain a very accurate picture of how the person who is marking feels about the speaker and about what he or she had to say.

### Sounding

Another kind of nonliteralness peculiar to Black linguistic communication is "sounding," in many locations also referred to as "playing the dozens." It is also sometimes called "cracking," or "ranking." These terms are all labels for what Labov calls "ritual insult." According to Labov, the phenomenon also occurs in White peer groups, but the forms and topics are relatively limited, and the activity does not occupy any con-

siderable time for the group.

Engaged in by males, females, young and old, sounding is most common amongst adolescent and pre-adolescent Black children. The purpose of sounding is to put down one's adversary by means of insults which tend to involve close relatives (especially the mother) and which make derogatory allusions typically to physical or sexual aspects of that relative. Although sometimes done solely for purposes of amusement, dexterity in this verbal skill is one way to achieve status in the male peer-group. Sounds often, but not necessarily, involve taboos and obscenities. Labov observes that the appropriate response to a sound is another (ideally more clever) semantically or syntactically related sound. The measure of success is given by the evaluative response(s) of the audience. Labov's paper, "Rules for Ritual Insult," based on observations of an inner-city community, is a fascinating, informative, and often very amusing contribution to our understanding of the subtleties of sounding. Its value lies as much in its rich psycholinguistic and cognitive implications as in its specific treatment of sounding itself.

Labov approaches his analysis of sounding from the perspective of a sociolinguist concerned with discourse analysis in general. Sounding, he argues, is a form of ritualized insult. There is a subtle, delicate boundary between it and genuine, personal insult, a boundary that occasionally is traversed either through ineptness or ignorance, and a boundary whose transgression is potentially capable of leading to dire, even fatal, consequences. One of the distinguishing features of sounding is that the protagonist asserts something about a close relative of his adversary, typically the mother, that is derogatory but *patently untrue*. The potential danger lies in the fact that the speaker has to have an appropriate knowledge of the adversary's background if he or she is to be sure that the allegation is indeed untrue. The falsity of the assertion is sometimes guaranteed by its absurdity (e.g., "Your mother so old she got spider webs under her arms." On other occasions, however, the claim could be true. For example, compare "I went in Junior house 'n' sat in a chair that caved in," with "When I came across your house, a rat gave me a jay-walkin' ticket." Both of these allude to domestic poverty and squalor. However, the first describes a situation which is a possible one. Since sounding always and necessarily involves an audience, the speaker would have to presuppose that the audience knew that it was factually false—but no such presupposition was warranted in the particular situation in which the utterance was made. The rules for sounding were broken and the response, rather than another (hopefully) "superior" sound, was a literal denial: "You's a damn liar..." It is situations of this kind, wherein the playful (if often cruel) nature of sounding is replaced by genuine "bad-mouthing," that have the potential for violent conflict. Both ritual and personal insults are means of putting

people down, but the former are socially acceptable means while the latter are not.

Although noting that there are exceptions, Labov proposes that the basic underlying structure of a sound is *T(B) is so X that P*, where *T(B)* is the target of the sound (e.g., Your mother), *X* is the attribute that is focused on (e.g., thin), and *P* is the consequent proposition (e.g., she dance between raindrops). Labov makes a convincing case for supposing that many sounds are derived from this underlying structure by deletion rules, especially of the attribute, which can often be inferred from the proposition.

### Some Psycholinguistic and Educational Implications

Signifying, marking, and sounding are fascinating communicative devices in their own right, but the skills that their production and comprehension require have some especially interesting psycholinguistic and educational implications. By way of introducing these implications, consider the following statements from, first, Labov:

one way to achieve excellence in sounding is to develop complex comparisons with a high degree of left-hand embedding which suspends the final proposition. (p. 311)

Perhaps the best known (response of the “At least my mother ain’t” form) is: “At least my mother ain’t no railroad track, laid all over the country.” Such forms frequently occur as simple similes, such as, “Your mother’s like a police station—dicks going in and out all the time.” (p. 286)

Labov goes on to note that:

In settings far removed from the classroom, under standards of performance that are alien to those of the school, peer-group members develop a high level of competence in syntax, semantics, and rhetoric. (p. 307)

Consider, additionally, the following two quotations from Kochman (1972a):

Abrahams observed that “ability with words is as highly valued as physical strength” (Abrahams, 1964, p. 62). In the sense that the status of one of the participants in the game [of sounding] is diminished if he has to resort to fighting to answer a verbal attack, verbal ability may be even more highly regarded than physical ability. (p. 227)

Without the control of the group, sounding will frequently lead to a fight. In the classroom, *from about the fourth grade on*, fights among Black boys invariably are caused by someone sounding on the other person’s mother. (p. 228, italics added)

Although the objection could be made that Kochman overstated the case vis à vis the causal connection between fighting and sounding, if we take all these remarks together, we come to an interesting conclusion. Children, often only 8 or 9 years of age, frequently engage in a linguistic activity that is bound by quite strict sociolinguistic conventions whose recognition is essential for successfully engaging in the activity. A high

degree of complexity is involved in both production and comprehension. Much of this complexity derives from the reliance of these forms on metaphors and metaphor-like relations. For example, sounding, which seems to appear developmentally before the other forms, is based upon metaphorical comparisons. Recall Labov’s representation of the canonical form: *T(B) is so X that P*. The form is that of an assertion to the effect that the target has some property to some degree. The degree to which it has it is implied in terms of some (bizarre) consequence that would follow from possessing that property to that extent. The consequence is literally untrue in just the same way that in a simile, or its corresponding metaphor (e.g., John is like a telephone pole), the referent, John, is not believed by the speaker or hearer to be *literally* as thin as a telephone pole, and, for that reason, is not believed to be *literally* like one. In other words, in such a simile or metaphor, John is alleged to be like, or as thin as, a telephone pole *only metaphorically speaking*. The two things being compared are not claimed to be *really* alike at all (Ortony, 1979b). Thus, if we recognize that similes are essentially metaphorical in nature, we discover that the use of metaphorical devices is rampant in the language of both Black adults and children, and that it is already widespread by the time children reach fourth grade.

The important point about the heavy dependence on metaphorical language in sounding and similar language patterns is that it suggests that children who have a lot of exposure to metaphorical uses are well able to understand them. This is interesting in the light of attempts by various psychologists to show that the ability to properly understand metaphors does not appear until early adolescence (e.g., Asch & Nerlove, 1960; Winner, Rosentiel, & Gardner, 1976) and that it may depend on the attainment of certain specific cognitive abilities such as late concrete operational thought (e.g., Billow, 1975; Cometa & Eson, 1978). This research has typically been conducted using middle-class White children. Yet, the mere fact that Black children understand and engage in sounding at such early ages, while their middle-class White counterparts are still reputed to be having difficulties, lends some support to an alternative account, namely that a major cause of inability to handle metaphorical language is a lack of appropriate experience in dealing with it.

Metaphor is important in all manner of communicative situations. It is especially powerful in its capacity to elucidate new concepts in terms of familiar, but superficially only remotely related, old ones (see, Petrie, 1979). Furthermore, the skillful use of metaphors in sounding and related culture-specific linguistic practices amongst young Black children can give rise to high-quality performance and attendant peer-approbation. Linguistic dexterity is highly valued among Black children. This presumably means that they aspire to it. It would be an interesting challenge to find a way whereby

the educational system could capitalize on this.

Given the current paucity of knowledge about the issue, it would be foolhardy for us to propose ways in which to tap the skills that underlie sounding, signifying, and marking in educational contexts. Certainly we do not advocate that the forms themselves be admitted into the normal communicative structure of the classroom. However, we can identify at least one direction that might be worth exploring. As noted earlier some researchers feel that one reason Black children do not perform well in today's schools is low motivation. Researchers taking the so-called "child-centered" approach (e.g. Zigler & Butterfield, 1968) have suggested that the child's level of motivation is not sufficient to promote, activate, or encourage high achievement. Others feel that achievement motivation is present but that it is not being adequately tapped (see, Boykin, 1978; Maehr, 1974). Perhaps utilization of the skill and dextrous manipulation of metaphorical devices exhibited in sounding, signifying, and marking is one way to uncork this source of potential for higher performance. However, much research would be needed to determine if this was so. A first step would be to determine whether the Black child's adroitness in dealing with metaphorical language generalized beyond the specific, sometimes stereotypic, forms found in the subculture. Recent research findings (e.g., Winner, Engel, & Gardner, in press; Reynolds & Ortony, 1979) would lead one to suspect that sensitivity to metaphorical uses of language is largely dependent upon familiarity with various pragmatic factors. For example, the research suggests that young children can understand the metaphorical comparisons implicit in metaphors that they cannot understand when cast in the form of the corresponding similes. In other words, children need to know that sometimes making a comparison is called for even when it is not explicitly signaled (for example by a verb of similitude). The wide and frequent exposure to metaphorical uses of language that the Black child experiences bodes well for research along these lines, and perhaps, eventually, for the school performance of the Black child.

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## Literacy as Focused Interaction\*

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Recently researchers have been very enthusiastic about the issue of literacy. Looking at literacy as a historical phenomenon has allowed us to gain a perspective on our own activities in research and education. As contrasts are drawn with non-literate or oral ways of communicating, we have been gaining useful perspectives on the development of human cognition. In comparative research we are gaining new ways of looking at concepts of culture and ethnicity. In the post-Kuhnian game of guessing paradigm shifts it does not take a very shrewd player to notice the enthusiasm for talk about literacy. As a colleague remarked recently, "It explains everything."

Nevertheless, literacy is not the monolith needed to erect a structure that will include everything. As the work of the past decade shows, we need to think of at least several kinds of literacy. Much of what is attributed to literacy might be more narrowly restricted to what we would call essayist literacy (Scollon & Scollon, 1979). Literacy in this style involves the ability to read and write material that is decontextualized, high in the proportion of new information to old information, and internally logical. The relation of the text to the situation is deemphasized and a reading "public" or at least partially unknown audience is assumed, and therefore both readership and authorship are fictionalized. Essayist literacy is largely taught and learned through the formal process of schooling.

\*This paper is a revision of a paper presented at the 1979 meetings of the American Anthropological Association.

There are more pragmatically grounded literacies as well. These involve dealing with textual materials for which the audience is known, often a single individual; which are deeply contextualized in the situation; and in which the relationship between the persons of the writer and of the reader is emphasized. Learning of this literacy is generally informal. An example of this literacy, writing letters, must be thought of as conceptually very different from writing essays for publication.

A third kind of literacy involves the understanding of the religious value of texts. Generally, literacy in this style is unilateral, that is, one reads but does not write sacred texts; the value of "received" truth is emphasized. The learning of religious literacy takes place as an aspect of religious, moral, or ethical instruction. Again, this sort of literacy must be thought of as conceptually different from other literacies.

In fact, when we seek to unravel the social and cognitive implications of literacy we come up against not really knowing how many kinds of literacy there are. And as the number of varieties approaches the number of problems to be solved, however slow the approach may be, one begins to wonder just how much explaining is being done.

Our own research concern has been with non-literate traditions, especially Northern Athabaskan. As we have read the literature contrasting literate and oral societies (or world views, cognitive styles or orientations, developmental stages, and so forth) we have been unsatisfied with the assumption of an oral monolith underlying both individual and historical human development. We do not deny, of course, that literacy is "later" in a historical perspective. What troubles us is the assumption that there is only one way to be non-literate, that the "oral" state is in some way the absence or lack of development of the literate state. Researchers have too easily equated the non-literacy of competent elders in a non-literate society with the non-literacy of children in literate societies.

In our work with Northern Athabaskans in Canada and Alaska two kinds of material have driven home to us the need for a more particularistic view of oral traditions. The first was research based on the observation of our own children. Elsewhere (Scollon & Scollon, n.d.) we have argued that at two years of age our daughter was "literate" in virtually all senses of the most decontextualized form of literacy, fully three years before doing any real reading and writing. We have argued that her non-literacy was a "literate" non-literacy. In other words, we have argued that what makes literacy "literacy" cannot possibly be simply reading and writing text, at least in a developmental perspective. The child does not recapitulate the history. The non-literacy of our child, we have argued, was fundamentally different from the non-literacy of her Athabaskan peers and their elders.

The second observation we had to deal with was the

almost complete absence of formulaic speech among the Athabaskans—in both common day-to-day contexts and especially in their narrative performances (see Scollon & Scollon, n.d., in press). Formulas have come to be the hallmark of oral society. While there are various explanations for this, these explanations tend to cluster around time constraints on cognitive processing and the degree of contextualization in small scale face-to-face societies. From the literature on the subject (e.g., Goody, 1977, n.d.) one comes to expect lengthy, bard-like, if not actually bard, performances of highly elaborated traditional tales and legends in which the storyteller buys time to process his next line or verse by running down a list of time-consuming formulas or epithets. One is struck in an Athabaskan storytelling by the looseness of the interaction, the slowness of the pace, the contributions of the audience (which can be considerable), and the almost leisurely attitude toward getting the telling concluded. In some cases one is also struck by the brevity of what is regarded as a good telling. Historical studies show that these are clearly not the results of the disintegration of the tradition as some thought at first. Similar observations by members of other non-literate traditions indicate that our perception is not unduly colored by our own literate tradition. Our only conclusion is that Northern Athabaskans enjoy an oral tradition that is distinctly unlike the “bard and formula” oral tradition which the literature has led us always to expect.

The best evidence comes from a study of the texts themselves. Working with parallel versions of texts told in English and Athabaskan languages convinced us quite early that while the texts were laconic and the performances had been leisurely, they were by no means formally sloppy. Stories are constructed around lines, verses, stanzas, and scenes. Each unit in this hierarchy is formally marked and governs some aspect of the discourse such as information structure, foregrounding, and perspective. For example, anaphoric reference is governed by the stanza.

What took us some time to see, however, was that the formal structure of the Athabaskan narrative is an outcome of the interaction between the storyteller and the listener. As two examples we can look at the verse and the stanza. The verse is the unit of grounding. The last clause in the verse is the foregrounded material. Backgrounded material is given in the portion of the verse that precedes the final clause. In storytellings, the audience responds at the end of each verse as an indication that the story is being followed. In storytellings that people regard as good, the response of the audience is fuller. The storyteller provides the background information and the audience completes the verse with the verb or even the full foregrounded clause. In short, the “listener” tells the story. The role of the “storyteller” is to organize the endeavor and provide the background in a sufficiently full form that the listener can make his or

her own sense and even provide the wording.

Formally, the Athabaskan narrative verse is a riddle. It is a one-on-one situation with a storyteller and one person who responds. Others who are present do not interfere with this jointly produced performance. Both narratives and riddles are used as ways of teaching careful observation, indirectness, and non-intervention.

Moving up to the next level, the stanza is a group of verses that governs perspective. The first verse of the stanza gives the narrative substance of the stanza and presents the focal noun phrase, whether subject or object. As the storyteller monitors the participation of the audience, he or she provides further verses for clarification or elaboration. When agreement is reached, the storyteller begins a new stanza. In the best telling, the audience provides the right conclusion to the first verse and no further expansion is needed. That is, the more ideal the circumstances, the more easily agreement is reached and the shorter the storytelling becomes.

Without a long digression on the role of narrative in the social construction of reality, we would like to suggest that the Athabaskan narrative is performed as a mutually negotiated construction of a world through face-to-face interaction. It emphasizes respect for the sense the other is making of the situation and carefully avoids any unilateral attempt to make any one participant's sense of the situation or of the world “stick.” We now view the highly formal discourse structure of the Athabaskan text as a structural reminder of the living process of the storytelling interaction. It is the outcome of a situation in which the highest respect for the individuality of the participants is being mutually expressed.

Our argument, then, is that just as we need to talk about several varieties of literacy, we also need to talk about several varieties of non-literacy. This leads us to believe that there is something else going on. The high degree of negotiation and mutual respect is the key to understanding the difference between the “bard and formula” oral tradition and the Athabaskan tradition. We feel it is also the key to understanding what people are trying to get at with talk about literacy and non-literacy generally.

Some situations allow for more negotiation or repair work than others (Erickson, 1976). We would like to define as “focused situations” those in which repair work is closely limited. If we assume that people will always try to make some sense of situations, then we can see that a focused situation forces the adoption of some non-negotiated way of making sense. We could even go so far as to suggest this as a useful definition of power, the ability to make one's own sense “stick” in focused situations. The key to focused situations is the non-negotiated, unilateral process of making sense.

There are, of course, many sources of focusing. Time limitations can focus situations. Gatekeeping situations become focused among other reasons because the time in which they can be carried out is limited, there is not

time for the leisurely negotiation of positions. Crowding can also focus situations. We suggest that classrooms may become focused because of the difficulty or impossibility of separately negotiating positions among all members of the group. Some unilateral sense-making gets done and is made to "stick."

To the same degree but in a much different way, the medium can focus a situation. Writing to a "public" forces the author into a situation of non-negotiation. Because of the distance from the audience caused by the medium and because the audience is complex or unknown, the author must assume responsibility for unilateral sense-making. In this way, then, writing can also be thought of as a focused situation.

Taking this perspective we can see that essayist literacy and "bard and formula" non-literacy share a significant degree of focusing. Letter writing and Athabaskan oral narrative on the other hand are relatively less focused, Athabaskan oral narrative being the least focused, or as we would call it, non-focused. One bit of the "everything" that we might try to explain with this reorientation is why literacy "takes" in some oral societies and not in others. We would argue that the step from focused non-literacy ("bard and formula" non-literacy) to essayist literacy is taken across a much narrower gulf than the step from negotiated or non-focused non-literacy to essayist literacy. Other steps and gulfs can be imagined.

Now before there is a rush on this as a new taxonomy of cultures, societies, ethnic groups, or cognitive styles as focused or non-focused let us begin to close by suggesting that there is under this all a single value that needs to be assumed to produce the rest. We see the focusing of situations coming out of various pressures on the human individual, pressures of time, group complexity, and the distancing produced by media such as writing and television. Non-focusing can be seen then as the non-pressured assertion of the value of individual human difference. Researchers who have worked with Athabaskans have generally been struck by the deeply felt and strongly defended respect for individual difference. Our point then is that *the non-focused, non-intervening but very interactive and willingly negotiated Athabaskan oral narrative represents a very high regard for human individuality and distinctiveness*. The Athabaskan avoidance of focused situations represents a resistance to the pressured loss of that value.

More generally, then, we would suggest that as individuals engage in communication they will differ in the degree to which they wish to respect individual difference. *To the extent to which they wish to respect difference they will maintain the situation as non-focused*. They will keep open the means of negotiation; they will avoid hurried decision-making; they will avoid crowding of the situation with either too many participants or too many issues; they will avoid distancing media and seek face-to-face interaction. On the other hand, to the extent

to which they seek to emphasize the same sense-making they will move toward focusing the situation while negotiating which participant gets to make his or her own sense "stick." In our view any attempt to classify groups as focused or non-focused is largely beside the point. Focusing or non-focusing is not a structural property of groups but rather a quality of face-to-face interaction. In our view the structures of language, whether of text or utterance, are the artifacts of choices people make about human individual difference in face-to-face situations.

Since in taking a situation perspective we are seeking to avoid classifying groups such as ethnic groups, we need to answer the question now: How do we justify speaking of "Athabaskans" in contrast with "Whitemen"? We would argue that these characterizations of ethnic groups, at least in the north, only become relevant to the extent that individuals meet each other and for some reason do not communicate successfully. That is, we would argue that "Athabaskan" (or "Indian" on the one hand and "Whitemen" on the other, are at their foundation ethnic stereotypes based on typical behavior in face-to-face interaction. The typical interaction is a failure to reach agreement on the degree of focusing. The 'Whiteman' or member of mainstream modern American society (white or not) tends to approach any interaction with considerations of time constraints, institutional limitations, social complexity, and public audience uppermost in his or her mind. This leads to seeking a focusing of the situation. The Athabaskan participant tends to approach any interaction with considerations of respect for the individuality of all participants uppermost in his or her mind. This leads to seeking non-focused patterns of interaction. This in turn leads to one being stereotyped as pushy, egocentric, or ethnocentric, in other words, focusing the situation. The other is stereotyped as withdrawn, shy, or uncooperative, in other words, not accepting any unilateral sense-making. Only to the extent that members of any ethnic group will agree to these typifications is it possible to speak of one group as more or less focusing than another.

What is Athabaskan about Athabaskans, then, is not non-literacy; there are literate Athabaskans. It is also not non-focusing; Athabaskans in some contexts choose focusing strategies of interaction. What does often become identifying for Athabaskans, though, is a very high regard for human individuality, a deep respect for human difference, so much so that many Athabaskans feel quite uncomfortable with any designation that seeks to group or classify individuals, even the designation "Athabaskan," which is neither an Athabaskan word nor an Athabaskan concept.

As we have argued, this respect for human individuality is best guarded in non-focused interaction. Literacy in its essayist forms is focused interaction. The conflict should be apparent. To the extent one works



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through literate modes of expression, one is forced into non-negotiated forms of making sense. As writer one is forced by the medium to assume one's audience. As reader one is forced to accept the unilateral sense of the situation created by the author. This distancing of the medium, we argue, is in direct conflict with a central human value, the value placed on mutual sense-making. To the extent that this value is a significant Athabaskan value, literacy is a threat to Athabaskan identity.

To close now we would like to make a comment on method. Research on communication is fundamentally based on communication. It may be focused or non-focused. As focused communication, research easily becomes top-down assertion of one kind of sense-making, expressing the researcher's position, and is likely to be blinded to the position of the ones studied. As non-focused communication, research can be mutual sense-making expressing patterns of deference and respect for difference. Focused interaction as a research strategy cannot easily discover other ways of making sense because these cannot be negotiated in focused interaction. Only the process of negotiating ways of making sense can allow the discovery of other ways of making sense. It is our view that the discovery, investigation, and explanation of non-focused patterns of interaction, cognition, and teaching are absolutely founded on non-focused patterns of communication in research.

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In the most common type of cross-groups study the researcher conceives of a variable (frequently referred to as the independent variable) by means of which a certain population can be divided into subpopulations, or groups. The researcher then samples members of the subgroups (or creates the subgroups when the independent variable is under his or her control) and compares their scores on the dependent variable — the variable in the performance of which the researcher is interested. To cite just a few examples, one could divide human beings on the basis of ethnic origin and measure performance on a conservation task; or divide rats according to the schedule of reinforcement they have received and observe the resistance of their learning to extinction; or divide mental patients on the basis of their scores on the MMPI and measure their ability to delay gratification; or take seven-year-old children, divide them according to their performance on a conservation task, and measure their performance in a series-completion task.

The typical analysis carried out on the results obtained in such studies calls for a comparison between the mean performance of subjects in the different groups. To test if the differences observed are not due to the operation of chance factors, the researcher compares the size of the mean differences (or some derivation of it) with some measure of error, or noise, in the data. Only if the differences between the groups are large relative to the error term does the researcher conclude that the groups really differ in their performance. The measure of error is usually based on the variability of the dependent variable among subjects from the same groups. The rationale for using the within-group variability as a measure of the error variance is quite simple: all subjects in the same group have the same value on the selection (independent) variable, hence if there were a perfect correspondence between the two variables subjects from the same group should all exhibit the same performance on the dependent variable; any deviation from the group mean is due to the operation of error factors as far as the independent variable is concerned.

This basically "experimental" approach cuts across the classical distinction made by Cronbach (1957) between the experimental and the correlational disci-

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plines in psychology. Many cross-groups studies fall within the correlational discipline, since they divide individuals into groups on the basis of an already existing difference in the population (e.g., cultural group, scores on a test) rather than on the basis of values along an experimenter-manipulated variable. However, all cross-groups comparisons of the kind described above share the experimental view which regards within-group variation as error.

While within-group differences are indeed "noise" as far as the original selection variable is involved, they may also reflect the uncontrolled operation of other variables. Thus, for example, in comparing the performance of two age-groups on an arithmetic test, the researcher may worry about the possible effects of test anxiety on performance. Any differences within the same age-group due to the operation of the test-anxiety variable are lumped into the error term in the regular analysis. Cronbach (1957) proposed one general way of dealing with individual differences which might be caused by the operation of another variable. His approach, which was originally to be applied in experimental studies, calls for a search for Aptitude by Treatment Interactions (ATI). According to this method the researcher first divides the subjects into a number of groups on the basis of their scores on a measure of some aptitude. Different members of each subgroup are then subjected to various experimental treatments, and the researcher checks for differential effects of the treatment (i.e., checks for the existence of an interaction between levels on the aptitude measure and the experimental treatment). Since "treatment" can also be construed to mean any independent variable in the study, Cronbach's approach can be generalized to any cross-groups research of the kind discussed here. In the test-anxiety example given above the researcher would first assess the subjects' test anxiety, then separate them within each age-group into, say, groups of high-, medium-, and low-anxiety, and then test to see if level of anxiety interacts with age.<sup>1</sup> The ATI approach has been adopted by many researchers, and in a followup article Cronbach (1975) was able to report that the study of aptitude-by-treatment interactions was flourishing.

Cronbach has not been alone in his call for the study of individual differences in experimental settings (see, for example, Cattell, 1966; Eysenck, 1976; Underwood, 1975). Interest in the study of individual differences in such studies has recently been on the rise, and is evident

<sup>1</sup>One would also check of course for the main effect of test anxiety. In general ATI can be viewed as a way of introducing another factor into the research design. However, since the technique was originally suggested as a way of introducing measures used by correlational psychologists in experimental studies, it has attained a special status in the study of individual differences.

in most fields of psychology. To mention just a few examples, studies of individual differences have been conducted in fields as diverse as cognitive psychology (e.g., Hunt, 1978, Hunt, Frost, & Lunneborg, 1973), learning (e.g., Gagné, 1967), instruction (e.g., Cronbach & Snow, 1977), social psychology (e.g., Bowers, 1973; Sarason, Smith, & Diener, 1975), and personality (e.g., Eysenck, 1976; Magnusson & Endler, 1977). The work reported in Cattell (1966) covers most fields in psychology.

The ATI approach has a "top-down" flavor to it: the choice of the aptitude is done in advance, on the basis of some theory or former findings. The advantage of having such a controlling variable is that any findings can be tied in with the existing body of knowledge in the field. But the approach has its disadvantages as well: many times researchers do not expect their data to contain systematic individual differences, and even when they do it is not clear which variables might account for such differences. As a first step such researchers are probably interested in finding out if there exist systematic individual differences in the performance of their subjects. Only if such differences are detected will it be worth their while to try to understand their meaning. In other words, there is also a need for a "bottom-up" approach which starts with the data themselves, and attempts to discover individual differences in the performance of the dependent variable. The need for a technique which uncovers systematic individual differences in performance has probably been felt by many frustrated researchers who noticed that some of their subjects exhibit a certain response pattern while others exhibit a different one, but were at a loss about how to handle the differences. Such differences are sometimes confined within one group, while at other times they cut across groups. Pulling out these differences would both clarify the effects of the original independent variables, and point to ways in which subjects differ in their responses.

What I would like to describe in this paper is a bottom-up approach designed to uncover systematic individual differences in the performance of the task at hand. Only if and when found are such differences further pursued. This approach offers a number of advantages: First, by separating the variance due to systematic individual differences from the error term, the researcher increases the power of the statistical tests of the effects of the original independent variables. Second, since the starting point of the search for systematic individual differences is performance on the dependent variable, the researcher is assured of the relevance of the findings for the topic of interest. Finally, and probably most important, the analysis points out the ways in which subjects systematically differ in their performance of the task at hand. Once made aware of the different patterns evident in the behavior of the subjects the researcher can further pursue the question of what factors actually underlie such differences.

The basic idea is to observe the response patterns of the subjects and to test to see if they divide into a small number of different ones. A prerequisite for conducting such a search is, of course, that each subject respond under a number of different conditions (e.g., scores in a number of tasks or subtests). Once a vector of scores is available for each subject it is possible to compute the degree of similarity between subjects. To see how this can be achieved consult the schematic description for the results of some studies presented in Table 1. The table presents the results of  $n$  subjects, each having a score on  $K$  measures of performance. The subjects may or may not be from the same group, but in cross-groups comparisons they will have come from a number of separate groups. One common analysis performed on these kind of data calls for calculating the similarity between the different variables (usually by means of the correlation coefficient). The resulting data appear in a (correlation) matrix representing the similarity between all pairs of variables. Such a matrix is often factored in an attempt to identify clusters of variables which are relatively similar to each other and differ from the variables in other clusters. Such clusters, if and when found, are labeled "factors." This common approach, which determines the similarity between variables and across people, is the R-technique described by Cattell (1952).

**Table 1**

**Schematic Presentation of Data Obtained in Studies Which Enable the Application of the R-technique and the Q-technique**

	Variables						
	A	B	C	...	j	...	K
Subjects							
a	$x_{aA}$	$x_{aB}$	$x_{aC}$		$x_{aj}$		$x_{aK}$
b	$x_{bA}$	$x_{bB}$	$x_{bC}$		$x_{bj}$		$x_{bK}$
.							
i	$x_{iA}$	$x_{iB}$	$x_{iC}$		$x_{ij}$		$x_{iK}$
.							
n	$x_{nA}$	$x_{nB}$	$x_{nC}$		$x_{nj}$		$x_{nK}$

Instead of calculating the similarity between any two variables, one could take a different approach and calculate the similarity between any two individuals. In this case one would calculate the similarity between the profiles of the responses, across the variables. The resulting similarity matrix would then have people, rather than variables, as its rows and columns. Again, this matrix may be analyzed in a search for clusters of people who are relatively similar to each other, but dif-

fer from people who fall into other clusters. Such clusters of people, if and when found, are labeled "types." This approach, which uses the similarity between people across variables as its basic data, is the Q-technique described by Cattell (1952). When the subjects in a study have a number of response measures, the Q-technique can be used to uncover systematic individual differences, since it will separate subjects with different response patterns. Since in our case the patterns of behavior in question are observed within a single study, and in order to distinguish the products of the analysis from the more general types described in some personality theories, I label the results of the proposed analysis a "mini-typology."

The search for mini-typologies raises a number of technical questions: How many subjects are necessary to perform such an analysis? How many data points should there be in the response pattern of each subject? What measure should be used to determine the similarity between people? How should the similarity matrix be analyzed to yield the mini-types? In what ways could a researcher proceed once a mini-typology is detected? In the remainder of the paper I will address these questions and demonstrate the potential benefits of the proposed approach by presenting an example of a study in which it was applied.

### Technical Considerations

*Number of data points in the profile.* As a rule, the larger the number of data points in the profile the better. This is the case since the stability of the measure of similarity between individuals is positively related to the length of the profile used to establish it. Carroll (1978) recommends that to establish  $m$  factors (mini-types, in the present case) the sample size (the number of points in the profile) be at least  $(2m + 2^m)$ . Carroll's rule of thumb indicates that to establish the existence of two mini-types (the minimal number to be of interest) there should be at least eight measurements on each subject. Some studies may fall short of this number, let alone the one necessary to establish the existence of a larger number of mini-types. However, the small number of data points found in many studies is at least partially offset by the fact that each one of these points almost always represents mean performance over a number of replications. As a result each value appearing in the profile is quite stable, even if the profile itself is short. However, when the number of data points is small or when each one is not based on a number of observations, researchers should be very cautious in their interpretation of the ensuing mini-typology, and should probably avoid the application of the Q-technique altogether.

*Number of subjects in the study.* Another question has to do with the minimal number of subjects necessary to establish the existence of  $m$  mini-types. Carroll (1978) mentions here Thurstone's criterion which requires that

this number be equal to or greater than  $[(2^m + 1) + (8m + 1) \cdot 5] / 2$ , but recommends that a larger number be used. In most studies the sample size considerably exceeds this minimum requirement.

*The measure of similarity.* To determine which individuals "go together," the Q-technique uses the similarity between the profiles of all the pairs of individuals involved. In choosing a measure of similarity between profiles one should note that a profile carries three major types of information (Nunnally, 1967): level, dispersion, and shape. *Level* refers to the mean score of the variables in the person's profile. *Dispersion* is the extent to which the scores in the profile deviate from the mean performance. *Shape* is related to the actual contour of the profile. Various measures of similarity give each one of the three aspects a different weight. For example, the correlation coefficient takes into account only the shape of the two profiles and disregards level and dispersion. Other measures such as  $d$  (see Osgood & Suci, 1952; Cronbach & Gleser, 1953), or  $r_p$  (see Cattell, 1949; Cattell, Coulter, & Tsujioka, 1966) take all three into account. The researcher's actual choice of a similarity measure would depend on what he or she regards to be the most important aspects of the data.

It is important to note in this respect that when the variables comprising the subject's profile have different distributions, the researcher should first transform the variables to a common base, and only then proceed with the proposed analysis.

*Method for obtaining the mini-types.* The final question here has to do with the method for obtaining the mini-typology from the similarity matrix. Here it is possible to use factor-analytic techniques (see Harman, 1960; Horst, 1965), multi-dimensional scaling techniques (e.g., Guttman, 1968; Kruskal, 1964; Shepard, 1974), the additive-trees technique (Sattath & Tversky, 1977), or any similar method. The choice of the technique is up to the researcher. It is likely that the ease with which the researcher is able to determine the group (mini-type) to which individuals belong will emerge as an important consideration in deciding which method to use.

### An Example

The following is an example used to illustrate the application of the Q-technique to results obtained in a cross-groups study. The data were obtained in a developmental study involving memory for narrative prose. The subjects were 20 first-graders and 20 fifth-graders whose task was to judge whether certain assertions were true or false given the stories they had heard. The profile of responses consisted of eight points, each representing the subject's error rate for the different types of questions involved. The eight types of questions were derived from the factorial combination of three variables: type of information asked about in the question (semantic or episodic), total amount of information about the subject

of the question (much or little), and the correct answer to the question (true or false). Each one of the eight data-points reflected the mean error-rate to questions of this type. The distance measure  $d$  (see Nunnally, 1967) was used to calculate the similarity between all pairs of subjects. The value of  $d$  for any pair of profiles is obtained by summing the squared differences between corresponding points, and then taking the square root of that sum.

To find if the data indicated the existence of systematic individual differences the similarity matrix was analyzed by means of the additive-trees technique developed by Sattath and Tversky (1977; see also Tversky & Sattath, 1979). I have chosen this method for factoring the matrix over other techniques since the uninitiated find its output the easiest to interpret, and since it requires relatively few arbitrary decisions as to where the division line between the mini-types should be drawn. The output of the program is a "tree" representing the program's solution to the problem of arranging the subjects in such a way that the distances between them on the tree be as faithful a reconstruction as possible of the distances between them in the similarity matrix. The "trunk" of the tree appears on the left side of the output, while the individual subjects appear as "leaves" at the rightmost ends of the branches. The distance between any pair of subjects on the tree is the length of the horizontal lines which have to be travelled to get from one individual to the other. When a number of sub-branches all come out from a relatively long major branch then the subjects at the ends of those branches are all similar to each other, and different from subjects who are placed at the ends of sub-branches coming out of other major branches. In addition to producing the tree the program also provides measures of the quality of the solution.

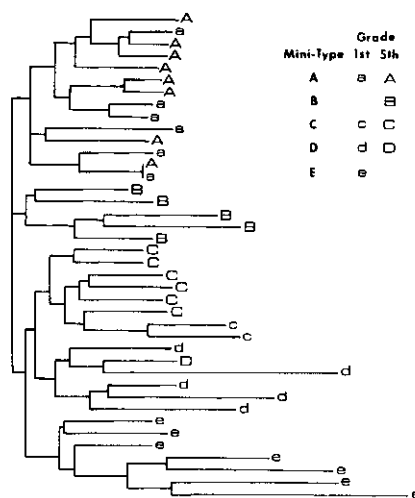


Figure 1. The output of the ADDTREE program and the division of subjects into mini-types.

The analysis of the results obtained in the present study resulted in the "tree" presented in Figure 1. The tree has three major branches, and the lowest one further divides into three branches. For illustrative purposes the subjects were divided into five groups, labeled by the letters A to E. Note that the decision as to how to divide the tree is up to the researcher. Inspection of the ages of the subjects in the resulting five groups reveals an interesting picture: the number of first-graders and fifth-graders in mini-types A through E are 6-8, 0-5, 2-6, 5-1, and 7-0, respectively. In other words, with respect to age there was one "mixed" mini-type (A), and two relatively "pure" mini-types within each age group. The mean response patterns of subjects in each one of the five groups are presented in Table 2. The response patterns are obviously different, and provide interesting information about the behavior of different subjects in the experimental situation studied. A regular analysis of variance of the data revealed that overall the error rate of the first-graders was significantly higher than that of the fifth-graders; however, we can see here that eight of them belong to mini-types (A and C) with a low error-rate, while the other twelve belong to groups (D and E) with a high error-rate. There are also five fifth-graders with a high error-rate (B), but their pattern of errors was sufficiently different from that of members of mini-types D and E to put them in a different cluster. Unlike the first-graders in D and E, the fifth-graders in B made fewer errors in judging assertions about semantic information and about the concept with much information. The division of the first-graders with the high error-rate into two separate groups — D and E — also reveals an interesting phenomenon: the six subjects in D exhibited a response bias to judge assertions as false, while the seven subjects in E had a strong bias to judge semantic assertions as true. Overall, then, many of the first-graders exhibited a response bias — a fact difficult to uncover in the regular analysis due to the opposite (and therefore cancelling) effects of these biases.

**Table 2**  
**Mean Error Rates: Pattern of Responses**  
**of Each of the Five Mini-Types**

Type <sup>a</sup>	Semantic				Episodic				Mean
	Much		Little		Much		Little		
Amount <sup>b</sup>	True	False	True	False	True	False	True	False	Mini-Type (n)
A (14)	.06	.05	.12	.17	.04	.05	.00	.12	.07
B (5)	.17	.00	.26	.26	.13	.17	.20	.47	.21
C (8)	.08	.00	.17	.17	.17	.02	.42	.00	.13
D (6)	.39	.28	.06	.06	.25	.14	.39	.11	.21
E (7)	.19	.48	.14	.47	.27	.21	.19	.24	.27

<sup>a</sup> Type of Information

<sup>b</sup> Amount of Information

In many cases one would probably want to reanalyze the original data with the mini-types as an additional factor. The discussion of the separate response patterns was presented to illustrate qualitatively the kind of findings one might expect following the detection of systematic individual differences. It should be clear, however, that the differences detected are to be regarded as suggestive, rather than conclusive. Now that the Q-technique has exposed the existence of systematic individual differences the researcher can decide what they might mean, and if and how they are to be further investigated.

## Discussion

Having demonstrated how the Q-technique can be used to derive a mini-typology I would like to discuss in more general terms the cases in which a mini-typology yields meaningful data, and the cases where it does not. In some cases the mini-types discovered by the Q-technique will reflect the operation of the independent variables rather than genuine individual differences. Such cases are easy to identify once the researcher is made aware of the possibility of their existence. If most or all members of every one of the original groups fall within one mini-type, and every mini-type consists wholly or mostly of members of one group only, then the mini-types merely reflect some effect(s) of the independent variable(s) under study. Other telltale signs of such a situation are that the independent variables have an effect even before mini-types are taken into account, and that replacing the original groups with mini-types in the analysis yields much the same picture. Mini-typologies uncover real individual differences when members of the same group fall into different mini-types, and/or when mini-types cut across the original groups. In such cases taking mini-types into account may well lead to different results than those obtained in the original analysis. When the mini-typology results in a further breakdown of the original groups the researcher is faced with some interesting individual differences waiting to be explained. When the analysis reveals mini-types which consist of members from different groups the researcher would do well to reexamine the relevance and the validity of his or her independent variable for the task at hand. Such a reexamination may be particularly useful when mini-typologies turn out to cut across assigned variables such as race, socio-economic status, sex, or age. While the prevalent analysis of cross-groups studies emphasizes differences between groups, the Q-technique provides a way of uncovering similarities between groups when they exist.

Mini-typologies are also useful for yet another (though specialized) purpose. This is when the researcher is interested in identifying subjects who exhibit response patterns which are atypical of most of the members of their group. As mentioned above, when the original groups differ in their performance the mini-typology would identify "pseudo mini-types," each

mostly consisting of members from one group. However, few members from one group may end up in a mini-type of members from another group. Such subjects may be of particular interest where notions of change and transition play an important role (say in studies involving development, social mobility, or cross-cultural change). The report in such a case might state, for example, that "Overall the nine-year-olds performed differently from the five-year-olds, but some of the young subjects exhibited a response pattern similar to that of most nine-year-olds." In many cases such unique subjects are of particular interest, and the mini-typology analysis may help in identifying them.

The search for mini-typologies in cross-groups studies has been suggested as a way of identifying systematic individual differences both within groups and across groups. One should be warned that any differences thus identified may represent chance variation. Researchers should do well to explore the meaning of the different response patterns identified, or to replicate their findings with another sample. However, the technique presented here offers a natural and inexpensive way to explore the possibility of systematic individual differences in the performance of the dependent variable. Such an exploration may help clarify the meaning of the independent variables studied, point to interesting ways in which people differ in their performance, and suggest new directions for future research.

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"The greatest error of individualistic psychology is the assumption that a person thinks. This leads to a continual search for the source of thought within the individual himself and for the reasons why he thinks in a particular way and not in any other. . . . What actually thinks within a person is not the individual himself but his social community. The source of his thinking is not within himself but is to be found in his social environment and in the very social atmosphere he 'breathes.' His mind is structured, and necessarily cannot think in any other way." (p. 268)

Ludwig Gumplowig, *Grundriss der Sociologie*, Vienna, 1885. [Provided courtesy of G. Mandler]

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# How the Smart Get Smarter\*

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To clarify my position on the issue, how the smart get smarter, I began by supposing that three or four young boys had asked me, as an educational psychologist, to teach them how to solve crossword puzzles. As I imagined it, I began by showing them a relatively simple puzzle, complete with a matrix of numbered squares and a corresponding list of clues. Acknowledging the importance of behavioral objectives and of informing the learner of them, I next explained the requirements of a solution: that letters must be placed in every open square, that the letters in every row and column segment must form words or abbreviations, that the term chosen for a given segment must bear a clear relationship to the numbered clue for that segment, and that, when two segments intersect, the common letter must be congruent with both terms. While describing these objectives, I exemplified each one by filling in two or three of the segments.

After conveying the objectives in this fashion, I continued by presenting another simplified puzzle for the students to work on as a group. I guided the learning of these aspiring puzzlers by calling their attention to the first clue, asking them what word it reminded them of and, after someone volunteered a possibility, by prompting the students to check whether the number of letters was appropriate. When it was, I let them enter the word in the segment for "1 Across." Next, I either directed the puzzlers' attention to another clue and repeated the preceding routine, or encouraged them to evaluate their answer for "1 Across" more thoroughly by checking whether it would accommodate the words called for by the clues for intersecting segments. While working through this sample puzzle, I was careful to provide informative feedback where appropriate, as well as to give positive reinforcement for correct responses.

At this point in the first session, I supplied each student with two or more additional puzzles, still relatively simple ones, to be attempted individually. As the stu-

dents worked through these puzzles, I monitored their efforts, providing prompts and feedback as necessary. When a student finished, I reviewed his solutions with him, again giving positive reinforcement and corrective feedback as appropriate.

I next imagined that as early as the end of this first session, while correcting the students' work, I noticed substantial differences in their performance. One student made few if any errors, completing his work rapidly, while another, working more slowly, made a number of errors and appeared entirely unable to supply some of the entries without extensive prompting. Reacting to the work of these two boys, I found myself thinking of the first as the "smart" one, and the second as the "slow" one.

During subsequent sessions, I offered distinctly different brands of instruction to the two students. While I praised the performance of the "smart" student and encouraged him to attempt a series of additional puzzles, graduated in difficulty to provide a challenge, I did little else in the way of "instructing" him. As for the "slow" student, I gently tried to counsel him out of the crossword-puzzle program, explaining that puzzling skill is hardly an essential of the good life and that his talents undoubtedly lay in other domains. In response, however, he protested, strongly affirming the importance of puzzling for his happiness, promising to try harder, and asking me for additional help. So it was that I decided to embark on a different course—one especially tailored to my diagnosis of his needs.

According to this diagnosis, it was inappropriate to regard the "slow" student as inherently unintelligent, or as being especially deficient in word knowledge. Instead, drawing on the principles of contemporary cognitive psychology, I concluded, with no small sense of pride in my analysis, that his problem stemmed from the lack of an effective, executive procedure for (a) attending to the relevant features of the puzzle segments and of the item clues, (b) successfully maintaining these pieces of information simultaneously active in working memory, (c) using features of the item clues as cues for searching the relevant nodes of his hierarchically organized nets of semantic memory, and (d) properly evaluating the targets uncovered by this search against the criteria being maintained in working memory. If the student had such an executive procedure, I believed, he could correctly decide for any given segment whether to terminate his search and fill in the entry or recipe or scan his semantic memory again.

Having completed this diagnosis and process analysis of the puzzle task, and being armed with estimable models of the contents and organization of semantic memory, I then devised the kind of instruction these principles seemed to imply. In short, I sought to teach the student a mental procedure to use in searching his memory to find alternative target words for puzzle clues, and in evaluating their fit with the segment and

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intersection requirements. I taught this procedure in direct connection with the student's work on numerous puzzles, fully expecting, after several such sessions, that his performance would improve so much that I would no longer need to think of him as a slow puzzler.

According to my fantasy, this instructional program succeeded, but only to a degree. When the day came for this special student and I to part ways, we both took pleasure in his accomplishments. By criterion-referenced standards he was competent, being able to solve certain specified types of puzzles within equally well-specified time requirements. He was also quite competent (judging by the progress he had made) in comparison with the low levels of performance he exhibited during our initial session together. Nevertheless, I winced at my inability to suppress the phrase that popped into my head as I watched him go on his way: "my poor slow student."

Perhaps, though, I should not judge myself so harshly. After all, I may have had reason for my reaction; the difference between the two extreme students was several times larger at the end of my instruction than it had been initially, with the "smart" student having advanced to the point of effectively solving multidimensional doublecrossics in half the rated time. Nevertheless, it was mystifying, and even somewhat embarrassing, that he had succeeded in achieving such excellence with hardly any help from me.

I believe that this example, except for occasional exaggeration, facetiousness, and technical inaccuracy, is more than merely hypothetical. I believe it embodies a general phenomenon: that the smart get smarter as a direct function of the amount of instruction imposed on slower students. A fundamental assumption in this description is that the meaning of the label "smart" is almost entirely relative. Virtually all students make progress and, therefore, get smarter in an absolute sense. But, in a comparative sense (the sense of the term that I believe has the largest impact on our perception of student differences), the smart get smarter whenever their apparent rate of progress is faster than that of "slow" or, even, "average" students.

To the extent that this analysis has merit, I believe it raises issues of legitimate concern to educators and psychologists alike. The purpose of this paper is to examine one of these issues: the question of whether the smart get smarter, at a relatively fast rate, precisely because they receive comparatively *little* instruction. Initially, this hypothesis will be elaborated with reference to the crossword-puzzle example, and then related to other examples from both psychological research and education. Then, on the assumption that helpful instruction can, in fact, be harmful, an attempt will be made to trace such instruction to one of its roots, that is, psychological theory and research. Finally, after considering an example from contemporary psychology, the implications of the hypothesis will be discussed with

reference to one of the possible services psychology might perform for society.

### **The Potential for Harm in Helpful Instruction**

Surely it is true that few endeavors are more well-intended than that of education. For example, as I imagined, the steps to take in helping children learn to solve crossword puzzles, my goal was to assist each child in achieving his own objective. Furthermore, my efforts toward this goal were guided by an appreciation of contemporary psychological models of memory and problem solving. By design, then, as well as intent, my instruction was created to be helpful. If so, how could such benign instruction result in harm for the slow student? In what sense would a smart student be lucky to elude it?

One way of answering these questions is to draw on conceptions about the ways individual differences and instructional differences can combine to form aptitude-treatment interactions (ATIs). As Cronbach and Snow (1977), among others, have urged, instruction that might assist some persons to learn can, nevertheless, be of no value or even be an impediment to others. An alternative interpretation is even more straightforward. According to this second view, the available instruction was equally useful to all students, and differences in their rates of progress reflect enduring ability differences among them. Stable differences such as these might be expected to persist despite exposure to even the best possible instruction (Jensen, 1973).

While I believe that each of these interpretations has merit, I hesitate to adopt them, for in doing so I would obviate the need to consider still another alternative. This third possibility is expressed in the hypothesis that in some major way my instruction was positively misleading. If so, any student, to the extent that he or she followed my teachings, would be hindered from attaining the proficiency necessary for successful performance.

In the crossword-puzzle example, according to this hypothesis, the misleading aspect of my instruction centered on the mental procedure I offered the slow student as a solution for the difficulties he was encountering. My rationale for presuming to save the student began with the premise that I knew what the smart student was doing, mentally, to advance so rapidly in puzzling proficiency. This conviction was all the more persuasive in that I could formulate my conjecture in terms of highly regarded models of human memory and information processing. Given the plausibility of this premise, it seemed to follow that the only requisite for accelerating the progress of the slow student was that of inducing him to adopt the procedure I recommended. Toward this end my efforts were unstinting. I prompted the slow student to follow the procedure and monitored his conformance with it so effectively that he had little chance to deviate. Thus, if my conjecture was correct,



the slow student—and I—should have succeeded.

But what if my conjecture was wrong? What if the smart student proceeded by a method other than analysis of features of the item clues and use of these features in systematic searches of semantic memory? What if the procedure followed by the smart student was incompatible with the procedure I imposed on the slow student? What if it is only a fiction that features can be used to search semantic memory? If so, the more effective my instruction in inducing the slow student to adopt the recommended procedure, the greater its potential for retarding his progress. At least in principle, then, instruction that is indisputably well-intentioned can nevertheless set a student on a dead-end road.

### **Well-Intentioned Instruction in Practice**

By itself, however, the idea that instruction might be harmful “in principle” hardly justifies active concern, as long as this potential is rarely, if ever, realized in practice. The question, then, is whether there are any genuine instances of apparently helpful instruction that have proven to be harmful.

Illustrations from psychological research on memory development suggest that the answer is yes. Hagen and Kingsley (1968), and Hagen, Meacham, and Mesibov (1970), for example, tried to isolate the factor or factors responsible for the superiority of older to younger children in remembering the serial order of pictures in an array. Surmising that this difference might be due to a stronger tendency on the part of the older than of the younger students to label pictures to be remembered, the investigators drew samples of students at several different age-levels. In trying to learn the order in which pictures of common entities were exposed, the students were either left to their own devices or were instructed to say the names of the pictures aloud. While among the 4-year-olds the effect of these well-intentioned instructions was nil, among college students this presumably helpful hint actually produced a decrement in performance. Thus, in a research setting at least, instruction that promises to benefit students has been shown either to afford no help or to be an actual impediment to success.

Nevertheless, the fact that we psychologists have occasionally been misguided in offering instruction to our subjects is little cause for alarm, since our eccentricities are well known. The more important question is whether comparable misdemeanors are committed in genuine educational settings, such as our public schools, for example. The answer to this question, of course, depends on one's interpretation. In my view, the answer is “yes.”

I have little hard evidence to support this answer, but I can relate some informal observations that I find persuasive. At dinner recently, Bret, one of our children, excitedly described a method he was using to solve certain arithmetic problems in his elementary-school math

class. Apparently, Bret's teacher often presented problems similar to this one: What number would you need to add to five in order to have nine? Bret's solution procedure was to start with the smaller of the two given numbers and, using his fingers as counters, to increment its value by ones until he reached the larger number. Then a simple count of the number of fingers he had extended provided the solution. After making his “counting-up” procedure clear to me, Bret proudly announced that it never failed.

Impressed by the evident pleasure Bret felt in his achievement, I asked whether he had explained his special method to the teacher. Horrified by my naiveté, Bret exclaimed that he followed the procedure secretly, his hands beneath his desk, because the teacher would not allow the use of fingers in counting. Instead of Bret's procedure, the teacher had urged him to use an alternative, one which he found both unreliable and so incomprehensible that he could not describe it to me.

Bret's experience is not unique. In California, the standard curriculum for teaching how to solve problems of the kind just mentioned—sometimes called missing addend problems—forswears procedures that rely on the use of counters. Not incidentally, this prohibition survives despite the fact that cashiers in banks and stores throughout the state rely on just such procedures every time they make change. The practice of forbidding students to use this counting-up procedure seems even more onerous in view of experimental demonstrations by Case (1978) and Gold (1978) that it greatly facilitates the attainment of performance criteria on missing addend and other subtraction problems.

An analysis of the preceding examples in terms of the present hypothesis about how the smart get smarter reveals some recurring features. The instruction offered to students often arises from conceptions, either explicit or implicit, of the characteristics that define able human beings, those who are intelligent, or bright, or adept or quick, in short, those who are smart. These conceptions include assumptions about what the smart know, how their knowledge is mentally represented and organized, and about how they acquire and learn it. Such assumptions about the defining characteristics of able human beings, then, serve as guides in the design of instruction. Students regarded as being able receive only minimal doses of this instruction and often achieve success without adhering to its recommendations. In contrast, when instruction is designed to induce the characteristics of smartness, we prescribe a heavy dose for those regarded as being less-than-able, in well-intentioned efforts to heighten their proficiency.

Ostensibly, such a policy for designing and dispensing instruction seems both rational and benign, and it might well be, except for a fatal flaw. The flaw is that the root conceptions of what it takes to be smart are often unsupported and may even be fallacious; that is, the success of the smart student may stem from factors altogether dif-

ferent from those specified by our conceptions.

In my view, we, along with professional educators, are far too ignorant of the psychological structures and procedures involved in most specific instances of learning and performance to justify restrictively prescriptive instruction in any but a few isolated cases. If this is so, by insisting that less-able students suffer our instruction, we impede rather than facilitate their progress, convincing both us and the students themselves that they cannot acquire what it takes for success. In this sense, then, conceptions of smartness can serve in a capacity similar to that of bad myths, leading to the creation of misguided instructional policy.

### Is Psychology a Source of Smartness Myths?

But, even if the present hypothesis has merit, why should psychologists take any responsibility for the educational effects of smartness myths? After all, our field is by no means the sole supplier of instructional prescriptions for education. Surely the field of mathematics, for example, must be at least as responsible as psychology for the premium placed on abstractness in curricula for elementary arithmetic, to say nothing of college-level texts for instruction in calculus and probability theory. Yet, it can hardly be doubted that psychology is a heavy contributor to one class of instructional prescriptions, those that arise from myths about smartness. Over the years, psychological theories have proven to be a rich source of assumptions about the factors involved in human intelligence. From the theories of Galton, Binet, Thorndike, Watson, and Kohler to those of Jensen, Cattell, Skinner, Piaget, and Bruner, psychology has furnished numerous conceptions of what it is to be smart and of what it takes to get there.

During any given time period, within the field of psychology, one or a few conceptions tend to dominate the others in the sense of having more adherents and more influence. Such periods of dominance, because they are temporary and because critiques continue to be made by tenacious adherents of less favored viewpoints, may have little ill effect *within* psychology. But, if psychologists export a currently dominant conception to education, the opposing critique is often left behind and a new smartness myth is born. In the world of education, it appears, the fashionable can be nearly as tyrannical as in the world of fashion itself.

At the moment, I am especially concerned about a smartness myth that draws at least a portion of its legitimacy from psychology. The noted writer, Joan Didion (1977), recently expressed this myth in a statement of her feelings of inadequacy as an intellectual:

I am not a scholar. I am not in the least an intellectual, which is not to say that when I hear the word 'intellectual' I reach for my gun, but only to say that I do not think in abstracts. During the years when I was an undergraduate at Berkeley I tried, with a kind of hopeless, late-adolescent energy, to buy some temporary visa into

the world of ideas, to forge for myself a mind that could deal with the abstract.

In short, I tried to think. I failed. My attention veered inexorably back to the specific, to the tangible, to what was generally considered, by everyone I knew then and for that matter have known since, the peripheral. During those years I was traveling on what I knew to be a very shaky passport, forged papers: I knew that I was no legitimate resident in any world of ideas. I knew I couldn't think.... (pp.3-4)

The conception of smartness that worries me currently, then, is the myth of abstract thought. In it, genuine thinking is the manipulation of abstractions and the avoidance of tangible reference, and intellectual competence is the capacity to think without the crutch of concrete experience. In terms that are general, though not, I hope, abstract, the problem with this smartness myth is that it leads to proclamations of the virtues of abstract thought in the absence of persuasive evidence that such thought is necessary, much less sufficient, for intellectual achievement.

Not only do I believe that the myth of abstract thought is problematical, I also believe that it derives some of its legitimacy from scientific psychology. In my view, this connection stems, at least in part, from an otherwise progressive shift in psychology from a dominance of behavioristic to a dominance of cognitive views of mental life. While Piagetian theory, for example, explicitly traces mental structures to their origins in concrete interactions, even in this approach, the capacity for reflective abstraction is the goal of intellectual maturity, and this despite the evident rarity of persons who consistently exhibit a reliance on formal operations.

In my area of special interest—learning, memory and its development—there has been a marked shift in the last 15 or 20 years from more associative to more cognitive views. Here, as elsewhere, I think the change is largely progressive in its effects. Surely there is more involved in human learning and memory than the repetitions of more or less fortuitous contiguities. Nevertheless, I sometimes fear that the conception of memory as an abstract, hierarchically-organized system of features and propositions will totally obscure its roots in concrete experience or that, by innuendo, the autobiographically-tainted episodic memory will be relegated to unimportance in comparison with the experience-free semantic memory (Tulving, 1972).

If the emphasis on abstractness of thought, deriving partly from cognitive psychology, were imported, without interpretation, into education, the consequences could be quite unfortunate. Moreover, the effects might be especially pronounced on those students regarded as being less than smart. Imagine that the situation in education were to shape up as a mixture of three factors. One of these would be a continuation of the long-standing tendency to regard a capacity for abstract thought as an essential hallmark of the intellectually competent student. Supporting this tendency would be a second fac-

tor, the message, exported in the raw from cognitive psychology, that truly-effective learning involves the acquisition of abstractions, and that efficient human memory requires their organization into even more abstract hierarchies. Another factor, the third ingredient in this stew, could be the insistence of the public and of certain educators as well that schooling should get back to basics.

Even if research psychologists were to accept this gross overstatement and oversimplification of the importance of abstractness, by training and temperament they would be cautious in drawing direct implications for instructional practice. In the practical world of the politics of educational practice, however, distinctions that might be essential to psychologists are often regarded as ignorable niceties, or as attempts to evade real issues and to delay the implementation of necessary reforms. Accordingly, the mixture of factors I have imagined (the three-ingredient stew) could have a dramatic result in educational circles, namely, the installation of almost entirely untested instructional policies and procedures. Students might be required to spend virtually all of their time in attempts to perfect the basic skills of reading, writing, and computation. They would be expected to acquire these skills, and the information that supports them, in the form of abstract principles. They would be offered instruction entirely divorced from naturally occurring contexts of communication and calculation. And, their success in this curriculum would be evaluated by test instruments that themselves would require performances divorced from context. Under these conditions, from the perspective of my present hypothesis, the smart should indeed get smarter.

### Psychology as a Corrective for Smartness Myths

What might be done to counteract the potentially harmful effects of smartness myths such as that of abstract thought or, for that matter, that of concrete thought? From my perspective, one answer to this question is clear and straightforward: build competing theories that are as cogent and persuasive as possible, and subject them to stringent and demanding experimental tests. But I think we must go even further. We must fulfill an even more traditional role, that of the critic. One of the essential functions of scholarship is that of subjecting ideas, policies, and practices to searching critical analysis. In the case of smartness myths, this function obliges us to formulate aggressive critiques, not only of educational policy and practice, but of the presumed implications of currently dominant psychological theories as well. With reference to such theories, our role would be: to reveal their weaknesses, to deflate any exaggerated claims that might be made in their behalf, and to increase the visibility of alternative theories that might currently be out of favor in psychology itself. In short, as a professional community we should be the gadflies—not only for our own field, but

for society as well. For, in my view, society could come to treasure criticism itself, especially criticism that serves to debunk misleading myths and, thereby, to assist us all, rather than just the smart, to get smarter.

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*"It is undeniable that 'intuition about linguistic form' is very useful to the investigator of linguistic form (i.e. grammar). It is also quite clear that the major goal of grammatical theory is to replace this obscure reliance on intuition by some rigorous and objective approach. There is, however, little evidence that 'intuition about meaning' is at all useful in the actual investigation of linguistic form. . . 'intuition about linguistic form' and 'intuition about linguistic meaning,' two terms that have in common only their vagueness and their undesireability in linguistic theory."*

N. Chomsky, 1957

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**Scollon, Ron, and Scollon, S.B.K.** *The Literate Two-Year-Old: The Fictionalization of Self*. Unpublished paper, Alaska Native Language Center, 1979.

In their article in this issue of the *Newsletter*, Ron and Suzanne Scollon refer to research based upon the observation of their daughter (R) which led them to conclude that although at two years old R was not yet able to read or write, she was "literate" because she had been socialized into interacting in a focused way when engaging in literate activities. "The Literate Two-Year-Old" details the observations which led them to that conclusion; it characterizes R's orientation to literacy and suggests the ways in which the Scollons, without conscious intention, socialized R for literacy.

The authors worked among the people of Ft. Chipewyan, Alberta. In this culturally different setting they came to see their daughter's literacy-related behavior in a new way. In relation to that community, R's typification of literacy was both different and inappropriate.

In Ft. Chipewyan literacy has been strongly associated with religious contexts and is socially located in the church (and to a lesser extent in the school) rather than in the home. Books are generally prayer or song books. One of the consequences of this association of literacy with religion is that the people generally regard themselves as readers (or singers) but not as writers. (After all, one could hardly presume to have the right to create the word of God.) Thus, the emphasis is on learning to read and repeat in liturgical fashion rather than on composing. The relationship of the Ft. Chipewyans to books, then, is unidirectional; the text is dominant, the reader is subordinate. There is also a tendency to see reading as appropriate mainly for adults.

The Ft. Chipewyans' orientation to literacy contrasts markedly with that of R who acted as if literacy involved both reading and writing, seemed to see literacy as appropriate to herself as well as to adults, and participated in literacy activities by reading aloud and making elaborate displays of text as well as by listening to older people read. For her literacy was a natural part of the home environment.

Thus, the idea of what activities and behaviors constitute literacy, the distribution of literacy among social roles, the situations in which literacy is seen to be important, and the set of values associated with literacy, among other things, were different for the Scollons and R than they were for the Ft. Chipewyans.

One of the most intriguing parts of this paper is the section in which the authors discuss how they "taught" R her orientation to literacy. They propose that there are

two areas of decontextualization which are central to written language. The first is an information structure which is high in new information, and the second is the fictionalization of self, author, and audience. The Scollons contend that, in their everyday interactions with R, through the process of vertical construction,\* they were constantly pushing for an upgrading of information on R's part, thus giving her practice with the information structures which she would find in written language. Furthermore, they cite examples of how their interactions with R in story-telling situations and their comments on ongoing activities gave her the opportunity to fictionalize herself and others. As a result they propose that through structured interactions with R, they had, without conscious intention, prepared her for a particular kind of literacy. This socialization had been effected long before she could either read or write.

The Scollons compare R's typification of literacy with that of one of the Ft. Chipewyan children (OS) in a careful analysis of stories told and written (in R's case "written") by the two children. They demonstrate that R's stories are marked by an oral reading prosody which indicates the information structure of writing rather than speaking and that R fictionalized herself as author, audience, and character in these stories. On the other hand, the story which OS tells is characteristic of face-to-face oral performance. It is contextualized and marked by a questioning intonation pattern which invites response. In fact, OS's audience does participate in the telling of the story: reaffirming, adding information, and asking questions at various points. Furthermore, there is no fictionalization of self in OS's narrative.

OS also wrote her story. The written version differs from the oral version in that it is more compact and the overt indicators of contextualization ("you know?", "eh?") are absent. However, there is still no fictionalization of author and character. OS's main concern in her written version seems to be the preservation of a four part structure which is characteristic of Athabaskan narrative. In light of these findings the Scollons conclude that "OS's brevity in the written version represents a formalization toward the oral tradition, not toward the written as it might at first suggest" (p. 37).

The evidence is convincing that R and OS are oriented to literacy quite differently. In this paper the Scollons attribute those differences to a commitment on the part

\* See R. Scollon. *Conversations with a one-year-old: A case study of the developmental foundation of syntax*. Honolulu: University Press of Hawaii, 1976.

of the two cultures to distinctive types of literacy (Qur'anic vs. essayist). In subsequent work that position has been modified to the view that diverse literacy orientations result from pressures in the social world toward either focused or non-focused interaction; however, the bases of the findings reported in this article remain firm.

The Scollons' observations and the conclusions which are drawn from them warrant our attention; they could have important implications for both research on and the teaching of literacy.

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**Salomon, Gavriel** *Interaction of media, cognition, and learning*. San Francisco: Josey-Bass, 1979.

**Huston-Stein, Aletha, and Wright, John C.** Children and television: Effects of the medium, its content, and its form. *Journal of Research and Development in Education*, in press.

The traditional view of television as primarily an entertainment vehicle has been challenged from a variety of disciplines. Specifically, the success of such educational programs as Sesame Street and Electric Company has prompted a variety of research studies on how children learn from television. In both the works of Salomon and those of Huston-Stein and Wright, television has been viewed as an integral part of society's educational system. It is considered as a system of symbols which children learn, and which interact with their own symbolic and conceptual cognitive processes.

Huston-Stein and Wright argue that the form of television presentations, or the combinations of the audio and visual production techniques, must be learned before the child understands the intricacies of the content. For the authors, the learning of form is a function of both age and viewing experience. Experience is theorized to affect the child's ability to recognize and extract the meaningful message units from television presentations. The authors have argued that the major task faced by the child viewing television is to overcome the perceptual attractions of the wide variety of visual and audio production techniques, and to use these techniques in a syntactic and semantic fashion to structure and make sense of the television production. They assert that very young or very inexperienced viewers are likely to be overwhelmed by the perceptually salient properties of the form, and will respond in an exploratory mode, in which the perceptual salience of the environment governs information gathering. With age and familiarity

with the medium, the viewer is less bounded by the perceptual properties of the presentation and more guided in the information search by the meaning of the presentation. The form comes to be used by the viewer to structure the presentation and to offer both visual and audio cues for processing.

Salomon also addresses the issue of television form as a structure of messages. He suggests that like other major media, television is primarily a symbol system. Media symbol-systems differ, according to his analysis, in that they call for different kinds of mental activities during knowledge acquisition, thus benefiting learning at different levels of knowledge; the systems also cultivate different kinds of mental skills, thus affecting different cognitive processes. Salomon's thesis is that viewers come to learn the filmic codes used by producers, and that these codes represent or model mental transformations used during thought. Thus, the codes can either supplant a mental skill (do it for the viewer) or they can elicit such a skill (call upon an already-existent process).

Much of Salomon's book is organized around comparisons of research findings from studies conducted in Israel and the United States. The cross-cultural comparisons outlined in his work suggest that a major effect of culture may be on the definition of the televiewing task. Social organizations which stress the information-gathering approach to viewing television will develop more literate viewers, or those actively engaged in symbolic processing; however, when the viewing task is perceived as entertainment, Salomon suggests that less processing, and consequently less learning, will occur.

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**Soames, S., and Perlmutter, D.** *Syntactic Argumentation and the structure of English*. Berkeley: University of California Press, 1979.

If you don't want to know linguistics, don't read this book. You will find it dull and silly and it will confirm any suspicions you might have about linguistics being a strange thing to do in the world. On the other hand, if you do want to know linguistics also don't read the book, but do work through it. The book is an extended tutorial for novices, an excellent reference for specialists, and a resource without comparison for tutors. There are two mutually supportive motivations for the organization of the book: one, pedagogical, and the other, deriving from the nature of modern linguistics.

The pedagogical organization is that of a zone of proximal development tutorial. The organization is apparent within each specific section and over the book as a whole. Novices are presented with the task of doing something that they cannot do: develop a well-motivated linguistic analysis. The tutor, in the form of the book, gives the novice a little bit of help and chance to go further in the task. The "help" consists of pointing out things to notice, or operations that can be performed, or of doing operations or noticing that the novice is not expected to be able to do. Some of the helping cues are metacognitive and some are related to the issue of transfer, i.e., the novice is asked to reflect on similarities of problems or techniques or is guided to see the relevance of applying a previously acquired skill or bit of information. From the beginning of the book the reader has a chance to participate in the development of well-motivated linguistic analyses; by the end of the book the initiate is presented with "Some Further Issues" which call for critical and creative use of the axiomatic and empirical argumentation skills that the tutoring nurtured.

This is no workbook. While some of the problems set for the novice have solutions in the back, not all do. Sometimes the solutions that should have been arrived at are explicitly or implicitly revealed in the next problem; the existence of alternative adequate solutions is not only acknowledged but used as a part of the general tutoring process. The redundancy and provision of some solutions compensates for the rough guesses that a book tutor has to make about what the reader is expected to be able to do. (Interactive tutors, human or computer, may be better able to negotiate the accuracy of the tutor's expectations, but the redundancy and solution provision processes used in this book are useful resources for such tutors both in content and in style.) Readers get lots of signals that what they have just learned matters for what they are trying to do currently and that what they are trying to do currently counts both for what they are about to do next, and for solving problems in linguistics.

Actually solving problems in linguistics is the basis for the other organization of this book. The topics covered range from phrase structure rules to constraints on variables in transformations, from issues about rule ordering and the cycle to treatments of THERE-insertion and Raising controversies. The history of and debate about basic axioms is exposed. The most important empirical findings in the last twenty years are dealt with and each topic concludes with a carefully prepared small chapter on the original source articles and other related views of the issue. There is a coherent viewpoint about the nature of language, and about an adequate analysis of structures of English; there are also honest statements about what is not known in the field and an avoidance of polemics. Time and space are not wasted on exercise for exercise sake. It is only with a zone of

proximal development technique that so many hard topics could be dealt with in such detail and so honestly and so coherently. By working with the reader from the beginning, Perlmutter and Soames teach both argumentation and modern syntactic theory; the remaining controversies and gaps in knowledge can safely be left in the hands of the reader who at the end knows linguistics.

The reader is respected, the field is respected, and pedagogy is respected. If the authors could be persuaded to write a similar zone-of-proximal-development-text on how to write a text, much of academia would probably profit.

Peg Griffin

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*"The description of movement cannot  
be restricted to Euclidian-Descartian geometry;  
movement isn't rectilinear, but swings like  
a spider's web in the wind."*

N.A. Bernshtein

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### **Announcement**

The Child Development Film Archives at The University of Akron has approximately 3,500 films, made for a variety of research purposes, which it makes available to scholars. The two largest collections were developed by Arnold Gessell and L. Joseph Stone; footage taken for Margaret Mahler is also included. An inventory sheet is available for each of the films and footage portraying the following topics can be identified: *child(ren)*—the number, sex, age, ethnicity, amount and nature of clothing, biopathology and psychopathology; *adult(s)*—the number, sex, and interaction with child(ren); *animals*—the number and species; *specific activities of child(ren) and adult(s)*; *the milieu*—laboratory, school room, museum, etc.; as well as *the equipment-laboratory appurtenances*.

For more information and a descriptive brochure:  
Child Development Film Archives  
The University of Akron  
Akron, Ohio 44325  
(216) 375-7285

# Second Annual Meeting of the Cognitive Science Society

June 16 - 19, 1980  
Yale University,  
New Haven, Connecticut

## Conference Information

An international conference sponsored by the Cognitive Science Society will begin on Monday night, June 16, and run through Thursday morning, June 19, on the campus of Yale University.

Dormitory accommodations are available to registered participants as well as optional meal plans. Rooms in nearby hotels are also available but reservations should be made as soon as possible to ensure a place.

The conference will include major addresses, panel discussions, and short papers authored or sponsored by members of the Cognitive Science Society.

## General Registration

Members \$25 (after May 15 — \$30)

Non-Member \$30 (after May 15 — \$35)

## Student Registration

Member \$20 (after May 15 — \$25)

Non-Member \$25 (after May 15 — \$30)

There will be a banquet on Wed. June 18. If you would like to attend, please indicate this on the registration form and include an additional \$10.00 for each person attending.

## Invited Speakers

There will be four major addresses, representing the fields of anthropology, linguistics, and psychology.

Roy D'Andrade  
Department of Anthropology  
University of California, San Diego  
"The Cultural Part of Cognition"

Charles Fillmore  
Department of Linguistics  
University of California, Berkeley  
"Some Problems with Ungenerated Language"

John Ross  
Department of Linguistics  
Massachusetts Institute of Technology  
"The Geography of Conceptual Space"

Thomas Bever  
Department of Psychology  
Columbia University  
"Comparative Cognitive Science  
in Humans and Animals"

Three panel discussions will be organized around three issues of interest to future cognitive science research.

## Coherence in Conversation

Chairman: Roger Schank  
Gene Charniak  
Wendy Lehnert  
Ray Perault  
Emmanuel Schegloff

## Mental Imagery and Reading

Chairman: Allan Collins  
Robert Abelson  
Wallace Chafe  
Stephen Kosslyn  
Allan Lesgold

## Cognitive Science Methodology

Chairman: Earl Hunt  
William Brewer  
Edward Feigenbaum  
Walter Kintsch  
Marvin Minsky

## For additional information contact:

Ms. Patti Oronzo  
Yale University  
Computer Science Dept.  
P. O. Box 2158, YS  
New Haven, CT 06520  
(203) 436-0606

## Related Meeting:

The Annual meeting of the Association of Computational Linguistics will be held June 19-22 on the campus of the University of Pennsylvania. Language related papers in the Cognitive Science conference will not be scheduled on June 19 to allow for joint attendance at the two conferences. For more information:

Gary Hendrix  
Artificial Intelligence Center:  
SRI International  
Menlo Park, CA 94025  
(415) 326-6200, ext. 4664

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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.

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