QUALITATIVE/QUANTITATIVE RESEARCH METHODOLOGIES IN EDUCATION: A LINGUISTIC PERSPECTIVE[1]

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The study of language has a special role in discussions of qualitative methodology by anthropologists. In Section (I) the origins of this role are sketched, and in (II) some consequences and implications. The principal implication is the need for a study of language that is equivalent to linguistic ethnography, addressed to institutions of our own society, such as education. This need, the possibility that change in this regard is up against deeply embedded cultural views of language, and the study of assessment of language development outside of schools are touched on in (III). (IV) takes up the characterization of the kind of ethnography that is intended, stressing a distinction between "ethnography" and "field work," a conception of linguistic inquiry as generically the interpretation of codes, and a conception of ethnography as the discovery and interpretation of cultural worlds. (V) brings the discussion of ethnography and the preceding discussion of linguistic methodology together. (VI) adds reflections on uses of language by anthropologists. The possible democratic implications of one use of language in anthropology are also suggested. ETHNOGRAPHY, LINGUISTICS, SOCIO-LINGUISTICS, ANTHROPOLOGY, STRUCTURAL METHODS.

The study of language has a special role to play when one seeks to come to terms with the relation between quantitative and qualitative methods. The rise of linguistics in this century as an autonomous discipline is based on the discovery of a qualitative methodology. The success of linguists in discovering relationships that are capable of rigorous formulation, of patent reliability and validity, without recourse to numbers, has stood as an object lesson. It is an object lesson that has been heeded most of all in anthropology, where it is familiar in writings of Sapir, Kluckhohn, Lévi-Strauss, Goodenough, Lounsbury, Frake, and others, and has spawned a series of special approaches and debates. (One can mention componential analysis, ethnoscience, structural analysis of myth, paralinguistics and kinesics, and various forms of semiotics.) For whatever reason, this import of linguistics has not been particularly discussed in educational anthropology. I should like to sketch its history and present standing, so as to indicate both the value and the limitations of the perspective it brings. Linguistics is increasingly being extended today through attention to social context and use. Such attention entails ethnography, and I will end by trying to say how the linguistics and the ethnography fit.

I

A few dates and historical reference points are needed. Most people may not realize that there were no departments of linguistics in this country before the Second World War. The professional association of linguists, the Linguistic Society of America, is only 51 years old, roughly half the age of the major social science associations founded in the latter part of the preceding century. Fifty years ago what we consider the study of language was mostly the study of individual languages and language families, Indo-European having pride of place. Study of general linguistics, and study of Indo-European languages, as its foundation, were often considered equivalent. In the 1930s the introductory courses at the first Linguistic Institutes sponsored by the new Linguistic Society focused on Indo-European languages.

There were of course students of language in general. But if one sought a career in the study of language, one pretty much had to choose between becoming a specialist in the languages and literatures of some major language group of European fount, or becoming an anthropologist who could write down languages mostly unwritten. All this changed and changed dramatically. It changed in connection with the exploitation of a little-noticed gap in the existing academic citadel: the sounds of language. The study of speech sounds was hitherto either taken up within individual languages, language families (Romance, Germanic, Slavic), or taken up as an aspect of psychophysics, of phonetics as a distinct physical science. For the former purpose, the analysis was specific to the languages in question. For the latter purpose, one sought exactness of measurement. For many students of language, the two activities were wholly separate categories.

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To study languages was a human (or "moral" or "mental") science: a *Geisteswissenschaft*. To study speech sounds, those physical phenomena, was something altogether different: a *Naturwissenschaft*.

What happened in the 1920s and 1930s was that the men we now revere as founders of the discipline of linguistics—men such as Edward Sapir and Leonard Bloomfield in the United States—created a methodology, a qualitative methodology, which on the one hand generalized the insights into particular patterns of speech sound from the study of particular languages, and on the other hand transcended the phonetics of pure physical measurement. What they accomplished is loosely called the discovery of the concept of the phoneme and the creation of phonology as an entirely general science of the systematic properties of the dimension of language having to do with sounds. What they did, in effect, was to integrate the study of sounds in general into a *Geisteswissenschaft* of language study by replacing one conception of rigor with another. Rigor of measurement was replaced by rigor of functional contrast.

The classical locus of this discovery in American linguistics is the 1925 paper by Sapir, "Sound patterns of language." From that paper has flowed not only much of the development of methodology in linguistics, but also much of the effect that linguistics has had on conceptions of methodology for the study of behavior more generally (see Hymes 1970).

Sapir's essential point was the distinction between a physical event and an element in a system of signs. The distinction was dramatized by consideration of cases in which one and the same physical feature could have entirely different significance, depending first of all on whether or not it was an element in a system of signs, and, if it was, then on the relations into which it entered in the particular system of signs to which it belonged. As to the first, Sapir considered the difference between a breath through pursed lips to extinguish a candle, and such a breath as the beginning of an English word such as "when" (when pronounced in the standard form with the aspiration (hwen)).[2] The initial breath of the English word can distinguish forms within the language (hwen : wen (of the skin)), can be an object of attention as a difference (sometimes stigmatized) between styles of speech with regard to the adverb "when" itself, and makes possible exact or approximate puns (as in commenting on someone's reiterated "When?" with "This must be W(h)ensday."). More strikingly for the subsequent development of phonology, linguistics, and anthropology, Sapir compared hypothetical inventories of sounds for whole languages. Two languages might have identical inventories of sounds, according to observations of physical properties; yet when the functional relations among the sounds within the system of the language were considered, the two languages might be found to have quite different patterns, configurations, or structures of elements. In each, for example, one might hear both "p" and "b," "t" and "d," "k" and "g." In the one, such a minimal difference might be functional, serving to distinguish words by itself. A word beginning with (p) would be a different word from one otherwise the same but beginning with (b), and so on. In the second language, the difference between the two types of sound might not be functionally relevant. It might be a predictable alternation ((b) perhaps occurring always between vowels, and (p) never). In the second language there would be, from the standpoint of functional relevance, just one series of stops that could best be written /p t k/ (since it is the "voiced" sounds, b, d, g, that are predictable from their environment). In the first language, there would be two series, /p t k/ and /b d g/. To repeat, the difference between the languages would lie, not in the presence or absence of observed sounds, but in the status of the observed sounds within the system of the language. And the principle that determines the status is qualitative, an all-or-nothing principle that leads to invariant, fixed reference points. From this perspective, there is not such a thing as more or less of such a unit. There is rigor in the work, and a branch of formal scientific inquiry to which to appeal, but it is qualitative and discrete mathematics, not statistics or experimental measurement.

Sapir went on to complete the picture by considering two inventories that were different as observed sets of sounds but identical, once analyzed in terms of functional relevance, as elements within a system of mutually contrasting points in a pattern. A brief illustration: one language might have (p t k) as stops, all functionally relevant. A second language might have (p t k) and (b d g) as well, but, like the second language in the previous example, no relevant distinction between the two series. In sum, there would be three observed stops in the one language, six in the other, but just three systemically relevant units in each.

Some linguists resisted the development of phonology, feeling that it began to leave behind the concrete realities of the sounds of language. For to the principle of contrastive relevance (often called the principle of commutation), was added concern with symmetry and simplicity of the systems disclosed, and concern with elegant solutions to the sometimes complex consequences of tension between the phonological and the other sectors of a language. Sound patterns of languages are subject to the strains of historical change and communicative specialization, to the sometimes contrary pushes and pulls of external adequacy + internal economy, with grammatical and lexical considerations sometimes taking priority. Logical models invite a conception of a language as a monolithic system, with the meaning at one end and sounds or letters at the other, but history and comparative perspective quickly show that a more adequate conception is one of languages as composed of interconnected major sectors, somewhat like interconnected continental shelves whose occasional displacements can create untidy interfaces. The interrelation between phonology and the rest of a language is often one such untidy interface. In some languages the interrelation can be specified with few detours and only occasional mounds and valleys, whereas in others it is rather as if a mountain range had been thrown up.

II

The point of this extended analogy is that language is not as neat as linguists sometimes make it out to be. Herein lies the limitation of the lesson from linguistics. Any

consideration of qualitative methodology in the study of human life must take into account the success of linguistics in establishing a domain of study, central to human life, that has a methodology that is at once qualitative and rigorous. But our consideration cannot leave matters there. The student armed with qualitative methodology can be just as a priori in assumption, just as prone to overlook disquieting empirical facts, just as heavy-handed in the service of his methodological god as can the quantitative researcher of fabled evil. In short, the success of linguistics is often appealed to, and rightly so, as evidence that quantitative methodologies are not sufficient, not the only model of rigorous science, in the human sphere. That lesson is a crucial lesson. There is a tendency perhaps for the sophisticated statistician or sensitive experimentalist to believe that all methodologies ultimately reduce to his. Qualitative insight and observation may be given great scope, mindless counting deplored, but still the belief is that the final test comes with the quantitative or experimental design. This belief is unfounded, and linguistics shows it to be so. At the same time it is essential not to fall into the trap of believing that the foundations of linguistics as presently practiced are adequate and secure, such that quantitative measurement and experimental design can only complement and come after findings obtained by other means. This belief is the inverse of the other, and it is prevalent in American linguistics. It is equally unfounded.

The history of psycholinguistic research since the early 1940s shows the truth of the matter. At first in the study of the acquisition of language it was necessary to learn what in fact was acquired. Gradually psychologists interested in child development and language acquisition became knowledgeable about phonological and morphological units. The work of Roger Brown and numerous associates is noteworthy here. Once the rule-governed nature of language was utilized in planning research, one could investigate the presence or absence, and the stages of acquisition, of the specific rules. One could go beyond gross measures of length of utterance and the like to specific properties of the system concerned. It is rather like being able to go beyond comparisons of motors in terms of external properties such as size or color, to analyses of engines in terms of internal properties such as combustion pressure or piston rate.

When the course of modern linguistics reached syntax (having started out, as we have seen, with a focus on phonology), and when controversy over models of syntax was resolved effectively in favor of transformational-generative grammar, begot by Chomsky out of Harris, it seemed to some psycholinguists that almost a millenium was at hand. George Miller, who had been prominently associated with the development of information theory, became a convert—a nice example of a conversion from a quantitative to a qualitative "paradigm" (although not with loss of experimental design). Experiments based on the Chomskian model gave initially exciting results. It seemed that the grammatical model and psychological reality were twins, and the job of psychology was to devise ingenious experiments on the basis of the linguistic model. A few years later the bloom was off that particular rose. The relation between psychological reality (the mechanisms of the mind) and grammatical theory (the mechanisms of a model of grammar) came to seem increasingly remote. Indeed, a number of psychologists have come to the conclusion that experimental analysis of relations between linguistic elements is itself a primary source of knowledge. A certain command of linguistics is required in order to deal with the units of language; but where relationships among the units is in question in terms of alternative models, experimental study need not wait upon the linguist. To be sure, Chomsky has consistently maintained that other kinds of study concerned with language must wait upon the outcome of his. But that contention is increasingly ignored. We see some of the productive outcome of such independence in studies discussed by Cazden (1977) and Shuy (1977), and the use of such terms as "ecology of language," "functional linguistics," and "communicative competence."

In this history, there is a second methodological lesson from linguistics. It has to do with validity as much as does the first. The first lesson has to do with validity in the sense of structure. The second has to do with validity in the sense of function. The two are indeed interrelated. What Sapir showed with regard to phonology was that recognition of structure depended upon recognition of functional relevance. The acutest ear, the most careful design, could not take the fundamental first step in the analysis of sound patterns in language, so long as the presence of pattern was not understood to depend upon the linguist's version of experimental control, the test of commutation.[3]

The fundamental elements of a system were determined in terms, not of the relationship of sound to sound alone, but in terms of the relationship of sound to sound in the service of distinguishing units of another level (words, sentences). And internal analysis of the relationships among such elements might result in patterns that were rather different than observable patterns.

Linguists both learned and neglected this lesson in subsequent stages of their discipline. They learned it for phonology, as against phonetics, and for morphology, but many resisted it for a time when it became an issue with regard to syntax. The structure of sentences was studied in terms of similarities in the distribution of elements within sentences of the same type. Sentences such as "John is easy to please" and "John is eager to please" would be seen as sentences of the same type, and "easy" and "eager" as words of the same type. They contrasted as words, of course (another instance of form/meaning covariation), but not in terms of grammatical function. Chomsky's view, crudely put, was that fundamental syntactic structure depended upon recognition of functional relevance at a further level. This level was discernible when sentences of a different type and different in overt pattern were seen to be related, sharing invariant sets of grammatical functions and derivable from one another, or from a common base, by regular rules, and seemingly similar sentences and words to be different by the same token. "John is easy to please," "It is easy to please John," "Pleasing John is easy," show a common core of meaning and functional relationship among the elements "John," "please," and "easy." And "John is easy to please" no longer appears the simple analogue of "John is eager to please," when the same commutation test across that set of sentence types yields unacceptable sequences, *"It is eager to please John,"

*"Pleasing John is eager." (The asterisk marks the unacceptable sequences.)

It is fair to see here a parallel to the lesson Sapir taught in "Sound Patterns of Language." A major characteristic of the syntactic work inspired by Chomsky was that seeming diversities among sentences were found to have an underlying unity, and seeming likenesses an underlying difference.

Having established this lesson in syntax, Chomsky was to be confronted by students who insisted on applying it again in semantics. Syntactic relationships that were clear and distinct according to his model came to seem not so to them, when viewed from the standpoint of semantic relationships. The dispute between those insisting on the primacy of syntactic relationships and those insisting on the primacy of semantic relations continues. And Chomsky, having established syntax to his own satisfaction as the core of language, insisted that studies of use, of styles and such, was as dependent on the results of syntax, as any other study of language. But this is a partial truth. To be sure, as Cazden points out (quoting Crystal), one must attend to the specific units of language or one will not see any relationships at all (just as ignorance of the speech sounds of a foreign language will yield a sense of noise, not of phonology). But the relationships that are there will not all come into view if one stays at a given level. Each functional sector or level of language organizes units in a way not given by the units themselves. To use an old example of mine, the functional category of greetings may range from single morphemes to complex sentences, from "Hi" to "Well, I'll be a son of a gun, if it isn't Sid Mintz" (Hymes 1964). Nothing in syntactic analysis itself would bring these two together. One has to start with the category of greeting itself, and discover what elements and relationships among elements may serve it. Shuy's studies of functional language illustrate this principle in their examples of alternative ways to accomplish requests, directions, instructions, and the like.

We are almost to ethnography now, but not quite. In their recent papers on assessing language development both Cazden (1977) and Shuy (1977) point out the need for ethnography, implicitly at least. Cazden asks, how does one decide what communication functions are of the most worth, and where does the list of communicative competencies end? If an answer is not to be imposed a priori, ethnographic inquiry into the communicative repertoire of a community is essential. Again, Shuy, using extended observation and videotaping in a school setting, can recognize functions and probe them experimentally because of consonance with his own cultural knowledge. I shall try to indicate the character of a fully ethnographic approach below, but first let me finish the path begun with linguistics.

The path so far described for the course of linguistic methodology is step-wise. A level of functional relevance is recognized, the step of structure dependent upon it analyzed; then something of a kick and a leap must occur to move the field as a whole to the next step, so easily does the student of language become immersed in familiar form. The leap now before the field, though continuous with the rest, produces in many a sense of falling outside linguistics itself. It is the leap to the study of the relationships among linguistic elements in the service of speech styles.[4]

From the standpoint of a speech style, one has to do, not with an additional level of language, possessing an additional set of units, parallel to phonemes, morphemes, syntactic constructions, semantic features. True, the term "styleme" has been used, but it can really only refer to units (or co-occurring units) already identified and seen, from the standpoint of style, to be characteristic or expressive. With styles, one has to do with a novel organization of units at perhaps all the standard levels. What distinguishes a formal style, say, from an informal style, may have to do with pronunciation, choice of words, choice of syntactic construction, and preferred and inadmissible meanings. A style is more a configuration than a level. And the elements of a style may differ in scope from those of levels such as phonology and syntax. It is possible to give a sense of an "archaic" or "archaicizing" style (seriously or humorously) by occasional use of a few salient features—say a "thee" and a "thou," a "natheless" and a "howbeit," a syntactic inversion or two. The rest of what occurs may be indifferent.

In sum, the difference of a style, as a configuration, from a structural level of language is this. In phonology one has to do with elements and relationships that are exhaustive of sentences in one of their aspects. All of a sentence (or a discourse) can be represented as a sequence of phonological units, mapped in terms of phonological units. The same is true for morphological units, syntactic units, and semantic units. Indeed, when linguists speak of their subject matter as having to do with the relation between sound and meaning, with the mapping of the intervening structures, it is the exhaustive kind of level that is thought of as intervening. There is "total accountability," so to speak, for the linguistically relevant features at each level. Styles need not be like that. To be sure, they can be. In the Yana language spoken by Ishi, of whom Theodora Kroeber has written so well, men's speech and women's speech were distinguished in the phonological ending of every word. But the differences between men's and women's speech styles generally in American society are not evident in every word. Such gender-linked styles are indeed superb evidence of the need for a functional starting point. They entail differences that appear only when one sets out to discover them, starting from men and women, rather than from grammar.

We have, then, to do with language in which traits may constitute the relevant difference. And while some differences among styles may depend upon presence or absence, be all or nothing contrasts, others depend upon proportions and frequencies. (Shuy (1977) discusses some of these cases.) We recognize such phenomena when we speak loosely of someone having a "touch" of an accent or of someone having a "thick" accent, or of a high proportion of features at one end of the scale as "deep."

We have also to do with language in a respect in which it is inescapably sensitive to situation. Progress in linguistics has mainly been independent of social context, because it could be assumed that the features being analyzed were common to all users and uses of a language. That assumption is never wholly correct, and the relation between what is analyzed by a linguist as "English" and what you or I can say and understand may be very

problematic. The fundamental point here is that when we reach consideration of style, we inevitably reach consideration of styles. Even when a speaker of a language can be thought of as having a single grammar, he or she cannot be thought of as having a single style. When we reach consideration of styles, we must consider speakers as having, not a grammar, but a *verbal repertoire*. In some cases that repertoire may comprise more than one language. In every case the consistent continuation of the principle of functional relevance leads to the questions—What are the differences by which the styles in a speaker's repertoire can be described as contrasting? What are the dimensions underlying those differences? (What are the relations between the styles and their occasions of use?)

We have reached, in effect, a study of language that is inseparable from a study of social life, and in which quantitative differences are inseparable from qualitative effects.

III

Many linguists may say that such a study of language is not linguistics, but some other field, perhaps anthropology, psychology, sociology. Whatever its label, it is beginning to emerge into prominence, and it is the sort of study of language that is fundamental to education. From one standpoint, such a study of language may be "applied" linguistics, especially if it is concerned with language use in schools. But "applied" taken alone is a misnomer. Linguists do not now know enough about these phenomena for others to come to them to ask simply for application of knowledge already in hand. Research into these questions is not applied, but is foundational and at the frontiers of linguistics. Its practical relevance is obvious, but it is no less concerned with issues of theory for that. The plain fact is that practical needs and theoretical challenges coincide here, as they do in so many other places. And there are not enough who are taking them up.

In this regard we pay a price for the isolation of linguistic and educational research from each other, for polarization between qualitative and quantitative methodologies, for the lack of a sufficient cadre of linguistic ethnographers. Perhaps we must always pay this price. Perhaps the values that are institutionalized in our academic disciplines, our institutions and government, and our culture, are such as to prevent the growth of the work that is needed. It is so easy for modes of work to become frozen in doctrinaire niches. Language is a subject beset by prejudice and preformed opinion. In attempting to change the way in which it is studied and understood, one may unwittingly be challenging deep-set assumptions of the society. Perhaps language development is assessed as it is today, for the most part, because to do so supports the present order of things. Perhaps the vested interest of an elite in the notion that change and the masses corrupt language, and the vested interest of a highly stratified, bureaucratized social order with a democratic frosting, such that individuals must be considered to have ended up where they do as a result of their own doing, converge. Just as we would not know what to do if schools failed to keep millions of young people out of the job market, so we would not know what to do if schools succeeded in producing millions of young people with the language competence they take as an ideal. Or rather, perhaps we would indeed know what to do. If accents and dialects and vernaculars were to disappear and no longer be available as ways of discriminating, if everyone spoke standard English, we might simply substitute a finer lens in the microscope of correctness. No more "he do" for "he does?" But plenty of "transpire" for "occur," confusion of "infer" and "imply," jarring plain "impact" as a verb instead of "have an impact on." After all, a great many of the distinctions upon which we now insist came into existence only as a result of the rise of the middle classes in the eighteenth and nineteenth centuries. If new ones are needed, or more weight need be given to old ones in order to maintain the desired level of linguistic insecurity in the populace, the necessary sense that most people do not deserve better because of linguistic inadequacy, it could surely be done. (cf. Hymes ms.)

All this is speculative, of course. If it is too dramatic, my excuse must be that there seems to me an issue here that constantly eludes us, and must somehow be forced into attention. Before we can make satisfactory contributions to the assessment of language development, we must know far more than we do about the role of the assessment of language development in the history of American schooling and American society. Fortunately, a few scholars are beginning to pioneer in research in the history of our language attitudes and policies (e.g., Shirley Brice Heath, Glendon Drake). The histories I have seen give little attention to it. A broad picture is clear enough: wipe out the Indian languages, erase linguistic differences due to immigrant origin, disvalue or stereotype dialect, insist on a single standard as a badge of intellectual and personal virtue. Little seems to be known about the formation of these views in schools of education, their implementation in schools and school districts, the tensions, interactions, and adjustments in specific regions, where specific configurations of linguistic difference and verbal repertoire prevailed. One senses a pervasive difference in attitude today between groups differently situated in the class structure, a pattern of difference perhaps between the Eastern seaboard and the West, but without adequate documentation.

Perhaps we need to step back, imaginatively, in a way analogous to "zero-budgeting." What if there were no assessment of language development at all? Would anything be lost? Most of mankind in time and space, after all, has not had explicit assessment of language development of the sort with which we are concerned. Why do we? Why do we have to? How did it start? What functions, latent as well as manifest, does it serve? Is it possible that language development of children in our schools would improve if it were *not* assessed? To what extent are the functions and effects of assessment different for different children, according to region, class, sex, program?

Some essential light can be shed by knowledge of assessment of language development as it occurs outside the classroom. Insofar as parents and communities have not come to accept the figures of formal tests as the only criterion of achievement, there must remain at least residues of informal assessment according to the norms of local cultures. One would want to know what kinds of use of language are valued, which users of language are valued, how these values are exhibited, experienced, and acquired.

One would want to know how the relationship between language use in school and language use outside of school is viewed, where there is continuity, where conflict, where compartmentalization.

Research outside schools could discover evidence of abilities in community situations that might put the display of abilities in schools and test situations into perspective. It is well known that display of abilities may be tied to situations. Sometimes it is a matter of appropriate content, sometimes a matter of appropriate norms of interaction. In their study of Kpelle children, Cole and Gay found that children who had difficulty in school with lessons in mathematics that dealt with certain principles could be seen to employ these same principles in certain work situations. Susan Philips has documented the cultural pattern that underlies the "shyness" of Indian children from Warm Springs reservation, when directly questioned by teachers. The teachers perceive the children often simply as "shy," not talking. Observation of the children in play and home situations shows that they can be very talkative indeed.

Here may be an opportunity to unite research with effective change. One might start with what the teachers and the school personnel perceived as the problems associated with language development of a group of children. Given this definition of problem, as perceived in the school situation, one could undertake a study of the language activity of the children in the full round of their lives, putting the phenomena of the classroom into perspective as part of that round. One could involve the teachers as participants in the ethnographic study to a certain extent, or at least as participants in an on-going seminar in which the ethnographic inquiry was regularly reviewed and discussed. The results of the study, of course, might confirm the teachers' initial perception. If the results indicated a different interpretation of the children's language activity in school, the teachers would not be confronted with it cold. They would have participated in its development, and understand the process by which it was reached. Such involvement in the process might make acceptance of the product more likely.

IV

Let me now take up a conception of ethnography, in order to make clear what the term implies for me, as method and discipline that can be vital for educational research in language.

One hears the word "ethnography" more often these days in educational and linguistic circles, and one also begins to hear the question, "What is ethnography?" There is no single answer. Almost anything that involves direct contact with people as a source of information may find itself included under the label, especially if the contact is made by an anthropologist. The conception I sketch here is shared by some but not by all. It is intimately connected with the sketch of the development of linguistic methodology given above.

It is important to distinguish between *"ethnography"* and *"field work."* There are two related reasons for this. First, "field work" is a suitable general term for any contact with people as sources of information; second, and most important, not all "field work" in this sense is "ethnography" in the sense I intend. There are again two reasons.

First, contact, having been there, is not enough. Sometimes the claims of anthropologists to a distinct and even superior methodology embody an element of "I was there." This ought to give us pause when we reflect on how many people we met there whose views of the people we studied we would not trust. Those of us who work with Native American communities often enough meet the local resident who "knows all about the Indians" from having lived there. We often discover that what is known is limited to what Indians were willing to disclose in certain, partial contexts, or is colored and constrained by an economic or social relationship that closes off certain kinds of knowledge as uncomfortable.

If the anthropological methodology in field work is effective, it is based on more than being there, however romantic some of us may make the field work experience sound. It may, to be sure, be based on insights and intuitions, but these are nourished and controlled by a certain kind of training. It is this training that is often missed by those who have not had it, giving rise to an equation of ethnography with sheer presence in the field. Indeed, a focus on field work may easily miss the training, because the training commonly occurs apart from the field. It has to do with the systematic, comparative knowledge of phenomena and systems like those under study which the ethnographer brings to the description and interpretation of the particular case. It has to do with the knowledge that enables him or her to recognize in a funny use of words for "aunt" and "uncle" a kinship system of an Omaha, or Crow, or other type; in a problem of attendance at schools or jobs the persistence of a seasonal round; in a consistent failure to say "Thank you," not ingratitude, but a pattern of reciprocity that avoids closure in single situations.

Such inferences presuppose the skills to obtain the information from which they are inferred, and these too entail more than presence and observation. These skills involve the questions asked in the mind, if not in speech, that guide presence and observation. This brings us to the second way in which I should like to restrict the term "ethnography." I should like to give "ethnography" the connotation of inquiry that is open to questions and answers not foreseen, for which possible observations need not be precoded, and for which the test of validity need not fit within a prestructured model. When anthropologists limit their inquiry to observations and questions for which the set of alternative answers is already fixed, I should like to say that that may be field work, but not ethnography.

These two poles of validation through field work, then—"I was there" contact, precoded content—represent a Scylla and Charybdis between which true ethnography steers. The steering is not reducible to a routine; that is what makes it hard, and some seemingly ineffable at times. Sensitive awareness, empathy, and intuition are not ruled out, far from it, but merely not enough. Pre-existing models and frameworks are inseparable from the requisite training, but one must be able to get beyond them. It all comes down, unfortunately, to being attentive and smart. Being there won't allow one to sop it up; methodology won't allow one to grind it out. The steering is indeed cybernetic, a matter of feedback, of dialectical interplay, if you will. It

has to be so, because of the kinds of situations in which one works and the kinds of knowledge one seeks. One works in situations which require the trust of others, accommodation to their activities, participation in ways that often preclude writing or recording at the time. One sacrifices certain kinds of reliability for the validity that one hopes and often finds to come with depth. In a sense, one is half in the position of a child or newcomer learning the ways of the community. One has not the time and full immersion of the child or newcomer, but a measure of orientation through training that compensates. Just so in acquiring a local language: the limited opportunity to hear and use it is compensated for by a training in what to listen for and what to do with what one hears.

In a word, ethnography is inquiry that begins with recognition that one is at work in situations that are indeed, massively prestructured, but prestructured by the history and ways of those among whom one inquires. At the heart of it is a process of which linguistic inquiry is indeed a model, if we set aside any particular model of grammar, and think of linguistic inquiry in the generic sense as *the interpretation of codes*.

For the ethnographer in this sense, the world of inquiry is neither merely a source of raw data for general schemes, nor a gallery of essences that can only be intuited and expressively expressed. It is a world of many codes, of many structures. Not a single natural world, indeed, but a plurality of worlds (*Lebenswelt*); worlds that are *constituted* in the lives and experience of participants in a group or activity, in important part through selecting and grouping and reinterpreting received traditions, traditions which from the point of view of other traditions may seem unintelligible or irrational. From the standpoint of a merely universalizing or generalizing science, such traditions and worlds may seem arbitrary and parochial. Yet even a science that wishes to rise from human worlds to something called "Man," if it wishes to effect change, must reach into these worlds, be mediated by them, if change is to be consonant with intention. From the standpoint of a science dedicated to generalization and universals, the specifics of each world may seem simply boundary conditions, specific constants and ranges to which the parameters of general theory must be adjusted. From the standpoint of a science imbued strongly with a historical sensitivity, the specifics may contain qualities of emergence. To the one view, qualities that are rare or unique may seem something that can be set aside because of their infrequency. To the other view they may seem opportunities for insight, configurations that disclose hitherto unrealized and unsuspected potentialities. To take an example from language: a general theory of language can regard a specific language as an exemplification, and perhaps a test, of features of the design of language in general. Some would hold that only universal considerations are important. Others would regard a specific language from a typological point of view—remaining concerned with universal language design, but concerned as well with the recurrence of major types of structure, themselves seeming to reveal potentialities of language structure that recur independently of history, and that are not easily reducible to a single model, if the model is at all rich in content. Still others, myself included, accepting and valuing the preceding interests, would want to keep in view a third concern. Navajo is what it is because it is an instance of human language; it is an instance of certain types of language structure that have great interest; it is also what it is because it is the language of the Navajo. To a great extent its structures are what they are because of possibilities and impossibilities inherent in language structures mediated by the mind. Its flesh and blood, as it were, the meanings it has for those who use it, the texture that it takes and gives in their speech and reflection, are what they are because of the specific experience of those who have spoken and continue to speak it. What role the language can play in the modern world, in schools, is to be understood in terms of that history, valuation, and outlook. Linguists long ago, in what must seem the ancient time of Boas and Sapir, established that there is nothing intrinsic to the structure of any language that precludes its adaptation and elaboration to serve new needs of whatever kind. If there are limitations and disabilities, when a language is confronted with new circumstances, these reflect nothing about its potentiality; they reflect the fact that its vocabulary and idiom, its conventional speech acts, routines, and genres, the assumptions as to etiquette of speech, have evolved and been embedded in a certain way of life. (As is true, of course, of any language, even those which become world languages through their adaptation to the needs of commerce and science, and their association with world powers. Their near universal currency is a demographic, political, and cultural fact, not one due to any unique structural property.)

It follows that there can be field work with a language, even field work devoted to applied goals, educational goals, that falls short of ethnography. One can devise an orthography to permit the use of a language in primers and bilingual education, but knowledge of the role of writing and reading, of language in visual form, is needed, if the written material is to be used. Indeed, knowledge of the meanings associated with the alphabets and letters known to the people is necessary, if the alphabet itself is to be successful. One cannot make general assumptions about the role of literacy. One has to find out through ethnography what it means in the case in hand. In general, it is not enough to decipher the code of the language itself; one has to decipher the codes associated with the use of the language, as an element in the verbal repertoire of the community.

A brief citation from Lévi-Strauss on this role of ethnography. He once observed that if an object of art came to Paris, and the code was known, it would go to the Louvre, but if the code was not known, it would go to the Musée de l'Homme (the ethnological museum).

A further part of this view of ethnography as inquiry into worlds is a view of these worlds as inherently adapting and changing, recreated and reinterpreted by individuals in their own lives and in relation to the experience of the group as a whole. From this standpoint, one taken initially in American anthropology, so far as I know, by Sapir in his writings of the 1930s, a cultural world has not been accounted for if treated in terms of its conditions alone.[5] One such condition, a major concern in the development of anthropology, is of course historical provenience and transmission, cultures as interesting and distinctive wholes that persons acquire, manifest, transmit, but as their *locus* rather than their source. From this point of view, what is

cultural tends to be equated with what is in fact common or shared. From Sapir's point of view, the fundamental nature of the cultural is that it is *capable* of being shared, that is, of being communicated. A sharable symbolic trait, something capable of becoming more generally part of a group's repertoire of codes, is already within the sphere of the cultural. Such a point of view is necessary to cope anthropologically with the modern world, where the overt signs and diacritics of cultural traditions float, jostle and merge as if each city were an eddy, left behind by a flood that swept all detachable bits of culture about the world. Sometimes anthropologists have been able to see an object of study only in cultural worlds like those sketched in their textbooks, saliently distinct. Indians driving new cars, buying cases of pop, watching color television—where's the cultural world there? There is one, and one not so wholly like the non-Indian as appears; but it may go unwitnessed, if we shake our heads and mourn that the god of cultures has long ago finished his task of creation. When it comes to cultural worlds, the seventh day will never come. (This theme is developed in Hymes 1973; see esp. p. 34.)

Such a view, restricting anthropology to the "other" cultures that historically fell to its lot in the great handing out of subject matters a century ago, seems to rest on the assumption that isolation, or at least strong barrier, is necessary for the flow of culture to acquire distinctive form, and that this requisite is increasingly absent. There is truth in the assumption, but so to interpret it involves an inadequate conception of the nature of boundaries. On the one hand, the salient boundaries, marked by a language, a geographical barrier, a political line, have proven very permeable. On the other hand, the sense of a distinct cultural world depends ultimately upon taking something as a boundary. It is a function of self-definition, identification, of meaning given to whatever differences may obtain. The differences may be few in number, may be less in physical and observable traits than in configuration, or simply in shared experience and what Raymond Williams has called "structure of feeling." Such a basis for boundary may have been more important than usually realized in the cultures traditionally studied by anthropologists.[6] Such a basis may be prevalent within a conglomerate social structure such as our own. Perhaps each of us moves too much in a round of activities and people that matches our own conception of a world to appreciate the diversity about us. We can share a city such as San Francisco or Philadelphia with hundreds of thousands, and meet only professional colleagues. But if the paths of each were traced, and the meanings glinting on either side gleaned and understood, a multitude of distinct worlds might become evident. Beyond our own rounds, and the spheres defined as public problems by media, perhaps lie a great many worlds unmentioned and out of sight.

If this is so, then there is plenty of work for ethnography, and work that only ethnographers do (though the assistance of novelists is to be welcomed). The existence and character of these worlds, their bearing on schools and education, can become known only through participation.

Let me give an example. The Warm Springs Reservation, where my wife and I work with speakers of two Indian languages, has been a distinct political entity for more than century. Although three different peoples were brought together on it, and a lively awareness of the original tribal affiliations persists, reinforced by financial considerations in treaty settlements, much intermarriage and interaction has resulted in a common sense of membership in the "Confederated Tribes." A certain amount has been learned and recorded about the aboriginal culture of the people, and about its persistent elements, some of which—one of the languages, certain rituals, certain patterns of activity, etc.—remain today. A certain amount of attention has been focused on the Reservation as an example of Indian people seeking economic self-determination and self-sufficiency. The policies adopted in this regard, the various activities, the consequences, can be known. Various other aspects of life, such as housing and health, attract attention because of the involvement of governmental agencies. So far as I know, no one has addressed the question, what is it like to grow up and live at Warm Springs Reservation? How does the world appear? What is that world like? It is a world with color television, suburban-style housing developments, a resort hotel catering to whites, a golf course, an organized historical society, a new administration building, etc. It is also a world in which a good many of the brightest people become alcoholics, or so it seems to us; in which bright and motivated children often leave or are forced from high school, marry, get pregnant, go to work, whatever; a world in which some young people die every year in auto accidents; in which a major bulwark of the social fabric continues to be a number of responsive, responsible grandmothers; in which the security of assured shares of tribal income interacts somehow with severely limited opportunities for work and hopes for responsibility or authority; it is a world whose every member must at some point decide for himself or herself what it means to be an Indian, because there is no way to avoid or deny the identity.

One anthropologist appears to feel that there is not much more than tidbits to be gleaned there—the old people who "knew the culture" are almost all gone. Whether or not there is still at Warm Springs a "culture" in some of the older ethnological uses of the term, there is a cultural world. It is a world not wholly or analytically understood by its members, who have as categories of understanding mostly only either traditional ones or ones supplied by external institutions and the surrounding rural white society. What happens to children in schools appears to depend on how the children interpret their world, given such categories as they have available. To find out what they see and do, to convey that knowledge in a way that permitted some of the texture of their lives and world to come through, would be what I mean by ethnography.

The level of cultural worlds completes the chain of levels within which structure is to be discerned through functional relevance.

Let me try to show how this is so, and in so doing link the discussion of linguistic methodology with the discussion of ethnography.

V

The methodology at the basis of modern linguistics, as has been said, depends upon the notion of commutation, of demonstration of functional relevance through contrast (as against repetition), showing that a particular change or

substitution or choice counts as a difference within a larger frame of reference. This methodological principle should be taken into account in any general discussion of qualitative methodology, and it is capable of extension beyond what has been made of it in linguistics proper. There are indeed two important kinds of extension to be encouraged by those concerned with language as part of social life. One has to do with the basis of linguistic structure, the other with the building of it.

Our modern edifice of language structure has employed the principle of contrastive relevance primarily in the service of cognitive functions, what can be rather simplistically called "reference." The correlative notions of contrast and repetition have been used to establish features that enter into the kinds of meaning involved in naming, statement, logical claims, and that illuminate relations of grammatical structure in the service of such kinds of meaning. To be sure, there has always been some attention to features and meanings that can be called "stylistic" or "expressive," but their domain has seemed marginal or secondary. In point of fact, the principle of contrastive relevance applies to both kinds of meanings, and if it is a fundamental goal of linguistic theory to explain what counts as repetition, what counts as contrast, then expressive, or (as I prefer) "stylistic" functions are equally fundamental to linguistic structure.

There is not space to deal with the ramifications of this fact, but an illustration may show what is involved. The first, and common, kind of contrastive relevance is illustrated by contrast between /p/ and /b/ in English, such that "prattle" is something a baby may do, and "Brattle" a nonhomonymous street in Cambridge. The second kind is illustrated by contrast between a heavily aspirated and a weakly aspirated /p/. In the first case a difference in referential structure is conveyed, in the second a difference in attitudinal structure: emphasis perhaps to make the word clear, to express disgust or elation, whatever. Heavy aspiration of a stop such as /p/, precisely because it does not serve referential function, can serve stylistic function. It does so as a conventional device available to speakers of English, a part of their linguistic competence. Like the referential contrast between /p/ and /b/, the stylistic contrast between heavily and lightly aspirated /p/ is diacritic. That is, it distinguishes meanings, it does not embody them. The meaning conveyed depends upon further features of the utterance. For this reason, I would refer to the two, complementary bases of contrastive relevance as establishing two "elementary diacritic functions." (This point is elaborated in Hymes 1974a, ch. 8 and 1974b, and taken up in Hymes 1972).

The presence of the second, complementary function is implicit in the difference between paradigmatic sets of sentences, chosen to illustrate points of grammatical structure in the narrow sense, and paradigmatic sets of sentences, chosen to illustrate actual choices in the use of sentences in social life. An example of the first comes from Postal (1974:3):

a. I think that he is rich (*that* clause)
b. That he is rich is thought by me (?) (1st passive)
c. He is thought to be rich by me. (2nd passive)
d. But not: *I think him to be rich. (complement)

Leave aside that fact that Postal finds (d) unacceptable, whereas it seems perfectly natural to me, a slightly elegant or literary mode of expression. Leave aside the fact that (c), which Postal finds acceptable, and which illustrates his grammatical argument, seems odd to me, and that the only way I can make it acceptable is to introduce stylistic function in support, so that one would be saying (or hearing) a response:

"He is thought to be rich by *me*," following, perhaps, "No one ever thought him to be rich" (in my speech, not apparently in Postal's) cf. (d) above). The main point is that when one asks, what are the alternative ways by which one would express the notion (a) in conversation, people do not ring changes on the grammatical paradigm exhibited above, keeping other things constant, but change their utterances in a variety of ways. They make use of choices in other sectors of language, lexicon, intonation, other types of construction. Lexical options come readily to mind (rich, wealthy, loaded). What people appear to be doing is to consider the reasons (functions) for saying the thing differently, that is, they invoke possible differences of situation, both verbal and social, and consequent options of style.

One might refer to the kind of relations disclosed by the first kind of contrastive relevance as having to do with *resource grammar*. The bare bones of grammatical possibilities, preserving reference and neglecting style, are examined and collated. The second kind of contrastive relevance brings to light paradigms of a sort that might be called "natural conversational paradigms" (as opposed to "analytical grammatical paradigms").

This kind of contrastive relevance has to do with what can be called *discourse grammar*. It employs the recognition of stylistic functions to extend linguistic inquiry beyond the usual levels of language to the styles and choices involved in use of language. Even in studies of literary style, the question of contrastive relevance, of whether or not an observed feature represents a choice (for the author, or for the reader) is fundamental. (Vendler 1975 uses this principle nicely, e.g., pp. 13-6, 22-3). The theoretical approach of Michael Halliday makes use of such an approach in a particularly stimulating way, envisaging grammatical means (the discoveries of resource grammar) as organized according to four generic types of function, ideational, interpersonal, textual, and logical (see Halliday 1973). The conception remains to be tested fully in English and across a variety of languages (I do not think, for example, that pronouns would be found to occupy quite the same place in all languages). Much current work by linguists and others, studying texts, conversation, speech acts, and associated properties of coherence and conduct, does a great deal to explore language from essentially this standpoint, the standpoint of alternatives in actual uses of language. Extension at the base and extension at the top, as it were, are not always integrated, unfortunately. Speech acts, such as promises and threats, may be analyzed without regard for the role of stylistic (and communicative) features that enter into their meaning (cf. Hymes 1974a, ch. 9). Current studies of discourse, texts, conversion, speech acts, are doing a great deal to explore these areas. Social interactional meanings are beginning to receive their due.

We cannot adequately evaluate language development

and the uses of language that enter into education without attention to both these extensions of the principle of contrastive relevance. Properly pursued, they entail a general conception of language development and use as a matter of meaningful *devices*. The still common use of mean length of utterance as a measure of development is not in keeping with this principle. The measure may helpfully correlate with other things, but it can shed no light on what is happening, what is being acquired and used. Again, it is like comparing motors by their size instead of by their structure. Language, from sound to style, is a complex of *form-meaning covariation*. That is another way of putting the point of contrastive relevance. To discover what is there, what is happening, one seeks to discover which changes of form have consequences for meaning, what choices of meaning lead to changes of form. One works back and forth between form and meaning in practice to discover the individual devices and the codes of which they are part.

The limitation of linguistics proper has been that, despite the potentiality of its methodological principle, it tends to stop short of the full range of form-meaning covariation, and to stop short of ethnography.

This is an old story. Modern linguistics advanced decisively over the popular notion that one could tell something about the character of a people by the presence or absence of individual words ("they have no word for "thank you'," "the Germans have a word *Schadenfreud*," as if the absence of the word meant a posture of ingratitude, the presence of the word a special delight in the misfortunes of others). Franz Boas made central to his linguistic work the question of the categories that were not merely present in a language, but obligatory, that is unavoidably involved in verbal expression. (Tense is such a category in English when verbs are used, number when nouns). Benjamin Lee Whorf proposed to go beyond registering the obligatory categories to a study of their articulation with other features in actively employed "fashions of speaking," but the study of "fashions of speaking," which would entail ethnographic inquiry into styles, was not taken up. (This point is discussed in chapter 8 of Hymes 1974).

We see the same story today in studies that are called "pragmatics" or "discourse" in linguistics. From a strictly linguistic point of view, it is interesting to investigate how it is that a question may be the answer to a question in certain types of encounter—how to the query, "Do you have any coffee left?", the answer may be, "Do you want cream?", presupposing a positive but unspoken answer to the initial question. These ellipitical sequences are characteristic of exchanges in stores where the dimensions of the encounter are limited and mutually well known. An examination of such encounters must necessarily involve field work, that is, observation of actual cases, to obtain its data. But if the analysis is limited to the consequences of such sequences for a theory of language organization, it is not ethnography, but field work. A larger frame of reference of contrastive choice would be required. When are service encounters of this type appropriate, when not? When are people insulted by the restriction of an encounter to such an exchange? What does it mean to an old store-owner in an ethnic neighborhood that the new young sales representatives limit their interaction with him to truncated exchanges of this type? What genre of verbal exchange has been replaced? What is the nature of the verbal ability that now has no occasion? More generally, what is the range of the truncated service encounter in the society in relation to the full set of alternative types? And what are the common styles? One has the impression that the American style is found brusque to the point of insult in England, the English style overly polite to the point of archness of effeminacy in the United States. In sum, the full pursuit of form-meaning covariation would not stop with consequences for linguistic structure. It would discover something of the *resonance and consequence* of this instance of a genre within cultural worlds.

This reasoning of course holds for speech acts and small genres of all kinds, requests, commands, greetings, teasings, etc. Since such study unavoidably engages phenomena in change, as well as choices across a range of settings, quantitative information and analysis is essential. One expects to find proportions and trends as much as or more than categorical rules of appropriateness.

The principle of the linguistic ethnography that is needed can be put in terms of complementary perspectives. If one starts from social life in one's study, then the *linguistic* aspect of the ethnography requires one to ask, what are the communicative means, verbal and other, by which this bit of social life is conducted and interpreted? What is their mode of organization, from the standpoint of repertoires of codes? Can one speak of appropriate and inappropriate, better and worse uses of these means? How are the skills entailed by the means acquired, and to whom are they accessible? These questions lead one into the territory of the other starting point. If one starts from language in one's study, then the *ethnography* of the linguistic work requires one to ask, who employs these verbal means, to what ends, when and where and how? What organization do they have from the standpoint of the patterns of social life?

VI

In a critique stressing the use of language and ethnographic inquiry one should consider one's own uses of language as scholars and scientists. The discussion has concentrated on qualitative structures, with recognition of the relevance of quantitative methods. In mentioning resonance, and using a word like "texture," one raises the question of narrative reporting as well. To the best of my knowledge, some of what we learn and know and should convey can only be expressed through skillful prose. It is a commonplace in anthropology to admit, or enjoy, the fact that novels about a country may be a valuable source of understanding. In recent years a growing number of anthropologists have felt impelled to write a narrative about their field work. Having published the scholarly analysis, they write a second book to try to say what it was really like. This seems to me a healthy impulse. It has roots in the increasing concern with the reflexive nature of social science inquiry, but is not to be reduced to that. Much of what we know, in anthropology and in personal life, is known by means of narratives, anecdotes, first-hand

reports, telling observations. In the vital decisions and directions of our lives we willy-nilly rely on what we know by such means. In our scholarly chairs we find it difficult to acknowledge their validity, though we may admire their artfulness. There are many purposes and kinds of verbal art, but some of it, I believe, is a way of getting at the truth. One can read poems for fun, sanctity, duty or a livelihood, but some poems one can read for what they enable one to experience and know. If we are to extend our understanding of language to the full, so that we can fully comprehend its role in schooling, in education, in social life, in our own lives, we have to find a way to come to terms with the validity of uses of languages that are aesthetic. Some people are brilliant at numbers and research design, some excel in discovering and articulating qualitative structure and pattern, and some are masters of the art of conveying events and experience and insights in words. To admit this is not to give way to rampant subjectivity. We can and must discriminate, establish canons of judgment, make explicit our criteria for trusting one set of words, taking another under advisement, and distrusting a third. It is a job of verbal criticism and inquiry that has a great deal to contribute to the legitimation of much that anthropologists believe they know through ethnography.

These considerations bear on the final point to be made about the role of ethnography. Ethnography can of course be used for many purposes, serve different interests. In my conception, its validity is dependent in part on the knowledge already had of their ways of life by those whose ways of life one seeks to study. Behind every classic ethnography, I suspect, stands one or more members of the culture who were themselves ethnographers without portfolio. Wherever meaning in the third sense discussed above, having to do with resonance and consequence, is successfully conveyed, one suspects a process of inquiry that was collaborative. Such a process is one to which the members of the cultural world bring knowledge of its codes and experiences, and to which the ethnographer (who may be a member) brings methodological skills and comparative perspective. A good part of the knowledge held by members of the culture is necessarily tacit. Their languages, their expressions and styles, are indispensable sources of insight, but never in themselves a complete and adequate metalanguage for their own world. One of the fundamental questions of anthropology, indeed, or at least of linguistic ethnography, has to do with the degree to which a given language is an adequate metalanguage for the way of life of which it is part. What concepts and meanings have found explicit linguistic shape for reporting, discussion, reflection, and which not? And what is the role of language as such, as a means of communication more or perhaps less employed, enjoyed? Some cultural worlds are permeated with language, others not.

A consequence of this fact for ethnography is that native documents and testimony, while indispensable, as Boas insisted, are never sufficient. To a fair extent, subject to ethical choice and judgment, the process of ethnography can be an exchange of knowledge. Many linguistic informants have become fair analysts of their own languages in the course of contributing indispensable knowledge about it. The same can be true in ethnography generally. In this possibility lies a possibility for a mode of ethnography that is not exploitative and that contributes as well as takes in the world in which it works. Obviously this possibility is surrounded by many complications, not to be gone into here. But it is important to note it, especially when we are concerned with ethnography in institutions of our own society, such as schools. An ethnography that served only higher levels of government, national institutions, and theory, is hardly possible in any case, as superintendents and principals are quick to tell us. In the exploration of ways in which ethnographic inquiry in education can be founded on mutuality, questions of language themselves, of the sort considered just above, have a part to play. I would like to think that some of what one learns and knows and has to report is inseparable from uses of language that are continuous with those of ordinary life. These are the narrative uses, the uses into which an aesthetic consideration of apt expression enters. Cultivation and analysis of such uses may contribute to mutuality between ethnographer and school. And it may be a healthy thing for the democratic quality of our society if such uses can be given the justification and legitimacy they deserve. Indeed, such uses do play a vital part in decisions and perceptions, so that we handicap our understanding of educational institutions and the forces that affect them if we do not make them explicit objects of attention. Our own language development is in need of assessment.

ENDNOTES

[1] This paper was stimulated by participation in the Workshop Exploring Qualitative/Quantitative Research Methodologies in Education, held in Monterey, California, July, 1976, and sponsored by the Far West Laboratory for Educational Research and Development, in cooperation with the National Institute of Education and the Council on Anthropology and Education. An abstract of the paper, prepared by members of the Laboratory, appears under the title "Critique" on pp. 91-3 of this *Quarterly* (vol. VIII, no. 2). I am grateful to the editor of the *Quarterly* for finding space for the paper, which proved too long for the issue devoted to the Workshop as such. I should also like to thank the reviewers of the paper for the *Quarterly*, not all of whose cogent comments I have been able to act upon, since the result might well have been another whole issue. I hope that various references may help fill out what may be too cursorily treated here.

[2] John Dewey used the example of the expulsion of lip-rounded breath to blow out a candle or to begin an English word, before Sapir, but I have forgotten the reference. Sapir does not mention Dewey. Perhaps he had forgotten to.

[3] The logic is really much the same as in the discovery of significant relationships through the assignment of subjects to experimental conditions. It is just that in the core of language, viewed as a referential mechanism, the conditions sort out answers of a "yes" : "no", all or nothing sort, rather than of a "more" : "less" sort. A typical hypothesis, in terms of the simplified illustration previously given, would be that a given sound is (or is not) independently relevant. Consider (b) occurring between vowels. The hypothesis is tested by substitution (commutation). If (b) cannot be replaced by (p), then in this position it cannot contrast with (p); the difference between them cannot distinguish words in the language in question, and the similarity would lead one to group the two together as members of the same systemic unit. The difference would be readily explainable in terms of local conditions and general theory. Voiceless sounds, such as (p), often become voiced (as is b), when occurring between voiced sounds (such as vowels). The same inference and explanation would hold if (b) could be replaced by (p) between vowels, but the replacement had no concomitant (correlated) difference in meaning. (The commutation test is an application of the general principle of form/meaning covariation). Of course if replacement by (p) resulted

in a different word, the two sounds would belong to different units. The concomitant change would demonstrate that one had to do with not one, but two, elements of the minimal arbitrary code of the language.

[4]On this conception of a development, see Hymes 1974, ch. 8. Of course stylistics has been cultivated for a long time, and sometimes even seen as fundamental to linguistics, but for most linguists "style" has been a marginal category, and most investigations have been of specialized genres. I am arguing that the heritage of findings and insights into style becomes relevant now to the central challenge facing linguistics, that the study of speech styles can be seen, not as additional, but as fundamental.

[5]This is a poor place to try to open issues of social theory, but what I mean by "conditions" can be briefly sketched. The historically given traditions are one such condition, one means out of which cultural worlds are constituted. The forms, constraints, and possibilities of recurrent types of structure, ecological or economic structure, say, or social structure as a whole, can be distinguished as another set of conditions. The experiences, motives, minds of persons are another. There is a recurrent tendency to take some one of these as the object of study and theory. In Sapir's day, an impersonal objective "culture" in the sense of a historical set of traditions was often so taken, and his writings of the 1930's are in critique of that, for the sake of the role of third condition, the personal. What we call "social anthropology" often seems to fix upon the second, as if the first and third were secondary or epiphenomenal; Marxism that derives "superstructure" from "base" is akin. The fine insights of the ethnomethodological movement in sociology run the danger of reducing the whole to the third, as if the fact that cultural worlds are constituted by participants could be enlarged to the proposition that they are solely or wholly so constituted, or that only their constitution was worthy of study. All these things—received traditions, environmental and social structures, personal constitutive activity—seem to me conditions, origins, of cultural worlds, jointly, and even all together, not exhaustively. By occasional use of the word "configuration" I mean to suggest that cultural worlds, like lives and works of art, come out contingently and have to be experienced to be known.

[6]Ethnological studies of the distribution and diffusion of traits have shown how permeable mapped boundaries may be. I recall a Berkeley-trained ethnologist exclaiming that the Tübatulabal differed from a neighboring tribe in only two traits. One, to be sure, was their language. I suspect a specific structure of feeling would have been found also.

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