

## LINGUISTICS AS A TOOL IN AFRICAN STUDIES

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The explicit and systematic study of African languages, like the corresponding investigation of African arts, has long been among the "step-children" of the general field of African Studies. Precisely why this should be true of the arts is a question I must leave to the ethno-estheticians—although in passing I can hardly help noting that, in Africa as elsewhere, both language and art seem to serve a primarily communicative function. Moreover, since the new disciplines of paralinguistics and kinesics fall midway between the study of language on the one hand and that of music and dance on the other, it is hardly surprising—however disappointing it may be—that their application to African subject-matter has been so scant.

On the "lag" in African linguistics, however, I think I can speak with somewhat more assurance. To me it seems to have two sources. The first of these is the anachronistic but nevertheless persistent view of Africa as a "dark" continent, scientifically as well as racially and explorationally speaking. The second is the strong tendency, among scholars almost as much as among laymen, to take language itself for granted and to make explicit analyses only of those things to which language refers.

In view of the all-pervasive role of language in the lives of all of us, this striking "blindness to the obvious" seems itself to require explanation. My own would be that language is like air, in that we are so immersed in it that we ignore it, becoming aware of it only on those rare occasions when it is absent or inadequate. However, our languages are also like our faces and our minds, in that we are so close to them that we have difficulty seeing them in perspective—if, indeed, we can see them at all. Language, then, is ever with us, expressing us and affecting all our behavior, even that which is seemingly least linguistic in nature, and doing so even when we are least aware of it. To neglect language, therefore, is to neglect the supplest tool man ever invented; to ignore language is to ignore the most powerful weapon known.

The nonlinguistic specialist for whom linguistics is most useful, not to say indispensable, is the ethnographer; for unlike the tourist or the trader, he is trying somehow to grasp the "inwardness" of the culture of the people among whom he sojourns. To this end, of course, it is always best if he becomes fluent in their language. However, even if this is not feasible, he can at least learn, by carefully listening for repetitions, to isolate the distinctive elements—the phonemes and morphemes—of their language. From there he can proceed to isolate some of the longer units, conventionally known as words and sentences, that are constructed from these linguistic "building blocks."

Nor need he stop here. For—still short of fluency—he may notice that some words and sentences recur with extraordinary frequency in an unusual number of different (and often, to his mind, mutually incongruous) conversational environments. Sentences of this sort in our own culture are generally known, laudatorily, as proverbs and, derogatorily, as clichés. However, single words of this ilk lack a type name. My own inclination would be to call them

"culture words," although for the edification of those who prefer to borrow all technical terms from exclusively classical sources, I am equally willing to dub them "ethnologs." In either case, the point is that these words occur in a disproportionately large number of constructions with what seem, at least to an outsider, an astonishing variety of meanings. One such ethnolog is the English word *get*, which probably has something like 15 different "major" synonyms: contemporary Americans try hard, for example, to get "on the ball," to get going, to get rich, to get away from their worries, and to get to the moon before the Russians do. Whether or not these various goals are realistically compatible, of course, is beside the point. What matters to the anthropological observer is the fact that these usages, diverse though they may be, form a cultural constellation, characterizing an "acquisitive society" oriented toward the American Success Story.

In Bini, the Nigerian language with which I am most familiar, a comparable ethnolog is the verb *gbé*, "to beat," which can also mean "to strike, to break, to kill; to perform, to dance; to pick or pluck; to catch or trap; to dawn; to rot or be finished; to do something intensely, repeatedly, or to excess." Although my predecessor in Bini studies, Hans Melzian of Berlin, Germany, felt that these various usages had to be ascribed to at least four "different" (though admittedly homophonous) verbs, my own inclination is to regard them all as variations on the single theme of swift, forceful, and aggressive action, suitable to a well-centralized kingdom with a history of efficient military imperialism.

However ethnographers are far from being the only nonlinguistic specialists who can make good use of linguistic knowledge. Another group who depend heavily on linguistic expertise are the literacy experts. In areas where there is no accepted orthography for the indigenous language, their task is to create one on the basis of a good phonemic analysis of that language—whether this analysis is theirs or someone else's. On the other hand, in areas where there is an accepted orthography but where progress toward literacy is slow or wholly lacking, their task becomes that of re-examining that orthography to determine in what way its graphemes distort the phonemes: or, in less technical language, how its letters conceal, rather than reveal, the sounds of the language. A good case in point here is Ibo, the dominant language of eastern Nigeria, where an eight-vowel system used to be represented by only six vowel signs, with consequent hardship to writers, both native and foreign. (An even better case, of course, would be English, where nine vowels are represented by only five signs!).

In the area of African prehistory, comparative linguistics has been able to make substantial and sometimes decisive contributions. Vocabulary analysis, for example, reveals fairly definitely that the linguistic ancestors of the Malagasy came from Borneo and those of the Bantu from eastern Nigeria.

More recently, the development of the linguistic dating technique known as glottochronology has given promise of revealing not only which of a group of genealogically related languages diverged earlier and which later, but also the approximate period at which they diverged. In other words, we now seem to be close not merely to a relative but actually to an absolute chronology for the linguistic history of Africa.

Glottochronology, to be sure, still has methodological defects. Most glaring

of these is its inability to give reliable dates for the history of lexically mixed or "creolized" languages like Lucumí, a Yoruba dialect spoken in Cuba (and first studied systematically by William Bascom, who contributes to this publication). While lexico-statistic analysis makes Lucumí appear to have diverged from Oyo Yoruba nearly two millennia ago, historical documents make it unlikely that the actual divergence took place more than two centuries ago. However, special difficulties of this sort, it seems to me, do not invalidate the glottochronological method as a whole; they merely show that we must make more allowance than we did a decade ago for historically "intrusive" factors.

Now that most of the former colonial territories of Africa are on the way to nationhood, one of the more pressing political tasks for these countries is that of overcoming the extreme linguistic fragmentation that has, in the past, characterized most areas of Africa. For many administrative districts covering large areas, attempts have recently been made artificially to re-unify divergent dialects into a single "standard" language.

The success of these attempts has been uneven. In Nigeria, for example, the effort to combine the Onitsha and Owerri dialects of the Eastern Region into a "homogenized" Union Ibo has been relatively effective, thanks largely to the fact that neither of these dialects was conspicuously favored over the other. In the Western Region, however, the analogous effort to reunify the Northern Edo vernaculars (Bini, Ishan, and Kukuru) into a comparable "Union Edo" has been almost wholly fruitless, chiefly because of suspicion on the part of the non-Bini peoples that the whole scheme was but a veiled plot on the part of the Oba of Benin to re-establish his ideological, if not political, suzerainty over them.

Even more troublesome, however, has been the problem of *linguae francae* or national and pan-African languages. One solution to it is for each sovereign state to have its own national language: presumably that of the demographically preponderant people in each country. Thus in Nigeria this language would probably have to be Hausa. Another is to have a single indigenous language for the whole of tropical Africa. Again, on a demographic basis, this language would probably be Swahili but the great practical obstacle here is the fact that few West or South Africans now know any Swahili. The languages of which they do have a shared knowledge are primarily those of the metropolitan powers: English, French, and Portuguese. Nevertheless, ideologically, all of these languages are tainted by their aura of colonialism. All in all, the nonlinguistic factors involved seem to be so potent that the problem is one to the solution of which linguists cannot hope to make more than a partial contribution: the rest of the job will have to be done by social psychologists, political scientists, and (above all, perhaps) diplomats.

Semanticists, however, and linguists dealing with problems of style and usage probably can make a substantial, if not decisive, contribution to the task of helping diplomats, administrators, and businessmen avoid "word traps" into which many of them fall unwittingly and quite unintentionally, yet with the most deleterious effects on the good will most of them are trying so laboriously to create in their international and interregional relations. A simple English example here is the increasingly unpopular use of the words "native" and "un-

derdeveloped" in referring to the tropical Africans and their developing economies. However trifling such word taboos may seem to non-Africans, observance of them has, in effect, become part of the basic linguistic etiquette of the field of African affairs.

The converse, of course, can be remarked of the powerful positive mystique which has, in the past decade, attached itself to such terms as "négritude" and "the African personality." Although to sceptical European and American analysts these expressions seem to have little if any denotative content, the fact remains that connotatively they are "words to conjure with." To understand their force among Africans, non-Africans need only recall the aura that still clings to such phrases as "la mission civilisatrice de la France" or "the American way of life."

In summary, I should say that, while there are few African language problems that professional linguists can solve completely on their own, there are even fewer to the solution of which they cannot make a substantial, and often indispensable, contribution.

#### *Discussion*

H. D. GUNN: As my own particular interest is primarily in the line dealt with by Bascom, I propose to comment chiefly on that paper. However, I shall remark briefly on the remaining papers.

I very much agree with what Bascom has to say about the need for studies in technology and ecological adaptation. I was interested in his recommendation of Trowell's Uganda study as a model to follow. In essentials, with respect to general form, I am inclined to agree with this, disregarding the inadequacies of Trowell's implicit definition of art and of her treatment of "art" in Uganda.

However, in her analysis of the crafts of Uganda, Trowell falls back so heavily on the Hamitic myth that the study becomes a positive barrier to advancement in certain directions, and I think that warning noises should be made.

Nevertheless I do stress that, with regard to her actual cataloguing of artifacts, she provides a model that can be recommended to students with very few reservations.

Actually, it is rather ironical that technology has been more studied than published reports might indicate. Functionalists have received some sharp criticisms for the areas with which they have allegedly failed to deal. However, I should be surprised to know that anyone seriously believes that the functional analysis of culture proceeds by ignoring such fields as technology and even ethnology; I know, for a fact, that a number of British anthropologists who are rarely connected in print with technology have made a profound study of it in the field.

The existing situation is a commentary largely on what publishers will handle.

I have tried to encourage some interest in technology, specifically African technology, through some work that I did recently at the Commercial Museum, Philadelphia, Pa. The museum, as it happens, has a very interesting collection that is rarely on view, and it seemed to me worthwhile to publish at least some rough description of it so that students might know what they should have to work with there. I was, happily, able to achieve my aim.

Those who may be interested can correspond with the museum regarding facilities for research. The technological collection, particularly from the old French areas of West and Equatorial Africa, is very unusual in this country.

Regarding the paper on linguistics, not being a "linguistician" myself, I am rather loath to put forward any special views. I think that one of the major problems has been to persuade Americans that they actually can learn to speak another language, and it is only recently indeed that Americans—countering the most distinguished precedents—have spent long enough in the field to learn a vernacular.

At the present time, I think that "linguisticians" need to publish a more representative range of their views, hypotheses, and thinking in these matters in primarily anthropological (that is, general) publications. There was a time, a few years back, when a fairly representative sample was available to students in, for example, the *American Anthropologist*; however it seems to me that in recent years the development of linguistic periodicals has had the effect of cutting off the general reader from a certain number of advances, or trends, in linguistics.

I think, perhaps, Westcott might answer one point, namely, that to the field worker it is not so much African languages that are needed as the techniques of learning and describing a language. Linguistic differentiation is an important means of maintaining a group's identity and, in some areas, the wrong vernacular or the wrong accent can be a handicap, at the outset at any rate. In working with people with a relatively well-developed literature, such as one may find along the Guinea Coast for example, great discretion is needed. I have known the unbridled attempts of one linguist to substitute a phonemic notation for a nonphonemic one in his work to alienate informants literate in what we must designate the traditional orthography. However I wholeheartedly agree with Westcott's general points.

In connection with Schwab's paper, I have only one point to raise. I think it is an extremely valuable paper in that it stresses the fact that out of conflict unity frequently emerges. This is something that Americans in general have been inclined to doubt; until recently, there has been a strong tendency to neglect the sociology of conflict.

On the other side, I might remark that the concept of "traditional forms interweaving with new elements" bothers me a bit: that is, the actual words, rather than what Schwab means to say. The phrase might be taken by some to be calculated to mislead anyone not familiar with the African situation at first hand.

The colonial relationship referred to developed over about 400 years, and what the newcomer to Africa may see as "traditional" is actually something that has grown out of a contact situation and I think that this should be explicitly recognized: what looks traditional to us now is already a result of contacts, and not all of them are necessarily contacts with Europeans.

Many of these features arose in contact situations where Europeans were not present. In Northern Nigeria, centuries of developments that culminated in the Fulani jihad and the Fulani empire produced—apparently—among the peoples of the so-called Pagan Belt certain changes. They seem to have

produced certain marriage forms, for example, and family structures that Schwab's evidence suggests may be in process of emerging among the Yoruba more recently. The fragility of marriage, for example, was apparently very great in that situation.

ALFRED HARRIS (*Brandeis University, Waltham, Mass.*): I propose to discuss the papers in order, stressing one or two of the points made by Harold Gunn and perhaps adding some others.

In regard to William Bascom's paper, although I have been "tarred" with the British structuralist brush, I wholeheartedly second his plea for more studies of this sort.

There are one or two points upon which I hope he will see fit to expand. When Bascom discussed the need for distributional studies I did not get a sufficiently clear idea of their purpose. Presumably he had in mind their contribution to historical studies; this surely is related to archeological work, particularly where specific material objects are concerned rather than to techniques of production.

Bascom was concerned also with a problem of a wholly different order: popularization. I feel that this is a matter that eventually will fall not to anthropologists but to Africans; I think that they are quite capable of handling it. At present there is no doubt about the inaccuracies of the popular image, nor about the difficulties stemming from them. In teaching, for example, I have on occasion competed with what has been said on television programs, and usually I have been the loser.

In regard to the problem of functional aspects of material objects of art for example, may I stress even more strongly than Bascom—if that is possible—the need for more and better detail.

Bascom has outlined a number of areas in which our information is extremely meager and, in this connection, a point made by Murra is relevant. We cannot all be experts in every field, nor can we even gather data on each of the specialties mentioned by Bascom. Clearly there is a need for more active cooperation in the field on the part of various specialists to collect the data required; we also need much more in the nature of synthesis.

Harold Gunn pointed out that much material has been collected but remains unpublished. It is certainly true that lack of treatment does not imply that material culture has been ignored; it frequently has been considered. Furthermore there is much more available in the literature than is suggested by Bascom. DeSchlippe's study of Zande agriculture includes a detailed list of tools and a description of the ways in which they are used. Barnes has given us a lengthy paper on the material culture of the Fort Jameson Ngoni; there are other such studies. On the side of ecology, we have Stenning's work on the Fulani, Gulliver's on the Turkana, and many others.

The list could be expanded, although this may not be quite the sort of work Bascom has in mind. I might point out that, curiously enough, a large proportion of the published data on material culture and on ecology emanates not from scholars in the United States but from British or continental scholars.

With respect to Schwab's paper, I find myself in agreement with most of Gunn's comments. The central importance of conflict as a cohesive force is,

of course, familiar to many of us from the work of Simmel and the later reformulations of Gluckman and his students. In certain respects, Schwab has contributed further illumination here. Nevertheless Schapera in *Government and Politics in Tribal Societies* has raised some difficult questions with regard to Gluckman's position. It seems to me that some of these same questions might apply to Schwab's analysis of the Yoruba.

Another issue might be raised with regard to the points that Schwab chose to elaborate in his description. Many of these demonstrate clearly that the Yoruba have much in common with peoples elsewhere in Africa. It might have been helpful if Schwab had stressed somewhat more those features of the Yoruba situation that are distinctive. A paper such as his should do more than add confirmatory detail to a picture already reasonably well known. Such an emphasis might raise questions with regard to other parts of Africa. In other words, the importance of particular ethnographies is to be found not only in the way they confirm the data of others, but at least equally in the new questions that they raise.

Turning to R. W. Wescott's paper, may I point out that my contact with linguistics has been extensive but in part unhappy; although I am fond of linguistics I have difficulty in learning languages. In light of such experience as I have had in using linguistic techniques, I can only give a hearty "Amen" to many of Wescott's remarks. My comments will thus be brief.

The problem of bilingualism is an old and much discussed one. I have many doubts about the extent to which most of us attain "complete" command of the languages that we learn for use in the field. Perhaps it would be more realistic if we aimed for something less than complete bilingualism.

Some statements in Wescott's paper puzzled me: for instance, I did not quite see how he made the transition from the Beni word that he cited to the Beni kingdom so quickly. I was also puzzled by the matter of glottochronology (which puzzles me in any event). I do not quite understand why contrary examples, when available and historically documented, are not counted heavily.

Similarly I am curious to know how conflicts may be discovered in the absence of independent documentation when the linguist is relying wholly on his reconstructed forms. Glottochronology and lexicostatistics (the latter term is not given, although I have heard it used) baffle me; moreover I am not sure that this is the place to ask for enlightenment. However, if anthropologists generally are to make more use of linguistics (we clearly should), the question may not be wholly beside the point; I know other anthropologists who are, like myself, in need of enlightenment.

W. B. SCHWAB: The essence of the Gluckman approach seems to me to be the following. Every social system has conflict within it. Sometimes this conflict is contained by the existing social system and at other times the conflict is so great that radical modifications in the existing social system must occur in order to affect a resolution. Where there is no satisfactory resolution of the conflicts, the disharmony may lead to a breakdown of the system. However, conflict is not only dysfunctional. Often the conflict and its resolution brings about new bonds of cooperation and cohesion among opposing forces within a society; in fact, much of the stability of a social system may derive from the

various cleavages and hostilities in it. However in Africa today it is not only internal discord that must be taken into account in examining social systems. External social, economic, and political forces have generated conflicts that the traditional social system often has been unable to contain. As a consequence of these conflicts, basic alterations in social patterns have occurred.

The lineage system of the Yoruba is especially susceptible to an analysis of this kind. There have been many changes introduced into the Yoruba lineage system in the last 60 years; nevertheless the lineage has remained relatively intact. The conflicts that have been generated have been contained to a greater or lesser extent, and the Yoruba lineage today still provides the main basis for the social life of the Yoruba people. The authority structure within the Yoruba lineage is illustrative of this. Men who have achieved status outside the sphere of lineage influence today are seriously challenging traditional lineage authorities. However, very often the authority system has been modified sufficiently to allow men who traditionally would have little power to exert considerable influence. Out of the resolution of these conflicts new bonds of cohesion have developed within the lineage. The linkages between the traditional authorities and the new represent an important source of stability of the Yoruba lineage system today. However, it is perhaps important to remember that these bonds do not apply uniformly to all areas of behavior, and many of them may be fleeting.

When you consider conflict among the Zulu the situation is somewhat different. There are two main factors that account for the differences. First, in all of the southern and central Africa the primary cleavage is between black and white. Second, in the urban areas the changes that Africans undergo are abrupt and discontinuous. In an urban location the African is forced by the nature of the social situation to shed much of his traditional culture and accept a new way of life built around European values and institutions. Here the conflict is very sharp and the change is very rapid as the Africans undergo a major alteration in their social behavior. This social situation contrasts sharply with the Yoruba in west Africa, where change is slow and the conflict not as great. However even in the location where conflict is so great, cohesion and unity develops. For example, Africans and Europeans who are in direct conflict with each other in so many spheres are also dependent upon each other economically. The economic life of south and central Africa cannot operate without the bonds of cohesion that develop between Africans and Europeans. Another striking example of cohesion out of conflict to be found in the urban communities in south and central Africa are the alliances that Africans of one tribal group form with Africans of another tribal group. Under other circumstances these same tribal groups may be at odds with one another. However they have submerged these conflicts in opposition to their common adversary, the European.

With reference to comments on my paper on Oshogbo, one general point should be understood. African cultures, in the last 50 years, have been analyzed chiefly with reference to the effect that European culture has had on the indigenous culture. Few scholars, especially those in politics and economics, have been concerned with the effect that the traditional African culture has

exerted on the European institutions and values that are being incorporated into the indigenous societies. All African scholars recognize that the process of change in Africa has within it two major cultural ingredients, European and African, but too often scholars tend to deemphasize or play down the latter. Present-day change in Africa can not be understood unless full weight is given to the traditional African cultures in the social equation. It is possible that many of the social upheavals and political fiascos that have occurred in Africa in the last decade could have been greatly ameliorated if the controlling authorities had more knowledge of the traditional cultures, which in the final analysis have been the major motivating force of the social behavior of the African peoples.

ROGER W. WESCOTT (*Michigan State University, East Lansing, Michigan*): The most important branch of glottochronology, incidentally, is not *lexico-statics* but *lexico-statistics*. The reason for this is, quite simply, that the technique is a statistical method for assessing affinity between languages.

While we are being puristic, let me write a little equation. The word Yoruba is actually not often heard in ordinary speech among the Yoruba themselves. Instead, the term is more often heard in its contracted forms, Youba, Yooba, Yoba: of which the third is probably commonest in fluent speech.

Although originally Edo was a word used only by the Bini to denote their own tribe and language, it has come to be used by most linguists and ethnographers to denote the entire family of languages and dialects to which Bini belongs, including, in the north, Ishan and Kukuruku and, in the south, Urhobo and Isoko.

In reply to Gunn, may I say that I am not a linguistician; I am just a linguist. It is not that the latter is in any sense more "correct" than the former, but merely that it is briefer and simpler.

As to his point about training or persuading Americans to speak foreign languages, I can say only "Amen." I agree with him whole-heartedly. I follow William Welmer's school of thought on this matter. What Welmers feels is what most of us at the NDEA language programs in this country also feel: that the big hurdle for American students and scholars is that of foreign language as such. To get Americans over the fear of foreign languages in general and of African languages in particular is what we are trying to do. In this sense we are almost as much psychotherapists as we are linguists.

It shocks a great many people, even fellow Africanists, when I tell them that neither Wolff nor I, who are teaching African languages at Michigan State University, speak any African languages in the sense of having full conversational fluency in them—that is, of being able to engage in easy give-and-take on almost any subject. Like most ethnographers, we carry on rather stereotyped conversations, engaging for the most part only in greetings, negotiations in the market place, and the recitation of formulaic utterances such as proverbs. Nevertheless, we feel that we are fully competent to analyze African languages. You might say that we are like mechanics who are good at taking engines apart but don't actually drive the cars or, at best, drive them rarely and with some difficulty.

What we are really trying to do is to persuade our students that, if they can

learn Yoruba (and they usually find that they can do so far more easily than they had ever thought), then they can learn Ibo too, or Swahili, or Zulu, or almost any other related language. Furthermore, once they get in the field and find, as so many of them do, that they are not going to the area they had originally been assigned to anyway, they don't have to worry too much about this. They now realize that they have *not* wasted two years in learning Yoruba. Rather, when they go to Iboland (or any other such area), they realize that, methodologically at least, they have a big head start on the problem of learning Ibo or whatever other related language they are now turning their efforts toward.

On the practical matter of periodicals, I agree with Gunn again. It is unfortunate that linguists, for the most part, are now publishing in exclusively linguistic magazines—sometimes often in highly specialized linguistic magazines read only by the linguists who are centering their efforts on a very specific area. However, this is part of a general problem, it seems to me. There is nothing unique about linguistics in this regard. The entire problem of over-specialization is one that we all have to face. I think that Leakey's solution is probably the best one: as he observes, anthropology is a field that has become too big for any of us to encompass as individuals. On any really large problem, we now do much better to work as teams: one man specializing in language, for example, and another in anthropometry.

On the question of allophonic versus phonemic writing, I agree once more with Gunn that a strictly and exclusively phonemic alphabet is not always the best one. It depends on what the alphabet is primarily designed to do. If it is designed solely for the writing of a language by native speakers of that language, a phonemic orthography is usually best in the simple sense that it corresponds best to the distinctions they make when they speak.

If, however, the alphabet is designed primarily for foreigners (and this term would include even people from a neighboring tribe), it is often legitimate, not to say preferable, to engage in allophonic writing—an allophone being a phonetic variant of a phoneme.

Harris has asked how I got so quickly from the Bini verb *gbé*, "to beat" or "to act with great speed and decisiveness", to a psychological characterization of the Benin kingdom. The answer is that I happen to be one of those social scientists who old-fashionedly cling to the notion of national character. I do so on the grounds that, as I see it, questions about national character are usually posed in terms of two mutually exclusive and, may I add, false alternatives: (1) that national character indeed exists and is immutable, and (2) that national character doesn't exist at all. My position is that national character does exist but, like everything else, undergoes changes, often very dramatic changes, with time and place.

Therefore I am willing to generalize about Bini character in a way that many scholars would not, but it must be understood that I concede that Bini character has changed in the past, is changing now, and will probably continue to change in the future.

About glottochronology and historical reconstruction, there is much misunderstanding outside the field of linguistics: and, I should say, all too much even within the field.

The best way to present glottochronology, I think, is in terms of a kind of hierarchy. Historical linguistics is the broadest category that we should start with here: it includes glottochronology, which in turn includes lexicostatistics.

Historical linguistics is any linguistic study that is diachronically oriented and attempts to move from a point in the past to a point in the present and to show some relation or progression between them.

Glottochronology is more specific. It is, in effect, linguistic dating. It is a technique for establishing that such and such a linguistic event—for instance, the separation of one language into two languages—probably occurred before this but after that point in a certain sequence.

Lexico-statistics (at least as conceived by its creator, Morris Swadesh) goes even further. It is a statistical method for assessing the relation between languages on the basis of vocabulary correspondence quite precisely assessed; furthermore, for saying in rather absolute terms that two linguistic groups diverged, let us say, 1000 years ago, plus or minus 100 years, or something of that sort. (In a recent article, H. A. Gleason has used the term lexico-statistics to include certain historical techniques of his own in addition to glottochronology. In terms of this usage, of course, lexicostatistics is the including term and glottochronology the included one. However, most linguists still follow Swadesh and prefer to reverse relation between these two terms.)

Most reconstructions go on the first level mentioned above: that of historical linguistics. Glottochronologists do not normally reconstruct in the course of their glottochronological activity. They are not trying to say what the aboriginal word or sentence looked like but only that at a certain time an utterance in language *A* and a corresponding utterance in language *B* constituted a single identical utterance: which is simply another way of saying that these two languages originally formed a single language.

The question posed by Leakey of whether a language is better known from within or from without is, to my mind, strikingly analogous to the problem of whether a personality is better known from within or without. Who knows you better? You yourself? Or a stranger who met you for the first time today?

Obviously, in some respects you know yourself much better. For you have access to your inner stream of thought, while he does not. On the other hand, in certain other ways he already knows you better, in all probability, than you know yourself; because he can see you from a perspective from which you simply cannot see yourself.

I think this analogy holds for language. I think there are certain subjective aspects of any language that only a native speaker can appreciate. However I also think that there are certain objective (and often quantifiable) aspects of a language that an outsider probably appreciates more precisely because this language comes to him as something new and strange and as something that he does not in any way take for granted. Most speakers of a language, for example, remain unconscious of the phonemes of their language because they use them all the time, just as they tend to take the muscles of their legs for granted because they walk every day.

Objectively, the anatomist and the physiologist probably understand a

man's walking process better than he does, but subjectively he understands it better than they do in that he knows how to walk with his muscles while they might find it difficult if they suddenly had to trade sets with him.

On the matter of conceptual distinctions that are manifest in linguistic distinctions, I quite agree with Leakey that we have a strong tendency to project European categories onto African languages and to talk, for example, about past, present, and future tenses, which, in formal sense, may not exist at all in these languages.

However, if one is careful enough in analysis, even of his own or related European languages, he is really not likely to make this kind of naive mistake. If, for example, one looks detachedly at English, he will notice that, in a formal sense, English has no future tense: at least not in the same sense as that in which it has a past tense. That is to say, there is no ending that you can add to an English verb to make it future. Instead, we add auxiliary verbs. We say "I will walk." However, historically of course, what this expresses is a determination to walk. As a locution, it is not at all parallel to "I walked," since there is no ending you can add to "walk" to make it future.

Accordingly, historically at least, we have no future tense in English, and our future formation in English is not strictly a tense at all. Most of us tend to think that it is because we have been trained in Latin, in which you can say "ibo" for "I will go" and "ibam" for "I went" or "I was going". In the Latin case there is obvious structural parallelism between past and future constructions that we simply don't find in English, however semantically parallel we may feel them to be.

Moreover, in English we have a number of expressions that quite controvert our habitual ways of thinking about tense. For instance, we say, "I am going home for Christmas." Where is the "future tense" here? The answer is that we have substituted for the future what is conventionally the present progressive. Actually, however, we frequently use presents for futures in contemporary English; and, historically, as any of us who have read *Beowulf* know, we formerly used nothing but the present tense for references to future time.

Again, in the United States one usually says, "I've got an apple," where the Englishman prefers to say, "I have an apple." Technically, a citizen of the United States is here using a perfective form of the verb. What is the reason for this seemingly bizarre use of the phrase "I've got" with the meaning "I have?" The answer, I think, is that logically you cannot have something unless you have first got it.

To skip to a different European language, the ancient Romans often said "vixit" for "he is dead" instead of the more literal "mortuus est." Literally, of course, vixit means "he has lived." Why did they say "he has lived" when they meant "he is dead?" Because, as the British now put it, "he had had it!" Once again the locution really proves to be quite logical in the sense that someone who has finished living must be dead.

As regards numerals: Leakey is right again in stating that numerals in many African languages come from quite divergent origins and, initially, had quite distinct meanings. However this is equally true of many European numerals.

Contrast, for example, Latin *unus* with related *quattuor*: *unus* is declinable, while *quattuor* is indeclinable. Or, for that matter, consider English: we say "10", or "20" but "a hundred". In other words, we use adjectives for most numerals but nouns for a few. At one time, in fact, we used a noun for 20, calling it "a score." Furthermore, if you delve into the literal meanings of words such as "twelve," you will find that 12 morphemically, means "two left" or "two remaining": that is to say, two in addition to 10. However, now that the word twelve has come to be treated as both a formal and a semantic unit, we no longer realize how anomalous it is in comparison with simpler numerals such as, for example, "nine."

Leakey has also raised the question of vocabulary deficiencies in African languages. Let us say, for purposes of argumentation, that a future Pan-African Congress decides to adopt Swahili as the Pan-African language. How adequate to modern needs would its vocabulary be?

Certainly Leakey is right, I think, in stating that at present it isn't easy to express concepts astrophysical or even political in Swahili. However, if you think about it historically, all of the major European languages were at one time lexically deficient in these areas and yet did develop a vocabulary for even the most difficult concepts out of the words for similar concepts that they already had. Either they drew these from their own lexicons, as a modern German does when he calls a telephone a *Fernsprecher* (literally, a "far-speaker"), or when they borrow them from some other lexicon, as we do when we use the word "telephone", which means "far sound" in Greek. Such improvisations can be made, although from the practical point of view I admit with Leakey that this adjustment would take time and that Africans are now understandably impatient, and for that reason they do not wish to spend even as much as one generation in developing a vocabulary of their own. Many of them prefer to have a vocabulary ready-made. In English, of course, they have just this: at least for most practical purposes.

A final point—one that Leakey brought up before the meeting began and, to my surprise, did not repeat in his later talk—is that of the point of origin of the proto-Bantu language.

Most American linguists, following Joseph Greenberg, now believe that the linguistic homeland of the Bantu (which, it should be stressed, need not be the same as the racial or cultural homeland) was probably in Eastern Nigeria. The reason they think this is that the non-Bantu languages most closely related to Bantu are found on both sides of the Benue River of Nigeria, with some overlap into the Cameroons. Leakey's objection to this thesis, as I understand it, is that the languages that are today formally and phonologically closest to reconstructed proto-Bantu are found in East Africa, which seems to him to constitute a strong argument for the east African origin of the Bantu. However, to me this argument seems to be a *non sequitur*. If you consider languages that we are more familiar with—for example the Scandinavian languages of Europe—it is undeniable that modern Icelandic is the closest to old Norse and to proto-Scandinavian. Nevertheless we know historically that the Icelanders are products of one of the most recent Scandinavian migrations; more specifically, that they came from Norway. Although linguistically they

are the most conservative of all the Scandinavians, it would seem, on the contrary, that "migrationaly" they have been the most radical. The same was probably true, I think, of the Bantu.

ABSOLOM VILAKAZI (*Hartford Seminary Foundation, Hartford, Conn.*): I am particularly interested in Schwab's paper because it deals with the whole problem of conflict and cohesion in cultures. I have had much difficulty about this and it seems to me that the problem requires clarification. I remember, for instance, that Gluckman has used the same concept of conflict when discussing the Zulu situation.

The problem for me has been: What does cohere because of conflict? Gluckman's description, for instance, when he talks about the Zulu situation and the conflicts there, seems to overlook (rather surprisingly for Gluckman because he knows South Africa very well) the fact that the cohesion there is a result not of conflict but of the police, who would not allow any kind of breakdown in white-African relationships. The recent Sharpville massacres and the Pondoland incidents amply demonstrate this.

Here, cohesion is not the function of the conflict; it is the function of an outside force. In fact, conflict as such is disruptive in the South African situation.

I am also interested in this because of experiences in my own family where there were conflicts that arose from witchcraft accusations. These accusations broke the family. They did not lead to cohesion. The accused members left the group.

In some case studies that I made among the Nyuswa there was conflict between a man and his family because he loved his wife and was demonstrative about it. A man is expected to prefer his kinship group to his wife. This man committed suicide because the pressures of the family were so great that he could not go on living. There are many cases that can be cited, from Zulu material at any rate, where conflict has led not to cohesion but to desertions and breakdowns of families and of marriages.

It seems to me that here perhaps we need a little more refinement of the whole concept of conflict. How much conflict can an institution take? How much conflict can one stand before one breaks down? What sort of conflict is supposed to bring cohesion? What, in fact, does cohere?

This is one thing that seems to me to need attention. Finally, may I comment upon the matter raised by L. S. B. Leakey, that is, the problem of studying language from inside. I agree entirely with Prescott because, being a Zulu, I studied all the courses that were available in Zulu at our University. However, all I studied was grammar.

I took only one course in linguistics with Gleason in the United States, and that made it possible for me to analyze Zulu better than all the other courses in Zulu could ever have done.

Again, on the point of being a Zulu and studying the language from inside, may I say that there are certain things that I did not see. I think that one has blind spots in linguistics just as one has blind spots in culture if one belongs there, and I believe that there is great merit in a foreigner's coming in to study a language without any preconceived ideas.