Reminiscences by Pike on Early American Anthropological Linguistics

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COMMENT FROM THE EDITOR OF SILEWP

This is the last academic paper that Ken Pike wrote shortly before his death on December 31, 2000 at age 88. In October 1999 the editor of American Anthropologist had asked Pike if he would write a “historical essay” on linguistic anthropology for consideration for publication in the journal. Pike began work on this essay in early 2000 and submitted a final version to the AA editor on November 8, 2000. The editor’s review committee rejected that version of the manuscript saying it was too “folksy,” but wrote back with editorial suggestions and an invitation to resubmit. Pike’s untimely death came before he could revise the essay. While his essay published here may be too folksy for a theoretical science journal, those interested in the history of American linguistics and Pike’s role in it will find that his reminiscences provide insightful glimpses into the history of linguistics.

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Abstract [by the Editor of SILEWP]

In this essay, Pike reviews where, in his view, linguistic anthropology was in the past, where it is now, and where it may lead in the future. He describes how he got into linguistics in the 1930s, and then reminisces about his personal interactions with the godfathers of American structural linguistics: Bloomfield, Sapir, Fries, Bloch, Hockett, Nida, Swadesh, Trager, Voegelin, and others. He describes how his theory of tagmemics evolved, as well as his emic/etic concept; and he tells of the abrupt changes that came in American linguistics with the rise of Chomsky and transformational linguistics.

One advantage of growing old is that as one ages one gains a better diachronic perspective of one’s discipline. In my case I get to have a longer view of the history of the American Anthropological Association than most AAA members, and especially of its fourth field, linguistics. [Editor’s comment: Pike was a member of the AAA and had planned to publish this in an anthropology journal. The reader should keep in mind that Pike was writing here mainly to anthropologists, not just linguists.] I enjoy that view. And I remember interacting personally with some of the forefathers of
American linguistics way back before most of today’s AAA members were born. I thus determined in late 1999 to write a historical essay reminiscing about highlights and historical figures in linguistics and in linguistic anthropology. This treatise is not meant to be a survey of the discipline of American linguistics. Rather it is an attempt to summarize some of my own memories of early American linguists [and a few American anthropologists and British linguists] whom I knew personally. As I write here, I try to place these recollections in the context of the discipline of the past sixty-four years.

**Early Mentors**

**Cameron Townsend (1896–1982), Elbert McCreery (1877–1955) and Eugene Nida (1917–)**

Of all my mentors, William Cameron Townsend was the one who set the underlying direction of my life for sixty-plus years. Townsend was not a professional linguist himself, although he had been studying Indian languages and doing Bible translation in Guatemala since 1917. Yet he urged me into linguistics, Bible translation, and community service. In 1935 I attended Townsend’s second session of his small summer training institute (called “Camp Wycliffe” that summer, and from 1936 on, the “Summer Institute of Linguistics”). Townsend’s central content was teaching us how to analyze the grammar of an unwritten language, and to translate into it. His basic model was the Cakchiquel (Mayan) language of Guatemala. He had gone to Guatemala to sell Bibles when he was 21, leaving college before finishing, and decided to stay on to help the people who needed it—who needed alphabets, schools, business possibilities, and social esteem from appreciation of their language.

Townsend’s grammatical theoretical presentation, which left a deep impression on me in 1935, was a metaphor in the form of a physical model. (See my “Foreword” to *Mayan Studies I* edited by Benjamin Elson [1961].) It was a sheet of cardboard with a row of slots (or ‘windows’) cut into it, one for each of the classes of affixes, and one for the verb or noun stem. A strip of paper was prepared for each slot (position); and each of these strips had written on it a specific list of morphemes that was appropriate to that slot. By lowering or raising a combination of the respective vertical strips he was able to show through the cardboard windows any one of thousands of theoretically possible complex verb forms. It was Townsend’s presentation here that helped me to develop my “slot-class” tagmemic theory later on (Pike 1967, 1982).

Another enormous impact on me was a ten-day series of lectures in 1935 by Elbert McCreery (of Biola University) on general phonetics, following his missionary work abroad. I do not recall what his sources were, but the impact on me was to be felt by
my students for the next several decades. In the fall of 1935 I went with Townsend to Mexico, and chose to work on the Mixtec language (which I mention below in relation to tone). The following summer, 1936, Townsend brought me back to his “Summer Institute of Linguistics” school in Sulfur Springs, Arkansas, to teach phonetics—which I continued to do for some years (including my dissertation on Phonetics, published in 1943), until my sister Eunice Pike took over that part of my task. During that summer, Eugene Nida (mentioned below in relation to Bloomfield’s book) started as a student, but because of his training in linguistics in California, he was asked to help teach morphology later in the summer. At the end of the summer Townsend urged that the three of us (Townsend, Nida, Pike) write books on linguistics! I did not like the idea. (I was then 24, and my experience was not that great!) Eventually we did our assigned tasks. Nida wrote the important book Morphology (1946, second edition 1949)—which he used as a textbook when he taught with us for some years at SIL’s linguistic courses at the University of Oklahoma. Townsend wrote his book (Townsend 1961). In the meantime, hoping to avoid my assigned task, I chose to return to the Mixtec fieldwork area in Mexico. On the way there, in the fall of 1936, I broke my leg trying to help my Indian friends unload corn from a train to a warehouse. It took two days for my local friends to send me to a hospital in Puebla, where I eventually woke up to find my leg resting in a basket. Not sure whether it was God or Townsend who had put me in this position, I decided to get to work on my assigned phonetics book. Unable to get out of my hospital bed, with my leg now in a cast and tied to a pulley, and suffering from malaria, I wrote phonetics eight hours a day for the next several weeks.

In the spring of 1937, using a cane to start with, I walked for a month from the highlands of the Mixtec area (8,300 feet above sea level) to the coast, to survey other Mixtec-related linguistic needs on the way.

In the summer of 1937 Townsend sent me off to Michigan to study with Edward Sapir. Beginning in Guatemala, Townsend had for years been making friends with leading scholars and government officials (including corresponding with Sapir). And in 1937 in Mexico he was pushing us to write and publish linguistic articles (see two, of that date, in my bibliography edited by Brend 1987), and to attend linguistic conferences.

The Sapir–Bloomfield Era

Edward Sapir (1884–1939)

In the summer of 1937, I studied with Sapir at the Linguistics Institute of the Linguistic Society of America being held at the University of Michigan. At that time I
was a young, 25-year-old naïve beginner, with only one summer of previous linguistic training in 1935, and two years of fieldwork study of the Mixtec language in southern Mexico. In the first hour of hearing Mixtec, working through an interpreter, I knew that the language was tonal—the numbers one and nine differed by pitch only. But after two years of intense study of that language, I could still not tell how many contrastive pitches there were. Then, in one “coffee cup” session with Sapir in 1937, there in Michigan, he showed me how he had analyzed the tones of Navajo, using frames: The Navajo word ni with high tone meant “he said”—and nothing went higher in any sentence with it. The word for “horse”, following “he said”, was as high in tone pitch as “he said”, but the word for “grass”, following “he said”, went lower. So it was clear that there were contrasting high and low tones in Navajo speech. At the end of my summer with Sapir, I went back to the Mixtec area, tried the technique, and it worked—Mixtec turned out to have three tones. And across the state, the language of the Mazatec people, where my sister Eunice worked with Florence Hansen, had four tones. I am deeply indebted to Sapir for that help so long ago, a field research technique that I have since passed on, directly or indirectly, to hundreds of students over a half-century of teaching.

Sapir taught me more than linguistics. I looked to him as a role model in handling personal relations. He was kindly to beginners. For example, one day I came into the building where he was seated. I sat down beside him, and started to show him the list of words in English, which I had just received from Bernard Bloch (who was teaching us the structure of English vowels at the LSA Institute). He took the sheet, put his finger on the first word, and said, “‘Father’—now how would I say that word?” And it was through Sapir’s article “Sound Patterns in Language”, published in 1925 in the inaugural issue of Language (the flagship journal of the Linguistic Society of America, of which he was a crucial founder) that I learned to appreciate the important significance of relationships among sounds, rather than just their objective features. This became the basic philosophical foundation for my understanding of the difference between phonetics and phonemics. A phoneme, for a particular language, is considered here as psychologically a reality for the native speaker of that language. But the view of general phonetics as an approach to understanding the total list of phonetic items in the world does not have that kind of psychological “local” status.

**Leonard Bloomfield (1887–1949)**

However, it wasn’t from Sapir that I first heard the word “phoneme”. That term came to me in 1936 out of Leonard Bloomfield’s classic book *Language* (1933). Eugene Nida, a student at SIL’s 1936 linguistic course in Sulfur Springs put the book into my hands that summer. But my understanding of the concept grew only after I had read Sapir’s “Sound Patterns” article. Bloomfield’s book was more surprising to me because of the elegant way in which he described the development of historical
linguistics, of which I was ignorant. His handling of morphological structure was also important to me. The whole book was a heuristic surprise to my initial thinking about how language worked.

In terms of personal relations, Bloomfield seemed to be very different from Sapir. Whereas Sapir frequently commented publicly on topics presented at the summer meeting of the Linguistic Society in Ann Arbor, Bloomfield rarely did so—and only when an important theoretical point was at issue. One linguist told me that Bloomfield was living in his home for the summer, but had instructed him not to invite visitors. Bloomfield was much more reserved, socially, than Sapir. What a surprise, therefore, when Bloomfield invited me to stay over night in his home in Chicago, when I went on a train with him. (This was in June 1938 when I was en route to Arkansas for my fourth summer at SIL). He was following a request Charles Fries made to him to consider helping me to get credit for the summer’s class in Ann Arbor, even though I had to leave early. Bloomfield asked me about Mixtec, and my work with SIL; and he requested that I write to him about my research on Mixtec as a way to complete the Ann Arbor course and satisfy Fries.

Bloomfield also encouraged me about a book I was writing on intonation. He wrote a letter in 1943 (quoted in Pike 1989:220), saying that “the book is beautiful and it is the first thing on intonation that I have read with any interest or profit.” As a young aspiring linguist, one can imagine how this encouraged me in my budding career. Some time later, when I was in Bloch’s office at Yale, Bloomfield came in, and suggested to Bloch that he publish my intonation volume (1945). Bloch refused. (He apparently preferred the work of Rulon Wells.) Bloomfield had known of my Christian commitment, and although his book Language had appeared to be behavioristic in scientific approach, he one day stated in an LSA meeting: “We have no objection to someone saying that there has been a revelation from God, but we must handle our linguistic science separately.” In 1945 I met Bloomfield for the last time. (He died in 1949 at age 62.) We were dining at the faculty club at Yale. But during this dinner, he leaned forward in his chair, and said: “If when I die I find that the Roman Catholic priest is right, I’m going to laugh!” He said this of himself, seriously, in a potentially humorous context, to a Christian student. I enjoyed seeing that humorous side of Bloomfield, who was a Jew. And this brings me to mention here an interesting question that arose in my mind in those days: How was it possible that so many of my great teachers (e.g., Sapir, Bloomfield, Bloch, Trager) were Jews? My own guess was that some centuries before my memory span, the Jews were not allowed to own big property, nor to play an important role in politics—but no one objected to their studying at universities—so linguistics and other academic fields were some of their outlets, to which I am indebted so deeply. There was some prejudice against Jewish professors in those days. I occasionally heard students make
offhand bigoted remarks against them, and it made me uncomfortable. (Marvin Harris [1999:71] also mentions this prejudice against Jews in his new book, and that major universities did oppose admitting Jews in the 1920s.)

**At the University of Michigan**

**Charles Fries (1887–1967)**

As Townsend was my mentor for my work in SIL, Fries was my mentor for my work at the University of Michigan. He arranged for me to do my doctoral studies in the summer sessions of the LSA there, since I could not leave my SIL work in Mexico in the winter. (Students were not allowed to do their doctorates in the summers only, since many of the most experienced faculty members would be absent—but in Michigan, because of the summer sessions of the LSA, many special faculty were present.) Fries had me present my work on tone and phonetics to faculty seminars, which led to further help for me from that faculty. He arranged for the Michigan publication of several of my books, when I did not have another outlet for them. When I was on furlough from Mexico, he had me work on phonetics and later on intonation, for the English Language Institute. He arranged several grants in relation to these projects. We published an article together (Fries and Pike 1949) on “Coexistent Phonemic Systems”. I am deeply indebted to him in these academic and personal ways. In gratitude, I published a poem about him in 1968 (Pike 1968, see now in Heimbach 1997:4.201), after his death:

**A Great Shade Tree Has Fallen**

A great shade tree  
Has fallen low.  
Yet high it rose  
O’er battle shout  
And lonely vigil kept.

Wail, starting sprouts—  
Grow ye must,  
Fill forest gap  
With shadow young and straight,  
Grow lumber tall,

While cities waiting strength  
Crave knotless planks.  
Age gave seed formed
In cones torn from limbs
By winds blown high in trees.

Leslie White (1900–1975)

In 1948 I was appointed as an associate professor in the departments of Anthropology and English at the University of Michigan. (Later, the Michigan Linguistics Department was formed, and I moved from English over to that.) Leslie White was the chairman of the Anthropology Department. Whereas I was a theist, he was a mechanist. My wife and I had him over to our house for dinner one evening in the late ’40s, where he and I enjoyed talking freely about things where we differed, but were both interested. White said to me something to the effect that “We have no free choice except in the bathroom.” To this I replied, “How, then, can you expect students to choose voluntarily that your teaching is to be accepted as valid or true?” His reply to me was: “What is truth?” To answer that question I responded: “Truth, for me, has to be in conclusions resulting from the careful testing of observations using what equipment and context might be available. For example,” I said, “it would mean that if some population could see only in straight lines, then they would see the sun appearing on the horizon, pass above them in a straight line, disappear, only to reappear the next day doing the same thing. If all other testing confirmed that observation, then that would be truth for such individuals.” White said that he liked such a statement very much, and would use it.

E. Adelaide Hahn (1893–1967)

My wife Evelyn and I first met Prof. E. Adelaide Hahn in one of the summer sessions of the Linguistic Society of America (LSA) in Ann Arbor, Michigan at the University of Michigan. This was in the early 1940s. She was a vibrant, enthusiastic, highly competent classicist, also involved in research on the Hittite language. She was on the faculty of Hunter College in New York City. She was in the organizing meeting in December 1924 of the LSA, and was elected president of the LSA in 1945.

We were intrigued and delighted with her presentation at a summer session of the LSA at UT Austin in 1961. She was discussing the genitives in Greek. She listed illustration after illustration together with where each was to be found in the literature during that lecture. Evelyn recognized the ones that she quoted from the New Testament, so we were sure that all her sources were just as accurate. However, if she had any notes at all, they must have been on the ceiling, because that was the only place she looked when she wasn’t looking directly at her audience!

Another time in Ann Arbor a phonetician was giving a paper and to emphasize his point he played a tape of some ten utterances of phonetic data, asking the audience to
write down what they heard. When the dictation was finished, he gave the answers. No sooner had he finished than Prof. Hahn called out, “Gordon, you have mixed up your tapes. I’m not used to getting so many things wrong! [because his answers did not match those which she had written down].” Yes, the next day Gordon apologized to everyone; he had indeed mixed up his tapes!

**J. R. Firth (1890–1960)**

I knew Firth both in England, when I was lecturing there, and at the University of Michigan, where he was lecturing. Two personal items stand out in Evelyn’s and my memories. In England I walked into one of his classrooms, and was talking to his students or colleagues. He walked in, and appeared a bit uneasy because I had not checked in with him first. I told him that I had been saying to his students that I felt that a hierarchical approach needed to go higher—sentence was not enough, paragraph was not enough, etc. He then said, “OK, if you want to go half way to heaven, Pike!” To which I replied: “What’s wrong with that?” In Michigan, he was speaking to a local audience, discussing how phonology spread more widely than the word; and they had difficulty following him. I stood up and tried to explain Firth’s position to them. Firth then said to me: “That’s right, Pike, you are the only one here who understands me!” We continued to be friends.

In my 1967 book I included dozens of references—see the index—to his work, more than most other scholars (and I approved of his dealing with hierarchy beyond the word). For example (see reference in Pike 1967:619), he treats meaning as being on a “series of levels” (Firth 1951:76) where the meaning “of the whole event is dispersed and dealt with by a hierarchy of linguistic techniques descending from social contextualization to phonology.” Also (in Pike 1967:616, Firth 1937:126) he affirms that words “mean what they do”, being both “affecting and effective” in meaning. Hierarchy also affected phonological theory, for him. He states (in Pike 1967:415, Firth 1949:129) that “we may abstract those features which mark word or syllable initials and word or syllable finals or word junctions from the word, piece, or sentence, and regard them syntagmatically as prosodies, distinct from the phonemic constituents which are referred to as units of consonant and vowel systems.”

**M. A. K. Halliday, (1925–)**

In my view, Halliday was very much a successor to Firth. In various ways, however, many of his principles can be seen as related to some of mine. My most extensive discussion of that fact can be seen in my book *Language* (1967:506–509), in a section entitled “On Halliday’s Prosodic Approach to Grammar”. (I will be drawing heavily on that section, for my comments here.) In his1961 article (p.247), Halliday refers to fundamental categories as including “unit”, “structure”, “class”, and “system”. They
are comparable to tagmemic terms, except that by “unit” he implies “syntagmatic level”, rather than definition by way of tagmemic contrast, variation, and distribution.

We would differ, in that I would handle phonetic variants of a phoneme as all parts of that same unit—so that the aspirated [t] in the first syllable of my American pronunciation of “tatter” would differ from the flapped unaspirated [t] of my second syllable of that word—and he would not grant a single phoneme unit for the two. When I deal with a phoneme from the perspectives of particle, wave, and field, I leave room (in the wave perspective — compare his and Firth’s prosodies) for features, which spread, from one phoneme to give partial overlap with another. Halliday also goes beyond language, for example in discussing the analysis of a meal (1961:278–279), with a “mouthful” as a “gastronomic morpheme”. Compare my extensive analysis of a recorded breakfast meal in my book *Language* (1967:122–128).

**The Bloch–Trager Era and later**

**Bernard Bloch (1907–1965)**

I have indicated above that I profited in studying about American vowel structure with Bloch in Michigan in the summer of 1937, but that he did not want to publish my intonation materials in the Language series. Similarly, he did not want to publish my tone volume since, as he told me, he did not like my sources about Japanese pitch. He also later rejected my essay “Grammatical Prerequisites to Phonemic Analysis”, which clashed with the Bloch/Trager search for science via the autonomous analysis of different levels of language structure. (I felt that they could not be handled separately.) So I submitted the essay to the new society founded by (among others) Roman Jakobson and Andre Martinet. Jakobson said that they founded the new society (International Linguistics Association) because Bloch rejected his article on poetry—saying something to the effect that poetry was not appropriate for scientific study. So the new society published my essay in *Word* (1947b) (where it became one of the most widely cited of my earlier publications).

**George Trager (1906–1992)**

Added to his rejection of overlapping levels of phoneme and word in scientific description, Trager—like other theoretical academicians of those days—wanted scientific research, as such, strictly separated from practical language learning or analysis. This showed up in his very critical review (1950) of my book *Phonemics: A Technic [sic] for Reducing Language to Writing* (1947a). He said (p. 158) that he as reviewer “must therefore condemn the book as a theoretical work, and even more as a
textbook—since as the latter it will lead astray many who might otherwise be valuable workers in linguistic science.”

But our differences of theoretical viewpoint did not prevent Trager and me from having both an academic and personal interlocking friendship. One summer, for example, about 1947 at SIL (now moved from Arkansas to the University of Oklahoma), I had Trager give two lectures, since I wanted our students to know the principle linguists of the time, in order to learn important approaches from them even when they had different presuppositions or methodology from mine.

Morris Swadesh (1909–1967)

When Townsend sent Sapir some of my materials, Sapir responded by sending me some articles by Swadesh. I found Swadesh’s discussion of the pedagogy of phonemics (1934, and 1937 on long consonants) very helpful. On the other hand, when Swadesh went to Mexico, and got involved in applied linguistics there, I could not accept his view about [practical] alphabets [for indigenous languages], where—if I understood him correctly—he favored writing in a classical phonetic form rather than in an alphabet adaptable to Spanish orthography.

Zellig S. Harris (1909–1992)

Although Zellig Harris and I had some interaction many years ago, I did not know him well. I drew from his work, however, and I remain grateful to him for his contribution to linguistics. In fact, I was astonished recently when I was checking my references to Harris in my Language in Relation to a Unified Theory of the Structure of Human Behavior (1967). In the index there were some 50 references to him—more than to almost any other scholar! Many of these were brief mentions in relation to the work of others, but a number included relevant quotes.

Harris was a teacher of important students, including three of my colleagues from SIL (Sarah C. Gudschinsky, Robert E. Longacre, and Richard S. Pittman), Noam Chomsky, and others. Harris early (1952) published on discourse structure, even though he considered that some of the information “goes beyond descriptive linguistics” (1952:1) in that it was “beyond the limits of a single sentence at a time.” This he considered to be one of two types of problems. The other “is the question of correlating ‘culture’ and language” (i.e., nonlinguistic and linguistic behavior). This problem led in 1948 to my developing a holistic view, to include all of human behavior, resulting in my material of 1967, discussed below.

Harris also thought of structure as a network of relations (1954:149). He considered that the grammar as a whole consists of a kernel. These made up of a “set of
elementary sentences and combiners, such that all the sentences of the language are obtained from one or more kernel sentences (with combiners) by means of one or more transformations” (Harris 1957:335).

Archibald A. Hill (1902–1992)

Hill was interested in voice quality, as I have been, but he felt (in Allen 1958:21) that these elements were “not parts of larger metalinguistic structures.” He was also interested in various poetic elements, but felt (1955:973) “that rhyme and alliteration are structures correlated with language but not a part of it.” He was also interested in other items under discussion at the time, such as the validity of movable units of language, such that (Hill 1962:345) it is “quite obvious that the formulae for ‘deriving’ a passive sentence from an active one could not be stated as a manipulation unless the active sentence contained isolable items capable of being manipulated.”

Charles F. Voegelin (1906–1986)

Voegelin became the editor of the International Journal of American Linguistics in 1944 after the death of its founder Boas. In that role he continued to keep up the interest in the descriptive analysis of American Indian languages. (It was a delight to me to find in Voegelin’s first issue of the journal my article on the “Analysis of a Mixteco Text” [1944:10.113–138].) By 1952 Voegelin and Harris (1952:325) felt that linguistic descriptive tools were “uniquely fitted to the data of language, rather than to culture in general.” (This was at the very time I was drafting the first volume (1954) of the early edition of my unified theory (Pike 1967).

Mary R. Haas (1910–1996)

The work of Haas is of special interest to me, since she did her doctoral dissertation in 1935 on an American Indian language (Tunica; see Haas 1941), the same year I started on my study in Mexico on Mixtec; and she, also, studied with Sapir and Bloomfield. Later she worked on Thai, and its tones (e.g. Haas 1958)—which also interested me, because of my struggles with the tones of Mixtec and other American languages. Still later, she worked on historical linguistics, beyond my own studies. (For further discussion of her work, see Pike 1998.)

Floyd G. Lounsbury (1914–1998) and Ward H. Goodenough (1919–)

Lounsbury (1956:193–195) dealt with the structure of semantic fields (in relation to the universe of discourse), in defining the meaning of words. Such a field may be subdivided by one or more variables. This, which I approved of, brought semantics and discourse into attention before many scholars drew it on. That same year,
Lounsbury tried to utilize kin structure by using an analogy to relate phones to kinsmen, or kin type, or kin class—relating them to phonology rather than to grammar. This approach interested me, since it carried some relation to my then-developing unified theory (Pike 1967).

Goodenough, like Lounsbury, was interested (1951:64, 1956:195, and 1957:169) in the analysis of kinship in a larger cultural setting, relating it to procedures of linguistic analysis.

**Charles Hockett (1916–2000)**

I met Hockett, as I did many other linguists, at the summer linguistic meetings at Ann Arbor. It was probably in 1938 that I was asked to give an informal talk about phonetics. (I had disagreed with Trager about some phonetic principles.) At the end, Hockett, who was at the meeting, asked me if he could have a copy of my talk. I told him yes. Unfortunately, for me, I had not prepared a written copy. So I left Michigan, and started to try to write up the material. But it took longer and longer—and required very much reading of the available literature, in the States, and in Mexico.

After several months, I had a draft of the material. I took it to Michigan. Prof. Fries had me talk about it to an advanced seminar (including various faculty). Fries then suggested that I write my dissertation on phonetics instead of on tone, as I had intended. I did—with the resulting 1943 book *Phonetics: A Critical Analysis of Phonetic Theory and a Technique for the Practical Description of Sounds*.

**Martin Joos (1907–1978)**

My impression was that Joos occupied a theoretical position comparable to that of Bloch and Trager. Specifically, for example, I mentioned above that Bloch had rejected my article (1947b) on “Grammatical Prerequisites to Phonemic Analysis”. Joos (1957:96, in an added editorial note) referred to my view there as “the ghost of the slain dragon” which “continued to plague the community of linguists.”

On occasion, however, he made suggestions to me that I thought were very interesting. Once, for example, he suggested that even the general height of the voice may prove to be discretely split into four ‘kinds’ of levels which I later came to call relaxed, normal, intense, and excited.

**Dell Hymes (1927–)**

Hymes, an anthropologist, reviewed my 1967 book in *American Anthropologist* (1969). He was one of the first to show interest and approval of my
attempt to deal with the structural analysis of culture beyond linguistics. That pleased me—although he was disappointed that “the work [Pike’s 1960 third volume] concludes by falling back upon language and society as separate, parallel structures, the very approach that the opening chapters had sought to transcend” (Hymes 1969).

In a more recent work by Hymes and Fought (1981), we have an extensive history of American Structuralism. (It goes far beyond the detail which I have given here.) It was an encouragement to me (after they pointed out that much of my material was somewhat ignored at that period) when they added that: “the more one insisted, as did Pike, that native speaker reactions, judgments, were essential tests of validity, the more one would want theory to be consistent with practice (as did Pike).”

**Marvin Harris (1927–) and Pike’s Etics and Emics**

In the first 1954 volume of the early edition of my *Language* (1967), I took the well-known terms “phonetics” and “phonemics” and generalized on them by deleting the first syllable “phon”, about sounds, and using the balance of the terms to relate to all language and human behavior, as well as to mental or social relations in a culture. I developed the terms *etic* and *emic* in order to help to capture the essential academic difference between a general approach to an etic system covering all structures of the world, and the analysis of some specific emic local structure of some particular language or culture. The emic/etic concept today has become very widely used in many disciplines, textbooks, encyclopedias, dictionaries, and anthropological descriptions (although often without reference to the origin of the terms or the concept). In some instances, also, the emic/etic terms are described differently, or are used differently, from what I had originally intended (Headland 1990).

Native reaction (both to and by language and nonverbal behavior) is important to identifying emic material relevant to a person’s cultural structure. But scholars may analyze such systems differently. One general approach might be called “psycholinguistics”, which does not seem to tie the parts as closely together as we wish to do in our “unified” theory of human behavior. (For a discussion of variants of this last approach, with some bibliographical references, see Pike 1967:351–354.)

Clearly, the most saliently different use of the emic and etic terms was developed in the 1960s by Marvin Harris (1964), the leading theoretician in materialist anthropology today. Harris appears to consider the emic term to be more mental, and the etic term more physical. Harris and I constructively worked out some of our differences in meaning and use of the emic/etic concept in a public debate before 600 anthropologists at the annual meeting of the American Anthropological Association in 1988 (published in Headland, Pike, and Harris 1990). Recently, Harris (1999:31-48) has made efforts to further clarify his use of emics and etics, and recognized there (p.
37) that he had misread me for decades. Harris and I still do not conceptualize emics and etics in the same way, as he makes clear in his new book (1999:32, 37). For example, as I read him, he continues to equate emic and etic with ideal versus real behavior (ibid., p. 44, 45). Still, I am glad to see that the concept has been so helpful in anthropology today. It has helped cultural materialists in their theory building for over a quarter century. I never imagined that the concept would lead others in such a direction; but I am rather satisfied that it did.

The work of etics and emics by Kenneth Pike was supplemented for phonetics by Eunice Pike and for grammar by Evelyn Pike. In 1955 my sister Eunice and I, in a small booklet titled *Live Issues in Descriptive Linguistic Analysis*, tried to survey (for graduate students) the scholars and problems discussed during the times of the two eras mentioned here (the Sapir-Bloomfield era and the Bloch-Trager era). As an introduction to each bibliographical section, we asked questions to try to highlight issues involved. A bit later, in a second edition (Pike and Pike 1960), we added more items involving publications from the Chomsky era, plus some other scholars and their publications from earlier times and from European sources.

My wife, Evelyn, and I published our classroom textbook *Grammatical Analysis*, (Pike and Pike 1977, revised 1982) and in 1983 we published our *Text and Tagmeme* to try to present to our students our understanding of the relation of grammatical to referential (content) structures (illustrated by a text), as well as the phonological hierarchical analysis of a poem from sound to text, plus a tetrahedron model for seeing the application of a matrix in practical description. Before that I published my *Linguistic Concepts: An Introduction to Tagmemics* (1982) to help students have access to a general summary (hopefully intelligible to them) of tagmemic theory.

**Noam Chomsky (1928–)**

In 1957 a young linguist named Noam Chomsky published a book which dramatically changed the face of descriptive linguistics in America. Chomsky emphasized formalism in terms of transformational grammar. He focused in his first book only on the sentence or below, and ignored semantics or discourse structure. I have never controlled Chomsky’s approach in detail, and shall thus leave its discussion to others, including the addition of semantics or other features. I enjoyed, however, having Chomsky speak to my linguistics seminar at Michigan. I believe this was about 1964. He was pleasant to talk with, and I very much wanted my students to be acquainted with him as a person and as an innovative theoretician. I could not, however, adopt his basic starting point for mine, since I was working with unwritten languages, with no one to suggest new and unwritten sentences; and I saw no way to ignore working from a holistic viewpoint as a way to get into (and live within) the language and culture of a
preliterate people. (In a related way, the philosopher Sinclair once said [1944:129] that explanation shows how “the previously unrelated fact or situation falls into a place in the pattern.” I should say here that I have various SIL colleagues who have built components of some variety of transformational grammar into their own holistic approaches.)

**Major Shifts in Twentieth Century American Anthropology**

I have reviewed here many interesting historical events that I have personally seen or experienced in the academy during my 65 years as an American linguistic anthropologist. But what do I see as being major movements since I first took up linguistics in 1935? There have been two major changes in my subfield of American anthropology during my academic career, as I see it. Both were uncomfortable for me. The first shift was the movement of linguistics away from the other three fields of anthropology. That became salient to me in the 1960s. I am glad to see the American Anthropological Association’s emphasis today to try to bring linguistics back under the old four-field umbrella, where I think it belongs. [Editor’s comment: Pike is here referring to the “four-fields” of American anthropology—cultural anthropology, physical anthropology, archaeology, and linguistics—as anthropology was practiced until linguistics moved away to become a separate discipline in the middle of the twentieth century. Today linguistics is being increasingly brought back as a part of American anthropology.]

A second major change was the paradigm shift in linguistics from descriptive (or structural) linguistics to Chomskyan transformational linguistics. While this was good for anthropological linguistics—all linguistics is anthropological, by the way—it was unsettling for me personally because I came out of the Bloomfieldian descriptive linguistics school, and especially because the transformational revolution shoved my own tagmemics theory to the back-burner. A humbling experience for me, but not surprising when we think of Thomas Kuhn’s (1970) model.

**The Condition of American Anthropology Today?**

I end the above subtitle with a question mark, because while I can see far into the past where anthropology has come from, and especially its fourth field (linguistics) I struggle as I try to interpret the anthropology academy’s current and future events. For me, at least, the most salient movement in American anthropology in the last decade has been Postmodernism.
While deconstructionism and postmodernist ideas were beginning to influence American anthropology before the 1990s, the growing strength of the movement didn’t really impact anthropology, as I see it, until the last decade. Or at least most “modern” anthropologists didn’t realize they were under attack until then. It is still going on in anthropology, though perhaps less obviously now than it was in the early ’90s.

This is not the place to review Postmodernist anthropology. Suffice it to say here, it appears to me that it is not only changing the way anthropology is viewed by the public but also the way anthropologists practice their trade. I have heard numerous colleagues over the past five years say that they think American anthropology is in a major crisis today. I hope they are wrong, but I fear they may be right. I admit I am especially depressed about this as I write the final words of this essay, because of the hundreds of worldwide news stories published in recent days against anthropologists. I have never seen anthropology take such a beating in the press as it has this month (November 2000). These stinging news reports concern a book published just this month (November) by journalist Patrick Tierney (2000) accusing certain anthropologists of crimes and human rights violations against an indigenous group in the Amazon. Two of the most critical stories against anthropologists in general were both published on October 8 (Cockburn 2000; Zalewski 2000). Exaggerated unfair stories, I suspect, but enough to make serious anthropologists cringe. Who knows at this point whether the story is true. But it is clear that it is not helping the public image of anthropology right now. Maybe journalists are more influenced by Postmodernism than anthropologists. [Editor’s comment: Pike was right about the seriousness of this controversy. In the weeks after he wrote this paragraph, thousands of articles appeared in newspapers, science magazines, and on the Internet about this “Darkness in El Dorado.” Readers looking for information on this will find “Doug’s Anthropological Niche” the best place to go to begin surfing on the topic http://www.anth.uconn.edu/gradstudents/dhume/darkness_in_el_dorado/index.htm.]

I objected, some time ago, to a deconstructionist approach, which seems to me to say that words have no meaning, which can be shared, with some degree of certainty, with others. So I wrote a poem objecting to this (see it, now in my 1997 poems, Heimbach ed., Vol. 2.12):

**Inkblot Poetry—A Query**

“Me, trying to say something?”
“Oh, no—you’re just the author.
Someone else must say
What’s said … Right?”
(Author’s rights
Are semantically bankrupt,
Faced by interpretive
Staked-out claims
Affirming that poems
Are pretty inkblots
Waiting readers’
Dreamy impositions! …
But reader “re-write” rights? …)

“And when you are
Saying I said—
Are you then ‘saying’
Something to me?”

“No, just throwing words
At other unhearing wordless ‘things’.”

From Now into the Future—A Pikean Dream

In a 1999 article in Scientific American on “A Unified Physics by 2050?,” Steven Weinberg says (p. 68): “One of the primary goals of physics is to understand the wonderful variety of nature in its unified way. The greatest advances …have been steps towards this goal… terrestrial and celestial mechanics by Isaac Newton… space-time geometry and the theory of gravitation by Albert Einstein… and of chemistry and atomic physics through the advent of quantum mechanics…” This jolted me into remembering my experience in 1948, when I was tired of writing books on phonology, and wanted to turn to write on grammar. But I did not want to lose my past experience in phonology, so went through my Phonemics book to see what there was there that could carry over, in general principle. I came up with items that I now call “contrast, variation and distribution”. I had no trouble seeing that [p] and [b] contrasted in “pie” and “buy”; or that “John” was distributed as subject in “John shot the dog”, or as object [of a preposition] in “The dog was shot by John”. But, I asked with difficulty, “What is the English language distributed into?” I replied to myself: “with different dialects, yes—but all of them together, into what?” And now the important jump: “All types of English are distributed into culture.”

This set the tone of my research for years. I could no longer study just phonology, or any other topic isolated from culture, when I wanted to discuss its distributional component. (I used footnotes to state that further work needed to be in done, in related matters, which should now include such distribution.) Nor could I ignore hierarchy, in
grammar, phonology, and lexicon, which drove me at least that far (and on up to
theism—see above my discussion with Firth). So in 1948 I started working on this,
revised and published together (1967) under the title of Language in Relation to a
Unified Theory of the Structure of Human Behavior. Various kinds of cultural
nonlinguistic material (but with some verbal materials often involved) were
mentioned (1967:27–32), e. g., a motion picture of a wedding, a banquet for departing
students, bargaining (in the Mazatec language) with whistles, water polo, a basketball
game, a parade, gestures; plus a football game (pp. 98–106).

These materials show that I would not be willing to stop with a separation of
phonology, grammar, and social materials with their meanings, nor to stop with some
feature or domain isolated from culture. In short, I wanted a UNIFIED theory. And
this fact has affected my view of the past developments of linguistic theory, as
discussed above. I do not want to be “neutral” or “abstract” or “domain oriented”, but
rather unified in an approach to human nature as I experience it.

Here, I would like to utilize the metaphor of a helix (Pike 1991) to keep graduate
students from being surprised, or bothered, when (1) new ideas come on the horizon
which seem to challenge their favorite professors, or (2) old ideas come back again
into unexpected but partial focus. A helix can be thought of as a vine spiraling up a
post, winding around it, so that from a certain direction it is at first seen, then lost
from sight, and then seen again, and again. In linguistics, by analogy, a new idea does
not necessarily destroy an old one, but may add to it or change observer focus for a
moment. So, here, I wish to mention several approaches to some part of the discipline
that I have enjoyed, and hope that the discipline will use such approaches in ways
beyond my own experience with them. Perhaps, also, the helix would make it easier
for students to deal with “both-and” rather than only “X versus Y”.

Linguists need to be interested in philosophical presuppositions underlying
anthropology. In the 1980s my interest focused on linguistics in relation to philosophy
(see my Talk, Thought, and Thing: The Emic Road Toward Conscious
Knowledge, 1993). There I urged students’ attention to my belief that a theory of
cross-cultural knowledge needs to focus upon personal interaction in a social-physical
context. The observer must be included in the description of all social interaction, or
of rule-oriented material. Phonological, grammatical, and referential features all affect
human behavior hierarchically. Each needs to be treated in relation to features of slot
(where?, context, position in behavioral structure), role (why?, function,
meaningfulness or relevance), class (what?, the list of alternatives with different
semantic impacts in that position), and cohesion (how controlled?, its agreement with
other items in a particular context, or its control of them or by them). The outsider
analyst or the insider mother-tongue analyst can choose at any one moment to focus
on the function of an item as if it were a separate particle, or as if it were merely a nuclear point in a smear from one unit to another (a wave), or as if it were primarily an analyzable point in an intersecting matrix of items or events or thought. A holistic view of truth requires the search for pattern within pattern within pattern of knowledge. (But basic presuppositions, in my view, come from person, not from logic—and there is an observer at the source of every such search, varying, however, if one is a theist—as I am—or a mechanist). In relation to such viewpoints, I choose to put person beyond logic (Pike 1992).

For a partial understanding of some of these relations between person and person, person and event, person and thing, person and feeling, person and ultimate reality, I would hope that poetry could in some way be helpful to the anthropologist. In this past decade, for example, Sharon Heimbach (1997) gathered a thousand of my poems in five volumes. Along with the study of poetry in its emotional relation to humanity, there is the need to be aware of the mode of physical pronunciation of poetry (e.g., its intonation, or the harshness of its voice quality), which carries much of the inter-social meaning of human nature, in its holistic relation to people and context.

The physical, geological, and social contexts can be seen interweaving in the following poem of mine (a poem in Heimbach 1997:4.202). A time change, from one day to the next, occurs at a point in the Pacific called Wake Island. I have used that point as a metaphor for the need for “passing on the baton” in scholarship, from an old person (me) to the young (you!):

**Placard at Wake Island**

“The U.S. day begins at Wake”—
Start the clock;
run the full race
to end—and early sleep.

String from
Pole to Pole
marks life in two.
“Hello Wednesday!”
“Goodbye Tuesday!”

*Someone* must begin,
lest all die.
Spin the world’s dial:
come the sun.
Warm the hands by day,
rest by night—
or care for airstrip
so others may embark.

Our day is done.
Who carries on
in wind and tropic rain
on journey round the clock?

References Cited


Harris, Marvin. 1999. Theories of Culture in Postmodern Times. Walnut Creek, California: AltaMira Press.


