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ABSTRACT

This document consists of the four issues of this quarterly linguistics and language research serial issued during 1992. Typical significant articles include: "The Indispensable Tape Recorder" (Geoffrey Hunt); "Analysis of Tone Systems" (Eunice V. Pike); "Sentence Repetition Testing for Studies of Community Bilingualism: An Introduction" (Carla F. Radloff); A Good Phonology Program" (Geoffrey Hunt); "Formal Linguistics and Field Work" (Daniel L. Everett); "Language and Communication" (John W. M. Verhaar); "Focus Shift Problem" (Howard W. Law); "Syllable-Based Hyphenation" (David Weber); "Project '95 Now? A Vision" (Arnd Strube); "Some Guidelines for Writing Linguistic Survey Papers" (2 parts) (Thomas M. Tehan); "Report on Tape Recorder Quality" (Ernie Zellmer); "A Few Practical Tips for Using CECIL" (Mike Cahill); "Terminal 2/2+ in Language Learning" (Sue Wright); "Linguistics: An In-House Thing? A Diary Entry" (John Verhaar). Conference reports, book reviews, and professional announcements are also included. (MSE)

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NOTES ON LINGUISTICS

NOTES FOR SUBSCRIBERS ■ *Notes on Linguistics* is a quarterly publication (February, May, August, November) of the Linguistics Department of the Summer Institute of Linguistics, Inc., designed to share information of practical, theoretical, administrative, or general interest. It is specifically intended to provide linguistic field workers with news, reviews, announcements and articles that will stimulate a current interest in linguistics.

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COORDINATOR'S CORNER

I trust that in this issue you will find some down-to-earth articles and reviews that are helpful.

Geoffrey Hunt's article on choosing a tape recorder examines points that we might overlook if we don't consider the prospect of using recordings in CECIL. You might already have good recordings of tone frames or flowing texts. If you use those recordings as input to CECIL to extract the pitch and stress contours only to find that the recordings have had distinctions blurred by various kinds of interference, Geoffrey's tips might help you out. If you would like to commission someone to just "pick up" a new recorder for you, by all means read Geoffrey's article first. It might spare you unfruitful expense.

Eunice Pike's article on tone comes from her wide experience as a consultant in tone analysis. One of the things that Eunice's approach does is to take advantage of known typical patterns of tone phenomena: it builds on what the world already knows. As you read, keep in mind that the phonology textbook by Burquest and Payne, a clear and extensive treatment of typical patterns of both segmental and autosegmental phonology, will receive its "final exposure" this summer at Oregon. We are looking forward to its publication.

Carla Radloff's article on the application of sentence repetition tests to measure bilingual competence arises from testing children's language capability. I subjected myself to a quick test and found that I could repeat sentences all right in languages that I knew, even when the sentences were fairly complex. But when called upon to repeat sentences in languages that I am incompetent in, my performance was dismal. If, as Carla says, the test only attempts to measure "one thin slice" of all that is desirable in evaluating second language competence, the time and expense of the sentence repetition approach can sometimes achieve extraordinary savings over other approaches and still enable one to come to valid conclusions. In this article you will see how it works.

Don't overlook the notices of professional meetings in the portion of the table of contents on the back cover. These notices often come to us too late for you to participate, given the delays between composing a volume of *NL* and its arrival on the field. Hopefully, the ones listed have enough lead time to give you opportunity to plan. We are sometimes asked if we know of people going to one meeting or another so that colleagues might meet, share room expenses, etc. If you keep us posted, we'll try to serve as a clearinghouse of helpful information.

A Letter to the Editor

I appreciate reading *Notes on Linguistics*, issue by issue, and the good job you do on editing this.

I have a niggle about your classification of linguistic theories in the November, 1991 issue of *NL* [page 3]. You have "Systemics" there as the model that was taught until recently at the British SIL. What the British SIL has been teaching is not "Systemics" but a model developed primarily by John Bendor-Samuel, formerly called "Structure-Function", later "Syntagmatic Grammar". This is a cousin of Systemics in that, like Systemics, it owes a great deal to Firthian linguistics. But it differs radically from Systemics in a number of ways. One of the most significant of these is that it does not give the same degree of priority to the relation of form and meaning (the focus of your diagram). The British SIL model is probably in about the right place in your form–meaning scale. But Systemics proper (as developed by M.A.K. Halliday) should be considerably lower down towards the Meaning end of the scale. A primary concern of Systemics is the relation of Meaning to Form. It starts with "system networks" presenting sets of meaning distinctions that are expressed in the language. Different system networks of meanings cover different kinds of meaning (referential, interpersonal, functional, logical, intertextual). Each system network, or set of system networks, relate to particular units of speech, e.g. word, phrase/group, clause ..., the units being hierarchically arranged. The grammatical description of a language in these terms comprises the description of how each set of meaning relations is realized by specific forms (e.g. presence of a particle, word order, tone, etc.). There is a basic assumption that every change of form signals a change of meaning and that a good grammar has to account for every change of form in terms of what meaning it signals. Lexis is viewed as the lower end of the hierarchy, the point at which grammatical generalizations are no longer possible. From a well-formed grammar and lexicon it should be possible to generate grammatically and lexically correct sentences. Note too that systemicists have been amongst the first to recognize the importance of discourse studies; witness Halliday and Hassan's *Cohesion in English*. They have also given a lot of attention to the practical applications of linguistic studies (in literacy: *Breakthrough to Literacy*, language learning, speech problems, etc.).

I have found Systemics a very practical and insightful model to work with and it has always seemed a pity that we have not given more attention to it in SIL.

KATY BARNWELL

The Indispensable Tape Recorder

Geoffrey Hunt

Introduction

There was a time, before the 1980s, when the linguist had no access to computers that could be used in a field situation. There was also a time, before the 1960s, when the linguist had no access to tape recorders that could be used in such a situation. In recent years, a lot of attention has been given, and continues to be given, to the computer, but very little emphasis is placed on the indispensable tape recorder.

I have been very much involved with the CECIL project, which enables users to look at speech waveforms on the screen of a computer. CECIL also reveals a set of problems inherent in many tape recorders and their recording processes. These problems can be divided into three main areas, namely, the frequency response, the method of controlling the recording process, and noise.

The frequency response of a tape recorder

Some years ago I expected that if I were buying anything but the cheapest type of tape recorders, I would be able to find out the frequency response from the manufacturer's specifications. Sadly, this is no longer the case. One looks in vain for such information, except in the highest quality tape recorders, and, in some such cases, the frequency response may go so high that normal human beings cannot benefit from it.

Perhaps I had better explain at this point that when such specifications are given it does not mean that any frequencies outside the specified range will not be recorded, but that they will not be recorded adequately. The problem of frequency response can be greatly accentuated if the tape heads are not cleaned often enough.

The highest frequencies that are important in distinguishing speech are of the order of 7000–8000 Hz (Hertz), so I don't need a tape

recorder that will give me a frequency response up to 16,000 Hz. On the other hand, a tape recorder that only records up to 6000 Hz is inadequate, making it difficult to distinguish some voiceless sounds.

There is also a problem at the other end of the scale, because tape recorders do not record low frequencies well, either. The lowest frequencies used by a low-pitched male voice will be of the order of 70 Hz, so a tape recorder that does not adequately record below 150 Hz is not adequate for recording speech accurately.

Recently I came across the specifications for some low cost cassette recorders manufactured by reputable Japanese firms. At last, some specifications—but what a disappointment. The cassette recorders were being sold for recording dictation, but the frequency responses were entirely inadequate for a linguist. The recorder that used normal cassettes had a frequency response of 250 Hz to 6300 Hz. The two recorders that used micro-cassettes had a best frequency response of 400 Hz to 4000 Hz.

But even if you have an adequate tape recorder, if the microphone you use with it is not adequate, you will not get an adequate recording. In fact, the microphone's specifications should be better than the minimum required frequency response. A minimum adequate frequency response from 60 Hz to 10,000 Hz would be fine. Fortunately, it is not difficult to find a reasonably priced microphone that meets these specifications.

The method of controlling the recording

In the early 1970s the only method of controlling the recording level was a manual one. First you made a trial recording, adjusting the manual control until the peaks on the recording meter did not go beyond the indicated maximum, but were also not far from it. Then you made the actual recording at this setting, perhaps making slight adjustments while recording.

It was not a very convenient way of making a recording, so the manufacturers introduced an automatic way of adjusting the recording level. This was known by various names, but I will use the one that seems to be most common—Automatic Level Control (ALC).

ALC works by monitoring the signals coming in for recording and adjusting the amplification within the tape recorder so that signals placed on the tape will always be within the desired range. If a recording is being made and a sound louder than any previous sound occurs, the recorder will very quickly reduce the amplification of the incoming signal and hold the amplification at this new level, only increasing it very gradually until another loud sound causes a further sharp reduction.

Unfortunately, ALC brought a number of problems with it. Firstly, if an utterance started quietly and increased in steps to its loudest point, the initial changes of loudness could be completely lost as the recorder continually adjusted the amplification. Secondly, if a speech or some music had an extended quiet passage following a louder passage, the recorder would be gradually increasing the amplification, so that the quiet passage would falsely be getting louder and louder. And thirdly, recordings made with ALC were often noisy; that is, noise was added by the ALC during its operation.

All of these issues cause problems for the linguist; they are particularly acute if the linguist wants to use his recordings for acoustic phonetics. It is obviously much better to use a manual method of controlling recording, but it is now very difficult, perhaps impossible, to find reasonably priced tape recorders that have this feature.

In 1969 I purchased one of the original cassette recorders. It was made by Philips and had only a manual method of controlling the recording. In 1971 or 1972 I made recordings of the language my wife and I were working in. In 1988 I took two excerpts from those recordings and transferred them to my computer through one of the prototype CECIL boxes. They are among the cleanest tape recordings that have been transferred in this way. Most tape recordings are noisy, and it shows up quite clearly in CECIL.

Noise

Some noise is caused by the ALC, but the ALC is not the only source of noise, and I am not even sure whether the ALC is the most important source of noise. Some low quality recorders have inherent noise from the electronic circuitry. Other noise is caused by the

recording situation—perhaps because a computer with a hard disk is situated nearby or it is very windy. Whatever the cause, I know I see too many recordings that are noisy.

On one occasion I was visiting a phonetics laboratory and they brought out some of their tapes for analysis with CECIL. Each one they brought out was noisy.

CECIL can produce a satisfactory tonal graph from a noisy tape because the noise is high frequency noise and CECIL is looking for low frequency noise; namely, the frequency of vibration of the vocal cords. However, the noise will affect the graph of loudness, and it will be much more difficult or impossible to tell whether or not a sound has friction or not. Any spectrograms will also be inaccurate.

Some suggestions

It is obviously important that a linguist has an adequate tape recorder. Buying a professional tape recorder such as the Marantz (sold by JAARS) or the Sony Professional could be the solution. This will set you back more than $220, perhaps as much as $350, and you must choose the option of recording with manual control. If you buy a Marantz, it will be adequate and easier if you buy a monophonic recorder rather than a stereo.

But linguists have many calls on their money and may not be ready to spend so much (perhaps in addition to customs charges) on a tape recorder. Certainly, many of them have had only an inadequate tape recorder. SIL schools also find it difficult to buy such expensive equipment. Is there any alternative solution to this problem?

Of course a tape recorder intended for dictation, whether a cassette or a micro-cassette, will be inadequate. However, I have to travel a lot carrying computer equipment, so I must have a lightweight tape recorder. Such a tape recorder is better than no tape recorder.

A personal stereo recorder is much more likely to provide an adequate frequency response, but would still have the problem of ALC. I have seen a brochure of one such recorder made by Aiwa that should satisfy the frequency response problem, but it only has playback through headphones. In addition, for use with CECIL, a

simple splitter is necessary so that the recording from only one track is used; splitters can be purchased for $3.

A possibility for getting around one of the problems with an ALC tape recorder is to switch the pause control on before starting any recording. Then switch on the "record" mode and speak into the microphone at the expected recording level. In this way the ALC is set at a reasonable level before the pause is released and the recording is actually made. One should not have to go through this procedure, but it does make the beginning of the recording more accurate.

There might be another possible solution, namely, the use of an ALC deceiver. I have never seen one of these marketed, but saw the suggestion in an electronics magazine many years ago. The basic idea is that the ALC deceiver plugs between the microphone and the tape recorder and mixes a high frequency sound with the signal from the microphone. The frequency of this added sound would probably be about 20,000 Hz, too high to be heard by most people and too high to actually record on the tape recorder. Nevertheless, it may still be able to fool the ALC, keeping it adjusted at a constant value. It would probably not work with all tape recorders, but if it did work, it could overcome two of the problems caused by ALC, and may even help with the noise problem. And it should not cost very much, perhaps $10.

The Communications Department at JAARS is looking into the problem, and they also encouraged me to write something for *Notes On Linguistics* to explain the problem as I see it. Here is that explanation.

Conclusions

A good tape recorder is a very valuable tool for the linguist, and my recent experience shows that many linguists do not have good tape recorders. Since the production of the CECIL boxes, many of the tools of an acoustic phonetics laboratory are available to linguists in a field situation, so the tape recorder is more important than ever. Please give adequate consideration to this indispensable tool. ■

Modern Language Association Prizes

The Modern Language Association awards prizes annually for outstanding books. Descriptions and deadlines of awards for books published in 1991 are given below.

1991 James Russell Lowell Prize—For an outstanding literary or linguistic study, a critical edition of an important work, or a critical biography. Authors of nominated books must be current members of the MLA. Deadline: March 1, 1992.

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■

Analysis of Tone Systems

Eunice V. Pike

0. There are a few specific steps which, in my opinion, make the analysis of the tones of a tone language easier. I will try to describe those steps.

1. Line up a hundred or more two-syllable nouns. Group them according to tone sequence; those that seem to have the same tone sequence should be in the same group. Then listen to one group at a time. If you had some in the wrong group, change them to the correct group. This approach is based on the procedures described in Kenneth L. Pike's *Tone Languages* (1948:43–67).

2. Listen to the different groups of nouns with a frame. A frame is a word or morpheme which can be said with each item in the group. The frame should be kept in the same position, always preceding, or always following, the items of the group which are being contrasted.

A contrastive set is a frame with its meaning, paired with one word (in this case a noun) from each of the different groups. Each noun lines up in the contrastive set so that the tones in contrast can be seen easily (note that the contrast occurs on the second tone of the nouns):

frame	word to be tested	meaning
	HH	
	HM	
	HL	

In addition to a contrastive set with the frame preceding the items, it is good to have a contrastive set with the items preceding the frame, as in:

word to be tested	frame	meaning
HH		
HM		
HL		

In the Huautla de Jiménez dialect of Mazatec of Mexico, we can show a contrast of four tones as follows. The verb stem *sa³sen¹*

"likes" is the frame. The person markers following the frame show the four tones in contrast.

sa^3sen^1–na^1	"we like"
sa^3sen^1–li^2	"you like"
sa^3sen^1–na^3	"I like"
sa^3sen^1–le^4	"he likes"

It is good to get another contrastive set with the tones in contrast being in the first position, or tone of the word, as in:

frame	word to be tested	meaning
	HL	
	ML	
	LL	

James and Luchi (1962) give a good example of why a frame is needed. They tell about the Siane language of Papua New Guinea. They explain that when words in that language are said in isolation, words with the emic tones high-high, mid-mid, and low-low are all said with the same pitch. When a frame is used which has, for example, a high pitch, the groups are clearly differentiated. A frame is clearly necessary for analysis.

3. Check to see if the tones are being conditioned by certain consonants. Ladefoged (1968:42) says that the pitch of a syllable is often conditioned by the voiced versus voiceless consonants that comprise it. That is, syllables with voiced consonants frequently have a lower pitch, whereas syllables with voiceless consonants frequently have a higher pitch. For example, in Carrier of British Columbia, the syllables with voiceless consonants have a higher pitch than syllables with voiced consonants (Pike, Eunice, 1986).

4. Check to see if the vowels are conditioning the pitch. Ladefoged (1968:41) says that syllables frequently have a higher pitch when those syllables have an [i] or [u] vowel.

For example, in Otomí of Mexico, syllables with the vowel [i] are higher in pitch than syllables with low vowels in the same environment (Blight and Pike, 1976:55).

Also in Ayutla Mixtec of Mexico, prepause nasalized vowels have a lower pitch than oral vowels of that same emic tone and environment (Pankratz and Pike, 1967:292).

5. Check the words with a final glottal stop. Syllables which end with a glottal stop frequently have higher allotones than similar syllables without a glottal stop. This is true in Popoloca of Mexico (William and Pike, 1968:372). It is also true in Ayutla Mixtec of Mexico (Pankratz and Pike, 1967:291).

6. Compare the verbs with the nouns. Do they have the same tone sequences? In some languages of Africa, there is considerable difference. Pilszczikowa (1972) points out that in Hausa the verbs usually have one syllable that is higher or lower than contiguous syllables. That is, the tone on the middle syllable is higher than the surrounding tones, or the tone on the middle syllable is lower than the surrounding tones. Thus, the tone sequence is high-low-high or it is low-high-low. Most of the verbs in Hausa, according to Pilszczikowa, have either a high-low-high sequence or a low-high-low sequence. A noun, however, may have all low tones or all high tones.

7. Check to see how stress is indicated. In a language with contrastive tone, stress is usually indicated by duration (Lehiste, 1970:36).

In a tone language, stress may also be indicated by allotones. For example, in Tenango Otomi (Blight and Pike 1976:55) a stressed low tone (the first syllable of the stem) is lower than a preceding nonstressed low tone (a proclitic).

8. Word-level tone. If the stressed syllable is the only environment in which you hear the tone easily, and if the pitch of the nonstressed syllables seem to fluctuate, the language may have emic tone on the stressed syllables only.

Sometimes the pitch of the nonstressed syllable fluctuates, and sometimes the pitch of the nonstressed syllable is conditioned. That is, the pitch of a nonstressed syllable tends to be high when contiguous to an emic high tone, but probably not as high as the stressed syllable. If the nonstressed syllables are following a syllable with a stressed low, they may have mid pitch.

Either way, one of the main features that points to word-level tone is the fluctuation of the pitch of the nonstressed syllables. For a good description of word-level tone, see May and Loeweke, 1965.

9. Contour tone on the word level. Etung, a language of Nigeria, has two tones—high tone versus low tone. All possible sequences of tone occur in three syllable words, namely:

1. H H H	5. L H H
2. H L H	6. L L H
3. H L L	7. L L L
4. H H L	8. L H L

In two-syllable words, glides may occur on the second syllable, but they occur only there. The same tone sequences occur in two-syllable words as in three-syllable words, if a glide is considered to be a sequence of two tones.

The tone sequences of two-syllable words are as follows:

1. H H	5. L H
2. H L–H	6. L L–H
3. H L	7. L L
4. H H–L	8. L H–L

An example of the correspondence between the two- and the three-syllable tone sequences is the tone sequence number 2. H L H of a three-syllable word corresponds to the tone sequence H L–H of a two-syllable word. The tone sequence number 4, H H L of the three-syllable words corresponds to the tone sequence H H–L of the two-syllable words.

The only language that I know of with contour tone of this type is found in Etung of Nigeria. It is described by Edmondson and Bendor-Samuel, 1966.

10. One falling tone per word. Yotsukura says that the only significant thing about the tone system of Japanese is the fall. The contrast consists in the place where the fall occurs. Words with a fall in tone contrast with words with no fall. Japanese also has a complicated intonation system (Yotsukura, 1967).

Carrier, an Athapaskan language of Canada, has also been analyzed as having one emic fall per word. Syllables following the fall within a word have low pitch (Pike 1986).

Luganda, a Bantu language of Africa, has been analyzed as having one fall per word, or as ending in high tone (Kalema, 1977).

11. One fall or one rise per word. In another type of word tone, the contrast is a change from low to high, or from high to low. The place where the change occurs is contrastive. In writing emically, the tone (high to low or low to high) needs to be written only on the syllable before the change. The pitch of the other syllables is predictable. That is, after a syllable marked low, the following syllables are high. After a syllable marked high, the following syllables are low. Dogon of Burkina Faso has a tone system like this (author's field notes).

For a similar tone system, see the Oska dialect of Japanese (Pierrehumbert and Beckman, 1988).

12. Level versus moving contours. Another type of word tone is found in Nepal. There is a "moving contour" versus a "level contour" as described by Hoehlig and Hari, 1976:38–42.

They describe the contour as follows:

(1) The low moving contour is breathy and rising.
(2) The high moving contour is clear and falling.
(3) The low level contour is breathy.
(4) The high level contour is clear and higher than the low level contour.

In orthography, the moving contour is preceded by an apostrophe. The breathy contour is marked by an *h* following the first vowel.

	CLEAR	BREATHY
Level	CVCV	CVhCV
Moving	'CVCV	'CVhCV

13. Syllable weight. According to Newman, 1972, the pitch of the verb tone in Bolanci of Nigeria is predictable on the basis of syllable weight. (Light syllables are CV, whereas heavy syllables are CVV or CVC.) He says, "If the initial syllable is heavy, then the verb will have Lo–Hi tone, if the syllable is light, the tone will be Hi–Hi" (page 307).

Jones, 1971, says that word-stress in Hindi is predictable on the basis of heavy versus light syllables. A light syllable has a short vowel and is not nasalized. A heavy syllable has a CVC syllable or a long vowel. It may have a short nasalized vowel.

The stress rules are as follows:

1. In a word with one heavy syllable, that syllable is stressed.
2. In a word with two or more heavy syllables, the next to the last heavy syllable is stressed.
3. In a word with no heavy syllables, the next to the last syllable is stressed.

14. Downstepping terrace tone. The presence of a process phoneme, that is of terrace tone, can be suspected by the restricted distribution of the "mid" pitch. Notice that with terraced tone the mid pitch occurs only when following a high tone and never when following a low. Furthermore, the mid pitch never occurs postpause. Postpause following a low there is a contrast of only two tones, but following high there is a contrast of three tones. This phenomenon is caused by the process phoneme (which is marked by "!"); it is the process phoneme that causes the contrast of three tones. Emically there is a contrast of only two tones plus the process phoneme.

Kenneth L. Pike (1967) speaks of a process phoneme. A process phoneme cannot be identified in isolation; it can be identified only by the influence it has on a sequence of tones. A process phoneme always occurs between high tones. The high tone following a "!" is lower than the high preceding it.

Welmers (1973:87–90) described downstep and said it might be due to the historical loss of a low tone.

15. Tone sandhi. Tone sandhi is the change of emic tones in a word. For example, the change may come in accordance with where the word occurs in the sentence, that is, whether it occurs sentence initial or sentence final, etc. In Acatlán Mixtec (Pike and Wistrand, 1974:91), some words change in accordance with their occurrence postpause versus nonpostpause.

The emic tones of a word may change in accordance with the tone of the word they are contiguous to. For example, in Villa Alta Zapotec of Oaxaca, Mexico, a mid tone is changed to a high tone by whatever

follows it, unless it is immediately preceded by a high tone (Pike, Eunice, 1948:165).

The emic tones of a word may change in accordance with the word to which they are contiguous, independent of the tone of that word, but dependent on the class to which that word belongs. For example, in San Miguel Mixtec one class of words causes a change. Another class of words with the same tones does not cause a change. That is, words with the tones high-high, high-mid, mid-mid, mid-low, and low-high must be divided into classes. One class causes other words to have a change in tone. The other class, however, even though it has the same tones, does not cause such a change (Pike, Kenneth L. 1948:77–80).

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Sentence Repetition Testing for Studies of Community Bilingualism: An Introduction.

Carla F. Radloff

South Asia Sociolinguistic Survey Team

Editor's Note: The following is reprinted from the 1990 *Survey Reference Manual* (Second Edition). The SRT and RPE have both been tested against the Second Language Oral Proficiency Examination (SLOPE) in Cameroon in 1991. Results are to be published soon.

A sentence repetition test (SRT) is an indirect measure of second language proficiency. It is a screening test, quickly administered and, thus, ideally suited for testing large numbers of people and giving a broad picture of the varying levels of second language proficiency which occur with n the community. This article is a brief introduction to the technique and its application in bilingualism surveys.

Proficiency testing in a bilingualism survey.

Careful goal setting is the hallmark of every good sociolinguistic survey. Researchers might see the need to examine dialect boundaries, they might trace the effects of different attitudes on language use, or they might search for the reasons for language maintenance in the face of a changing social climate. The goals chart the course the survey will follow.

The investigation of community-wide bilingualism is only one of many potential components of a sociolinguistic survey. The choice to investigate bilingualism within a community is based on the wider goals of the survey, that is, whether patterns of bilingualism are a question to be answered by this particular study. Similarly, the testing of second language proficiency is only one component of a focused bilingualism study.

The choice to conduct second language proficiency testing within a bilingualism survey is based on the need to know the different levels of proficiency at which different segments of a society function. There are bilingualism surveys whose goals, for example, require proficiency scores in order to draw the desired conclusions for language planning. There are other bilingualism surveys, for example, whose goals are fully met through the administration of questionnaires or in-depth interviews regarding the attitudes underlying language shift. Therefore the importance of second language proficiency testing to the goals of the particular bilingualism survey must be established.

If second language proficiency is indeed a question, then, the context for its examination must be detailed. Community-wide bilingualism is usually not examined in isolation, rather, it is studied as it relates to language attitudes, or language use, or patterns of language contact or other such phenomena within a community. The depth, or breadth, of study is related to the desired extendibility of the results. If the goal, as is usually the case, is to make predictions for the larger population from the results of the sample studied, then very careful, detailed planning is necessary. This planning must include the identification of critical factors for sampling the population, as well as preparing the instruments for collecting information on the related phenomena to be investigated.

Selection of the test instrument.

Another part of that planning is the actual selection of the instrument to be used for assessing proficiency. Second language proficiency is ultimately an individual matter, yet the aggregate of individual proficiencies produces a profile of bilingualism within a community. The instrument selected should be one whose methodology facilitates the construction of that community-wide profile, that is, it should be practical. The results it offers should allow the researcher to draw conclusions and make predictions in keeping with the goals of the bilingualism survey, that is, it must be valid and reliable.

Many instruments or techniques for obtaining second language proficiency levels have been shown to be both valid and reliable. It is incumbent upon the researcher to select from among them the instrument that best suits the goals of the bilingualism survey

undertaken. If, for example, the goal is to obtain an in-depth understanding of the strengths and weaknesses of the second language proficiency of a few, selected individuals in the community under investigation, a diagnostic type of instrument should be chosen. An example of the application of this type of instrument would be where certain individuals or a certain narrowly defined class of individuals is determined to represent the highest level of second language proficiency in the community. In keeping with the goals of the survey, an in-depth oral proficiency interview would be conducted with each of them in order to determine their individual strengths and weaknesses. The identification of such individuals could be accomplished through extensive administration of questionnaires or in-depth interviews.

On the other hand, if the goal is to obtain an extensive profile of second language proficiencies present in a community, a different, screening type of instrument should be chosen. An example of the application of this second type of instrument would be in a situation where a community is sampled on the basis of a number of specified factors (e.g., age, education, sex, etc.) and a representative number of people in each category is tested on a brief, easily administered proficiency test. Efforts would be made to ensure that the entire community is represented in the sampling and a large number of people would be tested so that the results could be used to draw conclusions about and make predictions for that entire population.

Sentence repetition test for community-wide testing.

The sentence repetition test is a good example of this second type of instrument. It is a screening test, designed to facilitate testing the large number of people required for drawing conclusions about the whole community. It has been shown to discriminate the broad range of second language proficiency levels, so is suitable for formulating a profile of community-wide second language proficiencies.

The sentence repetition test (SRT) is an indirect test methodology, but its scores are calibrated with a separate, more direct assessment of proficiency so that the results of the testing are interpreted in terms of that correlation. Its administration time is brief and it can be scored on the spot—a 15-sentence SRT with three practice sentences requires approximately five minutes. The SRT technique is so

designed that once developed and calibrated it is ready to be used without further modification wherever bilingualism in that particular language is a question. Also, the same test administrator can give the test in all locations, without additional training.

The SRT procedure is easy for people to understand; indeed, it follows the natural tendency of people to repeat what they hear. It is not necessary to tape record participants' responses nor is it absolutely necessary to isolate them for test administration. These attributes extend the applicability of the SRT methodology into more conservative cultures or situations where political or social conditions impose such constraints. The construction of a particular SRT can be accomplished in an area different from the actual site for testing; this would maximize time spent in actual assessment in the test area.

There are undoubtedly many different ways in which a test utilizing the repetition of sentences could be constructed. The SRT methodology that is prescribed here is based on the premise that a test must be developed separately for each language. That is, unique sentences are selected for each test and these sentences are calibrated with a separate, external proficiency standard (e.g., RPE, see below). The format consists of using fifteen unrelated sentences, and scoring a maximum of three points for verbatim repetition of the entire test sentence.

The procedures for developing an SRT are designed so that researchers who do not speak the language being tested or have only rudimentary proficiency are able to oversee and participate in the development of such a test. This is primarily accomplished by using the assistance of educated mother tongue speakers of the test language at all stages of constructing the test. The procedures for training test administrators are such that an administrator with minimal proficiency in the test language should be able to administer and score a test reliably when fully trained, although, of course, use of a mother tongue or fluent speaker of the test language would be more expedient. Although the actual administration of an SRT is brief, the construction and calibration of it requires an investment of time and attention to insure a quality instrument. It is probably best used, then, in the investigation of bilingualism in a language of wider communication or other study where extensive testing is necessary.

For the development of an SRT in a given language, sentences are selected from discourses elicited from educated mother tongue (MT) speakers of that test language (according to local standard) and/or from textbooks or other published sources. These sentences should represent a variety of registers and complexity of vocabulary and syntax. The sentences are evaluated for naturalness and then tape-recorded by an educated MT speaker of the test language. This preliminary form of the test (40–50 sentences) is given to a sample of second-language speakers of the test language who demonstrate varied levels of proficiency. Based on their performances, fifteen of the sentences are chosen for the final form of the test.

Sentences chosen should display a range of difficulty from easy to difficult; also, each sentence chosen should be repeated more accurately by those whose overall performance is good, and more poorly by those whose overall performance is poor. To insure this, appropriate statistical formulas are utilized in sentence selection according to standard SRT development procedure. These fifteen sentences, along with three easy-to-repeat practice sentences, comprise the final form of an SRT. Performance on the test is scored as follows: a verbatim repetition of a test sentence is awarded three points, one mistake two points, two mistakes one point, and three mistakes or more, zero points. The maximum score for an SRT is, thus, 45 points.

Calibration of an SRT.

For maximum interpretability of test results it is desirable that an SRT be calibrated with a more descriptive assessment of second language proficiency. Throughout the pilot study and field study stages of development of the SRT technique, an instrument called the Reported Proficiency Evaluation (RPE) has been utilized for this purpose. (This technique was formerly called the Other Evaluation or OE.) There are many assessment techniques that could be used for calibrating an SRT. The RPE was developed and has proven useful as a practical tool fitted to the exigencies of developing a test under field conditions.

The RPE is a descriptive estimation of second language proficiency. In this procedure educated mother tongue speakers of the test language evaluate the proficiency of a number of second-language

speakers of that language with whom they are well acquainted. These MT raters are guided in their evaluations by a trained field worker, who is, ideally, a local person.

The MT raters grade their second-language acquaintances according to a standard set of proficiency descriptions, which describe differing levels of attainment in the areas of accent, grammar, vocabulary, fluency and comprehension. These proficiency descriptions are the original FSI proficiency descriptions (ETS 1970). The MT rater's evaluation of his acquaintances' second-language ability is expressed in terms of proficiency levels called Reported Proficiency Evaluation (RPE) levels. The range of these levels extends from RPE level 0+ (very minimal proficiency) to RPE level 4+ (approaching the proficiency of a native speaker). A statistical correlation between the second-language speaker's RPE level rating and his performance on the SRT developed in that language is established according to the statistical procedures of standard SRT development. This results in a range of SRT scores being assigned to a given RPE level of proficiency in the test language.

Manual for developing and using SRTs.

The SRT and RPE techniques have been developed by members of the South Asia Sociolinguistic Survey Team. A full description of these practical methodologies and their application in bilingualism surveys is presented in the SRT Manual (see Radloff). A step-by-step description of the development and calibration of an SRT is provided in this manual. The statistical procedures which are used in standard SRT development and which help ensure test quality are explained in a non-technical fashion. Case studies of SRT use in actual bilingualism studies and a survey of the literature in second language testing supporting the use of sentence repetition are included. The history of the development of these techniques in the field study stage is described, along with studies in validity and reliability.

Correlations of $r = .90$ and $r = .91$ were found between SRTs and RPE evaluations in the development and revision of an SRT in the Pashto language. Field experience has proven SRT an effective tool for evaluating proficiency levels in communities where Pashto is the language of wider communication. An Urdu SRT has also been developed and is similarly used. Ongoing bilingualism research using

SRTs in other languages is currently in the development stage. The reader is urged to consult the manual and follow the prescribed methodology for developing and using an SRT in a field situation.

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The 19th International Systemic Functional Congress

The 19th International Systemic Functional Congress will be held from July 13–18, 1992 at Macquarie University, Sydney, Australia. Plenary speakers will include John Bateman, Margaret Berry, Chris Candlin, Ron Carter, Robin Fawcett, Allan Luke, Claire Painter, and Joan Rothery. Daytime sessions will be devoted to plenary and section papers, with the Wednesday set aside for workshops or sightseeing. Evening sessions will be devoted to panels and to presentations about the semiotics of theatre and music.

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Advances in Systemic Linguistics: Recent Theory and Practice

Advances in Systemic Linguistics, edited by Martin Davies (Stirling) and Louise Ravelli (Birmingham) will soon be available from Pinter Publishers for approximately forty-five pounds sterling. The book is a compilation of recent work in systemic-functional linguistics. It is divided into five parts:

Part I — Framework
Contributors: John McH Sinclair and M. A. K. Halliday.

Part II — Metafunctions
Contributors: Christian Matthiessen and J. L. Lemke.

Part III — Lexicogrammar
Contributors: Kristen Davidse and Gordon Tucker.

Part IV — Functional Sentence Perspective and Theme
Contributors: Jan Firbas and Linda Stump Rashidi.

Part V — Text Studies
Contributors: James D. Benson and William S. Greaves, Catherine Emmott, and Daniel Kies.

Selections were chosen by the editors as examples of advances in current theory and practice. Some of the papers comprising this book have their roots in the 17th International Systemic Congress (July 1990). More information about this book can be obtained from Pinter Publishers.

Also newly available is the related volume, *Occasional Papers in Systemic Linguistics 6, Special Issue: Papers from the Seventeenth International Systemics Congress, 1989*. The series editor is Dirk Noel, and guest editors for the issue are Martin Davies and Louise Ravelli. Seven papers given at the Congress (and not included in the abovementioned book) are included. Authors of the papers are: Jonathan Fine, Elke Teich, William McGregor, Meriel Bloor and Thomas Bloor, Peter H. Fries and Gill Francis, Carman Caldas-Coulthard, and Glenn Stillar.

This publication should be available in early 1992. For more information contact the publisher: Department of English Studies, The University, Nottingham NG7 2RD, England. ■

Report on the 47th International Congress of Americanists

David Captain

Columbia Branch

The 47 International Congress of Americanists (ICA) convened on the Tulane University campus in New Orleans from July 7–11, 1991. The substance of the Congress consisted of a total of over 100 symposia in the diverse fields of archeology, anthropology, history, political science, culture, linguistics, macroeconomics, sociology, international relations, rural studies, and general topics, with papers from almost 2,000 participants.

Before the symposium in which I participated opened, I did take the opportunity to sit in on the presentation of a few papers in two of the anthropology symposia dealing with native American religions and belief systems, as well as several of the papers in the Tupi-Guarani and Cariban Linguistics symposium.

Our Arawakan Linguistics symposium, organized by Mary Ruth Wise, Elsa Vilchez, and Nancy Hickerson, met on the final two days of the congress. Nineteen papers were presented, some of which were read in the author's absence. Even in this single symposium the topics and methodological approaches varied considerably.

Three papers dealt with phonology. Steve Parker's work on the sonority grid of phonemes in words evidenced a very nice handling of morphophonemics and syllabification in Chamicuro. Sonority clashes are reduced by the shifting of sounds on the sonority scale. Gurlekian et al presented a highly technical report on their spectral analysis of speech for vowel identification and specification.

Another three papers dealt broadly with text analysis. Fleming showed how problem and resolution figured in the text structure of a Garifuna folktale. Heitzman showed various discourse devices, such as quotations, antonyms, and negatives, for adding redundancy to a text in order to manage the information rate in Asheninca. Rich Mansen reported on the incorporation of the noun "heart" in

idiomatic expressions in Guajiro and how their use in discourse generates emotive force.

Three papers dealt with dialectal and socio-linguistic factors in literacy in the Campa languages of Peru. Three other papers dealt with morphosyntax, especially with respect to transitivity and ergativity. Alvarez's paper exemplified this in several syntactic structures in Guajiro, where the gender-number suffix on the verb either agrees with the subject or, in transitives when the subject is marked by a person prefix, with the object. I have yet to see any work to show what discourse considerations indicate when the subject prefix should be used.

The remaining seven papers fit the broad category of comparative. Evidence was given by Payne for including Apolista and by Malone for excluding Chimila from the Arawakan family. Alexandra Aikhenvald-Angenot, a Russian linguist teaching and conducting field research in Brazil, figured prominently in the symposium with her questions and comments as co-author and presenter of three papers. One was a lexicostatistical analysis of Maipuran Arawakan languages of the upper Rio Negro. Another, co-authored with Angenot and Ritchie Key, actually suggested a genetic affiliation of Proto-Arawakan with Nostratic, with some 40 supposed cognates as evidence. A couple of the papers dealt with comparative morphosyntax. My paper was a phonological reconstruction of Proto Lokono-Guajiro, with a discussion of its place within the Arawakan family.

In light of the size and diversity of the Congress, I am left wondering what the purpose is in bringing the entire enterprise together at a single time and place. The Congress could probably be enhanced by providing a significant number of plenary sessions dealing with integrating themes. Perhaps there are those in attendance with no responsibility to a single symposium who are able to sample the work being done in several areas, but for those of us with a commitment to a particular symposium, time and opportunities were limited for participating in other areas.

Perhaps for this reason, and also considering that the next ICA will be held in Europe, those in our symposium discussed the possibility of meeting again, independently of the ICA, in the next two or three years somewhere in South America.

All in all, the Congress provided a good setting for our Arawakan symposium, and the overall experience was one of positive interaction academically and socially with our professional colleagues both in and outside of SIL. ■

The 19th LACUS Forum

The Nineteenth Annual LACUS Forum of the Linguistics Association of Canada and the United States will be held August 4–8, 1992, at the Laboratoire de Neuroscience de la Cognition, Université du Québec à Montréal, in downtown Montreal, Quebec. In addition to four days of refereed papers, there will be three evening lectures by eminent scholars, including the Presidential Address by Sebastian K. Shaumyan.

Papers presented at the Forum will appear in the LACUS yearbook, *The Nineteenth LACUS Forum 1992*, a few months after the conference. The LACUS professional membership fee of $25 per year ($35 Canadian) and student and emeritus membership fees of $15 ($21 Canadian) include a free copy of the current year's *Forum*.

To be put on the mailing list for further information regarding the Forum and/or to join LACUS, write to:

Valerie Becker Makkai, Secretary-Treasurer, LACUS
P. O. Box 101
Lake Bluff, Illinois 60044 USA

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International Linguistic Association Conference

The Thirty-seventh Annual Conference of the International Linguistic Association will be held from Friday evening, April 24 through Sunday afternoon, April 26, 1992. The conference will be held in the Intercultural Center on the campus of Georgetown University in Washington, D. C. "Functional Linguistics" is the theme of the conference. Simon Dik, Talmy Givón, Andre Martinet, Ken Pike, and Thomas Sebeok have all been invited to speak.

Conference registration fees are as follows:

> Received before March 15, 1992 (preregistration by mail): professional, $25; student (with certification), $15.
>
> Received after March 15, 1992 by mail and on site: professional, $30; student (with certification), $20.

To preregister, send your check or money order for the appropriate amount in U.S. dollars (payable to "International Linguistic Association") to:

> Professor Ruth M. Brend, Conference Chair
> 3363 Burbank Drive
> Ann Arbor, MI 48105 USA

Accommodations are available at Georgetown University's Leavey Conference Center for $90/night (single occupancy) or $109/night (double occupancy). Reservations should be made directly by mail or phone with:

> Leavey Conference Center
> Attn: Reservations Office
> 3800 Reservoir Road
> Washington, D. C. 20057 Tel (202) 687-3200

Note: From April 20–23, 1992, the Annual Georgetown University Roundtable will be held. The topic is "Language, Communication, and Social Meaning". For further information, contact:

> Professor Carol Kreidler
> Chair, Georgetown University Roundtable
> Office of the Dean
> School of Languages and Linguistics
> Washington, D. C. 20057 ■

The 24th International Conference on Sino Tibetan Languages and Linguistics

Lon Diehl

This year's International Conference on Sino-Tibetan Languages and Linguistics met at Ramkhamhaeng University (Bangkok, Thailand) and Chiangmai University (Chiangmai). Ramkhamhaeng University hosted more than 160 participants October 7–9 and Chiangmai University well over 120 participants October 10 and 11.

This was the second time the conference has been held in Thailand; Ramkhamhaeng University hosted the 18th conference in 1985. The Thai venue was vividly reflected in the conference proceedings: the opening address in Bangkok was given by HRH Princess Galyani Vadhana; the opening session in Chiangmai was a panel discussion, "Linguistic Evidence in King Ramkhamhaeng Inscription."

The keynote speaker, Jim Matisoff of Berkeley, described three periods of work in Sino-Tibetan linguistics in terms of associated technology:

(1) pencil and paper: the period of activity beginning around the turn of the century with the work of Grierson and others;

(2) typewriters: the period beginning in the late 1930s with the work of Robert Shafer, Paul Benedict, and others;

(3) computers: the period beginning in the 1970s, characterized by an avalanche of new data.

Sino-Tibetan linguistics has indeed developed over recent years, and this is aptly reflected in the development of the conference itself. Not so many years ago the Sino-Tibetan conference was dominated by papers on historical Chinese phonology; Tibeto-Burman with its hundreds of languages and dialects was represented by a handful. Participants were mostly from the U.S. and elsewhere in the English-speaking world, with overseas Chinese and a few European colleagues lending an international flavor to the enterprise.

Both participants and subject matter of the Sino-Tibetan conference have become increasingly diverse. This year papers were given by

colleagues from China, India, the USSR, Vietnam, and other countries. Even though the work presented at this conference was mostly historical-comparative and descriptive, there was no lack of variety. Diversity was at the very heart of this growing, largely congenial conference.

Even more diverse is the range of languages those involved in the conference work on. It is significant that a conference on Sino-Tibetan languages and linguistics includes whole sessions on Austro-Thai languages and Mon-Khmer languages. This reflects not just engaging congeniality but a serious and growing interest in the Sino-Tibetan Linguistic Area as such. As battles about genetic affiliation versus massive borrowing have simmered and raged over the years, workers have begun to develop a certain respect for the role of language contact and resultant areal features, which are important to any real understanding of the languages of this linguistic region. This conference has become a place where workers specializing in genetically different language groups have an opportunity to sift out areal phenomena together. Also, individual workers are not limiting themselves to genetically-defined groups as much as they did in the past.

Thinking of growth with its resultant diversity and size, we could say that conferences are like people: their greatest strengths are also their greatest weaknesses. I remember the frustration I felt the first year I had to choose among parallel sessions. This gets more painful every year I attend the conference. Do I want to take advantage of the diversity of expertise here, or shall I take advantage of easy access to the latest work of colleagues in my own concentrated area? The frustration is only worsened when conference organizers make a rich mix of each session!

It is gratifying to see SIL beginning to make a rightful contribution to the Sino-Tibetan conference. Our presence and contributions at this conference are of value to everyone involved. We have a lot to learn, and we have a lot to give. To my mind this was a landmark year in that regard. For the future I hope this conference has an increasing role in the development of our Asia Area workers, and I hope we SIL Asia Area workers make more of a proportionate contribution to the development of the field defined by the Sino-Tibetan Linguistic Area.

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REVIEWS OF BOOKS

The Life of Shong Lue Yang: Hmong 'Mother of Writing' Keeb kwm Soob Lwj Yaj: Hmoob 'Niam Ntawv'. By Chia Koua Vang, Gnia Yee Yang, and William A. Smalley. Translated by Mitt Moua and Yang See. (Southeast Asian Refugee Studies, Occasional Papers, Number Nine) Minneapolis: Center for Urban and Regional Affairs, University of Minnesota. 1990. Pp. 192. Paper.

Mother of Writing: The Origin and Development of a Hmong Messianic Script. By William A. Smalley, Chia Koua Vang, and Gnia Yee Yang. Chicago: University of Chicago Press. 1990. Pp. xii, 221. Hardback and paper.

Reviewed by Frank Blair

Georgetown University

Editor's Note: This review is reprinted with permission from *The Georgetown Journal of Languages and Linguistics*, No. 1, Vol. 4., pages 501–506.

The Hmong are a people six million strong, widely distributed throughout southern China and northern Southeast Asia. They speak a language also known as Hmong, which has several varieties. Hmong belongs to that group of Miao-Yao languages which show affinity for both the Tibeto-Burman and Austroasiatic language families. The most widely used orthography for Hmong is known as the Romanized Popular Alphabet (RPA). It was devised in the early 1950's by Bible translators and linguists, Smalley among them, working in various Hmong communities in Laos. Beginning in 1975, large numbers of Hmong have emigrated as refugees to Western nations, including 85,000 to the United States. Although there have been many studies of the Hmong language and culture, both in traditional and non-traditional settings, thus far we have not had a life history which describes the traditional life-cycle of this people.

American anthropologists of the twentieth century have frequently used life histories as a means of exploring and understanding unfamiliar cultures and ways of being. Notable examples in this tradition have been Oscar Lewis' *La vida: A Puerto Rican family in*

the culture of poverty—San Juan and New York, James Freeman's *Untouchable: An Indian life history*, and *Ishi in two worlds* by Theodora Kroeber. Researchers interested in life histories conduct extensive ethnographic interviews with their informant(s). The data from these interviews are organized in a biographical framework to highlight those events of cultural significance and personal interest to the informant's life-cycle. Information presented in this way provides a context for understanding beliefs and practices which might otherwise be inaccessible to readers, allowing them to be evaluated from within their own frame of reference. Life histories excel in the description of traditional lifestyles; they tend to accentuate those parts of an individual's experience most likely to remain unchanged from generation to generation.

Vang, Yang, and Smalley's *The life of Shong Lue Yang: Hmong 'Mother of Writing'* (hereafter referred to as *The life of Shong Lue Yang*) is not primarily intended as a life history in the anthropological sense. The information presented is not the result of ethnographic interviews with Shong Lue Yang, nor is the book organized around events of importance to the traditional Hmong life-cycle. In some ways it will prove to be inaccessible to those without previous knowledge of the Hmong. Smalley, whose direct influence in this work is largely limited to the English translation, acknowledges that the English text is supplementary to *Mother of writing: The origin and development of a Hmong messianic script* (hereafter referred to as *Mother of writing*). For Vang and Yang the Hmong text is a primary document; it is unlikely that they would consider it to be supplemental to anything. For them Shong Lue Yang's life is a remarkable lens; the events of that life are used to create a deeply etched picture of an entire people in transition. In the middle of this century, from the most humble of origins to imagine in today's world, there emerged a man named Shong Lue Yang. He accomplished an intellectual feat of great genius: the creation of an alphabet with which to write his people's language. The last forty years have seen enormous changes visited upon the Hmong; many thousands have had their lives disrupted to the point that traditional ways can no longer be maintained. In such a milieu the goals of a traditional life history lose some of their validity. Vang and Yang are striving to hold onto those things they perceive as uniquely Hmong; they are justifiably proud of Shong Lue Yang and have done a great service in providing a record of this man and his times. They thread together the

pericopes which preserve the events of Shong Lue Yang's life like a string of fine pearls.

The text of *The life of Shong Lue Yang* has a complex format, dictated by the fact that it is addressed to multiple audiences. The book is a diglot, published both in Hmong and English. The Hmong variety used is Hmong Daw, sometimes referred to in English as White Hmong. The Hmong text is presented in two scripts, the *Pahawh Hmong* and the RPA. Even though it appears as though the camera-ready copy had to be generated on a dot-matrix printer, the Center for Regional and Urban Affairs is still to be commended for bringing a volume so technically formidable to press. The fact that both the *Pahawh Hmong* and the RPA are used indicates that the book is intended to reach the widest possible Hmong audience. The fact that an English translation is included demonstrates that the intended audience is not limited to Hmong. A map showing the location of the places mentioned in the text would have been a useful aid for such readers.

In style and content *The life of Shong Lue Yang* may be more properly called a gospel than a life history. It is perhaps one of the first experiments with this literary form since the second century. Vang and Yang are followers of Shong Lue Yang. They wrote this account of Shong Lue Yang's life in order to persuade the world that Shong Lue Yang is a Hmong messiah or divine incarnation. The evidence to support this claim is that Shong Lue Yang, an unschooled Hmong farmer, created a writing system—the *Pahawh Hmong*. The story of Shong Lue Yang is the only documented example of an illiterate person creating an alphabet *ex nihilo*. Properly understood, the phrase *Niam Ntawv* 'Mother of Writing' is a messianic title, not a descriptive phrase. (In Hmong, the word *niam* may mean 'source' as well as 'mother'.) Shong Lue Yang attributed his ability to create an alphabet to his belief that he was one of God's twelve sons. Two of his brothers, in a series of visionary visits, revealed the *Pahawh Hmong* to Shong Lue Yang, as well as the purpose behind his incarnation.

In addition to being known as the *Niam Ntawv*, Shong Lue Yang was also known as the *Theej Kaj Pej Xeem*, a title which translates as 'Savior of the Common People'. He was born in the rural mountains of Northern Vietnam, near the Lao border. His family moved across the border into Laos while he was still a young child and he grew up

in that country. Shong Lue Yang emerged, apparently reluctantly, into public life as a visionary and a prophet in 1959. He was about thirty years old. A lively people movement[1] sprang up around his teaching, which centered on the need for unity and reconciliation among the Hmong. The *Pahawh Hmong* was seen as a vehicle for this unity. Shong Lue Yang's movement became known in Lao as *Chao Fa* 'Lord of the Sky', a name which has unfortunately caused it to become confused with other people movements among the Hmong. Chao Fa arose among the Hmong at a time when they were being caught up in the larger anti-colonial wars of Indo-China. In later stages of that conflict, some Hmong factions supported the communists, while others fought at the behest of the CIA against the communists. Accused by both sides of being in the employ of the opposite side, Shong Lue Yang was assassinated in 1971 by agents of General Vang Pao, a Hmong leader widely believed to have been associated with the CIA. The messianic movement that Shong Lue Yang inspired went underground for a few years and then resurfaced in Laos and in the communities of the Hmong diaspora. Shong Lue Yang and the movement he inspired is still an object of controversy in Hmong communities. Vang and Yang penned this account of Shong Lue Yang's life in order to bring forward their perception of who he was and what his life meant.

If *The life of Shong Lue Yang* shows the distinctive imprint of Vang and Yang, then Smalley's mark is especially revealed in *Mother of writing*. The last chapter of this work contains his sensitive and graceful assessment of this most remarkable story. It is particularly poignant considering Smalley's lifelong involvement with the Hmong and his role in designing the RPA. The fruits of his knowledge are amply demonstrated throughout this book. A concise description of Hmong phonology is provided as a basis for understanding the development of Hmong orthographies. The central chapters of this book are concerned with the evolution and contemporary use of the *Pahawh Hmong* through its various stages. A chapter on the technological development of systems used to print the *Pahawh Hmong* is compelling. In the space of twenty years the leap is made from wood block printing to desktop publishing. Chapters on other Hmong orthographies and the historical development of alphabets around the world are equally cogent.

The world has, of course, a great many writing systems. Ideographic and lothographic systems appear to have been the product of great cultural traditions; the Chinese, the Sumerians and ancient writing systems of Central America are examples of these. The Sumerian writing system ultimately evolved into the various consonant-nuclear systems of the Indic and Semitic traditions. From these, scripts as varied as the Devanagri, the Arabic, and the Roman evolved. Although writing systems have been very widely diffused, they seem to have been only rarely invented. The *hankul* writing of Korea is the only independently invented alphabetic system that is widely known. It was developed more than five hundred years ago by people who could read Chinese, although the *hankul* system owes nothing to the Chinese system.

Writing systems developed by people who, like Shong Lue Yang, were not previously literate are almost as rare. In North America perhaps the best known is the syllabary developed for the Cherokee Nation by Sequoya at the beginning of the nineteenth century. In the early part of the twentieth century, Afaka developed a syllabary for the Ndjuka language spoken in Suriname. In West Africa, in the middle of the nineteenth century Momolu Duwalu Bukele developed a syllabary for the Vai language. In each of these three cases the writing system is clearly not the product of cultural diffusion. The basic unit of the phonological analysis upon which each of these orthographies is based is the syllable. There is some evidence that each of these people experimented with a logographic system before settling on a syllabary. The case of Shong Lue Yang is unique because his orthography, the *Pahawh Hmong*, is phonemic, in the sense that it is based on units smaller than the syllable, from the very first. Shong Lue Yang's orthography differs from all other known systems in that its original form is vowel-nuclear rather than consonant-nuclear or one which symbolizes consonant and vowels equally. It is the only vowel-nuclear alphabet known.

The *Pahawh Hmong* exists in four versions, which developed through time. Each stage shows a progressive simplification of the system, and some later stages show increasing degrees of linguistic abstraction. The original version, which dates from 1959, is known as the *Phajhauj Paj* 'Source Version'. Technically, this orthography is demi-syllabic, representing each syllable with two symbols, one for the onset and another for the rhyme. Such a system is interesting in that

it appears to provide the support of psychological reality to certain claims made by metrical phonology for an intermediate level between the phoneme and the syllable. The third version of the *Pahawh Hmong*, known as the *Phajhauj Ntsiab Duas Peb*, was introduced in 1970. It shows a greater degree of linguistic abstraction than the Source Version. Hmong is a tonal language and the Source Version represented each of the thirteen possible syllable nuclei with one of seven tones, yielding symbols for a total of 91 rhymes. In the Third Version, tone is symbolized by diacritics independently of the vowel symbol, which greatly reduced the number of symbols needed to account for all the rhymes.

The *Pahawh Hmong* is a remarkably efficient writing system. As was previously mentioned, it represents every syllable in the Hmong language with two symbols, one for the rhyme and one for the onset. The symbol for the rhyme is written first,[2] followed by the symbol for the onset. If no onset is written, the default syllable onset is [k]. Those few syllables which consist solely of a rhyme are written with two symbols: one for the rhyme, and a second symbol which indicates that there is no onset. Hmong has only [ŋ] as a syllable-final consonant and its occurrence is phonologically predictable based on the nature of the preceding vowel. Consequently an alphabet for Hmong does not need to represent syllable-final consonants and neither the *Pahawh Hmong* nor the RPA does so. In representing syllable onsets, the *Pahawh Hmong* does not distinguish between single consonants and consonant clusters. This is characteristic of the way in which most Hmong perceive their language, but it also seems likely that the tragic assassination of Shong Lue Yang prevented him in making further progress in his analysis of consonants. Yet even in the absence of such further analysis, the *Pahawh Hmong* elegantly and simply accounts for the effect of every phoneme in the language.

Students of people movements as diverse as the Sun Dancers of the Great Plains, the cargo cults of the South Pacific, and the messianic cult that became Christianity will be interested in the first-hand account of the genesis of a similar movement that these books present. Social workers with case loads from within the communities of the Hmong diaspora will welcome the insights into traditional Hmong culture that are provided from a Hmong point of view. Psychologists interested in the development of genius in a

cross-cultural context and educators looking for non-Eurocentric examples of intellectual acuity and scientific contributions will also find these accounts valuable. These two works provide rare insight into the inner working of a mass movement toward literacy on the part of a previously illiterate people. Linguists and language planners interested in orthography design and acceptance will recognize a previously unknown brother, of remarkable caliber, working under the most difficult of circumstances. Readers unfamiliar with the Hmong ways of being will find that Smalley, Vang, and Yang have provided the necessary cultural context. Though it is recommended that such readers start with *Mother of writing*, they should by no means neglect *The life of Shong Lue Yang*.

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Notes

1 *People movements* is here used as a general term for nativistic revitalization movements of the sort described in Wallace (1956) and Linton (1943).

2 Starting with the Third Version, this symbol included the diacritic used to represent tone. ■

Style: Toward Clarity and Grace. By Joseph M. Williams. Chicago: The University of Chicago Press. Pps. xviii, 208. $17.95. (Also available as a textbook with exercises, Style, 3rd ed., from the Scott, Foresman and Company.)

Reviewed by Charles Peck.

Of all the style books and guides to writing that I have looked at, the ideas presented in *Style* by Joseph M. Williams come comes nearest to the ideas I presented in *Readable Technical Paragraphs* (*NL* #49). Professor Williams, himself, writes with clarity and grace, and I envy the students who have used this book as a textbook during the past nine years. It is a book everyone should work through.

Most style books just give a dry set of rules. Williams gives examples that need improvement and shows how to improve them, making his book an excellent how-to book. He also insists that his rules are not hard-and-fast rules but are general principles to be violated when the context requires one to.

In chapter two, Clarity, he deals with clarifying sentences. The first principle is to de-nominalize all nominalized verbs to arrive at the basic clauses. The basic clauses contain the participants as subjects and all the actions as action verbs.

Consider the following example:

> Our lack of knowledge about local conditions precluded determination of committee action effectiveness in fund allocation to those areas in greatest need of assistance.

What are the basic clauses with their subjects and verbs? They are:

> "We did not know about local conditions,"
>
> "We could not determine the effectiveness of the committee's effectiveness,"
>
> "The committee allocated funds to areas,"
>
> "The areas needed funds."

Now look at one way to combine these clauses to make for easier reading:

> Because we knew nothing about local conditions, we could not determine how effectively the committee had allocated funds to areas that most needed assistance.

Later in the same chapter he mentions the need to avoid formulaic sentence beginning, such as "it seems that ..." "it appeared that," and metalanguage such as "we will show ..." "we will explain ..." Also, a long series of nouns in a noun phrase, such as "early childhood thought disorder misdiagnosis," should be changed to "misdiagnose disordered thought in early childhood."

Chapter three, Cohesion, talks about paragraph cohesion. First he gives two general rules:

> 1. Put in the subject/topic of your sentence ideas you have already mentioned, or ideas that are so familiar to your reader that if you state them at the beginning of a sentence, you will not surprise anyone. (p. 56)
>
> 2. Among groups of related sentences, keep their topics consistent if you can. They don't have to be identical but they should constitute a string that your readers will take to be focused. (p. 56)

Two consequences of these rules are:

> 1. You may find yourself writing as many passive sentences as active. But if active sentences create a less consistent string of topics, leave the sentences passive. (p. 56)
>
> 2. You may find yourself using nominalizations as topics because those nominalizations refer to ideas in sentences that went before. That is an important use of nominalizations: to sum up in one phrase actions you have just mentioned so that you can comment on them. (p. 56)

"Here is a quick way to determine how well you have managed your topics in a passage. Run a line under the first five or six words of every sentence (in fact under the subject of every verb in every clause, if you can do it). Read the phrases you underlined straight through. If any of them seems clearly outside the general set of topics, check to see whether it refers to ideas mentioned toward the end of the previous sentence. If not, consider revising." (p. 56) (This process is similar to the charts I proposed in *NL* #49.)

"Again, do not take this to mean that you have to make your topics identical or that all your topics have to be in subjects. A topic string is consistent to the degree that your reader can see connections in the sequence of words and phrases that open your sentences (or clauses). You will change your topic strings as you begin a new section or paragraph. The crucial point is not to force your reader to begin each new sentence in a sequence of sentences with information that the reader will find startling, unfamiliar, unexpected, or disconnected from any of the other topics or from the end of the immediately preceding sentence." (p. 56)

Chapter four, Emphasis, deals with a semantic view of sentences. The first part of a sentence is the topic, and the rest of the sentence is the stress. (p.67–8) The topic is the place for previously mentioned or commonly known participants. The stress is the place for material that is to be stressed. So two rules are:

> Shift less important information to the front of the sentence, to the topic part. (p. 68)
>
> Shift important information to the back of the sentence, to the stress part. (p. 69)

Existential and cleft sentences are useful because they let you put important information in the stress part of the sentence. Existential THERE-sentences, WHAT-pseudocleft and IT-cleft sentences can place the material to be stressed at the end of the sentence.

These rules here apply to expository text, not narrative or descriptive discourses. Williams gives an example of narrative text that is made less vivid by applying these rules for clear expository prose. (p. 76–7)

Chapter five and six, Cohesion in Paragraphs and Discourses, are coauthored by Gregory G. Colomb.

A cohesive paragraph has two consistent topic strings, one in the topics/subjects of the sentences, and another in the stress-parts of the sentences. The first topic string tells what the paragraph is about, and the second string shows the development of thought in the paragraph.

A cohesive paragraph has the following structure:

> Paragraph = Issue + Discussion (p. 92)

The "Issue" contains the first one-to-three sentences of the paragraph, and the stress-part of the last sentence of the last "issue" must contain the meaning topic of the paragraph. [Williams' "issue" includes the TEXT (and any preceding INTRO sentences) of an explanatory or expository paragraph (in Longacre's terms).]

The "Discussion" part of a paragraph contains the rest of the sentences of the paragraph. [It includes all the EXPOS of the paragraph (in Longacre's terminology).] Some paragraphs have their topic sentences at the end of the paragraph, but such paragraph-final topic sentences are rare.

The same structure applies also to essays, papers, and articles. They also are divided into an "Issue" and a "Discussion".

"Regardless of how many sentences we use to introduce the body of a paragraph (or a document or one of its sections), we have to grasp the central principle: Whether readers are conscious of it or not, they try to divide units of organized discourse—paragraphs, sections, or wholes—into two sections. 1) A short opening segment: Toward the end of this segment, in the stress position of the last sentence, readers look for the concepts the writer will discuss in the following section. Those words are often topics but they must also include themes. 2) A longer following segment—the rest of the paragraph (or section or whole): In this segment, the writer develops—and readers look for—new ideas against a background of repeated topics and themes." (p. 92)

In a larger composition, the paragraph at the end of the "Issue" must have the semantic theme(s) in the "Stress" of its last sentence. Paragraphs in the "Discussion" will normally have their semantic themes in the "Stress" part of the first sentence of the paragraph. (Only is special circumstances might a "Discussion" paragraph have its theme in its last sentence.)

In North America, we expect to see the topic and some indication of the conclusion near the beginning of an essay. Otherwise we feel unsure about where the paper is going.

Europeans and others do not put any indication of the conclusions of the paper at the beginning of the paper. They use the essay to lead

the reader to the conclusions at the end. (My feeling on such essays is that they require much more careful reading.)

"But as readers, we may have a problem with a document whose main POINT is at the end: when we begin reading the document, we cannot always be certain whether the sentence(s) that we find at the end of the issue are the main POINT sentences of the whole document, or whether we will find a more important main POINT sentence at the end of the document." (p. 104)

"In general, however, most readers ... want to see the POINT up front. So unless you can justify creating a POINT-last document, don't do it. But if you must, then you should observe two more principles of construction. At the end of the introductory issue of your document, you must,

1. offer some kind of specific anticipatory POINT sentence(s) that clearly promise a main POINT still to come; and

2. include toward the end of that anticipatory point sentence the themes and topics that you will pursue.

Whether you make your point early or late, you must always frame the space that your reader is about to enter." (p. 109)

"Headings as Test for Coherence": Headings are a familiar feature in professional writing. We usually think of them as most helpful to readers, because they give readers a general idea about the content of the sentence they head. They also show readers where one section stops and another starts and indicate the levels of subordination.

"But if headings are useful to readers, they are more useful to writers, because writers can use them to diagnose potential problems with the perceived structure of a document." (p. 109–10)

"Not all readers like headings; some feel they give a crude vocational look to writing, that good readers don't like them. Whatever your feelings, you ought not to underestimate how useful they are as a way to anticipate how your readers are likely to respond to the form of your paper. If you are not certain where to locate headings, you can be certain that your readers will find your document confusing. If you think headings are déclassé, you can always delete them." (p. 110–11)

Chapter seven, Concision, deals with reducing wordiness. Two rules are: 1) "compress what you mean into the fewest words, and 2) don't state what the reader can easily infer." Simple sources of wordiness are: redundant pairs (such as "full and complete," and "hopes and desires"), redundant modifiers (such as "anticipate in advance" and "completely finish"), redundant categories (such as "pink in color" and "accurately aligned"), meaningless modifiers (such as "for all intents and purposes" and "generally"), pompous diction (such as "intransigent," "contingent upon," and "envisage"). Complex sources of wordiness are: belaboring the obvious ("imagine a picture of ...") and excessive detail (boring, pointless, commonly known details), a phrase for a word (such as "before you do anything else" for "first" and "due to the fact that " for "because").

Metadiscourse is another source of extra wordiness. These are: hedges (such as "usually" and "apparently") and needless emphasis (such as "everyone knows ..." and "it is clear that ..."), over elaborate sequences and topicalizers (such as "In this section of this report ..." and "In this essay I will discuss ..."), attributors and narrators (giving the progress report instead of just the conclusions), and negatives (such as "not many" instead of "few" and "did not consider" for "ignored").

Chapter eight, Length, discusses how to avoid series of short, choppy sentences. We can put one subordinate clause at the beginning of a sentence and one or more subordinate clauses or appositional clauses at the end of a sentence.

Inside a clause we can coordinate predicates and predicate-plus-object and complement. "We can join grammatically equal segments with *and, but, yet, or* anywhere in a sentence. But we do it most gracefully after the subject, in the predicate. If we create a long subject, our reader has to hold her breath until she gets to the verb." (p. 116) If we coordinate unlike structures, we get faulty parallelism.

Post sentence subordinate clauses include: resumptive modifiers (clauses introduced by a noun or an adjective); summative modifiers (a clause introduced by some noun or nominalized verb that sums up the preceding part of the sentence); and free modifiers (present or past participial clauses).

"A well-managed long sentence can be just as clear and crisp as several short ones. A writer who can handle a long sentence gracefully lets us take a breath at reasonable intervals and at appropriate places; one part of the sentence will echo another with coordinated and parallel elements. And if she avoids muddling about in abstraction and weak passives, each sentence will move with the directness and energy that a readable style demands." (p. 143)

Chapter nine, Elegance, deals with how good writers manipulate the rules given so far in this book.

"Let's assume that you can now write clear, coherent and appropriately emphatic prose. That in itself would constitute a style of such singular distinction that most of us would be satisfied to have achieved so much. But though we might prefer bold clarity to the turgidity of most institutional prose, the relentless simplicity of the plain style can finally become flat and dry, eventually arid. Its plainness invests prose with the virtuous blandness of unsalted meat and potatoes—honest fare to be sure, but hardly memorable and certainly without zest. Sometimes a touch of class, a flash of elegance, can mark the difference between forgettable Spartan prose and an idea so elegantly expressed that it fixes itself in the mind of your reader." (p. 153)

"Now, I can't tell you how to be graceful and elegant in the same way I can tell you how to be clear and direct. What I can do is describe a few of the devices that some graceful writers use." (p. 153)

"We can enhance the rhythm and grace of coordination if we keep in mind a few simple principles. First, a coordinate series will move more gracefully if each succeeding coordinate element is longer than the one before it ... [Second, you] can make these coordinate patterns more rhetorically elegant if you consciously balance parts of phrases and clauses against each other." (p. 155)

Use metaphors to make your prose more vivid.

In chapter ten, Usage, Williams discusses the rules of "proper" English. He points out that most "rules" of English were created by grammarians of the late seventeenth century.

Their rules were not observed then nor are they observed today.

> "... there are different kinds of rules. 1) Some rules account for the fundamental structure of English: *I saw a horse yesterday* vs. *Horse yesterday a saw I*; 2) Some rules distinguish the dialects of the educated and the uneducated: *knowed* vs. *knew, he don't have no idea* vs. *he doesn't have any idea*; 3) And some rules belong to that category of rules observed by some well-educated people, and ignored by others equally well-educated: split infinitives, *which* for *that* ..." (p. 177)

One may choose to follow the rules of the third type if you need to satisfy some critical readers.

If we could take Professor Williams' advice to heart, our linguistics papers would be much more readable. ■

Proceedings of the Eighth West Coast Conference on Formal Linguistics. 1989. Edited by Jane E. Fee and Katherine Hunt. Stanford, CA: Stanford Linguistics Association.

Reviewed by Michael Maxwell

WCCFL 8 was held at the University of British Columbia in February 1989. Of the forty-three papers presented at the conference, thirty-four appear in this volume. The volume closes with an index of topics and languages, a useful innovation for conference proceedings. In this review, I will discuss only those papers which appear to be of greater interest to field linguists.

The papers were submitted in photo-ready form by the authors. This led to one annoying typo: pages 431 and 432 appear in reverse order. Several authors also had trouble with word processors that did unexpected things to non-English words (e.g. *fli:sa* "freezer", page 449, is split across two lines at the colon).

Eithne Guilfoyle, Henrietta Hung, and Lisa Travis: Spec of IP, Spec of VP, and the Notion of 'Subject'

The question of what is a subject in Malayo-Polynesian languages has long been a notorious problem. This paper, written from the perspective of Government Binding (GB) theory, examines the question for the languages Malagasy, Malay, Cebuano, and Tagalog. The authors assume (for reasons unclear to me) that all four languages have a common deep structure word order of SVO. Surface word orders are determined by the positions which are assigned Case. When the verb cannot assign Case to an NP's underlying position, the NP is forced to move to a position where it is assigned Case, generally to the position of the Specifier of IP (= Inflection Phrase, i.e. S). From their assumptions and slight language-particular variations in parameters, such as whether INFL (= inflection, i.e. tense/aspect) assigns Case by agreement, the authors are able to derive the various surface word order possibilities for these four languages. The analysis is fairly successful for the four languages they consider, although like many recent GB analyses it seems both convoluted and procrustean.

Aaron Halpern: Silent lexical items in the Hausa continuous aspect

"Null elements", that is, syntactic positions which are phonologically empty, are currently a hot topic in theoretical linguistics. (An example is the subject position of the infinitival in "John tried to go"; many theories assume *to go* is a clause with an empty subject position.) This paper examines a peculiar clause type in Hausa which appears to lack a verb. (A typical example is *Yana riga*, literally "Continuative-aspect shirt", but meaning "He is making a shirt".) Halpern claims that these clauses in fact have a verb, but that the verb is phonologically null. Furthermore, it is not null due to lack of lexical insertion or movement (the usual source of empty categories in theories which countenance them), but the verb is actually phonologically null in the lexicon. While this claim appears counterintuitive, Halpern supports it with a good deal of evidence. Whether you believe Halpern's claim or not, it will give you a new perspective on elliptical constructions.

Kathryn Henniss: 'Covert' Subjects and Determinate Case: Evidence from Malayalam

Another subject (pardon the pun) of recent interest to theoretical linguists is non-nominative case assignment to subjects. In some languages, while subjects normally take nominative case, a small set of verbs has subjects with "quirky" (non-nominative) case. In Malayalam (Dravidian), quirky case assignment has an unexpected interaction with the coordination of infinitival complements: subjectless and "subjectful" infinitivals may be freely conjoined, **unless** the two infinitivals are both subjectless and their verbs assign different case to their respective subjects. Henniss shows how this is problematical for both Generalized Phrase Structure Grammar (GPSG) and GB theories, but she develops a solution within Head Driven Phrase Structure Grammar (HPSG). (An alternative to her analysis might be that subjectless infinitivals of verbs which assign nominative case to their subjects are VPs, but subjectless infinitivals of verbs which assign quirky case belong to a different category—perhaps S/NP.)

Mark Hewitt and Alan Prince: OCP, Locality and Linking: the N. Karanga Verb

Hewitt and Prince develop an analysis of tone spreading for a set of recalcitrant data from N. Karanga (a "dialect" of Shona, Bantu). Their purpose is to support the "Obligatory Contour Principle" (a constraint on the well-formedness of phonological representations under generative phonology), but the analysis should be of interest to anyone dealing with the spread of suprasegmentals. However, it will be heavy going for readers without much background in Lexical Phonology.

Diane Massam: Part/Whole Constructions in English

Still another topic of interest to theoretical syntacticians is the so-called "Possessor Ascension" construction, a construction in which the possessor and the possessed appear as independent complements of the verb. (An excellent background on this construction is Baker 1988.) Massam takes a subset of such constructions, the part/whole construction ("I hit John on the nose"), and concludes that the construction is "licensed by verbs which express a localized physical

event which is undergone by a whole entity" (pg. 243). As a result, only "verbs which physically affect but do not transform their objects" (pg. 240) can participate in the construction. Massam's data is taken from English; it would be interesting to see how her results extend to other languages.

Joyce M. McDonough: Argument Structure and the Athabaskan "Classifier" Prefix

This paper treats so-called "classifier prefixes" in Navajo, which attach to verbs and have been vaguely characterized as having a voice or transitivity function. McDonough's claim is that the Navajo verb root specifies the semantics of its arguments, but the classifier prefixes specify their syntax. It was unclear to me how this analysis differs from treating these prefixes as causatives or other transitivity changing affixes.

Soo Won Kim: The QP Status of *Wh*-Phrases in Korean and Japanese

Much of the early work on *wh*-movement was based on English, in which a *wh*-phrase can be an unbounded distance from the gap it binds (as in "Who do you think Jane said Bill thought ... Mary saw?" where *who* binds a gap—a missing NP—after *saw).* Languages in which the equivalent of *wh*-words cannot be moved out of a clause are usually analyzed in a similar manner to English, the only difference being that in English *wh*-movement takes place between Deep Structure and Surface Structure in English, whereas in languages lacking long distance movement, *wh*-movement is understood to take place between Surface Structure and Logical Form. Kim argues instead that in Korean, and presumably other languages lacking overt *wh*-movement, there is no equivalent of *wh*-movement at all. Rather, *wh*-phrases and quantifiers are indistinguishable in their syntax; an ambiguous phrase may be interpreted as a *wh*-phrase only if it appears in a clause containing a question morpheme. (This is an oversimplification of Kim's analysis.) In a very real sense, Korean *wh*-phrases **are** quantifier phrases. Again, it will be of interest whether this analysis will hold for other languages lacking overt *wh*-movement.

Leslie Saxon: Control and Control Verbs: Two Sources of "Control Effects"

Drawing on data from several languages (including Dogrib, Spanish, Saramaccan, Mandarin, Romanian, and Persian), Saxon argues that what has been called "control" in reality encompasses two distinct phenomena. Obligatory control (in *try*-class verbs) involves selection of an anaphoric category by a predicate of obligatory control; non-obligatory control (for predicates other than those of the *try*-class) arises when an anaphoric pronoun happens to be in subject position. The analysis will be of interest to those familiar with GB theory. (There is an error concerning examples (3) and (4) on pg. 348: both are subjunctive, not indicative.)

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Baker, Mark C. 1988. Incorporation: A Theory of Grammatical Function Changing. Chicago: University of Chicago Press. ■

Markedness Theory: The Union of Assymetry and Semiosis in Language. 1990. Edna Andrews. Durham, North Carolina: Duke University Press. Pp. ix, 220.

Reviewed by Karl Franklin

Pacific Area

Although the theory of markedness is associated with Trubetzkoy of the Prague School, it was Roman Jakobsen who first illustrated the principles clearly in his 1957 analysis of the Russian verb (reprinted in *Selected Writings, II*, 130–47). Still later Joseph Greenberg popularized the findings in his monograph on *Universals of Language* (1963). Markedness theory grew out of Jakobson's theory of oppositional binary relations, which was first proposed, according to Andrews (p. 13), in 1921. Andrews now provides an extensive study of the theory of marked and unmarked categories examining, in

particular, the writings of Jakobson, G. S. Brown, Thom, Pierce, and van Schooneveld.

Andrews divides her study into five parts: (1) The Principles of Jakobsonian Markedness Theory; (2) Peirce and Jakobson Revisited; (3) Markedness Theory as Mathematical Principle; (4) Myths About Markedness; and (5) The Category of Grammatical Gender in Russian, Serbo-Croatian, and Modern Greek. I shall have little to say about (2) and (3) and shall begin with (3).

The first myth about markedness is that it is related to statistical frequency. Andrews, however, claims that "within the framework of Jakobsonian-based markedness theory, the justification for the determination of markedness correlations cannot be established based on statistical frequency" (p. 137). Rather, statistical frequency is representative of a tendency. It is not invariant and is complex because "markedness oppositions in morphology and semantics involve more than two categories" (p. 139). The second myth has to do with neutralization, which "entails the systematic cancellation of an opposition in a particular syntagmatic environment" (p. 139). Andrews argues that the phenomena of syncretism in morphology, which can be compared to neutralization in phonology, is not purely distributional, but is a "paradigmatic double signaling by a particular form", such that categories are paradigmatically based (p. 143). The third myth is about markedness assimilation, where a normally unmarked value for a given feature occurs in an unmarked context and the normally marked value in a marked context (p. 144, quoting Shapiro). Anderson feels that this principle applies only to phonology and does not meet the requirements of a rule, based as it is on a large number of assumptions. A fourth myth is on markedness reversals, such as the neuter gender in Russian cases shifting to an unmarked position in caseless forms. As Andrews explains, no one method can be used to determine which member of a given opposition is marked (p. 155). In fact, "the basic methodology of Jakobson and others in morphology has been to derive a set of invariants from a definitive source of language data in order to explain the occurring phenomena" (p. 156). The final and most problematic myth involves substitutability (Greenberg's facultative expression). Andrews demonstrates that since hierarchy implies that two elements in a hierarchy are not equivalent, then in a strict paradigmatic sense substitutability is not possible.

Having defined in a general way what markedness is not, we shall now turn to Chapter 1, which outlines the classical theory according to Jakobson.

In 1936 Jakobson analyzed the Russian case system, showing that each of the six cases could be described in terms of three invariant, semantic features (directionality, marginality, and quantification). Once Jakobson transported his theory to the U.S. he referred to distinctive features, or binary opposites. Trubetzkoy never followed the theory of purely binary features in the phonology, instead referring to the three types of phonemic oppositions: privative, gradual, and equipollent. In the privative sense two phonemes are identical except that one contains a "mark" which the other lacks; gradual refers to degrees of some property; and equipollent oppositions refer to those where each member has a mark that the others lack.

Andrews divides the schools of linguistics which use the markedness theory into two camps, those who work with meaning and those who attempt to describe language as a purely formal system (p. 16). Chomsky and Halle, for example, applied markedness to English by general distributional rules and disagreed with Jakobson by omitting a distinct phonemic level for phonology.

Among those linguists who have applied markedness theory to meaning are Greenberg, Lyons, and van Schooneveld. Andrews demonstrates van Schooneveld's concept of deixis in linguistic sign systems. He distinguishes between transmissional deixis (making reference or pointing) and perceptional deixis, which is defined by the narrated situation, not the speech situation. The unmarked type is the latter, but all forms must inherently possess some deictic referent in order to be meaningful (p. 35).

Chapters 2 and 4 give details on theoretical viewpoints and justifications for the viewpoints of Peirce and Jakobson. In providing this Andrews is attempting to reconcile their positions. Entering into the discussion are comments on interpretants, icons, indexes, symbols, and artifices. The going gets heavy, with remarks on dyads and triads, as well as deductive, inductive, and abductive reasoning. One can follow the main argument by noting the axioms for markedness, namely that (1) a difference in form signifies a difference in meaning; (2) every set can be well-ordered; (3) given a set which is ordered, a

binary relation can be defined to explain the ordering; (4) for any two sets, there exists a set that they both belong to; and so forth (p. 97 ff).

For those who like to see some application of a theory, Chapter 5 is a relief. It discusses the category of grammatical gender in Russian Serbo-Croatian, and Modern Greek. As Andrews establishes, the feminine gender is not always the "marked" gender in languages (p. 166). This is hardly big news to linguists like Bob Litteral and his studies on the Angor language of PNG.

We can best summarize the contributions of Jakobson and others with Andrews' own comment:

> Markedness theory is a tool that enables the linguist to define more precisely the systematically-given oppositions and hierarchies represented by linguistic categories. ... if applied rigorously [it] will prove indispensable in uncovering the fundamental principles that define the broad spectrum of linguistically-given oppositional relationships, encompassing all levels of linguistic meaning (p. 187).

There is no doubt that the theory has been helpful for SIL fieldwork and will continue to be so in the future. ■

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McCawley, James D. 1988. The syntactic phenomena of English, Volumes 1 and 2. Chicago: The University of Chicago Press. 768 pp. ■

Fifth International Conference on Functional Grammar

The Fifth International Conference on Functional Grammar will be held at the University of Antwerp, Belgium, from August 24–28, 1992. The conference will be devoted to recent growth in the theory of Functional Grammar as developed by S. C. Dik.

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NOTES ON LINGUISTICS

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Coordinator's Corner

In this issue, Geoffrey Hunt presents *A Good Phonology Program*. He is talking about something into which an immense amount of time and planning have been invested. One of the concerns that many readers might well have before they try to use such an analysis system is, will it digest my data as it is? The answer: it depends on how your present phonetic data are coded. FindPhone 5 requires data to be in the format it is set up for, without variation, but the good news is that the creators included the ability to display on screen both CGA composite characters and the multi-stroke representations of the FP5 system, so by using FP5's editor, you can produce a consistent changes table with reasonable assurance that the equivalences you see are accurate.

Do you have too much data in an article you have prepared, yet don't want to release the article without including the data? Periodicals indicate that standards for electronic publishing are being discussed and set, which is a necessity, given the variety of machine configurations and wordprocessing software combinations prospective readers will have. If readers of NL indicate interest, in a future issue we'll try to include some information on developments in electronic publishing.

I have been told that one of the reasons the Native Text Series of IJAL withered away some years ago is that, in the published texts, the authors did not include aligned, morpheme-by-morpheme analyses of the text lines. It was just too hard to do with the typewriter technology of that day, but the unanalyzed product was not usable by linguists who would otherwise have welcomed the contribution. Now SHOEBOX is being revised at JAARS, Waxhaw, NC, for its second release, this time with capability of handling the SIL font system. It won't be ready until sometime after July 1992. Its use will make it possible to overcome the limitations of the first attempt to make native texts available, and the number of linguists linked together via a university computer network would seem to be an indicator that the potential market for electronic data is growing.

That network is what provided the contributions for the tribute to the memory of Dwight Bolinger. Never having met him, I remember him most for insightful articles highlighting data that current formulations of linguistic theory didn't account for.

-Eugene Loos

A Good Phonology Program

Geoffrey Hunt

INTRODUCTION

I have been involved in SIL with the production of computer programs for linguists since 1982, and from the early days have been a proponent of the view that linguists will not use computer programs effectively until the computer programs themselves become more friendly. Many people have heard me express it this way: "If your computer is so clever, why doesn't it speak my language?"

In January 1991 I was travelling around in Germany visiting various language projects and working with the linguists involved. I shared with one team a new version of the FindPhone program, version 5, and the reception was so enthusiastic that I decided to share it with another team, and again I got the same enthusiastic reaction. There had also been enthusiasm for CECIL and Shoebox. I stopped to ponder this phenomenon. For the first time in more than eight years, I had found an enthusiasm for some of our SIL computer programs among linguists that did not take easily to computers. I realized that we had turned a very important corner in the struggle to make these programs friendly.

This was underlined for me recently by the enthusiastic response I got when teaching at the French SIL School. The students had already been instructed in the basics of using a computer, so I started with linguistics programs, teaching first the programs the students would use in their forthcoming language project. While I was teaching, the students were seated in front of computers, with approximately three students per computer, using the appropriate program. I taught CECIL and at the same time gave a short course on acoustic phonetics. I then taught FindPhone, and during most of the first lecture, we spent our time searching linguistic data rather than worrying about how to use the program. Finally, I got to Shoebox, but this was a bit more complicated because of all the options that are available to the user.

WHAT MAKES A PROGRAM FRIENDLY

There are a number of features that seem to make a program friendly, which I judge by the way people react to new programs and also, to some extent, by how easy I find it to get started in a program. Clear, meaningful menus, the ability to escape from any situation, in-built helps, and windows seem to be most important. For non-English speakers, the ability to translate menus, prompts, commands, and helps into another language is very important. My experience suggests that icons and a mouse are not important, except for specific types of programs such as desktop publishing. However, even though a mouse is not essential, it may still be appreciated.

Firstly, then, menus. Menus must appear on request and must try to avoid computer jargon. If the titles within a menu are too abbreviated—a fault that could be directed at the main CECIL menu—they will be more difficult to learn. Where necessary, it must be possible to pull one menu out of another menu so that the particular choices made in reaching the present position can be clearly seen.

It has been argued that it is essential that menus on different programs be similar in order to help the user learn each program. I am sure there is some truth in this argument, but there is also some evidence that any good menu system, if it is optimized for the task in hand, will greatly benefit the user. As a case in point, one could consider FindPhone 5: it has a very different menu structure from anything else I have seen, but it is very easy to learn and use. As long as the user remembers to use ESCape to get out of any window, including to save data, the rest is fairly intuitive. When the user has followed a trail of menus down a wrong rabbit trail, it is essential that he or she be able to escape from the situation by just pressing the ESCape key, perhaps several times.

In-built helps vary enormously. Some programs like Shoebox have limited context-sensitivity but allow access to a menu of helps. Those for CECIL are very context-sensitive, which is helpful, but they need some rewriting.

Windows are very helpful in showing relevant information that is pertinent to a particular job. Shoebox, for example, allows excerpts from both a text file and a database file on screen at the same time.

Some of its menus also produce windows of information to help the user. CECIL allows between one and six frames on a screen at the same time and will also produce further windows to indicate, say, what the numerical readout of the frequency is. FindPhone 5 also makes good use of windows, but in a different way.

Only a few SIL programs have the facility to be translated into another language without any change to the program itself. CECIL and ED are the only programs I know that do this, CECIL rather better than ED. The French translation of CECIL still needs improvement, but that is only a question of changing a text file.

I have not mentioned documentation. Obviously, for a complex program like Shoebox, very detailed documentation is needed, both as to how to run the program and how to do linguistics with it. Now the Shoebox documentation is among the best that we have for any SIL program, but if the program itself were not friendly, the good documentation would not make enough difference for the program to be used widely.

THE HISTORY OF FINDPHONE

FindPhone started life as a small program written for use at the British SIL School in January 1985. Over the years it has been enhanced by various programmers, and it has continued to be used for the major language analysis project each year.

Late in 1989 David Bevan suggested that he rewrite the whole package to make it more friendly and more powerful. I encouraged him to do so and got hold of the other two SIL phonology packages for him. From FindPhone 4, the other two phonology packages, his experiences during the Field Methods course, and the requests of other students, he put together the specifications for FindPhone 5. The basic program was written in Britain during 1990 and is now being finished in Papua New Guinea.

FINDPHONE VERSION 5

The new FindPhone program (I have version 5.03) may be used on an IBM-compatible computer with CGA, DSCGA, HGC, or VGA graphics. Only one floppy drive is needed, but it is better to have two

or a hard disk. When starting the program, if the settings file indicates that the user has been working on a file or files, these files will be loaded immediately and checked for any errors. Figure 1 shows what FindPhone 5 might look like at this point.

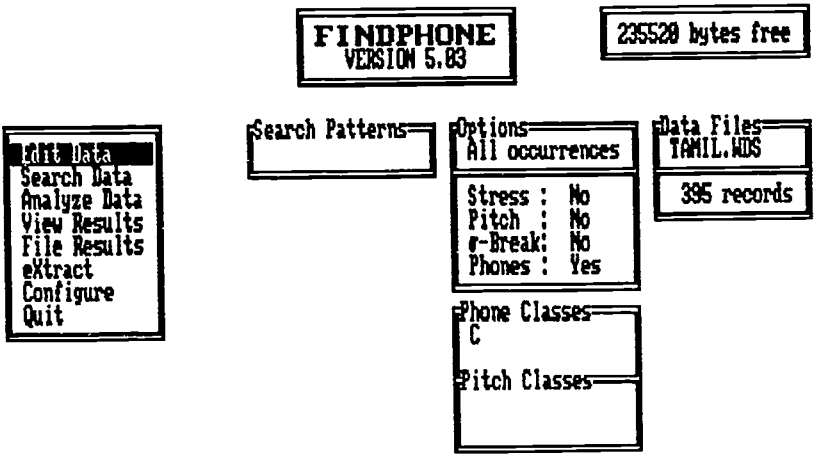

Figure 1. FindPhone 5, immediately after loading files

Data may be typed in using IPA. It was originally hoped to include the American Phonetic Alphabet (APA) as well, but there was so much to the IPA that it became clumsy to add anything else. Then IPA began to be more widely accepted in SIL, so it was decided not to include APA.

Pitch patterns may be recorded as pitch patterns above the utterance, just the way we were taught in our phonetics classes.

The keystrokes to be used for typing phonetic characters are defined by the program, and the program knows how to distinguish a phone, even if it is made up of several bytes. This knowledge of what a phone is also allows the program to automatically produce a list of sounds that are found in the data for the classes C (consonant) and V

(vowel). These may be edited so that they are always presented on screen in the form of a phonetic workchart. Other sound classes, for example, N (nasals), may be obtained by editing either the C or V class. Figure 2 shows a class of sounds on the screen.

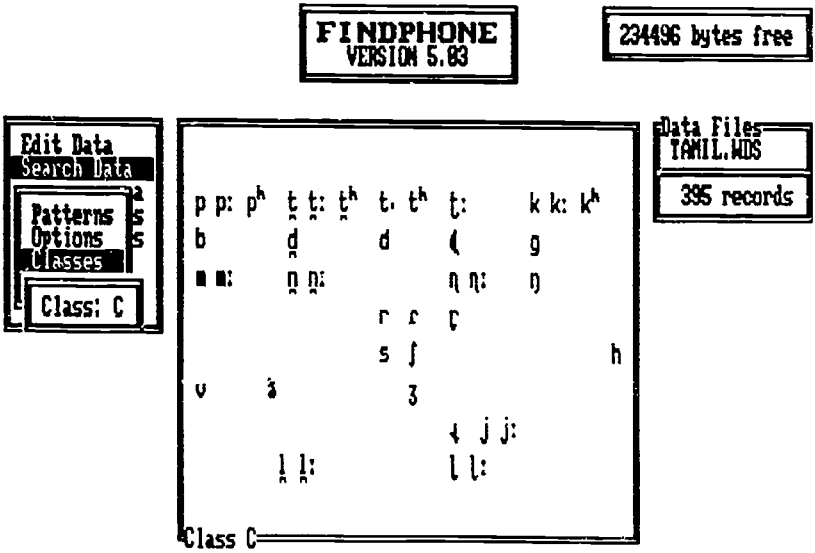

Figure 2. The consonants found in the data

The search facilities found in FindPhone 5 are very fast and very powerful. The data is all held in memory, which together with optimized routines allows searches to be fast, even when a search is complex, such as in terms of a sound class in a defined environment and when the computer has only an 8086 processor. The search facilities are smart enough to take uncertain data into consideration, and to make sure the search produces as results all possible examples of the desired combination(s) of phones. Searches can ask just for one example or for a count. This is very helpful when searching in terms of sound classes.

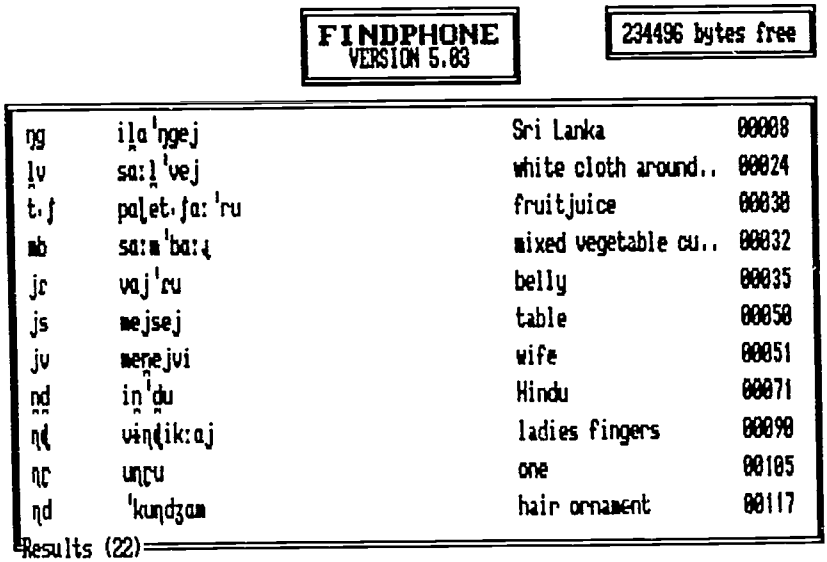

Figure 3. One example of every CC combination

The results of a search may be viewed, sorted, systematically pruned, and sent to a file or printed.

There is much more to FindPhone 5 than this brief introduction, and there are two features that have not yet been implemented: namely, the listing of minimal pairs and the production of contrastive opposition charts.

There are only two criticisms I would level at the program. One seems to be a bug, namely, that under MS-DOS version 2 it has problems when it comes to extracting data, but very few people still use DOS 2. Secondly, on the distribution disk all the sample data files are only lists of words, whereas phonological analysis should contain sentences and even full texts as divided by pauses. The latter problem can be easily remedied. I am not sure about the former.

The program disk contains a batch file that helps the user to write articles, such as phonologies, that need to use phonetic characters, allowing the results of searches in FindPhone to be imported into such articles.

CONCLUSIONS

In my opinion this program is a winner, and I have seen a lot of other people become enthusiastic about it. I hope that by the time this article is published the finished version will be available. I know there are going to be more features in it than were originally planned.

It is my opinion that every phonology consultant should encourage consultees to use FindPhone 5. It would then be much easier for the consultant to help the consultee because the consultant could very speedily search the phonetic data for relevant information. Even without knowing anything else about the language, significant discoveries could be made. ■

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Formal Linguistics and Field Work

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1 INTRODUCTION

1.1 Purpose and organization of paper [1]

The purpose of this paper is twofold:

(i) to argue that field-training courses, such as those offered by the Summer Institute of Linguistics (SIL), should include training in formal linguistics and

(ii) to argue that any research program which sees linguistic form as primarily a reflection of communicative function is going to run into serious difficulties accounting for those areas of grammar wherein the principle function may be representational rather than communicative.

Since we have no principled basis for assuming *a priori* that either of these functions best accounts for linguistic form (in fact, it is more likely that this question is unanswerable), it would be mistaken to assume the priority of communicative function in accounting for linguistic forms. The relation of form to function cannot be settled *a priori*. It is an empirical issue. While I come down here on the side of formal linguistics, by arguing that there is an epistemological priority to form over function and that forms must be studied prior to studying their functions, the main point expressed is that both perspectives are valuable.

The paper is organized as follows. In the remainder of this section, I briefly mention the lamentable state of division in linguistics between the formalists and the functionalists. This is followed by explicit definitions of the terms *theoretical linguistics* and *field work*, as I use them here, concluding with a statement of the major arguments to be pursued (in outline form) in the paper. The next major section takes up the notion of *function*, showing that this is not so trivially identifiable as we might otherwise think. That is, communicative

function is not the only function of language. This section also discusses Chomsky's (1986) notions of *E-language* vs. *I-language*, arguing that form is prior to function. The third major section discusses one example, chosen at random, that illustrates my contention that there are indeed many linguistic problems that do not involve communicative function in any transparent way. The final section argues that the greater difficulty inherent in training field workers in formal linguistics is worth the effort, although no one approach should be taught to the exclusion of others.

1.2 Division in the field

Linguistics, like other scientific disciplines, is divided into various subareas and perspectives, each with its own particular research objectives and agenda. This is a common situation in a healthy science. However, such division could be detrimental for a discipline if any subarea behaved as though it had a monopoly on truth.

Unfortunately, linguistics today is divided in this detrimental sense into hostile factions which too often pride themselves on ignorance of what other factions are doing and the idea that their peculiar perspective is the definitive one. This leads to misleading caricatures of the other groups and obstructs the view of language otherwise available to newer linguists.[2] Two factions illustrating this divisiveness in synchronic analysis of grammar today are commonly known as the *functionalists* and the *formalists*. The functionalists study language as a communication system guided by a general, all-purpose, central cognitive system. They analyze linguistic units in terms of their communicative function and generally emphasize that discourse is **the** vestibule that leads to grammar.

Formalists' objectives and assumptions differ from those of functionalists, sometimes drastically. For example, they assert that forms should be studied independently of their communicative function—in fact, that knowing what the communicative function is will be of little use in understanding the majority of linguistic forms and principles. They tend to understand linguistic structures and the principles which constrain them as determined primarily by a non-general, highly specific cognitive module. They also argue that the highest level of purely linguistic organization is the sentence, not the discourse.

These two factions often do not get along. Members of one group almost never go to meetings organized by the other group. With a few exceptions, e.g., the journal *Language*, which accepts articles from any group as long as they are of high quality, they usually publish in separate journals (e.g., *Linguistic Inquiry* vs. *Studies in Language*). It is rare to find people in either camp who seem knowledgeable about what the other group's research objectives are. This "pitched battle" mentality is so unfortunate, especially for beginning students. We do indeed need both perspectives. Jakobson (see, for example, Jakobson (1990)) argued, as others have, that sciences, including linguistics, must progress via dialogue between the various parties concerned about the discipline. This attitude must be inculcated in all of us.

Yet, I think that we too often avoid dialogue or act as though the dialogue is over and either that general agreement has been reached (the myopic, group-internal view) or that no agreement is possible. What concerns me most in this regard is that formalist approaches seem to be often mischaracterized or misunderstood within SIL, especially in regard to the importance of formal linguistics to field work. The common misconception is that while functionalists do field work and are concerned about lots of languages, formalists stay in their offices and talk about English or reanalyze data published by functionalists. It is even heard occasionally that formal linguistics is a branch of speculative philosophy and of little use to field workers or "real" empirical investigation. A supposedly bottom line objection to teaching formal linguistics in SIL is often stated like this: "Even if formal, theoretical linguistics had something to tell us from time to time, it is too complicated and obtuse to bother learning—the returns are not worth the price of initiation" (i.e., learning the relevant literature and jargon).[3]

In this paper, I am going to argue that formal linguistics is crucial for field workers and that solid field work cannot be done without it. However difficult it may be to break into the literature on formal linguistics, the price must be paid. In particular, SIL and other field-training organizations would be better served if they gave greater attention to formal linguistics in educating their workers/researchers. Of course, this paper simultaneously shows that formal linguistics depends on field work in a vital, ongoing fashion. We cannot learn all there is to know about Universal Grammar by just studying a couple of Indo-European languages.

1.3 Definitions

I will understand *formal linguistics* in the Chomskyan tradition to be that branch of science which seeks to discover and understand cross-linguistically valid principles of the nature, representation, and organization of grammatical forms and the mental basis of these forms which accounts for their acquisition. This enterprise will necessarily involve quantification and schematization of its proposals and results; it will require specialized jargon to increase expressive precision and save time in communication, thus rendering it relatively opaque to nonspecialists. As is the case with all scientific fields of investigation, the results obtained via formal, theoretical linguistics may be at odds with our naive intuitions about the world and thus are to be evaluated via their empirical and conceptual success in predicting (positively and negatively) an agreed-upon range of phenomena to be accounted for, rather than on their *a priori* plausibility to the nonspecialist.[4]

By *linguistic field work*, I mean sustained, focused, empirical investigation and collection of linguistic utterances from real people. Language learning and analysis both constitute field work. Indeed, I take language learning to be a prerequisite for analysis.[5]

1.4 Time and resources

In the context of SIL or other training for field workers, however, there are two important practical issues to deal with that touch on this larger problem of division in the field (in addition to the sociological problems alluded to above). First, linguists need to develop a talent for "brainstorming" or "hypothesis generation." This can often lead to insightful analyses by breaking down thought barriers and seeing solutions in new ideas outside of one's normal, comfortable range of assumptions. More effective brainstorming results from having more ideas—ideas are fodder for thought. To get problem-solving ideas, though, linguists need to read and broaden their knowledge of the field. We should oppose any suggestion that linguists be encouraged to think along narrow, elitist lines, avoiding contact with broader inter- and intradisciplinary work on language. This consideration alone would force us to offer something more than a passing mention of the principal currents in the field to our students.

The second practical consideration, however, seems to contradict the first: field workers are limited in what they have time to learn. They are faced with various practical problems and cannot dedicate themselves continuously to linguistic learning. In addition to trying to coax useful data out of the mouths of frequently indifferent native speakers, the linguist will also be concerned about improving the language community's general well-being. When these concerns are added to demands on daily living faced by field workers, e.g., hauling water, digging outhouse holes, cleaning fish, suturing wounds, and getting sick, it is not surprising that field workers and the organizations that train them are highly selective in how they invest their time. When it comes to linguistics, we might be excused for learning and teaching those perspectives which are perceived as providing the maximum "return" on the minimum investment of time.[6]

Now, there is no doubt that the theoretical literature is harder to break into than typological or functional work. Compare most articles published, say, by Chomsky, with just about anything published by Comrie, and you will see what I mean. Articles by the former explicitly require much more knowledge about technical terms and concepts than those by typologists, e.g., Comrie (not meaning to overlook the unusual clarity with which Comrie writes). Nevertheless, I want to argue here that formalist literature is worth "breaking into"; that field workers cannot do an adequate job without formal linguistics; and that formal analyses are a prerequisite to functional analysis. My arguments will include the claims that:

1. Form is epistemologically prior to function. So, forms must be understood *qua* forms before any understanding of their communicative function can be gained.

2. Communication may not be the basic function of language. As Fodor (1975; 1983; 1987) and Chomsky (1980; 1986; 1989) have often pointed out, language's principle function may be to serve as a computational representation system for the expression of thought. If so, it may have properties that are appropriate for a computational system, e.g., economy of representations and deviations, which are at odds with communication needs (which might favor **lack** of economy to cope with environmental "noise").

3. The grammar of sentences cannot be determined or does not "fall out of" a grammar of discourse. Indeed, talk about a "Grammar of Discourse" uses the word *grammar* in a different sense, only distantly related to its sense when applied to sentences and their constituents. That is, discourse is **not** part of a grammatical hierarchy with sentences, although, to be sure, the analysis of discourse is logically and empirically dependent on the analysis of the sentences of which it is composed. Part of the controversy over form and function may derive from two common usages of function which should be, but are not, distinguished.

2 ON FORM AND FUNCTION

2.1 Notions of function

An illustration of the two uses of the term *function* is found in tagmemics. One of the useful insights of tagmemics was the recognition that linguistic units are form-function composites, as in the analysis of (1):

(1) a. John eats goobers.

b. $S = \frac{S \mid NP}{A \mid} + \frac{V \mid V}{P \mid} + \frac{O \mid NP}{U \mid}$

The formula in (1)b is to be read as follows: *subject* (S) **functions as** *actor* (A), and the subject *slot* is manifested by a noun phrase (NP). But to say that *subject* "functions as" *actor* in (1) is to ascribe to the subject a function **internal** to the linguistic system. The notion of function, i.e., internal to the linguistic system, is to be sharply distinguished from notions of function which involve **system-external** functions, e.g., contextual salience, foreground vs. background, information flow, social flow of interlocutors, etc. If I understand this part of tagmemics, the empty cells in (1)b were intended to include this latter type of information, but it could never be adequately formalized; I am not surprised. This second conception of function (which tagmemics used to call "cohesion" in my early linguistics career) is very abstract and amorphous; in fact, it actually denies the existence of a linguistic system autonomous from context, if taken to its logical conclusion. I think that tagmemics' efforts to incorporate both system-external and system-internal notions of function were admirable, given its objectives, in spite of the problems that this led to. Nevertheless, this effort seems to have resulted from a failure to

distinguish between these two notions of function. The fact that theories run into problems by failing to make this distinction means that they need to more carefully define their object of inquiry and to be conscious of the logical and practical need to separate form from function.

2.2 E-language vs. I-language

A related question may seem unnecessary, but it is worth asking just the same: what is it that linguistics studies? To some readers, it may come as a surprise to hear that many of us do not see our principle object of study as "Language." In fact, it is hard to imagine any discipline which **could** study Language. To see this, just reflect on what you mean when you use the term. Most people will not focus on a single object but on a hodgepodge of cultural, linguistic, and political values and behaviors. So, what is Dutch? Spanish? Standard English? "Language," whatever it refers to, is inherently transdisciplinary. Yet this transdisciplinarity of language need not cause us to despair.

There is indeed a coherent area in language that a single discipline can study. Chomsky (1986) has helped clarify this issue by introducing the terms *External (E) Language* vs. *Internal (I) Language*. On a particular interpretation, E-language is an enumeration of formal units that we find in communication, expression, and society in the broadest sense. I-language is the system of grammar and the lexicon which E-language draws upon—but it is only a part of E-language, along with cultural values, politics, gender, etc. E-language affects discourse structure and sentence selection. However, E-language does not actively affect sentence **form**, *per se*. An E-language "grammar" in this sense would be nothing more than an extensional set, a list, of the allowable sound-meaning matchings that discourse can use. The assertion that discourse and sentence structures are different objects is supported by the fact that while people vary in their ability to construct fluent discourses, no native speaker is unable to produce grammatical sentences. So, for example, it makes sense to ask who is the best story teller in a certain group but not who is the best sentence utterer. Discourses depend on background information and ability to subtly integrate external context and intersentential relations. Sentence structure *per se* does not.[7]

2.3 Form is prior to function

It really is important to study forms independently of function. Biologists have long known this. So, consider Darwin's admonition in 1859: "In considering transition of organs, it is so important to bear in mind the probability of conversion from one function to another" (p. 191).

It takes only a little reflection to see that forms get separated from functions. Anyone who has ever had a wisdom tooth or appendix removed knows that these are body parts with definite forms, but with little or no function. That they likely **had** a function is beside the point. Other examples include lungs, which used to function as swim bladders, and salt in the blood, presumably from our previous existence as fish. All of these examples just illustrate leftover forms. However, they illustrate something more profound, I think, namely, that functions are often determined by the forms, rather than forms by functions. One of the best examples of this is wings. Wings may have been built out slowly from more and more efficient bodily heat regulators. They certainly did not originate first as instruments for flying (i.e., no prebird creature was just born one day with fully formed wings and then learned what they were for). The **function** of flying was made possible by, i.e., subsequent to, the **form** that thermal regulators came to have. In fact, all of natural selection is predicated on the assumption that unmotivated changes in form (mutations) led to new functions for the organism. Natural selection assumes the priority of form over function.[8]

Therefore, we would be seriously misguided to always study form in connection to E-language functions. However, let us set these arguments aside and assume instead that no matter what anyone says, function drives form. Then, the question arises—what is the function of language? Communication? But this just cannot be the principal function of language; at the very least it **need** not be and probably isn't. From evolutionary and synchronic perspectives, communication is logically dependent on thought formation and expression. Any expressional system rich enough to support communication requires a representational, computational system (whether symbolic or connectionist, although in the latter case, "representation" is perhaps not the best word) to support it.[9] Moreover, as Fodor (1983; 1987) has pointed out, this representational system must not permit intrusion by general cognitive processes. That is, it must be

informationally encapsulated. In this sense, language is like other perceptual input systems, such as vision or hearing. So, just as you do not have to think about seeing in order to see (i.e., vision is automatic in the nonpathological case), so linguistic competence is automatic (which is why nobody has to think about understanding sentences in their native language—they just understand them).[10] In this sense, function becomes meaningful only within the I-language, i.e., the computational system (howbeit perhaps inefficient in its computational design at some points; cf. Chomsky 1989). So, principles and domains of grammar, e.g., phonetic form, logical form, the projection principle, the 1-advancement exclusiveness hypothesis, etc., have functions **within** the I-language, with no obvious connection to the E-language. However, it is consistent with formal linguistics to argue that I-language **outputs** can be given communicative functions.

I-language thus produces sentences which are organized into larger communicative units called discourses. But sentences are I-language outputs while discourses are E-language entities. So, discourses reveal no "informational encapsulation." They in fact force us to draw upon all we know about our culture, language, and world to effectively understand them. Poetry is an excellent example of this. It is no wonder then that those who fail to distinguish discourse from sentential syntax as qualitatively distinct categories also believe that language is governed by a general cognitive mechanism, rather than a specifically linguistic cognitive module. Let me sum up what I have intended to say to this point:

(2) a. I-language and E-language are logically independent objects of study;

b. The construction of I-language forms should be studied independent of (i.e., as epistemologically prior to) E-language functions;

c. I-language issues are a subset of E-language issues.

I take I-language to be the focus of formal, theoretical linguistics and E-language to be the object of inquiry for functionalist theories. But we should note a very interesting fact: E-language is abstract, while I-language is more concrete and empirically testable. That is, I-language is concerned with sentences and their structure, and thus its data and objects of study are much more accessible to researchers than E-language objects and notions, e.g., function. This will become

clearer as we consider an example of an exclusively form-oriented problem.

3 EXCLUSIVELY FORM-ORIENTED PROBLEMS

There are numerous problems which face field workers at the outset of their field experience which have exclusively formal solutions, i.e., which do not admit in any obvious way of a functional analysis. I will discuss one example of this, from myriads of examples that could be chosen.

Consider the phenomenon of reduplication in Tupi-Guarani, as analyzed by Everett and Seki (1985):

(3) a. *omotumun* 'he shook it'→ *omotumutumun* 'he shook it repeatedly' (cf. **omotumuntumun*)

b. *omokon* 'he swallowed it'→ *omokomokon* 'he swallowed it repeatedly' (cf. **omokonmokon*)

This pattern is found almost without exception throughout the Tupi-Guarani (TG) family. The crucial thing to notice is that the addition of the reduplicative suffix *-mokon* in (3)b causes the final *n* of the root or stem to delete.

This follows from a general prohibition against consonant sequences in TG. There are no word-internal C-sequences in TG, and should a sequence be created by the syntax or morphology, it is simplified by the deletion rule in (4):

(4) $C \rightarrow \emptyset / C \_\_\_\_$

Compare the nonreduplicative sequence in (5):

(5) *akan* 'head' *meb* 'wide'→ *akaneb* 'widehead' (cf. **akanneb*)

Thus far, the problem of C-deletion in TG is trivial. But now consider the reduplicative example in (6):

(6) a. *o-etun* 'he smells'→ *oetuetun* 'he keeps on smelling' (cf. **oetunetun*)

b. *a-pot* 'I jump'→ *apoapot* 'I keep on jumping' (cf. **apotapot*)

Why does the final stem consonant in the forms in (6) delete? The stem-final consonants in these examples are apparently not followed by a consonant, since the reduplicative suffix here is vowel-initial. Thus, the rule which deletes stem-final consonants in reduplication might be different from the rule which deletes stem or word-final Cs in sequences such as (5). So, we might have to propose a rule like that in (7), as was indeed originally proposed by Rodrigues (1953):

(7) C → Ø / _____ [+Reduplication]

On the other hand, if an analysis were able to unify these two accounts of C-deletion, it would be on the face of things superior to an analysis in which they are disjoint. As Everett and Seki (1985) point out, a unification of deletion processes in Tupi-Guarani languages is possible only within a multilinear phonological framework, where the structural description and structural change of rule (4) are stated at the level of the CV-skeleton, along the lines of Marantz (1982). (The reader is referred to the jointly authored paper just mentioned for further details.)

The point is that the account of this very important morphological process in these languages has no obvious functional motivation. Notice that this is not merely an example of a single form without a communicative function. It is intended to be an example of an **entire grammatical process** with no communicative motivation. Rather, the motivation is to be found within the formal apparatus provided by the multilinear phonology as a model of the linguistic computational system involved, i.e., the phonology. To solve this problem, formal linguistics was crucial. And this type of problem, in my experience as a consultant, is much more prevalent among beginning field workers than discourse genre identification, topic treatments, etc. There are many other examples that one could give. Virtually any treatment of syntax, morphology, or phonology that are found in journals like *Natural Language and Linguistic Theory*, *Linguistic Inquiry*, etc. deal with exactly this kind of issue—formal problems with no testable or transparent link to communicative need.

4 FORM VS. FUNCTION AS INVESTMENT "RETURNS"

Now, any field worker with an ounce of curiosity wants to know about more than the purely formal, grammatical aspects of the languages s/he is studying. They want to know how these structures are put to

use, how they are interwoven within the culture and context of the speakers of the language.[11] All of the really interesting answers to this type of question that I am aware of involve the **utilization** of forms to satisfy communicative needs, not the structuring of forms for this purpose. Communicative structuring only applies above the sentence. Again, grammar is logically separate from the use of grammar.

There are some obvious candidates to serve as counterexamples to this claim. So, for example, what about things like evidential affixes (affixes which supply information as to the reliability, source, or value of the evidence for a particular report on an action)? Don't these obviously illustrate the morphological manifestation of information at the level of the speech act? Of course they do. However, formally they are still just affixes, simply filling a slot on the verb. Sometimes there is a great deal to say about the formal interaction of evidentials with other affixes, sometimes their morphosyntax is trivial. How evidentials and other clearly speech-act related suffixes are used never impinges on such issues as the formal constraints on their morphological position (e.g., affixes can never be moved away from their root/stem), their morphophonemic properties (e.g., whether or not they will undergo internal sandhi), etc.

Now, there is an enormous history to this issue, and the interactions or claimed interactions between function and form-building (as opposed to form-selection) are numerous and occasionally sophisticated.[12] In the context of SIL training, however, the problem is not merely a theoretical or philosophical one. It is a practical matter. Students with only two semesters of grammar who have been taught to look for form as an output of function are just not going to be equipped to analyze facts like Tupi-Guarani reduplication except as they set aside this assumption momentarily and work with the formal analysis procedure they have learned, including any reading they might have been exposed to from modern phonological theories. Only a formal account of these facts is possible (any reader can take this as a challenge).

The functional accounts that might be proposed for the grammar of the sentence and its constituents are always going to be more difficult to falsify, often less rigorous, and usually more difficult to apply in the beginning stages of field work than formal accounts. Field workers are much more in need of the type of skills that they will

obtain via formal analysis, uncluttered with speculation as to communicative function (in the naive assumption that communicative function is always more important than representational/ computational function, or that there is any principled way to evaluate one function with regard to another or to determine with complete certainty which is the function in focus in a particular grammatical or lexical constituent).

Spending the time necessary to learn about (and keep on learning about on the field) formal models of syntax and phonology are more likely to provide a better "return" from the prefield training investment than functional linguistics. This does not deny the importance of going on to learn about functionalist approaches. It does establish a priority of what will need to be learned via an instructor.

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NOTES

[1] This paper was originally given as a colloquium at the University of Texas, Arlington and the International Linguistics Center, Dallas, Texas. I want to thank Des Derbyshire and Don Burquest for inviting me to give this paper and to interact with numerous SIL grammar specialists.

[2] It would be absurd to suppose that formalism or functionalism are either one devoid of leading ideas on the nature of form and function in language—both are vitally concerned with these two issues, although the jargon used varies from group to group. And both sets of literature contain much of interest and importance to the other. This is common sense. However, this conclusion might elude the linguist who does not do field work and thus risks an overly "sterile" research environment.

[3] The Relational Grammar course at UND-SIL and the TG courses at UTA are exceptions to this, but I sense that, overall, greater "sympathy" is often given to functionalist accounts. I could of course be wrong. But the major point of this paper would be unchanged.

[4] Perhaps the best and most enduring example of science conflicting with naive intuition is Plato's challenge to Western Science to explain why planetary motion is well-behaved in spite of the fact that it seems to the naive observer to violate otherwise general rules of celestial movement; cf. Tarnus (1991). We are probably all aware that linguistics is usually hard for nonspecialists to "swallow" whenever it interposes a bit of grammar between direct sound-meaning correspondences. This supposed direct link between sound and meaning is indeed the most "primordial" conception of language. So, consider Bickerton's (1990) claim that what most distinguishes higher primates from lower primates is grammar, the **representational, computational** system of language. For example, chimps apparently do learn sound-meaning correlations (e.g., proper vs. common nouns) and use these in communication, but they do not ever show evidence of acquiring syntax in any interesting way. One might be excused for speculating, then, that our anti-grammar intuitions come from an earlier evolutionary stage.

5 In a review of Longacre's (1964) *Grammar Discovery Procedures*, Postal remarked that language learning is the most important such procedure.

6 Although, what constitutes a "return" on investment in linguistics within SIL and other organizations is usually not clearly defined. I return to this below.

7 So the fact that the verb agrees with the subject is true regardless of whether that subject is discourse topic or not. The way in which something gets to be the subject is beside the point from the perspective of its form. We want to know how things are treated morphosyntactically once they become subjects. There are a good deal of things we need to know about subjects internal to the clause before we can study their use in discourse.

8 Creationism must necessarily assume an even greater hiatus between form and function, since the Creator could have used just about any form to instantiate the functions S/he wanted us to have. God could have given men nipples, for example, just out of "artistic flair."

9 In a recent book, Merlin Donald (1991) argues that the development of human language involved complex human interactions between communicative and representational functions. Whatever the nature of these interactions, and this will always be to some degree speculative, it is clear that communication and representation are logically independent functions in the evolution of human cognition.

10 There are of course cases in which our ability to understand or parse sentences on-line suffers some breakdown. This is an interesting psychological issue precisely because of its rarity and because its explanation is in terms of nondiscourse-based sentence structure. See Gibson (1991) for one of the better studies of this phenomenon.

11 In Everett (1985) and Everett (1989), I attempted to demonstrate some fairly subtle yet significant interactions between form and function that do need to be accounted for at some point.

12 The distinction between communication as a *form-selecting function* vs. communication as a *form-structuring function* seems quite important to me. I readily accept the notion that discourse function can lead you to select the active or the passive or to favor a pronominal over a proper name in some situations. But communicative function will never make word order free in English nor will it alter the fact that rules of the grammar only affect constituents. That is, function uses (grammatical) form here; it does not determine it. Notice, though, that this is only at the level of the sentence and below. Surely, communicative needs do **structure** the discourse, i.e., at this suprasentential level communication is form-structuring. But, this again just means that the discourse is **not** grammar in the same sense as the sentence is. ■

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LANGUAGE AND COMMUNICATION: A Diary Entry

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My title this time is ambiguous. It evokes the journal *Language and Communication*—I have vol. 11, no. 1/2 in front of me. This volume attempts communication about language, or perhaps rather about linguistics, and that's the second thing my title stands for.

Frederick J. Newmeyer opens the volume with what amounts to a position paper. He wants to relate functional linguistics to innateness, and thus holds out a generative olive branch to unregeneratcly contumacious functionalists. His peace offer is not without conditions: he demands that functionalists accept that "grammar" is "autonomous." Indeed, this point, which functionalists see as demonstrably false, seems nonnegotiable.

Sixteen reactions follow Newmeyer's opening piece, and most of them train heavy guns on it. They make fascinating reading. Newmeyer's concluding response plaintively opens with "It takes two to tango, so why am I dancing alone?" He then concludes that in most of the responses there is an unfortunate taboo against evolutionary theories (on which Newmeyer relies for his peace mission. arguing that the results of functionalism may be considered as "adaptive" and thus not necessarily at odds with innateness), and decides, "undaunted as always," to refocus the discussion. Undaunted? Or unapproachable?

* * *

Newmeyer's problem is that he engages in philosophy rather than in linguistics. He wants to begin "with what linguists of all persuasions agree is the task of linguistic theory, namely to relate sounds to meanings."

Well, count me out—and I may be in large company. First, it would have to be clear what "theory" is. Second, we don't have to relate sounds to meanings, for those **are already** related.

As for the first: if "theory" is the name of what we discover and phrase economically, in the matter of language universals (empirically, on the basis of a sufficiently comprehensive data base), then we do not "need" a "theory" to begin with, but get one to end with—all the time looking for counterevidence in order to perfect the theory, and keep the whole endeavor empirical.

My second comment may sound like a quibble: perhaps (it could be said) Newmeyer means only to **discover** how sounds and meanings are (already) related. But this won't do, for Newmeyer says that "meanings, whatever their ultimate nature, are first and foremost **mental** realities"; and sounds are "physical realities **par excellence**" (emphasis original). So there we are: meanings are mental, sounds are physical; so we have mind and matter, and how do you marry them? What does this have to do with linguistics? What, indeed, with philosophy, except to perpetuate a nonproblem more than two millennia old?

* * *

What does it mean, actually, to say that grammar is "autonomous"? "Autonomous" is a relative notion: "autonomous" in regard to what? I suspect generativists would say "autonomous" in regard to whatever functionalists would think determines much of grammar; for example, requirements of discourse, of participant-tracking in discourse, of communicative and cognitive needs, of deictic organization. But then Newmeyer allows functionalists' findings and wants to call those processes of "natural selection"—and so "mentalism" would be safeguarded because it would allow for an evolutionary explanation.

Functionalists have always recognized a relative degree of autonomy of grammar (as indeed Newmeyer allows), and indeed of another subsystem such as that of phonology. What functionalists do not want to do is make their work dependent on some dubious philosophical system. They also want to work with a great variety of languages, and not just with West-European ones, give or take a sprinkling of other languages (such as Japanese—which shares with West-European languages the property of being dependent-marking).

* * *

In the *Language and Communication* volume, the two paradigms of generativism and functionalism do not meet for the simple reason that they can't. Generativism relies on what is in effect a cultural particular of the West: the distinction between mind and matter, with pronounced ascendancy of the "mind." Functionalists by and large do not want any such metaphysical system to supervise the search for what characterizes human languages. The generativist metaphysical frame of reference sees language as representative of what goes on in that "mind," not of things we talk about (or so Chomsky says), and not of what happens in actual speech (as "performance"), nor of intersubjective processes such as those of communication in actual speech.

In other words, generativists are "structuralists" in the traditional Saussurean sense of relational "values" as (only) "differences," but not in the more evolved sense of "structuralism" whereby we are interested in how speakers and hearers differentiate themselves from one another in transmitting information. Functionalists approach human languages with an almost extremely comprehensive data base cross-linguistically, and as "communication" between those who linguistically never stand "alone," as individuals—whether or not as "mental" beings. Hence functionalists cannot work with a sample of languages too small for significant generalizations, or with sentences detached from their discourse context, or with a "competence" disengaged from actual "performance," or with a "Universal Grammar" working with what in fact boils down to typological particulars (such as "government" or "inflection" or some a priori small list of word classes).

* * *

The two paradigms cannot meet because of different mind sets, on either side of a fence never erected by the functionalists. Generativists cannot see why anyone not of their persuasion could fail to see what is so clear to them—such as the "autonomy" of grammar, or the essentially "mental" nature of the linguist's business. Newmeyer's approach is fairly exceptional among generativists in that he clearly knows something (even though as clearly not enough) of what functionalists are doing, and of course in that he seems interested in some kind of rapprochement. I do not know of any

functionalist who is notably interested in making converts, and unfortunately tensions with generativists are generated in the quite undogmatic arena of academic politics.

I believe that a meeting of minds is possible, at least after some time, provided there's no political provocation (as there isn't, so far as I can see, in Newmeyer's case); provided also there comes an end to that need to proselytize; and provided linguists are seen as free to be interested in human beings and their languages from more approaches than only that of "mentalism." It does take two to tango, and I am not sure Newmeyer isn't still caught up in solo gyrations.

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Focus Shift Problem

Howard W. Law, Ph.D.

DEFINITION: Focus shift is a figure of speech that changes the subject or object pronominal (free or affixed) or possessive pronoun in a sentence, clause, or phrase from one grammatical person to another either without changing the referent (from second person to third person, or vice versa) or by changing the referent (from first person to another person, and vice versa). It might also be called a stylistic device.

As one of many figures of speech or tropes found in Hebraic literature such as the Bible (see examples below), the use, form, and function of focus shift, therefore, could be of interest to Bible translators, especially those working on the Old Testament. Not only are some tropes found in several languages (English and Hebrew simile, metaphor, parable, etc.), but some may occur uniquely in a single language or a few languages other than Indo-European ones. The translator may be tempted to "straighten out" the grammar or syntax of the receptor language. Instead, if he understands that the "strange" pattern is the language's unique or special stylistic device, he will incorporate it into his translation with significant gain in understanding and appreciation by the speakers.

In the standard or typical English composition class, students are told not to shift the focus of a subject in their writing, for example, from third person to second person, although this happens a lot in the English Old Testament. Numerous instances of this occur in the Psalms, Isaiah, and Zechariah (and perhaps in other places), for example, Psalm 116. [All quotations are from the NASV; punctuation and capitalization are sometimes altered.]

> 116:1–15 I love **the Lord**, because **He** hears my voice and my supplications. Because **He** has inclined **His** ear to me, therefore I shall call upon **Him** as long as I live. (And so on through vs. 15) (All third person)
>
> 116:16–17a **O Lord**, surely I am **Thy** servant, I am **Thy** servant, the son of **Thy** handmaid, **Thou** hast loosed my bonds. To **Thee** I shall offer a sacrifice of thanksgiving, (All second person)

116:17b–19 and call upon the name of **the Lord**. I shall pay my vows to **the Lord**, Oh may it be in the presence of all **His** people, in the courts of **the Lord's** house, in the midst of you, O Jerusalem. Praise **the Lord**. (All third person)

In this example, the first person subject is always David; the third person subject and object are often God; the second person possessive pronoun refers to God. The problem here is that there is a shift for the same referent (God) from (a) third person (talking about God), verses 1–15, to (b) second person (addressing God), verses 16–17a, to (c) third person (talking about God), verses 17b–19.

Another example is found in Psalm 18.

18:1 I love **Thee, O Lord**, my strength.

This verse is represented in the NASV as something David said to the Lord (second person), followed by statements about the Lord (third person) through verse 15a. Verse 15b starts a series of rotations between second and third person that continues through the rest of the psalm.

18:15b At **Thy** rebuke, **O Lord**, at the blast of the breath of **Thy** nostrils. (All second person)

18:16–34 **He** sent from on high, **He** took me; **He** drew me out of many waters. (And so on through vs. 34) (All third person)

18:35–40 **Thou** hast also given me the shield of **Thy** salvation, And **Thy** right hand upholds me; And **Thy** gentleness makes me great. (And so on through vs. 40) (All second person)

18:41–42 **They** cried for help, but there was none to save, Even to **the Lord**, but **He** did not answer them. (Also vs. 42) (All third person)

18:43a–45 **Thou** hast delivered me from the contentions of the people; **Thou** hast placed me as head of the nations. (And so on through vs. 45) (All second person)

18:46–47 **The Lord** lives, and blessed be my rock; And exalted be **the God** of my salvation. (Also vs. 47) (All third person)

18:48a **He** delivers me from my enemies; (Third person)

18:48b–49 Surely **Thou** dost lift me above those who rise up against me; **Thou** dost rescue me from the violent man. Therefore I will give thanks to **Thee** among the nations, **O Lord**, And I will sing praises to **Thy** name. (All second person)

18:50 **He** gives great deliverance to **His** king, And shows lovingkindness to **His** anointed, To David and his descendants forever. (All third person)

Other instances in the Psalms are even more complex.

Some other Hebrew writers often shifted the focus of the referent apparently to suit their particular purpose at the time, for example, Zechariah in:

1:17 Again, proclaim, saying, "Thus says [I] **the Lord of hosts, 'My** cities will again overflow with prosperity, and [I] **the Lord** will again comfort Zion and again choose Jerusalem.'"

By inserting an interpretive "I" (first person), the last third person set becomes a first person set, and there is no shift of focus. Can a similar interpretive insertion clear up other supposed shifts of focus (third person to first person to third person, same referent becomes third person to first person)?

9:4 Behold, **the Lord** will dispossess her and cast her wealth into the sea; and she will be consumed with fire.
9:7b Then they also will be a remnant for **our God**.... (Third person)

9:8 But **I** will camp around **My** house because of an army,...For now **I** have seen with **My** eyes.
9:10a And **I** will cut off the chariot from Ephraim,... (First person)

9:10b And **He** will speak peace to the nations; and **His** dominion will be from sea to sea.... (Third person)

9:11–13 As for you also, because of the blood of **My*** covenant with you, **I** have set your prisoners free from the waterless pit....This very day **I** am declaring that **I** will restore double to you. (And so on through vs. 13) (First person) [*"My" is not in the Hebrew text.]

9:14–17 Then **the Lord** will appear over them, and **His** arrow will go forth like lightening; and **the Lord God** will blow the trumpet, and will march in the storm winds of the south. **The Lord of hosts** will defend them....And **the Lord their God** will save them in that day as the flock of **His** people; for they are as the stones of a crown, sparkling in **His** land. (And so on through vs. 17) (Third person)

No explanation, however, seems available in these other instances (as is proposed for the Psalms). These writers may not have been concerned with a shift of focus as we have been in English composition classes. Or, they may have purposefully selected

alternating focuses as a stylistic device—and that may not be satisfying to the analytic modern reader (or critic!)!

If the above rationale (interpreting focus shift as a stylistic device) is acceptable, then in some cases, perhaps in longer passages, the style may be "antiphonal": two or more groups of speakers (a) assuming different parts and (b) alternating or rotating their parts, for example, the speaker (first person), the spoken to (second person), and the spoken about (the third person).

In other cases, usually shorter, the shift may be associated with or mark a sudden emotional, devotional, or parenthetical outburst* by the writer. (See line three below.) Example (a current chorus):

I love you, Lord, and I lift my voice
To worship You.
*O my soul, rejoice!
Take joy my King....

The shift may also occur as an aside or casual comment, or as a moralizing insertion in an otherwise objective text.

Two instances illustrating these uses seem to occur in Shakespeare. At line 141 in the poem *The Rape of Lucrece*, the following lines occur with a shift from third person to first person. (Bold type is this author's editorial change for identification of the shift.)

The aim of all is but to nurse the life
with honour, wealth, and ease in waning age;
And in this aim there is such thwarting strife
That one for all, or all for one **we** gage:
As life for honour in fell battle's rage;
    Honour for wealth; and oft that wealth doth cost
    The death of all, and all together lost;

So that in the vent'ring ill **we** leave to be
The things **we** are for that which **we** expect;
And this ambitious foul infirmity,
In having much, torments **us** with defect
Of that **we** have: so then **we** do neglect
    The thing **we** have; and, all for want of wit,
    Make something nothing by augmenting it.

Shakespeare then returns to third person grammar in the subsequent lines, e.g., "they," etc., and proper names.

A second instance occurs at line 17 in the poem *The Phoenix and the Turtle*, in which the following lines occur with a shift from third person to second person. (Bold type is this author's editorial change for identification of the shift.)

> And **thou** double-dated crow,
> That **thy** sable gender mak'st
> With the breath **thou** giv'st and tak'st,
> 'Mongst **our** mourners shalt **thou** go.

This instance may be treated as imbedded in the figure of speech known as, or similar to, antimetathesis, in which the reader is absent but addressed as though he or she were present. (See also the example in I Corinthians 7:16.) Shakespeare again returns to third person grammar at line 21 of the poem.

Shift of focus seems to occur also in other poems by Shakespeare such as *The Passionate Pilgrim*.

One conclusion surfaces from these studies of the Biblical and Shakespearian material, viz., shift of focus occurs only in poetical works and passages.

Does this shift of focus feature occur in the New Testament? Or in Classical Greek? Or in other languages? It does not seem to be a special style limited to Hebrew devotional writing.

If this focus shift is a recognized figure of speech in the Old Testament and other places, what is it called other than shift of focus? And, if so, what sources could be consulted to learn how it has been treated? How should it be treated in new translations of the Old Testament? Are there some other uniquely Hebraic or Semitic figures of speech/stylistic devices occurring in the Bible?

Other figures of speech (tropes) in Hebrew literature include allegory, metaphor, simile, metonymy, synechdoche, hyperbole, personification, and apostrophe. Other stylistic devices found in Hebrew literature would include as many as eight types of parallelisms (including chiasms), acrostics, and possibly antiphonals. But none of these seem to offer an explanation of the phenomenon of shift of focus.

The editor and this writer would welcome your response and suggested solutions to this problem.

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The 24th International Conference on Sino-Tibetan Languages and Linguistics: Mon-Khmer Section

Ramkhamhaeng University and Chiangmai University, Thailand
October 7–11, 1991

Brian Migliazza

This conference was held for 3 days in Bangkok at Ramkhamhaeng University, and for another 2 days in Chiangmai at Chiangmai University. Although the conference was titled "Sino-Tibetan," it seemed that almost one-fourth of the 75 papers presented were on Mon-Khmer languages. Most of the papers were well-presented and informative. It was definitely worth one's time to attend, not only for the content, but also, as William Gedney remarked, to talk with the academic luminaries in person and to sometimes see that they too have "feet of clay."

Several Thailand Group SIL members gave papers, which are listed below, on a Mon-Khmer topic: Nancy Bishop, Brian Migliazza, Debbie Paulsen, and David Thomas.

The opening plenary session on Mon-Khmer linguistics included:

Christian BAUER (Mahidol University, Thailand)—*Early Thai-Mon contacts: Their demographic, geographic, and linguistic implications.* New evidence suggests the presence of Mon speakers in the Chao Phraya Basin by the 14th century, with Thai being used as a second language by Mon speakers at Sukhothai/Sri Sachanalai, and the diffusion of Mon grammatical features into Thai. This would also account for the grammatical changes taking place in Khmer by the 8th and 9th centuries, Mon being the source of diffusion.

Suriya RATTANAKUL, Sophana SRICHAMPA, and David THOMAS (Mahidol University, Thailand)—*Some century-old West Bahnaric data.* A recently discovered manuscript, written in the Thai script by a Thai official in the late 19th century about the peoples and languages of southern Laos, including word lists and cultural

comments, allows for the comparison of his languages with their modern descendants—which indicate very little basic difference.

Gerard DIFFLOTH (Cornell University)—*Tarieng-Alak: A new branch of Bahnaric*. Tarieng (different from Tareng, but closely related to Alak) data indicates that registers found in North Bahnaric languages are not due to devoicing of initials but to evolutions in the Bahnaric vowel system, which leads to some new alignments of the Bahnaric languages.

The closing plenary session on Mon-Khmer linguistics included:

Debbie PAULSEN (Payap University and SIL, Thailand)—*Tone and intonation in Plang*. The CECIL package is used to analyze the characteristics of Plang (Palaungic branch, Waic sub-branch) tone and intonation, and the interplay between these two.

Herman JANZEN (Payap University, Thailand)—*Form and function of topicalization in Southern Taang discourse*. Topicalized elements in Southern Taang (Palaungic branch) discourse are basically nominals which mark critical positions in the overall narrative discourse structure.

Somsonge BURUSPHAT (Mahidol University, Thailand)—*Kui narrative repetition*. Large-scale repetition in Kui (Katuic branch) narrative discourse is used to spread out the information load so that a theme line is highlighted.

Some of the other Mon-Khmer papers presented included:

Nancy BISHOP (Thammasat University and SIL, Thailand)—*A preliminary analysis of Maniq oral monophthongs*. Previous studies in this language posited a set of nine simple oral vowel phonemes, while Bishop's recent research indicates that there is an additional contrast between tense and lax oral monophthongs, which adds three vowels to the set.

Michel FERLUS (CNRS, France)—*Nasal and liquid augments in Mon-Khmer nominal infixes: The Vietnamese dialect of Vinh*. Central Vietnamese is the result of a progressive superimposing of Proto-Vietnamese on a local Pong-Chut type of language.

William GAGE (Rochester, NY)—*Putting Vietnamese in its Mon-Khmer place*. Considers various phonetic developments that seem to lead towards Robert Headley's 1976 proposal of an Eastern +Mon subfamily which includes Vietnamese.

Brian MIGLIAZZA (Thammasat University and SIL, Thailand)—*So and Bru in Northeast Thailand*. Many peoples in Northeast Thailand are variously labelled "So" and "Bru" (Katuic branch), which a linguistic analysis helps to differentiate into two main groups.

Suwilai PREMSRIRAT (Mahidol University, Thailand)—*Khmu color systems and their elaborations*. The Khmu color system (five primary and six secondary colors) shows universality (the semantic universality of human experience) as well as uniqueness in their perception of the world.

Another interesting session was a two-hour panel discussion on *Linguistic Evidence in King Ramkhamhaeng Inscription*. The King Ramkhamhaeng Inscription is a stone, generally called the Sukhothai Inscription Stone No. 1, that was discovered in 1833 by King Mongkut (Rama IV) of Thailand, when he was still a monk prior to his ascending the throne. It had always been regarded as being written by King Ramkhamhaeng and dated around 1283. The dating of the stone is crucial since its inscription was the basis for long-held fundamental ideas concerning the early history of the Thai nation's birth. The writings in the inscription credit Ramkhamhaeng with creating the modern Thai alphabet and portray life in the Sukhothai kingdom as prosperous, having free trade, and ruled by a paternalistic monarch. Many other notions about Thai history are dependent on the dating of the inscription. Thus, if it is false, and is actually a product of King Mongkut, then a whole re-examination of other assumptions of Thai history is needed.

PIRIYA Krairiksh (Thammasat University, Bangkok) and Michael VICKERY (University Sains, Penang, Malaysia) led those who claim the stone is a fake. Vickery's arguments were based on linguistic evidence, e.g., history and type of script used and loan words, and Piriya's arguments were based on textual analysis:

1. Some of the words used have different meanings from those in other Sukhothai inscriptions,
2. the art and architecture mentioned are not supported by archeological and historical evidence,
3. some of the phrases and sentences seem to have been lifted verbatim from the writings of a later Sukhothai king, and
4. many of the words and some of the contents of this inscription are found in late 18th and 19th century literature).

Anthony DILLER (Australian National University, Canberra, Australia) and DHAWAJ Poonotoke (Ramkhamhaeng University, Bangkok) led the many loyal defenders of the authenticity of the inscription. Dhawaj considers the historical records in the texts themselves, some morphological aspects, and the orthography used in the inscription. Diller confronts the question of why the Ramkhamhaeng inscription shows a fully-developed phonemic tone-marking system, while in other surviving Sukhothai and Ayudhya texts, tones were only marked sporadically, if at all. (Regular tone marking only appears at a much later date.) Diller counters that there were two different writing systems accepted throughout the Sukhothai and Ayudhya periods, even on up to the reign of King Rama III (1824–1851), which accounts for the orthographic variations of several types and amounts of tone marking.

The discussion was stimulating and sometimes rather vigorous! The following anthology containing the various positions in this debate, both pro and con, has recently been published:

Chamberlain, James, ed. 1991. The Ram Khamhaeng controversy: Collected papers. Bangkok: The Siam Society. 565 pages. ■

Third International Symposium on Language and Linguistics: Pan-Asiatic Linguistics

Chulalongkorn University, Bangkok, Thailand
January 8–10, 1992

Tom Tehan and Brian Migliazza

The Third International Symposium on Language and Linguistics was held at Chulalongkorn University in Bangkok, Thailand on January 8–10, 1992. The theme of "Pan-Asiatic Linguistics" was represented in papers covering languages from South, Central, East, and Southeast Asia, including the Philippines and Indonesia. Scholars from Europe, North America, Australia, Oceania, and Asia presented papers.

This conference was one of the best that we've attended. The Linguistic Department at Chulalongkorn University did an excellent job of organizing and running the conference. You get what you pay for, and at this conference we got a lot for the price. Included in the price were two bound volumes of the proceedings, which will be published sometime in February 1992 (see Reference). The proceedings were varied and professional; they should be worth any linguist's time to peruse.

The conference had the privilege of being officially opened by Her Royal Highness Princess Maha Chakri Sirindhorn. Then there was an opening plenary session, ten parallel sessions, and a closing plenary session. All speakers were allotted 30 minutes.

The opening plenary speakers were:

Arthur ABRAMSON and Donna ERICKSON—*Tone Splits and Voicing Shifts in Thai: Phonetic Plausibility*
Kenneth PIKE—*Matrix Formatives in N-Dimensional Linguistics*

The closing plenary speakers were:

Stanley STAROSTA—*The Case-Marking System of Proto-Formosan*
John and Manjari OHALA—*Nasals and Nasalization in Hindi*

In his keynote paper, Kenneth Pike proposed a new matrix-oriented (field) approach to linguistics for Asian scholars to consider as a complement to particle- and wave-oriented Western linguistics. The remaining papers were divided into eight categories: (1) Phonetics and Phonology, (2) Morphology and Syntax, (3) Natural Language Processing, (4) Psycholinguistics and Neurolinguistics, (5) Diachronic Studies, (6) Typology, (7) Language Contact, and (8) Sociolinguistics. SILers giving papers were: Kenneth Pike, Rodolfo Barlaan, Stephen Beale, Robert Busenitz, and Brian Migliazza.

In a conference of this size, it is hard to pick out overall themes. National languages, especially Thai, were well analyzed and discussed. However, minority languages from Thailand, China, India, Indonesia, Russia, Vietnam, and others were discussed too. We attended several papers on natural language processing with computers. Dependency grammar seems to be of much interest here, as well as organizing dictionaries and parsers to handle the many sources of ambiguity in natural language. However, only a few small texts are able to be processed. One person asked how the Japanese Interlingua programs handle word breaks in Thai (there are none in Thai orthography). The answer: "Humans do it"!

Those interested in Southeast Asian languages will want to read the papers that discuss the following LANGUAGE FAMILIES and languages: KADAI, KAMMUIC, MIAO-YAO, KATUIC, TAI, TIBETO-BURMAN, VIET-MUONG, Black Tai, Burmese, Hmong Njua, Isaan, Khmer, Malay, Pali, Sanskrit, Tai Nuea, Thai (several dialects in several countries), T'in, Vietnamese Thai, White Hmong, White Thai, and probably a few others that I have missed. There were also papers dealing with tones, phonology, syntax, discourse, and national policies for minorities.

The following is a small sample of the 115-plus papers presented during the ten parallel sessions.

Rodolfo BARLAAN—*An Autosegmental Analysis of Reduplication in Isnag*. An autosegmental approach for analyzing reduplication (which is seen as simply an affixation process) in Isnag (an Austronesian language of the northern Philippines) has three significant advantages over traditional analyses: (1) it avoids intermixing of levels, (2) it is more economical, in that it uses only

one template, and (3) it provides a viable mechanism for handling the apparent overapplication of some phonological rules.

Robert and Marilyn BUSENITZ—*Spatial Deixis in Balantak*. The demonstrative system of spatial deictics of Balantak (an Austronesian language of Central Sulawesi, Indonesia) shows a seven-way distinction between three dimensions: distance to the speaker and addressee (with 3 subcategories), a horizontal axis (with 2 subcategories), and a vertical axis (with 2 subcategories).

David BRADLEY—*Language Policy for Minority Languages in Thailand and China*. David was not able to be at the conference, so his paper was read by Peter Paul of Monash University. He discussed the orthography situation among five minority groups (Lahu, Akha, Lisu, Mong, and Mien) which are found both in Thailand and China.

Gerard DIFFLOTH—*Proto-Katuic Phonology*. Using the diagnostic of uniquely shared innovations, glottalization is reconstructed at the Proto-Katuic level, and a distinct sub-branch of Katuic is set up where this phenomenon has been preserved.

Anthony DILLER—*On the History of Tone-Marking in Asian Languages*. An historical overview of Greek, Sanskrit, Chinese, Vietnamese, Mon, and Burmese marking of tonal (or tone-like) phenomena, in order to develop a strong case that the "Lai-su' Thai" system of Sukhothai was the world's first comprehensive phonemic tone-marking orthography.

Brian MIGLIAZZA—*Lexicostatistic Analysis of Some Katuic Languages*. A lexicostatistic comparison of nine So and Bru dialects in three northeastern provinces of Thailand with nine other Katuic languages in Thailand, Laos, and Vietnam (from each of the five subgroups of Katuic) shows that the So and Bru dialects are definitely in the North Katuic subgroup.

Chhany SAK-HUMPHRY—*The Status of MAN and TEL in Pre-Angkorian Khmer*. The words *man* and *tel*, from pre-Angkorian inscriptions of 600 to 800 A.D., (found between the head noun of an NP and a following verbal relative clause) are analyzed as nouns which are external to the verbal relative clause, and thus are heads of equational attributes of the nouns modified by the relative clauses.

Eric SCHILLER—*Parts of Speech in Southeast Asian Languages: An Autolexical View.* An examination of the nature of word classes (prepositions, classifiers, coverbs, and expressives) in Khmer and some other Southeast Asian languages, with particular reference to autolexical theory.

Long SEAM—*Khmer Toponyms of Sanscrit Origin (in Inscriptions of Cambodia VI–XIV).* One third (about 400) of the whole amount of toponyms attested in the inscriptions of VI–XIV are denoted as Sanskrit loans. An examination of these provides a better understanding of the ancient linguistic loans and the influence of the religious superstructure in the process of the formation of toponyms in Cambodia.

Martha RATLIFF—*Grammar and Tone in Asian Languages: A Typological Study with Diachronic Implications.* A discussion of the various grammatical uses of tone, citing evidence from Biao Min, Putian-Northern Min, Hakka, Shimen Hmong, White Hmong, Burmese, Cantonese, Vietnamese, Kaili Hmu-Hmongic, Shanghai Northern Wu, Tangsic-Northern Wu, and Suzhou-Northern Wu.

Nantana RONNAKIAT—*Evidence of the Thai Noi Alphabet Found in Inscriptions.* Examines the origins of the writing system of the Northeastern Thai dialect based on an analysis of twenty-two rubbings of inscriptions.

Theraphan THONGKUM—*The Raising and Lowering of Pitch Caused by a Voicing Distinction in Sonorants (Nasals and Approximants): An Epidemic Disease in Southeast Asian Languages.* Evidence from the Mon-Khmer, Tibeto-Burman, Tai-Kadai, and Hmong-Mien languages shows that the "birth" and "death" of voiceless sonorants have the effect of raising the pitch of the following vowel.

Kalaya TINGSABADH and Daranee KRISNAPAN—*Tonal Overlapping: An Instrumental Study of Suphanburi Thai.* The sub-dialects of Central Thai are almost entirely differentiated by tones. The overlapping tonal realizations in the central Thai of Suphanburi Province depend mainly on degrees of stress, so that tone 1 and tone 4 are neutralized in both the unstressed syllable and the prominent-stressed syllable of disyllabic words.

Peansiri VONGVIPANOND—*Model of a Discourse Grammar for the Analysis of Thai*. A study of Thai discourse data reveals that at least four systems are needed in the grammar to account for linguistic phenomena at the discourse level: (1) an information structuring system, (2) a coherence system, (3) an attitudinal system, and (4) an illocutionary system.

Another interesting development was a meeting held on the evening of the second day to discuss the possibility of forming a Pan-Asiatic Linguistic Council (PALC). This council would have regular conferences and possibly a journal. It was felt there might be the need for more regular conferences which would be open to papers on all types of Asian Languages. This might possibly be able to dovetail with the SEALS (Southeast Asian Linguistics Society) meetings which are currently being held annually in the U.S.

At the close of the conference, Dr. Suwilai accepted, on behalf of Mahidol University at Salaya, the job of hosting the next International Symposium on Languages and Linguistics in 1996.

Reference

Pan-Asiatic Linguistics: Proceedings of the Third International Symposium on Language and Linguistics. Volumes I and II. 1992. Bangkok: Chulalongkorn University Printing House. ■

New Publication

UCLA Occasional Papers in Linguistics
Volume #9

Ay Baati Wolof—A Wolof Dictionary $9.00
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Wolof is the major language of Senegal and Gambia. Ay Baati Wolof is the first Wolof-English dictionary and one of only two modern full-scale dictionaries of Wolof. It has a 154 page Wolof-English dictionary and a 94 page English-Wolof index. The Wolof-English entries contain grammatical information, examples, and frequent cross-referencing to related entries. It is based on the Dakar dialect and includes many recent innovations in Wolof as used in Dakar.

The following Occasional Papers are also available:

#3	*Logical Types for Natural Language* by Edward L. Keenan and Leonard M. Faltz (1979)	$7.00
#5	*Studies in the Structure of Toba Batak* edited by Paul Schachter (1984)	$7.00
#6	*Muskogean Linguistics* edited by Pamela Munro (1987)	$7.00
#7	*Morphology as a Computational Problem* edited by Karen Wallace (1988)	$8.00

For more information, write:

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REVIEWS OF BOOKS

A natural history of negation. By Laurence R. Horn. Chicago: University of Chicago Press, 1989. Paperback $34.95.

Reviewed by James K. Watters

In various indigenous languages of the Americas (as well as of Africa and Oceania), the standard translation of *bad* is, literally, *not good*. Some of us from other language communites, who have grown used to being able to proclaim something as *not good, but not bad either*, have found ourselves in a bit of difficulty at this point from time to time. (I've tried *it's not good but not not good either*, but it just doesn't work.) How can we give a coherent account of how English *not good* differs from *not good* in these other languages?

Most of us have learned somewhere in the past that Ancient Greek has two negatives, *ou* and *mee*. It's not uncommon in field work to find yourself dealing with a language with two or three kinds of negation. Do their different functions follow any regular patterns cross-linguistically? And if so, what general theory might account for the different sorts of negatives found across languages?

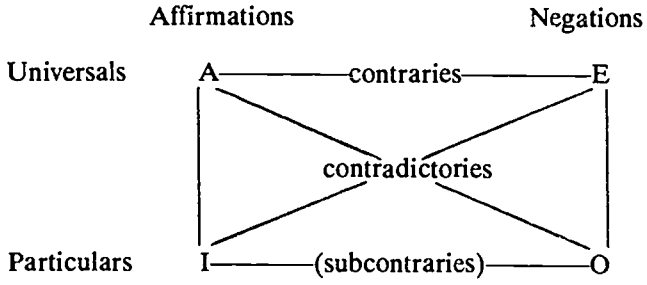
Why is it that in French, Tepehua (Totonacan, Mexico), and various other languages, the expression that looks like it should mean *It's not necessary that you do it*, actually means *It's necessary that you not do it*?

When we hear a comment such as *John didn't eat his hot dog, he devoured it*, we know that the speaker doesn't really mean to deny that John ate the hot dog; but how does such a use of negation fit into a theory of meaning?

These and many other issues are addressed in this remarkable book. Rarely does one come across a book that deals with a particular class of linguistic phenomena in such historical depth and analytic detail. Horn takes the reader from the analyses of the Greek philosophers through medieval scholastics to present-day disagreements among linguists regarding the nature of negation in natural language. Along the way, he travels such formidable terrain as semantic and pragmatic presupposition, scalar semantics, markedness, scope and quantifiers, conjunctions, pragmatic ambiguity, and propositional logic vs.

(extended) term logic. He discusses negative constructions in a variety of languages, and while by far the majority of his examples are from English, they illustrate principles that apply cross-linguistically.

He begins with Aristotle. This is not merely out of a sense of obligation to note historical precedence (though Horn sets a new standard when it comes to acknowledging those who have said it before); Horn's arguments and conclusions are in many places prefigured by Aristotle. Furthermore, one of the central explanatory tools Horn uses throughout the book is the square of opposition, derived from Aristotle's work:



The four corners (labeled with the first vowels of Latin *Affirmo* and *nEgO*) correspond (following Aristotle) not only to quantifiers:

A—ALL
I—SOME
E—NONE
O—NOT ALL

but also to both deontic and epistemic modals:

A—be obligatory, be certain
I—allow, be possible
E—not allow, be impossible
O—not be obligatory, not be certain

These and other concepts introduced in chapter 1 come up throughout the book.

Here Horn presents Aristotle's distinction between contraries and contradictories (and between correlation and privation, though these latter two aren't so central to our concerns). Contraries (either terms or propositions) such as *good* vs. *bad* can never both be true when predicated of a subject, but they may both be false. Contradictions

(always propositions) such as *Socrates is ill* vs. *Socrates is not ill*, are either true or false. To further distinguish these kinds of oppositions, Horn reviews literature regarding the Aristotelian Law of Contradiction (which applies to both contradictories and contraries) and the Law of the Excluded Middle (which only applies to contradictories).

Horn goes on in chapter 1 to contrast the predicate denial and predicate term negation of Aristotle with the (external) propositional negation of the Stoics, a contrast that also continues throughout the book. The Aristotelian version basically distinguishes (a) a negation that denies the application of a predicate to the subject and (b) a negation that forms a negative predicate which is applied to the subject. The Stoic version, adopted by Frege and most modern philosophers of language and taught in most textbooks on logic, does away with the subject-predicate distinction and presents negation simply as an external operator. This leads one to the conclusion that the following sentences are true in all the same instances (i.e., synonomous), simply asserting the falsity (or denying the truth) of John's happiness:

(1) John is not happy.
(2) John is unhappy.

Horn, drawing on work in philosophy of language and modern linguistics (including numerous references to Zimmer 1964), argues that this is misguided. While (1) has a (possible) reading as a contradictory of *John is happy*, (2) invariably has a contrary reading (closer to *John is sad*.)

Another theme introduced in chapter 1 and developed throughout the book regards the question of whether the relation between positive and negative statements is one of symmetry or asymmetry. There is a long tradition that says there is something more basic about positive statements, that negative statements are somehow parasitic. This notion (which seems to match our natural intuition) comes out in work by modern philosophers (e.g., Searle who treats negation as a type of illocutionary force) and linguists (e.g., Givón who treats negation as semantically marked). However, Horn points out that Frege (among others) has argued that there is a problem here: while we may agree that the positive-negative relation is functionally asymmetric, it can be shown to be logically symmetric.

Having laid the groundwork in chapter 1 (with a ten-page expedition into non-Western territory, especially that of India), Horn moves on in chapter 2 to discuss in more detail that class of propositions which seem to be neither true nor false. This includes propositions with vacuous subjects; the *The king of France is bald* example comes in for re-evaluation. Note that this famous sentence seems to presuppose the existence of a king of France. So here, the discussion leads into a review of some of the literature on the existence (or not) of semantic presuppositions. Another class of odd propositions familiar to linguists is then discussed by Horn: category mistakes, more commonly known as violations of selectional restrictions, or collocational clashes, e.g., *2 is red*.

Now, consider the negative of the last sentence, *2 is not red*, or the negative of the previous sentence, *The king of France is not bald*. On one reading, these statements are true, and on another reading, they are nonsense. Such observations have led to a long list of writers who have posited two kinds of negation, presented by Horn in a helpful summary table (p. 140–41). (This apparent ambiguity of negation comes up again as a major topic in chapters 6 and 7.) With these observations, Horn concludes the chapter with a summary of the two kinds of negation proposed by Karttunen and Peters to account for the uses of negation in the examples below:

(3) John didn't manage to solve the problem. (ordinary negation)
(4) John didn't mánage to solve the problem, it was quite easy for him. (contradiction negation)

The use of negation in (4) is discussed further in chapter 6.

Chapter 3, "Markedness and the psychology of negation," explores the "marked" or special nature of negative statements vis-à-vis their corresponding positive ones. Horn reviews the psycholinguistic literature, dealing with studies of both the acquisition and processing of negative constructions. Horn concludes with a pragmatic account of this markedness, i.e., he shows that the marked nature of negation is not to be found in the meaning of negation, but in its use. In this context, he introduces Grice's famous maxims of conversation (Quality, Quantity, Relation, and Manner) with their nine subprinciples. As he points out, these have been reformulated and reduced in various fashions since Grice's initial presentation. The version most familiar to readers of *Notes on Linguistics* is, no doubt, that of Relevance Theory, which claims to have successfully reduced

them to one overarching principle: Relevance (see Blass 1986, 1990; see Sperber and Wilson 1987 for summary; compare Levinson 1989 for a relevant critique). Horn offers his own reduction (of all but Quality), not to one principle, but to two: **Q** (mnemonic for Quantity: "say as much as you can") and **R** (for Relevance: "say no more than you must"). **Q** proceeds "from a speaker's nonuse of a stronger or more informative form to the inference that the speaker was not in an epistemic position to have employed the stronger form" (p. 195). The standard examples are scalar implicatures. The statement *I believe he's coming* implicates that I don't *know* (or I would have said *I know*). **R**, on the other hand, is employed when the use of a weak form implicates a stronger reading. Here the standard examples are euphemisms, e.g., *I'm going to the bathroom*, implicating more than a trip to another chamber. The use of **Q** and **R** comes up again crucially in chapter 5.

In chapter 4, "Negation and quantity," Horn explores territory that should interest any field linguist: scalar terms. He takes as his base Jespersen's observation that negations of scalar terms generally have the reading *less than* (e.g., *Tom doesn't have four children* → Tom has less than four). The discussion includes the quantifiers associated with the square of opposition above (ALL, SOME), and all other sets of terms that fall along a semantic scale, such as the scale *boiling, hot, warm, lukewarm, cool, cold, freezing*, or the infinite series *1,2,3,4...* Consider the following sentences:

(5) a. The water is warm, in fact it's hot.
    b. The water isn't warm, it's hot.
(6) a. Tom has three children, in fact he has four.
    b. Tom doesn't have three children, he has four.

In the (a) examples, the reading of the first clause is compatible with the reading of the second clause; in the (b) examples, the negation of the clause is compatible with the same following clause. Here we might argue that the initial clause of each sentence is ambiguous, i.e., has two possible semantic representations. Horn, however, argues that there is not a semantic ambiguity here but rather a pragmatic ambiguity: such expressions "are unambiguous in semantic representation or logical form but open to two different uses..." (p. 250), a point he returns to in chapter 6.

In the final section of chapter 4, Horn gives evidence for an apparent linguistic universal: while there are numerous languages that

lexicalize the negative "northeast" corner of the square of opposition (English, NOT SOME → NONE; "Malagasy tsy 'not' and misy [miš] combine to form [tsiš] 'no', literally 'not some'." (p. 254)), no languages lexicalize the negative "southeast" corner of the square (**nall*, **neverybody*). This observation applies not only to quantifiers, but also to binary connectives (cf. *neither...nor* vs. **nand*) and to modals. Horn doesn't stop at noting the apparent universal, however; he goes on to account for it. The two lower corners of the square (SOME vs. NOT ALL) are compatible, and clearly the use of one term implicates the other. (If I say *I saw some of your pictures*, you understand, *I didn't see all of your pictures*.) Nothing about logic or the arrangement of the square of opposition tells us which of these two corners should have second class status. Rather, "it is the [pragmatic] markedness of negative statements and the formal marking of negation itself which are responsible" (p. 264).

Chapter 5, "The pragmatics of contra(dicto)ry negation," deals with phenomena that have been of interest to linguists for years. It starts with an extended discussion of affixal negation (in English; there's extensive literature on the topic) and its occurrence in double negative constructions. The evidence clearly points to the fact that the linguists, and not the Fregean philosophers of language, have it right: the double negation of a proposition is not synonymous with the non-negated form of a proposition. ("Two negatives make a positive" may be true in math and standard logic but not necessarily in natural language.)

The second section of chapter 5 reviews the literature on NEG-raising, a rule involving sentences in which a negative morpheme in the higher clause of a complex sentence is associated with the lower clause. As is typical, Horn doesn't merely trace the discussion back to the Generative Semantics era, but to St. Anselm. The literature is full of attempts at giving syntactic and/or semantic accounts of the ambiguous nature of sentences such as *I don't want you to go* (which could be paraphrased by either *It's not that I want you to go*, or by *I want you not to go*.) Horn again argues convincingly for a pragmatic account of the ambiguity rather than a syntactic or semantic account.

The third section of the chapter, building on the first two, discusses the use of negative constructions which literally appear to be contradictory to the corresponding positive sentence (*I don't like Bill*)

but instead generally have a contrary reading (=*I dislike Bill*). In this chapter, Horn maps scalar epistemic adjectives onto the square of opposition and is thus able to give a pragmatic account of why some have NEG-raising readings (*It's not likely that...*) and others don't (*It's not possible that...*). In these examples and in instances of affixal negation, the strongest reading possible is the reading given to the construction. These, then, are instances of the application of Horn's principle **R** (see above): a stronger-than-literal reading is implicated by the use of a particular negative construction (often used for politeness; see Brown and Levinson [1978] 1987).

This brings up an important distinction between Q-based readings and **R**-based readings, especially as they apply to the conventionalization of narrow readings. A Quantity-based narrowing of meaning is

> linguistically motivated in the sense that an already existing lexical item serves to limit or restrict the use—and sometimes eventually the meaning—of a more productively formed lexical item or expression. Thus the existence of *thumb* tends to restrict the domain of *finger* to nonthumbs (even though a thumb *is* a finger). (p. 358)

On the other hand **R**-based narrowing is "culturally or socially motivated":

> When *drink* and *smell* take on narrowed readings denoting a particular type of drinking and smelling...it is because we can count on an addressee who shares our culture to be able to figure out just which salient, highly charged member of the extension the speaker would have sufficient reason to avoid naming directly. (p. 358)

It should be noted, though Horn doesn't pursue this point, that this insightful distinction is one that would be difficult to account for within Relevance Theory, which has nothing corresponding to the distinction between Q-based and **R**-based implicature.

Another important notion raised by Horn in this chapter is what he calls (following Jerry Morgan) "short-circuited implicatures," or SCIs. These are associated with certain constructions and are basically conversational implicatures on their way to becoming conventional implicatures, i.e., moving from the realm of pragmatics into the domain of lexical semantics. Throughout most of this book, Horn's approach is one that assumes a fairly strict modular approach with clear (and important) boundaries between syntax, semantics, and

pragmatics. This is one passage where that latter boundary seems to weaken:

> While conversational implicature is basically a matter of parole, the short-circuiting of implicatures into usage conventions takes place on the boundary between parole and langue. (p. 347)

Though this "fuzziness" may only be meant in a diachronic sense (as the context suggests), it has interesting implications for synchronic accounts and the modularity thesis in general.

Chapter 6 is largely a revised version of Horn's (often-referred-to) 1985 article. Rather than reviewing it here, I will refer the reader to that article. It introduces the important notion of metalinguistic negation. As in my sentence about John and the hot dog at the beginning of this review and in (4) above, this type of negation is special in that it negates not the content of an utterance, but how it is said. Horn convincingly argues for an account based on pragmatic (rather than semantic) ambiguity.

Chapter 7, "Negative form and negative function," reviews evidence for two classes of negation in natural language and for the non-existence of a Stoic-Fregean external negation. This is presented in terms of the distinction between Aristotelian term logic and Fregean propositional or predicate logic. Horn presents his case on two fronts: theoretical and empirical. He shows how Montague's representation of the syntax and semantics of negation is compatible with a term logic account. And he gives cross-linguistic evidence for the existence of two types of negation:

> ...[1] one negation employed for straightforward negative predications (predicate denials) and for nonexistence claims and [2] another employed for negating identity statements or nonverbal constituents... (p. 451)

However, what is not found cross-linguistically is sentence- (or clause-) peripheral negation, i.e., anything that corresponds to Frege's standard proposition-external negation. Horn also argues that the morphosyntactic evidence suggests negation is similar to tense, a predicate-level operator, and distinct from interrogative markers, operators that have scope over the entire proposition. The natural language data, then, support Horn's extended term logic, in which standard negation (predicate denial) is a mode of predication by which subject and predicate can be combined, not a

proposition-external operator. Pursuing the issue of the position of negation within the clause, Horn discusses Jespersen's Cycle which accounts for the historic drift of negation from pre-verbal position (in VO languages) to post-verbal position.

In the final section of the chapter, Horn deals with complex phenomena involving the scope of negation and quantifiers. He presents one way to formally capture the difference between predicate denial and predicate term negation (as suggested in Generalized Phrase Structure Grammar). He goes on to discuss the interaction of these two types of negation with quantified subjects. He argues that the resulting quantifier-scope ambiguities are truly semantic in nature and not fixed by the syntax in logical form (as current theories might suggest). Finally, he shows how (wide-scope) predicate denial may have either a wide-scope or narrow-scope reading, depending not on syntax or semantics but on discourse pragmatics.

This book is not easy going. It is nearly 600 pages long (including 55 pages of nonsuperfluous footnotes), yet the information rate is not slow. Horn compresses a good deal of significant material on each page. As would be expected in a book of this sort, it often takes some time simply to determine one's own intuitions of the example sentences.

Furthermore, it's not always clear what level of background Horn assumes the reader has. At times he's very "reader-friendly." Thus, at one point, he seems to assume little familiarity with pragmatics, introducing the concepts of conventional and conversational implicatures and their relation to "what is said" and how these notions relate to the semantics-pragmatics interface (p. 144–46). As noted above, he introduces the reader to the basic linguistic contrast of marked vs. unmarked (in the Praguean sense). At another point, he carefully lists Grice's maxims which, like implicature, should be covered in a standard textbook or course on pragmatics or the ph. osophy of language (p. 193).

Yet elsewhere he assumes at least some acquaintance with the literature on formal semantics. Thus the reader is expected to be acquainted with terms such as "rigid designator," with issues and formalisms of Montague grammar (and with the common abbreviation PTQ, which refers to Montague's terse work, *The Proper Treatment of Quantification in Ordinary English*), and with what it

means for an implicature to be "calculable" or "detachable" (features on which conversational short-circuit implicature and conventional implicature differ and agree, respectively (p. 558, fn. 40)). Acquaintance with De Morgan's Laws (covered in standard texts on logic) is assumed on p. 134 but later presented and discussed on p. 222–24 (granted that the discussion of their formulation is relevant in the latter section).

Apparently in the course of writing such an epic, Horn's conception of his audience (understandably) varied. This drawback is compensated for by two features: the book has two excellent indexes (one of authors, another of topics); and Horn's unique wit and writing style encourage the reader to keep on.

As I would expect of University of Chicago Press, there are very few typographical errors. Here are two more significant ones: there is a line missing on p. 361; and in example (40)a on p. 492, *neither* should be starred rather than *so*.

Presumably, most who read this review (typical SIL members) count themselves as members of that set designated by Longacre's OWL label (ordinary working linguist). Reading this book may not seem to be worth the effort. I would recommend it, nevertheless. Even if you aren't looking for a solution to the semantics of negative constructions in a language, this book will teach you a great deal, not only about negation but about wider issues in semantics and pragmatics and the relation between the two.

For those who have some introductory background in logic, the philosphy of language, and/or pragmatics, this book will be enjoyable, though it may be slow going at times. For others, I'd recommend they have basic texts on these topics on hand (especially Levinson 1983 for pragmatics) to help them exegete the tougher sections. Anyone who puts out the effort required to follow the discussion in the book will be rewarded.

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Talking power: The politics of language in our lives. By Robin Tolmach Lakoff. New York: Basic Books, 1990. Pp. xii, 324. $22.95.

Reviewed by Charles Peck

Waxhaw, NC

Professor Lakoff's thesis in this book is that there are many occasions in our experience when we are subjected to someone of greater authority talking to us or when we talk with authority to someone of less power. Such talking power is normal and useful, but it is sometimes abused.

Part I, *The Politics of Everyday Language*, which includes the first three chapters, is introductory.

Chapter one discusses power, politics, and language, in which language is a tool people use to get or express power and politics. People are always using language to play interpersonal politics, and sometimes we like it and sometimes we don't like it. On a larger scale, people go to war to preserve their rights to use their own language.

Chapter two talks about language and linguistics, and how linguists describe language as having form, semantics, and pragmatics. This book is concerned with the pragmatics of language, how people use language in speech acts. People use indirect language for politeness

or for smartness; they use loaded or neutral terms to achieve their desired ends.

Chapter three tells about conversation analysis in terms of conversation type, reciprocity, formality, privateness, spontaneity, and power allocation. Most conversations are more ritualized at the beginnings and endings than in the middles. Conversations in public, on TV, on the telephone, and in the classroom differ from each other.

Part II, *Language and Institutions*, deals with talking power in Freud and psychotherapy, in jury trials, in jury selection, and in University faculty politics. All these situations involve structured social relations within which certain people have power to talk with power. They say some things and leave some things unsaid, and people without power have to learn what they can say and what they must not say.

Chapter four talks about Freud and his followers. They believe that if they can get the patient to talk about his/her difficulties, the patient will be helped. There are, of course, great opportunities for abuse in the therapeutic treatment.

Chapter five discusses the talk-power in criminal court cases—who has the privilege of talk and when.

Chapter six describes the power-talking in jury selection procedures and in the life of the jury.

Chapter seven compares the verbal interchanges in therapeutic discourses and legal discourses in court. Both are open to abuse, but they contrast along most dimensions of difference and are similar in other ways.

Chapter eight tells about the power struggles within university faculty committees on curriculum and hiring, and about the changing power balances in conversations between faculty and students as the students advance in graduate school. The chapter ends with a discussion of academic writing and how it has to be obscure until after the student has been granted the doctorate. (My own dissertation is one of the most obscure pieces of writing that I have ever produced, and I am not proud of it.)

Part III, *Language Across Cultures*, deals with how people in different cultures and subcultures adopt different strategies of communication

and how they have difficulties communicating with each other cross-culturally.

Chapter nine deals with the Cooperative Principle (based on Grice's maxims) and how it is violated to produce politeness, irony, and style. The four maxims for conversation are:

Quantity: Say just as much as is necessary.
Quality: Be truthful.
Relevance: Be relevant.
Manner: Be direct, succinct, and clear.

Grice also proposed another mechanism, Conversation Implicature, to handle purposeful violations of the four maxims.

One of the basic requirements for communication is cooperation and goodwill. It is when people of different cultures try to communicate with each other that misunderstanding occurs; for example, British vs. American humor and irony, and Japanese honorifics and a general vagueness in Japanese expository discourse.

Chapter ten discusses how we use "we" versus "they" based upon sexual, political, religious, and cultural differences. Lakoff analyzes President Reagan's use of "we" in one of his speeches. She also discusses our use of "they" to refer to our opponents in wars of various kinds.

Chapter eleven deals with the power imbalance between men and women. Generally, in cultures around the world, men hold the social, political, and economic power and are surprised and even resentful if women try to break into the power circles. These feelings are reflected in their use of talking power.

Chapter twelve talks about how to be persuasive in Greek, Chinese, and American cultures. Then follows a comparison of speeches by a prosecutor and by a defender in a Latin court case and similar speeches in a California court case. The Romans valued "gravitas" because their culture was a "shame" culture. Americans value "liteness" because their culture is a "guilt" culture.

Chapter thirteen deals with the pragmatics of presenting oneself in a good light. It compares Ceasar's reports home from the front in Gaul and Oliver North's testimony before the television cameras and

congress. Both speakers used third-person references to themselves for special effects.

Part IV, *The Language of Power*, deals with the talking power of politicians, image consultants, and language bosses.

Chapter fourteen discusses how politicians in the new television age in the United States have to learn from and lean on image consultants and spin doctors. They have to learn how to appear competent, warm, and human, and they have to learn how to parry improper and embarrassing questions. They and their helpers have to come up with short "sound bites" for the evening TV news programs, and the sound bites have to touch hearts, not just minds.

Chapter fifteen discusses the "language bosses" who keep trying to make us use proper English. Mostly, they are resisting inevitable language changes. And it is the articulate, well-known language bosses that get heard most.

The theme of this book, "talking power", does not break much new theoretical ground, although it does give us a label to put onto certain human behavior. It seems to me that this new label is the main value of the book.

The book is interesting to read because of its interesting descriptions of various institutions and situations.

The book is moderately difficult to read because of very poor paragraph cohesion. Most paragraphs have good topic sentences, and the sentences themselves are mostly of reasonable length and complexity. But the sentences do not carry the paragraph topic forward. Each sentence in a paragraph seems to be about something different, which makes the material difficult and discouraging to read. ■

Autolexical Syntax: A theory of parallel grammatical representations. By Jerrold M. Sadock. Chicago: The University of Chicago Press, 1991. Pp. vii, 254. Paperback $22.50.

Reviewed by Karl Franklin

Pacific Area

Autolexical Syntax was written while Sadock was a resident at the Center for Advanced Studies in the Behavioral Sciences at Stanford. Some of the materials were also used in classes which Sadock has taught at the University of Chicago.

SIL scholars should note that "the theoretical models of tagmemics (Pike and Pike 1982) and stratificational grammar (Lamb 1966), rough contemporaries of early transformational grammar, are in some ways closer to the model proposed in this book than orthodox grammar is" (p. 222, note 1 of chapter 1). Sadock also mentions lexical-functional grammar but claims that his model "is sufficiently different form all of these models that [he] will not make explicit comparisons with them here" (p. 222). We note that it will be helpful if such a comparison is made in the future.

There are seven chapters in *Autolexical Syntax*: an introduction (pp. 1–17), a sketch of autolexical syntax (pp. 19–43), cliticization (pp. 48–76), incorporation (pp. 78–106), a survey of morphosyntactic mismatches (pp. 111–159), autonomous semantics (pp. 163–184), and extensions of the method (pp. 185–215). The book concludes with an appendix of abbreviations, notes, references, and an index.

The plan of the book is as follows: Chapter 1 discusses the autonomous components of the model, namely, syntax, semantics, and morphology. The lexicon plays a special role within the three modules. Chapter 2 gives a grammatical sketch using the three modules, each a "context-free phrase structure grammar" (p. 17). Chapter 3 deals with cliticization, in that this provides a natural interface between syntax and morphology. Chapter 4 is an historical interlude on incorporation because, in Sadock's view, "noun incorporation offers excellent evidence against the hierarchical model of the relation between morphology and syntax" and because it was one of the principle motivations for *Autolexical Syntax* (p. 78). Chapter 5 takes incorporation and cliticization further by surveying a

number of languages for morphosyntactic mismatches. Chapter 6 treats semantics as a "fully autonomous component, responsible only for the combinatoric regularities that have to do with the meanings of lexemes" (p. 162). Finally, chapter 7 suggests a few extensions to the theory, such as the possibility of a level of discourse-functional organization.

Autolexical Syntax is well illustrated throughout with materials from a variety of languages and with trees that have roots and crossing branches. The notation is fairly transparent and follows generative grammars for the most part. Certain general grammatical constraints or principles are introduced in *Autolexical Syntax*, such as the Control Agreement Principle, the Constructional Integrity Constraint, the Incorporation Principle, and the Linearity Constraint.

I am not sure if *Autolexical Syntax* will have any place in the teaching of grammar at SILs. Although it may be introduced in the survey of theories courses, it is only piecemeal at this point, and until some languages are described using the model it will be of limited value.

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Tributes to Dwight Bolinger

Dwight Le Merton Bolinger was born in 1907 and died on Sunday, February 23, 1992. He was known to most linguistics for his work in intonation. He was professor emeritus of Harvard University and Stanford University. The following excerpts were shared on "Linguist," a university electronic network.

This is to let y'all know that one of our most cherished associates, Dwight Bolinger, died last night at 11:30 PM at a hospital in Palo Alto. For the many of us who have been inspired by Dwight's work and encouraged by his generous sharing of his time and knowledge, this is a true loss. Dwight has never spared himself, his time and his unfailing attention and wit in encouraging young people in their work. While few people in linguistics could actually boast him as their professor, scores could justly claim him as their teacher. Dwight never allowed his temporal age to interfere with his unbounded enthusiasm, nor dull his curiosity, nor curtail his delight in talking to rank beginners about the subject dearest to his heart—language. Through the thick and thin of structuralist dogmas, Dwight was a beacon of common sense and inspiration to all of us who persist in the simple-minded assumption that language is about communication. Dwight was a gentleman of the old school who could nevertheless appreciate the young and their foibles. We will miss him sorely.

–Talmy Givon

I don't expect to attend a memorial service for Dwight Bolinger, and so would like to pretend that this network is a gathering of his friends and colleagues, come together to toast his passing. I hardly knew him, personally, having met him only twice, but I remember the first time I sent him a paper, more than ten years ago, when I really was, if not a rank beginner, a complete unknown. He sent back four pages of comments, all flattering and a mild query: I had cited Quine as the source of the witty observation that when we invite meaning into a linguistic description, he is sure to bring along some uninvited rowdy friends as well. As it happened, Bolinger had written something along those lines himself—was this a case of convergence? As I discovered over many years, so many of the ideas, and so much of the poetry, which I misattributed to Quine and others, or thought

to have discovered myself, I owed to him. I salute his brilliant mind, and his sweet and generous heart. He was the greatest.

–John Haiman

I want to second John Haiman's touching tribute to Dwight Bolinger. I never met him, but had brief contacts with him via correspondence on a number of memorable occasions and have always admired him not only for the astuteness of his observations but for the panache with which he delivered them. No doubt it is this which inspired Jane Hill, in her 1970 review of *Aspects of Language* (Lg. 46.667–670) to describe him as "up to his elbows in the muck of language"—a marvelously apt characterization.

I think of something someone said after the death of the pianist Glenn Gould, which applies as well here: he's gone, and the rest of us are just going to have to get used to it.

–Michael Kac

I have a story also about Dwight's kindness to fledglings. I had been out of graduate school for about a year when I met Dwight. I went up to introduce myself to him at some kind of gathering and he said, "Oh, yes, I just quoted you in a paper I'm working on."

–Susan Steele

Dear colleagues,

Like John Haiman I cannot attend a memorial service for Dwight Bolinger, but I will gladly and sadly take a moment to recall a man whose delight in the discussion of language was so evident. I met Dwight only once, and recall him clearly. I feel now that a little light up the road that we are all travelling has just gone out. Talmy—thanks for letting us know. Peace,

–Ed Keenan

It saddened me very much to hear of Dwight's passing. I didn't know him very well, but I was very impressed with the seemingly inexhaustible supply of knowledge he had at his fingertips. For example, at the Stanford Child Language Conference in 1984 I gave a paper about the acquisition of "even though" subordinate clauses. As a side comment, I noted that the form "although" is acquired even later, and seems to have a slightly different meaning, implication, whatever... Dwight was in the audience. He came up to me after the

paper and told me of a paper written in 1948 for publication but never actually published about that very subject. Later he sent me a copy. If I remember correctly it was by Bloch (I can check, but I'm responding to this topic without preparation—and I clearly don't have a memory anywhere near as impressive as Bolinger's). Those of you who knew Bolinger well can say with authority what kind of person we have lost. I can't say that, but based on my limited experience I realize we lost a lot. More than we'll probably ever realize.

–Benji Wald

I'd like to add my sentiments to those expressed by colleagues for Dwight Bolinger. He was an especially important scholar to me because my work is on intonation. I feel so very lucky to have been able to meet and talk to him—in spring of 1990, when I was in Palo Alto for a brief visit, he invited me to his home for a meeting, because he was too weak to attend my talk. We had a wonderful conversation, full of performed examples of contours, delighted glimpses of recognition as we exchanged observations...and when we politely noted our disagreement on certain issues, he shook my hand with both of his. It was a memorable experience; I left his house feeling like he embodied a standard in scholarship.

We have lost a remarkable man.

–Cynthia McLemore

Yes—linguistics has lost a great scholar and the human race a magnificent human being. To misquote an old union song, Dwight would no doubt now be saying, "Don't mourn for me, work and teach and save the environment."

–Vicki Fromkin

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NOTES ON LINGUISTICS

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Coordinator's Corner

The Linguistics and Phonetics Departments of the School of Oriental and African Languages in London has received notice that it will be shut down beginning in October, for financial reasons. The closing of this department, founded by the renowned Firth, and its associated publications, will be a tragic loss. The announcement is incongruous with the LSA call for renewed impetus for research on minority languages of the world while there is still time to document them.

We will soon inaugurate the *SIL Linguistic Database*. The usefulness of such an archive of data depends heavily upon the cataloging system used to store and retrieve its information, largely missing in the Oxford University Text Archive. Rich Hoffman has provided for us a good system of "handles" so that you can distinguish clearly between dictionary files, word lists, interlinear text, translation materials, language descriptions, etc. Sharing linguistic information via the *Database* should be a boon to:

designers to SIL courses; consultants on the field;
sharing between co-workers; diachronic studies;
typological studies; orientation for new researchers;
the structure, problems, and solutions that people have found
in languages related to those they might work on.

Let you imagination work a bit as you read Arnd Strube's article, "Project '95 Now? A Vision." Suppose you are a consultant or a consultee who is looking for solutions to a linguistic problem. Your scenario might be quite parallel to the one Arnd describes. Someone struggling with a problem in linguistic analysis could get biographical recommendations, suggestions of related areas to research, ideas from colleagues who have tackled the same or similar problems in other languages, theoretical insights offered by one or another approach, etc. It's not just a dream: on LINGUIST, the network for linguists in university circles, a request from a man in Australia for biographical suggestions regarding a particular problem was quickly answered by respondents who supplied him with material that they knew had been useful to them.

–Eugene Loos

Syllable-based Hyphenation

David Weber

SIL Peru

For many years, I resisted hyphenating Quechua text. I once said, "I don't care how long the lines have to be, I don't want to break up words." I thought that words broken across lines would be very hard for the Quechua reader. Then I tried a double column format with a 34-character line. Since Quechua has lots of long words, I had to hyphenate heavily.

The reaction of Quechua speakers was overwhelmingly favorable. When I said to one of my Quechua co-workers, "But just look at how many words are hyphenated!" his reply was "We don't care if **every** line is hyphenated; this is easier to read!"

I am now convinced that hyphenation is very positive. This is partly because hyphenation makes it possible to use shorter lines, which reduces the difficulty of getting from the end of one line to the beginning of the next. But hyphenation also facilitates reading by breaking up big words—the ones most likely to be broken—into visually less ominous and more manageable pieces.

Not long ago, hyphenation was left to the typesetter. Computers have changed that. Now hyphenation is usually automated to some degree or another, giving the author some measure of control. This usually involves putting "discretionary" hyphens—indications of where the text might be hyphenated—into the text. The formatting program then introduces actual hyphens at these points when they are needed to produce an esthetically pleasing page.

So how does one automate the introduction of discretionary hyphens? This can be a difficult problem. English has to be one of the most difficult challenges, in part because hyphenation conventions are not based very solidly on phonological realities, and in part because hyphenation depends on meaning; witness the discretionary hyphens (<->) in "I will pre<->sent him with a pres<->ent." The most successful approach for hyphenating English that I know of is that described by Knuth (1984:449ff), as used in TEX.

Fortunately, for many languages hyphenation is based much more directly on syllabification: a hyphen could be introduced at any syllable boundary.[1] This article is about such cases. Throughout, I will use "hyphenation" to refer to the introduction of a discretionary hyphen at syllable boundaries.

For some languages it may be possible to do an adequate job of hyphenation with a stream editor such as CC (JAARS 1988a).[2] For example, productions like the following might introduce a hyphen at the relevant points:

$$akya \Rightarrow a-kya$$

Of course, it is not practical to list all such possibilities—there might be thousands! Fortunately, CC affords mechanisms for declaring and referring to sets of characters. This makes it possible to keep the number of productions down to a reasonable number.

Stream editors tend to be awkward tools for hyphenation because they are designed to manipulate strings of characters, generally without higher level constructs. Thus they do not lend themselves to modeling linguistic realities like the syllable.

HYPHEN (Black et al. 1987) is a specialized program for hyphenating text. It provides a simple mechanism for declaring classes of characters and stating hyphenation rules in terms of them. For example, if *O* is declared as the class of characters representing obstruents, *G* as the class of glides, and *V* as the class of vowels, then the following production could be given:

$$VOGV \Rightarrow V-OGV$$

HYPHEN allows the user to declare how far from the beginning and end of the word hyphenation should be introduced, so as not to strand too small a piece of a word. It also allows the user to declare exceptions.

Doing hyphenation with HYPHEN is significantly easier than with a stream editor, but HYPHEN shares with stream editors two deficiencies for syllable-based hyphenation: (i) it incorporates no notion of syllable and (ii) hyphenation is introduced by a list of productions (string changes).

Now a new tool is available for hyphenation: SYL.[3] It is one of a collection of programs for facilitating the writing of inital reading instructional materials; see Weber (1991).

SYL is a parser: it breaks words into syllables based on user-supplied information about syllables. Fundamentally, the user declares (i) for initial, medial, and final syllables, whether the onset and coda are obligatory or optional, and (ii) what characters or sequences of characters may occur as onsets, as nuclei, and as codae.

SYL's parser is lodged between a TEXT INPUT and a TEXT OUTPUT module; these allow the user to modify the text before the parser sees it, and—after parsing—before the text is output. The user can control the parts of an input text to which SYL will be applied, e.g., only the vernacular fields of a standard format dictionary. Further, the user can mediate—both on input and output—between the practical orthography of the text (its special character conventions, etc.) and some more linguistic, internal representation. This is relatively easy because SYL incorporates the phonologically-oriented string-changes notation of AMPLE and STAMP's input and output modules (Weber et al. 1989, 1990).

We will now describe how hyphenation is set up using SYL for two languages, Quechua and Spanish.

QUECHUA

Quechua has very simple syllable structure. Non-initial syllables are *CV(C)*, i.e., they have an obligatory (consonantal) onset, a (vocalic) nucleus, and—optionally—a coda. In the first syllable of a word, the onset is also optional. SYL provides a notation that makes it almost trivial to express these facts.

We must now tell SYL what characters can occupy those positions. Let's consider first what would be needed just for native Quechua words and then we will return to words borrowed from Spanish.

The following may occur either in the onset or the coda: *p, t, ch, k, q, l, ll, m, n, r, s, sh, w,* and *y*. *h* and *ñ* may occur only in the onset, and vocalic length may occur only in the coda.[4] The following occur in the nucleus: *a, e, i, o,* and *u*.[5]

Suppose we wish to represent phonemes in these declarations in some way other than the way they are represented in the practical orthography. For example, in texts we might represent long vowels by a vowel followed by a "hat" (^), whereas internal to SYL we wish to represent it with a colon (:). Or *ñ* might be represented as "n~" in the texts but as "~n" internally. So upon input, we make a text conform to the internal representation by changing "^" to ":" and "n~" to "~n."

There are a few other things we must inform SYL: (i) letters used to form words other than the standard alphabetic ones; (ii) what fields of the input text to exclude (or include) from consideration, e.g., we might exclude identification lines (which begin with the field code \id) and cross references (beginning with \r); and (iii) what bar codes might be used, e.g., |i for italic, |b for bold, and |r for normal roman type. (We don't want |baywasha|r to be taken as *baywasha* and *r*, but as **aywasha**.)

We may want the output text to reflect the conventions of a particular word processing system. For example, let us suppose we are hyphenating text that will be formated with LATEX. Its discretionary hyphen is a backslash followed by a hyphen(\-), so we instruct SYL to output all hyphens as \-. Likewise, we change accented vowels to, e.g., \'{a} and ~*n* to \~{n}.

We might also want to suppress hyphenation when it occurs too close to the margins of the word. We do not want to alter the way SYL **syllabifies**; we simply want to suppress discretionary hyphens in some environments. To this end we can give environmentally conditioned rules that delete certain hyphens. For example, to avoid stranding a single vowel, we inform SYL that upon output it should delete hyphens that are separated from the beginning of the word by only one vowel.

Having given SYL this information, we can now hyphenate text. The native Quechua vocabulary is correctly syllabified, but many Spanish loans fail to be parsed into syllables, particularly many Biblical names.[6]

To some extent Spanish loans have been assimilated to Quechua phonological patterns, but they have also expanded the phonological system. For example, native Quechua words have at most a single

consonant in the onset, but Spanish loans have brought clusters composed of an obstruent followed by *l*, *r*, *y*, or *w*.[7] So let us now consider what we need to add.

- To the consonants that can occur in either the onset or the coda, we must add *b*, *c*, *g*, and *z*.
- To the consonants that occur only in the onset we must add *d*, *qu*, *rr*, *v*, and the just-mentioned clusters.[8] To the consonants that occur only in the coda, we must add *x* (as in *Félix*).
- To the characters that may occur in the nucleus, we must add the accented vowels (*á*, *é*, *í*, *ó*, *ú*) and—for Biblical names—about 30 (!) vowel sequences.[9]

After running SYL on some text, we see that a few Quechua words are no longer correctly syllabified. For example, *musyan* is syllabified as /mu.syan/ rather than as /mus.yan/ and *aywarraq* is syllabified as /ay.wa.rraq/ rather than as /ay.war.raq/. These problems arise because we have added *sy* and *rr* to the list of onsets. When SYL breaks words into characters, it favors longer possibilities over shorter ones. Since the parse succeeds with *sy* and *rr*, nothing forces SYL to give a parse with /s.y/ or /r.r/.

Here is how to force SYL to give the right answer. **First**, when text is input, we flag those *s* and *r* of *sy* and *rr* sequences for which the *s* or *r* is really a coda. There are only about a half dozen such cases for *sy*, namely, the roots *asya-*, *musya-*, *pedasya-*, *qosya-*, *turasya-*, and *usya-*; we change these to *as*ya-*, *mus*ya-*, *pedas*ya-*, *qos*ya-*, *turas*ya-*, and *us*ya-*. There is only one such case for *rr*, namely, the suffix *-r* followed by the suffix *-raq*. We can catch all—and only—these by changing the sequence *rraq* to *r*raq*.[10] **Second**, we must add *s** and *r** to the list of consonants that occur in the coda, thus making it possible for the words containing the flagged sequences to be parsed. **Finally**, since we do not want the output text to contain * as part of a word, we tell SYL to delete it upon outputting the hyphenated text.

SPANISH

In considering syllabification and hyhenation in Spanish, Harris (1983), Mañas (1987), and *The New World Dictionary* (Ramondino 1968) have been very helpful.[11] About hyphenation and syllabification, Mañas writes:

> Spanish is a language with very precise and regular orthographic rules. It is clear how to hyphenate words and how to syllabicate.
>
> ...Spanish has a more rigid lexical structure [than English—DJW] where rules can be precisely stated without exceptions.

I only wish Mañas were right! Before considering various complications, let's start with the basic facts. The core of what we must inform SYL for Spanish is as follows:

1. For all syllables (initial, medial, and final), both the onset and coda are optional, that is, syllables are *(C)V(C)*.
2. The following characters occur in both the onset and the coda:[12] *p, t, c, k, j, b, d, g, f, s, z, l, r, m, n,* and *y*.[13]
3. The following occur only in the onset: *ñ, qu, gü, h, ll, rr, gu, ch, pr, pl, tr, cr, cl, br, bl, dr, gr, gl, fr, fl,* and *v*.
4. The nucleus can be filled by one of the five vowels (either accented or unaccented), by a diphthong,[14] or by a triphthong.[15]

Having declared just this much, SYL does a pretty good job. But "pretty good" isn't good enough. Let's consider some of the problematic cases. First, we will consider various "rules" for syllabification:[16]

1. Two adjacent STRONG VOWELS (*e, a, o*) form two syllables, e.g., *roer* /ro.ˈer/, *léelo* /ˈle.e.lo/, *boa* /ˈbo.a/.
2. An unstressed WEAK VOWEL (*i, u*) and either a strong vowel or another weak vowel form a diphthong (so are part of one syllable rather than two); for example, *bién* /ˈbʲen/, *creación* /kre.a.ˈsʲon/, *puesto* /ˈpʷes.to/, *cuanto* /ˈkʷan.to/, *continuo* /kon.ˈti.nʷo/, *fué* /ˈfʷe/, *cuál* /ˈkʷal/, *continuó* /kon.ti.ˈnʷo/, *oigan* /ˈoʲ.gan/, *óiganme* /ˈoʲ.gan.me/, *veréis* /ve.ˈreʲs/, *tráiganmelo* /ˈtraʲ.gan.me.lo/, *eucalipto* /eʷ.ka.ˈlip.to/, *cáustico* /ˈkaʷs.ti.ko/, *triunfo* /ˈtrʲun.fo/, *viuda* /ˈbʲu.da/.[17]
3. If a weak vowel is stressed, then it acts like a strong vowel, forming its own syllable; for example: *maría* /ma.ˈri.a/, *raíz* /ra.ˈis/, *continúo* /con.ti.ˈnu.o/.
4. These rules apply even if the silent letter *h* intervenes:

   (a) The following do not have diphthongs because *h* is flanked by strong vowels: *abraham* /ab.ra.ˈam/, *ahoga* /a.ˈo.ga/, *ahora* /a.ˈo.ra/, *ahorcó* /a.or.ˈko/, *enmohecido* /en.mo.e.ˈsi.do/, *moho* /ˈmo.o/, *mahalaleel* /ma.a.la.le.ˈel/, *quehaceres* /ke.a.ˈse.res/, *rahab* /ra.ˈab/.

(b) The following are diphthongs involving a strong and a weak vowel across *h*: *ahijar* /aⁱ.ˈxar/, *prohibir* /proⁱ.ˈbir /, *sahumerio* /saᵘ.ˈme.rⁱo/, *desahuciar* /des.aᵘ.ˈsⁱar/, *buhardilla* /bᵘar.ˈdi.lla/, *buhonero* /bᵘo.ˈne.ro/.

(c) The following are not diphthongs because the weak vowel is accented and thus acts like a strong vowel: *búho* /ˈbu.o/, *ahínco* /a.ˈin.ko/, *ahí* /a.ˈi/, *bahía* /ba.ˈi.a/, *nahúm* /na.ˈum/, *prohíbe* /pro.ˈi.be/.

5. **For writing purposes,** *ui* is always a diphthong.[18]

Unfortunately, for every rule there is an exception (or perhaps many!). Let's consider some complications involving vowels:

- Although two adjacent strong vowels (*e, a, o*) should form two syllables, I have yet to hear *Aarón* pronounced as more than two syllables (/a.ˈron/).
- Consider *viajante* and *viaducto*; the *ia* in the former is a diphthong (/bⁱa.ˈxan.te/) whereas in the latter it is two syllables (/bi.a.ˈduk.to/). Likewise, in *viaje* it is a diphthong (/ˈbⁱa.xe/) whereas in *viable* it forms two syllables (/bi.ˈa.ble/).[19]
- By rule, *almohada* should be syllabified as /al.mo.ˈa.da/, but in Perú it is pronounced /al.ˈmᵘa.da/. *Orihuela* is syllabified as /o.ri.ˈʷe.la/, rather than according to one of the two possibilities given by the rules, namely, /o.rⁱu.ˈe.la/ or /o.riᵘ.ˈe.la/.
- *uí*, that is, *ui* with an accent, is a diphthong in pre-penultimate and final syllables (e.g., *cuídalo* /ˈkᵘi.da.lo/ and fuí /ˈfᵘi/) but not in penultimate syllables: *huída* /hu.ˈi.da/, *destruído* /des.tru.ˈi.do/, *constituída* /cons.ti.tu.ˈi.da/, *contribuído* /con.tri.bu.ˈi.do/, *destituído* /des.ti.tu.ˈi.do/, *incluía* /in.klu.ˈi.a/, *incluído* /in.klu.ˈi.do/, *ruído* /ru.ˈi.do/.[20]

  This shows one way that syllabification is context sensitive: determining whether *uí* is or is not a diphthong requires determining whether or not it occurs one syllable from the end of the word.
- *ui* is not a diphthong when immediately preceded by *g* or *q*, for in that case the *u* is part of the letter *gu* or *qu*. This is so obvious to humans that the rule does not mention it. Unfortunately, nothing is obvious to a computer.

  More seriously, although the Academy decreed that *ui* is always a diphthong for purposes of writing, that is certainly not the case for speech. For example, *nuir* is pronounced as /u.ˈir/, not /ᴺir/ or /uⁱr/.

  Once we realize that *huir* is really *huír*, but lacking a written accent,[21] it fits the previous observation that *uí* is not a dipthong word finally.

This shows another way that syllabification is context sensitive. Determining whether *ui* is or is not a diphthong requires knowing whether it occurs in a stressed or a non-stressed syllable. That in turn depends on (i) its position, (ii) the final letter of the word, and (iii) conventions like the one just mentioned.

Our problems do not end there, however. Let us consider some other problems, mostly involving consonants:

- *x* represents the phoneme sequence /ks/. When *x* is intervocalic, the syllable boundary falls between /k/ and /s/. For example, *éxito* is syllabified as /'ek.si.to/.
- Words with certain prefixes do not follow the normal patterns, e.g., the normal pattern would be to syllabify *inepto* as /i.'nep.to/, just as *inerte* is syllabified as /i.'ner.te/. However, since /in-/ is a prefix, *inepto* should be hyphenated (and perhaps syllabified?) as /in.'ep.to/, with /n/ as the coda of the first syllable rather than the onset of the second. Likewise, because /trans-/ is a prefix *transatlántico* should be hyphenated (and syllabified?) as *trans.at.lán.ti.co* rather than as *tran.sat.lán.ti.co*.
- At the beginning of a word, the first consonant of *gn, mn, ps*, and *pt* is silent. In other places these sequences are two consonants. Examples follow: *gnat* and *gni.do* but *dig.no, mne.mo.tec.nia* and *inna.són* but *on.no, psi.co.a.trí.a* but *lap.so, pterod'actilo* but *ap.to.*

  Because the contexts of these cases are well defined, they are fairly easy to handle. It is more difficult when the sequence occurs in the middle of a word. For example, an alternate spelling for *infrascrito* /in.fras.'kri.to/ is *infrascripto* (in which the *p* is silent, unless a speaker wants to show off his knowledge of Latin). Likewise, an alternate spelling for *setiembre* /se.'tiem.bre/ is *septiembre*.

  Because such cases are few, and because pronouncing the /p/ is not incorrect, we will allow such cases to be analyzed as /p.t/.
- *bs, ls, ns, ps, rs* and *x* (=/ks/) may occur in the syllable coda;[22] for example: *obstante* /obs.'tan.te/, *vals* /'bals/, *monstruo* /'mons.tr^{u}o/, *solsticio* /sols.'ti.s^{i}o/, *bíceps* /'bi.seps/, *perspicacia* /pers.pi.'ka.s^{i}a/, *expulsar* /eks.pul.'sar/, *sexta* /'seks.ta/, *félix* /'fe.liks/, pólux /'po.luks/. However, in other words each of these sequences could be broken across a syllable boundary; e.g., *absurdo* /ab.'sur.do/, *Elsa* /'el.sa/, *ansioso* /an.'s^{i}o.so/, *rapsoda* /'rap.so.da/, *sarsa* /'sar.sa/, *éxito* /'ek.si.to/.

Let us now consider how to meet these challenges using SYL. Our strategy will be the same as described above for handling *Quechuarr* and *sy*. It involves three steps: (i) we have SYL identify and flag special cases on input, (ii) we inform SYL how the flagged sequences

should be handled by the parser, and (iii) upon outputting text we have SYL strip out all flagging.

For cases discussed above, we instruct SYL to make the following changes to incoming text:

- We flag *gn, mn, ps,* and *pt* when these occur word initially. There are various ways to do this; the one used was to insert an asterisk between the letters.
- We flag any instances of *bs, ls, ns, ps,* and *rs* that do not precede a vowel. This was done by inserting an asterisk between the letters.
- We change *x* to *k*s* if it does not precede a vowel. Otherwise we change it to *ks*.
- We flag the instances of *uí* in the penultimate syllable. (Recall that we wish to stop SYL from treating these as diphthongs.) We do this by adding *$* between the *u* and the *í*.

  To flag *uí* only in the penultimate syllables, we restrict the introduction of *$* to the following environment:[23]

  /~{g,q}_(C)V({n,s})#

  Making this constraint known to SYL is quite straightforward because it recognizes a very similar notation.

Having instructed SYL to make these changes to the input text, we must now tell SYL how to treat them in the parser itself. We make the following additions:

- We add *g*n, m*n,* and *p*t* to the set of characters that can occur only in the syllable onset.
- We add *b*s, k*s, l*s, n*s,* and *r*s* to the set of characters that can occur only in the coda.
- We add *p*s* to the set of characters that can occur in both the onset and the coda (to handle words like *psiquiatría* and *bíceps*).
- We add *$* to the set of characters that can occur only in the onset, forcing *uí* to be analyzed as /u.$i/.

Before text is output, we want SYL to make the following changes:

- *k*s* must be changed back to *x*. All other cases of *ks* should have been analyzed as /k.s/, so they appear as *k-s*. What we instruct SYL to do with these depends on our purpose. If we are interested in the phonological realities, we will probably have SYL put them out as is. However, if our purpose is to hyphenate text, we will probably have SYL change *k-s* to *x*, thereby disallowing hyphenation around *x*. For example, *ek-si-ge* would be changed to *exi-ge* and *flek-sio-na* to *flexio-na*.
- * must be removed so that *g*no-mon* becomes *gno-mon*, *ob*s-tan-te* becomes *obs-tan-te*, and so forth.
- *$* must be removed so that *in-clu-$i-a* becomes *in-clu-i-a*, *in-clu-$i-do* becomes *in-clu-i-do*, and so forth.

We might also want SYL to make further changes to customize hyphenation. For example, to avoid stranding a single vowel (one of the Academy's rules for hyphenation), SYL would have to delete any hyphen separated from **either** margin of the word by a single vowel.[24]

SYL has another capability that has not yet been mentioned: we can declare exceptionally hyphenated words and SYL will introduce the declared hyphenation into these words in text. We might include exceptions like the following: *ca-pa-ra-roch, eh, etc, gui-tart, jrus-chef, li-ving, lud-wig, mé-xi-co, marx, marx-ismo, oh, o-ri-hue-la, po-po-ca-té-petl, ré-cord, sáenz, tiant, trans-at-lán-ti-co, wal-ter, y.* We might also add Roman numerals like *v, viii,* and *x.*[25]

This list includes words that cannot be parsed (based on the information we have given SYL), such as *tiant* and *living*, and others that are parsed but for which we want something different than what SYL produced on the basis of the data supplied to it. This is the case for *inexacto*, *méxico*, and *transatlántico*, for which SYL would produce *i-nek-sac-to* and *mék-si-co* (or *i-nexac-to* and *méxi-co* if we change *k-s* to *x*), and *tran-sat-lán-ti-co*. For a few idiosyncratic cases, the expedient thing is to declare them as exceptions. If there are many, based on some generalization, then we should seek to capture this. For example, for the prefix *trans-* we could modify the output with a rule like the following:

$$tran-s \Rightarrow trans-/\#\_\_$$

CONCLUSION

This article has focussed on difficult aspects of syllable-based hyphenation, many of which prior attempts at automated hyphenation simply ignored. But it is time to reflect.

Because SYL is a parser, it gives us a framework in which to address these problems, one in which the overall complexity is considerably less than if we were to address them simply on the basis of strings of characters. To take but one example, we mentioned that *gui* and *qui* are exceptions to the rule that *ui* is a diphthong. What did we have to do to make this clear to SYL? Precisely nothing. Simply by declaring that *gu* and *qu* are possible syllable onsets, SYL parses *gui* as *gu-i* and *qui* as *qu-i*. When *gu* is not followed by a vowel, then it is treated as *g* followed by the vowel *u*. We simply do not have to worry about a great many such cases because SYL is a parser.

The moral: because SYL's basic engine is a parser, the overall complexity of implementing accurate, syllable-based hyphenation is considerably less than for other approaches.

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NOTES

1 We may wish to exclude those that occur too close to the word's margins, for which an actual hyphen would strand too small a piece of a word.

2 Another possibility is SED, a utility that has been around for a long time in the UNIX environment and which is now available from the Free Software Foundation for MS-DOS.

3 Beth Bryson created the first version of SYL based on an early version of SYLCHK; see Black et al. (1987). Gene Chase updated Beth's SYL to bring it in line with the programs in Black et al. (1987). Beth implemented further enhancements. Steve McConnel is responsible for SYL's final implementation; he brought it into line with the programs described in Weber et al. (1989, 1990) and added some major features.

4 Support for the claim that long vowels as a vowel followed by a consonant is given in Weber and Landerman (1985).

5 I am assuming that mid-vowels have become phonemic. Some Quechua languages have only three phonemic vowels, and in most—but not all—instances, mid vowels are allophones of the corresponding high vowel.

6 For purposes of testing, a word list was made of the Spanish New Testament (UBS 1970) (using the WF program, another in the collection described in Weber 1991).

7 These are: *pr, fr, br, tr, dr, kr, gr, pl, fl, bl, kl, gl, py, fy, by. ty, dy, ky, gy, jy, sy, pw, fw, bw, tw, dw, kw, gw, jw*, and *sw*.

8 We generally write Spanish loans according to Quechua orthography, so /k/ is represented as *k*, and /b/ is written as *b*, whether or not it is written with a *b* or a *v* in Spanish. However, Biblical names are written as in Spanish, thus necessitating *c, qu, v*, etc.

[9] Among these are the following: *aa, ea, ia, oa, ua, ae, ee, ie, oe, ue, ai, ei, ii, oi, ui, ao, eo, io, oo, au, eu, iu, ía, aó, úa, éu, ío, ió, eí, úi, and oí.* In Spanish many of these would be two syllables, but for Quechua I have decided to treat them as a single, indivisible nucleus because (i) when parsing Quechua syllables for linguistic purposes, I am not interested in these words, and (ii) when parsing for hyphenation, I do not want these broken because I have no idea how Quechua people will pronounce them.

[10] There are a huge number of Spanish loans in which *sy* is an onset, among them the following: *albansyo:so, administrasyún, albasya, anutasyun, aptusya, awdensya, basyu, bindisyun, bisya-, deklarasyun, denunsya, disgrasya, dispasyu, dinunsyu, disprisya-, desyemre, dewrasyas, hwisyu, idukasyun, eliksyun, emerhensya, indiksyun, inlisya, insensyu, instruksyun, kawsyun, kunsyenti-, maldisyun, nasyun, negusyanti, negosyu, palasyu, pasya-, pasyun, pirdisyun, rilasyun, risyu, rubinsya, sisyun, superbisyun, syentupye, tilibisyun, tintasyun, ubligasyun, ofisyal, urasyun,* and *osyo:su.*

[11] Ramondino (1968) was particularly valuable because it gives a phonemic form of the word, with syllabification and stress. The pronunciation guides of some dictionaries are of little use. Those of Velázquez (1974) are the labored pronunciations of slow speech; e.g., for *viaje* it gives *ve.ah'.hay,* whereas Ramondino (1968) gives *'bja.xe.* (I would represent this as *'bia.xe* in this paper.)

[12] *Neftalí* and *reloj* are the only words I know of that have *f* and *j* in the coda. Except for these words, we could place *f* and *j* with the consonants that occur only in the onset.

[13] Note the absence of *x*, which is handled as /ks/, as discussed below. *sh* might be added for Peruvian place names, but it is probably better to include these as exceptions.

[14] These are: *ái, áu, éi, ói, ai, au, ei, eu, iá, ié, ió, iú, ia, ie, io, iu, oi, uá, ué, uí, uó, ua, ue, ui,* and *uo,* as well as roughly the same combinations with an *h*: *áhi, áhu, éhi, óhi, ahi, ahu, ehi, ehu, ihá, ihé, ihó, ihú, iha, ihe, iho, ihu, ohi, uhá, uhé, uhí, uhó, uha, uhe, uhi,* and *uho.*

[15] *iéi, iái, uéi,* and *uái.*

[16] These rules are endorsed by the Royal Academy of the Spanish Language. Most dictionaries and many textbooks have a summary of them.

[17] In Andean Spanish I think these last two are pronounced /tri.'un.fo/ and /bi.'(y)u.da/ respectively. The syllabifications for these given in the text above are those of *The New World Dictionary* (Ramondino 1968).

[18] From the Royal Spanish Academy's "New definitive text in force as of 1 January, 1959," as quoted in Velázquez (1974;781ff).

19 *The New World Dictionary* (Ramondino 1968:241) gives /bja.'xan.te/ but /bi.a.'duk.to/, and /'bja.xe/ but /bi.'a.ble/.

20 The reasoning runs as follows. Accent is not written on a stressed penultimate syllable. Thus, in the penultimate syllable, *uí* must indicate something other than a stressed diphthong. Therefore the accent must serve to mark a weak vowel (in this case *i*) as a strong vowel. Consequently, it must be /u.i/.

21 The Academy declared (as quoted in Velázquez 1974:782) that "Infinitives ending in *-uir* will continue to be written without an accent."

22 Harris (1983:15ff) gives a very useful chart of the various ways syllables can end.

23 I have used one bit of non-standard notation here, namely ˜{} to indicate the absence of the enclosed segments. We do not want to flag *gui* and *qui* because *gu* and *qu* are themselves letters, so *ui* is not potentially two vowels following them.

24 Recall that, for Quechua, it was only necessary to state this for the beginning of the word because—in Quechua—the onset of non-initial syllables is obligatory; hyphenation would never be introduced before a vowel at the end of a word.

25 *i, ii, iii, iv, vi, ix*, and *vii* can be parsed because *i* is a vowel. A hyphen would not be output if we suppress hyphenation that would strand a single vowel. ■

Project '95 NOW?
A Vision

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SCENARIO 1

Linguist X working in Southeast Asia has a problem in his analysis of the language. He has nobody out there to turn to. His consultant will only come to see him in one month's time, letters are slow, and the answer might come even later. He has no CD-ROM drive and, of course, Project '95 is still in the works, so even if he did have one, it would not give him the information he needs.

However, X does have a modem connecting his computer to the telephone line, and he works in a country which has access to international data networks (there are probably very few countries that don't).

So he fires up his computer, formulates his problem into a message, starts the CompuServe Information Manager, and with a couple of keystrokes (mouseclicks, if you prefer), he logs into the SIL forum and posts his message there. On the way, he picks up a font file from the Special Characters Library. Finally, he discovers that there seems to be a write-up from somebody in a neighboring country that might shed light on the problem he's wrestling with. Another couple of keystrokes—5 minutes downloading time (N.B., with **any** 2400 Baud modem, you can suck in roughly 10–12kBytes per minute), and the file is on his computer. As he plans to work on Mark's Gospel next, he also downloads a file with the latest edition of exegetical helps on Mark (not printed yet!). 20 minutes. He picks up electronic mail waiting for him, saves it, and logs off.

Now he can leisurely go through the materials he just collected. Tomorrow, he will call in again to find that a bright young linguist in Dallas picked up his request and returned some helpful comments on the problem—much as a consultant would have done. Incidentally,

this man himself uses an Apple Macintosh. He might work at a large university computer system. It does not matter.

I don't know exactly how much X would have paid for this session. Here in Europe, he'd pay $0.25 per minute for CompuServe and $0.18 per minute for the telephone call, which amounts to $12.90 for this particular session. This is not cheap. However, neither is mailing a diskette or a book, let alone paying for a consultant trip. And neither medium could have given him **what** he needed **when** he needed it. Of course, he also had to buy a modem (I would advise everyone to get one with a new computer!).

I am a "computer man." I am out there to help people with their questions. Mostly I can. Sometimes though I have questions myself, especially with new equipment or new software (talk about Windows!). There are few people I can ask around here. They might say, "Yea, I know there is a way of doing it. I just can't remember how." So I turn to CompuServe. And I get answers most of the time. The big software houses are definitely more helpful on CompuServe than they are in their European offices. New insights or tools shared in a forum will quickly find their way to others who need them.

Now I have to admit that I cheated a little bit. The thing is—**there is no SIL forum** on CompuServe. Not yet in any case. It **could** be there. I wish it were. And I wish more people would know about those resources.

It is possible to have closed forums on CompuServe. I neither know the costs involved nor the procedure for making that happen. And of course, we'd need a SysOp. That's the guy (a large forum takes several) who keeps things running smoothly, makes sure the right stuff is in the libraries, keeps out the viruses, and answers those questions that nobody else picked up.

Is there anybody else out there who would like this vision to become reality? It could become reality **now**. Everybody could use our resources corporation-wide as they are collected, and not only when they are finally going to be stamped into some CD-ROM along with more sophisticated software yet to be delivered.

SCENARIO 2

I get a call from a person some 300 miles away. She has problems running Shoebox. She just doesn't seem to be able to open any of her "boxes" anymore. I tell her to hook up her modem and start LapLink Pro. Putting down the phone, I fire up my own copy of LLPRO. I instruct it to dial her number, and within a few seconds, there is a connection to her computer. I can view her hard disk from my terminal. I decide to copy her AUTOEXEC.BAT and CONFIG.SYS to my machine to get a better picture. But wait! There is no CONFIG.SYS. Only CONFIG.BAK. Well, so I copy that. I hang up the phone. Yes, that's it. Since there was no CONFIG.SYS, of course the files=20 statement was missing, and that had caused her problem! I make sure that CONFIG.BAK contains all statements that it should, rename it to CONFIG.SYS, and dial her computer a second time to copy it back. Done.

Unfortunately, reality is still different. Only three members of our branch own a modem. That includes the director and me. Nobody has really encouraged people to buy modems so far. And I keep on getting those calls for help that could have been much more easily solved than by just talking over the telephone and making that poor lady recite CONFIT.SYS (err, CONFIG.SYS—well, maybe she does get into fits!). ■

Some Guidelines for Writing Linguistic Survey Papers

Part I: How to Get Started

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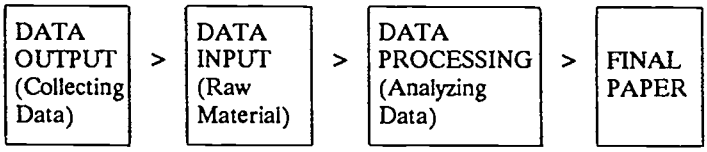
1 INTRODUCTION[1]

What is the final product of all your research? That's obvious. You write a wonderful paper with a good governing question (i.e., thesis), an interest-catching beginning, a fascinating discussion and analysis of your data, and an insightful and satisfying conclusion. You answer everyone's questions about the languages you have looked at. Your presentation examines every relevant point in regards to literacy, translation, and appropriate approaches and methods. From reading your paper, linguists all over the world will have the phonological, morphological, syntactic, and discourse data to study thoroughly your language's position in the world and review current theories on the universals of language. That's obvious. It's also **obviously impossible.**

So what you will be forced to write is an approximation. You may never be content with what you have written; you may not feel that you have reached the end of your investigation. Your paper is an offering to the world of data and analysis that is valuable and should not be kept to yourself. Others need the information to accomplish their goals. Your imperfect information is better than nothing.

So you swallow your pride, and you cast your paper and your reputation into the arena of published scholarship—**if** you can ever get the paper written up.

What is needed can be pictured as a rough diagram of output, input, and process.



That is, you collect all of your data—wordlists, questionnaires, interviews, texts, etc. Obviously this collection is never finished, so you will be analyzing, writing, and publishing before you have a perfect set of data. The data is then the input, the raw material, to your processing, i.e., your analysis. You take what you learn from your analysis and write it up in your final paper. If you look at the whole process from the other end, you will see that for a publishable paper, you will need (a) effective and clear writing that (b) explains your adequate analysis of the data that is (c) based on good data (d) collected from many sources. Although these stages feed into one another, you will be performing all stages concurrently, and so you should begin writing as soon as you can.

Part I of this discussion, which appears here, will provide some guidelines for technical writing. Most of this will deal with some parts of a technical paper. Part II, which will appear in the next issue, offers a list of questions to help organize and stimulate your analysis of the data. Appropriate collection and analysis guidelines will need to be found elsewhere.

2 PARTS OF A TECHNICAL REPORT

There are eight parts of a classical paper. Those parts can be applied to linguistic reports with the happy result of making them both easier to read and easier to write. In contrast, some other writing is often seen as having three parts: beginning, middle, and end. I have found these three generic parts not nearly so helpful in organizing papers as the eight more meaningful classical names.

The eight names (Latin names are in parentheses) and their purposes in a paper are as follows.

2.1 Beginning

The beginning of a paper can have the following four parts.

2.1.1 Hook

The first part is the hook (exordium). It is your reader's first contact with your paper. The purpose is to interest your readers, to prevent them from going on to something else they consider more interesting. It consists of the paper's title and the first sentence(s).

This is one of the parts where you can and should be as creative as possible. The following is a list of possible hooks or beginning stimulators:

1. a controversial statement
2. an element of surprise
3. a note of contradiction
4. a short, dramatic statement
5. the use of statistics
6. a figure of speech such as a metaphor, a simile
7. the use of a quotation (be careful you don't choose a "dry" one)
8. reference to a current event of interest to the readers
9. a few rhetorical questions (make sure the answer is affirmative for your audience)
10. a creative definition
11. an anecdote from the people you are describing or from your work
12. a combination of some of the above

The people whose language you are describing are living and fun-loving human beings. Take something from your contact with them to make them come alive to the readers.

In contrast, the following beginnings tend not to be hooks but "slides" to urge the potential reader on to something else. So don't begin with:

1. an apology (e.g., "I really don't know much about this subject, but...")
2. a complaint (e.g., "I didn't want to write this paper, but nobody else would so...")
3. a platitude (e.g., "They say linguistic papers are a dime a dozen, so...")
4. a direct reference to the title (e.g., "I entitled this paper *How to Write Papers* because I wanted to write about how to write papers.")

I find it is helpful to review this list from time to time and write about what comes to mind. I then use the material generated to write my hook when it finally comes time to construct my paper.

But these creative approaches to your writing should not be confined to the hook. You can use them also as part of the background or digression of your paper (see below).

2.1.2 Background

The second part of your paper's beginning is the background (narratio). It is an orientation to provide background for your topic and to continue to build interest. For example, you often want to introduce previous studies that bear on your topic and to discuss how and why you undertook this project. At this point you don't need to give all the details—just enough information to orient and prepare the reader for the next statement.

2.1.3 Thesis

The most important sentence or two in your paper is your thesis (propositio). It is the governing question of your research topic. The thesis states the position that you are taking in your paper. It also begins to imply the organization of the paper. For example, one might write: "This present study presents the results of a lexico-statistical survey of six GEF languages found in..."

As a minimum, your thesis should state your topic and a position on it. It will imply your purpose and orient your reader to your paper's direction. Avoid forms that begin like "I will write about..." A sentence like "It is the purpose of this paper..." is adequate (and apparently sometimes almost unavoidable) but not terribly interesting. A simple declaration like the following can avoid overused frame sentences: "The 15 villages surveyed for the XYZ language can be divided into four dialects." Sometimes a more complex declaration is necessary if it is likely that some difference of scholarly opinion must be considered. For example, "In spite of the recent paper by ABC, the material gathered for this report suggests that at least four separate dialects are needed to explain the various attested forms." Expressions like, "although," "however," and "in spite of" can be useful for this type of thesis statement.

2.1.4 Partition

Following your statement of purpose, you should write a partition statement (partitio). This is a sentence or two where you present a map or table of contents supplying the names of the coming sections of the paper so the reader stays aware of his place in the material. You can actually supply a short outline, or just write a sentence that states your main points in the order that you discuss them.

Your paper should have a good outline that supports your thesis. Remember, this is the final product. The outline has evolved as you have researched and written. I flip back and forth constantly between my text and outline at the top of my file. If your word processor has an outlining function, it can be very helpful to indicate if your intended outline matches your implemented outline. The partition statement may be one of the last sections of the paper you write.

2.2 Middle

The middle of the paper may have three parts.

2.2.1 Prelude

The middle often begins with an anticipation of questions or reservations that might occur to readers. It is called prelude or rebuttal (reprehensio). For example: "Several factors mitigate against the classification of X as an A language and for its assignment with Y to the B language family." Any questions about the main position should be considered here. Your goal is to set your readers' minds at rest because you have considered their reservations. Then, those readers can concentrate on your evidence. You should also consider if you need to anticipate objections to minor parts at the appropriate places in your paper.

2.2.2 Body

The major part of your paper is the body (confirmatio). It is the paper. Here you discuss the details of your paper in the order of the partition. Statistics, analysis, and charts that present the data and analysis are included here. The description is short, but perhaps 90% of what you write is body, the arguments to establish your position.

2.2.3 Digression

The last logical part of the middle is the digression (digressio). A digression is something that constructively breaks the flow (or monotony) of your paper to stimulate interest, e.g., charts, pictures, jokes, or any creative element. Even some footnote discussions can provide a digression. Here you can use some of the creative ideas that were stimulated by the list in the hook description above, and they can actually be located in many parts of the paper, not just at the end of the middle section.

2.3 End

Finally, you must write an end (pereratio) that is appropriate for the type of article you are writing. In the ending, you can focus back by using words from your introduction, restating your thesis, or summarizing your main points. You can also focus on the future where your paper has led by forecasting the future, calling for a particular kind of action, discussing implications, or pointing out the significance of your ideas.[2]

3 SOME PROBLEMS OI .VRITING

Make your own personal checklist that contains some suggested solutions for the problems and weak points in your writing with reminders of what you want to see in your papers. Include strengths that you would like to develop with words (e.g., Have you ever used "mitigate" in a paper? This is the first time for me. Don't use worn out lexemes.), sentence types (e.g., If you almost always write "SVO," then look for good example sentences to add to your checklist.), and discourse features. Build the checklist file on the computer. Put in notes on organization, clever beginnings, sentence construction, and paragraph construction.

4 SOME METHODS OF WRITING

So how do you start writing? Write now, not later. Anything is better than nothing. It's raw material. Write when you have a lot of time; you can't afford to pass up the opportunity. Write when you have very little time, otherwise you'll never get anything written.

Write when you're inspired by a new idea, even if it's just a couple of lines. These lines may be among the most interesting in your paper and lend sparkle to the paragraphs you had to grind out. Write when it's all work and no fun. Write every day.

Also don't wait until you're done analyzing to start writing. Your writing can point out gaps in your analysis. Each section is made of paragraphs, and each paragraph answers a question. Each paragraph is made up of sentences, each of which is made up of..., etc. Also there are special paragraphs. Your beginning doesn't have to be written at the beginning. Your conclusion doesn't have to be known before you write. Digressions can add interest to your writing.

Use what's called a template, that is, a skeleton outline on a computer file. It would include date, file name, a good thesis (governing question), and a lot of little questions and suggestions for paragraph topics. In fact, make yourself a master list of questions to ask. (A beginning list for a survey report is offered in Part II of this paper.) Then write your paper on the computer, in part by answering the questions that you have in the file. Later you can arrange your answers into paragraphs and sections in your developing outline.

Remember: Back up the file and print it often. How often should you back it up? Answer: How much can you afford to lose?

Look for *Part II: Questions to Use in Writing Up Survey/Feasibility Studies* in the next issue. It will offer a beginning list of questions for a linguistic survey paper to help organize and stimulate your analysis of the data. It is a list that could also be adapted to other types of linguistic writing.

NOTES

1 An earlier version of this paper was first presented at the 1991 Thailand Group SIL Socio-linguistic and Linguistic workshop, 4–5 October 1991, Bangkok, Thailand.

2 Helpful ideas on writing introductions and conclusions can be found in Michael E. Adelstein and Jean G. Pival, *The Writing Commitment*. New York: Harcourt, Brace, Jovanovich, Inc., 1967. pp. 296–308. The list of beginning and ending ideas is adapted from their book. ■

Report on Tape Recorder Quality

Ernie Zellmer

JAARS Communications Department

INTRODUCTION

A recent article in *Notes on Linguistics*, *The Indispensable Tape Recorder* by Geoffrey Hunt, outlined the history of tape recorders and their use in gathering linguistic material. Also mentioned was the use of CECIL in analyzing such data.

Two of the primary problems experienced in using inexpensive machines ($60–$100) in language work is (1) inadequate frequency response and (2) inability to disable the ALC (automatic level control) feature.

FREQUENCY RESPONSE

JAARS Communications Department chose five monaural units for a frequency response check. They included three standard-size cassette recorders priced from $50 to $70 (Panasonic RQ 2309, Realistic CTR-69, Realistic CCR 81), a microcassette recorder (Pearlcorder S910), and a semi-professional recorder (Marantz PMD201) priced at $220.

Graphed responses are shown in Figure 1 and Figure 2.

Experience shows that with CECIL or other linguistic material gathering best results are obtained with a frequency response from 70 Hz to 8000 Hz. A normal rule of thumb, accepted in audio circles, states that a drop of 6 DB below a referenced response at 1000 Hz should be the lowest level acceptable in a recording.

In looking at Figure 1, which refers to two of these recorders, you will note that the Marantz PMD 201 shows an acceptable low frequency response to below 40 Hz, and a high frequency response to approximately 12 KHz. The Realistic CTR-69 is the best of the inexpensive recorders, giving a frequency response from 90 Hz to approximately 7.0 KHz.

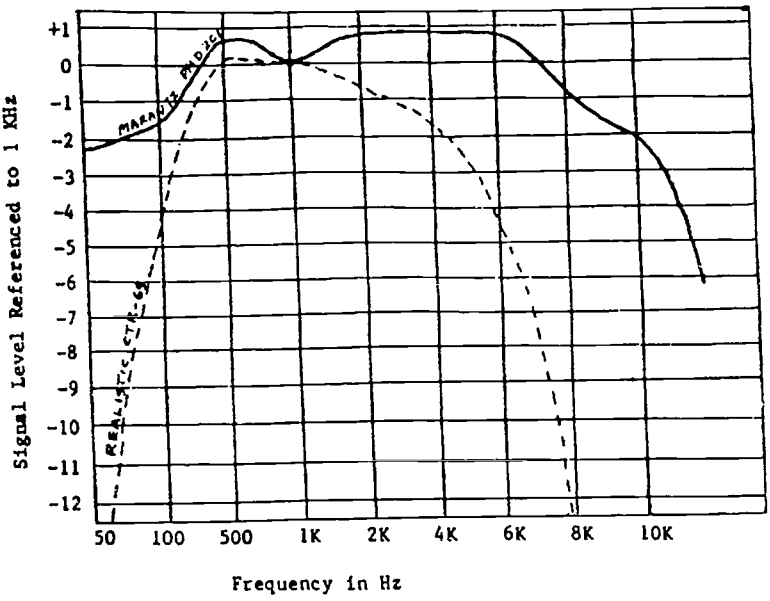


Figure 1.

The results in Figure 2, which are for the other three units, show that none of these machines will provide an adequate recording for linguistic purposes.

The Pearlcorder S910 microcassette, for example, is quite adequate for voice dictation. However, its 450 Hz to 6.0 KHz response is very inadequate for language needs. In general, our tests indicate that we cannot expect microcassettes to give an adequate frequency response.

MANUAL LEVEL RECORDING VERSUS AUTOMATIC LEVEL RECORDING (ALC)

The Realistic CTR-69 from Radio Shack has a MAN/ALC switch. It is very important, as indicated by the referenced article, that recordings for linguistic analysis be done in the Manual position, disabling the ALC. The CTR-69 is the only inexpensive machine that we have located with this feature.

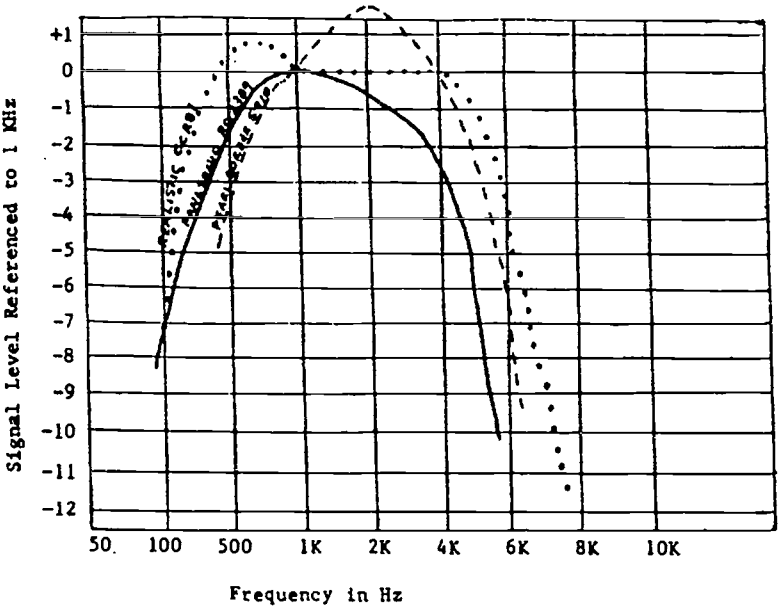


Figure 2.

CONCLUSION

Our test results show that the Realistic CTR-69 will do a fairly adequate job of recording for linguistic needs. It is marginal at the low frequency end, but this may be circumvented by not choosing a male subject with a deep bass voice for recording. The high frequency response, which is borderline, can be increased somewhat by turning the Tone control to maximum treble position in Playback.■

The 22nd Annual Conference on African Linguistics

Nairobi, Kenya
July 15–19, 1991

Rod Casali

The Annual Conference on African Linguistics in past years has always been held in the U.S. This year it was held in Africa for the first time. (Next year's ACAL is being planned for some place in California and the following year's for Yaounde, Cameroon.)

One consequence of holding the conference in Africa was that (according to people who had attended ACAL's before) the number of linguists attending from the U.S. was considerably smaller than in previous years. There were a number of very well-known linguists from the U.S. who had paper titles listed in the conference program but did not show up, possibly because they had not been able to obtain funding. On the positive side, the number of linguists attending from Africa was presumably greater than in past years, although (perhaps not surprisingly) the vast majority was from East Africa. Including myself, there were fewer than five people from West Africa. There was a good number attending from Europe and even a small contingent from South Africa.

The program included both plenary and parallel sessions. One of the most enthusiastically received of the plenary sessions was a lecture by Peter Ladefoged (UCLA) in which he discussed the use of microcomputers and other equipment for doing phonetics research in the field. Areas covered in the parallel sessions included syntax, morphology, phonology, and issues related to language policy. (Personally, I was surprised and a bit disappointed to find that there were only six papers devoted to phonetics or phonology.) I got a strong impression that, on the whole, there was a much higher interest in areas like language policy and language teaching among the African linguists, whereas the American and European linguists

tended to be mainly concerned with theoretical and/or descriptive issues.

The conference was well organized, although the fact that many papers which had originally been scheduled were not given meant that a lot of last minute changes had to be made. The facilities used for the conference were more than adequate.

Perhaps the biggest highlight of the conference for me personally was the chance to meet and interact with other linguists, especially SIL colleagues working in other parts of Africa. The experience of presenting a paper at this type of conference (a first for me) was also a very valuable one—I found it to be a lot easier and less threatening than I had expected. I heard similar comments from some of the other SIL members who attended. One person who did not present a paper said that he might have considered doing so if he had known how little was involved. He is now looking forward to presenting a paper at a future conference. Perhaps more people would present papers at conferences if they were first encouraged—or even helped financially—to attend one conference at which they did not have to do anything other than observe. ■

Linguistic Association of Great Britain Spring Meeting

Ronnie Sim

Kenya

The LAGB Spring Meeting was held in Brighton, England, 6–8 April 1992. The meetings fell into three organized parts:

1. A teach-in on HPSG led by Ivan Sag (Stanford), Bob Borsley (Bangor, Wales), and Richard Cooper (UCL), and a lecture by Sag entitled *A non-configurational theory of anaphor-binding.*
2. Three parallel sessions of conference papers.
3. A final half-day on the topic "Linguistics in Education."

I will deal with each in turn.

TEACH-IN ON HPSG

The teach-in is evidence of growing attention being given to modern monostratal (phrase structure only) approaches to syntax. Head-Driven Phrase Structure Grammar is one of a number of powerful rivals to transformational grammar, and Ivan Sag, along with Carl Pollard (Ohio State University), is one of its prominent proponents. Within the UK, Borsley has been a major figure. Cooper recently completed a PhD at Edinburgh on the semantics aspects of HPSG.

HPSG utilizes complex, organized lexical signs, somewhat reminiscent of those of Lexical Functional Grammar, but significantly more highly structured. Signs incorporate phonological, syntactic, and semantic information. They combine in a unification process, and all subcategorization is handled by part of the signs' feature structures. The semantics information structure is modelled on situation semantics.

Sag did a particularly good job. Volume 2 of Sag and Pollard's introduction to the model should be available from CSLI or University of Chicago Press by the end of this year.

SESSIONS OF CONFERENCE PAPERS

The following is a list of the papers given during the parallel sessions:

Michiko Takeuchi and Reiko Itani (Kanagawa, Japan)—*Utterance initial* but *and utterance medial* but: *A relevance based analysis*

Yael Ziv (Hebrew University)—*Relevance and causality*

Robyn Carston (UCL)—*Conjunction, explanation and relevance*

Liliane Haegeman (Geneva)—*Negative concord: Speculations on the typology of agreement*

Eric Haeberli (UCL) and Liliane Haegeman (Geneva)—*Old English word order: Evidence from negative concord*

Catrin Siân Rhys (Edinburgh)—*Negation and aspect in Chinese*

Mike Davenport (Durham)—*The characterisation of nasality in dependency phonology*

Máiri Ní Chiosáin (UC Dublin)—*"Empty" onsets in Irish: Evidence from palatalization and initial consonant mutations*

Iggy Roca (Essex)—*Paradigmatic stress*

Deirdre Wilson (UCL)—*Reference and relevance*

Tomoko Matsui (UCL)—*Bridging reference and the notions of "topic" and "focus"*

Linda Roberts (Manchester)—*Pseudo-adjuncts, applicatives and type raising*

Stella Markantonatou (Essex)—*Thematic adjuncts*

Richard Ogden (York)—*Phonetic exponency in YorkTalk: What are we actually doing?*

Lynne J. Cahill (Sussex)—*Combining a syllable based approach to morphology with the YorkTalk non-segmental approach to phonology*

Gerald Gazdar (Sussex)—*Paradigm function morphology in DATR*

Roger Evans (Sussex)—*Derivational morphology in DATR*

Max Wheeler (Sussex)—*On the hierarchy of naturalness principles in inflectional morphology*

Maggie Tallerman (Durham)—*A split-infl analysis of Welsh*

Peter A. Schreiber (Wisconsin-Madison)—*Branching direction and prehead modification*

Rita Manzini (UCL)—*Locality theory for movement and binding*

Elly Ifantidou (UCL)—*Parentheticals and relevance*

Vladimir Zegarac (UCL)—*The progressive and point of view*

Mimo Caenepeel (Edinburgh)—*A discourse level approach to the past perfect*

M. Staurou-Sifaki (Thessaloniki)—*Adjectives in Modern Greek: An instance of predication, or an old issue revisited*

Doug Arnold and Louisa Sadler (Essex)—*Adjectives in HPSG*

David Adger and Daniel P. Flickinger (Edinburgh)—*Relative clauses in HPSG without null heads*

Bob Borsley (Bangor) and Maria-Luisa Rivero (Ottowa)—*Clitic auxiliaries and incorporation in Polish*

Yoryia Agouraki (UCL)—*Clitic left dislocation and clitic doubling: A unification*

Anna Roussou (UCL)—*Complementation and the syntax of psych-verbs in Modern Greek*

Katarzyna Jaszczolt (Oxford)—*Beliefs* de dicto *and* de re: *Evidence from English and Polish*

Richard Coates (Sussex)—*Singular definite expressions with unique denotata and the boundaries of properhood*

Dick Hudson (UCL)—*The case against case*

Brit van Ooyen, James McQueen, and Anne Cutler (MRC APU, Cambridge)—*Lexical representation of regular versus irregular inflected forms: Evidence from spoken word recognition*

Bernadette Plunkett (Bangor)—*The continuity hypothesis and the landing site for Wh movement*

Judy Delin (Sussex)—*Why can't* It *be the focus of an* It-*cleft?*

Catrin Siân Rhys and David Adger (Edinburgh)—*Argument structure and the English gerund*

Siew-Yue Killingley (Grevatt and Grevatt)—*Linguistics in education: How the linguist can help*

Andrew Thomas (British Council)—*Linguistics and language awareness in language teacher education*

I attended the fairly full program of papers on issues related to Relevance Theory and other pragmatics approaches (Discourse Representation Theory). Relevance Theory was well represented, although it must be said that it is still heavily based on University

College, London. Papers tended to test out its handling of a number of issues otherwise called "discourse." It does have a lot of explanatory power, although I have been thinking ever since of two important critical points that were raised:

(a) *What would counter-evidence look like?* Currently I have no answer, although I suggest the critic has to show that an explanation of some piece of data in relevance terms is inferior in some way to an alternative.

(b) *There is sometimes an uncomfortable feeling that whatever "best explanation" is offered for some data is said to be just what relevance theory posits.* I hope to obtain a number of the full texts, and would be happy for interested parties to copy these.

The first student textbook on pragmatics written from the approach of Relevance Theory has just appeared:

Blakemore, Diane. Understanding utterances. Blackwell, 1992. Pp. 190. Large format paperback, £15 retail.

I brought several copies to Nairobi to try out this May term with students here. It looks clean!

LINGUISTICS IN EDUCATION

The following papers were given on this topic:

Mark Aronoff (SUNY, Stonybrook/LSA)—*The effect of linguistic research on the elementary school curriculum*

Mike Taylor (LINC (Language in the National Curriculum), Eastern Region)—*The LINC materials*

Katharine Perera (Manchester)—*"Educational Linguistics" with linguistics and with education*

David Graddol (Open University)—*What can linguists usefully say to teachers in the 1990s*

David Slee and John Hobson (Hatfield)—*Lexical and logical imprecision in legal language: Implications for automated systems*

Adam Kilgarriff (Sussex)—*Frequency, predictability and polysemy*

Carol Chapman (Manchester)—*A diachronic argument against the split morphology hypothesis: The case of analogical umlaut in German dialects*

Charles Prescott (Newcastle)—*Non-specification in Hungarian suffix harmony*

It turned out that a major focus was the new proposed national curriculum in England, so that the mindset concerned what teaching in tertiary education ought to be doing for future teachers of primary and secondary sectors. That was a narrower focus than I had hoped for, or expected, although it still had some useful comment to make. Mark Aronoff appeared here, comparing the difference in spelling and grammar books for American grade school, and deciding that the former were far more carefully constructed and more interestingly produced for two reasons:

(1) Research has been done on English spelling that shows a correlation between formal classroom effort and student progress; this is lacking on the grammar side.

(2) Linguistics doesn't seem so sure of what it might do in the classroom for the benefit of students on a grammatical level.

From the whole parasession, I think the major result on my own thinking was to wonder how the levels of creativity, product quality, and carefully planned development of materials to stimulate conscious awareness of students' language resources might be applied to SIL's literacy efforts. A mammoth undertaking!

As it turned out, the Linguistics in Education session was the most difficult to turn to practical use within SIL, and this was, of course, because of its local focus on national curriculum in England. The other two foci were of personal benefit, and recent ideas on pragmatics will feed into the degree programs here.

A final comment is in order, for the attention of the SIL/UK school. LAGB is a forum where members on study programs or home-based linguistic personnel could be encouraged to be more active. Although some knew Stephen Levinsohn from previous years, I was (as far as I know) the only SILer in attendance. ■

REVIEWS OF BOOKS

Comparative Kadai: Linguistic studies beyond Tai. Edited by Jerold A. Edmondson and David B. Solnit. Dallas, TX: SIL, 1988. Pp. 374.

Reviewed by Thomas M. Tehan

Payap University, Chiang Mai, Thailand and SIL Thailand

INTRODUCTION

"Kadai linguistics is something of a field apart, even in Southeast Asia. One enters the area rather like a gladiator entering an arena—fearfully" (p. 323). Here Benedict is referring to the complexities of the Kadai languages and their study. This book is an attempt by many "gladiators" to bring together some of the comparative and historical information available in this arena. On the whole, it succeeds well.

Mark Hansell also captures a guiding spirit of the book when he writes about his own paper (see article #12 below): "Large amounts of the relevant data are presented throughout the paper, so that those dissatisfied with the author's analysis will have the means to perform their own" (p. 239).

In the introduction, Edmondson and Solnit write,

> This volume is dedicated to presenting data and analyses about some of [the] less-well-studied cousins of Thai...not because we feel that KADAI-MINUS-TAI forms any coherent subgroup. The justification is that...there is still far more linguistic work in print on Tai—indeed on Thai—than on all the remaining languages of Kadai combined. (pp. 1–2)

Perhaps it would be useful at this point to orient the reader to the Kadai family as a whole. The linguistic map of southern China and Southeast Asia resembles a crazy-quilt layout of the Sino-Tibetan, Kadai, and Mon-Khmer language families which have intermixed and influenced each other linguistically. Kadai has well over 100 separate languages. Many Kadai languages have been placed into Tai and Kam-Sui divisions, with several languages having disputed positions in the language family tree. Tai includes languages as far north as

central China; its largest representative is Thai, the national language of Thailand. Kam-Sui languages are concentrated in China. The Kam-Sui division contains the Kam and Sui languages, which both have given their names to subdivisions within Kam-Sui.

SOME GENERAL ASPECTS OF THE BOOK

The book contains two parts (I. Kam-Sui proper, and II. Other Kadai) preceded by an Introduction and a section of maps of various areas in southern China. Part I, on the Kam-Sui subdivision of Tai-Kadai languages, has nine papers and covers 191 pages. Part II offers four papers on non-Tai, non-Kam-Sui languages in 121 pages. There are two indexes at the end. The first one indexes forms cited in the various papers. The forms are grouped by languages, and the index covers 30 pages. The second index references Authors, Languages, and Subjects in 4 pages. There are 374 pages total.

There is no collective bibliography. Instead, each paper lists its own references. The bibliography for the introduction, which must somewhat reflect the editors' competence, contains items in several languages and authors from several continents. Publishing dates cover the 1930's through the late 1980's.

Since this book is not a theory and practice book as such, it does not contain any exercises. However, there are many detailed language samples and reconstructions in the book that will challenge the student of linguistics to follow in detail. The reader is welcomed to take the ample data of the book and perform her own analyses.

As one flips through the book for the first time, one is impressed by the scores of tables that systematize phonemes and words within a language and among languages. There are also several branching tree diagrams depicting the relationships among the languages in the Tai-Kadai family.

The articles in the book regularly reference Li Fang Kuei and Paul K. Benedict as the most crucial scholars for Tai-Kadai studies. Other important scholars are Andre-Georges Haudricourt, James A. Matisoff, and William Gedney. Many of the contributors to this volume are leading Asian and Kadai scholars. Many Chinese scholars have contributed important descriptive data to the volume.

PARTS OF THE BOOK

The articles are well-written on the whole. The nature of the articles means that there will be many detail-filled paragraphs that are suitable for either skimming or working through in detail. A "normal" reading pace will either waste time or leave the reader bewildered (and sometimes bored) if she is reading for the big picture and general trends only.

(1) Introduction. One of the hardest tasks in beginning to understand the material presented in this book is mastering the terminology. There are a couple of approaches to describing tones and their historical development. The editors give a good introduction to what the different symbols (such as tone A1) mean for different scholars. However, for those new to the field, those paragraphs will have to be marked and flipped to often. The editors have provided a good conceptual map for us to refer to. Unfortunately, the scholarly and linguistic terrain is complex enough that even the map is difficult to understand.

(2) Yang Quan from the Central Institute of Nationalities (Beijing, China) in *Developmental Tendencies in Kam Phonology* aims to "elucidate [Kam] developmental tendencies, using materials [he had] personally gathered from different varieties of Kam and related languages" (p. 27). The chapter is a good example of dialect variation in southern China Kam languages. He discusses tone splits, which he relates to Chinese analysis studies, initial consonants, and vowel simplification in Kam.

(3) Zheng Guoqiao and Yang Quan from the Central Institute of Nationalities (Beijing, China) wrote *The Sounds of Rongjiang Kam*. It is an example of a description of one dialect of Kam and its adoption as the official dialect for orthography. There is some discussion of related dialects. "This paper elucidates the sound system of Zhanglu, but also introduces other varieties of Kam spoken in Rongjiang County" (p. 43).

(4) Shi Lin and Cui Jianxin from the Chinese Department, Nankai University (Tianjin, China) present *An Investigation of the Ai-Cham Language*. I appreciated the interesting geographical, historical, and cultural introduction to the article. The authors present the following interesting "quick and dirty test" (as it would be called in the

computer world) for historically and genetically classifying related languages. Li Fang Kuei's 1943 Kadai genetic affiliation chart is presented, and then a new one is presented based on that method. They choose just four words (in this case reflexes in several Kadai languages for *tongue*, *vegetable*, *soft*, and *sit*) for the main splits from Proto-Kadai. Then, moving down the genetic tree, they only employ enough words (generally two at a time) to differentiate further language relationships. One wonders if the tree could be completely of partially reshaped based on representative or non-representative word choices. "In summary, [they] have described here [their] preliminary research into the Ai-Cham language" (p. 84).

(5) Ni Dabai from the Central Institute of Nationalities (Beijing, China) discusses the *Yangfeng Mak of Libo County*. Again, this article is a good source of lexical and phonological data. Ni is a bit more ambitious than some in this volume in that he ventures a bit beyond phonology to talk about complex words (i.e., compounds), and then spends several pages discussing some phrase and sentence syntax. He ends with a comparison of this dialect of Mak to one described by Li.

(6) David Strecker in *Gedney's Puzzle in Kam-Sui* takes a problem documented in the Tai branch of Kadai and finds a similar problem in Kam-Sui, but with different vowels in different environments. He thus provides additional support for a reconstruction of polysyllabic words in Proto-Tai and Proto-Kam-Sui as opposed to the earlier monosyllabic reconstruction of Proto-Tai by Li.

(7) Wang Dewen from the Central Institute of Nationalities (Beijing, China) in *A Comparative Study of Kam and Sui Initial Consonants* presents generalizations on related languages in Kam-Sui and discusses implications for reconstructions of proto-forms. The article contains good data on initial consonants. Kam has only twenty-odd initials; however, Sui has over seventy. As with several of these descriptions of languages (as opposed to the larger-view theoretical discussions), there is a limited bibliography.

(8) Jerold A. Edmondson from the University of Texas at Arlington and Yang Quan from the Central Institute of Nationalities (Beijing, China) in *Word-initial Preconsonants and the History of Kam-Sui Resonant Initials and Tones* offer an article that is good and is fun historical comparative linguistics. They discuss the word and tone structure of Proto-Kam-Sui and its implications for Li's theory of

single syllable roots in Kadai and Benedict's theory of multisyllabic Kadai roots. An extensive bibliography is included at the end.

(9) Zheng Guoqiao from the Central Institute of Nationalities (Beijing, China) in *The Influences of Han on the Mulam Language* discusses some interesting phonological borrowings. He also groups the Mulam Lexicon into semantic domains and presents some word order analysis to begin a syntactic description. In addition, he shows example sentences to illustrate the variation between Han (Chinese) and native Kadai word order. In some cases, the Kadai order is no longer used in Mulam. Again, this is another paper with lots of good data. He maintains that Mulam is one of the Kam-Sui languages most deeply influenced by Han (p. 167). One count of common Mulam words found that 59% were borrowed Han words.

(10) Graham Thurgood from the California State University, Fresno and Universiti Pertanian Malaysia in *Notes on the Reconstruction of Proto-Kam-Sui* presents a preliminary reconstruction of Proto-Kam-Sui (PKS). There are lots of good data, a good bibliography, a genetic classification language diagram, and an extensive appendix. The appendix presents a summary chart of many words in seven languages and English glosses with the proposed PKS words. He gives the degree of certainty he assigns to individual reconstructions, and he states when no explanation can cover the existing data.

(11) David B. Solnit from the University of Michigan examines *The Position of Lakkia Within Kadai* in the first "Other Kadai" article in Part II of the book. He begins with a good, short overview of historical scholarship, i.e., who wrote what, when, about what. This section of his article is history of linguistics as opposed to historical linguistics. He also has a short discussion on principles of subgrouping languages. After a thorough discussion of Lakkia's position within Kadai, he concludes by continuing to group Tai, Be, Lakkia, and Kam-Sui (KS) together into a Kam-Tai family, suggesting that Lakkia is closer to KS but not within KS.

(12) Mark Hansell from the University of California, Berkeley examines another language's genetic position in *The Relation of Be to Tai: Evidence from Tones and Initials*. He uses many Proto-Tai reconstructions in his detailed discussion of many correspondence pairs with Be. He concludes that Be is in a class by itself and "that

the divergence of Be from Tai is centuries old, and quite profound" (p. 284).

(13) James A. Matisoff from the University of California, Berkeley wrote *Proto-Hlai Initials and Tones: A First Approximation*. He compares nine dialects of Hlai with each other, as well as citing forms from Proto-Tai, Proto-Tibeto-Burman, and Siamese Thai. He has amassed much good data with good analysis. However, even with his title caveat of "A First Approximation," I wished for some speculation of the relationship of Hlai to other Kadai languages. But Matisoff offers no comment on Hlai's place within the Kadai stammbaum.

(14) The last article is from one of the senior scholars in Asian linguistics. Paul K. Benedict proposes *Kadai Linguistics: The Rules of Engagement*. He summarizes Proto-Austro-Tai characteristics and begins with a historical summary of Asian languages as he sees the big picture. When he discusses the big picture, one catches the scope and excitement of Asian linguistics. He is able to relate Chinese, Tibeto-Burman, Japanese, Oceanic, and Austronesian examples to Kadai words. He also brings a wealth of detail to discussion of individual words. These paragraphs are often terse with condensed etymological data and enough Chinese to lose the average reader. On the whole, he does a good job of drawing together an understandable overall picture of Kadai linguistics.

SUMMARY

As a beginning student of Thai, I was amazed at how many cognates I recognized in word lists from the branches of Kadai more distant from Thai. Students who struggle through the tones of Thai (5 in one way of counting; 8 in another way of counting) or Chinese should be thankful they're not mastering the 15 tones of Kam. In reading this book, a student of Kadai or Tai languages can come away knowing the important scholars in the field and what they have contributed. The several bibliographies will give the student of these language groups many titles to pursue.

The whole book represents careful scholarship and sound methodology. A great deal of data is gathered together, so the book will be useful to many who are studying Asian languages or tone

languages. The word lists in many dialects, languages, and reconstructed proto-languages will be invaluable to those who are working in Tai, Kadai, or other languages that are potentially related to the Kadai languages.

So who will enjoy and profit from reading this book? Anyone who is curious about where the modern Thai, Tai, and Kam-Sui languages come from, and any historical-comparative linguist working in Asian/Pacific languages or tone languages. AND anyone who enjoys the excitement of the linguistic arena! ■

Lunatic lovers of language: Imaginary languages and their inventors. By Marina Yaguello. London: Athlone, 1991. Pp. xviii, 223. $39.50.

Reviewed by Alan C. Wares

Inventors of languages are more numerous than one might suppose. The biblical account of Adam is considered mythical by this author, as is the story of the tower of Babel where the primitive language of mankind was confused (pp. 10–11). Although Plato, in his *Cratylus*, presented some notions concerning the supposed evolution of the Greek language (pp. 131–132), it wasn't until the early 17th century that philosophers and writers of fiction began to consider devising a universal language that would unite all mankind and thus avoid misunderstandings among nations.

John Wilkins, a member of the Royal Society in England, was one of the early advocates of a universal philosophical language. Descartes of France, Leibnitz in Germany, and Comenius in Moravia were among other notable 17th-century scholars whose works dealt with human communication, especially with unknown societies on the other side of the world, or even with inhabitants of the moon, as in the tales of Cyrano de Bergerac (pp. 32–34).

Late in the 18th century, Sir William Jones's *Discourse on Sanskrit* initiated the study of Comparative Philology and an attempt to arrive at the language of ancient Indo-European by a scientific study of sound changes (p. 46). Bopp, Schlegel, and the Grimm brothers

made some very astute observations that gave the study its scientific basis, unlike the twaddle of certain other writers, examples of whose works may be read in the appendix to this book.

During the 19th century, the search for the "primitive" language of mankind seemed to make headway with attempts to reconstruct Proto-Indo-European. In addition, there were efforts to devise a "universal language" that everyone would learn and speak, thus overcoming the barriers of natural language (pp. 188–193). Probably the best known and the most widely spoken of these is Esperanto, first devised by the Russian, L. Zamenhof (1897). This has been revised numerous times, so that now there are several dozen dialects of the language.

Mystics as well as scientists began to take an interest in languages, particularly in the phenomenon of glossolalia, or speaking in tongues. A century earlier, Swedenborg claimed to have spoken in the languages of Jupiter and Mars (p. 181). A French medium, Hélène Smith, claimed to have held conversations with inhabitants of Mars (p. 83). Oddly enough, the Martian language as she spoke it bore many striking resemblances to French.

Although the author claims she never reads the Bible, she refers to the religious glossolalia of the Pentecostal movement, which she identifies with what the Apostle Paul describes as taking place in the church at Corinth (p. xii), and carefully distinguishes it from the xenoglossia, or speaking in foreign languages on the Day of Pentecost in Acts 2 (p. 13).

Several names stand out among the 20th-century linguists: Saussure, Sapir, Whorf, Bloomfield, and Chomsky. Of course, there are a host of others whom the author either chose to ignore or else was not familiar with. Alongside of the writings of serious linguists are works of fiction (or science fiction) that include invented languages.

The author devotes an entire chapter to the Georgian, Nicolas Marr (1863–1933), who represented "a continuation of the linguistic fantasy which finds its roots in myth and its justification in utopian schemes" (p. 65). Marr's father was an elderly Scot who married a Georgian and died when his son was eight. The younger Marr was a misfit in society (labeled English at school, even though he spoke Georgian as his mother tongue) whose peculiar notions about language were

adopted as the official Soviet language policy during the regime of fellow Georgian, Josef Stalin. The utopian dreams of Marr's linguistic theories hampered genuine linguistic study in the Soviet Union for decades, even after Marr's death. Extracts from his writings are included in the appendix (pp. 172–178).

The book is half text, half appendix, with four parts to the former and two to the latter. *Part One: From Myth to Utopia* (pp. 3–28) is for the most part historical. *Part Two: Down the Stream of Time* (pp. 29–62) continues the historical account to the present. *Part Three: At the Two Poles of Linguistic Fantasy* (pp. 63–109) recounts the story of Nicolas Marr and the influence of his theories on Soviet language policy in the '40s and '50s, and the story of Hélène Smith and her claims of contact with the spirit world. *Part Four: In Defence of Natural Languages* (pp. 111–123) refers to the Sapir-Whorf hypothesis and touches lightly on semantics. Appendix 1 (pp. 125–127) lists landmarks in the history of ideas on languages, beginning with Galileo (1610) up to the era of Chomsky and his *Aspects of the Theory of Syntax*. Appendix 2 (pp. 128–193) consists of a score or more of selected texts pertaining to language. There are also 15 pages of end notes (pp. 194–208), an 8-page bibliography, and an Index.

The author of this work (originally published in 1984 as *Les Fous du Langage: Des Langues Imaginaires et de Leurs Inventeurs*) is a French linguist at the University of Dakar in Senegal. She mentions two radically opposed approaches to language:

> On the one hand, an intellect, a rational, analytic, and logical understanding, a utopian-constructive one which aims to organise the world, and is masculine in essence. On the other, a grasp that is intuitive, instinctive, spontaneous, globalising, sensual, primitive-infantile, fanciful, subject to hidden drives, in short hysterical, all of which are defining characteristics of women, children and lunatics (p. 24).

As fascinating and as informative as this book is, there are several areas of invented languages that are barely mentioned in passing, such as the languages of the underworld, understandable only to those in the "inner circle." Another is the arcane area of computer languages—not spoken, it is true, but probably today's most widely used means of human communication. A third area of invented languages, also not spoken, is the sign language of the deaf—not a

universal language by any means, but one like Esperanto, with many varieties.

The author's enthusiasm for her subject is evident as she describes the research she has done in invented languages. She has a penchant for referring to subjects before she gets to them, and for making generalizations such as that lunatic lovers of language are always men and that glossalalists are always women, but these hardly detract from the scholarly and informative nature of her work. ■

Language in Australia. Edited by Suzanne Romaine. Cambridge: Cambridge University Press, 1991. Pp. xviii, 415. $79.50.

Reviewed by Bruce Hooley

This is a comprehensive study of the language situation in Australia. It is an excellent overview, and functions as a companion to two somewhat similar volumes, *Language in the USA* and *Language in the British Isles*. The book comprises a series of papers by scholars having an interest in Australia and Australian languages, with an introduction by the editor, Suzanne Romaine. There are overviews and historical data, along with one or two brief linguistic sketches, notably of Kalaw Kawaw Ya, Torres Strait Creole, and Kriol, but the main thrust of the book is sociolinguistic in nature.

The complex linguistic situation in Australia is reflected in the five sections into which the book is divided. In her introduction, the editor aims

> to provide a sociohistoric background to the evolution of the major varieties of language now found in the Australian continent. One of the most interesting developments I attempt to trace is how a new ideology of pluralism arose in the 1970s in response to social and political changes. This was in direct opposition to the earlier 'White Australia' policy, which projected an image of an ideal Australia which was monocultural, monolingual and monoracial (p. 1).

The five sections into which the main part of the book is divided are:

Aboriginal and Islander Languages—spoken by the people living here when the Europeans arrived.
Pidgins and creoles—the contact languages that developed between indigenes and immigrants.
Transplanted languages other than English—languages brought by other immigrant groups.
Varieties of Australian English—local developments and modifications of received English.
Public policy and social issues—the development of language policy, past, present, and future.

ABORIGINAL AND ISLANDER LANGUAGES

There are eight papers in this section, the largest. They range from an overview of the indigenous languages, with some broad indications of structure, to considerations of social dialects, child language development, and the development and variety of Aboriginal English. The last paper in the section deals with questions of language death and language maintenance, and what can be done to prevent the loss of the remaining aboriginal languages of Australia.

As Walsh indicates in the first of these papers, when the first European settlers arrived, there was a total of approximately 250 languages spoken by the aboriginal peoples. There were thought to have been at least about 300,000 speakers of these languages, but more recent estimates put that figure up around 900,000. The current reality is that there are about 70 languages with viable communities of speakers left, and only about 25 of these have populations of 250 or more. There are some 60 other languages no longer considered viable, being represented by only a few speakers. This situation has arisen because of pressure from the dominant immigrant culture. Aboriginal peoples were frequently treated as less than human, and until recently official educational policy was English only.

Australian aboriginal languages form a linguistic isolate, and all derive from a common ancestor. The major generally accepted classification is into the Pama-Nyungan family which covers four fifths of the continent, and a diverse group comprising perhaps up to 20 families covering the rest of the country. In the second paper, Sutton takes up questions of multilingualism, and of social and geographical dialects. The complexity he deals with has been exacerbated by the government's earlier policy of forcing groups,

often comprising people of several different languages, to live together in one settlement.

Kaldor and Malcolm take up the question of Aboriginal English, which has only recently been studied seriously. They point out that there are many different varieties occurring all over the continent. These varieties have a complex history of development, distinguishable from the creoles, since they are mostly the product of a formal learning process and show more variation among themselves than is displayed by creoles. The authors do suggest a continuum between English and the creoles, setting up four groups which they call urban/metropolitan, northern and desert, northern creole speaking communities, and interlanguage varieties in bilingual settings. They go on to discuss the implications of Aboriginal English for language planning and policy. The next papers, by Eades and by Koch, go on to consider the ways in which various dialects of Aboriginal English are used in communication and social interaction, and the problems faced by the courts in dealing with land claims.

Bavin and Shopen in turn examine the acquisition of Warlpiri, one of the most virile of the Aboriginal languages, by children. They note changes in pronunciation, and a tendency to overgeneralize in several aspects of the grammar. The children have also introduced some innovations of their own. The authors assert that some of these changes are being retained by the children as they grow up, leading to overall changes in the language itself.

Ford and Ober give a brief sketch of the Kalaw Kawaw Ya language from Cape York Peninsula, and the last article in this section, by McConvell, tackles the how and why of language shift, and how our knowledge might be applied to the maintenance of the Aboriginal languages that are still viable. Although he admits that solidarity with or distance from certain social groups is the most important function of language choice in bilingual situations, he believes the social and cultural factors are also very important. On the social side, a particular language is chosen to identify the speaker and/or the listener with a particular social group. On the cultural side, the choice of language carries with it a particular, and often contrasting, world view. He claims that the complexity or difficulty of learning a particular language may also affect the probability of its survival.

PIDGINS AND CREOLES

There are five papers in this section. Mühlhäusler first presents an overview of the use of pidgins and creoles in Australia, including a sketch of their historical development. In the second paper, he discusses Queensland Kanaka English, describing its origin and demise and drawing some theoretical implications.

In her paper, Shnukal describes the history of Torres Strait Creole and outlines its structure. Originally, she says, this language had a certain amount of prestige because it was believed to be English. Since then it has gone through a cycle of rejection, and is now being rediscovered and revalued by younger Islanders who see it as a mark of their identity. Harris in the next paper tells of the development of the present day Kriol language from varieties of pidgin. Also looked down on until recent years, Kriol has become widely understood and used across northern Australia, with large numbers of second language speakers using it in addition to those for whom it is their first. It is now rapidly becoming accepted as a language of community identity and political action. The last paper in this section is by Sandefur who presents a sketch of the structure of Kriol, including reference to the variation in usage which exists in various parts of northern Australia.

TRANSPLANTED LANGUAGES OTHER THAN ENGLISH

It is not possible in a book of this size to cover effectively the multiplicity of languages which have come to Australia by migration, especially since the end of World War II, so a selection had to be made from current scholars and research activities. The first article is by Clyne with an overview of the way the current situation has developed. There are 75–100 immigrant or community languages in Australia, depending on one's definition of language. These are now accepted as part of multicultural Australia, with over 13% of the population claiming to use a language other than English at home. Fourteen of these languages have more than 40,000 speakers in the country.

When the mass immigration began in 1947, those formulating policy assumed that Australia would remain a monolingual country, and that the newcomers would learn English and be absorbed into the

Australian community. The whole migration program was set up with this in mind, and it was some time before it was realized that things weren't happening in quite the way intended. Since the '70s there has been a radical shift in attitude, resulting in a widespread acceptance of multiculturalism and multilingualism, and giving a whole new ethos to the country. At the same time, there is no community language which does not show some degree of English influence. Just how much varies considerably, and some languages are already in an advanced stage of shift. Clyne also discusses current research and outlines some of the gaps in our knowledge which need to be filled.

The remaining papers in this section (by Pauwels, Clyne, Tamis, Bettoni, and Doucet) do describe various aspects of the research that is going on. The languages represented are Dutch, German, Greek, Italian, and, for Queensland, Serbo-Croatian. Of these, Dutch is the most assimilated, apparently because the Dutch do not consider language to be a core value in their cultural system, and so they are not greatly concerned about maintaining it. On the other hand, Greek, the second most widely used community language in Australia, shows the strongest pattern of maintenance. This is attributed to the fact that for the Greeks their language is inseparable from their cultural ethos, but also in part because it is structurally further removed from English, and therefore it is probably more difficult for Greek speakers to learn English. Greek is also the most popular of all the community languages in terms of the availability of secondary and tertiary level courses. Although people of Italian background form the largest immigrant group in Australia with over 400,000 speakers, the shift to English is progressing rapidly. One reason for this seems to be that the language consists of many popular and regional dialects with standard Italian already a second language for most people.

VARIETIES OF AUSTRALIAN ENGLISH

In recent years, Australian English has become an object of study in its own right, and three papers are devoted to it in this volume. The first, by Bryant, is a study of regional usage of particular lexical items and describes a project aimed at producing a dialectal geography type survey of the whole country. Bryant describes some interesting innovative methodology forced upon her by constraints of

time and money. The survey itself deals only with lexical items, and it is being conducted by one person only rather than by a team of researchers. One of the innovations relates to the fact that, since the project was located in Canberra, a center that draws its population largely from elsewhere in Australia, a large amount of the data was collected there from people who had moved from interstate. Another is the use of pictorial rather than textual questionnaires. There is not as much regional variation in Australia as in some other English speaking countries, but neither is the speech community as homogeneous as has been asserted in the past, so that there is enough variation to make the study worthwhile and interesting. Only preliminary results are available to date.

The other two papers, by Horvath and Pauwels respectively, deal with the level at which migrants enter the English speech community, and the affect that gender has on Australian English usage.

PUBLIC POLICY AND SOCIAL ISSUES

The three papers in the final section of the book deal with quite diverse topics. The first article by Ozolins summarizes the history and development of language policy in Australia. Australia is in fact unique among English speaking countries by virtue of now having an official National Language Policy. Ozolins describes the radical switch in attitude which has taken place over the past few decades, moving from an emphasis on a monolingual white society to a multicultural, multilingual view. The White Australia Policy, which was used to discriminate against immigrants considered undesirable, was in force right up into the 60's. The changes which have taken place in attitude and policy since then are summarized well in this article, including such things as the development of ethnic schools, language and interpreting services, ethnic radio and TV, and the maintenance and development of languages other than English.

The second paper by Corson dealing with social class differences in the lexicon might just as easily have gone in the preceding section on Australian English, except that it does address some social problems. It shows that children of working class families, but not of migrant families, are disadvantaged in the present school system, and calls for curricular and organizational changes to redress the balance.

The final paper by Eagleson called *Plain English: Some Sociolinguistic Revelations* is one of considerable interest and might be read with profit by professionals of all kinds, even linguists! In 1984 the Australian Government declared a policy of plain English in official documents. Some non-governmental services such as insurance companies have also picked up the idea with profit. The idea is a viable option but has not been received with universal joy by those responsible. Eagleson demonstrates that some of the resistance to adopting this practice on the part of legislative drafters and others is unjustified, being based on adherence to tradition rather than on any inherent difficulties. In fact, he points out, in using the traditional officialese, these people "are at times saying not what they intend, but the sheer convolution has hidden the error."

FINAL NOTE

Given my Australian nationality, this book was of particular interest to me, since it provides a valuable overview of the linguistic picture in Australia. It includes helpful historical material, a good summary of the current situation, and an indication of the kind of research that is being carried out. The linguistic scene in Australia may not have much direct relevance to most us, but, given the fact that many of us live in countries with equally complex linguistic situations, it is enlightening to see how other people have tackled their language problems. Questions concerning the viability of languages, and what happens when languages come in contact are of considerable interest to us, and the last word has not been said on these topics yet. ■

When verbs collide: Papers from the 1990 Ohio State Mini-Conference on Serial Verbs. (Working Papers in Linguistics, 39.) Edited by Brian D. Joseph and Arnold M. Zwicky. Columbus, Ohio: Ohio State University, 1990. Pp. 366. Paperback.

Reviewed by Mike Cahill

1 INTRODUCTION

For those of you who have thought of "serial verbs" as solely an African phenomenon, this collection of eighteen papers may be a real eye opener, covering as it does such diverse tongues as Mandarin Chinese, Marathi, Walapai, Korean, Arabic, English, and others!

Since a verb can only be considered "serial" when in combination with another verb or verbs, the phenomenon is more properly termed a "serial verb construction" (SVC). The example that at least half the authors cited was from Sranan, a Caribbean Creole:

Kofi naki Amba kiri
Kofi hit Amba kill
"Kofi struck Amba dead"

What makes this an SVC, roughly, is that two full verbs in the same clause have the same subject (and object, in this case) and refer to a single action, but with no conjunction or complementizer between them.

Interest in SVCs has been sparked in recent years by a couple of things. One is the increased study of Creole languages and the realization that SVCs are common in them and not limited to African languages. One author notes that SVCs are also common in India, Southeast Asia, the Far East, Papua New Guinea, and "perhaps elsewhere."

Another stimulus to the study of SVCs was the rise of the formal Chomskyan approach to syntax and the realization that within it there was no obvious good analysis of serial verbs. Thus, SVCs serve as one point to test the adequacy of theoretical models. In connection with this was the eventual appearance of influential publications by Sebba (1987) and Baker (1989). These were cited by almost every contributor to this volume, who used these works as a starting point.

The papers in this volume were all given at a mini-conference at Ohio State University in 1990. Two main types of papers were evident—those which described SVCs in a particular language and attempted to account for them in terms of theoretical structures, and those which sought after a universal definition of SVCs.

2 THE ISSUES

The first thing that struck me after reading a few articles was that there is no general agreement on a precise definition of SVCs, though almost everyone would agree that they are two or more contiguous verb phrases in the same clause which agree in tense, aspect, mood, and polarity, with no conjunction or complementizer between. Even the criterion of same subject was not universally accepted.

Thus, one of the major issues running throughout the book is the precise definition and delimitation of an SVC. The issue is complicated by the fact that SVCs are not the only verb constructions which can be contiguous. Various coordinate and subordinate structures must be considered, and useful and valid criteria must be proposed for differentiating them from the true serial verb construction. And, of course, each author has his own criteria. For example, there is disagreement on whether constructions with *come* and *go* in English (*go get the pencil*) are true SVCs or not. Seuren (who claims to be able to define SVCs universally) and some others want to exclude English *come/go* constructions, noting that no one has ever thought of English and other European languages as serial verb languages; his definition of SVCs rules them out. On the other hand, Pullum, whose paper focuses exclusively on English, has a definition of SVCs that would include the English *come/go*. Several other contributors take one side or the other on this issue. After encountering quite a few claims to define SVCs universally, it was somewhat refreshing to read Hussein's assertion that "several descriptions of SVCs have demonstrated that there is no single universal criterion which can exclusively define them cross-linguistically" (p. 341)!

Another issue somewhat related to the first is what the grammatical structure of SVCs is. Various tree structures are proposed. some purportedly universal and others language-specific. Some argue that

all SVCs have the same structure, while others maintain the existence of several types of SVCs, each with its own structure.

3 THE ARTICLES

The articles were generally of very good quality. Here is a sketch of the contents.

Articles by Zwicky, Seuren, Schiller, and Mufwene are primarily theoretical, questing for the Holy Grail of the perfect definition of SVCs. Welker shows how SVCs would be handled in categorial grammar.

Two articles have a historical twist. Byrne considers the question of whether Caribbean Creoles have SVCs as a result of an African (Kwa) substratum or as inherent language properties. Quinn suggests that Classical Japanese derivational affixes were serial verbs in an earlier form of Japanese.

Riddle, Joseph, Winford, Pandharipande, Nagarajan, Pullum, Redden, Jo, and Hussein have articles that center on Hmong, Modern Greek, Caribbean Creoles, Marathi, Tamil, English, Walapai, Korean, and colloquial Arabic, respectively. All tie their language-specific observations into the debate on the definition and structure of SVCs. Some points of interest: Joseph, after applying a number of tests to various candidate structures, concludes that modern Greek in fact has no SVCs; the SVCs of the Indian languages Marathi and Tamil are very much different from those of Africa; and Pullum finds clear criteria to distinguish the *go get* from the *go and get* construction (e.g., **I went got the paper* versus *I went and got the paper*).

Chang's and Dai's articles are also language-specific and consider the same language—Mandarin Chinese. Both distinguish SVCs from coordinate verb structures (using different criteria!) and agree that previous analyses were inadequate, but they differ quite a lot in approach and conclusions.

4 THE VALUE OF THE BOOK

When Verbs Collide can be a valuable resource for those who have SVCs in the languages they are studying. If you are fuzzy on exactly

what a serial verb is, you will get a much better idea after reading a few of these articles (the disagreements over precise definitions are much smaller than the areas of agreement). The articles may give you ideas for analysis of SVCs in your language, as well as giving you the capacity to recognize them in the first place. There are many examples of how to test candidates for "SVC-hood," some of which can surely be applied in a language you are working with (learn about the Coordinate Structure Constraint!). There are schemes of classifying verb concatenations and distinguishing them from each other and from SVCs (this itself could be a workable paper in many languages).

When Verbs Collide represents current ideas of scholars working in the field, and how they are expressed. It is a snapshot in time of what issues are important to at least some scholars. It is a good example of how many of these scholars argue their points and the kind of data they use to try to do it. For example, most of the arguments on definition used data from Akan, Sranan, and Saramaccan. There is a historical link between Akan as a Kwa language and Sranan and Saramaccan as Caribbean Creoles, so the Kwa substratum is the one dominating the discussion these days. (Remember *Kofi naki Amba kiri* "Kofi struck Amba dead" from Sranan!)

We in SIL have an opportunity here to contribute data papers on SVCs to widen the scope of the discussion. If the papers in this volume strike you as missing some vital point, write that up, too!

When Verbs Collide is not an easy book to read, especially if your grammatical theory is rusty. A background in Government and Binding and also Generalized Phrase Structure Grammar is necessary to follow the structural formulations and arguments in detail, but those without such background (such as me!) can skip over the heavy theoretical parts and still get considerable benefit from the book. For those just beginning work on verbal structures, Wiesemann's (1989) article would probably be a better point of entry.

The stated desire of the editors was "to stimulate discussion on current research, not to archive the research of years gone by." Thus, the papers were not edited, but printed as the authors submitted their camera-ready copy. As a result, there are a variety of type styles, sizes (all half-size of original pages), spacing, and even darknesses represented. The varying type fonts and especially the small print

necessitate a good light source and your reading glasses. There are a few typos, especially in references. Some works referred to in the text of a couple articles are altogether missing in the bibliographies.

The price of the book is not marked, but from listed prices of other recent Occasional Papers from Ohio State, I would guess it to be not much more than $10. At that price, it is a bargain for those interested in serial verbs—much cheaper than the first two references at the end of this review!

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Syntax: A functional-typological introduction. Volume I. By Talmy Givón. Amsterdam/Philadelphia: John Benjamins Publishing Company, 1984. Pp. xx, 464. Paper $24.95.

Syntax: A functional-typological introduction. Volume II. By Talmy Givón. Amsterdam/Philadelphia: John Benjamins Publishing Company, 1991. Pp. xxv, 553. Paper $24.95.

Reviewed by Charles Peck

Waxhaw

The second volume of Givón's *Syntax* has just recently been released. It is a continuation of Volume I published in 1984.

To indicate the contents, I will quote his own summaries and chapter titles:

Volume I:

Chapter 1: philosophical and historical preliminaries.
Chapter 2: methodological preliminaries to the functional-typological approach to syntax.
Chapter 3: a sketch of lexical categories ("word classes") and lexical semantics.
Chapter 4: semantic structure of propositions, predication and case-roles.
Chapter 5: morpho-syntactic typology of case-marking systems.
Chapter 6: word-order typology.
Chapter 7: necessary prerequisites and basic information-theory concepts for the study of discourse-pragmatics, including propositional modalities.
Chapter 8: tense-aspect-modality systems from semantic, pragmatic and morpho-syntactic points of view.
Chapter 9: negation and its semantics.
Chapter 10: pronouns, demonstratives and grammatical agreement.
Chapter 11: nominal modalities, reference and definiteness.

Volume II:

Chapter 12: noun phrases.
Chapter 13: verbal complements.
Chapter 14: voice and de-transitivization.
Chapter 15: relative clauses.
Chapter 16: contrastive focus constructions.
Chapter 17: marked-topic constructions.
Chapter 18: non-declarative speech-acts.
Chapter 19: interclausal coherence—subordinate clauses.
Chapter 20: grammar of referential coherence: a cognitive reinterpretation—topicality.
Chapter 21: markedness and iconicity in syntax.

Givón treats all these constructions from semantic, pragmatic, and morpho-syntactic points of view.

The name of this theory is the "Functional Typological Syntax." In this theory, one begins with a typological discussion—a discussion of the order of constituents in a construction and any variations possible. Do the verbs have prefixes or suffixes? Do the modifiers precede or follow the noun? And so on. This discussion of typology gives the reader a quick way to compare the language being described with any other languages the reader knows about. The down side of this discussion is that it seems somewhat redundant with the discussion that follows.

The second part of the presentation is a discussion of the "functions" of the constituents in the construction. What do the various constituents add to the construction? The discussion of functions is good.

In my workshops, I have tried to get translators to write more about the constructions they were describing, but it has proven to be a difficult task. In field workshops, most OWLs in the beginning of their projects find that isolating a construction is an achievement; writing about the functions of the constituents is just another burden. But when people have written more about the constructions, their papers are much more interesting.

Givón can give lots of discussion because he brings in all sorts of examples from other languages, which makes his books good encyclopedic surveys of grammatical constructions.

Givón's discussion, however, does not extend to the external distribution or uses of the constructions, not until he is talking about markedness and topic structures near the end of the second volume.

Givón's theory cannot see paragraphs or discourse structures. It can only talk about topic accessibility and topic persistence, using counting and statistics, in narrative discourse. If Givón could write a third volume on paragraphs and discourses, he would have a complete theory.

The books are only moderately hard to read. The paragraphs do not have good structure, but, mercifully, most paragraphs are very short. Examples make up at least half the pages in the book, which makes the reading easier and more interesting.

Givón's two volumes will join the list of books I plan to use in my consultation. The other books are Comrie's *Aspect*, *Tense*, and *Language Universals*, Longacre's *Discourse Grammar*, my own *Survey of Grammatical Structures*, and Shopen's three volumes.

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NOTES ON *LINGUISTICS*

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NOTES ON LINGUISTICS

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COORDINATOR'S CORNER

The rate of rejection of linguistic papers by major journals is as high as 90%. Don't get discouraged if your efforts are turned down! Not everyone's ideas match the parameters that may be set, so we welcome the second part of T. Tehan's *Guidelines for Writing Linguistic Papers*. Compare them with other treatises on this topic as noted at the end of his article.

Sue Wright's article, *Terminal 2/2+ in Language Learning*, deals with an interesting aspect of language acquisition: the matter of not only freezing at a particular level, but being TERMINALLY stuck there. Sue contends that contrary to what published authorities say, the condition is remediable. Since all field teams are interested in making continued progress, the prospect of freeing one's self from being a permanent Level 2 should be extendable to freeing one's self from remaining at any level less than what one would desire.

Mike Cahill in *A Few Practical Tips for Using CECIL* does a service that is needed. As experience with the use of CECIL, or should I say CECIL-Grams, on the field is widened, it is important to share findings with this important tool for linguistic analysis. With it we can not only come to conclusions more rapidly and reliably in complex cases, but we can print the facts that hitherto we had to claim were detected auditorily.

The number of books that we have received from publishers and sent out for review is gratifying. Sometimes when readers request a particular book, we find it has already been sent to someone whose request arrived sooner. Since no alternative or broader area of interest has been indicated, other publications cannot be substituted. If we can interest you in a different choice than the one first listed, do indicate your area of interest.

The Linguistics Consultant Training Course offered at SIL-UND by Austin Hale is reported to have been very successful. The only added component that we might wish for would be an on-field workshop in which to offer the same or a similar training. Let Bob Dooley (at this address) know if you have a likely situation.

—Eugene Loos

Some Guidelines for Writing Linguistic Survey Papers

Part II: Questions to use in writing up survey/feasibility studies

Thomas M. Tehan

Payap University, Chiang Mai, Thailand and SIL Thailand

1 INTRODUCTION*

This article consists of questions designed to stimulate thinking in conducting and writing up a language survey. Some of the questions overlap; different phrasing will stimulate different perspectives, insights, and responses. More than likely, not all questions will be answerable, especially from early reports. Some of the questions you will need to answer for yourself, but they should not necessarily appear in the paper you are writing at the present. Those answers may or may not be used in the future, but they are still a vital part of writing your paper.

Create a computer file with these questions for the Beginning, Middle, and End. Then, as you collect your data and begin your analysis, answer a few questions every day. That way you will have plenty of raw material for your paper when it comes time to write it.

2 BEGINNING

2.1 Hook

What information do I have that will interest people at the beginning—a good hook, e.g., any of the following?

a. A controversial statement
b. An element of surprise
c. A note of contradiction
d. A short, dramatic statement
e. Some statistics
f. A figure of speech (metaphor, simile, etc.)
g. A fresh quotation
h. A reference to a current event
i. A rhetorical question
j. A definition
k. An anecdote

2.2 Background: Why?, and a quick Who? Where? and When?

1. Why did I do this research?
2. Briefly, who were surveyed, where, and when?
3. How did I decide to do this study?

2.3 Background: Who else has done work?

1. Prior knowledge about the group (anthro, linguistic, etc.).
2. What is the big picture: language affiliation and characteristics?
3. Sources of information—especially acknowledge those resources behind your write-up: libraries, authorities, national assistants, contacts, sponsors, encouragers.
4. Who do I thank (in a footnote)?
5. Cause of paper: how does my work fit into a government plan?
6. Scope of the paper, scope of research itself, etc.

2.4 Thesis (Research Question)

1. What is my ultimate goal in this research?
2. Summary statement of purpose of the paper.

2.5 Plan of Paper

How is the paper organized (major sections, probably not sub-sections)? Use same words as headings of sections; shortening of section titles is encouraged here.

3 MIDDLE

3.1 Anticipated Objections, Reservations, and Questions

1. Problems encountered, questionnaires, access, methodology, cultural sensitivities. Where was my data skewed by conditions of my data

collection? Where am I uncertain in my analysis, and in the END, also?

2. "Why questions" re: the results that I obtained. Why does Village A have this attitude towards Village B? Why did I disregard some of my data?
3. What are the limitations of my research? of this paper?

3.2 Background: Where? and When?

1. Where was the research done? Which villages?
2. Date of the research. How many times? How long?
3. "When questions": dates of wordlists used, library resources, seasonal considerations, migration/work patterns, etc.
4. "Where questions": refugee camps, capital, regional city, institutions, displaced communities. Site of the original village location (in the informant profile).
5. Map of the locations I visited.
6. Describe the geography: main features, cities, villages, populations, etc.
7. What previous historical and anthropological references to people groups exist?

3.3 Background: Who?

1. Why did I choose the particular villages? the particular language? Why one province over any other one?
2. Language community? Speaker informant profiles: age, sex, education, exposure to other languages, physical conditions.
3. How many people did I survey?
4. How much language vitality did I find, e.g., amount of generational spread in usage, domains, attitudes, marriage patterns, work patterns, number of speakers, villages, contact patterns, etc.?
5. What is the usage and distribution of traditional and national language scripts? What attitudes about them are evident?

3.4 Method: What was I Looking for?

1. What did I expect to find before I started my research—before I left the library and began collecting my own data?
2. What is my ultimate goal of this research? intelligibility? dialect mapping? bilingualism? language vitality? literacy in traditional scripts or national language scripts? usage and attitudes?
3. What is the data? word list of #, comprehension tests of what nature, etc. Refer to appendix for complete one.
4. How much bilingualism is there, and how good is it?

3.5 Method: Collection

1. How was the data gathered?
2. How was the data handled? What technology was used, what tools, what instruments? Then describe what was done with what equipment, thoroughness, etc.
3. What orthography am I using (IPA, SIL, etc.)? What standards am I using for transcriptions, writing my paper, etc.?
4. What do the symbols mean?
5. What level (phonetic, phonemic, etc.) of transcription did I use in collection? different level in analysis? Give examples.
6. What have I left out?
7. How much data do I have available?
8. How reliable is my data?
9. Did I collect words or sentences? Did I anticipate syntax, etc.? conclusions?

3.6 Method: Analysis

1. Why did I choose the particular instruments, methodology, strategy?
2. What definition of a cognate did I use? historical? transparent intelligibility? How will borrowings fit in? What are the implications of my choice of definition? (Include references to back up choice.)
3. Determination of cognates. What was my methodology? How did I handle borrowings, historical conclusions, intelligibility, etc.? (You may defer analysis until a later paper on some aspect of your research.) Give examples that illustrate your contrasts.
4. Were any diachronic or synchronic distinctions made?
5. How are cognate results reported (number, percent, ratio, etc.)?
6. What ranges of error apply to each decision and figure?
7. What was the interaction of neighboring minority languages/dialects?
8. Analysis questions:
   a. Give number of exact matches.
   b. By how many phonological features do the vowels differ? Which are they?
   c. What are my categories?
9. Do clusters relate to single segments?
10. How did I handle tones in my cognate determinations? How did I indicate tones? e.g., Does tone 55 relate to tone 35 which relates to tone 33?, etc. Did I compare starting points, ending points, contours, amount of change, or use a unitary symbol? If unitary symbol, does it make sense when I look at the actual tone shapes and levels?
11. How did I handle rhymes? Is the vowel enough by itself?
12. Statistics is one window on language relationships. Did I use other windows, e.g., comprehension tests?

3.7 Discussion of Result

1. How did the phonology differ? Were the differences regular?
2. Do I have separate phonological descriptions?
3. What phonemes are involved?
4. What clusters are found in what environments?
5. What rhymes, tones, registers affect the vowels?
6. How many of the words were the same? How many were cognates? Can I make a topological map based on the cognates?
7. What is the matrix chart(s) of the dialects covered?
8. What discussion of the matrix chart(s) is profitable/necessary?
9. What groupings (and subgroupings) are reflected in the matrix chart(s)?
10. Which two languages/dialects have the highest correlation percentage? (From this nearest pair or neighbor, can I build an organized chart of mutual intelligibility?)
11. How do ballpark figures (>90% fairly certain, likely mutual comprehension; <80% unlikely mutual comprehension) relate to my own experience?
12. How well can people understand each other from dialect to dialect?
13. What conclusions did I come to for synchronic mutual intelligibility?
14. Can I use a typological summary?
15. Draw a family tree relating the dialects/languages.
16. How did the dialects/languages diverge? When?
17. How do the results relate to other languages or universals?
18. What comments and conclusions can I make about historical relations?
19. How do my conclusions relate to previous research?
20. How do my conclusions relate to previous reconstructed proto-languages?
21. How much variation is there within a dialect or village? Give examples.
22. What symmetry and patterns are discoverable?
23. What doesn't fit?
24. What tension is in the data? Do some data point one way and some another?
25. What can I say about morphology, syntax, and discourse? (We all know that it will be incomplete at this stage.)

3.8 Digressions

1. Am I having fun yet? What was fun?
2. What anecdotes, overheard conversations, folk stories, etc., could serve to make the people more human?
3. What anecdotes, overheard conversations, folk stories, etc., support or illustrate my conclusions or discussions?
4. What did I or my helpers observe (see, hear, etc.) that is not reflected

in my statistics or is not quantifiable?
5. What did I **over**hear?
6. Is there anything from the "Hook Question" at the beginning that could be used as a profitable digression?

4 END

4.1 How can it be Summarized?

1. How does what I discovered compare with perceptions of intelligibility, prestigious dialects, perceptions of speakers, expatriates working in the area, and other sources?
2. Summary of my results.

4.2 What Questions Remain Unanswered?

1. Orthography possibilities and suggestions: can some orthographic symbols be used differently in different dialects in order to lessen comprehension problems?
2. What implications are there for literacy, translation, etc.?
3. What are some further research questions?
4. What are some recommendations for myself or other researchers?
5. If I had time, what other data would I collect, and how?
6. If I had time, what other analysis would I perform?
7. What are some recommendations for translation, literacy, survey, etc.?

4.3 End Matter

1. Retype!! in consistent format and orthography.
2. Include appendices, footnotes.
3. Include relevant tables, symbols, abbreviations, charts, statistics. (Some tables and charts belong in the body of the paper, while others should be put in appendices.)
4. Include sample test form, wordlist summary of each dialect/language, comprehension test sample, etc.

NOTE

* In part, these questions are a result of a workshop in writing linguistic research papers at the 1991 Thailand Group Sociolinguistic and Linguistic Conference, October 1991.

[Editor's Note: The following entries published in *Notes on Linguistics* treat to one degree or another the problem of writing up a linguistic survey. If other articles are available, the editor would be glad to publish the bibliographical information in future issues.]

Collins, Wesley M. Survey Report of "A dialect survey in the Mam area of Guatemala." 46.4–11.

Day, Dwight. Checklist for writing book reviews. 52.39–40. (This is a set of questions that could be used to make a template file for writing book reviews.)

Gregerson, Ken. Some thoughts on writing technical papers. 5.16–17.

Law, Howard W. Writing "nickel knowledge" articles. 45.4–7.

Law, Howard W. Writing for scholarly publications. 52.11–16.

Marchese, Lynell. Tips on writing papers. 17.20–22.

Naden, Tony. Keep it snappy. 18.12–13.

Payne, Thomas E. How to write a grammatical sketch (without putting your readers to sleep). 44.10–15.

Peck, Charles. Readable technical paragraphs. 49.7–30.

Thompson, Edward T. How to write clearly. 18.8–11.

Yorkey, Richard. How to prepare and present a professional paper. 9.29–32. ■

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A Few Practical Tips for Using CECIL

by Mike Cahill

I INTRODUCTION

The CECIL hardware and software package is the most FUN computer program that SIL has ever put out. I think that's because it has both visual and audio components that are not a part of the usual language-crunching we associate with linguistics-type programs. I have been using it for a couple of years now and have found it very useful.

This is a potpourri of suggestions that have come out of my experience in applying CECIL to an African tonal language — Kɔnni, a *Gur* language of Ghana. (I do not deal here with the SPECTRUM program that accompanies CECIL.) While these ideas are not new or revolutionary, I offer them to those who are just beginning with CECIL in the hopes that some confusion may be avoided and CECIL be used more effectively.

I assume that the reader has a basic familiarity with CECIL from the tutorial books.

II THE INPUT

A. Get a local speaker with good voice quality. By this I mean a clear voice, not raspy or gravely. Very low men's voices do not show up well on the tone analysis.

B. Using the microphone and a live speaker is great when you are giving a demonstration, but there are several advantages to using taped rather than live speech for the bulk of your analysis. It is relatively easy to get a speaker to be consistent on speed and volume when he's doing lots of words fairly quickly into a recorder, but more difficult when you use the microphone and CECIL. Wait for it to process the utterance, then go on to the next utterance. Also, a tape recorder is often more patient than a live person in your fiddling with

various adjustments. (By the way, this suggestion assumes you have a good quality tape recorder, like a Marantz PMD–221.) For best quality, you will need to get a patch cord to connect your recorder and the CECIL box.

C. A quiet place outside, if available, may be a better spot to record than a room with a lot of echoes. It should go without saying that the fewer roosters and goats on the tape, the better!

III THE TRANSCRIPTION

A. For some applications, putting in a transcription is not necessary, or you may need to transcribe only one word of an utterance or one syllable of a word. The main purposes of a transcription are to give yourself a visual representation of what was said, and to denote segment boundaries. The latter are important if you are trying to isolate a sound or measure its duration. Since accurate placement of the transcription can be the most tedious and time-consuming part of CECIL, don't do it if you don't have to.

B. For my situation, the "change" graph has been of limited usefulness in deciding where to insert a segment symbol. For Kɔnni, at least, which has a fairly straightforward CV/CVV syllable structure, the easiest thing is to start with the data graph, move the cursor to a suspected boundary, and listen to the sound on either side. Often the Fs graph will have a discontinuity at a boundary, too, and can be used as an indicator. (See the beginning of the vowels in Fig. 1.)

C. The more resonant the segment, the harder it is to place its transcription precisely. Stops are easy. Liquids and glides are not. An [ŋ] at the end of a word is very difficult to place, especially since the preceding vowel tends to be nasalized as well. (90% of Kɔnni nouns end with /ŋ/ in citation form!)

IV INTERPRETING TONE

I think all of us have had dreams of putting a puzzling utterance into CECIL and getting a neat, well-defined graph that looks something like:

and solves all our problems. Instead, the first time we try it, we get something that looks like Figure 1! A few notions I've found useful in interpreting such a graph are:

A. Voiced stops universally have a tendency to be lower-pitched than surrounding vowels. (See the two "b's" and the "d" in Fig. 1.) This lowering can persist into the first part of the following vowel.

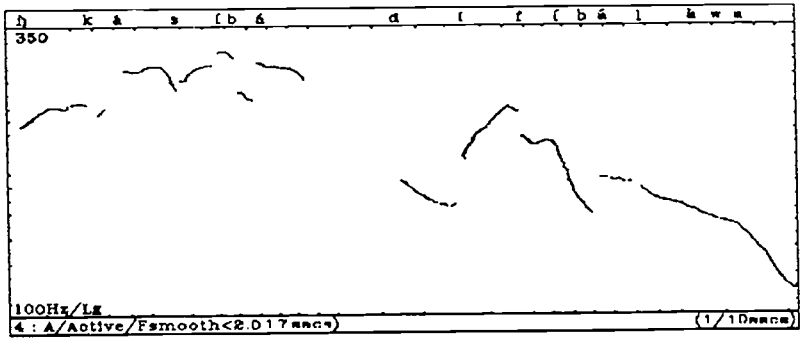

Figure 1. ŋ ká síbá dìí !íí !bál à wà

B. Voiceless stops can sometimes raise the pitch of the first part of a following vowel (not evident in Fig. 1).

C. Sentence-initial syllables sometimes take several hundredths of a second to "get up to speed" and so have a rising glide quality, even if phonemically level. This shows somewhat in the initial syllabic [ŋ] in Fig. 1.

D. The more sonorant the consonant between vowels, the less distinct will be the boundary on tone between syllables. See the downslope of tone over the span of [alawa], ending the utterance of Fig. 1.

E. Be sure to adjust the range of the graph to effectively display the tone. The default settings will flatten out the graph and obscure the changes in frequency.

F. The "real tone" you're concerned with is usually on the vowels. Ignoring or even physically blotting out the tone on consonants may give a clearer picture (but in some languages tone on nasals, laterals, etc., may be significant).

V OTHER APPLICATIONS

One of the most useful features of CECIL for me has been its ability to isolate a sound and play back a single syllable or segment or even part of a segment.

A. **Vowel quality.** English speakers tend to use several cues in their perception of vowel quality. When these cues don't match the language you are studying, there is much potential for confusion. For example, I didn't hear short [i] well at all in Kɔnni (and still don't). The problem is that in English the phonemes /i/ and /ɪ/ are distinguished not only by vowel quality, but also by length. English /i/ is pronounced with a glide ([iy]) and is considerably longer than English /ɪ/, so when I hear Kɔnni short [i], my immediate reaction is to write [ɪ]. By isolating the vowel with CECIL, you can hear the true quality of the vowel without being influenced either by length or by surrounding segments.

B. **Vowel length.** For languages with timing contrasts (long vowels and consonants), you can enter the transcription of the utterance, then using <HOME> and <END>, isolate the vowel. The duration in seconds can then be read directly from the bottom of the screen.

The caution on this, of course, is that timing is relative. For comparing durations of long and short segments in different words, they must be taken in environments where the speaker is speaking at a consistent speed. Therefore, taking samples from different parts of a narrative text, where the speaker slows and speeds up according to the drama of the moment, is not a good technique. If you can get your language helper to recite what you want at a fairly steady pace,

fine. As mentioned above, using tape-recorded speech is preferable to individual utterances directly onto CECIL.

For the most precise measurements of vowel length, use vowels between stops. As mentioned above, the placement of the transcription is more dependable with stops than with sonorants.

C. **Aspiration, labialization, palatalization.** For those languages where aspiration is significant, CECIL is a convenient tool for hearing and seeing it. The aspiration can show up on the data screen. Also, by isolating the consonant, aspiration can clearly be heard on playback. The same principle applies somewhat to labialization and palatalization. When the consonant is isolated and played back, a distinct difference can be heard between, e.g. [k] and [kw].

D. **Discourse.** Though the time of recording data is limited with CECIL, there still may be some useful things to discover about a whole discourse. One possibility is to use CECIL's capabilities to identify phonetic correlates of discourse structure. (You will need to feed in the discourse a sentence or two at a time.) For example, I initially assumed a certain narrative was divided into episodes, and these episodes into paragraphs. I made these divisions somewhat impressionistically, and confirmed and sometimes corrected them by the presence of various discourse markers. But besides the semantic and syntactic markers, my wife, Ginia, wondered if there were any phonetic indications to confirm the divisions. There were.

I input each sentence of the story into CECIL, and for each sentence, read off the high points of both amplitude ("stress") and fundamental frequency from their respective graphs. I then plotted them on a graph, and found that there was a fairly good fit between these and my postulated paragraph breaks. At the beginning of a paragraph, the amplitude and frequency was high. It lowered in the course of the paragraph, then became high again at the start of the next paragraph (Fig. 2). Interestingly, as the peak of the story was reached, the overall amplitude and frequency both increased—the storyteller got louder and shriller. This is not surprising, but with CECIL I could quantify it, however crudely at this point.

Another possibility for using CECIL to study division breaks is to measure the length of pauses between clauses or sentences. In this Konni story, however, this was not possible, due to the story-telling

technique. The storyteller says a sentence, then another man repeats approximately the same sentence, sometimes starting his utterance before the first man finishes his!

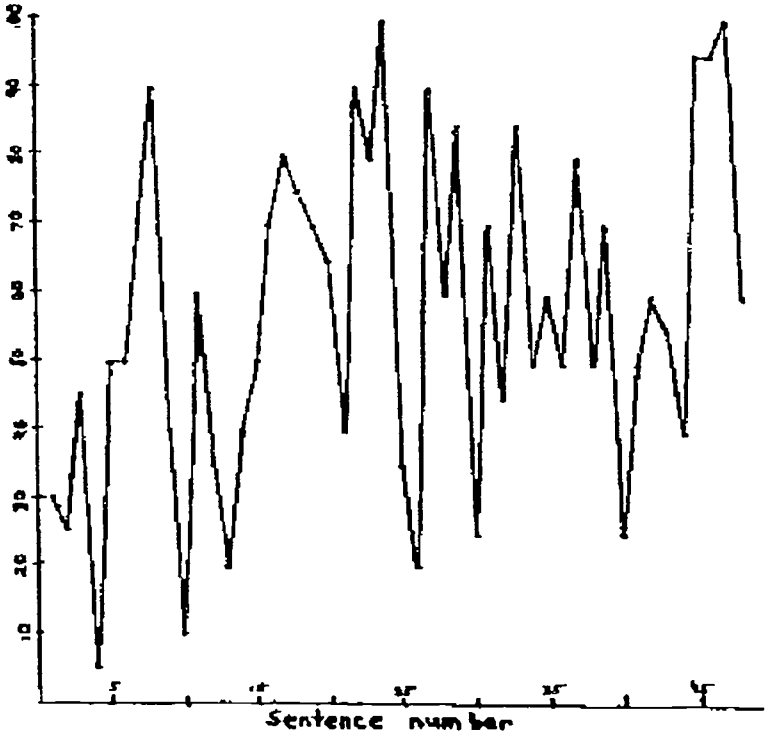

Figure 2.

■

Terminal 2/2+ in Language Learning

by Sue Wright

Have you ever felt like your language learning is going nowhere? I was surprised to learn that there is a documented condition known as "terminal 2/2+". Higgs and Clifford (1982) use the term to describe students who are stuck at the 2/2+ level on the FSI scale. To give you an idea of what this would sound like, I've included their examples of what each level of the FSI would be like in English.

Level	Visa Officer's Reply to Applicant:
5	Under U.S. statutes, your affiliation with the Communist Party renders you ineligible for a regular tourist visa. There exist, however, waiver procedures which may be invoked. These are the steps that you should initiate...
4	According to U.S. lawss, your affiliation wiz ze Communist Party makes you uneligible for a regular tourist visa. You may, however, request a waiver. Zis iss what you must do...
3	Zee laaw zayz zat mambears of zee Communistic Partee caanoht bee geeven a regoolair tooreest veesaa. Owehvair, egzeptions are zohmtaymes dunn. You must do zees...
2	You cannot legulally get toolest visa. It is no light, because berong to Communistic Palty. But you can ask for a special permission. You to do this...
1	You commyunist. No gyet vyisa. Got tryy cgyain. Take thyis. Fill in, plyeez.

Someone at the 2/2+ level can get his meaning across but still has problems with pronunciation and grammar. A terminal 2/2+ (one who is not improving) will have a wide vocabulary and often be quite fluent. In other words, he speaks the language smoothly and quickly, but what he speaks is wrong. His problems are not missing grammatical patterns or sounds but fossilized incorrect patterns. Unfortunately, Higgs and Clifford claim that such fossilized incorrect patterns are not remediable. A terminal 2/2+ who takes an intensive

language training program will improve in vocabulary and fluency but not in grammar and pronunciation.

The reason for becoming a terminal 2/2+ is found in the method of language learning used. If the method stresses communication first, using what you know even with errors in it, there is more chance of becoming a terminal 2/2+. However if the method stresses accuracy first, people tend to move through level 2 into levels 3 and 4. This is in direct contrast to how the methods appear to be working in the initial stages. Someone in a communicative method will quickly start interacting with people on the street and develop a reputation as a good learner. On the other hand, the accuracy first method doesn't give the student much to talk about. Initially he will appear to be making less progress although his long term prognosis is much better.

So what should you do if you're like me? You read this and think, "Oh no, I'm condemned to speaking lousy Thai for the rest of my life." First of all, we don't know how long it takes for a wrong pattern to be fossilized. Is it a month, a year, five years? Don't decide you can't change until you try it. Secondly, speech therapists make a living helping people change fossilized patterns so, although it might take a lot of work, I'm not convinced it's impossible. However, if you are really going to improve, the stress needs to be on grammar and phonology instead of learning new vocabulary. The first thing to do is find out what you are doing wrong. Have a friend listen to you and write down words you mispronounce or identify ungrammatical structures you use. (I taped myself and gave it to my students for error analysis. A humbling experience!) Next choose one error to work on. Make some drills on the correct way to say it and practice them. Also enlist the help of everyone you know. Tell them, "I've been saying X and I know that it's incorrect. Whenever I say this will you please correct me?" You might need to stress that you are only working on X at this time. When you are no longer receiving corrections on X, or when you find yourself supplying the correction before your friends can, it's time to choose another problem.

When you start learning your next language keep the idea of a terminal 2/2+ in mind. The old practice of writing a phonology statement and grammar before starting anything else makes a lot of sense. By focusing on these two areas in the beginning, your chances of successfully speaking the language improve. Of course that doesn't mean you stay in your house bent over your computer. There must

be a balance of things. But it does mean that putting off analysis until your teaching semester and using the village time for just "language learning" might have far reaching effects that you will regret later.

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5TH INTERNATIONAL SYSTEMIC WORKSHOP

UNIVERSIDAD COMPLUTENSE DE MADRID, SPAIN

JULY 26, 27 AND 28, 1993

CALL FOR PAPERS

Main topic will be LINGUISTIC AND TEXTUAL EXPLORATIONS: CORPUS-BASED DISCOURSE STUDIES. Work in progress dealing with different aspects of Discourse should preferably be based on a significant amount of data, in order to validate conclusions on discourse phenomena. Papers dealing with topics from previous Nottingham Workshops will also be considered.

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Facultad de Filología
Universidad Complutense
28040 Madrid, Spain
Fax: (9)1- 3945298 Tel.: (9)1- 3945396
E-mail contact: send messages to Julia Lavid to the account: lavid@dit.upm.es

Abstracts should not exceed half-page. Include a heading with title of paper, name of author and institution.

Linguistics: An In-House Thing? A Diary Entry

John Verhaar S.J.

I

Academic people have something vaguely 'monastic' about them—of course, in the West, universities developed from monasteries. The ritual paraphernalia are still there: academic processions; robed professors (who 'profess' their 'doctrine') fastening gazes of wisdom from behind lecterns on those as yet uninitiated; formulaic modes of address with superlative-form attributes attached only to professors (and to those being 'granted' doctoral degrees); solemnized 'convocations' and 'commencements'; red wax stamps on bulls in Latin. Communities 'set apart' on campuses.

The demonasticization of universities over the centuries has, I ponder, perhaps mainly taken two forms. First, knowledge began to replace wisdom, and then wisdom retaliated by transforming itself into 'theory'. Second, monastic discipline developed into due process, and academic controversy proceeded in what I call the 'forensic' model. Both changes have been progress of some kind, but we need more.

Theory—yes, it is still prestigious all right. If you don't have a 'theory', what, pray, **are** you going in for? Taxonomy-triggered increase of paper flow? Mindless juxtaposition of random things? Mere performance, perhaps? Just description? Don't you know that Plato detested Homer precisely for that kind of shallowness? And that Descartes 'proved' the existence of the 'external' world from the existence of God, which in turn he, mentalist *par excellence*, 'proved' from that 'thinking thing', until then characterized as the 'intellect' that 'judges'? Historically, theology was faith made polysyllabic, then philosophy was theology made user-friendly for the skeptic; and theory is wisdom defrocked. Culturally, defection from theory is the secularization of atheism. 'Secularization'? Yes, of course, for the whole concept of atheism is parasitic on belief in the divine. (What was that? Oh, you feel all this would take four fat volumes to argue? In fat volumes, yes; but this is a diary.)

And 'judgment'? It is a metaphor deriving from a court of law. The rules together create a 'forensic' model. Its origin was neither Plato nor Descartes but the Stoics, who turned Aristotle's *krisis* (separation—'objectively', as we would now say—of true and false) into a ('subjective', individual) *krisis*, or *iudicium*, still with us as that 'judgment' of the intellect. The academic sub-culture still has the Stoic model. If, *pace* Lakoff and Johnson, 'argument is war', then academic forensic model is like a court case, occasionally acrimonious, about—yes—'property'.

Theories are like possessions. A 'theory' may lack obviousness or intuitive plausibility, but if you're the first to 'stake out' your 'position', it's yours. In academic controversy, the idiom we use is revealing: we 'claim' such-and-such, or so 'contend' or 'maintain'—setting out those 'stakes'. Perhaps someone else 'claims' the same, but rightly so? Was the other one 'first' in claiming the same? The files emerge: oceans of references, almost like an appeal to the land registry office.

If you make your claim after I did, then, say the rules, the 'burden of proof' will 'revolve' on you—you tried to register the same property—later. We then 'contest' each other's 'positions'—and fight about words. You feel that I 'misrepresent' your 'position', and I feel you do in regard to mine. Perhaps, of course, then you or I will 'grant', 'concede' or 'admit' this point or that, in passing, of course, and only strategically. We certainly don't share, let alone enjoy together, truth. The rules will make that impossible.

But who is the 'judge'? Well, 'reason', of course. Yours or mine? Both, naturally, so either you're in my court or I am in yours, and we never share anything except those rules—fighting rules. And truth is no longer communal—it has been individualized (so committees for rank and tenure can deal with it).

But, slowly, 'theory' is losing. We don't know what a theory is worth unless we know what kind of thing would prove it wrong. And so theories have become 'hypotheses', and the umbilical cord with wisdom has been severed permanently. 'Hypotheses' are more communal. You or I don't 'claim' them; rather, we 'propose' them—also to ourselves. You may be trying to find counter evidence to what I propose, but so am I. Thirst for knowledge will move you and me to prove our own hypotheses wrong. You and I have no

basic quarrel about either your or my hypothesis: each of us wants to overturn both. The claims have gone, the judge has disappeared. Remains some friendly sparring, with a common interest in finding new things. It then becomes exciting to work together, and to enjoy truth together.

In a convocation of the wise, no two persons can be equals. Theorists pursue equality through common rules, but those are all they have in common, and equality is unattainable. Those who pursue knowledge will feel they need one another.

II

From this perspective, the best linguists are field workers. They are at the high end of a consistent development in our own century. Field workers teach mainly what they have learned from people who learned without being taught—native speakers. Those speakers are hosts, and their guests would never dream of 'claiming' anything, and thus there's no need for granting, admitting, or conceding anything, and there are no 'burdens of proof', and there's nothing to 'contest'. You and I won't think native speakers have a 'theory'—except, of course, if (perish the thought) we were to decide to be Platonists and to fasten the expression 'theory' on to what speakers know so well they don't know they know it. Native speakers wear no academic robes and don't utter utterances from behind lecterns. They are such experts because they don't worry about what 'founds' their expertise.

Learning a new language from its speakers is the *via regia* to intelligence—which is, as Herbert Simon (he of the Nobel prize) once said: *The result of repeated short cuts through experience*. In contrast, theories are crutches.

III

So the forensic model, too, is losing. In linguistics, functionalism would be impossible without this cultural change. Functionalists immerse themselves in people and in evidence—not in theories. They don't proselytize, and in their quest for language universals they follow hunches, and, in the light of new evidence, follow other hunches they never realized they had. They now begin to know a number of things languages have in common. They wonder what makes grammar grammar and don't like to assign a thing called

'grammar' to any registry office for fear the file will languish there until someone 'contests' what it says on its fading pages.

IV

All this, of course, is part of Kuhnian 'paradigm' changes. If you promote your favorite (and relatively new) paradigm in the court of academic discourse you won't get far—unless your followers are many and start 'registering' along with you. True innovators stay out of court. From seeing Saussure's *Course in general linguistics* you wouldn't think he had already had a distinguished career in diachronic research behind him. It also helps to write things without any reference to anyone, as Edmund Husserl and Ludwig Wittgenstein did in philosophy. Some cynic once said that nothing advances scholarship faster than the death of scholars. But no death knoll is needed for renovation—just start again, out of court.

These things now happen also by tearing down barriers separating 'different' fields of enquiry. One may, for example, read an innovating work on Freud by a Professor of English, or one on Shakespeare by a Professor of History. Professors of Literature now regularly write philosophy, to the point where Professors of Philosophy feel their identity is deprofessionalized. These are excellent developments—the distinction of disciplines is pretty much a matter of academic bureaucracy anyway.

My diary ponderings move me to mention a great man recently honored on the occasion of his 75th birthday, Joe Greenberg. He is a distinguished anthropologist—and in linguistics set a more powerful development going than perhaps anyone else. But then, Joe never 'registered', never proselytized, and never sought to create a 'school'. He has always pursued knowledge, and has been at the source of a veritable explosion of it among linguists untold. ■

Symposium On Mood and Modality

University of New Mexico, Albuquerque
May 8–10, 1992,

John R. Roberts

1 Introduction

A symposium on mood and modality was held on the campus of the University of New Mexico, Albuquerque. This symposium was called by Joan Bybee to discuss the grammatical expression of mood and modality in natural language. It was requested that papers for the symposium should approach the question of the function of mood and modality markers from a discourse, diachronic, developmental or cross-linguistic perspective. The modalities to be considered would include both the root or agent-oriented modalities of obligation, necessity, root possibility, permission, ability and volition, the epistemic modalities of possibility and probability and the modalities specific to subordinate clauses, such as subjunctives and conditionals. It was requested that papers should not focus specifically on conditionals or evidentials since previous conferences and collections of papers had focused on these topics.

This symposium belongs to a series of symposia and conferences called to discuss various phenomena of linguistic grammaticalization from a typological-functional, cross-linguistic perspective and dates back to the Symposium on Subject and Topic held at the University of California, Santa Barbara, March, 1975. Currently the results of these symposia are published in the Benjamins Typological Studies in Language series.

2 Participants

Participants were invited to attend as either discussants or to present a paper. Over forty participants attended and they came mainly from universities in North America, Europe and Australasia.

3 Papers Presented

Twenty-one papers were presented and the contents of each paper are briefly discussed below in their order of presentation. The papers were circulated between participants beforehand and were presented at the symposium for discussion and comment. One paper by Talmy Givón entitled *Irrealis and the subjunctive* was circulated among the participants but was not presented for discussion. Also Jennifer Coates and Elise Kärkkäinen circulated papers outlining their past and present contributions to the discussion of mood and modality but did not present a paper specifically for the symposium. The diversity of topics covered in the papers reflect the multifarious nature of the linguistic notion of modality and its grammaticalization. The more important themes covered by papers in the symposium were: (a) The relation of agent-oriented (deontic) to epistemic modality (Abraham, Heine, Sweetser, Lichtenberk); (b) The process of grammaticalization of mood and modality (Abraham, Bybee, Bavin, Lunn); (c) The realis/irrealis dichotomy as a means of defining the cognitive domain of modality (Roberts, Mithun, Romaine, Chafe, Givón); (d) The use of modality in interactional discourse (Silva-Corvalán, Myhill and Smith); (e) The acquisition of modal functions and forms by children (Guo, Choi); (f) The interaction of modality and negation (Palmer); (g) The modal function of complementizers (Frajzyngier).

The aspectual basis of the deontic--epistemic distinction in modal verbs
by Werner Abraham, Rijksuniversiteit Gronigen

Abraham discusses the distinction between epistemic and root meanings of English modal verbs and compares them with those of the modal verbs in German and other Germanic languages, such as Danish. Specifically, English modal verbs have remarkably fewer root meanings (deontic, i.e. permissive and volitive) than German modals have, whereas for epistemic and alethic meanings the modal verbs in both English and German are on a par. Also English modal verbs have completely lost quite significant properties that generally characterize main (fully lexical) verbs, such as the ability to form periphrastic forms, gerunds, control (*to*—) infinitives, *do*-support, multiple clustering, and inflection with respect to person and number. German modal verbs, by contrast, have all of these properties. A.

attempts to present a reason for the quite different diachronic development of English and German modal verbs. His hypothesis is that the English modal verbs developed into a quite distinctive class of verbs due to their loss of certain aspectual properties, viz. terminative meanings. This then produced particular structural reflexes in Early Middle English, such as the loss of participial and infinitival forms and meanings, and the loss of the periphrastic perfect and future which was replaced by *be going to*.

Agent-oriented vs. epistemic modality: some observations on German modals by Bernd Heine, University of Cologne

Heine discusses a number of questions related to recent research on the basic modal distinction between what has variously been referred to as deontic, root, objective, pragmatic or agent-oriented modality on the one hand and subjective, hypothetical or epistemic modality on the other. His questions are:

(i) What are the factors that can be held responsible for the fact that the use of linguistic expressions of agent-oriented modality is extended to also express epistemic modality?

(ii) If a given linguistic expression is used for the expression of both agent-oriented and epistemic modality, are we dealing with an instance of homonymy or of polysemy?

(iii) Is the transition from one kind of modality to another discrete/discontinuous or gradual/continuous?

(iv) How can the shift from agent-oriented to epistemic modality be described in a model based on grammaticalization theory?

H. then presents a survey of German modals in which he demonstrates that their association with agent-oriented or epistemic meanings form various types of cline. For example, a modal with the highest values of agent-oriented and lowest values of epistemic modality is *wollen* 'will' which has a high number of verbal properties in German.

Verbs of action and terminative verbs are predominantly associated with agent-oriented modality. There are various linguistic contexts in which an utterance is more likely to have an agent-oriented interpretation:

(i) In interrogative rather than in declarative utterances;

(ii) In negative rather than in affirmative utterances;

(iii) When the subject is a first person referent rather than when the subject is a third person referent.

The choice between the two kinds of modality also depends on a range of conceptual properties. Agent-oriented uses of German modals are associated with the following properties:

(i) There is some force (F) that is characterized by some "element of will";

(ii) The event is performed typically by some agent (A);

(iii) The event is dynamic (D);

(iv) The event has not yet taken place at reference time, i.e. it is later than reference time (L);

(v) The event is non-factual but there is a certain degree of probability that it will in fact occur (P).

H. also suggests that in the discussion of the diachronic development from agent-oriented to epistemic interpretations we have two main models at our disposal to account for the conceptual shift from the concrete/lexical to the abstract/grammatical meaning. One rests on the metaphorical interpretation of the process, and is termed the metaphor model, and the other relies on context-induced reinterpretation, and is termed the context model.

H. comes to the following conclusions. It is well established that, diachronically, epistemic uses of modals develop out of agent-oriented ones. It is also the case that epistemic uses are therefore more grammaticalized in language. With respect to German modals it is not possible to describe the transition from agent-oriented to epistemic meanings exclusively in terms of linguistic categorization. Various types of contextual frames have to be taken into consideration and also various conceptual properties. Finally, the transition from one kind of modality to another has characteristics of both discontinuity and continuity and both the metaphor model and the context model are required for an adequate explanation.

Mood and modality in Acholi and related Nilotic languages
by Edith Bavin, La Trobe University

Bavin describes the grammatical forms used to express various modalities in the Nilotic languages of Acholi, Lango, Dholuo, and Alur. These languages are all related genetically and share many lexical items yet, as B. points out, the formal expression of the different modalities varies from language to language.

B. discusses the divergence of modal expression in these languages with respect to the hypothesis that epistemic modalities develop out of deontic or agent-oriented modalities.

Contextual conditions for the interpretation of poder and deber
by Carmen Silva-Corvalán, University of Southern California

Silva-Corvalán presents syntactic and semantic arguments for treating *poder* 'to be able' and *deber* 'to owe, to have to' as a distinctive class of modal verb in Spanish. S-C. examines the different interpretations of the meaning of *poder* and *deber* in different contexts and concludes that: (a) A monosemantic model seems to account for all the uses of *poder* and *deber*; (b) The relation between the basic meaning of these forms and the messages they convey in actual use is dependent upon both linguistic and extra-linguistic factors; and consequently, (c) The most adequate approach to study the semantics of modals appears to be one which examines them in contexts of use.

The interactional structuring of meaning: children's use and development of the Mandarin modal néng
by Jiansheng Guo, University of California, Berkeley

Guo attempts to illustrate, through the use and development of the Mandarin modal *néng* (roughly translated into English as 'can') in child speech, how the interpersonal function of this modal constitutes an inseparable part of the meaning of the word, and how this interpersonal function saturates the form and consequently changes the semantic content of the form. G. analyzes the semantic content of *néng* in children's speech in terms of force dynamics, e.g. initiating force, resisting force and final state. He analyzes the overall meaning

of *néng* as 'challenge', i.e. in the physical abilitative meaning, it is used to challenge the addressee's presupposed assumptions; in the social permission meaning, it is used to challenge the addressee's actions; and in the epistemic-like meaning, it is used to challenge the addressee's assertion made in the immediately preceding discourse. G. proposes that the change in the semantic content of *néng* in children's speech is brought about as the child develops his/her use of the word in different social and discourse contexts.

The development of sentence-ending modal forms and functions in Korean children by Soonja Choi, San Diego State University

A number of studies have shown a consistent diachronic pattern in the development of modals in language, viz. that epistemic meanings develop out of forms with purely deontic meanings. Likewise, a number of studies in child language acquisition have shown that children consistently develop deontic meanings of modals before they develop epistemic meanings. In fact, it seems to be the case that English speaking children do not develop epistemic meanings until they are 3-5 years of age. Choi conducted a study of how three Korean children used the sentence-ending modal forms in Korean and concluded that (a) these Korean children acquired several epistemic distinctions before 3 years and (b) they acquired certain epistemic distinctions before they began to acquire the deontic modal system.

The gestural expression of modality in ASL
by Phyllis Wilcox and Sherman Wilcox, University of New Mexico

Wilcox and Wilcox begin by saying that there are four pre-empirical postulates that traditionally underlie formal/structural linguistic theories:

(1) Language is a separate module of the mind/brain, not part of 'general cognition';

(2) Structuralism in the analysis of language; that is, language structure can be analyzed independently of its communicative function;

(3) The sign-relation between the linguistic code and its mental designatum is arbitrary, unlike the obvious iconicity seen in pre-human communication;

(4) Some abstract, idealized entity—be it *langue* or *competence*—is the 'object' of linguistic analysis.

W&W challenge all of these postulates with a study of the production of the modal verbs in American Sign Language for the deaf. On the basis that in ASL physical gestures which are clearly iconic are used to express modality, W&W make the following counter-proposals:

(1) Visible, physical objects are the means by which signed languages are realized—these objects are moved into and out of spatial configurations by neuromuscular gestures, not by abstract rules;

(2) A theory of signed and spoken languages must acknowledge the physically embodied grounding of linguistic competence—in a very real sense, the body is in the mind, the essence of language is bodily activity;

(3) When physical gestures are used to represent mental designata, there is the likelihood that an iconic or isomorphic relationship will exist between the two—not only at the lexical level (which has been denied), but also at the propositional and pragmatic/discourse levels (which has been ignored).

Grammaticalization routes and the root/epistemic relationship
by Eve Sweetser, University of California, Berkeley

The diachronic development of epistemic meaning from deontic modals is well established. However, Sweetser says that various motivations have been suggested for the connection between the two domains, which, at first glance, might seem to be in competition with each other. S. reviews the various suggestions that have been made in the literature for how epistemic meanings develop from deontic modals and then reduces them to two basic positions: the metaphor-based analysis and generalization or pragmatic metonymy-based analysis. In the first case (metaphor), the referential schema abstracted from the morpheme's meaning is mapped onto some other domain of meaning which need not be "adjacent" to the original one. For example, in English the intentional future meaning has been mapped onto the lexical expression 'to be going to'. In the second case (metonymy), a morpheme broadens its class of referents to cover some class which subsumes its old meanings. For example, a verb in English has a punctual meaning, as in 'The light **flashed** once'. In this case the event is conceptualized as a point in time. However, as a line may be conceptualized as a series of points, so a series of punctual events may be conceptualized as an event in

progress, as in 'The light **flashed** for half an hour'. S. suggests that there are two opposing kinds of cognitive forces at work in the broadening of semantic categories: speakers find it useful to group like things together, and also to group different things separately. So a category may grow broader, motivated by the first tendency; but then speakers find a need to re-name the central subcategory which gave its name to the new broadened category. The semantic broadening of a category is the metonymical extension and the semantic renaming is the metaphorical extension.

The discourse functions of obligation expressions
by John Myhill and Laura A. Smith, University of Michigan

Myhill and Smith develop a framework for the description of the textual function of obligation marking. This system for categorizing obligation is developed initially on a text database from four quite unrelated languages, viz. English, Chinese, Hopi and Biblical Hebrew. M&S found that the traditional distinctions made for obligation markers, such as 'strong' and 'weak' obligation, could not be tested independently so they used a range of discourse function criteria to establish the domain of meaning of a particular obligation form. M&S propose that their set of criteria and methodology could be used on a wider range of languages for the cross-linguistic comparison of obligation function. One thing M&S claim is that obligation expressions do not simply report objectively necessary actions; rather, they are devices used by speakers to evaluate and justify actions, so that the only way to analyze these references is in terms of usage rather than 'meaning'.

The modal functions of bai in Tok Pisin
by Suzanne Romaine, Oxford University

Romaine discusses the development of *bai* in Tok Pisin into a grammaticalization of irrealis marking with particular reference to Bickerton's (Bickerton 1981) claims about the universality of a creole prototype consisting of a three-member inventory of preverbal particles marking tense-mood-aspect (TMA). R. also discusses the the development of *bai* as a future marker with respect to claims made about the course of development of such markers

cross-linguistically by Bybee et al. (1991). Based on a large amount of written and oral Tok Pisin data R. argues for the following points:

(1) There is no evidence to indicate that creolization and/or urbanization is associated with the incorporation of TMA markers into the verb phrase in preverbal position in Tok Pisin;

(2) The existence of preverbal *bambai* with the future meaning shows that the preverbal slot was available for TMA markers long before phonological reduction or the existence of a community of native speakers;

(3) The grammaticalization process of *bai* follows in many respects the universal paths of development for futures predicted by Bybee et al. (1991) and intersects with many of the key routes leading to modality.

The negation of possibility and necessity modals
by Frank Palmer, Professor Emeritus, University of Reading

Palmer discusses two kinds of irregularity with respect to the negation of the modals of possibility and necessity. The first irregularity is that there is a simple lack of one-to-one correlation between form and meaning. For example, in the expression 'you mustn't go', the modal 'must' is formally marked for negation. It should therefore have the meaning 'there is no obligation to go'. But the meaning is actually 'there is obligation not to go'. With this irregularity the negation is said to be 'misplaced'. The second irregularity is that there are often gaps in the potential paradigm, though these often appear to be filled by suppletion, usually from other modal forms in the language. For example, in English:

You must go.
You mustn't go.
You needn't go. (instead of 'You not must go')

Obligation to act and obligation not to act are expressed by forms of **must**, but for no obligation to act the negative form of another verb, **need**, is required.

P. conducted an ad hoc cross-linguistic survey of twenty-four languages spoken by Ph.D students in the Department of Linguistic Science, Reading University, and found that the irregularities cited in English, French and, Italian occurred in much the same way in these languages too. In conclusion P. attributed the suppletion

phenomenon to economy of usage but was not able to give a satisfactory reason for the misplacement of negation phenomenon.

Choosing the best description of the Spanish Subjunctive
by Patricia Lunn, Michigan State University

Lunn discusses the use of the subjunctive form of the verb in Spanish across a range of Spanish dialects. The subjunctive form is used much more widely in Spanish than in a language like English, for example, and has many contexts of usage. L. explores these various contexts and concludes that all subjunctive marked propositions express non-assertion. A non-assertable proposition will lack two qualities: it must be both reliable as to truth value and informative as to news value. L. shows that Latin American dialects of Spanish differ quite markedly from Continental Spanish in the use of the subjunctive. Also there are novel uses developing in some sub-dialects. For example, in journalism the specialized press which addresses a knowledgeable readership has a tendency to use the past subjunctive to mark information which can be assumed to be known to assiduous readers.

The semantic development of past tense modals in English
by Joan Bybee, University of New Mexico

Bybee traces the development of the past tense English modals 'would' and 'should' from their usage in Old English through Middle English and into Modern English. B. first demonstrates that 'would' and 'should' had purely past tense meanings in OE. By the ME period, however, they occurred in present contexts. B. also demonstrates that these past forms used in present contexts also developed the modal meanings connected with their use in past contexts, such as including conditions on the completion of the main event. In Modern English these modals have lost their past meaning altogether. B. proposes that these modals came to be used in the present via the hypothetical conditional. B. also claims that it is the combination of the modal sense and the past sense that produces the hypothetical reading, and not past alone, nor modality alone. One consequence of this is that the classic Chomskyan synchronic analysis

of Modern English which derives 'would' and 'should' by combining Past Tense with 'will' and 'shall' cannot be justified.

The syntax and semantics of Tok Pisin modals
by Gillian Sankoff, University of Pennsylvania

Sankoff discusses the syntax and semantics of the four modal verbs in Tok Pisin *ken*, *mas*, *laik* and *(i)nap* which are all lexically derived from English. Only two, *ken* and *mas*, however, have English etymons that are themselves modals. The 'future' or 'irrealis' marker *bai* is also discussed but separately from the modal verbs. S. discusses these modals in a number of syntactic contexts, i.e. main verb, auxiliary, and impersonal higher predicate, and semantic contexts, i.e. imperative, prohibitive, epistemic and 'moral' (obligation and permission). S. shows that *ken* and *mas* have evolved away from their English etymons. For example, *no ken* is equivalent to English 'mustn't'. Also *nap* is the only modal in Tok Pisin which can occur in all syntactic contexts. One of the main points of this paper is to show that although the modal *nap* is lexically derived from English it has non-epistemic meanings in higher predicates, which is completely opposite to what the English speaker would expect. S. therefore cautions against using the development of the English modal system as a basis for making universal predictions.

The category 'irrealis' in Papuan medial verbs
by John Roberts, Summer Institute of Linguistics, Papua New Guinea

In this paper Roberts further elaborates on the binary realis-irrealis distinction marked on the medial switch-reference verb system in a number of Papuan languages. This is therefore a follow-up to Roberts (1990) and a response to Bybee et al. (i.p.). A number of linguists have claimed that realis vs. irrealis is the basic distinction in modality, for example, Givón (1992). However, Bybee et al. (i.p.) challenged the validity of irrealis as an overarching category in general and Roberts' proposal for such a category in Papuan medial verbs (Roberts 1990) in particular. R. presents the data again from Papuan languages and shows that the medial verb form is in agreement with the final verb category in respect of its 'reality' status, i.e. whether it describes an event that the speaker believes has

happened or not. In eight different languages a simple binary distinction of realis vs. irrealis modality is marked in the medial verb form. R. then presents a number of arguments for the validity of the irrealis category in these Papuan languages at least. For example, R. justifies the fact that the verb final category of past habitual is marked as realis in some languages but as irrealis in others by arguing that in some languages it is categorized as an aspect (realis) whereas in other languages it is categorized as a mood (irrealis). To further substantiate his arguments R. then presents evidence that the realis-irrealis marking in medial verbs in these Papuan languages has most likely developed from an original nonfuture vs. future tense marking.

On the relativity of irreality
by Marianne Mithun, University of California, Santa Barbara

Mithun notes that the terms realis and irrealis have been applied to constructions in languages of Papua New Guinea, Austronesia, Africa, Europe and to various creole languages. They have also been used in grammatical descriptions of North American Indian languages. Mithun also notes that the precise nature of the realis/irrealis distinction is far from uniform cross-linguistically. The kinds of events classified as irrealis in one language are often classified as realis in the next. It is also the case that the formal expression of the distinction varies cross-linguistically. Nevertheless M. proposes that despite the heterogeneity of the distinctions it is applied to, the terminology can prompt fruitful cross-linguistic comparisons with potential for furthering our understanding of certain processes of grammaticalization. M. then presents data from Central Pomo to show that in this language a realis/irrealis distinction is marked on the medial switch-reference verb with a similar function to that cited by Roberts (1992) for Papuan languages. The main difference between irrealis in Central Pomo and irrealis in Papuan languages is that, whereas this category has future tense as its cognitive, and perhaps diachronic, basis in Papuan languages, in Central Pomo the cognitive basis is hypothetical, since realis as well as irrealis medial forms can co-occur with final future forms. The occurrence of a realis-future combination indicates the speaker's evaluation that the future event will definitely happen. M. concludes by saying that the striking similarity of the irrealis categories that

have evolved in the Papuan and Central Pomo languages can only suggest a cognitive validity to the realis/irrealis distinction. M. also notes that if such terminology were not used, such cross-linguistic convergence might go unnoticed longer.

Realis and irrealis in Caddo
by Wallace Chafe, University of California, Santa Barbara

Chafe describes the realis/irrealis pronominal prefixes that are an obligatory feature of the verb in the Caddo language. Contexts in which the irrealis prefixes occur are: yes-no questions, negation, prohibitions, obligations, conditionals, and with a number of prefixes expressing the more unusual categories of simulative, 'as if', infrequentative, 'seldom', and the admirative which expresses surprise. Contexts in which the realis prefixes occur include: question word questions, futures and imperatives. So the Caddo realis/irrealis distinction is quite different in semantic content to that found in Papuan languages and Central Pomo.

A functional theory of complementizers
by Zygmunt Frajzyngier, University of Colorado

Frajzyngier assumes that the category COMP, as defined in formal theoretical frameworks like Government-Binding Theory or Generalized Phrase Structure Grammar, is a valid structural component of the sentence. Drawing on evidence taken from English, French, Polish, Guider (Central Chadic), Lele (East Chadic), and Mupun, F. proposes that one of the primary functions of COMP in sentence structure is as a place-holder for modal expression. This hypothesis by F. explains, for example, why SUBJ–AUX inversion in the embedded interrogative is ungrammatical in English, i.e. 'I asked whether {*should he / he should} go'. Under F's hypothesis 'whether' is COMP and marks interrogative modality, the same modality that is marked by SUBJ-AUX inversion. Therefore, since the use of two markers for the same modality would be redundant, only one device coding interrogative function is used in the interrogative clause.

Mood and modality across languages by Evelyn Ransom

Ransom proposes defining modality cross-linguistically in terms of two basic modes, which she terms 'information mode' and 'evaluation mode'. These two basic modes are then divided into further sub-modes. The information modes are defined in terms of restrictions on the content of the proposition and the evaluation modes are defined in terms of a scale of alternatives.

Apprehensional epistemics
by Frantisek Lichtenberk, University of Auckland

Lichtenberk discusses a type of modality which he terms 'apprehensional-epistemic'. L. claims that this type of modality has both epistemic and attitudinal meaning. He also claims that it is not a case of polysemy but of mixed modality. L. then presents arguments that the modal marker *ada* 'lest' in To'aba'ita has both epistemic and attitudinal functions. L. also compares the semantics of 'lest' forms in other languages, for example, in Diyari, Martuthunira, Hua, Czech, Classical Greek, English, and Fijian. On the basis of evidence from these languages L. proposes that the likely diachronic development of apprehensional-epistemic forms is: precautioning > 'fear' > apprehensional-epistemic.

Moods and MetaMessages: Alienation as mood
by John Haiman, Macalester College

Haiman's paper is perhaps the most innovative with respect to the topic of the symposium. H. proposes that there are other types of propositional attitudes that could reasonably be termed 'mood' but fall outside of what is generally accepted as belonging to this category because they are not codified or grammaticalized in language. The propositional attitudes that H. discusses include: the sarcastive, that mood which expresses the speaker's belief that the content of his message is not only false but ridiculous; the guiltive, that means of expression whereby the speaker makes the hearer feel like a worm; and the mass productive, i.e. by repeating something it makes it true. H. notes that, while sarcasm is highly coded in a language like English, in a Papuan language like Hua there is no evidence that the

concept exists at all. In conclusion H. speculates on why these more enigmatic means of signalling lack of speaker's commitment have not achieved the status of the subjunctive or interrogative, for example. Perhaps it is because we as humans cannot (yet) encode modalities at this level of abstraction.

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First Asia International Lexicography Conference

Manila, Philippines
October 5–9, 1992

Les Bruce

The First Asian International Lexicography Conference brought together Asian and European scholars from 16 countries. The main attraction was Professor Ladislav Zgusta, the keynote speaker. Professor Zgusta, from the Center for Advanced Study at the University of Illinois, addressed the conference daily on current topics of interest to lexicography. The topics he selected for us were *Prototype theory, New definitional styles in lexicography, Recent decompositional approaches to lexical semantics, Recent political tendencies and lexicography.* and *Expected future developments in lexicography.* Throughout his presentations he emphasized that a good definition is not a strict, logical formula, but more like an explanatory mechanism. He praised the precision of Wierzbicka's definitions, albeit debating their perspicuity.

The widespread interest in Learner's dictionaries was the other recurring motif. Professor Zgusta identified the trend of including more and more grammatical information in modern dictionaries, such as collocational potentials in the new Combinatorial Dictionary of English.

The professor was optimistic about the future of lexicographical activities. The computer will become more and more a factor, leading to electronic dictionaries; he was cautious about the possibility of machine translation, but would not count it out of the future. Now that many schools of linguistics in North America are turning away from the autonomous, abstract syntax of the 60's, we may see a resurgence of developmental work on M.T.

Some 31 papers were presented by other delegates. A primary aim of the conference was to bring together local scholars around Asia.

This goal was clearly achieved as the following list of presenters demonstrates.

Dai Qing Xia (China)
Peri Bhaskararao (India)
Husen Abas (Indonesia)
Abdul Jalil Faisal (Indonesia)
Harimurti Kridalaksana (Indonesia)
Noresah Bte Baharom (Malaysia)
Asha Doshi (Malaysia)
Rita Lasimbang (Malaysia)
C.M. Bandhu (Nepal)
Tej R. Kansakar (Nepal)
Ernesto Constantino (Philippines)
Suwilai Premsrirat (Thailand)
Nguyen Trong Bau (Vietnam)
Vuong Toan (Vietnam)

Scholars from outside the Asian area proper contributed also. Some of these included SIL members Charles Grimes and Jennifer Lee (Australia), Richard Brewis (Indonesia), Leonard Newell and Ronald Krueger (Philippines), Peter Wang, and myself, Les Bruce (USA). Others representing universities and commercial organizations included Margaret Sharpe and Susan Butler (Australia), Curtis McFarland (Japan), Richard Benton (New Zealand), and David Zorc (USA).

The success of the conference demonstrated how much activity and interest there is in lexicography throughout Asia. A majority of the papers were reports on dictionaries and other projects related to lexicography. Dr. Pineda, Chairman of the Commission on the Filipino Language, agreed to coordinate an ongoing newsletter to keep us abreast of lexicographical activities in Asia including early planning for another conference in three years.

The planning committee headed by Leonard Newell, Andrew Gonzalez, and Ponciano Pineda will be attending to the publication of the papers presented in the conference. That committee and the three sponsoring organizations—Commission on the Filipino Language, Linguistic Society of the Philippines, and the Summer Institute of Linguistics (Asia Area Office and Philippine Branch) are to be congratulated for a fine conference. They all appreciated the original impetus for the conference in the person of Lou Hohulin, one of many of Dr. Zgusta's former students. ■

REVIEWS OF BOOKS

Songs of Nepal: An anthology of Nevar folksongs and hymns. By Siegfried Lienhard. (Asian Studies at Hawaii, 30.) Center for Asian and Pacific Studies, University of Hawaii: University of Hawaii Press, revised edition 1984. 221 pp. $17.00

Reviewed by Margaret H. Daly

Summer Institute of Linguistics, Mexico Branch

According to Lienhard,

> The main purpose of this book, which is a shortened, revised version of my *Nevārīgītimañjarī: Religious and Secular Poetry of the Nevars of the Kathmandu Valley* (Stockholm, 1974) is to present to a broad public material on the thought of the Nevars, their way of life and religious beliefs, their history and folklore. Most of the songs included, religious hymns as well as folksongs and ballads, are still very popular among the Nevars, and much care has been taken to include...the greatest possible variety of songs... (Preface).

The book is divided into five main sections which include: I. Religious Poetry (songs 1-25); II. Songs about Love and Marriage (songs 26-76); III. Epic Poetry (songs 77-97); IV. Didactic and Enigmatic Verses (songs 98-100), and finally, the Nevārī text of the poems. Lienhard uses the terms "poem" and "poetry" interchangeably with "song" and I will do the same.

A persistent student desiring to know more about the customs and culture of the Nevars of the Kathmandu Valley of Nepal will find a rich source of material gathered here in one place, in many cases from texts which until publication of this volume were available only in manuscript form. An extensive bibliography is also given, including ten works written in the Nevārī language.

Nevar is a Sino-Tibetan language, although Lienhard identifies the language by saying:

> The term Nevārī, which is so common in Western philology and linguistics, has never been current in Nepal. Even today Nevārī-speakers call their language *Nepālbhāṣā*, i.e., language of Nepal... Before the final conquest of Kathmandu...in A.D. 1768, Nevārī was the state language in the whole of the Valley, which, by

> many of its inhabitants, was considered as being Nepal in the true sense of the word... It is significant that even in these days ordinary Nevars, when speaking of Nepal, in fact mean the Valley... (p. 3).

Perhaps of most interest to modern linguists is the extensive vocabulary introduced by the wide range of subject matter in the poems. The songs were written over a period of three and half centuries, from A.D. 1560-1574 to 1881-1911, or in some cases, even later.

> Linguistic and stylistic evidence have rendered good service when trying to ascertain...the age of a poem. Fortunately, many compositions are dated or, in their concluding lines, mention the King of Rāṇa during whose reign the respective hymn or poem was composed. Direct dates are either stated exactly, naming, as is usual in colophons, the year, the bright or dark half of the lunar month, and the day of the week... (p. 6).

Material contained in the poems will be of interest to students of Sino-Tibetan culture.

> Religious poetry naturally abounds in descriptions of deities...but not infrequently this poetry presents itself as an invocation or prayer... Among the songs about love and marriage we find both lyrical and narrative poems... The thirty-nine poems preceding...(numbers 26-64) deal with love and matrimony from almost every conceivable aspect... The ballads, rice-transplantation and historical songs, treat original matter...some of the rice-field songs were originally rain-charms later elaborated into ordinary songs... Other favorite topics in the ballads are extraordinary and unexpected events such as journeys (usually to Tibet), pilgrimages, or sudden death (pp. 7-9).

> Of the utmost importance are the various details related to folklore, social obligations, and customs, which, in fact, are but rarely separable from religion... Particularly valuable for our knowledge of Nevar society are the accounts of engagements, marriage, married life with its frequent collisions between...the husband's first wife, and...his second, of divorce, funeral rites, and *satī*... (p. 12).

On the whole, I found the book extremely interesting from a linguistic or cultural anthropological point of view, but for an ethno-musicologist it was disappointing because only two of the songs have been transcribed. It is also unfortunate that "the glossary and many notes of a purely text-critical character have been omitted from this abridged version as being of interest only to the specialist reader" (Preface). There are many instances where either no English

translation is given (for example, see the last word of the previous paragraph which is evidently a borrowing from a Hindu word for the immolation of widows) or where only by following a trail of footnotes which direct one to other footnotes or to a single word lost on a page of intricate descriptions, can one find the meaning of a given vernacular word important to one's understanding. Perhaps these words are common knowledge to students on Sino-Tibetan languages, but not to the "broad public" audience Lienhard wished to reach. ■

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Correction to review of When Verbs Collide (NOL, Aug. 1992), by Mike Cahill:

Arnold M. Zwicky's article, **What are we talking about when we talk about serial verbs**? should not be classed with others as "primarily theoretical, questing for the Holy Grail of the perfect definition of SVC's". Though he does try to bring some order to the phrase "serial verb", his point is that "serial verb" is not a formal **term** of linguistic theory at all, but the label for a diverse collection of problematic phenomena. (MC)

* * * *

Talking voices: Repetition, dialogue, and imagery in conversational discourse. By Deborah Tannen. Studies in Interactional Sociolinguistics 6. Cambridge: Cambridge University Press, 1989. Paperback $16.95; Hard Cover $54.95

Reviewed by Karl J. Franklin

SPSIL, Kangaroo Ground, Australia

This book is on discourse with a difference. It is not related, except in a most general way, to what SIL students learn about linguistics and discourse in Longacre, Grimes, Larson, or even Van Dijk, and Halliday and Hasan, although the latter three are cited in Tannen's bibliography.

Tannen acknowledges in particular the works of R. Lakoff, J. Gumperz, W. Chafe. A. L. Becker and P. Friedrich. She is concerned with involvement in discourse (Ch. 2), repetition (Ch. 3), the construction of dialogue (Ch. 4), imagery (Ch. 5), and oratory (Ch. 6). The final chapter (7) is called "Afterward: Toward a humanistic linguistics," consisting of a brief essay extolling linguistics as a scientific, humanistic and aesthetic discipline (p. 197).

Tannen's study can be of help to translators because it focuses on aspects of discourse that involve cross-cultural communication, a continuing interest of hers (Tannen 1984). She includes the dimension of aesthetic response (13) in the interaction of the speaker and the audience.

The involvement strategies which Tannen proposes for conversation are: (1) rhythm, (2) patterns of repetition and variation of phonemes, morphemes, words, collocations, discourse sequences, (3) stylistic figures of speech that include indirectness, ellipsis, tropes, dialogue, imagery and detail, and narrative. Each of these are illustrated.

Tannen says that "the play between fixity and novelty [make] possible the creation of meaning" (p. 37). In other words, there are certain linguistic patterns and conventions that are fixed but there is also a creative enterprise on the part of the discourse participants. She also claims (p. 47) that "few studies have focused on repetition in

conversation or other non-formal texts." Apparently she has not studied the various texts which numerous students of Grimes and Longacre have been analyzing for a number of years. The functions of repetition in discourse include: production, comprehension, connection, interaction, and coherence. Again, examples are given for each of these.

Chapter 4 examines how dialogues are constructed in conversation. Tannen claims that the distinction between direct and indirect quotations is fuzzy (p. 99). Therefore even so-called "direct" quotations involve creations on the part of the speakers and no one can speak another's words without some distortion. She claims that "uttering dialogue in conversation is as much a creative act as the creation of dialogue in fiction and drama" (p. 101).

Some of the ways in which reported speech is constructed in dialogue is by means of: 1) representing what wasn't said; 2) instantiation, i.e. by making the dialogue seem pertinent to the occasion; 3) summarizing, i.e. giving the gist of the dialogue; 4) choral response, i.e. attributing the dialogue to more than one speaker; 5) inner speech, i.e. reporting the thoughts of one's self or someone else; 6) listener supplying lines; 7) fade-out or fade-in with voice quality; 8) vague referents; and 9) by adding nonhuman speakers.

All of these features are illustrated with dialogue in English, and some with Greek and Brazilian narratives as well.

Imagery and detail (Ch. 5) also play an important role in conversation. For example, music often influences the simultaneous creation of meaning through the senses and images. One imagines something because of the participation. Tannen gives various instances of details and lists to show how imagery and detail are reinforced or created.

Chapter 6 (Involvement strategies in consort: literary nonfictional and political oratory) focuses on an involvement strategy. It incorporates repetition, constructed dialogue, and imagery and details (p. 167). An interesting example of political oratory is from a speech by Rev. Jesse Jackson delivered at the 1988 Democratic National Convention. Tannen demonstrates the use of repetition, appeal to common ground, the use of dialogue to anticipate and animate others' points of view, details and images from Jackson's childhood,

common and elaborate metaphors, parallelisms of contrast, and prosody.

Talking Voices is a book that SIL fieldworkers should be aware of and that translation and grammar consultants should read. It will not be as structurally oriented as they are used to, nor will it contain all of the methodological apparatus that they would like. However, it will be stimulating and it will demonstrate clearly that conversational discourse is a highly creative act and that a clear grasp of cultural and sociolinguistic dimensions are necessary in effective field work and translation.

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The Cambridge encyclopedia of language. By David Crystal. Cambridge: Cambridge University Press, 1987. Pp. 472. Hardback $49.50, Paperback $24.95.

Reviewed by Genevieve M. Hibbs, Ph.D.

Want a well-designed, general purpose tool that even lay people can easily understand? This is a book that could find a place on the coffee table, while also providing research and technical data.

The appendices are a delight and include:

— glossary (1500 words and including cross references), with an introduction to its own rationale, conventions and abbreviations.

— special symbols and abbreviations (non-phonetic symbols and abbreviations ("with references where different sources cited use the same symbol with different meanings"); phonetic symbols "...all the phonetic symbols illustrated in this book, with the addition of some variant forms;" diacritic and other conventions (with examples of the symbol's use where appropriate)).

— table of the world's languages (omitting those with fewer than 10,000 speakers, but referring to Voegelin's 1977 *Classification and Index of the World's Languages* "for further details, and in all cases of omission" from the table given here).

— further reading (with practical and potentially really useful notes).

— references.

— index of languages, families, dialects and scripts ("cited in this book").

— index of authors and personalities.

— index of topics.

This is not the reference for detail of the specialist's knowledge, but it does provide excellent background and related material.

Rating: Highly recommended. ■

Language and the politics of emotion. Edited by Catherine A. Lutz and Lila Abu-Lughod. 1990. New York: Cambridge University Press. Pp. 217. Paperback, $14.95.

Reviewed by Jim Lander

East Africa Group

Language and the Politics of Emotion is the first in a Cambridge University Press series, *Studies in Emotion and Social Interaction*. It is a book by anthropologists for anthropologists. Accordingly, the language is steeped in that field's vocabulary. Readers whose

expertise lie predominantly in other fields might find the going a little difficult. However, those who persevere will come away with a new understanding of emotions in the human experience.

The editors offer nine essays supporting their premise. Studies originate from India, Fiji, the United States, Egypt, Senegal, and the Solomon Islands. The essays offer new insights into emotion, as well as suggesting ways in which further research may be conducted.

The chapters are original essays by anthropologists occupied with the relationship of language, emotion and power. These authors suggest that the study of emotions should focus on society's politics rather than on the psychology of individuals. The essayists hold that emotional discourse and discourse about emotion are intertwined with societal power structures and struggles. Therefore they also propose that studies on emotion be discourse-centered.

Emotion, according to the editors, is a relatively new area of inquiry for anthropologists. Until recently anthropologists have assumed emotion to be universal. That is, that anger feels and means the same thing in every culture. This work challenges those beliefs and signals a change in the anthropologists' thinking.

The essays do not imply that emotion is not located in the psyche and natural body. Instead, they argue that the reality of emotion is also social, cultural, political, and historical. The editors and contributing authors believe a shift in the focus for the study of emotions would illuminate our understanding of emotions. Their case seems to have merit.

As with any compilation of essays, some articles seem more useful than others. Catherine Lutz's discussion is the most interesting to this reviewer. Other readers might prefer different articles. However, Lutz's essay most clearly illustrates the theme this book is supporting.

Readers on a tight time schedule should start with the Introduction, then move to the article by Lutz, then on to those by Arjun Appadurai, Judith Irvine, and Lila Abu-Lughod. If time allows, the remaining articles add to those already suggested.

This book's layout follows the frustratingly inconvenient fashion of the times, using end notes in lieu of footnotes. References appear with

each essay, as they should in works of this type. The last eleven pages contain a useful general index which includes referents.

A brief review of each article follows.

Chapter One—*Introduction: Emotion, discourse, and the politics of everyday life*, by Lila Abu-Lughod and Catherine A. Lutz.

As the title implies, the first essay serves as the introduction to the rest of the book. The coauthors lay out the book's theoretical foundation and preview the remaining essays.

The theoretical foundation of the book, state Lutz and Abu-Lughod, is contrary to common thinking about emotion. They assert that emotion is not universal. For example, anger does not feel, nor mean, the same in every culture. Instead, emotion is a sociocultural construct. It is, therefore, subject to sociocultural analysis.

The editors argue that the proper focus of such analysis is on emotional discourse and discourse about emotion. The essays in their work, they add, offer ways for carrying out such inquiry.

Discourse, as used in this book, is a specialized term. The editors believe that discourse is not a well defined term in academia, therefore, they carefully explain their usage. Readers wishing to get the most from this book should pay close attention to the discussion in this section of the chapter. Linguists, who are likely to have a more definite understanding of discourse, should be all the more attentive. The usage is more anthropological than linguistic.

Chapter Two—*Shifting politics in Bedouin love poetry*, by Lila Abu-Lughod.

In this chapter, Lila Abu-Lughod examines a discourse sample selected from her own work among Egyptian Bedouins. The discourse she examines is a Bedouin love poem recorded on cassette.

Abu-Lughod describes how, in the past, love poems have played a role in the defiance of young men and women to the power of elder men. The poems persist in the face of changing economic and social structures. However, the new structures now work to deny women their traditional avenue of defiance. Young men retain this particular means of rebellion.

The author also shows how the poem, an emotional discourse, was used in differing contexts for differing purposes. In one instance, she suspects the recording was played as a means of resisting her departure from her host. In another setting, the poem was played as a means of exercising male control over the women in a family.

Chapter Three—*Moral discourse and the rhetoric of emotions*, by Geoffrey M. White.

White examines a social practice from the Solomon Islands known as disentangling. He shows how this form of discourse, which has an overt purpose of making bad feelings public, also serves to affect reconciliation between alienated parties. White also demonstrates how a speaker can use disentangling settings to establish his or her own moral superiority over those with whom he or she is in conflict.

White seconds one of Abu-Lughod's observations in chapter two. He warns linguists and anthropologists that a single discourse can have multiple purposes. This idea merits careful investigation.

Chapter Four—*Engendered emotion: Gender, power, and the rhetoric of emotional control in American discourse*, by Catherine Lutz.

Lutz presents her analysis of discourses about emotion by American men and women. In her analysis Lutz concludes that the discussions about emotions are only superficially about people's internal state. In reality, they are about social life and power in human relations. Lutz contends that talk about emotions is used as a means of men's control over women in American society. In spite of the feminist anger which underlies the article, readers who care to do so are able to glean keen insights into emotion and power in social settings.

Chapter Five—*Topographies of the self: Praise and emotion in Hindu India*, by Arjun Appadurai.

Appadurai analyzed praise in Hindu India. Like Abu-Lughod and White, he shows how discursive forms differ in meaning in differing contexts. He also demonstrates how those meanings are put to different uses in social settings and relationships. For example, praise is used to flatter politicians, but beggars use praise to coerce.

Appadurai supports the opinion appearing throughout the book that emotion is cultural. He recognizes that Hindi theories of emotion

conflict with Western sensibilities. He argues that Western conclusions that Hindi emotional practices are excessive, or shallow, no longer hold up under the new methodologies of investigation presented in this book.

Chapter Six—*Shared and solitary sentiments: The discourse of friendship, play, and anger in Bhatgaon*, by Donald Brenneis.

Donald Brenneis reasons, as do White and Appadurai, that indigenous theories of emotion must be taken into account. Indigenous beliefs carry with them informative value about emotional discourses.

Brenneis also emphasizes that the audience's role in emotional discourse must not be ignored. He notes that in some discursive forms, such as the theater, the effect on the audience is of greater concern than that of the speaker's emotion.

In line with that, Brenneis' research among the Hindi-speaking Indians of Fiji reveals that people who are excluded from particular social events may be denied experiencing emotions associated with those events. In his example Hindi women may be prevented undergoing emotions associated with social settings because they are excluded from those settings.

Chapter Seven—*Registering affect: Heteroglossia in the linguistic expression of emotion*, by Judith T. Irvine.

Irvine analyzes discursive forms between two social classes in Senegal. She contends that usage variations, or registers, exist in many languages and that these registers have an affective dimension.

Irvine contends that registers impact the social images of the speakers. For an example not from her essay, a college professor is associated with a language use which is considerably different than that which is associated with a Kentucky coal miner's use.

Irvine's example contrasts the language use of Wolof nobles and griots, a caste of bards. Irvine demonstrates how these language variations define relationships of power among the Wolof.

Chapter Eight—*Language in the discourse of the emotions*, by Daniel V. Rosenberg.

Rosenberg's essay is unique among this collection. He provides an overview and critique on earlier anthropological analysis of emotion and personhood. He points out how prior methodologies have skewed insights, thus, making it difficult to distinguish methodological differences from cultural differences.

Finally, he suggests areas to which future investigators should pay closer attention, such as distinctions between semantics, reference, pragmatics, and ideology.

Chapter Nine—*Untouchability and the fear of death in a Tamil song*, by Margaret Trawick.

This essay springs from a tape-recorded song performed by an untouchable Tamil woman from southern India. Trawick understands the song as representative of the young woman's concerns over societal inclusion and exclusion; poignant topics in caste societies. The singer's language deviates from local grammar and social codes. Trawick argues that the young woman's deviation in codes challenges the caste system by means of this strategy.

In conclusion, this book introduces its readers to an emerging area of anthropological investigation. The book lays out its premise well. It then proceeds to illustrate that premise with equal aplomb. Those linguists who mingle anthropology with their research and production may find some useful insights between the covers of this work. ■

Propositional Attitudes: The Role of Content in Logic, Language, and Mind. CSLI Lecture Notes No. 20.
Center for the Study of Language and Information.
By C. Anthony Anderson and Joseph Owens. Stanford University
Pp. xvi, 342. $16.95

Reviewed by R. J. Sim
Nairobi

Since 1984, CSLI Publications has made an important and distinctive contribution to the serious, formal study of theoretical perspectives on natural language, often with information theory or computational aspects in focus.

As its title conveys, this present volume is focused on issues of propositional attitude, covering the question of the various attitudes speakers hold towards the information they communicate. The topic is explored from the perspectives of formal logic, philosophy of language, and philosophy of mind.

Each paper is a careful argumentation of (sometimes quite subtle) theoretical issues. Logicians have (for good reason—namely, to limit the scope to something manageable) generally considered language as a logic, and have neglected its social and communicative aspects; in other words, they have dealt with sentences, to the neglect of actual utterances. Several papers in this volume have begun to bring a new emphasis by raising the societal side. However, as already said, the contents are not for beginners to formal semantics, but there is some interesting material here.

The papers were presented at a conference of the same name held in University of Minnesota, 14-16 October, 1988. Of the contributors, Hans Kamp, Robert Stalnaker and John Searle will be the best-known to readers of *Notes on Linguistics*; all fourteen authors are professors of philosophy rather than linguistics, which ought to be an adequate signal that this volume is not for those who are not attracted to formal linguistics and like their study of meaning to be readily digestible.

The following comments summarize the papers in the volume:

Quine and Quantifying In (Kit Fine, pp. 1-25) reconsiders Quine's arguments against combining both quantification and intension by data examining logical necessity.

Prolegomena to a Structural Theory of Belief and Other Attitudes (Hans Kamp, pp. 27-90) follows up the author's recent important work in Discourse Representation Theory (DRT). DRT attempts to provide a calculus which can be applied to the explication of meaning in discourse. Discourse representation structures (DRSs) are (partial) information structures which are linked in a 'web' with subsequent DRS's. This provides a more complex history of the cumulation of information, and of the developing interpretation, than do most methods of propositional relationship. The present paper elaborates DRT to cope with belief and desire by pairing DRSs with *mode indicators* which formally represent and pass on the attitudes they encode. While DRT will have some interest to students of discourse, it is likely to appear a rather cumbersome notation to many readers.

A Study in Comparative Semantics (E. LePore & D. Loewer, pp. 91-113) compares the approaches of Gottlob Frege and Donald Davidson to propositional attitudes. Both were committed to a truth conditional approach and to compositionality, but Frege followed an intensional, and Davidson an extensional logic. The authors find that objections to each of these can be countered, and that the superiority of either approach remains to be demonstrated.

Wherein is Language Social? asks Tyler Burge (pp. 113–130), in approaching the role of social context in construing interpretation. He concludes that speakers defer to others in the community in forming their definition of word-meaning, and at the same time form their individuated concepts and meanings through social interaction. This paper could spark off further interest in the relationship between pragmatics and conceptualization.

Narrow Content (Robert Stalnaker, pp. 131-145) picks up earlier ideas by Tyler Burge and Brian Loar which suggest that a notion of **narrow content** helps to explain the access we have to our internal mental states, which among other things, bear upon our attitudes to propositions/situations that we entertain. Narrow content is adduced to support the attitudinal content, over against the propositional content of such sentences as 'Water is the best drink for quenching thirst'. He finds Loar's notion of narrow content to be preferable.

Cognitive Access and Semantic Puzzle (Joseph Owens, pp. 147–173) also deals with the explanation that narrow content offers for this inner access coming to different conclusions. The term itself might be useful; my first impression is that it is not going to prove of substantive value, however, and that consideration of the ways cognition interacts with the linguistic and non-linguistic context is likely to be more fruitful in the long run.

J. Wallace and H. E. Mason, ***On Some Thought Experiments About Mind and Meaning*** (pp. 175-199), attack the presupposition of **short content** of propositional attitudes, or, in other words, of their determinacy, and emphasize the need to recognize the social basis of language. Long-standing interest in both structural and lexical ambiguity seems to have borne results in recent pragmatics work which looks into indeterminacy in language, undoubtedly an area worth further thought.

Belief and the Identity of Reference (Keith Donellan, pp. 201-214) and ***A Millian Heir Rejects the Wages of Sinn*** (Nathan Salmon, pp. 215-247) both defend the direct reference thesis (that proper names refer purely in virtue of their being tags or labels), against the criticisms brought by Saul Kripke. A neutral bystander seems destined to watch a lot of play yet!

The Mode of Presentation Problem (Stephen Schiffer, pp. 249-268) argues that belief is not a two-place relation between a subject and a proposition. After proposing and rejecting **modes of presentation** of the things beliefs are about, as an intermediary construct, he considers a revision of propositional theory, in which propositions are **deflated** to the status of a useful metalanguage construct, but are not a theoretical prime, and even contemplates the abandonment of compositionality. This would be a radical departure; it remains to be seen how far Schiffer himself will follow it.

Consciousness, Unconsciousness and Intentionality (J. R. Searle, pp. (269-284) and ***Consciousness and Intentionality: Robots with and without the Right Stuff*** (Keith Gunderson, pp. 285-324) both argue that attempts to understand intentionality without giving due place to some notion of consciousness, are ill-conceived. It all sounds rather obvious, but philosophy of language seems to be a long way from achieving this. ■