TESOL IN THE '70's: Some New Directions

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In 1962 MIT published an English translation of a series of papers by the emminent—but at that time virtually unknown —Soviet scientist, Lev Vygotsky. *Thought and Language,* as the book is titled, had been written by Vygotsky during the late 1920's and 1930's—a time of intense Stalinist paranoia—and, like the fate of many of his contemporaries' works, as well as persons, it was suppressed by the government. His research lasted a scant fourteen years and in 1934 Vygotsky died at the age of 38.

I have begun my discussion with the subject of Vygotsky's work because it seems to me that the time of the book's publication in the West marks a distinct chronological break between the three crucial periods of research and theory in second language teaching; Vygotsky's findings, too, I believe, will be proven to be of great importance in this field. It is unfortunate that his work arrived so late. Had it appeared in the west at an earlier date and had the times been more propitious for its reception, I'm sure that the last twenty years' controversy in language teaching circles would have been much less unrewarding.

Let me illustrate for a moment. The period before 1940 could, I believe, be characterized as a pre-scientific period—a very long one, at that; many centuries as a matter of fact—during which teachers and scholars expressed their opinions about what language is, and how it should be taught. Articles,

treatises and even books were written on the subject but none of these were based on much more than haphazard observation, not very fruitful teaching experience, or—what is worse—*a priori* reasoning. The writers' intentions were no doubt admirable, but their methods and reasons were—it now seems—unscientific and subject to all kinds of interpretation.

Just preceding the period of the 1940's, anthropologists-turned-linguists (like the great Leonard Bloomfield) and even grammarians (such as Charles Fries) turned into linguists, too. Fries, speaking as a linguist, then proceeded to advise the government on matters of language teaching. It was a time of national emergency; the second world war had broken out and the American government was suddenly confronted with the urgency of training thousands of interpreters.

Somehow the wartime language teaching programs improved. The fame of the methods advocated by so-called linguistic specialists spread. Eventually the term "linguistic method" was coined, though certainly not by linguists who knew anything about the subject. "Intensive" might be a more accurate way of describing these programs; anyway, as such they represented nothing new at all: they were no more "linguistically based," in most cases, than the child with his tutor or governness. There was simply greater exposure to the target language than had been possible previously for so many students at the same time. And probably greater motivation (America had a war to win).

During the late 1950's and 1960's the military action of the war was replaced by a fierce linguistic controversy raging between the elders (the so-called "structuralists") and the youth (the "transformationalists"). The structualists, parroting Professor Fries' dicta that teaching materials should be based

on a description of both the native language and the target language—a contrastive analysis—, demanded that patterns should be drilled again and again to insure memorization of grammatical forms.

The transformationalists, while not specifically propounding any method or approach, argued that these structural patterns are no more than mere *surface* representations of underlying forms and do not, therefore, show contrast at all. Students, the transformationalists argued, do not really develop communicative ability by these drills: they simply become well-trained parrots. There is obviously much truth in this statement, for we know that thinking and talking are cognitive, not behavioral, phenomena. Chomsky, using the *langue et parole* idea of the Swiss linguist Ferdinand de Saussure, then proposed the *competence and performance* criteria.

Now this point is critical, I think, because it generates the question "What is competance and how is it achieved and how may we represent it?" And transformationalists are not begging the question; they have attempted, sometimes very successfully, to represent competance by a generative grammar.

Transformationalists do not claim to be and do not want to be language teachers; nevertheless, it is through them that important material is made available. Competence-Performance criteria and a generative grammar, that is a grammar which maps out the process of speech, are of great use to the language teacher.

The structuralists, on the other hand, supply us with immense amounts of material, lots of good wishes, and a great deal of advice which seems to me to be somewhat meretricious in that it *appears* reasonable, when in fact it is often mere rationalization to justify actual practices in the classroom.

Certainly the premises of contrastive analysis and "overlearning" are built on very shaky, if not very unsound, foundations. (I will return to this point later if you like.)

Last year, at the annual meeting of Language and Linguistic Studies at Georgetown University, a distinguished scholar and teacher referred to the last decade as "the winter of our discontent" in linguistics and second language teaching. Today, the 1970's are beginning a new epoch in language teaching, at least in theory if not in actual practice. Much of this seems to tie in with what the transformationalists have begun to do; namely, to map out huge segments of our linguistic universe, and to account for its operation.

Yet neither the transformationalists nor the structuralists have done much to answer the all important question: how do we learn language? And they cannot, therefore, be expected to tell us how to teach it. This unanswered question has given rise to the formation of yet another academic discipline—language acquisition. I feel certain that research in this field will sooner or later give us the answers or, if not, at least the methods, to solve the problems of second language teaching. While this field of study did not formally begin until the 1960's in the United States, much of the basic research was begun in the Soviet Union in the 1920's. It was done there in the field of psychology. Which brings us back to Vygotsky.

It should not come as too much of a surprise to learn that Russian scientists have long been investigating the way man learns language; after all, language study has been going on there for centuries. It may interest some of you to learn that the first Japanese language school in Russia began in 1772, in the city of Irkutsk. Whether for commercial, political or

military purposes, the Russians have succeeded in and excelled at language teaching. Their recent successes, and I could quote many, are probably due to the fact that there have been—and no doubt are at present—men like Vygotsky at work.

The very feeble and highly unsuccessful attempts by philologists (the precursers of European linguists) and anthropologists (the precursers of American linguists) and other worthies to formulate theories of language in the 19th and early 20th centuries are still the subject of a great deal of humor. Indeed, the speculations of the ancient Indian and Greek thinkers seem much more plausible and intelligent than many of those of our near contemporaries.

It was not until after the first decade of this century that a more or less scientifically-based study of language began, and it was made possible by the discovery—or at least the acknowledgement and use of—the phoneme. Language research then became a rather jealously-regarded enterprise of the linguist who often displayed a single-mindedness unworthy of a scholar, and at times a very cavalier attitude toward those who, in other disciplines, were going about the problem in different ways, using different tools.

Thus the phoneme enabled the linguist to describe the features of language, to analyze it, to compare, contrast and record it. But how to account for it? In the United States most of the material on language acquisition comes from indirect sources—the speech clinician and pathologist, the child psychologist, and even the neurologist until, that is, the last half-decade.

Vygotsky's research in language was but a part of the great work being undertaken by Pavlov, whose behaviorist notions had predicated the bulk of social and psychological

research in the Soviet Union. He was able to begin almost from scratch by direct observation of controlled situations involving language behavior. Not that Vygotsky considered himself a linguist—far from it. He was a psychologist investigating behavior, specifically what is called today "verbal behavior" and how it is acquired and, presumably, how it may be controlled.

Most of the data he used was based on the behavior of children and chimpanzees, whose physical development and physiognomy closely resemble the human and whose physical speech mechanism—the larynx—exactly duplicates man's. His discoveries of the correlations between thought and language at various stages of human and simian development have helped to lay some of the foundations of current knowledge, but—more specifically—Vygotsky's research has greatly helped to define how language differs from, say, the cries of animals, or the babbling of children—though the latter is clearly a stage in language acquisition.

At the age of about two years, the curves of thought intersect with the curves of speech and a new form of behavior —language—results. This phenomena does not occur in animals. They are not able to manipulate audible symbols (and language, it must be remembered, is a *symbolic,* function) though very recent evidence seems to show that they are capable of handling some kinds of primitive visual cues. It should be remembered, though, that this is behaviorist, not cognitive phenomena; it is Pavlovian. Neurological evidence demonstrates that there are probably physiological reasons for the fact that humans can and animals cannot handle symbolic manipulations. This evidence is located in the brain itself. Humans possess what is called an "association cortex," a series

of connecting fibers and nervous tissue, which lies between the visual, auditory and somesthetic centers of the brain: this tissue is lacking in animals and, hence, it is reasoned, they cannot interpret and reinterpret symbols. They can only relate to signs such as specific shapes, colors and simple configurations which may trigger physical motion or sometimes vocal cries.

That animals can and do react to certain types of vocal or auditory stimulation is interesting indeed; in fact, this phenomena suggests that there is probably a pre-linguistic stage wherein animals and humans are roughly the same. But infants become children and learn to talk: animals do not.

Speech pathologists have long known that the development of speech follows along a definite chronological path, and that at a certain age—from 16 to 28 months—speech, genuine speech in the form of a code, a symbolic operation, takes place.

Physiological barriers, such as are caused by accidents or malformations, retard the development of speech; there are cases on record of children who have been isolated from human speech environments for years, after which normal language acquisition takes place quite rapidly, much more rapidly, in fact, than in cases of normal children.

Are there implications here for the second language learner? I would assume so, since there are certain optimal ages for language learning and hence language teaching; these ages are chronologically conditioned and involve physical and mental degrees of maturation. From the purely physical view, as we shall see later, certain vocal operations preceed the development of speech. Is it possible, then, to reproduce these early physical-vocal operations as a kind of preparation for learning a second language? I believe it is, at least to some

degree.

This brings us to the crucial point, the real subject of this talk; the role of SSP clusters or "phonemic phrases" in language acquisition and language teaching. On this point there could be much controversy, and much of it would center on statements made by Professor Chomsky which *seem* to minimize the importance of intonation in his generative-transformational grammar. In his *The Sound Patterns of English,* he has nothing to say about pitch patterns because, "it is clear even from a superficial examination that the contours are determined in some manner by the surface structure of the utterance." I will not argue this point, although I think it is mistaken. When applied to single lexical items alone, Chomsky's claim seems credible. Most of you are familiar with Chomsky's statement that "The fundamental principle of orthography is that phonetic variation is not indicated where it is predictable by general rule." (Chomsky and Halle, 1968. p.49). But these rules of phonological representation are not always easy, even for the native speaker. Note the examples Chomsky offers:

photograph	photography	photographic
telegraph	telegraphy	telegraphic

Even the native hesitates and sometimes stumbles, even though he may know that pitch and stress distribution changes with certain inflections. The point here is that the native learns through hearing—and writing and reading is unnecessary if not actually disadvantageous—while the second language learner, in most situations, cannot be expected to internalize the rules (which, for the most part, have not been codified) and seldom does manage to learn them. Here, I think, is an example where paradigmatic representation—treated, in effect, as intonation drill—would cause the student to internalize these

rules. And certainly tests of a student's competence could be based on his ability to operate these rules by speaking; in such a case, it can be clearly seen how reliable the claim may be that "I can read but I cannot speak." It follows that an inability to handle phonological/suprasegmental rules—in some ways at least—accounts for an inability to understand what is written.

How crucial then is intonation in language acquisition? Let me sketch briefly and badly the development of speech—not as psychological but as a *physical* process. Most authorities agree that there are three successive stages in the evolution of speech. I shall use Martin Joos's terminology because it seems simple, unpedantic and direct: screaming, babbling and talking.

We do not know much about the screaming stage, but could, I think, safely term it a stage of vocalized emotional or physical expression. Screaming may even be a kind of physical exercise in much the same way as kicking, clawing, twisting and humping—that is, a purely motor action performed by various muscles, of which the larynx is but one.

The babbling stage, though, according to nearly all psychologists, is that period in the child's development—usually from eight to eighteen months—wherein a controlled modulation of the breath stream and larynx takes place, and one in which the child begins to assimilate, modulate and reproduce the sounds which he monitors. Children also practice self-monitoring—a very important operation, according to many psychologists, one which is essential to the transition from "inner speech" to "outer speech," to use Vygotsky's term. Self-monitored auditory sounds are later replaced by internal silent dialogues which we use until we become senile. At that

time, we revert to self-monitoring our audible sounds.

These first sounds, of course, are mere noises which—by conscious and unconscious monitoring—become small, ill-defined-at-first intonation curves. That these sounds or, as I shall call them, "SS clusters" have meaning—in that they both signal and trigger responses—is proven. These SS clusters are manipulated by the babbling child and the chimpanzees to express feelings—definable, observable and predictable feelings.

In her book, *Language in the Crib,* Ruth Wier shows us the incredible extent to which children play with their language in a purposeful and creative way. In this study, the child, in his tape-recorded evening monologues, drilled paradigms systematically and created rhythmic and even rhymed sequences which show that what Jakobson and others have called the metalingual and poetic functions of language can be surprisingly well developed at the age of two and a half. Even for a child, according to Charles Ferguson, language is not just communication; it is grammatical analysis and artistry. Here, he believes, theories of linguists must cope with realities so far unexplained by them. The great—perhaps the greatest of all—child psychologist Jean Piaget could be of great help to us here. If only we would listen.

It is here, at this stage of the child's speech development, that a profound change takes place in the human but not in the animal. The child babbles and his vocal and auditory senses interact in such a way that he experiments and adjusts his babbling to model that of a pattern he has already unconsciously learned. These first patterns are suprasegmental, not segmental, because he has not yet learned to discriminate vocalic and consonantal features. Thus this "babbling" becomes a controlled modulation, an imitation of an already internalized

linguistic feature, which the child then reinforces by repetition.

It has been noted again and again by speech pathologists that pitch, or, if you will, *intonation* discrimination preceeds segmental discrimination in children. It has also been found in many tests that speech-defective children as well as adults are much lower in pitch-discrimination ability. These seem to be a lesson here for the second language teacher, too, for most teachers concentrate on the segmental features of the second language, thus leaving the more important foundations of the language totally neglected. This neglect is directly responsible for much of the failure in language teaching in this country.

Philip Lieberman, in *Intonation, Perception and Language,* writes, "At some point in the development of speech, intonation takes on a linguistic relevance." [even] . . . When the total patern—the phonetic form together with the intonational form—is effected, the intonational form dominates the learner's response." Tt this very early stage, then, it is clear that supra-segmental features are central, and not peripheral, in language acquisition.

Only rather recently have these intonational contours become the subject of much investigation, let alone classification and analysis. Lieberman calls them "phonemic phrases," and shows that they are specific characterizations, that they may constitute a sentence or act as constituents of a sentence and —very important for our study—may cause a speaker to divide sentences into breath groups, to pause, or even to rephrase utterances. Even the traditionalist Daniel Jones observes that pauses for breath are normally made at points where pauses are necessary or allowable from the point of meaning. This certainly indicates an unconscious predilection and feeling for the internalized phonemic phrase.

One of the most noteworthy studies of intonation to date is that of Bierwisch (1965), a German linguist who, using the German language, demonstrates that it is quite possible to generate an intonation contour if only the superfical syntactic structure, primary accents, and what he calls "syntactic intonation markers" (SIM) are considered. He defines intonation in terms of pitch contours, and notes that the fundamental frequency of the utterance is the primary accoustic correlate of intonation. He believes that stress is simply an abstract characteristic of a sentence that is determined by its derived phrase marker. This is interesting indeed. Who, for example, has collated intonation features or contours according to frequency or according to deep structure features? You may note here that this view seems to oppose that of Chomsky who, you will remember, states that pitch was somehow determined by the surface structure of the utterance.

It seems to me that the taxonomy of the SS cluster, or phonemic phrase, could be based on the simple features of pitch and terminal juncture. Collated according to frequency and allowing for a free-floating stress phoneme, such paradigms could be of immense value to the second language teacher. We know, for example, that in any language system the incidence and possibility of certain segmental combinations is strictly limited; speakers learn this unconsciously. I think the same thing can be said for the suprasegmental features as well.

It seems to me to be very clear that this is the one area of language acquisition and teaching where a little more research would probably yield what may well be the missing link in TESOL. Research, culled from such fields as linguistics, psycholinguistics, psychology, and speech pathology has supplied us in this decade with information that will enable us to develop

more scientific and effective language programs, and to be better teachers. This is the promise of the '70's.

We have proceeded in this paper from chimpanzees to children, from structural to generative grammar, within the framework of applicability to language teaching. Perhaps, too, we have preceeded from the screaming stages, through the babbling stages, and on to the speaking stages of our work.

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