

SYMPOSIUM NO. 2: THE PSYCHOLOGICAL REALITY OF PHONOLOGICAL DESCRIPTIONS

(see vol. II, p. 63-128)

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Panelists: Lyle Campbell, Anne Cutler, Bruce L. Derwing, Wolfgang U. Dressler, Edmund Gussman, Kenneth Hale, Per Linell, and Royal Skousen

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VICTORIA A. FROMKIN'S INTRODUCTION

The topic of this symposium is a controversial one. We are hopeful that the debate will lead to new insights and understanding and will help to clarify issues which are important to all sides of the argument. We expect new questions to be raised, questions which we are certain will stimulate the search for answers as to the nature of human language and speech.

Throughout this IXth Congress, the complexities of speech production and perception have been discussed. While we have learned a great deal about these phenomena in the 48 years since the first International Congress of Phonetic Sciences, we still have more questions than answers. The heart of our problem is like that of all scientists, "to explain the complicated visible by some simple invisible." (Perrin, 1914) This is the aim of theory construction, the effort to find a simple, elegant, but "true" (or as close to truth as it is possible to get) accounting of, description of, explanation for the complexities of the phenomena of interest. There is, however, no single approach to how one goes about constructing and validating a theory. That this symposium attests to such differences is revealed in the proceedings (vol. II). We do not even agree as to what constitutes a true theory. The disagreements are, of course, philosophical rather than "scientific". One side of the philosophical debate is set forth by the Nobel prize winning geneticist, François Jacob (1977):

"... the scientific process does not consist simply in observing, in collecting data, and in deducing from them a theory. One can watch an object for years and never produce any observation of scientific interest. To

produce a valuable observation one has first to have an idea of what to observe, a preconception of what is possible. Scientific advances often come from uncovering a hitherto unseen aspect of things as a result, not so much of using some new instrument, but rather of looking at objects from a different angle. This look is necessarily guided by a certain idea of what the so called reality might be."

What the reality is constitutes the subject of this symposium. In our case, the reality is a mental or psychological one. We have thus rejected as too confining an earlier definition of linguistics as a classificatory science. (Hockett, 1942) It is no longer enough for a grammar to account for the facts, i.e. the raw data, with the "maximal degree of generalization". The grammar must be a model of the internal grammar constructed by the child; only then will we provide a true description of the language, or a psychologically real grammar.

Even when there is agreement on this aim, different approaches to the job before us are taken. Some linguists and psycholinguists believe that to achieve this goal, it is necessary to test each posited rule in any descriptive grammar to see if it is truly "real". Others suggest that what we are seeking are, rather, constraints on the form of grammars, or a theory of grammar which will answer the question "what is a possible language?" This latter view suggests that with proper constraints any language specific grammar which is permitted by the theory will be psychologically real in that it would be learnable, acquirable by the child when confronted with linguistic data. We all agree that a grammar which is in principle or in fact not "learnable" cannot be psychologically real.

The psychological reality problem did not arise, nor could it have arisen, among linguists such as those who followed Bloomfield in America as they rejected any form of mentalism in linguistics. But even in the early period of the transformational/generative grammar paradigm, the period in which the notion of language as a cognitive system was reintroduced as a legitimate one, there were too few constraints placed on grammars.

I am reminded of the Schachter and Fromkin (1968) phonological analysis of Akan in which final stop consonants /p/, /t/, and /k/ are posited in lexical representation. These

voiceless stops do not surface phonetically in this context. The question that such an analysis poses is whether the Akan child language learner can hypothesize the existence of these final consonants when they never occur in any forms the child hears. Chomsky and Halle (1965) discussed this question a number of years ago.

"For the linguist or the child learning the language, the set of phonetic representations of utterances is a given empirical fact. His [sic] problem is to assign a lexical representation to each word, and to develop a set of grammatical (in part, phonological) rules which account for the given facts. The performance of this task is limited by the set of constraints on the form of grammars. Without such constraints, the task is obviously impossible; and the narrower such constraints, the more feasible the task becomes."

There are no a priori principles which can tell us what the child is capable of constructing and what she is not. We do not know what the mind is capable of, either the adult mind or the immature mind. In fact, the goal of phonological theory is to provide an answer to the questions concerning the kinds of phonological representations the child can construct, and the rules which can relate these to surface phonetic forms, if indeed there is a difference between these levels. This too is a question for which there is no a priori answer.

The task then of establishing constraints on such a theory such that it will delimit the class of possible grammars to those which are psychologically real, which can be, and which are, acquirable by at least some children, is a task facing us all. If this is the general goal for phonological theory, and let us assume it is, then the question of "psychological reality" is a non-question. We need rather to ask of a theory: "Is it correct?" not "Is it psychologically real?" Or perhaps we should say that the answer to these questions will be identical. In other words, a correct theory of grammars will be a theory of psychologically real grammars.

Unfortunately, even if we agree on this, we find disagreements as to what is meant by psychological reality. I have

therefore asked the participants in this symposium to address this question, to tell us their conception of psychologically real phonological theory.

Closely tied to this basic question are those concerned with the kinds of evidence which can be used to show the reality of a grammar, a lexical entry, an abstract segment, a rule, evidence used to validate or invalidate general theories or particular phonological analyses. In a number of the papers presented in volume II a distinction is made between "external" and "internal" evidence. "External" evidence, as I noted in my summary (p. 63-66), included acquisition data, language disturbance, borrowing, orthography, speech and spelling errors, metrics, casual speech, language games, historical change, perception and production experiments etc. (Cf. Zwicky, 1975) Internal evidence, according to those who make this separation, refers, on the other hand, to facts drawn from the grammar itself, significant generalizations, simplicity factors, distributional criteria, morphemic alternations, etc.

There are linguists, including some of the participants in this symposium, who regard external evidence as more worthy of consideration, as data to be more highly valued than internal evidence. It is not quite clear to me why this should be so. And, in fact, it has been argued that if internal and external evidence are contradictory, internal evidence should prevail. (Cf. below for discussion of Gussman's paper.) External evidence is often performance data, either elicited or observed in actual speech or perception. Speech error data are of this kind. Although I have found, in speech errors, evidence for the independence of features as shown in (1)

(1) Target: Cedars of Lebanon Error: ... Lemadon  
where only the value of the feature [nasality] is switched, Klatt (1979) finds "little evidence in the speech error corpus to support independently... movable distinctive features as psychologically real representational units for utterances." While I am not ready to concede to Klatt, let us assume, for the purpose of this argument, that he is correct. Can we conclude from this that a theory of phonology should not represent segments as bundles of features? If we did, we would obscure important

phonological universals in both synchronic and diachronic descriptions; sounds do function in classes, classes which are specified by the features common to their members.

Because the question of internal vs. external evidence has assumed such an important role in discussions on psychological reality, I have asked the symposium participants to present their views on this question.

Each participant has also received one or more questions specific to his or her paper. Let me mention these.

Campbell presents some interesting evidence from Finnish and Kekchi showing the reality of certain posited phonological rules and Morpheme Structure Conditions. He discusses language games played by speakers of these languages. The game data support the rules posited by linguists using internal evidence. Suppose in the language games, these rules were not evidenced. Can one conclude, then, that the P-rules, and MSC's do not exist? That is, what does one do about negative evidence?

This, of course, is not simply a problem that is faced by Campbell, but one faced by all linguists, and, in fact, by all scientists.

Cutler also uses "external" evidence, this time from speech errors, to show that "morphological structure is psychologically real in that English speakers are aware of the relations between words and can form new words from old." She also concludes that "The principles underlying lexical stress assignment are psychologically real in the sense that speakers know the stress pattern of regularly formed new words." This, however, she suggests is in keeping with a "weak" version of psychological reality, which claims simply that speakers can draw on their knowledge of the grammar, as opposed to the "strong" version which would claim that the rules are isomorphic to processes.

It would be interesting to know what kind of evidence would be needed to support the strong version of psychological reality in relation to the posited stress rules of English. What, if anything, does the following error tell us about the psychological reality of the nuclear stress rule?

(Note: for those readers who are not fans of American basketball, Jim West was a famous basketball player with the

Los Angeles Lakers. The meaning of the phrases is paraphrased.)

(1) Target: Jim West Night Game. (The game to be played for the special occasion called Jim West Night.)

Error: Jim West Night game? (the night game played by Jim West.)

Derwing, in his preprinted paper as well as in other of his published works, seems to reject a concept which I hold, i.e. the difference between linguistic knowledge and linguistic behavior. I am therefore interested in how he can find support for psychologically real grammars or rules, given the great variation, including speech errors, false starts, ungrammatical sentences, neologisms, even sounds not ordinarily found in the language that one finds among different speakers of the same language, and even within one speaker on different occasions in both speech production and perception. Is it possible to find exceptionless regularities in behavioral data which permit any generalizations at all? Suppose, for example, one finds five speakers who, to use one of Derwing's examples, relate fable and fabulous, and five who do not. Can we conclude anything? Or should we be constructing individual grammars for each speaker at a single point in time? Or can we conclude instead that, since even one speaker draws certain generalities, the rules which represent them must be psychologically real and permitted by the theory of phonology?

Dressler has distinguished between "naturalness", "productivity" and "psychological reality". How do they relate? Is it possible for a phonological rule to be psychologically real but highly unproductive? And how would such a rule manifest itself. Is there some way that these aspects of language should be delineated in a theory of grammar?

Gussman differs from some of the earlier papers in pointing out that we can not depend on external evidence in our attempts to validate or test phonological hypotheses because it is often the case that different kinds of external evidence are contradictory. It is therefore of interest to know what kinds of constraints he believes should be placed on grammars and how we can

find evidence in support of these constraints. Even while he argues that external evidence may be unreliable, he provides such evidence to argue for phonological representations which some linguists would call "abstract". Is this in itself contradictory?

Hale presents a principle which he suggests is needed in a theory of language, the recoverability principle. How is "recoverability" related to psychological reality? Since the principle refers to an evaluation metric for grammars, i.e. a measure by which we can compare the value of grammars, can the metric itself be used to judge whether a grammar is psychologically real? Or, perhaps even more important, how do we judge the psychological reality of any proposed evaluation metric?

Linell gives us a number of interesting definitions. He defines phonology as "language specific phonetics" and rules as "norms". It is thus not immediately clear what the contents of a theory of phonology as distinct from a theory of phonetics would be.

Finally, Skousen has argued that a linguistic description must be directly inducible from the data. At the beginning of this paper I quoted a statement from Jacob which strongly contradicts such a view. The particular paragraph I referred to ends with a further statement: "[Scientific advance] always involves a certain conception about the unknown, that is, about what lies beyond that which one has logical or experimental reasons to believe." Certainly a linguistic description, in the form of a grammar, should be a "scientific advance", an hypothesis, a theory, which goes beyond the collected data. If Jacob is right, why should stronger or different requirements be placed on linguists than are placed on other scientists? And is it possible for us to discover "new truths", to make "new advances" if we are forced to induce all our hypotheses directly from the data?

These are the questions that have been posed for the panelists. We are sure that there are many other questions from the audience which we look forward to hearing.

Whatever our disagreements, we who are the participants of this symposium agree, as I am sure all in the room agree, that

to whatever extent possible we are seeking the "truth", we are seeking a theory of language, and in particular a theory of the sound systems of language, which will bring us a little closer to understanding the beauty as well as complexity of the abilities of the human mind.

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#### COMMENTS FROM THE PANELISTS

L. Campbell stated his acceptance of the generative phonology goals of descriptive adequacy for particular grammars (which means we should aim at psychologically real grammars) and explanatory adequacy for theories. This requires evidence as to what psychological reality is. Campbell claimed that we cannot find the answer on the basis of internal evidence alone, and one must give greater relative weight to the importance of external evidence. He stated his concept of psychological reality: what is in the head of speakers, i.e. the traditional definition of competence. The more interesting question, he said, is not what psychological reality is, but how do we find out what it is, suggesting that this can only be accomplished by the use of external evidence.

Campbell's answer to the question concerning negative evidence was a simple one: if there is no evidence, there is no evidence. We can conclude nothing. He suggested that a more interesting question concerns counter evidence, which must be used to invalidate theories. He denied the existence of conflicting evidence, despite the reference to such by others. Rather, he suggested that such seeming contradictions are the result of wrong interpretation, theory, or practice.

A. Cutler stated that as she was the lone psychologist on the panel, she would emphasize the "cognitive reality" part of the symposium title by citing some psycholinguistic evidence that prosodic structure is psychologically real. She supported and illustrated her notion of psychological reality by reference to the temporal structure of English, which language is said to exhibit a tendency towards isochrony, in that speakers adjust the duration of unstressed syllables so that stressed syllables occur at roughly equal intervals. She pointed out that there is, however, little evidence that English is physically isochronous; the psychological reality of isochrony is much stronger.

Firstly, English speakers certainly perceive their language as isochronous. In a recent study Donovan and Darwin (1979) presented listeners with sentences in which all stressed syllables began with the same sound, e.g. /t/, and asked them to adjust a sequence of noise bursts to coincide temporally with the /t/ sounds in the sentence. They could hear both sentence and burst sequence as often as they liked, but not together. Donovan and Darwin found that the noise bursts were always adjusted so that the intervals between them were more nearly equal than the intervals between the stressed syllables in the actual sentence--i.e., the listeners heard the sentences as more isochronous than they really were.

Secondly, there is the role of rhythm in syntactic disambiguation. Lehiste (1977) argues that speakers trade on listener expectations by breaking the rhythm of utterances to signify the presence of a syntactic boundary. Durational cues certainly seem to be the most effective at resolving syntactic ambiguities (see, e.g., Streeter, 1978); and recent work by Scott (forthcoming) has demonstrated that boundaries are indicated not merely by

a pause or by phrase-final syllabic lengthening, but crucially by the rhythm--the fact that the foot (inter-stress interval) containing the boundary is lengthened with respect to the other feet in the utterance. Moreover, in a further study of syntactically ambiguous sentences (Cutler & Isard, in press), it was found that speakers tended to lengthen the foot containing the boundary to an integral multiple of the length of the other feet, i.e. "skip a beat" and thus maintain the rhythm.

Finally, there is relevant speech error evidence (Cutler, in press): when an error alters the rhythm of an utterance (a syllable is dropped or added, or stress shifts to a different syllable), it is almost always the case that the error has a more regular rhythm than the intended utterance would have had. In the following examples (syllable omission and stress error), each foot (marked by /) begins with a stressed syllable:

(1) /opering /out of a /front room in /Walthamstow

(Target: /operating /out of a /front room in /Walthamstow)

(2) We /do think in /specific /terms

(Target: We /do think in spe/cific /terms)

The number of unstressed syllables between the stressed syllables is more equal in the errors than in the target utterances. The consistent pattern of such errors supports the notion that isochrony in English is psychologically real: the speakers have adjusted the rhythm of their utterances to what they feel it ought to be.

B. Derwing began his discussion agreeing with Popper (1965) who stresses the importance of the testability of a theory. He then discussed a view which he characterized as that of "autonomous linguistics". According to Derwing, this view holds that there is or may be an idealized natural language system which can be scientifically investigated apart from considerations of the minds and bodies of individual language users. In arguing against such a position, he said that its origins can be traced to a philological notion that a language is an organism complete unto itself and subject to its own unique laws of evolution and change. He referred to a statement of Jespersen that the essence of language is human activity between a speaker and a hearer, and that

these two individuals should never be lost sight of if we want to understand the nature of language and of grammar. Jespersen wrote that words and forms were often treated as if they were things or natural objects with an existence of their own. Derwing agreed that such a view is fundamentally false since words and forms exist only by virtue of having been produced by a human organism. For these reasons, Derwing stated he does not embrace the goal of constructing a theory of language, per se, or a theory of possible grammars.

He suggested that modeling the language user is a better goal, since there can be no doubt that speakers learn something when they learn to speak and understand their language, that they know various things as a consequence of this learning, and that they engage in various kinds of internal activity when they put this knowledge to use. The details of this activity and knowledge are amenable to a wide variety of tests. It is thus not the concept of psychological reality which bothers Derwing, but the concept of autonomous linguistics. In fact, he suggested that the question of psychological reality is debated in linguistics only because there are still a large number of linguists who refuse to admit that linguistics is, or at least should be, a branch of psychology.<sup>1</sup>

Derwing stated that only external evidence can provide definitive answers; such evidence is in fact external only from the standpoint of a theory which ignores it. Both kinds of evidence are useful grist for the same mill.

He concluded by saying that it makes no sense to talk of a true theory of natural language since the object of that investigation probably does not exist. The concept of an idealized, monolithic system of language is a notion we can get along very well without. We can, however, subject claims about human linguistic knowledge and abilities to the test of truth. In this enterprise internal evidence is important and suggestive but hardly conclusive.

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1) In his remarks Derwing did not cite Chomsky (1968) who may have been the first in recent linguistic circles to consider linguistics as "the particular branch of cognitive psychology".

W. Dressler stated that he conceives of psychological reality in the "weak" sense (Cf. Cutler, vol. II, p. 79-85) in that he is trying to account for the competence of linguistic behaviors. His stated approach is to elaborate a deductive theory of natural phonology and a deductive theory of natural morphology, starting from a few basic theoretical concepts. Conflicts concerning naturalness as pertaining to phonology, morphology, the lexicon, etc. would be derived from the theory. Therefore, hypotheses about the psychological reality of these different types of competence would be derived and tested if the intervening variables in each domain of evidence are controlled.

Dressler stated his disagreement with the Chomsky/Halle (1965) statement quoted by Fromkin in which they say the task for the linguist or the child learning the language is similar; the intervening variables for the two are too different for this to be so. Furthermore, he stated that we should not overemphasize child language acquisition at the expense of other kinds of evidence; it is not the privileged domain, and in fact could lead to wrong conclusions. Besides, massive restructuring of the grammar occurs later.

In Dressler's view, external evidence is not extraneous or some sort of supplementary confirmation or disconfirmation, but a central part of the testing procedure. Thus, external evidence can show that an analysis is wrong. He illustrated this with an example from Italian. The masculine article has two forms, il and lo. Phonological and morphological internal evidence suggest overwhelmingly that lo is the basic form. Yet, an Italian asked to give one form in isolation will produce il. Second, the hesitation form, before pause, is il. Finally, change in progress argues for il. These three kinds of external evidence confirm each other and override the internal evidence. The reason is because the techniques for handling internal evidence have mainly been devised for regular phonological and morphological processes and the system of the Italian articles is neither phonologically nor morphologically regular.

E. Gussmann stated that, if phonological descriptions are to be psychologically real, either in the strong or the weak sense, if, that is, they have some kind of correlates in the mind of the

user, then the basic question is how we can check or verify the reality of the proposed description. He suggested more caution in evaluating external evidence, pointing to the surprising and, in some cases, contradictory results in direct experiments. Specific examples of this are shown in experiments conducted related to the English regular plural formation rule. In some experiments, subjects responded only 50% in the predicted way, but in others 100% of the forms were those predicted by the regular rule. These experiments say little about whether the English plural rule is productive or psychologically real, but do call for a theory of linguistic behavior which can explain the strange results. What needs to be explained is not only why say, 70% of the answers obtained conformed to the predicted regularity, but, also why 30% failed to do so. In other words, he suggested, one cannot conclude there is no regular rule even when one finds that 30% (or more) responses of subjects in an experimental situation are unpredicted by that rule.

This problem relates to the relative roles of internal and external evidence. Internal evidence, he declared, is primary because it is only in reference to such evidence that external evidence makes any sense.

He went on to discuss the need to reconcile external and internal evidence, pointing to the Dressler proposal for representing the velar nasal in German as deriving from /ng/, and the M. Ohala argument in favor of an abstract schwa in Hindi. It is noteworthy, Gussman claimed, that such cases are usually disregarded by proponents of concrete phonology. Given these abstract analyses, supported internally and externally, one should try to formulate the principles speakers must have access to in formulating such rules and representations. Presumably, he added, one would want these principles to be part of a theory of phonology rather than the phonology of a particular language. It is such principles that we should be seeking.

K. Hale addressed the question of his conception of psychological reality, by stating the question can only be answered when related to the linguist's view of the nature of language itself. In his view, language is a complex human capacity, comprising autonomous, but interacting, systems, each of which has

its own inherent principles of organization. Psychological reality, according to such a view of language, is the goal of linguistic inquiry. It is not given a priori. A logical consequence of this is that it is impossible to ask whether a given linguistic analysis is psychologically real or not, independent of the notion of what is the most highly valued grammar. Thus, the psychologically real, or better still, the most real analysis in a particular instance can only be the one that is best according to some appropriate evaluation metric, functioning internal to the particular framework in which a particular analysis is cast and resulting in some natural way from that framework. He added that, in his candid and probably unpopular view, the traditional generative grammarian's notion of a simplicity metric is on the right track. The problem is to have the right metric, no simple matter.

In discussing the question of internal vs. external evidence, he said he finds it difficult to make the distinction, preferring to distinguish between good and bad evidence. When a field linguist is faced with two or more possible analyses of some data, (s)he needs to look at any kind of evidence to decide. In the case of the Maori passive which he discussed in his paper (vol. II, p. 108-113), the analysis he arrived at after looking at ten different kinds of evidence was the unexpected one, setting up a conjugation system among verbs rather than presenting a purely phonological analysis. Yet the phonological rule analysis would probably be the one required of any student who wanted to pass a phonology course. Hale argued that strictly linguistic reasons favor the morphological analysis, referring to Jonathan Kaye's "recoverability principle". This principle also appears to operate in Papago, to select an analysis which could be considered to be just the opposite from that in Maori, although the surface phenomena are identical. This principle may then be a subcase of a more general simplicity metric, affirming the importance of such linguistic principles. He concluded by stating that the psychologically most real analysis will be that most highly valued by a valid simplicity metric.

P. Linell argued for a behavioral performance perspective on language, stating that a language should be viewed as a system

of grammatical and phonological phonetic conditions placed on the stream of meaningful and phonetic communicative behavior. He thus would assign a role to phonological form both as related to plans for the pronunciation of the expressions in question and as related to perceptual schema. Phonological entities are phonetic entities, i.e. phonetic behavioral articulatory plans, intentions, perceptual schemas etc. There are phonological aspects of morphological formation patterns which he said also belong to other components of the grammar, but these, too, concern surface phonetic entities.

Linell suggested that whether one considers psychological reality a non-issue depends on one's theoretical preference. If a language is seen exclusively as a set of abstract sound-meaning correspondences, isolated from behavior and communication, it probably is. Thus, he maintained, autonomous linguistics aims at capturing all detectable generalizations at all levels, and this is a legitimate concern. But if one is interested in psychological reality, Linell proposed that it is necessary to look at production and perception behavior, language learning, and language storage. A language user does not need all the linguists' generalizations and it is thus doubtful that these are psychologically valid. It is more likely, he claimed, that there is great redundancy in the grammar leading to processing short cuts, heuristic routines, parallel strategies etc.

In arguing against formal conditions on rules, or principles, he stated that too often such discussions are pointless since when, for example, we raise the question of recoverability, why should morphophonemic forms be recovered at all, by whom are they supposedly recovered, and for what purpose.

The problem cannot be solved by experimentation, he added, unless we know how to interpret the hypotheses we are testing. If, for example, we find speakers make the vowel substitutions predicted by the vowel shift rule in SPE, we should not conclude that the way the rule is formulated is correct. (Chomsky & Halle, 1968) Or if speakers relate fable and fabulous it is a non-sequitur to conclude that there is one morpheme form underlying both words. This is the generative way of describing the relationship, but there are other possibilities.



Linell concluded with the suggestion that it may be artificial to separate out psychological reality from social and biological reality. What we want is a true synchronic theory of the linguistic practice of language users.

R. Skousen suggested that the psychologically real descriptions which we seek may not be composed of rules such as the kind that have been postulated, or any rules at all. Although linguists may characterize behavior in terms of rules, it is not certain that linguistic behavior itself is rule-governed.

He illustrated his point of view by a discussion of "probabilistic" rules. He considered a hypothetical language in which the verbal past tense is realized by one of two forms, in what has been called in the past free variation. But, suppose in observational studies it is found that a given speaker produces one of these forms two thirds of the time, and the other, one third of the time. He provided reasons why one should not posit a rule which specifies the probability of occurrence of either form in that speaker's grammar. A linguist can construct such a rule, but this does not mean that a speaker can or does construct a rule of this form.

He followed up this example with a discussion on apparent regular rules with exceptions and questioned whether in many of these cases we should conclude that the speaker utilizes a rule rather than looking for specific forms and then using these forms analogically to produce new and novel forms.

#### DISCUSSION

A discussion ensued, participated in by the panelists and by the following speakers from the audience: C.J. Bailey, R.P. Botha, J. Bybee Hooper, R. Coates, T. Gamkrelidze, W. Labov, A. Liberman, L. Menn, J. Ohala, and J. Ringen. There will be no attempt to cover all the interesting points presented.

A number of the discussants continued on the topic of internal vs. external evidence. Ohala posited that this is a false dichotomy, a point made earlier by Hale, since evidence is evidence. He suggested, however, that there is a continuum in the quality of evidence, since some evidence may be less ambiguous and more capable of refinement than other evidence. He

stated that "internal evidence" is highly ambiguous as to what it reveals about psychological entities; evidence from speech errors is of slightly higher causality, and evidence from experiments the least ambiguous and the most capable of refinement because of experimental controls.

On the same question, Bybee Hooper referred to the external evidence used to support the velar nasal as deriving from /ng/ and said that there are other interpretations which can be made, thus warning against making unwarranted assumptions about linguistic structure from such evidence. Both Gussman and Campbell agreed that unwarranted assumptions shouldn't be made about anything.

Hale pointed to the possibility that there may be opposing analyses for which no external evidence is available, and suggested that it is highly possible that a child confronted with a language has a problem similar to that of the field linguist who has only the language data. He suggested that we therefore need some internal principles which permit both the linguist and the child to come up with an analysis. He pointed to problems in interpreting external evidence like that of language games. He has found that in Australia, where secret languages are elaborate and a key intellectual activity among the aboriginal people, some are very good at these games and others very bad. Thus one gets variable data.

Labov followed the lead of Linell's suggestion that one must consider other forms of reality such as social reality, and, in fact, argued that this may have greater importance than psychological reality. He pointed to evidence from child language acquisition showing that children use different strategies before their grammars converge, and he said such differences probably persist in the more irregular portions of the language for some time. In his study of Philadelphian English, he has found that some Philadelphians use a complex rule to derive two phonetic vowels, whereas for others, it appears, two underlying forms exist. Much of the evidence we seek refers to the social reality of the system rather than the processing of individuals.

Bailey also considered the importance of language change, going so far as to say a dynamic approach must be used rather

than a static one in looking at language.

Campbell also added to the discussion on social factors by pointing to the fact that they can complicate phonological descriptions. He has found that in some societies the avoidance of "dirty words" causes phonological complications. Dressler noted that considerations of social reality and the social and communicative function of language was key to a concern for universals in phonology. In discussing variation across individuals Derwing noted that sociological reality was nothing more than a sum of the psychological reality of many individuals. If, he said, we are studying language users, we do not expect them to be the same. Linell suggested that rules should be construed as socially acquired and socially shared, which, he added, is the traditional notion of a rule as a norm for behavior.

Ringen and Botha both discussed the role of the philosophy of science in theory construction and validation. Botha stated there is no such thing as the problem of psychological reality of phonological descriptions. There may be a problem, and this depends first, on the aims of the theory, and second, on the philosophical approach of the linguistic scientist. The notions of "truth", "reality", and "evidence" are theory bound. Ringen also noted the relevance of philosophical questions. He also affirmed the importance of theories of performance in deciding whether evidence is internal or external.

Cutler also argued for the need for a theory of performance but, as a psychologist, pointed to the difficulties in attempting to set up psychological experiments which would get at the strong version of psychological reality. Coates also stressed the importance of working with psychologists in our attempts to establish the kinds of association between linguistic units which exist. The notion of units was discussed by Lieberman, who stated that the basic task for phonology is to segment the non-discrete speech signal into the correct discrete segments.

Gamkrelidze noted that the goal of constructing a theory which would provide for psychologically real grammars was not one which arose with the transformational linguists, who, instead, he believes placed their emphasis on cybernetic considerations. He pointed to the difficulties, however, of trying to

determine what is in the mind of speakers, from their utterances, which parallels the difficulty of trying to determine the inner mechanisms of a clock from watching the hands move. Many models can be constructed which give the same output but only one model is the correct one. This point was similar to one made by Skousen in discussing the need for real world interpretations of formal linguistic constructs, providing an interesting analogy with a formal system of Euclidian geometry which can only have "reality" when the formal primitives are given substantive interpretations.

Menn was concerned with the fact that linguists, or some linguists, seem to ignore the variety of things which can legitimately be considered knowledge and the necessity of distinguishing among them. SPE ignores the degree of rule productivity, she noted, and most experimental linguists ignore the difference between active and passive knowledge and the difference between explicit metalinguistic knowledge ("I can tell you that word A contains morpheme B") and implicit knowledge ("I guess that word A is more likely to mean something about rocks than sugar.") We need to set up sufficiently subtle experiments to be able to differentiate between these phenomena, she said.

To conclude the symposium, the moderator, Fromkin, presented some of her own thoughts. She agreed that it is not possible to proceed without any biases or a specific philosophy of science. One would hope, however, that despite different philosophies, linguists will provide increasing information which will reveal something about the phonological systems of the languages of the world.

She referred to some of the arguments concerning "autonomous linguistics" and expressed confusion as to what that phrase really does mean, or why some people consider it negatively. No one can deny that language is used in society, that language is a product of evolution, that there are brain mechanisms underlying language, that language is used by speakers in producing utterances and in comprehending speech, that it is used for humor, for making love, for expressing hate, for selling soap, but, she asked, why is it not legitimate to attempt to study the language systems which underlie all these uses, to investigate language

per se. The history of science shows the isolation of different facets of reality in order to better understand them. Do we need to study the persuasive and disgraceful use of ambiguities by advertising agencies before concluding that for some speakers of English writer and rider are homophonous even though write and ride are not? And that the homophony arises from an "alveolar flap rule"? Whether or not one believes in the reality of rules, in describing the sound patterns of English, we certainly must reveal this "fact".

This does not mean, she added, that we can ignore the bridges between one part of the complex phenomena and another. But it certainly is legitimate to say that human language exists and we should try to understand it. The question then arises as to whether language is a cognitive system which can be viewed apart from the behaviors of those who have acquired it. Those who hold this opinion point to various kinds of evidence to support it. For example, many if not all of us produce utterances which we, in hearing a tape of our own speech, will regard as "improper" or ungrammatical. This judgment must come from some stored knowledge. Clearly we can and do say, produce, and understand the meaning of utterances that we also declare to be ungrammatical sentences. Thus utterance is not equal to the theoretical construct, sentence.

Fromkin continued her discussion on "autonomous linguistics" saying that the pursuit of language per se may be a worthy one. This does not imply that linguistics is not a subset of psychology. Derwing's dichotomy does not necessarily hold, if we view language as a system of knowledge that is a mental reality. There are of course many subsets of psychology. One can pursue research in the field of vision without conducting research on auditory perception. Furthermore, psychology is concerned with behavior but not exclusively so. There are as many differences of opinion among psychologists as there are among linguists, many stemming from differing philosophical views. Fromkin stated that she could probably point to as many psychologists who agree with her view of the aims and proper subject matter of linguistics as can Derwing in support of his views.

However, she wished to emphasize that this does not mean that the construction of performance models is not a worthy one for linguists. Her own research has been primarily concerned with performance, but she added that this research has been guided by the insights provided by linguists working on language structure, rules, and representations.

Failure to distinguish between linguistic behavior and knowledge would create problems for those analyzing speech errors. Similarly, the study of aphasia shows that in many cases the linguistic deficits are performance deficits, while the stored grammar is intact. Otherwise one could not explain why an aphasic patient is capable of production, retrieval, and perception on one day, and incapable of one or the other aspect of performance on another occasion. Manfred Bierwisch pointed to this discrepancy many years ago when he posited that most aphasia symptoms can only be explained as performance breakdown.

Fromkin concluded with a quote from Poincaré (as cited in Chandrasekhar, 1979):

"The scientist does not study nature (only) because it is useful to do so. He studies it because he takes pleasure in it because it is beautiful. If nature were not beautiful it would not be worth knowing and life would not be worth living."

She ended by saying that we who are interested in human language know how meaningful this quote is, since human language, like all of nature, is beautiful, and the study of it is therefore worth doing.

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