

PERSPECTIVES IN PHONOLOGY¹

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It was only after a good deal of hesitation that I accepted the invitation of the committee to speak about phonology in a plenary session of this congress. I have never made any significant contribution to phonological theory, nor have I tried to apply it to any concrete language. It is true that I have just written a book on phonology, but that was, so to speak, unintentionally; it somehow grew out of my teaching (what I really wanted to do was experimental phonetics). - As for phonology, I have been an interested, now and then somewhat baffled, spectator of the development. But perhaps, what the committee looked for was a relatively unbiased spectator.

Let me add that I have been lucky to be able to draw heavily on the expertise of my colleagues Jørgen Rischel and Hans Basbøll, whose stimulating criticism has been extremely valuable and has led to significant improvements in the present paper.

When you look back at the development of phonology it seems to have followed a rather tortuous path, or rather various paths. The phonologists may be compared to a somewhat disintegrated group of mountain climbers, aiming more or less at the same peak (or group of peaks) which - when viewed from the valley - seemed within quite easy reach, but which went out of sight as soon as they started climbing. Sometimes, what looked like the best track went horizontally for a long while, farther and farther away from the last resting-place, and some found that the start had been quite wrong and should have taken place from a wholly different angle. Sometimes the track disappeared completely,

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and various climbers insisted on using detailed maps and planning each step carefully. The maps, however, did not seem to be sufficiently exact, and some years ago some climbers rejected the use of maps altogether and found it safer to use their intuition and hope for the best. Although they got many adherents, there are still some who would like to know where they are. And I think a congress is a good occasion for just taking breath for a moment and asking ourselves where we are, what we are aiming at, and how we can hope to reach - or at least approach - that aim.

I. The first question - where are we now? - can be slightly reformulated to mean: What have been the most conspicuous developments in phonological theory since the last congress?

It should first be stated that in the course of these years generative phonology has been adopted, or at least studied and discussed, by an increasing number of young linguists all over the world. It is now taught in a great number of universities, and there may even be young students who do not know that other respectable trends of phonology have existed and still do exist. Such a general spread of a linguistic theory has hardly been seen since the days of the Neo-grammarians. During the intervening structuralist period there was a locally determined split-up into rather deviant schools.

There are various reasons for the success of generative phonology. Better communication has been a condition, but it is, of course, by no means a sufficient explanation. What has been very important is, I think, the fact that transformational grammar, including generative phonology, has broken the isolation of linguistics resulting from the endeavour of structuralism to make linguistics an autonomous science, a laudable endeavour at that time, but in the long run detrimental to a fruitful development. Transformational grammar opened up wider perspectives by empha-

sizing the relations to psychology and the importance of studying universals and by claiming that linguistics should not only provide descriptions, but also explanations, perspectives which had been almost cut off in American structuralism. But it should not be forgotten that the interest in universals and in explanation was vivid in European structuralism, particularly in the Prague School, and that generative phonology owes much to Roman Jakobson in these respects. - The endeavour to set up models and to formulate explicit rules also contributed to the success. It should, however, be remembered that the formulas used, for instance, in "Sound Patterns of English" are simple abbreviations of normal prose. They may be a practical means to avoid ambiguity and to make sure that nothing has been left out, but they do not constitute a sort of mathematics. - Some linguists, searching for a means to get rid of the non-uniqueness of structural descriptions, may also have been impressed by the assertion of the first adherents of transformational grammar, that they had found the only correct solution, the one corresponding to the tacit knowledge of the speaker-hearer. This assertion, however, is not only one of the most interesting but also the most dubious of all the assertions of generative phonologists, and the incredible self-assurance with which it was propounded (again, by the way, reminiscent of the Neo-grammarians) also had the effect of keeping a good many, more level-headed linguists aloof from the new ideas.

Along with the diffusion of generative phonology, however, a remarkable relaxation of the orthodoxy has taken place. Important modifications of the theory have been proposed, both by professed adherents, for instance by Kiparsky (in a series of excellent papers), by Schane, McCawley and Stephen Anderson, and also by linguists who are, in the main, in sympathy with the endeavours of generative phonology but do not consider this the only possible way of describing language (like Rischel and Bas-

bøll)¹.

The high degree of abstractness of underlying forms in early generative phonology was criticized by Kiparsky as early as 1968 (1968a). He proposed in particular two modifications: (1) underlying forms differing in phonological specification from surface forms should only be set up in the case of alternations, and (2) they should not contain segments which are never realized on the surface (the so-called "absolute neutralization"). This excludes, for instance, æ or x in English. Various phonologists have joined in this criticism (e.g. Shibatani 1971, Vennemann 1972a, Wang 1973); others (e.g. Brame 1972 and Hyman 1970) have maintained that the simplicity obtained by using such abstract underlying segments is legitimate in language description. In some later articles (1971, 1972), Kiparsky discussed the matter again and proposed that absolute neutralization should be allowed in cases where the underlying contrast is crucial to more than one rule of the language.

Restrictions on language specific (extrinsic) rule ordering were proposed by Chafe (1967). Stephen Anderson (1974) goes further and attacks the general notion that rules are, on the whole, linearly ordered. He assumes that only pairs of rules, not whole sets of rules, are mutually ordered and only with respect to a given form. In accordance with Kiparsky (1968b), he suggests that rules tend to apply in a universally determined order, the two most important principles being maximum effect of the rules and transparency of the result. In a later paper (1971), Kiparsky gives preference to the latter principle. Complete abolition of extrinsic rule ordering has been required by Koutsoudas, Sanders and Noll in 1974.

1) The reference list at the end of the paper contains a choice of what I have considered the more interesting contributions to phonology since the last congress, together with a few older papers. It includes also some surveys where further references may be found.

As for the distinctive features, various revisions have been proposed by Halle and Stevens, and particularly by Ladefoged, but as this topic has been treated in other sessions, I shall not enter into it here. I should only like to emphasize that I find it necessary to distinguish more sharply between the universal set of phonetic dimensions and the features of concrete languages, which generally do not consist in a simple choice of the general dimensions, but in various combinations of these general dimensions, and whose phonetic definitions will therefore vary from language to language.

More severe is the criticism which has been raised against the very basis of generative phonology, the claim that the description has psychological reality. It is true that some transformationalists, for instance Ruwet, do not make this claim and seem to consider generative phonology primarily as an efficient descriptive technique. But for most adherents this is a crucial point, because it is just this psychological basis which should justify the claim that the transformational description is superior to all previous descriptions.

This was expressed quite clearly by Chomsky and Halle in 1965: "Without reference to this tacit knowledge there is no such subject as descriptive linguistics. There is nothing to be right or wrong about". This cannot simply mean that the linguistic description should be able to generate the same sentences or forms as those generated by the speaker, for this could be done in different ways, and this is what almost all linguistic trends have aimed at. It must mean that both the underlying forms and the rules belong to what is called the internalized grammar of the speaker. They also use - on purpose - the term 'grammar' ambiguously, both of the description of the linguist and the competence of the speaker, which means that they claim a close correlation between the two.

The criticism raised by adherents of the theory against very abstract underlying forms is also, among other things, based

on the argument that they can hardly be part of the speaker's competence. There has been a tendency to assume that the normal speaker has the same knowledge as the linguist about etymology, although few will go so far as Lightner, proposing common underlying forms for the words ten and decimal in English.

What is astonishing now is that Chomsky and Halle do not attempt to test this claim empirically, but instead set up a purely formal evaluation measure. The formal machinery must be able to account for "linguistically significant generalizations". But the decision as to what are significant generalizations is based on a purely hypothetical assumption concerning the way in which a child acquires language. It is assumed that he has an innate knowledge of possible structures and that he will always operate with maximally general and natural rules.

The purely hypothetical character of these assumptions has been demonstrated very convincingly in some highly interesting recent monographs by Botha (1971), Derwing (1973) and Linell (1974). Linell concentrates his criticism on the problem of the psychological reality of underlying abstract morphemes and sets up an alternative analysis based on the assumption that speakers have only stored concrete wordforms and relations between these concrete wordforms. Derwing also criticizes the postulated psychological reality of underlying forms, but his criticism is particularly concentrated on the postulates of generative phonology concerning language acquisition. As early as 1968, McCawley characterized the, admittedly counter-factual, assumption of instantaneous language acquisition set up by Chomsky and Halle for reasons of simplicity, as too unrealistic. What really happens must be a constant restructuring. Both Derwing and Linell emphasize that at the start the child must store concrete wordforms, and they cannot find any proof for the assumption that at a certain stage the strategy is changed to the storage of abstract underlying forms. Derwing also demonstrates that there is no support for the contention that

language acquisition presupposes a highly structured specific set of innate linguistic universals. A general capacity for generalization and structuring and for using symbols must be a sufficient hypothesis. If children cannot be supposed to be able to learn transformational grammar without the specific innate universals, it may be transformational grammar which is wrong.

Derwing's book also contains a penetrating criticism of Chomsky's varying use of the terms competence and performance, and his highly relevant criticism of generative phonology is, on the whole, based on general considerations of scientific methodology.

It is thus a characteristic feature of the present situation that most of the basic assumptions of generative phonology have been the object of serious and convincing criticism, and that many points are being revised also by the professed adherents of the theory.

This revision also includes a changed attitude to structural linguistics. Structural descriptions are no longer characterized as absurd or senseless; on the contrary: many concepts of structural phonology have been taken up again and their introduction into generative phonology reconsidered. The necessity of describing surface structure (phonotactics), for instance in order to understand the treatment of loanwords and phonological change, has been emphasized by various authors (e.g. Kisseberth 1970, Shibatani 1971, Kiparsky 1971 and 1972, and Rischel 1974). The syllable was reintroduced by McCawley (1968), and its importance for phonological rules demonstrated by Vennemann (1972b), Hooper (1972), and Basbøll (1972 and 1974). The importance of surface contrast was stressed by Schane (1971) and Wang (1973). Some have even admitted that perhaps the first transformationalists had been too rash in throwing out the phoneme with the taxonomic bathwater (for instance Schane 1971). The possible role of phoneme systems in language change is also being reconsidered (for instance by Vennemann 1972a and Kiparsky 1972). On the

whole, many structural concepts are becoming respectable again. I think that this growing tolerance is very promising for future research.

A criticism which has been raised against both orthodox generative phonology and some trends of structural linguistics is the extremely formal approach which involves a neglect of physiological and acoustic phonetics and the contributions these disciplines may give to the explanation of phonological facts. This criticism has been raised particularly by John Ohala (1971, 1972c and 1974b), M. Chen (1971), and B. Lindblom. I think above all of Lindblom's very important paper at the last congress in 1971, which will be continued at this congress tomorrow.

Let me finally mention that the sociological aspect of language has until now been neglected in all trends of phonology, and quite particularly in generative phonology. The importance of this aspect for the explanation of phonological change appears very clearly from the works of W. Labov (e.g. 1971 and 1972a). Of particular interest is his observation that the command of heterogeneous structures is part of also unilingual linguistic competence and that consequently Chomsky and Halle's assumption of an ideal speaker-hearer in a homogeneous speech community prevents a realistic conception of language change. Very interesting is also his success in evaluating the relative contributions of the parameters age, style and social class in various cases of phonological change in progress.

A good many interesting positive contributions from a purely structural point of view have, of course, also been made during these years. A number of the papers given at the phonological conference in Vienna in 1972 belong to this category, but the proceedings of this conference have appeared so recently that I have not been able to utilize them (See Dressler and Mareš 1975). I should, however, particularly draw attention to a number of interesting and original papers by Henning Andersen (1969, 1972, 1973), treating various problems of diachronic phonology.

II. This is, I think, approximately where we stand. The next question: "What are our aims?" is more difficult to answer. In a very vague sense we may perhaps be said to have the same ideal, distant aim of arriving at a description which accounts for all important facts and generalizations, which can be used to explain things, and which corresponds to some type of psychological reality. But we may not all agree on what are the most important generalizations, nor what we want primarily to explain. And at the present stage of phonological research we should not conceal our lack of knowledge by proclaiming one theory and one method as the only correct one. Language is a complicated phenomenon, and various descriptions from various angles may be complementary rather than contradictory. What is needed is mutual tolerance and coordinate efforts.

But I think that, at the moment, many are interested in taking up the challenge of generative phonology concerning the psychological reality of phonological phenomena, and I should consider it one of the primary tasks of phonology in the coming years to attempt to come to grips with this problem. And in the remaining part of the paper I will deal particularly with this task.

III. Before trying to answer the question: "How can we approach our aim?", we must, however, stop for a moment and ask what is meant by "psychological reality". This is by no means clear.

It cannot, generally, be taken to mean "conscious awareness". For there are very few phonological phenomena of which naive speakers are consciously aware. One of them is the phonological difference or identity between wordforms. This is utilized in the usual pair test or commutation test. But even this knowledge may be defective. Labov (1972b) has recently observed that informants may be unable to distinguish minimal pairs which differ in their own pronunciation. The Russian linguist Panov (1967) has observed similar cases in Russian, and gives the following very plausible explanation: If many, or most,

members of a speech community do not make a given distinction, it loses its communicative value, and even those who make the difference themselves stop taking notice of it. - Speakers are also generally aware of differences carrying social or stylistic connotations, and they are normally able to indicate the number of syllables in a word. They may also be willing to indicate the number of segments, but on this point they are usually so heavily influenced by orthography that their answers are extremely difficult to interpret.

But as soon as we get to the real points of disagreement between linguists (features, rules and underlying forms), no conscious awareness on the part of the speakers can be expected. What, then, do we mean by psychological reality in these cases? We can, as far as I can see, only mean that the speaker's linguistic behaviour seems to presuppose that he has, somehow, command of the units or rules set up by the linguist, or, to be cautious, of some equivalent of these units or rules. I shall (in agreement with the Danish psychologist Svend Erik Olsen) call this "functional psychological reality". It is very probable that the units or rules in question differ somehow as to psychological level, but I cannot see that we can say anything about this for the present.

As for the means to decide problems of functional psychological reality, we can draw inferences - with varying degree of safety - from various types of linguistic behaviour. This is nothing new, since most of these facts have been used in one or the other of the structural schools or in generative phonology as arguments for preferring one analysis to the other. A general list is found in a paper by Zwicky (1973). What we need now is a more detailed evaluation of what these sources can be used for. But I must content myself here with a brief survey, arranged in preliminary groups.

(1) There is first: normal linguistic behaviour - which has, of course, been utilized in all previous structural and generative statements about language. To take an example: Vowel harmony and other kinds of assimilation may give information about the distinctive features used by the speakers.

(2) The second group consists of various types of linguistic change: (a) Sound change may give information about the character of the pattern from which the sound change started: about the features, contexts or units that have been relevant for the change. This source has been utilized very much by Kiparsky (cf. also Schane (1971)). (b) The accommodation of loanwords is another very important source, giving information about possible segments, structural constraints and sometimes also phonological rules. (c) Acquisition of language by the child and learning of foreign languages by adults can be considered as a specific and very important type of change, from which inferences can be drawn concerning a number of different phenomena. It is important to observe both the strategy used by the child and the mistakes he makes.

(3) The third group comprises various speech errors: slips of the tongue, and aphasic disturbances. It is, for instance, an interesting observation that such errors generally respect the phonotactic surface constraints of the language.

(4) As a fourth group we may mention metrics and rhyme, phonetic puns and games and secret languages like pig-Latin.

(5) A fifth group consists of direct experiments. I shall return to some problems connected with this type of source in a few minutes.

(6) A last type of source is orthography. The invention of alphabetic script has often been mentioned as a proof of the reality of the phoneme. But this is a very sophisticated achievement which not every naive speaker can accomplish. The orthography of concrete languages and its development is, of course, our main source of knowledge about earlier phonological systems.

Orthographic errors are also of interest, and finally we may mention attempts at making illiterate persons construct an orthography for their mother tongue (cf. the famous experiments by Sapir).

The next question is, what we intend to infer from all these sources. Again here I shall try to set up some major groups of problems.

(1) It may be interesting, in the first place, to find evidence for the psychological reality of the various units set up by linguists: syllables, segments, features. (a) Are units of these different sizes stored somewhere in the brain of the speaker? Preliminary observations of speech errors (e.g. Fromkin 1971), as well as evidence from metrics, rhymes, puns and facts of assimilations and sound change, all bear witness to the existence of these units. Conclusions can also be drawn from experiments on speech sound perception, for example various tests of identification and discrimination (partly in the form of dichotic listening), or similarity judgments. Phonologists sometimes tend to ignore this information, perhaps because they think that it concerns performance and not competence. But we can only reach competence through performance, and the two should not be separated. (b) A somewhat different question is which particular segments and features are used by the speaker of a concrete language. Here the same sources may be used. Research is particularly needed to find out which features are used by the speakers. In this field the non-uniqueness of solutions is really confusing at present.

(2) The psychological reality of structural constraints is another important problem. Here loanword studies and experiments with nonsense words are particularly rewarding. It seems already pretty clear that surface structure is the decisive factor in the treatment of loanwords and also in judgments on the acceptability of phonological words.

(3) It is much more difficult to find safe arguments concerning the possible psychological reality of underlying forms and of phonological rules. There has been a strong tendency in generative phonology to set up underlying morphemes and rules leading from underlying morphemes to surface forms in all cases of alternation. But it is by no means obvious that this corresponds to the speaker's tacit knowledge. The first question to be posed when investigating psychological reality is whether a given linguistic regularity is synchronically productive or not, i.e., will the speakers apply the regularity to new linguistic material and to new combinations of linguistic material? External sandhi is a very appropriate field of study for this purpose, whereas word-internal assimilations (for instance in derivatives or inflected forms) cannot generally be used because the words may be stored as wholes. Such assimilations may, however (as mentioned by Rischel 1975), be informative in the case of polysynthetic languages like Greenlandic which have an almost unlimited possibility of suffixation involving obligatory assimilatory changes at the boundaries. In such language types the speaker simply cannot have heard and stored all possible combinations. Rischel mentions that there is a theoretical possibility that he has stored all possible dyads of morphemes, but since not all dyads constitute meaningful syntactic wholes, this is not very probable.

The productivity or non-productivity can, however, be more easily inferred from the treatment of loanwords. For interesting studies of this type, we may refer to Hyman 1970 (cf. the criticism by Linell 1974, p. 131 ff), Shibatani 1971, Skousen 1972 and 1973, Rischel 1975). Direct experiments may also be useful, for instance experiments in which informants are asked to make unusual derivatives of existing words (this type of experiment has been used by John Ohala (1972a and 1974a) and Manjari Ohala (1973)), or experiments with nonsense words, which, so to speak, function as artificial loanwords (this type has been used by

Ladefoged and Fromkin 1968, Zimmer 1969, and Hsieh 1970).

If the regularity is not found to be productive, it is hardly possible to get much farther. In this case, there are various possibilities: The speaker may have stored the alternating words as individual, unrelated words if the etymological relation is not very obvious (this may be the case with many derivatives and compounds). I think relatedness among words is far less obvious to the normal speaker and even to linguists, than often assumed in generative phonology. An English colleague of mine told me that she had not until recently realized that the word discover might be related to cover. And only a few days ago it occurred to me that plumpudding might have something to do with plums. If the speaker has not stored the words as unrelated, he may have stored them as related together with some phonological mechanism which he does not use productively, and in this case it is very difficult to say anything about what this mechanism is.

If, on the other hand, the regularity is found to be productive, then the possibility that the speaker has stored the forms as unrelated words can be excluded, and we can be sure that he has command of some type of phonological mechanism; the next problem will thus be to find out what sort of mechanism that is. This is by no means an easy task. He may have stored underlying morphemes together with rules, or he may have stored an alternation pattern operating between surface forms, and we must also make allowance for the possibility that he has a rule, but that some of the alternating forms belonging to the paradigm are also stored individually. And we must expect a good deal of variation among individuals according to their linguistic experience. It is not easy to devise experiments which can decide these questions. For instance, the fact that Ohala's informants seem to use analogy when presented with leading examples does not prove that they would use this method in other situations.

A particular difficulty involved in the use of nonsense words is that there may be different rules for native and foreign words; or a rule is only used productively for native words; and we cannot always be sure whether the informant will treat a nonsense word as foreign or as native.

Many precautions must be taken in this field if we want safe conclusions. Perhaps, for the time being, we must be glad if we can reach the modest aim formulated by Rischel (1975), that we should try to "distinguish regularities which are likely to be relevant to the way in which users of the language master it, from other possible generalizations which may be irrelevant from that point of view".

But it is certainly an attractive and important field of study. And we must hope that co-operation with psychologists may bring us some steps forward.

Finally, one question: What if we find out that the psychological reality is much more redundant and complicated than the descriptions linguists have aimed at up till now? This is not just an empty speculation. Recent research has shown that allophonic variation is in many cases not a peripheral mechanical phenomenon but planned in the innervation of the muscles and part of the speaker's unconscious knowledge of his language. What is then the correct description, or is there more than one? Must we set up one description which accounts for the facts in the most simple way without redundancy, and which may be useful for various descriptive and practical purposes, and one, more redundant description which corresponds more closely to speakers' reactions and which must be used for explanations, for instance of optimal phonological systems and of sound change?

Well, we must leave our mountain climbers where they are, hoping that with mutual help and openmindedness to suggestions from their co-climbers they may come closer to the peaks they are aiming at. - - But it may be that some of these peaks will forever be shrouded in fog!

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