

CAN PHONOLOGICAL DESCRIPTIONS BE MADE MORE REALISTIC?¹

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Abstract: This is an informal presentation of some reflections on current phonological theory, with special reference to notions such as "psychological reality", "productivity", and "naturalness".

The study of sound patterns in language largely reflects the theoretical bias in linguistics in general. For the last fifteen years transformational grammar has been by far the most influential trend. Before this epoch the international scholarly debate was a reasonably proportioned mixture of contributions from different structuralist schools, but with the advent of the transformational-generative paradigm the situation changed in the course of a few years. Transformational-generative theory, as it was developed by Morris Halle and Noam Chomsky, came to enjoy an unparalleled world-wide popularity, and soon dominated the thinking of a generation of young linguists inside and outside the U.S.A, the ideas emitted from the scholarly center at M.I.T. being adopted with something akin to orthodoxy in many parts of the world. Outside the U.S.A. this kind of linguistics caught on first and foremost in countries which had not

1) This paper was presented in part as an introduction to a discussion of phonological theory at the 4th Scandinavian Meeting of Linguists, Hindsgavl (Denmark), January 6-8, 1978. It must be emphasized that the paper does not attempt anything like a survey of the relevant literature, cf. that the references are limited to a couple of items which happen to be mentioned in the text.

had a strong and influential structuralist tradition. In old structuralist quarters the conversion rate was considerably slower, and there remained a great many sceptics or straightforward opponents to the whole trend. Even in the U.S.A. there were contemporaneous groups representing independent approaches such as tagmemics and stratificational grammar, the latter furnishing the most elaborate alternative in terms of phonological theory.

Now, why is orthodox transformational-generative phonology (henceforth OTG-phonology) increasingly under attack these years? It is obvious that the prestige of OTG-phonology is declining, and it is interesting to study who the leading figures are in this recent development. - One might assume that after a period of defeat the recognized structuralists have finally gathered strength enough to strike a fatal blow. But in fact this is not what is happening. Rather, the ground for a revolte has been prepared by an increasing flow of radical revisions of OTG-phonology, marked by eager disputes on the most deep-rooted ideas as well as a general shift of bias, cf. the increasing scepticism toward abstract morpheme representations and toward ad hoc rule ordering, and the increasing interest in surface structure. Phonological feature theory, in particular, is as far as ever from providing a basis for the unification of our science.

This revision has been and is being undertaken by linguists who for the most part share some of the notions of generative phonology (in a very general sense). That is, it is to a considerable extent an inside job. Formerly devoted adherents to OTG-phonology have to take a stand to these issues, and after the forceful attacks of recent years it may seem necessary to find out whether there is anything left that is of use to linguists.

The present writer belongs to those who - though rooted in a structuralist tradition - felt that transformational-generative phonology, at the time it appeared, had distinct advantages over other descriptive paradigms, not just because it pro-

vided such elegant solutions, but rather because it formalized the relationships among levels of representations more precisely than structuralist approaches (including glossematics) had done. Seen from this angle, and perhaps especially from a European viewpoint, transformational-generative phonology was not as totally different from classic structuralism as its proponents wished it to be: it seemed to be rather a matter of developing the formal apparatus needed to account for the somewhat neglected morphophonemic component of grammar. As for the insistence on the linguistic relevance of underlying representations, and the rejection of surface phonemics, such a viewpoint was not at all far from that of glossematics. To the present writer, the great attraction and challenge in transformational-generative phonology lay, and still lies, in the fact that this approach invites research serving to fill the gap between syntax and phonetics, and even seems to provide some means for approaching the difficult field of prosody in its interrelations with syntax. (Deplorably, the advance in research on that very point has not been nearly as glorious as one might have hoped.) I feel that the generative approach as such IS fruitful despite all well-founded attacks on current versions of the American OTG-phonology; it is fruitful precisely because it lays emphasis on aspects of linguistic structure which were in part neglected within previous descriptive paradigms.

As this decision is formulated here, it has to do only with a strictly limited goal, viz. that of stating the patterning observable in language. That in itself is certainly no simple task, although the scientific challenge of it is often ignored in lofty discussions of abstract interpretations and restatements of data furnished by other linguists. (I think there is a considerable danger in the widespread inclination to make theoretical constructs on the basis of data the intricacy of which is only properly understood by a linguist who has been doing field-work extensively himself.)

The present reaction against OTG-phonology has to do with a much higher - and admittedly more fascinating - goal than that of mere description of sound patterns. The all-pervading question is: how do we arrive at statements that reflect REAL properties of languages, and not just refer to a more or less adequate (but in principle arbitrary) model of language? There have been rather successful attempts to demonstrate that some - especially some of the intuitively far-fetched generative solutions are psychologically implausible or even at variance with empirical evidence. This is an important objection because it has been implicit or explicit in the OTG-phonological literature that this paradigm represents a serious hypothesis about internalized phonologies. Such a claim has seemed to many of us quite unwarranted and only indirectly useful, and it is a relief that it has now become a commonplace to realize this.

Unfortunately, however, there may have been an exaggerated enthusiasm over recent attempts to change the paradigm in the direction of a psychologically real phonology. I do not see that our science has advanced very far on this issue except for the very general observation that phonology is probably less abstract than some phonologists have liked to assume.

Much excellent work is being done by people studying verbal behaviour, speech defects, etc., and from phonetics we begin to learn quite a bit about peripheral processes." But this whole field of cumbersome observation and experimentation does not at present endorse the advancement of ambitious claims about the fine structure of internalized phonologies. The descriptive phonologist must content himself with much more modest claims.

One of the most immediately useful questions a linguist may ask if he discovers a regularity in his data, is: do speakers of the language master this regularity? If not, the regularity in question may perhaps still be worth stating, since it may throw light on earlier stages of the language, or it may possibly be relevant to practical applications of the linguist-

stic description. But one might reasonably define it as a major goal for linguists to find out what pieces of structure and what generalizations across these pieces of structure the speakers of the language have somehow internalized. If this could not even be recognized as a goal which may in theory be accomplished, synchronic linguistics would indeed be mere taxonomy, and synchronic patterns would be explicable solely in terms of diachrony.

However, recognizing a goal is not tantamount to reaching it. There is a danger in just stating that from now on we should do psychologically real phonology and ridicule linguists who do not do this. It is not that simple. I certainly share the view that OTG-phonology may have hampered empirical research in this field by axiomatizing away the issue, but the sad state of affairs is that we are still waiting for substantial results from research on internalized phonologies of adults. One may attempt to derive a criterion of PLAUSIBILITY from the scattered pieces of research available so far, and one may strongly emphasize the tentative character of phonological descriptions as long as there is no workable criterion such as psychological reality. But it is absolutely essential not to content oneself with a vague belief in concrete phonology as being "psychologically real" by virtue of its concreteness, since this easily results in axiomatizing away the issue once more.

One basic difficulty is that it is not clear a priori what kind of "psychological reality" we are after. Whom is the allegedly psychologically real pattern to be attributed to? We do not seriously entertain the idea that all speakers arrange their linguistic knowledge in exactly the same way. What then? Are we referring to some kind of inter-subjective common core, or are we constructing a linguistic superman like Chomsky and Halle's ideal speaker-hearer? Is a psychologically real phonology a closed system, or should we rather attempt to design our linguistic description in such a way that it explicitly takes care of the range of the alternative ways in which different

speakers of the language may arrange the ingredients? I personally should greatly prefer the latter alternative, although at present I see no practical solution to this.

Secondly, we must state what is representationally implied by the notion "psychologically real". Nobody thinks of the current rule format as being piece by piece represented in the brains of people. (Incidentally, stratificationists have made stronger claims as to representationality than has ever been done in OTG-phonology, as far as I know.) We rather think of the contents of rules as having possibly a psychological reality, which may be tested by studying whether the predictions of the rules are borne out under conditions provoking their application. Thanks to the work and argumentation of John Ohala, Bruce Derwing, Per Linell, and others it can be safely concluded today (if it was ever doubted) that there is something psychologically unrealistic about OTG-phonological descriptions. However, testing a single rule is a difficult matter since rules typically form close-knit wholes one part of which may be crucially dependent on how the other is stated. It may not be difficult to demonstrate that this or that rule in some OTG-phonological description fails to be represented as such in the brains of speakers of the language in question, but the conclusions to be drawn from such a proof are sometimes of limited interest exactly because of the trading relationship between different parts of a phonology. It may be more realistic to take the whole phonology as a black box with an input end and an output end and investigate whether it functionally matches the competence of speakers of the language. Thus, what may be tested off hand, is whether the phonological description covers the kinds of information about the language that is available to the user of it. This is, indeed, an interesting issue, and I think that if the result of such a test comes out positive one may reasonably claim that the phonological description does in a certain, very restricted sense satisfy the demand for psychological reality, even though there may be little isomorphy between the descriptive statements and internalized represen-

tations. The emphasis is shifted here from the question about the reality of rules to the reality of phonological information, that is, it is no longer specifically transformational-generative phonology that is at issue but descriptive phonology as such. I think that is a useful change of viewpoint. The "psychological reality"-issue is in fact independent of the transformational-generative versus the structural viewpoint. It is used today to criticize OTG-phonology, and that is perfectly legitimate since this type of phonology has been associated with claims about internalized representation, but one might at the same time give OTG-phonology credit for having provoked the current interest in this highly important topic.

Testing whole phonologies tells us something, if it can be accomplished. However, we are still badly in need of information about details of internalized phonologies. In many cases it is perfectly possible to take regularities (in the sense of equivalences or alternations recurring over sets of forms) and ask quite generally whether they are mastered by speakers of the language. Taking a trivial case like the alternation between non-final [d] and final [t] in German, for example, the problem at issue may be detached from the generative solution according to which underlying /d/ is rewritten as [t] in final position, and stated instead as a question about the alternation: do speakers make use of the fact that there is an alternation between [d] and [t] recurring in several forms, or would it not make any difference if the alternations in individual sets of forms were quite idiosyncratic? - Is a certain regularity used productively? This is an extremely important question, but not the only important question. Couldn't it be the case that a given, restricted regularity is accessible to users of the language even if it is not used productively? How can we test whether that is the case?

There are questions enough to be answered before we even approach the formalizations of specific phonological descriptions. And even in the simplest possible conceptual framework we run into difficulties when we wish to state exactly what is

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for: by foreign (or nonsense) items ...

read: by foreign (or nonsense) items combined with an already existing item, where the latter exhibits alternation in accordance with the phonological make-up of the various foreign (or nonsense) items ...

at issue. Taking the notion of PRODUCTIVITY, which is a central concept in the current debate (Skousen 1975) we immediately realize that regularities meet this criterion in an absolute sense if they will be extended to new combinations of lexical items meeting the structural conditions, specifically so that existing lexical items come to exhibit an alternation not otherwise found with them, or so that foreign (or nonsense) words turn out to exhibit alternation. We expect this especially with alternations which are "necessary" to prevent violation of some output constraint (unless some quite different strategy rescues the form in question). - But there may also be alternations shared by a closed set of lexical items where each item has an alternant which is strictly phonologically conditioned and occurs if and only if the appropriate feature (complex) is present in the context of the lexical item in question. This situation is not irrelevant to the issue: the context may be new and still turn out to trigger the alternation if and only if it is phonologically appropriate. One such type of data is represented by unfamiliar compounds or derivatives consisting solely of existing lexical items; another type is represented by foreign (or nonsense) items with which it is combined (example: vowel harmony in suffixes, cf. Rischel (1975)). What may be tested in these cases is not whether an alternation is productive in the sense that it can be extended to apply in new lexical items, but whether it is productive in the sense that items already exhibiting this alternation turn out to distribute their alternants in new contexts in accordance with the phonological properties of these new contexts. - It does not make much sense to use the term "productivity" without distinguishing carefully between these different phenomena, of course. But all of them are crucial for phonological theory, since the very existence of strictly phonologically conditioned alternations will prove that speakers perform some kind of phonological analysis (i.e., that not every wordform is just stored as a gestalt differing as a whole from those of other wordforms). No matter

what a "real" phonology looks like, the very possibility of proving the existence of phonology is a rescue for all of us who hope that in our strictly descriptive work we are nevertheless contributing tiny bits of information which can be used in the construction of hypotheses about speakers' and listeners' linguistic competence. If this is so, phonologists and phoneticians can feel reassured that their accomplishments will eventually converge, no matter how far we are at present from making educated guesses about the ways in which speakers' command of their language is organized.

So much for the productivity issue. At the bottom of all assumptions about regularities, possible uses of rules (or analogy, whatever that precisely means), etc., lies the notion of RELATEDNESS among lexical items. And this is really where the basic research needs to be done.

Structuralist phonology as well as "generative" statements about morpheme structure or surface constraints, all of this is about structure: items, hierarchies, and rules of combination or of dependency. The essence of transformational-generative phonology, on the other hand, is about projection of one representation of a chunk of language onto another, more abstract or less abstract, representation of the very same chunk. "Abstraction" here implies that lexical items in more or less invariant shapes enter the representation. Now, in fact all descriptivists deal with relatedness among sentences (or utterances). Structural phonemics concentrates on partial similarities in terms of segments and suprasegmentals, with more or less disregard of the way in which the strings are composed of lexical items. Transformational-generative phonology, on the other hand, emphasizes the partial similarities in terms of lexical material and accounts for these by positing abstract representations in which lexical items occur, in more or less invariant shapes, as constituents. (Glossematics attempts to unite both of these viewpoints.) - If one chooses the latter approach it is crucial how we identify parts of wordforms, i.e.

morphemes, in different contexts. One prerequisite to this procedure is to decide whether a set of wordforms are synchronically related at all. These very problems were shared by classic morphophonemics and *Morphologie* as well as glossematic analytical practice, and it is indeed remarkable that phonological theory has developed for half a century without more progress being made on this point over the years. As I see it, much of the current dispute about alternative formats of description is of marginal interest compared to the very question: how do we decide whether two forms are related for the purpose of synchronic description? A satisfactory solution to this problem is a prerequisite to the identification of relevant regularities and hence it should ideally be solved before one turns to the next important issue: how do we decide what is a linguistically significant generalization?

What can we do about all these questions in actual field-work? It is no difficulty to recognize the existence of these issues, but apparently, linguists have also found it easy enough to continue doing descriptive work without having any satisfactory solution to them. Now this is coming into the focus of interest, and it must be generally recognized as a strict obligation of contemporary linguistics to cope with it.

(Notice that the question of relatedness faces any descriptivist, no matter whether he looks for psychological or immanent structure.)

Now, taking it for granted that there are crucially related forms and significant, phonologically stable regularities pertaining to them, the next question is: do these regularities operate according to the OTG-phonological paradigm, i.e. in terms of abstract invariant morpheme representations and rules mapping these onto actual phonetic representations, or is the mutual relatedness among surface forms rather to be stated in terms of inferences (Eliasson 1977) or interpretive rules (Leben and Robinson 1977)? It seems attractive that the relationship between abstractness and allomorphy falls nicely

into place with the latter analysis: the less alternation, the less complex the statement of relatedness. Even if a particular alternation, say, vowel shift in English, can be stated in rule form, it is simpler if occurrences of a form can be identified without use of the rule. With this view of phonology recent ideas about recoverability, transparency, and paradigmatic cohesion seem to fall naturally into place. I think it is probable, however, that the truth lies somewhere in between the generative and interpretive views. It seems to me wildly improbable that all wordforms should be stored lexically; at any rate, this does not make much sense for polysynthetic languages (cf. Rischel 1975).

Assuming that there is some generative mechanism producing complex wordforms does not, however, entail that we must assume the existence of morphemes behaving according to current analytical practice. I do not feel that it is particularly plausible that naive speaker-hearers process their language in terms of morphemes with exactly the boundaries which phonologists like to set up in order to account for alternation with a minimum of suppletion in underlying representations. There may be quite different strategies which override this specific notion of descriptive simplicity.

Another question is the relation between levels of distinctness, fast speech as reduction of slow speech-forms, etc. Off-hand, this sub-component of phonology seems to invite a generative treatment (in accordance with Linell's suggestion concerning "concrete phonology" (1974)).¹ - One must exploit the possibility of matching observations of phonological variation with studies of speech production mechanisms. Fast speech data obviously provides an aid to the latter field of research, and vice versa.

1) It may be appropriate to keep syllabated speech (as in over-distinct dictation) outside this generative sub-component, cf. Rudes (1976).

A good deal of the recent work in phonology is referred to under the cover term "natural phonology". Without attempting any kind of definition of this term, I assume that natural phonology is characterized, *inter alia*, by emphasis on generalizations statable over surface forms (i.e., "concreteness") and by the role of performance motivated processes in the metatheory. Probably every phonologist would agree that it is desirable to have a metatheory providing a universal repertory of possible processes, just as phonetic theory provides a framework for the specification of possible types of sound segments. But it is a difficulty that feature theory is still so controversial. Moreover, it must be emphasized that the principles of hierarchical organization of speech are understood only to a very small extent. There is an enormous lot of research to be done in this field. Finally, it goes without saying that the specification of "natural" processes must depend not only on language typology but also on advances in the phonetic analysis of motor processes and perceptual processes. I do not think that one should distinguish rigidly between "competence" and "performance" in this context.

As I see it, what one can accomplish at present is to put constraints on phonological descriptions which make these somewhat more PLAUSIBLE hypotheses about internalized phonologies, and at the same time provide a better framework for statements concerning a variety of dynamic phenomena such as language acquisition, fast speech and speech errors, and language change, which in turn may provide crucial evidence for the theoretical constructs. Also from the point of view of strictly descriptive work (with no ambitions concerning psychological reality) a theory that is maximally constrained by substantive universals may offer a better chance of describing the phonologies of different languages in an analogous fashion so as to make the descriptions comparable for typological applications.

Naturalness should not be equated with the psychological

reality issue, although there is an affinity, of course. It seems immensely plausible that natural, universal tendencies play a major part in language acquisition, language change, etc. But we hardly arrive at psychologically real, static descriptions of adult persons' internalized phonologies just by referring to naturalness. It may be possible, for example, to set up some strict learnability criterion, but how do we know that every internalized phonology behaves in accordance with this criterion? Maybe internalized information may be rearranged in strange "unnatural" ways. And it need not make sense to ask whether a certain regularity is represented explicitly or whether it is simply implicit in the internalized lexicon. Maybe it is both, in many instances. We may guess that there are all kinds of redundancies in internalized representations, and all kinds of short-cuts in language processing. If we wish to make claims about internalized phonologies on the basis of a theory of natural phonology without having access to the mental processes of the speakers whose language is being described, there is probably nothing more to do than to state the simplest and at the same time most complete account of the observed data which is consistent with the theory. It seems reasonable to attempt to delimit phonological descriptions in such a way that they contain all and only the phonological generalizations which may possibly be utilized, under normal conditions of language use,¹ by speakers and listeners employing the language in question. Doing just that requires a working definition of LINGUISTICALLY SIGNIFICANT GENERALIZATION which supplies us with a real criterion. Maybe that is the highest goal one can reasonably set for linguistic theory in the present phase of the strive toward realism.

1) That is, including "creative" use of language but excluding introspection for the purpose of stating generalizations and the like.

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