

ABSTRACT PHONOLOGY AND PSYCHOLOGICAL REALITY

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For a number of years now abstract phonological descriptions have come under attack from two different but often related quarters.¹ Firstly, it has been claimed that even within the broad framework of standard generative phonology less abstract solutions are often available; reinterpretations of the data have been achieved by suggesting that certain putative phonological contrasts are in fact morpho-lexical generalisations, i.e. morphologically and lexically rather than phonologically conditioned. Re-analysis or change of underlying representations has also been offered as a viable alternative to manipulating abstract segments and opaque rules. Finally, various modifications in the rule component have been shown to lead to less drastic departures from phonetic representations than those called for by (relatively) abstract positions. The drive towards concreteness seems to have culminated in the rise of so-called 'natural generative phonology' of Vennemann, Hooper and others although a whole range of more or less abstract views has continued to exist; in fact these radically concrete positions are coming under attack now even from those linguists who generally favour concreteness in phonology (cf. Goyvaerts 1978, 125-133). In any case, the type of criticism of abstract solutions that is normally based on evidence internal to the structure of the language cannot be meaningfully discussed without taking into account the grammar as a whole, and this is obviously precluded here. It can be safely assumed that less abstract solutions will be acceptable even to those linguists who favour abstractness in phonology if it can be shown that abstract interpretations are not necessary, i.e. that either the required generalisations can be made without recourse to the abstract machinery or else that the generalisations are in fact wrong and must be replaced by others. It is perhaps worth stressing that in order to evaluate such arguments and counter-arguments one must consider not just individual pairs of rules but rather the phonology as a whole; there has been far too much specula-

(1) The bibliography of the subject is vast and would require several pages. In this report I have restricted myself to just a few items which are directly relevant to the discussion.

tion based on scattered examples and even on inaccurate data.

The other line of attack on abstract positions has involved external evidence which has come to be known as substantive evidence. It has been claimed that the generalisations captured in abstract descriptions are not those that speakers of the language make, i.e. that the abstract generalisations are, in a nutshell, a figment of the linguist's imagination devoid of any psychological reality. This line stresses the need to go beyond the structural facts of the language in search of support for true generalisations. Substantive evidence for such psychologically real regularities has been sought in historical change, the treatment of borrowings, in language acquisition and language loss (aphasia), metrics, dialectal variation, speech errors, secret languages as well as in direct phonological experiments (see Fischer-Jørgensen 1975, 290ff and Zwicky 1975 for good surveys). These are important findings which certainly cannot be overlooked by anybody seriously concerned with psychologically real phonology. They must, however, be handled with extreme caution given the present understanding of the ways in which language is actually used since, as was judiciously observed by Dressler (1977, 224), "the more modalities of external evidence one uses, the more divergent and incoherent results one gets". Let me consider just a few cases.

Polish has a general and typologically very natural rule of devoicing obstruents word finally. In actual speech one often finds that the rule is suspended in certain cases, e.g. in regularly used foreign words and names whether completely assimilated into the language or not - gro[g] 'grog' rather than gro[k], ko[d] pocztowy 'postal code' (in spite of the fact that [d] precedes a voiceless plosive!), possibly because the unvoicing would produce here the humorous kot pocztowy 'postal cat'; in native words it is also suspended for a variety of reasons as in dó[b] '24 hrs., gen.pl.', where the unvoicing would produce a somewhat improper word. Surely no one would like to conclude from such examples that terminal unvoicing is not a psychologically real rule in Polish. Generally speaking, foreign words exhibit specific properties, and most schools of phonology have reflected this fact in one way or another (in addition it seems that one should also recognise varying degrees of foreignness). The fact that some foreign or occasional native words (including, possibly, nonsense words) do not appear

to have undergone a rule cannot be taken as direct evidence for the non-reality of the rule.

Historical evidence, one of the most important sources of substantive evidence, is notoriously difficult to handle in that the paucity or lack of reliable and unambiguous data is not the only factor hampering definite conclusions; any interpretation of change for purposes of verifying general theoretical claims involves assumptions about the mechanisms of change which themselves are not well understood and it also involves assumptions about e.g. the interface between the rules of morphology and those of phonology which is likewise largely unexplored. In view of these problems it is not surprising that examples can be found in the literature purporting to justify both abstract and concrete positions by use of such evidence. The metric evidence available from the works of Kiparsky, Anderson and others seems to support the level of remote representations although, given the variety of theoretical machinery accessible to current linguistic thinking, alternatives could presumably be found.

Slips of the tongue have figured prominently as the window to psychologically real grammars, and Fromkin's (1971) seminal paper has stimulated a lot of interest in this area. Some of her evidence has now become part of the stock-in-trade of those arguing for abstract regularities as, for example, the celebrated case for /ng/ as underlying the phonetic [ŋ]. It would be easy for somebody trying to defend abstract phonology to claim that if /ng/ underlies [ŋ] in a psychologically real sense, then speakers of English must have at their disposal means of arriving at the abstract solution given the data internal to the language. These means could then be generalised to cases where no external evidence can be adduced; this is the position adopted by Kenstowicz and Kisseberth (1977) who incidentally find that the case of the English velar nasal violates all of their constraints on the abstractness of underlying representations. Such evidence is intriguing, but supporters of concrete phonology could easily dispose of it by viewing the slips as resulting from the influence of spelling or something else. I would like to further emphasise, however, that important as such evidence may be, it is not obvious whether much use can be made of it until more is known about the interaction of linguistic knowledge and language use. In our particular case we need some sort

of theory of speech errors against which we could evaluate individual instances for their linguistic significance since one frequently observes not only slips of the tongue that can be shown to reveal something about the underlying reality of language but also instances of errors that appear to make "no sense" linguistically. It is also worth mentioning that different areas often provide contradictory evidence (cf. also Dressler's remark quoted above). The following might be a possible example: slips of the tongue adduced by Fromkin appear to suggest that affricates should be treated as single segments phonetically in English. On the other hand, optional low phonetic rules frequently simplify affricates to spirants in certain contexts so that French and orange end in [ʃ] and [ʒ]. This, of course, could be interpreted as a change in the feature /cont/ but since one also finds the deletion of alveolar plosives in such words as rents, sounds, it seems more plausible to treat both these changes as cases of deletion of the plosive between a nasal and a spirant. This would require, however, that affricates be clusters at some stage in the derivation.

The need for the study of the ways of utilising linguistic knowledge in speech is further confirmed by some surprising results obtained from direct phonological and grammatical tests. Earlier studies attempted to show that certain rules of the SPE phonology are not psychologically real as speakers fail to apply them to novel forms (nonsense words). Haber (1975) has shown that contrary to what might be expected speakers of English do very badly in tasks intended to test the productivity of the regular plural formation rule (the -(e)s ending), i.e. one that with good reason is generally assumed to be fully productive. It does not matter here whether the relevant mechanism is purely phonological, morphological or something else (the rule is transparent and could be formulated in surface terms). If tests fail to confirm the psychological reality of this simple rule, then most linguists would agree, I suppose, that there is something fundamentally wrong with the tests themselves; Kiparsky and Menn (1977, 64) ascribe it to "a "strangeness effect" which causes the subjects' performance to deteriorate relative to their normal speech" and are also (66-67) "skeptical about the ability of production tasks to show much of anything, at present, about the form of internalized linguistic knowledge, given the near-total obscurity surrounding the question of whether

and how this knowledge is used in speech".

As far as other areas of substantive evidence are concerned let me just mention two points: firstly evidence from an aphasiological study by Stark (1974) strongly suggests that the German velar nasal should be regarded as being derived from underlying /ng/, and this thus strengthens the case for an abstract interpretation of this problem vis-a-vis the stand taken by natural generative phonologists. Secondly, there is the case reported in Kiparsky and Menn (1977, 69-70) of an "invented language" which appears to exhibit two rules extrinsically ordered, which would indicate that the ordering of rules in itself cannot be difficult or impossible to learn as has been sometimes claimed. As Kiparsky and Menn point out, the charge that synchronic rule order mirrors diachronic developments cannot be made against speech invented by children.

The above discussion has not been meant to decry the importance of substantive evidence; conversely, in view of its potential significance I think it is necessary to stress that there is much in it which is arguable and which is itself in need of explanation and so can hardly be taken as definitive evidence for other theoretical concepts.

One final point that I would like to make is that the theoretical apparatus of abstract phonology is required to account for uncontroversially related, low phonetic details of pronunciation (see also Kiparsky 1975). Modifications, permutations, deletions and insertions of segments are well-known not only from abstract derivations but are also exceedingly common in accounts of rapid speech phenomena; thus, there is nothing basically new about abstract derivations that could not be found closer to the surface. Examples of the various modifications are well-known, and I would like to present a couple of examples from Polish where allegro rules introduce segments and contrasts totally absent from lento speech.² The phonetic inventory of Polish vowels contains six basic elements [i, ɨ, ε, a, ɔ, u], thus being again fairly regular typologically. Allegro forms introduce on the one hand a contrast of length which

(2) The examples are taken from Biedrzycki (1978) who interprets such data in terms of autonomous phonology and sets up phonemic distinctions for allegro styles which do not appear in lento styles.

does not appear in slow speech, e.g.: da 'she gave' [da:] vs. da 'she will give' [da], stó 'table' [stu:] vs. stu 'of a hundred' [stu] corresponding to the lento forms [dawa - da] and [stuw - stu], respectively, and also several segments which are not known elsewhere, e.g.: in sp[ə:] czeństwo 'society' cz[ə:]m 'hi' - lento sp[ɔwɛ] czeństwo, cz[ɔwɛ]m; zapomni[ə:]m 'I forgot', chci[ə:]m 'I wanted' - lento zapomni[awɛ]m, chci[awɛ]m; cz[o:] 'one felt', ok[o:] 'one shod' - lento cz[uwɔ], ok[uwɔ]. The low level, optional rules which produce such forms are psychologically real and by producing new contrasts they seem to work like absolute neutralisation in reverse. If we were to postulate length contrast phonologically for Polish and then absolutely neutralise it, the abstractness sin would be committed; speakers of the language, however, seem to find nothing unusual about neutralising certain contrasts and introducing new ones when passing from lento to allegro styles. The force of these examples should not be overstated but they seem to show that there is nothing abnormal about rules merging and producing contrasts or about segments which appear at one level of representation but not at another.

The abstractness debate will no doubt continue both on language internal and external grounds. There remains much to do in both areas so that any final verdict at this stage would be premature.

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