

P h o n o l o g y

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PHONOLOGY

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The report to follow is a personal evaluation of some trends in phonology which have been more or less dominant since the last congress, as well as an overview of a subjective selection of individual contributions to phonology during the same period.¹ It is the reporter's hope that the most important lacunae of the text and bibliography will be filled by the co-reporters and the audience, and that the personal form and content of the report will provoke rather than prevent discussion.

A few words should be said about what will at most be covered in passing in this report. A number of the most central issues in current phonological debate have been selected as topics for semi-plenary symposia: Phonetic universals in phonological systems and their explanation, The psychological reality of phonological descriptions, Acquisition of the phonological system of the mother tongue, Social factors in sound change, and The relation between sentence prosody and word prosody (stress and tone). Consequently, these subjects will only be mentioned briefly or not at all in the present paper, and we shall not devote much attention either to the topic of the syllable in phonological theory, which will be treated in a working group.

Whereas section 1 is devoted to some general points characteristic of post SPE models of generative phonology (in the broad sense), there is no section of the present paper covering exclusively non-generative types of phonology. Some new theoretical developments of general interest within such theories will be mentioned in sections 2 and 3, however. It is outside the aim of this report to cover phonological descriptions of individual languages which are not intended to be contributions to phonological theory as well. The above principle of demarcation for this report is of course by no means to be taken as implying that phono-

1) I am indebted to the following people who have made a number of useful comments (both concerning content and style) on an incomplete version of the manuscript: Laurie and Winifred Bauer, Niels Davidsen-Nielsen, John Dienhart, Stig Eliasson, Eli Fischer-Jørgensen, Leif Kvistgård Jakobsen, Per Linell, and Jørgen Rischel. Unwisely, I have only followed some of their suggestions, and the responsibility for all flaws of the paper is, of course, mine alone.

logical analyses of individual languages are without scientific merit or interest. On the contrary, such descriptions are the fundament of our discipline, and a theorizing without a solid foundation in careful phonological and phonetic analyses, using field work and not merely reinterpreting the findings of others, is doomed to be an image (beautiful though it may be) with feet of clay.

1. Some trends and developments in generative phonology

1.1 Is there a school of generative phonology?

In his interesting overview with the characteristic title "Phonology since generative phonology", which in fact almost exclusively covers "the field of natural phonology", Bailey writes (1976, 5): "The writer's European experience convinces him that many linguists outside America believe that the newer phonology is just another development within generative phonology. For the most part, this is certainly not true. In fact, most natural phonologists rebelled as early as 1968 against generative phonology, often against the entire framework...". This point of view is not uncommon, but is nevertheless only true with certain modifications. In addition to the historical continuity of persons (and partly of institutions in the widest sense) natural phonology - and one could include other theoretical developments as well - is a continuation of standard generative phonology in the following two respects: there still seems to be a common basis of argumentation or, to put it in a simple-minded fashion, the various scholars speak closely related languages and understand each other reasonably well; and finally, but importantly, there is a crucial common core of theoretical references, both as concerns published work and, more fatally, semi-published or privately circulated papers. This may be less of a linguistic problem and more related to the field of the sociology of science, but I think that the notion of a linguistic or other scientific "school" belongs to the latter sphere too. All this is not to deny the existence of fundamental disagreements between standard generative and, say, natural phonology, but only to make clear why I think it is reasonable to speak of a generative "school" of phonology in the following. Opponents of the present view should compare standard generative phonology to both natural phonology (to see the similarity) and to, say, the "functional" trend of Martinet, or to stratificational phono-

logy, or to Soviet phonology (except Šaumjan), just to see the difference. Not only is generative phonology (including the field of natural phonology in the broad sense of Bailey 1976²) a "school" in this sense³, it is the dominant school as shown by the many references to it in phonological work of other theoretical persuasions, whereas reference within the generative school to competing theoretical views outside the school is frequently scarce, to say the least (except concerning sources of data or such predecessors to the school as Bloomfieldian linguistics). That insiders are in general less willing than outsiders to identify their own scientific context as a school is not a surprising state of affairs.⁴

1.2 Two main trends in generative phonology

I believe it is possible to discern two main directions within the evolution of later generative phonology, although all such rough categorizations must, of course, be taken with at least a grain of salt. The trends, or attitudes, which I have in mind might be termed "substance based" and "formal", respectively. To clarify this proposed distinction and its relation to standard generative phonology, let us briefly consider what may happen if a certain formulation of a phonological rule, e.g. from SPE, is taken really seriously.

One can focus upon the correspondence between the rule as stated, or even one part of the rule, and observables, i.e. give the rule a direct psychological interpretation or interpret it in

2) Bailey distinguishes four different trends within "Natural phonology": NP in the original form of e.g. Stampe, Drachman and Dressler (cf. now Donegan and Stampe, forthcoming); "Natural generative phonology" or "Concrete phonology I", worked out by Vennemann, Hooper and Rudes; "phonetology" or "dynamic phonology", i.e. Bailey's own trend; and finally, the phonology of e.g. Wang and Chen (which Bailey terms "Concrete phonology II"). One should be careful not to overlook the differences between these four types of "natural phonology", e.g. with respect to the distinction between "formal" and "substance based" generative phonology (section 1.2).

3) Of course, many levels of subschools seem to exist in this sense, both dominant and dominated. Consider, e.g., the fact that Hooper's textbook (1976) makes no reference to Bailey's work.

4) This observation also applies to non-generative linguistics, of course. Consider, e.g., the common claim among Danish linguists that there never was such a thing as a "Copenhagen school" (cf. Fischer-Jørgensen 1975a, 114). In the sense used here, this is not quite true, but it may of course be correct in other respects (as argued in Fischer-Jørgensen, loc. cit.).

some other way which is directly comparable with certain facts (cf. section 2 about 'evidence'). Such a "substance based" attitude is hardly compatible with a very "abstract" interpretation of the competence:performance distinction: it probably presupposes that competence is a 'competence for performance' where the path between the two is direct, short and due to factors which are in principle known (e.g. from studies of memory limitations).

An elegant example of a critical piece of argumentation which merely interprets (in the above sense) the details of a number of rule formulations taken from SPE is Stampe 1973. John Ohala and others have tried to test such rule formulations directly. An increasing number of authors have come to realize, however, that it may be a better research strategy (further see the end of the present section) to try and identify natural processes from "external" but "real life" data (sound change, speech disturbances, fast speech phenomena, language acquisition, etc.) and to consider each rule, or sometimes even a block of several rules, a unity for that purpose (on different types of rules, cf. section 1.4). The focus of interest is thus no longer isolated rule formulations, or even their parts, but functionally defined processes.

The "formal" trend alluded to above takes the formalism seriously in a different way. It is not the psychological or other empirical interpretation of the single parts of the formalism which is in focus (apart from certain premature claims about what is "psychologically real"), but partly the formal ingredients of the system (such as 'constraints' and a number of alleged 'formal universals' supposed to be innate, all with the purpose of narrowing 'the class of possible grammars of a natural language'), partly the generative capacity of the system as a whole (in this case considered as a black box). This trend agrees with Chomsky's position as evidenced in, e.g., "Conditions on transformations" and "Reflections on language"⁵, and it is particularly well represented in the recent journal Linguistic Analysis (which according to its characteristic subtitle aims to cover studies in formal syntax,

5) Chomsky's earlier position, on the other hand, has clear connections with logical empiricism, as evidenced e.g. by the striking similarities between "Syntactic structures" (1957) and the introductory part of Carnap's "Logische Syntax der Sprache" (1934).

semantics and phonology). The strong Chomskyan position may be illustrated by a handful of quotations from Koster et al. (1978, 3-4): "Human cognitive behaviour involves the interaction of diverse cognitive structures. [...] A direct route to performance, use, process, and the like, seems ill-conceived, because it would involve the result of interacting factors that are themselves unknown. [...] The analysis of cognitive structures has to precede the study of the enormously intricate synthesis which we call behaviour [...] The kind of cognitive psychology we advocate therefore rejects the holistic study of behaviour as hopelessly premature." The ultimate goal is "to account for the language faculty, and hence for the linguistic theory (the theory of Universal Grammar), in terms of human biology." There are a number of epistemological and methodological problems in this attitude (cf., e.g., Derwing 1973), but it is an interesting and maybe surprising fact that this strong Chomskyan position seems to have had hardly any consequences for linguistic analyses and argumentation as compared with that of other formalists within the generative school (like Milner) who reject that their object of study is anything like the state of a mental organ: in fact, only linguistic evidence is accepted.⁶ Regardless of whether or not adherents of what I have labelled the formal trend of generative phonology consider their discipline as being a branch of cognitive psychology (in the Chomskyan sense), the analyses and explicit argumentation are thus in general intra-linguistic, and evidence from psychological tests and the like is quite generally not considered (due to the con-

6) Milner has concisely formulated his position like this: "Les propositions de la linguistique sont falsifiables, mais ne le sont que sur la base d'une évidence tirée des langues elles-mêmes. Aucune falsification tirée de l'évidence psychologique (ou biologique, ou de quelque ordre non-linguistique que ce soit) n'est donc pour moi admissible. Ce qui me frappe, c'est que cette position est celle de tous (ou presque tous) les linguistes génératifs, y compris ceux qui admettent [que la réalité du langage et des langues soit de substance essentiellement psychologique, et qu'une réalité psychologique soit un état spécifiable d'un organe mental]. J'en conclus que [les deux propositions entre crochets/HB] ne jouent aucun rôle réel dans la construction de la théorie linguistique" (1978, 9).

ception of the competence:performance distinction).⁷

These two directions of evolution within the school of generative phonology have been distinguished and presented in this way mainly for expository reasons. Although most concrete phonologists belong to the "substance based" trend, there is also a certain amount of formality here; and even though e.g. the scholars around Koutsoudas are formalists in the sense used here, they in fact sometimes make use of substantive evidence. In short, the bifurcation presented here is based upon several elements which are logically and empirically distinct, and furthermore the "substance based": "formal" distinction is not strictly binary: the two terms mark the endpoints of a scale.⁸ A version of this scale has sometimes been known as the "abstract:concrete"-opposition, crucial to all phonological theory and practice, and the first of the converging tendencies we shall consider below is precisely the non-abstractness of lexical representations.

1.3 Non-abstractness of lexical representations and the issue of directionality

Abstraction is an inescapable condition for all sorts of descriptions including scientific ones, i.e. some aspects of the object to be described must necessarily be disregarded in order to obtain a description. However, the notions of "abstraction" and

7) Per Linell (personal communication) interprets the distinction between the "substance based" and the "formal" trend like this: Phonologists belonging to the former trend aim at describing language specific rules of certain ("linguistic") aspects of the production and perception of speech, whereas the latter type conceive of significant phonological generalizations as pertaining to much more abstract ("cognitive" or "mental") principles, presupposing - rather arbitrarily - that intralinguistic methods can yield such "cognitive" results.

8) More than anything else, I think the two proposed trends differ with respect to research strategy: The "formal" phonologists consider the rules and notation as given for a certain purpose, thus drawing conclusions concerning the interaction of rules etc. from the notation (cf. the use of models in theoretical physics). The "substance based" phonologists, on the other hand, do not accept any proposed rules without recourse to data outside normal linguistic behaviour (cf. certain "empirical" types of psychology). The two trends thus differ with respect to their general confidence in the proposed formal systems of phonology. Both attitudes may per se be scientific, their difference lies mainly in what they consider fruitful lines of research in the present state of our phonological knowledge (cf. section 2).

"abstractness" play a more crucial role in phonology than in most other scientific disciplines (including linguistic ones), since one distinctive trait of phonology as compared to phonetics can be claimed to be one of abstractness, with the further proviso that what has disappeared as a result of the "abstracting away" or reduction (together with the linguistic and non-linguistic context, and so on) is the phonetic details.

The above remarks apply to both generative and structural phonology. And in fact there seem in principle to be two distinct ways of abstracting from phonetic details to phonological forms (for discussion, see e.g. Rischel 1974, 361-365)⁹: One can either remove more and more redundancy from the class of possible pronunciations - within the language norm in question, of course - of a given word form; or one can go backwards in the derivation, so to speak, within a rule component constructed to account for (morphological) relatedness between different word forms. Although both of these types of abstraction have been used in both structural and generative phonology, the emphasis laid in these two theories clearly differs: structural phonology favours the first type, generative phonology the latter. The notion of surface contrast, which is essential in many structuralist schools of phonology, is reasonably well defined¹⁰ except for the possible identification of members of different inventories belonging to distinct positions in the chain. If one goes further toward abstract forms, however, it is hard to find non-arbitrary criteria for where to stop the abstraction, in structuralist as well as in generative types of phonology.

9) It might be added, however, that this should not be taken to imply that semantics or pragmatics is necessarily more abstract than phonetics, although this implication may be tempting to both phoneticians and generative linguists. I should rather say - from a European structuralist point of view - that phonology is an abstraction vis-a-vis phonetics, in much the same way as semantics is an abstraction vis-a-vis pragmatics.

10) Bailey's interpretation of the "traditional phoneme" (1976, 14f) does not seem quite fair to me, e.g. as regards the Prague school notion of the phoneme (including the concepts of 'neutralization' and 'archiphoneme', which have now been revived in natural phonology): "-merely a redundancy-free phone. What few (less than a dozen) predictions, trivial or non-trivial, can be wrung out of this now ancient artifact all seem to be wrong -- not least those involving linguistic change and psychological reality".

As mentioned in the previous status report on phonology (Fischer-Jørgensen 1975b), one main development in the early seventies within generative phonology was in the direction of more concrete analyses. This trend could be seen partly as a reaction against very abstract phonologies as exemplified by Schane 1968, SPE, and numerous works by Lightner.¹¹ The problem with these abstract analyses was, of course, that they were consistent applications of the basic principles of generative phonology, and at the same time it appeared intuitively evident to most phonologists that they were highly implausible candidates for being components of a grammar which purportedly should be psychologically real. The fundamental reason why Schane, Lightner and others could reasonably arrive at such abstract analyses is that there was no operational criterion for the degree or type of relatedness between two word forms which would decide when one should posit a common underlying form and rules to make the derivation work (cf. Rischel 1978); and the simplicity criteria in use favoured common base forms in cases where a number of 'apparently unrelated' word forms could be related with only modest cost of rule complication (the generalizations were presupposed to be 'linguistically significant' but this concept had not been operationally defined either; however, cf. now Hurford 1977). Until today, not very much progress has been made concerning the establishment of criteria for relatedness between word forms (but B. Derwing has initiated research in that area). Instead, a number of authors have taken another route to reduce the run-away abstraction which can be tolerated in standard generative phonology: to find explicit constraints on the abstractness of the analyses, either on the lexical representations, or on the rules or the way in which they interact (see the next section), or in a combination of these.

A number of authors (e.g. Vennemann, Linell, Hooper, Rudes) - some of them even with a markedly 'abstract' past - have proposed (more or less) similar criteria on lexical representations

 11) The position of Foley (1977) is quite isolated: He criticizes SPE-phonology (which he rebaptizes "transformational phonetics") for being much too concrete, and favours a very abstract, non-psychological phonology. His theoretical views are reminiscent of those of glossematics about immanence and substance-independent glossemes. The present writer agrees that SPE argues too much from the notation, but I fail to see why one should exclude oneself from phonetic explanations, e.g. in the case of strength hierarchies (cf. section 3.2).

to the effect that these should correspond to surface forms in distinct pronunciations, but not necessarily with detailed phonetic specifications. Such a constraint gives rise to reasonable analyses, e.g. in Hooper's version (1976). It should be pointed out, however, that if the lexical representations are hypotheses about how speakers actually store their phonological information regarding individual lexical items, then they should in principle be falsifiable by "external" criteria (it is evident that analyses are not "psychologically real" just because they are concrete, cf. section 2). On the other hand, if the lexicon is seen as a collection of phenomena - in this case pertaining to pronunciation and perception - which are not predictable by rule, then the lexical representations cannot be considered hypotheses about anything outside the grammar itself (and thus empirical vacuity may result), since they will then be negatively defined by the notion 'rule', which in this context seems to mean any regularity that can be stated.

If the lexical representations are claimed to have some sort of psychological reality, it will of course be no argument against the anti-abstract proposals just mentioned that they are highly redundant (this would presuppose an additional premise to the effect that information is stored in the brain in the most economical (compact) way, whereas the amount of computation needed to derive the actual forms, as well as different forms of retrieval, are less 'costly' for the overall system). One may challenge the plausibility of such concrete lexical representations as proposed e.g. by Rudes 1976 (syllabified whole but phonetically incompletely specified word forms) in view of (1) the amount of fully productive (both semantically, morphosyntactically and phonologically) formation of words, particularly in languages like Eskimo, and (2) the human ability to syllabify sound chains according to rules, in slow-careful speech as well as in allegretto speech, etc. It must be remembered, however, that the possible psychological reality of the lexical representations is an empirical issue that should be subjected to rigorous testing, but this is only possible after a further clarification of the notion 'psychological reality' (cf. Linell, forthcoming).

A consequence of the postulation of more concrete lexical representations may be that phonological rules are divided into

more abstract ("pre-lexical") rules (morphophonological or the like), and more concrete ("post-lexical") rules (phonetic or the like). A division of phonology into two types of phonological rules, 'abstract' and 'concrete', by no means presupposes concrete lexical representations, however. This issue will be taken up in the next section.

Another conceivable constraint that would automatically reduce the abstractness of lexical representations is the claim that all phonological rules should be bidirectional, or inferable, or (directly) recoverable, i.e. that the underlying form should be inferred from the surface (different formulations of such a constraint are possible, and it may pertain to rules, representations, or both).¹² The True Generalization Condition as used in Hooper 1976 (which in a sense generalizes proposals of Stanley 1967) in fact is such a constraint. Even authors who do not favour such a strong constraint have made use of the notion of recoverability, e.g. Gussman (1976). Eliasson in a number of interesting papers explores the notions of 'unidirectionality' and 'bidirectionality' in phonology, and he concludes that bidirectionality plays a much larger role than is usually ascribed to it in generative phonology.¹³

To end this section, let us briefly consider an apparently somewhat bizarre variation of generative phonology which nevertheless is not without virtues, viz. Leben and Robinson's "Upside-down phonology". Its basic idea is that the lexical representations are concrete surface forms (following Vennemann 1974), and that the whole machinery of e.g. SPE operates in the reverse of

12) It is evident that the formulation of the rules has an impact on the formulation of the lexical representations, and vice versa, and thus even strong constraints on only one of these factors may have very little over-all effect on the abstractness of the theory as a whole.

13) This renewed interest in bidirectionality is not only reminiscent of the bi-uniqueness criterion of Bloomfieldian phonemics, but also, e.g., of the stratificational classification of relations between levels in terms of neutralization, diversification, etc. As shown by Eliasson (e.g. forthcoming), there is clearly much insight to be gained from combining those structuralist viewpoints with the findings of generative phonology, and he explores e.g. various kinds of antiambiguity restrictions and historical restructurings which give substance to the notion of (partial) interconvertibility between levels.

the usual order (and thus ordering is necessarily extrinsic), not to determine the phonetic output (which was there in the first place), but to decide whether or not two forms are (morphologically) related (note that relatedness is thus not taken as something primary, as opposed to the rules). One undoes the phonological rules, one by one (and backwards, as stated), of the two word forms to be compared, and if they ever get alike during that process, then they are related. In fact, this restructuring of the standard generative model (into a parsing model) has a number of favourable effects, in particular concerning the notion 'analogy', as argued in the paper (although the criticisms of an overly concrete lexicon, of course, apply here too). One consequence of the model, when interpreted psychologically, is that surface similarity necessarily overrides paradigmatic regularity as an indicator of relatedness: e.g. obese-obesity (without vowel shift) are related by a 'shorter derivation', and thus - when the model gets a direct psychological interpretation - would seem more related (and, at any rate, not "exceptionally" related) than normal pairs like obscene-obscenity (with vowel shift). In that respect the "upside-down phonology" is not just a reinterpretation of the standard generative phonology.

1.4 Functional variety of rules and their order of application

One major convergence in recent generative phonology (in the broad sense used here) is the division of phonological rules into at least two different main types: 'abstract' or 'morphophonemic' as against 'concrete' or 'phonetic' or 'allophonic' rules, sometimes called 'processes'. It should be pointed out from the outset that this dividing line falls within phonology as opposed to (pure) phonetics, i.e. it is not identical to the distinction between phonological rules proper and phonetic detail rules, e.g. in SPE, where the difference is that the distinctive features (at least the non-prosodic ones, in contrast to e.g. stress) are all binary in the former case, whereas they are 'scalar' in the latter (in a framework which permits non-binary distinctive features at the phonological level, cf. section 3 below, the characteristic trait of phonetic detail rules may reasonably be that the features vary continuously within a certain scale). If coarticulation effects (or even the fraction which may be language specific) should be accounted for by rule at all, it is certainly not by the type

of phonological rule used in generative phonology (and thus arguments like that of Bach 1968, repeated many times since then, to the effect that e.g. fronting of velars between front vowels is crucial evidence concerning the formal nature of rules and the simplicity metric seem misconceived from the outset). The new convergence described above is thus a dividing line within phonology itself, supported by a number of authors like Vennemann, Hooper, Bailey, Linell, Rischel, Drachman, Dressler and Koutsoudas. The dividing line is reminiscent of Kiparsky's (1973) distinction between neutralizing and allophonic rules. But in fact, a number of criteria which have been used, or may be used, do not classify rules in quite the same way (see Linell 1977 on a functionally based typology of phonological rules; also cf. Brasington 1976 and Dressler 1977a).¹⁴ What is new is not only the distinction between an 'abstract' and a 'concrete' part of phonology, but also the emphasis on the latter.

In contradistinction to the authors mentioned above, Stephen R. Anderson (1975), while accepting the typological difference between 'morpholexical' and 'phonological' rules (with 'phonetic' rules as a third category, cf. above), claims - although not all of his arguments are wholly convincing to the present author - that they are interspersed (but he emphasizes that it may, in *casu*, be natural for a morphological rule to precede a phonological rule).

Some advances have been made in our understanding of the notion 'optional' rule (cf. Sanders 1977), partly from socio-linguistic investigations (e.g. by Labov and his associates). Also the influence of paralinguistic factors like speech tempo (cf. also Bolozky 1977) and style variation (as opposed to non-linguistic factors like sex, age and socio-economic group, layer or class), have come into the focus of attention, thanks not least to the work of Dressler and his colleagues. Due to such careful investigations, the psychological reality of word reduction phenomena has become apparent, as opposed to the realities described by many

 14) I should like to emphasize the following distinction which is not always observed in the literature: a phonotactic constraint (or condition) states which structures are permitted or prohibited, i.e. it is an intra-level notion; a phonotactically conditioned (or better, motivated) rule indicates only one means to obtain a certain phonotactic result and is thus an interlevel notion (the effect of such rules recalls what Kisseberth baptized 'conspiracies').

other phonological rules (cf. section 2).

Two of the criteria for the classification of rules mentioned in the present section, viz. morphophonological vs. phonetic and obligatory vs. optional, play a role in a certain general attitude to phonology, viz. one which adheres to the claim that all orderings in phonology can be predicted from a set of universal principles. A group of scholars around Koutsoudas (including Sanders, Noll, Iverson and Ringen) have investigated this hypothesis in a number of studies (starting with Koutsoudas et al., 1974), and a recent summary of the principles (Ringen 1976, 55f) lists the following: (1) The rules are scanned after each rule application to determine which rules are applicable to the new representation; (2) an obligatory rule must apply everywhere that its structural description is met unless some other principle predicts that it cannot apply; (3) rule A takes applicational precedence over rule B if the structural description of B properly includes the SD of A; (4) a derivation is completed when no more obligatory rules are applicable (and no more optional rules are opted for); (5) no rule can apply vacuously in any derivation (Ringen 1976, 57); and there is in addition a further principle (6) allowing consecutive and preventing nonconsecutive reapplications of a rule (a formulation is given in Ringen 1976, 62). It will be seen that principles (1) and (6) together with (4) and (5) define how rules are scanned and what counts as application and termination. (3) decides some cases where more than one rule is applicable, and further principles of this sort may be, and in fact have been proposed, e.g. that a morphophonemic rule takes precedence over an allophonic one (Koutsoudas 1977). Principle (2), finally, does not belong in any one category: it is partly a 'principle of precedence' (obligatory precedes optional), but, as pointed out by Ringen (*op. cit.*), that may be seen as a simple consequence of the meaning of the notion 'obligatory'; and the phrase 'unless some other principle predicts that it cannot apply' is a principle about the hierarchy among the principles themselves (viz. with respect to (3) here), i.e. a 'metaprinciple'.

The work just mentioned above clearly belongs to the "formal" trend of generative phonology (see section 1.2), and it is still controversial whether extrinsic ordering can be dispensed with within such a framework. Notice that this theory still allows

rules to be crucially and intricately ordered in derivations. In contradistinction to this, 'no ordering constraints' have also been proposed (e.g. by Vennemann) within natural phonology, i.e. within a "substance based" trend.

Two other directions of work within the "formal" trend of generative phonology concerning the application of rules deserve mentioning in the present context: One is the theory of local order proposed by S.R. Anderson (see e.g. 1974). A number of his original examples in favour of local order have been challenged recently (e.g. by Leben and Ringen), but I think one of my coreporters will give more information on that point if needed. The other is the phonological cycle in the SPE-sense (as opposed to e.g. that of Niles 1976, where the name 'cycle' is coupled to stylistic variation), which has been argued for by Brate and others. It is the opinion of the present writer that the phonological cycle in its original sense is unwarranted as a theoretical notion, cf. Rischel 1977.

In several papers (e.g. Basbøll 1975 with reference) I have generalised and applied McCawley's notion of rank so that every rule should apply with one of a small set of linearly ordered boundaries, including the syllabic one as its rank, i.e. with a string following by the boundary in question (or a stronger one) as its domain, and that a boundary should be allowed to occur properly only if it is the end of a rule. Furthermore, rules of a lower rank should apply only after a complete set of rules of a higher rank, and this may be taken as a suggested further principle of precedence in the SPE sense: a theory of 'no extrinsic order'.

References and sources in phonology

In the introduction "Phonology: Linguistics as Metaphysics" with the subtitle "On the rationality of non-empirical theories", Lass (1977, 1978) has drawn attention to the well known generative criticism for "metaphysics" in opposition to "metaphysics" or "philosophical" (1977, 1978) in the theory of the structure of the material. Lass con-

In the introduction "Phonology: Linguistics as Metaphysics" with the subtitle "On the rationality of non-empirical theories", Lass (1977, 1978) has drawn attention to the well known generative criticism for "metaphysics" in opposition to "metaphysics" or "philosophical" (1977, 1978) in the theory of the structure of the material. Lass con-

cludes that most linguistic theories are not 'scientific' in this sense, in particular not Chomsky's theory of grammar although its creator repeatedly calls it so (this, of course, is not new, cf. well known criticisms by Botha, Derwing, Linell, and Itkonen): "If refutability is the hallmark of scientific theories, and if the empirical content of a theory is in direct proportion to its refutability, what are we to make of the majority of theoretical proposals in linguistics? [...] It is quite clear that many of them are infalsifiable for structural reasons. That is, they make claims for which no 'crucial experiment' or even reasonable testing procedure can be devised" (1976, 215). His own way out, still in agreement with Popper, is that theories which are neither demonstrable nor refutable may be respectable nevertheless if they are rationaly arguable: they try to solve certain problems, and it can be rationally discussed whether a certain solution is fruitful, simple, etc. in relation to the problem-situation in which it was devised. Demanding that linguistics should be empirical would mean, according to Lass (219ff), a shift of basic emphasis away from 'insight' in the normal linguistic sense, and restricting the field to those aspects which are capable (e.g. by means of 'rigorous experimentalism') of having empirical claims made about them.

It is the opinion of the present writer that Lass here goes too far in renouncing falsifiability (in favour of rational arguability) for most linguistic claims. The heart of the matter is, I think (cf. Spang-Hanssen 1959) that a scientific description should be prognostic, i.e. it should make predictions (which in principle could be refuted) about something outside the material on the basis of which it was constructed in the first place (this presupposes that the material is - in principle at least - considered open). This notion of prognosticity applies both to intra-, para- and extra-linguistic data. If this point of view is accepted then most linguistic statements, I think, are in principle refutable when new sets of data are considered (presupposed, of course, that the theoretical terms can be operationally defined). If the linguist is satisfied with rational argumentation and renounces refutation, he may be almost back in the sometimes futile discussions on 'simplicity', 'elegance', and so on, of several structuralist traditions. Although we must sometimes, e.g. in meta-

theoretical considerations, content ourselves with rational argumentation, a major goal of our discipline should - in my opinion - be to try to open as many areas of linguistics as possible to empirical investigation (i.e., to speak short-handedly, to potential refutation).

The previous status report (Fischer-Jørgensen 1975b) contained an evaluation of different types of external evidence and a rather detailed discussion of the notion 'psychological reality' in phonology. The program of research sketched there is tremendous, and clear results in these areas have, predictably, not been obtained in the meantime, so I shall limit myself to a reference to her report in this context.

Skousen (1975) investigates in detail a number of cases of "Substantive evidence in phonology". In contradistinction to Skousen, however, Dressler (1977b) has had divergent and incoherent results when using different modalities of external evidence, but this "is, hopefully, only true if one uses external evidence in a somewhat superficial way [...] Today higher standards must be set: first it must be argued why, in the first place, a particular modality of external evidence should be relevant for the specific problem in question, and what factors, warrants, and marginal conditions must be considered in order to ensure that the particular evidence really confirms what it should confirm, or can be explained in the same way as data from another modality. Here theory of science must come in ..." (Dressler 1977b, 224). Notice that these warnings by no means suggest that the linguist should limit himself to rational argumentation.

To close the section, a few words might be said about sound change. A number of recent investigations of chronological (and other) variation of language have increased our knowledge of the invariant aspects of human language as well. Examples of such studies are Chen and Wang 1975, Brink and Lund 1975, Lass 1976, and Bailey 1977a. A basic insight e.g. of the latter work is that natural processes should be kept strictly apart from non-natural (e.g. morphologized) rules which are spread by Creolization (the importance of sound change for the study of marking will be mentioned in section 3.3). A very promising comprehensive socio-linguistic investigation, viz. the Tyneside project (see Pellowe 1976) should also be mentioned.

3. Segments, features and marking

3.1 The output of phonology: aspects of phonetic structuring

The question of the relation between phonetics and phonology is, of course, a vexed one (partly of a terminological nature, and both a normative and a descriptive one), and the most different opinions on this issue have had supporters in the past or the present, be it that they are identical, overlapping, properly included one in the other, non-overlapping, or in a relation of abstraction. A further possibility, in a "concrete" and "substance based" vein, is to use convention as the distinctive criterion such that 'phonology' should cover the language specific (conventional) and 'phonetics' the universal (biologically conditioned) aspects of sound structure. For the sake of clarity, we can put the question in the following form: Is phonology (in the broad sense used in this report) dependent on modern phonetic results, i.e. from physiological, acoustic or perceptual instrumental investigations? E.g., can it be the case that phonological theory has to be modified, or even radically changed, as the result of certain important new insights within phonetics? The question thus amounts to more than just asking whether phonology presupposes a certain basic phonetic knowledge (which probably no one would deny), and the answer depends on the phonologist who replies. What is interesting, however, is the fact that several new versions of phonology which build heavily on phonetic results have been propagated in print since the last congress. And I think it is fair to say that the understanding of the importance and even indispensability of phonetics in phonology is growing among phonologists. This evolution, which I for one appreciate, has been furthered by the work of phoneticians like Lindblom, Ladefoged, Fromkin, Lehiste and Ohala. As an example of this tendency a careful study on prenasalized consonants (with the revealing title "Phonetic analysis in phonological description") may be mentioned (Herbert 1977), in parallel to works on nasalization and palatalization by Chen and Mayerthaler, respectively. In the following, two more radical revisions of current phonological theory, viz. the auto-segmental and the non-segmental approach, will be considered in turn.

"Autosegmental phonology is", according to Goldsmith (1976, 23), "an attempt to provide a more adequate understanding of the phonetic side of the linguistic representation [...]; it suggests

that the phonetic representation is composed of a set of several simultaneous sequences of [segments, and, more concretely, it] is a theory of how the various components of the articulatory apparatus, i.e. the tongue, the lips, the larynx, the velum, are coordinated." It departs from the trivial but important phonetic observation that the speech chain cannot, phonetically, be sliced into a number of consecutive non-overlapping segments. Goldsmith proposes that certain features, mainly pitch but in some cases also nasality, should be treated on a level of their own (cf. the name 'auto-segmental'), and he examines the formal nature of the theory as well as a number of concrete cases (involving contour tones, tone stability, melody levels, floating tones, and automatic spreading of nasality) in support of the autosegmental view. His conclusion appears so sound to the present writer that it deserves to be quoted in part: "advances in phonological theory may start from an interest in low-level articulatory facts; [and] we do not begin our research with an understanding of the most elementary linguistic observables [...]. We should not restrict our attention to rules [...] at the risk of missing the very nature of the items involved." (1976, 67). As is immediately obvious even from the short summary above, the autosegmental approach shares a number of fundamental conceptions with the Firth school (or 'prosodic school'), although this historical aspect is not emphasized in Goldsmith 1976 (I think it would be a gain for our discipline if the work of our predecessors were taken into account more often than is the case today, cf. Fischer-Jørgensen 1975a). It should be added that Leben 1976 and Clements 1977 are interesting applications of the autosegmental approach to English intonation¹⁶ and to vowel harmony, respectively.

An interesting and promising contribution to the theory of phonology since the last congress is T.D. Griffen's 'Non-segmental

16) Although the dividing line between phonetic and phonological models of intonation is by no means clear, a few important studies of English intonation with general linguistic implications might be mentioned in this report: Liberman 1975, Bailey 1977b, and Pellowe and Jones 1978.

phonology"¹⁷ (see Griffen 1976 and 1977). Built upon recent advances in physiological phonetics (in particular the dynamic phonetic model of Mermelstein 1973), Griffen 1976 advances a phonological model in which the problems of segmentation in classical phonological theory, both structuralist and generative, are claimed to be overcome. He states - in agreement with e.g. Twaddell - that whereas the distinctive oppositions have observable correlates in phonetics, the segmental speech sound is nothing but a convenient fiction (partly due to the historical coincidence that writing when invented in the old world was alphabetical). Griffen "maintains a syllable in which the vowel is considered to be the articulatory base and consonants are constraints carried out on the vowel and concurrently with it" (1977, 375). This hierarchical notion of phonology which, as a matter of fact, reactualizes structuralist notions of hierarchy and dependency (cf. Rischel 1964 and Anderson and Jones 1974), is then applied to aspects of Modern Welsh. The new model has also been applied to a classical problem in phonology, viz. the relation between German [x] and [ç] (1977). It "eliminates the need for such allophones by attributing vowel characteristics to vowels and consonant characteristics to consonants" (ib.). Although this proposed explanation recalls prosodic analyses as well as Hockett (1955, 155-157), Griffen's proposal is interesting in itself because it follows from the so-called dynamic phonetic model. It is not improbable, however, that the conventional aspects of the distribution of German "ich" and "ach" are understated in Griffen's analysis. He claims that his model can describe the entire phonology by a simple hierarchical structure. To the present author, his analyses taken together seem rather convincing, but I find it a challenge for researchers with a major competence in modern phonetics to critically examine Griffen's model of non-segmental hierarchical phonology, and an important task for Griffen and others to develop and investigate this model

17) It should be noted that this use of the term "non-segmental" is not in agreement with that suggested by Chomsky and Halle where "non-segments" would mean "boundaries" (which, according to SPE 371, are units in the string with the feature [-segment]). This is, of course, no criticism of Griffen's use of the term, which is entirely reasonable and more immediately understandable than SPE's (whose conceptions of units and segments are, naturally, incompatible with Griffen's).

further. The main challenge to Griffen's theory is, as I see it, how it can be extended to deal adequately with a much wider range of phonological problems than have been covered within non-segmental hierarchical phonology until now.

3.2 The inventory and organization of features

It is probably an uncontroversial statement that some sort of distinctive features must have their place in a theory of phonology. A number of questions concerning such features which are anything but uncontroversial, however, will be briefly considered in turn (on marking, see section 3.3). I shall mainly build upon the work done in prolongation of Ladefoged 1971, which seems to me a more fruitful starting point for research in this area than e.g. SPE.

First of all, how should features be defined: articulatorily (cf. SPE), acoustically, perceptually, or in a combination (cf. Jakobson et al. 1952). The hybrid solution of Ladefoged (1971, 1975), Lindau (1975) and Williamson (1977) seems reasonable enough: they argue that the correlates of certain features are acoustically simple and articulatorily complex (e.g. "grave" - a feature which has also been argued for within an SPE-framework - and the basic features for vowel space according to Lindau 1975), and they should accordingly be defined acoustically. Other features should for a similar reason be defined articulatorily (e.g. "labial" - which has also been argued for within an SPE-framework - and "nasal"). This pragmatic view seems to the present writer to be reconcilable with the original Jakobsonian position, reemphasized by Henning Andersen, that the features are above all perceptual (although they will, in the present state of our knowledge, in general be better defined within other aspects of communication by sound-waves due to our lack of criteria for operational definitions within the realm of perception).

Another debated point is the question whether all features are binary. The strong binary position has never been convincingly argued for, in the opinion of the reporter. If the question of binarism is conceived of as an empirical one, the available evidence seems to suggest that some features are binary on the phonological level, e.g. nasality, and others multi-valued (with a small number of linearly ordered values), e.g. vowel height. The exact number of values of a feature is language specific within certain (biologically determined) limits. The preceding remarks apply to

a conception of phonology where the notion of surface contrast is in focus, but it still remains to be shown whether the question of binarism can be given any empirical content in much more abstract conceptions of phonology.

Concerning major class features, it is well known that SPE inherited the strange 'natural class' [h ? j w], defined as non-vocalic and non-consonantal sounds, from Jakobson et al. The drawbacks of this proposal have recently been discussed again (Lass 1976, 148-167). It is today generally accepted, I think, that the feature "vocalic" should be given up and the feature "syllabic" introduced instead (but cf. Andersen forthcoming). Problems arise, however, if "syllabic" is taken as a feature to be defined in a way which is parallel with other feature-definitions (cf. Ladefoged 1971, 94: "syllabic (correlates undefined)"). A better solution seems rather to be that 'syllabicity' should be taken as something separate, defined in terms of 'syllable structure', i.e. in a way prosodically, cf. Williamson 1977. The other useful major class features seem to me still to be "sonorant" and "consonantal". On this point I am unable to follow Williamson, who renounces both of these (1977, 870f), my counterarguments being both that approximants may be voiceless and thus non-sonorants, and that "consonantal" does not concern syllabic function - since e.g. glides are non-consonantal - and should therefore not be integrated into the description of syllable structure.

Lindau 1975 suggests that the frequency of F1 and of F2 - F1 should be used as the features replacing "vowel height" and "backness", respectively. Williamson 1977 argues that "stricture" should distinguish five sound classes: stop, fricative, approximant, high vowel and low vowel, and that sequential articulation should be allowed in the description of e.g. affrication and pre- and post-nasalization (cf. Anderson 1976).

"Consonantal" may be defined as a cover feature (Ladefoged 1971), so that consonantal segments are defined as the complementary class of the intersection of the classes of sonorant, continuant and non-lateral sounds (i.e. [-cons] is equivalent to [+son, +cont, -lat]), cf. Basbøll 1977. Such cover features are used more extensively by Lass (1976) under the name of 'secondary features' which are language specific (whereas the primary features are supposed to be universal). The purpose of these secondary features

is to define 'natural classes' which are useful in the description of a good many phonological processes in one or in several related languages.

Whereas cover features may be seen as abbreviations for sets of features (also cf. Anderson 1974 on glottal features), a possible ordering of the set of features has been discussed too, mainly in terms of hierarchies of strength (recently, e.g., by Hooper 1976 and Foley 1977). One main motivation for proposing these strength hierarchies, which are rooted in the sonority structure of the syllable (ultimately in degrees of physiological opening), is to account for phonotactics (cf. Basbøll 1977), but a lot of evidence from different modalities has been brought into the discussion (for a good critical overview, see Drachman 1977). Broecke has treated hierarchies and rank orders in distinctive features in a monograph (1976).

In addition to the simultaneous (or even paradigmatic) organization of features just mentioned, there exists of course the important temporal organization usually referred to as the syllable. Problems of the phonological syllable have been alluded to above (e.g. in the present and the preceding section), but a few articles on this topic could be mentioned here: The papers e.g. by Bell, Hooper and Vennemann presented at the symposium on the syllable in Boulder, Colorado, in October 1976 (not yet published, as far as I know), the work of Perry and of Kahn, and the discussion of syllabification in French as presented e.g. in Rudes 1976, Selkirk 1978 (who builds upon Liberman and Prince 1977, cf. note 15), Cornulier 1978, and Basbøll forthcoming.¹⁸

3.3 Marking

Although the Prague school notion of markedness in phonology has not been within the central field of investigation since the last congress, neither within the generative school (cf. the revival of the concept by Postal 1968 and SPE), nor outside, it has nevertheless been discussed and used in an interesting way by a number of scholars.

18) Since there is still a persistent and widespread misuse of syllable boundaries in the literature, even by otherwise careful authors, I should like to emphasize that e.g. rules which nasalize a vowel before a tautosyllabic nasal should be stated with the syllable as their domain, and not with a syllable boundary as their utmost limit to the right, since the latter formulation makes the wrong prediction that a consonant occurring between the nasal and the syllable boundary would block the rule.

An excellent account of the notion is found in Hyman's commendable textbook (1975), and a discussion of the markedness model of standard generative phonology is given by Eliasson (1977), who emphasizes the distinction between the formal approach to markedness used in SPE, and an external (or "substance based", cf. section 1.2) approach.

To Bailey (e.g. 1977a), markedness is a crucial concept. He discusses the two 'Greenbergian' (and 'Jakobsonian', one could add) principles: '(i) what is more marked changes to what is less marked', (ii) 'what is less marked is implied by (the presence of) what is more marked' in connection with a lot of data from speech variation (in the broad sense), including both "natural" changes and "unnatural" ones (which are very frequent, e.g. due to borrowing). In his account he makes use of the notion of 'feature weighting', i.e. the features do not form an unordered set, but may be weighted in different ways for different groups of languages (in different periods), e.g. "continuant" is a "heavier" feature with respect to "voice" in Romance (p > b > v) than in Germanic (p > f > v). On phonological "chains" and their relation to markedness, also cf. Fox 1976.

The notion of feature weighting (except for the terminology) has also been used by Henning Andersen (whose work belongs equally to the preceding and the present section) in connection with markedness in vowel systems (1975), and for another typological purpose in (forthcoming), viz. to distinguish between "vocalic" and "consonantal" languages (with different weighting of these features) while exploring a number of consequences (from sound change, etc.) of this typological distinction.

The concepts of markedness, neutralization and archiphonemes are, historically at least, very much connected, cf. the next section.

3.4 Archisegments

In the natural generative phonology of e.g. Hooper (1975, 1976) and Rudes (1976), the lexical entries¹⁹ consist of incompletely specified segments ("archisegments") such that all redundant features, both those that represent neutralized contrasts and those that are never contrastive in segments of a given type, are left blank in the lexical representations.

19) The lexical entries consist of whole words according to Rudes, whereas Hooper takes productive suffixes to be separate entries.

The term "archisegment" is formed on analogy with the Praguean "archiphoneme", and it is not surprising that the discussion of the notions of archiphoneme, neutralization and defective distribution has been most lively in a Prague-like functional tradition. Vion 1974 distinguishes between different degrees of relevance for a neutralizable opposition, and Akumatsu 1975 rejects Trubetzkoy's rather abstract notion of a "representative" of an archiphoneme.

Davidson-Nielsen in his monograph (1978), basing his claims upon e.g. speech error evidence and orthographic evidence, defines neutralization as contextually determined (in a purely phonetic/phonological sense) loss of one distinctive dimension (with some further qualifications). By an archiphoneme he understands a contrastive segment in weak position whose distinctive features correspond to the intersection of two contrastive segments in strong position which differ in terms of one feature only.

4. Concluding remarks

As mentioned at the beginning of this report, I am fully aware of the subjectivity of what I have written, both as regards selection and evaluation.²⁰ Many works of a general nature which are also relevant for phonology have been ignored (but cf. Tench 1976 for an interesting tagmemic account), and many problems and trends have not been considered.²¹ Although I have in many places expressed my scepticism about overly abstract approaches to phonology, I should like to state that linguistic generalizations presuppose abstractions, and that extremely concrete phonetic experiments alone do not lead to an adequate understanding of phonological issues. The field of theoretical phonology has not been reduced to any type of orthodoxy. It is still very much alive.

20) It is evident that the task is an infinite one, but I should nevertheless like to emphasize that I know many of the references only superficially.

21) An important problem which has not been discussed is how to settle an underlying form within generative phonology, cf. Zwicky 1975. An example of a trend which has not been covered here is the "atomic phonology", see e.g. Dinnsen and Eckman 1978. A combined example is Hervey 1978 on accidental vs. structural gaps within a functionalist framework.

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