The kind of understanding we can hope to achieve from phonological research, then, is arguably possible only if we abandon the ultimately unreachable goal of complete predictability. Attempts to achieve predictability by imposing arbitrary limits on the form of phonological descriptions, such as by the decision a priori that 'extrinsic' or language particular orderings are one kind of complexity that languages absolutely cannot tolerate, seem unmotivated and misguided. In the absence of an understanding of general cognitive processes underlying language that could explain them, such 'constraints' cannot be taken seriously as the motivation for particular decisions about the appropriateness of descriptions. As Basboll notes, such extralinguistic explanation seldom plays a real (rather than rhetorical) role in phonological theorizing.

To me, however, this suggests that much of the actual research Basboll characterizes as 'substance based' is ultimately unproductive, since it is based on the arbitrary imposition of restrictive principles which rule out otherwise well-motivated descriptions. We have no way of knowing a priori what sorts of complexity, abstractness, etc. are tolerated by natural languages, and the only way of discovering this is through the unbiased examination of the facts they present. This is not to deny that such programs can lead to significant insights, as in the case of their emphasis on a distinction between morphological and purely phonological rules, which has evidently led to major improvements in our understanding of sound systems. Nonetheless, far from suggesting that the study of formal problems in phonology, of the sort arising in the framework of SPE, should be abandoned, the lesson of this research seems to be that it is only by taking these formal matters seriously that their ultimate role in a comprehensive view of sound structure can be appreciated.

References

FORMAL AND SUBSTANTIVE APPROACHES TO PHONOLOGY
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The main report on phonological theory by Hans Basboll gives a rather thorough treatment of current phonological research. This response will mention a few additional works and issues, but it is intended primarily to supplement Basboll's report by presenting in somewhat greater depth an examination of the theoretical diversity underlying current phonological research. Our point of departure is the distinction Basboll discusses between a "substance based" versus a "formal" approach to phonological research. This distinction characterizes quite broadly two major research trends in generative phonology, but leaves out some important differences. In order to highlight several theoretical positions, the "substance" versus "formal" distinction will be divided into two separate distinctions which cross-classify fully. This brief report will discuss the resulting categories and the type of research emanating from each of them. Basboll noted that his classification of two types of phonology was only rough and ignored some individual differences. Similarly, the distinctions I will make are also rough, and are meant only as a useful organization of a diversity of research perspectives.

1. Two major issues
1.1. The most direct interpretation of the substance-formal distinction divides phonological research into that which investigates formal or structural properties of grammar and that which investigates substantive properties. The former research is concerned with levels of representation, and how they relate to one another, and with the formal properties of rules, and the formal relations among them. Substantive properties can be thought of roughly as content properties -- phonological features are the content of representations, and changes in phonological features in the presence of other related phonological features are the content of rules. For most investigators, the substance of phonology is phonetic (but see section 3.2).

This aspect of the substance/formal distinction is not so much a theoretical issue as a distinction between two types of interests, which are not mutually exclusive. Most researchers would agree that phonology has both a formal and substantive side, and that the two need to be studied together at least to some extent.
1.2. The second distinction that divides current work in phonology is a distinction in terms of theory, and thus has more serious consequences. Following common practice, this can be labelled the concreteness versus abstractness issue, although it is not abstractness per se that I will focus on here. There are many different degrees of abstractness. We can find a discrete division on this scale, however, if we consider one issue -- the use of data in analysis, in particular the importance of surface facts. In the transformational tradition, one working hypothesis seems to be that if x and y share some characteristics, then they must have the same underlying form. This produces an emphasis on the similarities between elements, and leads to a dismissal of their surface differences. Similarly, the goal of uncovering all the "linguistically significant generalizations" the data can yield makes it desirable to ignore counter-indications on the surface. The contrary position is that the rules of the grammar must be fully compatible with the surface data, and exceptions must be taken as giving evidence of rule productivity or the lack of it. Either of these approaches to the evidence can be combined with an interest in the formal or the substantive aspects of phonology.

2. The "abstract" positions

2.1. The tradition of the Sound Pattern of English (Chomsky and Halle 1968) combines the abstract approach to data with largely formal interests. Some new issues have arisen in this framework, such as recoverability, a relative of opacity (Leben 1977, Kaye 1978, as well as the references Bassbøll cites), and some of the older issues, such as extrinsic rule order and rule types continue to be discussed (see Bassbøll's report). It seems unlikely that these issues will ever be resolved, because of the approach to data customary in this framework. Since there is no requirement that a rule correspond in any predetermined way to the surface data, it is impossible to tell if the rules whose relationships are being studied are indeed rules of the grammar. It must be emphasized that the lack of importance of surface data is not an oversight, but rather is a deliberate component of this point of view, as is clear from the following statement by Keyser 1975. He has just argued for internal structural reasons that there is a rule of metathesis in Old English. He then says: "It is a rule whose output never appears unmodified on the surface. This fact may lead one to suppose that the rule is, therefore, not a possible rule of phonology. However, such a supposition seems to be based upon an excessive reliance on surface data" (pp. 410-411, emphasis mine, JBN).

Of course, Keyser's position is an extreme one. There are many more concrete works in the same general framework (e.g. Kiparsky and O'Neill's 1976 response to Keyser), and many explicit attempts especially by Kiparsky to make the theory more concrete. Despite Kiparsky's various conditions on grammars (e.g. Kiparsky 1976), his work remains in the same framework because he conceives of the grammar as something only indirectly related to surface data. This is evident in Kiparsky 1974 where apparent surface simplifications that must be represented as grammatical complications are lamented, and where the disparity between surface notions of opacity and paradigm uniformity and the formal notion of simplicity are discussed.

There are some works which explicitly disown the SPE model, while maintaining a similar view of surface data, and an interest in the formal aspects of phonology. One example is Leben and Robinson's "Upside-down phonology" which incorporates a very concrete level of lexical representation, while still allowing the formulation of abstract rules, such as the English vowel-shift rules, which are not disconfirmable by surface facts. It is claimed that this framework eliminates ad hoc exception features, but this strikes me as being of dubious value, since this is accomplished by saying nothing about exceptions at all.

2.2. The abstract approach to data can also be combined with an interest in substance, as illustrated in Chapter 9 of SPE. This particular proposal is probably the least satisfying of substantive proposals, because it was appended to a pre-existing formal machinery, and assumes the correctness of certain features and rules. Further, because of the view of surface data mentioned above, the theory does not generate testable hypotheses. Foley's 1977 approach seems closest to the SPE approach in its abstractness, but his proposals are more sophisticated because of a wider database, a unified theory and the ability to incorporate more than two values for a given feature.

Neither natural phonology (Stampe 1973, Donegan and Stampe 1977) nor polylectal analysis (Bailey 1973 and 1978, and other
articles, too numerous to cite) imposes strict empirical criteria on what may be a rule. Exceptions to rules can arise through extrinsic ordering without affecting the validity of the rule. In natural phonology the reason is that all processes are universal, and occur in all languages unless they are explicitly suppressed. Thus a language does not have to directly evidence a process in order to have it. A polylectic grammar must be quite abstract if a large number of surface variants are to derive from a common underlying form. Both theories claim a close relation between phonetics and phonology, which gives their proposals an empirical aspect, since hypotheses about phonetic motivation can often be tested. (This is in contrast to Foley's theory, in which it is quite explicit that phonology has nothing to do with phonetics.) Moreover, natural phonology makes the important distinction between natural processes and acquired rules, which delimits the input to a theory of rule naturalness. Bailey's work (e.g. Bailey 1978) deals primarily with the very concrete details of phonetic realizations. Both of these approaches differ from the more formal abstract approaches by recognizing variation, and considering many types of independent evidence.

Donegan and Stampe 1977 have entered the race to invent universal principles of extrinsic rule order, with an interesting twist -- a substantive determination of ordering, by which fortition processes apply before lenition processes. The difficulty here is in dividing all processes up into the two types without seriously distorting some of them.

3. The "concrete" positions

The theories treated here as concrete have in common the requirement, either implicit or explicit, that the rules of the grammar represent true generalizations about the surface data. The rules are therefore disconfirmable and serve as solid input to theory development in both formal and substantive concerns.

The formal issues do not include rule order, but do include rule type. The distinction between genetically-conditioned and morphologically-conditioned rules seems firmly established (Andersen 1969, Vennemann 1971 and Hooper 1976). Klausenberger 1978 compares this distinction to Kruskewski's categories of sound alternations, arguing for a third type of rule corresponding to Kruskewski's third category, which are rules with a general morphological function. There is some debate concerning Vennemann's 1972 via-rules, with Tierema 1978 arguing for bidirectional rules, and Leben 1977 arguing for a parsing model.

The issues concerning underlying representations involve the unit of representation, morpheme or word, and the presence or absence of redundant feature specifications. These questions are often argued on purely formal grounds, since substantive evidence about underlying forms is difficult to obtain. However, substantive evidence is presented by Vennemann 1978, who argues from historical data that full paradigms must be listed lexically, and Vincent 1970, who finds historical evidence that at least some paradigms must be listed. With regard to redundant feature specifications only Davidson-Nielsen 1977 has been able to present firm substantive evidence on this issue, and his evidence argues for archi-phonemic representations. Evidence about rules does not bear directly on underlying forms (as Basbøll implies, footnote 12), but rather these issues must be explored separately (see section 3.2).

Thus, for the moment, we must be content with formal arguments concerning underlying representation, such as those found in Hudson 1973, who argues that rules governing automatic alternations only add feature values, never change them, and Skousen 1977, who argues that constraints on underlying forms must be approached from the point of view of language acquisition.

3.2. There are quite a variety of approaches to substance from a concrete perspective, which I take to be a good sign, since this seems to be one of the most fruitful research perspectives. I begin with three proposals presented at the Bloomington Conference on the Differentiation of Phonological Theories.

Dinnsen 1977, Houlihan and Iverson 1977, and Sanders 1977 all propose theories that attempt to define "possible phonological rule". For the most part, they limit the input to their investigations to surface-true phonologically-conditioned rules, but none state that they would impose this limitation on individual grammars. Rather, it seems this limitation is imposed to make their hypotheses testable. Sanders' proposal concerning possible rules is embedded in the larger (formal) framework he calls Equational Grammar. His claim concerning phonological rules is that the directionality of rules is universal, so that if one language has a rule A→B, no language will contain the converse rule B→A in the
same environment. The directionality is functionally determined. It follows from his Simplex Feature notation that allophonic rules will only add features, producing more marked segments. Neutralization rules, on the other hand, produce phonetic structures that are relatively unmarked and communicatively more valuable (than the structures they apply to). Markedness is determined by universal distribution and communicative value "on the basis of physical, social and psychological efficiency" (p. 27). The specific claims are, e.g. that if one language has prothesis (as for example Spanish does), then no language has apheresis. The case is not convincing in view of the large number of counter processes discussed in Andersen 1972, and the small number of examples given by Sanders. 

Koulihan and Iverson 1977 make a similar claim; however, their definition of neutralization contains a built-in "blocking" device. They adopt Kiparsky's 1976 definition which says that a rule is neutralizing only if it produces strings or segments that are identical to some strings or segments that are input to the rule. Since the level of input to the rule is an abstract level of the linguist's own devising, potential counter-examples are easily dismissed. Thus it is claimed that English vowel reduction, which produces schwa in unstressed syllables is not a neutralization, since one cannot analyze English as having no underlying schwa.

These proposals refer to the structure of contrasts in the system to determine what is a possible rule for the language. This is a formal criterion. The substance involved is markedness. The "naturalist" point of view would oppose this "structuralist" point of view and claim that the processes have their own phonetic teleology, and care little about whether they are neutralizing contrasts in a language or not. It should be further noted that these proposals refer only to the structural change of the rule and say nothing of the environment. It seems to me that the environments are just as important and should be subject to cross-linguistic comparison as in Ferguson 1978, and other articles in Greenberg et al. 1976.

Atomic phonology (Dinnsen and Eckman 1977, Dinnsen 1977) incorporates certain testable claims such as, if fricatives devoice word-finally, stops will also devoice word-finally (the latter is the independent or atomic rule, the former its complement). In Dinnsen 1978 it is argued that these atomic rules are linguistic primes which are not further analyzable or explicable. Dinnsen argues explicitly against the position that phonological rules are "phonetically explainable" (as claimed in Hooper 1976). His argument is that different languages have different ways of resolving phonological problems. When tautosyllabic consonants differing in voicing arise by morpheme combination in English, they are subject to a progressive devoicing, while in Catalan, they are subject to regressive voicing.

Of course it is true that it is not possible at present to predict which language will have a certain process, especially on the basis of the kinds of information phonological grammars traditionally include. But it is certainly possible that the processes of a language are dependent upon one another, or on typological properties of the language. It is probably no accident that English and Catalan have different processes, since they also have different syllable structure, different stress and different rhythm. What is called for now are typological studies such as Andersen 1978, and studies that combine typological and phonetic substance. Alan Bell and I had this in mind when we organized the symposium whose proceedings are contained in Bell and Hooper 1978. The emphasis here is on phonetic, psychological and typological facts that may help us understand the diversity of phenomena associated with the combination of segments into larger units.

Finally, a very exciting new perspective is opening up. This is the possibility of approaching traditionally formal or structural problems from a substantive point of view. Hyman 1977 and Hooper 1977 quite independently come to the conclusion that formal distributional criteria cannot always determine the underlying representations of a language. Hyman gives the historical argument that if what is predictable gradually becomes contrastive, there must be a stage in which a feature is both represented lexically, and predicted by rule. Hooper 1977 presents language acquisition data that shows children treating a "predictable" feature (vowel nasality in English) as contrastive. Implicit in these studies is the notion that there may be some concept of "phonetic distance" that partially or fully determines the speaker's analysis into elements represented lexically and elements predictable by rule (cf. Stampe's notion of "minimal structural change"). This opens
up the possibility of substantive phonetic criteria for phonemic analysis. Along similar lines, Comrie 1976 points out that in several cases the development of exceptions to subparts of rules can be correlated with a greater phonetic change produced by that subpart of the rule. If it is possible that even exceptions are not totally arbitrary, it is all the more important to pay attention to them, and to other surface facts of phonology.

References

