REPORT: PHONOLOGY
(see vol. I, p. 103-152)

Reporter: Hans Basbøll
Co-reporter: Stephen Anderson
Co-reporter: Joan Bybee (Hooper)

Chairpersons: William Haas and Kenneth L. Pike

REPORTERS' ADDITIONAL REMARKS

Hans Basbøll: On an abstract level of discussion, it is very hard to disagree with Anderson's claim that one should avoid a priori statements about psychological reality and other linguistic issues, as well as "the arbitrary imposition of restrictive principles which rule out otherwise well-motivated descriptions" (p. 142).[1] I also fully agree with the claim that formal questions just like other scientific questions should be taken seriously.

I am in agreement with the claim that the very fact that part of the traditional field of study cannot be dealt with adequately within a certain framework is not a decisive argument against the use of that framework in other parts of the field. Thus I would suggest that the SPE approach towards markedness, which is considered quite unsatisfactory by both of my fellow reporters, can in principle be used in a rather specific subpart of that subfield of the study of sound structure which it was devised to deal with: namely, to account formally for implicational universals à la Roman Jakobson between sound types. What is outside the scope of the SPE approach towards markedness and similar approaches are other aspects of natural systems and natural segments (like prohibited segments and contrasts, or internal economy) as well as explanation, in any interesting sense, of the relation between phonology and phonetic substance. Such an explanation remains an important task of our discipline, of course.

While I also partly agree that certain efforts of Natural Generative Phonology might be termed reductionist, namely the axiomatization of strong constraints on the form of grammars, I would, on the other hand, suggest that a considerable part of the

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1) Pages refer to volume I.

efforts of SPE-phonologies is reductionist in the sense that large amounts of evidence, and thus potential counter evidence, is not taken systematically into consideration. The data considered as evidence is too often limited to a static set of occurring forms as against all the facts which the languages present (cf. p. 142), including those that may be revealed in psycholinguistic experiments, and in studies of language acquisition, language loss, and so on.

What is really at issue are two related fundamental problems: first, the question of predictability and second, the relation between model and reality - in particular: What is the model a model of? and how can it be tested?

I would like to emphasize that in my report I have not stated nor implied nor suggested that the goal of phonology is complete predictability (compare also Labov's variable rules which are probabilistic rather than deterministic). I have said, however, and that evidently is not very new, - that a scientific description should be prognostic in the sense that "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" (p. 117). That phonology could or should in principle be deterministic is a claim which would hardly be defended by anyone to-day, with the possible exception of a few radical behaviorists. I also think that most linguists would accept the hermeneutic goal of "ex post facto understanding" (p. 140), at least faute de mieux. I certainly also agree that the identification of mutually inconsistent principles may advance our knowledge (for instance the "internal" vs. "external" economy of sound systems according to Martinet), but in such cases our efforts should be directed towards finding constraints on the principles in question to diminish (or better, remove) the field of conflict between them. That a phonological description or theory should be prognostic, on the other hand, is a necessary condition for its being even partly tested for falsifiability, that is for one type of decision on how it relates to "reality".

What the model or theory is a model or theory of is, of course, a vexed question which is closely related to the issue of the reality of phonological descriptions in general, either psychological or sociological. I shall not go into that matter here,

but only briefly remark first that the frequently used phrase 'linguistically significant generalization' may have very different meanings according to the type of reality - if any - ascribed to phonological or other linguistic descriptions; and second, the question of psychological reality is not of the yes-no-type, but there would be a whole scale of possible relations between some internal grammar and an observationally successful model of it (as far as its output is concerned), stretching from a "black box" to a point-to-point-correspondence.

The relation between model and "reality" is of a dialectic nature: The model specifies a number of theoretical constructs, like "natural class" in the "model-internal" sense, defined as a certain set of co-occurring distinctive features, to take just one example. At the same time, real languages present natural classes of segments in the "model-external" sense, that is sets of segments that function as a class in real processes in languages, be it acquisitional, synchronic, diachronic, or whatever. The testing and modification of this part of the model is then a series (generally an infinite one) of steps whereby the sets of segments specified by the "model-internal" and "model-external" natural classes should be brought to coincide, while still respecting all other conditions on the theoretical constructs, such as other types of criteria for the establishment of distinctive features. The model specifies which types of data we should look for, and also which aspects of the data should be considered pertinent and which aspects irrelevant; it must then be independently decided whether the data is in conflict with the model or not.

Now, the point is that this partial testing procedure presupposes that the parts of the model not under consideration for the given purpose must be treated as given for that purpose (as I have said in my report): you cannot test everything at the same time. This is all right if the scientific paradigm within which you work is accepted as basically correct in its main lines, and that is exactly where a clear and fatal division of attitude towards the state of the art occurs, in particular whether the "conceptual richness" of SPE in Anderson's words (p. 136) corresponds to anything outside the model itself. Some people, like my fellow reporter Stephen Anderson, think that SPE represents "monumental results" (p. 138) and that it is methodologically sound whereas others, including myself, consider SPE - despite its monumental efforts and certain merits - as misguided in quite fundamental respects. I should like to stress once more that both of these two attitudes towards a research paradigm may per se be scientific.

Stephen Anderson: I will focus my attention on the apparent conflict between rationalist and empiricist approaches to sound structure, this being a distinction that I think is at least operationally similar to that raised by Basbøll as the distinction between formal and substance based approaches. This distinction can usefully be approached in terms of the following question: After we have taken into account all those aspects of speech that are associated with more general problems, and which can be approached from outside the domain of language per se, how much is left? Substance based views have typically pursued the possibility that virtually all aspects of language are accessible from one or another more general point of view, and that they can be treated as special cases of the functioning of the articulatory apparatus, of generalized perceptual strategies, of general limitations on memory and processing, and the like. As a result, these researchers have put a great deal of faith and emphasis on the possibility of experimental verification of the details of linguistic structure, for example on the devising of psychological tests to determine on the basis of constructed tasks whether particular proposed phonological rules are psychologically real or not. The substance based linguist takes the absence of such external evidence as establishing a case ex silentio against the proposed analysis as a correct account of language.

The formal approach, on the other hand, has been motivated by the feeling that there are distinct aspects of language which are proper to itself, not studyable necessarily as special cases of other systems. Hence, for the formalists, the absence of direct external accounts for some area of language is not very surprising, or a cause for alarm. This is because this line of reasoning allows specifically for the possibility that among the interacting domains that contribute to the facts of speech, we may find a language faculty which is not indeed reducible to features of other kinds. If so, there is no reason, in principle, to expect that such a language faculty, if it exists, ought to be directly accessible to inspection in other terms, through constructed psychological experiments of a given kind, for example. The validation of claims of this sort then, would rest not on the establishment of direct links between them and external observables but rather on the inferences that can be drawn from the success, or lack of it, which they achieve in facilitating and revealing regular connections among phenomena, and in uncovering orderliness and coherence within the complexities of languages.

It is important to see that the primary issue between these two views, that of the existence of a specifically linguistic aspect of cognitive structure, not accessible in other terms, could probably never be settled conclusively. One might, of course, establish that a given aspect of linguistic structure is a special case within some more general demand. However, if we construe the proposal that there are aspects of language which are systematically not studyable in such terms, we construe that proposal as an empirical proposition about the nature of language. It is hard to see such a position as other than completely mystical in the extreme. This is, however, not really a matter of empirical fact, but rather a matter of choice of research strategies. Whether or not one ought to limit the terms of linguistic description to elements that can be given an external foundation. As a matter of choosing between research programmes, it seems to me that the claim that all aspects of linguistic structure ought to have some more general basis and ought to be accessible from some other realm, is at least equally mystical, at least in the absence of any such account from any area of linguistic phenomena. The best way to motivate the decision on this issue is to attempt to establish not the correctness but the plausibility of one or the other position. One does this, of course, by demonstrating the ability of this position to provide satisfying and detailed accounts of regularities among the facts of natural languages.

To my mind, the formalist, or as I would prefer to say, the rationalist approach has much the better track record in this regard, though I am sure there are many who will disagree with that. Nonetheless, I hope to have suggested that the choice is by no means an obvious one and in particular, that the formalist programme is in no way vitiated, as is sometimes suggested, by its indirect relation to surface facts; that is indeed its essence and its greatest interest.

Joan Bybee Hooper: In the transformational generative tradition a working hypothesis seems to be that if X and Y show some characteristics in common, then they must have the same underlying form, so this produces an emphasis on similarities among elements and has led to a dismissal, occasionally, of surface differences. The results are hypotheses that are untestable because it is always possible to invoke what Botha calls blocking devices, caveats that put hypotheses beyond the surface phonetic facts. This position is exemplified by SPE. The contrary position, which is the one that I accept, requires that linguistic hypotheses be testable (either by comparing them with the surface forms of language or by some kind of experimentation). This is not an a priori constraint on a theory of phonology, it is a different way of approaching facts. Nor is it an attempt to do phonology without an appeal to any abstract entities, because, in fact, all phonology is abstract.

Basbøll expresses the opinion that there is not a big division among these two approaches to phonology. He says in his written report that they share common bases of argumentation and understand each other reasonably well. It seems to me that this is not always the case. There is not a single set of shared assumptions and, in fact, some misunderstanding does ensue. In his paper, Stephen Anderson presents an example from Javanese, intended to falsify the claim that morpholexical rules should apply prior to purely phonological rules. But all we can conclude from the data is that the morphological rule must apply to basic adjectives with round vowels in final position. Only if we assume that lexical representations cannot contain any information that is the output of productive rules does it follow that the morphological rule must apply after the phonological rule. If we do not make such an assumption, the example shows that lexical representations, i.e. the phonological representations relevant for word formation, contain predictable phonetic detail, or to put it another way: the lexical representation has been restructured to contain the output of productive phonetically conditioned processes. The example shows an important difference between the

two approaches: in generative phonology it is assumed that underlying representations are negatively defined by the rules, but I believe that underlying forms and rules can and should be determined independently of one another by examining various types of linguistic evidence and independent or non-structural evidence.

In a paper by Donegan and Stampe in the volume edited by Dinnsen from the Bloomington phonology conference, they characterize a theory of natural phonology by saying: "This is a natural theory in the sense established by Plato in the Cratylos, in that it presents language as a natural reflection of the needs, capacities and world of its users, rather than a merely conventional institution. It is a natural theory also in the sense that it is intended to explain its subject matter, to show that it follows naturally from the nature of things. It is not a conventional theory in the sense of the positivist scientific philosophy which has dominated modern linguistics in that it is not intended to describe its subject matter exhaustively and exclusively, i.e. to generate the set of phonologically possible languages." This characterization has two parts: The first one deals with the difference between whether the explanation for linguistic structure will come from general properties of human users of language, or whether it is contained in something that is specifically linguistic and not accessible to verification (although it is not clear to me how this specifically and uniquely linguistic thing is immune to experimental investigation). Secondly, they say that the goal of a natural theory is not to produce exhaustive descriptions of its subject matter. It seems to me that trying to meet the goals of observational and descriptive adequacy has often forced us into making unwarranted theoretical decisions which we may at the time characterize as arbitrary, but in fact then we accept them and never go back to reexamine them; however, such assumptions should be reexamined in view of empirical evidence. Notation is the tool of a theorist and should not be mistaken for the theory itself.

DISCUSSION

Charles-James N. Bailey, Edmund Gussmann, and Henning Andersen opened the discussion.

Charles-James N. Bailey: Basbøll stresses the role of prediction and explanation. But he does not observe that development is what explains states and their structures; states cannot predict anything but what is in their own scope, and they can explain very little. For minilectal linguists - those who posit idiolects as the object of linguistic investigation and accordingly limit their models to static models - logic suggests that they should give up the goal of exact prediction.

Stephen Anderson's position is quite consistent with his synchronic orientation. He claims that markedness is getting vaguer; but developmental linguistics has been able to define naturalness and markedness quite exactly. Two kinds of dynamic data are relevant for defining the natural and for analysis and description: dynamic changes and comparative patterns (pattern is created by the dynamic principle). With the anticomparative models of minilectal linguistics - phonemes, idiolects, dialects, etc. - the theoretically interesting aspects of linguistics are virtually ruled out, for they demand comparative analysis: naturalness, child language, historical and dialectological linguistics, etc., which are all excluded on principle according to the definitions of phonemes, idiolects, etc. To study development with static tools would be worse than trying to drive a nail with a screwdriver. Since patterns of development are gradient, non-gradient tools are likewise fairly useless. One cannot even describe the morphology of German nasal-stem masculine nouns adequately, for example, with non-gradient models.

Aside from gradience, larger conceptual differences separate the underlying segments of three theories: (1) The classical (taxonomic) phoneme was neither internal-reconstructive nor comparative. (2) The generative phoneme is internal-reconstructive but not comparative. (3) The phoneteme is both internal-reconstructive and comparative, or polylectal. Only the latter is valid for development (comparative tasks, including child language acquisition), for theory, and for pedagogy. Development has two sides. One is the inner-linguistic side, where explanations

in phonetology (dynamic phonology) must be sought in phonetics and ultimately in anatomy and bioneurolinguistics. The other side is the social side: a development must not only come into existence among children, but must also be adopted by others if it is to survive. Developments due to social or extralinguistic causes may be natural-like, or they may be, and often are, unnatural as in the borrowing of older or of foreign forms, hypercorrect rule-inhibitions, etc. This side of language is only semi-theoretical since many of the relevant conditions are hardly predictable, though creolistics is getting better at predicting changes under different social conditions and with different types of linguistic mixtures. Since Stephen Anderson seems to have a rather negative view toward extralinguistic explanations as well as doubts about some of the explanatory achievements of phonetics, he seems to be skating awfully close to advocating an YROEHT instead of a THEORY; An YROEHT predicteth not; - neither can it explain.

Since it is clear that some linguistic developments are natural and that some are not, and since all languages are mixed and have both of these elements, the immediate goal of linguistics ought to focus on understanding only natural developments and leave the rest for the future.

The abstractness controversy is merely an off-shoot of the really fundamental issue, namely, what are the facts to be analyzed? Our differing views on what is really real affect our views on what data are really relevant to linguistics. If I say that languages have both natural and non-natural phenomena, and you disagree, how could we ever agree on what data are to be admitted or excluded from linguistic analysis?

Even in connection with derivative matters there are several issues of phonetological analysis which are more fundamental than abstractness: There are reasons for believing that instructions from the central nervous system to the articulators are bundled differently in syllable-timed languages and in stress-timed ones, viz. in syllable-sized units and in measures, respectively.
One of the deepest issues today is to specify the differences between phonomorphological and morphophonic (phonetological) rules. Another matter of interest is the fact that the segmental and suprasegmental uses of prosodic features are different: several

rules of English are respectively forwarded and hindered by these different functions of length.

Stephen Anderson takes the wrong view towards different historical developments and their use in the erection of a predictive theory. The difficulties exist only if one excludes the appropriate answer and mechanism: creolizing substrates and superstrates.

If you deal with idiolects, you can always say: "that is your idiolect, not mine", which effectively excludes both proof and replication - and theory. The best way to do linguistics is the way children and adults "do languages", viz. polylectally. Theory - if it means explanation and prediction - depends on development and change, on ascertaining how structures come into being, and on a dynamic comparison of the variation patterns resulting from change. We must admit that it is development that explains states, not vice versa, and then either give up all hope of synchronic explanatory theories, or become developmentalists. This is the paradigmatic difference among frameworks today.

Edmund Gussmann: The so-called substance based approach is in fact also a formal approach, but formal in a different sense. In natural generative phonology certain theoretical restrictions and conditions are established on the basis of some external evidence. But then these restrictions are generalized and applied to other data for which no external evidence is offered or simply where the evidence is not available. This is, of course, perfectly legitimate, but it shows that Basbøll is not right in what he says in footnote 8 of his report. In fact, substance based phonologists proceed in exactly the same way as abstract phonologists, though their restrictions are largely phonetic. But this phonetic nature is, in fact, often avoided without any real justification. For example, the "true generalization condition" is exempt from applying in the case of different styles and tempos.

When professor Hooper claims that phonological rules should correspond to phonetic data in a predetermined way, then there is little for descriptive or practising phonologists to do, since we have here really some sort of discovery procedure.

The standard generative approach to the question of how much structure should be assigned to individual lexical items was autonomous by being divorced from rules of word formation. A number of problems could have been avoided, if the direction of morphological processes had been taken into account. In some instances you can show that the rules of word formation have to take as their input the surface phonetic representation, in other cases the data argue just as strongly for abstract underlying representations as their input. There is a general non-existence of a theory of word formation. Here English seems to be a bad language to start with. In Slavic the very common expressive formations, such as augmentatives, diminutives, which are highly productive, are morphological processes which involve a number of phonological consequences. These should be studied in the first place, and rather than wondering whether 'serene' and 'serenity' are related. It is precisely in the interface of morphology, both inflectional and derivational, and phonology, that one should seek justification of phonological generalizations rather than in arbitrarily imposed restrictions of any sort.

Henning Andersen: Stephen Anderson's report seemed to me a very gracious concession of the total defeat of TG phonology. His remarks today seemed to contrive admission that it has not produced any results as a consequence of the monumental efforts made.

Basbøll's choice of leaving aside the vast amount of papers and monographs that contain important theoretical contributions under language-particular headings is regrettable. As to his limitation to descriptive linguistics, Bailey has taken care of that. But when Basbøll, in one of his footnotes, defines the substance based approaches as ones that go beyond the normal use of language, he must mean by that that they are interested in real data, meaning the use of phonology in speech, including speech errors, in verbal games, in poetics, by children, by aphasics, and so on.

In the same footnote, 'substance based' does not mean 'substance based' but rather 'speech based', - the traditional distinctions between language and speech, form and substance, etc. should be maintained also in discussions of these issues. I would like to ask Basbøll and Hooper to clarify what they mean by the distinction between formal and substantive, or if they understand them as being as vague as I do.

It is important to understand that language is something which is constantly changing, whose existence is in transmission from speaker to speaker, from generation to generation. Synchronic analysis is an artefact of the analyst. One must not identify synchrony with the static, nor dynamism with diachrony: there can be dynamism in synchrony, and in diachrony you can talk about static facts, viz. the correspondences between two stages of a language.

In the transmission of language there are two logically distinct processes at work: deduction and abduction. Speakers know the grammar of the language and can produce deductively utterances which are correct. If you know the grammar, you can predict what sorts of utterances are going to be produced by that grammar. The other phase is the abductive one, by which speakers (children or adults) infer the grammar of the language from the speech they hear from speakers of the same dialect or from other dialects or even a foreign language. Logically, this is a process of hypothesis-making, about the content of the speech or about the grammar behind the speech. In this phase we cannot predict, but we can somehow understand the grammar. You cannot predict a grammar from the data, but you can form hypotheses about it. When we have constructed a grammar and understand that as a hypothesis, we can predict what sorts of innovation will be acceptable to speakers of that language, what sorts of verbal games will have which results, what kind of specific data would arise in aphasia - and we can test these hypotheses. On the other hand, given the speech data that learners of a language face when they acquire the language, we cannot predict the shape of the grammar they will produce. But we may be able to approach something like prediction if we understand that what they have to do in the process of arriving at a grammar is to make decisions, to form hypotheses. And if we understand that the data is susceptible to diverse analyses, contains ambiguities, we can capture these difficulties of analysis by formulating alternative hypotheses, and these hypotheses can then be subjected to empirical tests.

A proper theory of the ontology of language, which will be a proper theory both of synchrony and of diachrony, will enable us to both predict and to understand, will enable us to explain in both the senses that Bailey used, and hopefully future contributions of this kind will take in a wider scope of the field and see to what extent these various issues are faced by people working not specifically on descriptive linguistics but also on historical and pathological aspects of language, as well as the contributions made by people working in language-particular fields.

<u>Joan Bybee Hooper</u>: Gussmann says that if rules correspond to the phonetic substance in a predetermined way, then there is nothing for phonological theory to do. I think that is wrong. The formal theory may tell me what a rule is, given the phonetic data, it does not tell me how to figure out why there are these rules in particular rather than the other logically possible rules.

A clarification of the notion of substance: As an example we could consider the kind of criteria used in phonemic description; there are distributional criteria and then there is the criterion of phonetic similarity. Phonetic similarity would be a substantive criterion, while distribution would be considered formal. Another example: morphophonemics based on the properties of a morphological system would be a substantive approach, while morphophonemics treated as phonological would be a more formal approach.

<u>Hans Basbøll</u>: Synchronic linguistics seen as something absolutely static is a conception which I would not share.

<u>Stephen Anderson</u>: My view of the state of the <u>SPE</u> programme is that it proposed a particularly ambitious goal for constructing a logistic system that would reconstruct all of the content of sound structure. Certain fundamental inadequacies were clearly revealed in the comprehensiveness of the goals of that programme, as phonetic substance came to be taken more seriously into account. It seems to me that reactions to the perception of these failures have tended to throw out the baby with the bathwater and abandon the entire programme of <u>SPE</u>, and in particular its underlying rationalist assumptions, in an attempt to provide a rather radical sort of therapy for these problems. It seems to me that that is an overreaction; that one does indeed want to recognize that there are inadequacies in the attempt to reconstruct in such a logistic system all the content of phonology, but, nonetheless, one wants to preserve for that sort of system a central role in the development of phonology much as the sort of system in the <u>Principia</u> serves as a fundamental object of study within metamathematics.

<u>Victoria Fromkin</u>: The question is not: is the theory formal or substantive? but rather: is it a true theory of human language? I think that what Stephen Anderson has been trying to say is not that questions of articulation, etc., are not necessary for understanding certain aspects of language use, but that it is not necessarily the case that all aspects of language can be accounted

for by reference to these other aspects of language production and perception, etc. These questions of the philosophy of science are important because they have led us to look at different aspects which, hopefully, will eventually lead us to understand the nature of human language.

John J. Ohala: The issue of the psychological reality of phonological constructs has been raised during the discussion of this report and, in my opinion, has been made unnecessarily complex. I would like to simplify it with the following analogy, which is designed to appeal to the many academics in the audience. The problem of assessing the psychological reality of phonological constructs is very much like the problem the teacher faces in trying to verify that a student has mastered or knows the subject matter he has been exposed to in classes. How can this be done? Let us imagine three approaches: the teacher that takes the 'formalist' approach will just speculate on what it is possible for a student to know and will assume that that is what all students know. The teacher who would have most in common with those phonologists who have here been characterized as accepting 'substantive' evidence, would rely on additional 'external' evidence of a student's knowledge, e.g., what books he had in his library, whether he nodded sagely during the teacher's lectures, laughed at his jokes, etc. The teacher who would take the experimental approach would demand of all students some behavioral evidence that they had mastered the subject matter, e.g., performance on a written or oral test, an original paper or thesis, etc. Naturally this performance should not be attributable to anything other than the student's full mastery of the subject, e.g., cheating or random selections of answers to 'true/false' questions. I leave it to all those academics in the audience to decide which approach they would use. I would hope that whatever decision they make, however, that this would influence their practice in phonology, too.

The point is that different types of evidence in phonology vary considerably in their ability to unambiguously tell us what is in the speaker's head. Most of the evidence characterized as 'substantive' in this discussion, e.g., speech errors, sound change, is quite ambiguous in this regard. Only evidence from tests (experiments) can be minimally ambiguous. This is not to say that there cannot be a bad test. But the proper response to a bad test - both in academia and in phonology - is an improved test. Teachers expend considerable time and imaginative effort refining the tests they use to assess the psychological reality of students' knowledge. Why shouldn't similar effort bear fruit in phonology?

Natalie Waterson: I should like to draw attention to another theoretical approach: to Prosodic Phonology initiated by J.R. Firth in England. Very briefly: most phonological theories have phonemic segments as the basic units of description, whether explicit or implicit, yet there is general recognition by those who study speech perception that the phoneme has yielded little in the way of insights to our understanding of how speech is perceived and interpreted, and it is becoming plain that it is not the right unit for such studies. In Prosodic Phonology the unit of description is the word, phrase, or sentence, and features which synthesize the word, etc., into a whole as well as those that divide it up are taken into account, i.e. syntagmatic and paradigmatic relations.

The phonological system of a language is thus described in terms of different word, etc., structures and not in terms of a system of phonemic segments. No exposition of the theory is available but there is plenty of illustrative material in theses and papers produced in the Dept. of Phonetics and Linguistics, at SOAS, University of London. Most of the material is about Oriental and African languages and the only English material are my papers on child phonology.

It is interesting to see the influence of Prosodic Phonology on developing theories, for instance on Joan Bybee Hooper's approach, and autosegmental phonology.

Richard Coates: The SPE type of phonology, represented here by professor Anderson, has tended to specify a kind of codified norm, whereas professor Hooper's system specifies the linguistic rules which would characterize usage as being the starting point of changes. I think that together they comprise the native speaker's system, both a kernel, or norm, available to him, and a system of partly specified potential directions of the changes. Thus, the output of morphology would not be absolutely rigidly defined, and we may imagine a speaker who makes very few morphological connections between surface forms not connected by phonological rules, on the one hand, and on the other a speaker who fluently manipulates a morphological and phonological system (à la James Foley's native speaker).

Wiktor Jassem: Fifteen years ago, or more, three points were made about generative phonology: observational adequacy, descriptive adequacy, and explanatory adequacy. Now, in the old days so little observation was done that it is difficult to say whether it was adequate or not; descriptive adequacy described rather what was going on in the minds of the theorists; explanatory adequacy, for which the criterion was simplicity, led to rules which in structural phonology could be expressed by three or four symbols but which in TG took complete pages so full of things that you could not see the wood for the trees. My point is: I suppose that revolution in phonology did not start twenty or seventeen years ago with Chomsky, - revolution in phonology, according to what I have heard today and read in the Proceedings, is starting now!

Royal Skousen: Each approach to phonology proposes a method of analysis. In some sense they are all formal in that they look at the data and attempt to derive a description from the data, but I would prefer to call that a method of induction or learning. I would like to suggest that, in addition to these formal considerations or these principles of learning, there is a need also for an empirical interpretation of the description: What does my description actually predict about language usage, about language intuition? - Furthermore, we need first to explicitly determine how we get our description from the data, and secondly, to answer the question of what would convince us that our description is right or wrong, because in the absence of such arguments we do not really have a theory at all.

William Haas: There is another kind of opposition that has to be reconciled, namely the opposition between empirical and speculative. More than twenty years ago, Martinet published his "Phonology as functional phonetics". And that was a kind of reconciliation: phonology was to present criteria for relevance, criteria of selection, to apply to the mass of unorganized phonetic data. Now we seem to have had some fifteen years of something different: phonology as speculative phonetics, and we

are now not so much imposing criteria of relevance on phonetic research as asking the phonetician to provide us with criteria to decide amongst different formal systems of phonology. Amongst these criteria will be the old functional phonology which is now, as it were, part of the surface data.

Kenneth L. Pike: It is not possible to separate phonology from grammar, from lexicon, from meaning. We must have a tri-hierarchical structure: phonology, grammar, and meaning. But in each of the hierarchies there are thresholds. - No mathematical system of any complexity can be treated as consistent by looking at the data inside itself. Something external must be used. That which I use from outside the formal system, to make it relevant, is meaning and behavioral impact.

Hans Basbøll: I want to stress once more that if my report is to be read as a status report on phonology, it should be read in connection with the contributions to the symposia.

Stephen Anderson: Perhaps we can all agree that the fundamental problem for phonologists is the exploration of what can constitute the sound pattern of a language. Ultimately we all have to make our own choice about what is the most productive way to go about this investigation, and I think it is unlikely that there are determinate answers to the sorts of opposition questions that have been posed.