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THE STRATEGY OF GENERATIVE PHONOLOGY*

1. Introduction. The purpose of this paper is to examine some principles of argumentation and verification used by generative phonologists, so as to show areas of agreement and to highlight areas of controversy.¹ By 'generative phonology' I refer not necessarily to the analyses in *The Sound Pattern of English*, but rather to a wider body of practice, of which the Chomsky-Halle approach represents one rather extreme position out of a large class of possible positions.

I begin by attempting to clarify the central notion of *methodological principle*, which is meant to be opposed to *theoretical*, or *substantive*, *principle*. Briefly, substantive principles are theoretical requirements, methodological principles are theoretical biases.² For example, the sequential application of processes within derivations is a theoretical principle of generative phonology; to question this principle is clearly to advocate a new, though related, theory of phonology, in the sense that the two systems allow distinct sorts of phenomena as potential human languages. On the other hand, the greater worth of phonological variants as opposed to distributional restrictions (Principle (E) below) is a principle of good practice. To question this principle is to question its value within a given theoretical structure—but reversing the bias would not create a new theory (since all languages consistent with one theory would be consistent with the other).

I do not wish to give the impression that there is a clear or a priori distinction between the two sorts of principles. Obviously there is not. The same assumption may figure now as a substantive principle, now as a methodological one; consider in this light the assumptions that two segments that distinguish morphemes are underlyingly distinct, *ceteris paribus*

* This version of the paper was prepared after the Phonologie-Tagung and shows my responses to some of the suggestions made to me at the conference. In particular, I have included a fair amount of bibliography. The references cited are by no means exhaustive, however, and exhibit my personal biases. In some cases I cite an item to illustrate a point even if I disagree with the approach or find the argumentation faulty; this paper is intended to be primarily descriptive rather than prescriptive.

1 My aim here is similar to that of Trubetzkoy's rules (1935), Bloch's postulates (1948), and the structuralist manuals of Pike (1947) and Nida (1949).

2 See the discussion in Zwicky (1971a). A wider discussion would include also those substantive and methodological principles that are embodied in experimental methods and design.

(Principle (L) below) and that two segments that never distinguish morphemes are not underlyingly distinct, *ceteris paribus* (Principle (M) below). In some classic treatments of phonemics, these are theoretical principles defining the phoneme, whereas in generative phonology they are background assumptions, utilized when there is no contradictory evidence. That is, a generative phonologist is entitled to say that English *t* and *d* are distinct segments because of minimal pairs like *tip* and *dip*; in so saying he supplies the tacit assurance that he knows of no reason to suppose that the *t* in *tip* versus the *d* in *dip* ought to be predicted from considerations he hasn't mentioned.

Similarly, what most generative phonologists would take as methodological issues—the relative unacceptability of special underlying representations for non-alternating segments, like the *ʔ* in *button* (Principle (T) below) and the relative unacceptability of absolute neutralization (Principle (U) below)—Paul Kiparsky (ms. 1968) has promoted to theoretical issues, by hypothesizing that no languages violate strong forms of Principles (T) and (U).

Methodological principles are cited, or appealed to implicitly, in response to the question 'Why did you choose analysis X instead of analysis Y or Z?' and as guiding procedures in analyzing a language. So, for example, Principles (L) and (M) move the analyst to survey minimal pairs and look for elements in non-contrastive distribution.

Neither the structure of the evidence nor the rationale of ongoing analysis is exposed by presentations that lay out the result as a *fait accompli*, an extensive formal description with illustrative examples. Unfortunately, some of the generative phonological literature is of this sort, and some consists of exegeses of Chomsky's theoretical positions, with no regard for evidential or procedural issues. This reluctance on the part of us generative grammarians to take our colleagues into the kitchen has made many of them feel the cooking is done not with ordinary materials and by ordinary utensils but instead with soma and by divine inspiration. Fortunately, the literature now contains the reminiscences of some reputable, if not infallible, chefs. I refer to such works as Kuroda (1967) on Yawelmani, Schane (1968a) on French, Harris (1969) on Spanish, McCawley (1968) on Japanese, parts of Wurzel (1970) on German, and here and there sections of Chomsky and Halle (1968) on English. Especially valuable in this regard are the squabbles over controversial issues in the analysis of specific languages. For instance, there is the many-faceted discussion surrounding the underlying form of the English inflectional endings: Does the underlying shape of the noun plural suffix contain a vowel, or is it simply *z*?³ The vowel analysis was first defended by Bloomfield (1933:212), who cited 'an exact parallel in English

3 For the purposes of this discussion, the additional possibilities *s* and vowel plus *s* are excluded (though this exclusion must be defended, as in Lightner 1970a), as is the possibility that different inflectional endings are to be analyzed in different ways (a solution advocated by Sloat and Hoard 1970). Also I take no stand on which lax vowel would occur in the suffix; there are at least four alternatives—*i*, *e*, *i*, and *ə*—that are not obviously wrong.

syntax', namely the forms of the verbal auxiliary *is*, as supporting evidence; Nida (1948: sec. 3.03) gives the argument in detail. Hockett (1958: 282), on the other hand, argues for a vowelless analysis, on the grounds that *z* is the only plural allomorph whose selection is not automatic. Both positions are represented in the generative literature—the vowel analysis is maintained by Luelsdorff (1969) and Zwicky (1970a: 333f.), who give Bloomfield's argument; the association of auxiliary contraction and the noun plural alternations is attacked by Lightner (1970a) in an otherwise inconclusive article; the vowelless solution, assumed without argument in Labov (1969), is defended by Sloat and Hoard (1970), Shibatani (1971), and Delack (1971); more recently, Guile (1972: 468) and Miner (1972) have adduced new evidence in favor of the vowel analysis. This sequence of articles is especially interesting, in that not only are new data from English cited in connection with the English inflectional endings, as in Sloat and Hoard, Shibatani, and Miner, but also quite a variety of other lines of argument are offered—theoretical considerations proper, as when Shibatani considers the role of surface phonetic constraints and Miner the effect of eliminating extrinsic ordering statements; cross-linguistic generalizations, as when Guile cites a putative condition on syncope and epenthesis; non-standard dialects, used by Shibatani; and patterns of acquisition, mentioned by Delack.⁴

Some additional insight into the characteristic approaches of generative phonologists can be obtained from restatements, such as the reworking of the Southern Paiute material in Sapir (1946) by Harms (1966), Rogers (1967), Chomsky and Halle (1968: 345–9), Nessly (1971), Lightner (1972: 340–2) and Cairns (forthcoming); the recasting of Swadesh and Voegelin (1939) by McCawley (1969) and Lightner (1970b); and the discussion of Robins (primarily 1957) on nasalization in Sundanese by Langendoen (1968: 100f.), Howard (ms. 1971), and Anderson (1972).

4 Other *casus belli* include umlaut in German (Zwicky 1967; Bach and King 1970; Vennemann 1968; Wurzel 1970: Teil 2), word stress in English (Chomsky and Halle 1968: ch. 3; Ross ms. 1969; Lee 1969; Langendoen 1969; Sloat and Hoard 1972), sentence stress in English (Chomsky and Halle 1968: ch. 2.1; Bresnan 1971; Lakoff 1972; Berman and Szamosi 1972; Bresnan 1972; Bolinger 1972), Spanish plural formation (Foley 1967; Saltarelli 1970; Harris 1970a), metathesis in Greenlandic (Pyle 1970; Underhill 1971; Sadock 1972), vowel harmony in Nez Perce (Aoki 1966; Chomsky and Halle 1968: 377ff.; Jacobsen 1968; Kiparsky ms. 1968; Rigsby and Silverstein 1969; Zwicky 1971b), stem vowels in Finnish nouns (Harms 1964: ch. 1; McCawley 1963; Austerlitz 1967; Anttila 1967: 569f.), and Grassmann's and Bartholomae's Laws in Sanskrit (Zwicky 1965: ch. 5; Kiparsky 1965; Anderson 1970; Butler ms. 1972). Indeed, it can be fairly said that each language that has received more than cursory study from generative phonologists has its own puzzle areas—nasalization in French, retroflexion of *s* in Sanskrit, palatalization in Russian, vowel harmony in Turkish, the glottal fricative in Welsh, the coronal consonants in Japanese, the vowel shift in English, to add a few examples to those given already. In many cases, generativists' interest in these puzzles continues earlier structuralist discussion, of course.

Also valuable are sequences of argument on theoretical points illustrated by language-particular data, for instance on the representation of vowel length (Kenstowicz 1970; Pyle 1970; Fidelholtz 1971), on binary features for vowel heights (Kiparsky 1968: sec. 5; Wang 1968; Contreras 1969; Harris 1970 b; Naro 1971), on the assignment of phonological features to word boundaries (Zwicky 1965; Lass 1971; Halle 1971; Lightner 1972: 331-5), and on rule insertion (King ms. 1970; Watkins 1970; Dressler 1971).⁵

In the sections of the paper that follow, I try to bring out many of the methodological principles that figure in these and similar works.

2. *Data*. The first principles I mention are of a rather special sort; they declare what are acceptable sources of data.

(A) The data to be comprehended by a phonological analysis consists of

- (1) variant shapes of morphemes and
- (2) distributional restrictions on phonological elements.

This, the orthodox list, contains the data treated by structuralist morphophonemics and phonemics taken together, and no more. However, numerous other means have been suggested for the validation of feature systems, phonological representations, and phonological processes. These are listed in (B) together with a few works in which they are cited.

(B) Additional data can be obtained by considering

- (1) speech errors (Fromkin 1971),
- (2) misperceptions (Bond 1971: ch. 3),
- (3) language replacement (Dressler 1972 b),
- (4) aphasia (references in Whitaker 1971: 208-14),
- (5) borrowing (Hyman 1970 a, b; Ohso 1972),
- (6) cross-linguistic surveys of inventories (Miller 1972),
- (7) cross-linguistic surveys of processes (Foley ms. 1970; Lightner 1970 c; Schourup 1972; Stampe 1972 b; and many articles in *Working Papers in Linguistic Universals*, Stanford Univ.),
- (8) linguistic games (Sherzer 1970),
- (9) productivity of processes (Hsieh 1970; discussion in Dingwall 1971 a; Skousen 1972),
- (10) poetic requirements (Kiparsky 1968 b; 1971 a),

⁵ A full list of such topics would include at least: the alpha notation, the predictability of ordering relationships, cyclical application of rules, phonological conspiracies and targets, the representation of complex segments (e.g. affricates and diphthongs), strata of the vocabulary, variable rules, phonemic representation, rule repetition, syntactic constraints on phonological rules, and phonological constraints on syntactic rules.

- (11) historical change (Kiparsky 1968a, 1971b; and the phonological papers in Stockwell and Macaulay 1972),
- (12) acquisition (Edwards 1970; many articles in *Papers and Reports on Child Language Development*, Stanford Univ.; Drachman 1971 and elsewhere),
- (13) stylistic variation (Zwicky 1972a, b; Dressler 1972a),
- (14) patterns of dialect and idiolect variation, and
- (15) statistics of variation (both treated in many studies by Labov and his students and by Bailey, e.g. Labov 1971 and Bailey to appear),
- (16) orthography (Chomsky and Halle 1968: 49; Aronson 1969),
- (17) articulatory phonetics, and
- (18) acoustic phonetics (each separately or both together cited to support feature systems and formulations of processes in numerous works),
- (19) patterns of exceptions (Zwicky 1970c),
- (20) informant judgments on novel forms, and
- (21) psycholinguistic investigations of other types (for example Read 1971 and Gudschinsky, Popovich, and Popovich 1970: 86f.—two relatively unusual examples from a considerable body of material, much of it not explicitly generative),
- (22) distorted speech (Kazazis 1968, 1969).

Attitudes towards the types of data in (B) differ widely, from those who appear to believe that the types of evidence in (B) can never be used alone to justify an analysis, to those who hold that the two groups are of equal value, or even that some types of data in (B) are weightier than those in (A). Reliance on (A) alone, combined with a relatively 'abstract' stand on the nature of underlying representations and no special emphasis on regular or productive variants, or on variants as opposed to distributional restrictions, characterizes the *Sound Pattern* approach, whereas the phonological theory of Stampe (1972a) insists upon the psychological reality expressed by the considerations in (B).

For an illustration of the differences that can arise, consider how to analyze a language that appears to have the rule

$$s \rightarrow r / V_V$$

The most direct approach, and the one most in accord with (A), is to say that the rule shifts *s* to *r*. A less obvious approach would claim that two rules applying in sequence give the effect of *s* → *r*: roughly,

$$\begin{aligned} s &\rightarrow z / V_V \\ z &\rightarrow r / V_V \end{aligned}$$

The phonologist alive to the sorts of data in (B) would consider, for example, the occurrence of rhotacism rules across languages. One reasonable hypothesis (though data are hard to come by) is that if a language has intervocalic *s* and *z* in underlying representations, and shifts *s* to *r* in this position, then it shifts *z* to *r* in this position also. Accordingly, the direct shift of *s* to *r* is not a possible natural rule; the shift must proceed through an intermediate stage, even if that stage is not evidenced by synchronic alternations; or else the shift must be a morphologized remnant of several distinct phonological changes.

Beyond (A) and (B), other principles assign relative weights to different sorts of evidence: First, the disputed principle

(C) Data from (A) have greater value than data from (B).

Then the widely accepted principles

(D) A variant has greater value according as it is

- (1) more regular, and
- (2) more productive.

(E) Evidence from variants has greater value than evidence from distributional restrictions (cf. Vennemann 1970).

Finally, the principle of 'independent evidence':

(F) Insofar as possible, the choice of a particular remote representation should be motivated by several independent lines of evidence.

Thus, Kiparsky (1971b: 585), responding to Kisseberth (1969), admits that one might have to 'assume that wholly abstract segments are to be allowed when more than one rule refers to them crucially', despite the strictures of Kiparsky (ms. 1968).

3. Realism and Working Back. I turn now to methodological principles proper, beginning with two related injunctions that distinguish generative phonology from earlier morphophonemics; these are aspects of Postal's (1968: ch. 4) Naturalness Condition:

(G) Insofar as possible, the content of segments in remote representations is phonological rather than abstract.

Thus, morphophonemes are not distinguished by diacritics but by appropriate phonetically-based distinctive features, wherever possible.

(H) Insofar as possible, phonological rules are conditioned phonologically rather than arbitrarily.

That is, phonetically-based features are preferred to lexical markings.

Next, three principles bearing directly on the choice of remote representations, beginning with a third aspect of the Naturalness Condition:

- (I) Whenever possible, a remote representation for an occurrence of a surface segment is chosen from its set of variants.

That is, the analyst normally assumes that the underlying representation of a morpheme is one of its 'forms'.⁶

- (J) Otherwise, the content of segments in remote representations is assembled piece by piece, using information from variants and distributional restrictions ('homing in', as in Zwicky ms. 1972).

For example, we find the *Sound Pattern* (191f.) analysis of [ɔj] as underlying $\bar{\alpha}$ defended by a series of steps in which it is argued that on the basis of English rules previously motivated, a remote representation for [ɔj] must be first a tense vowel, which next must be nonback, then also low and round.

- (K) As far as possible, each set of surface variants has a single corresponding underlying representation.

Thus, insofar, as possible (excluding, e.g., suppletion), each morpheme has a unique underlying form; the listing of alternants is to be minimized.

Principles (G), (I), and (K), taken together, require the analyst to assume that phonological representation is identical to phonetic representation, unless he has evidence to the contrary. Consequently, analysis proceeds by 'working back from the surface'. At each stage it is argued that some representations are underlain by different representations (for which I have been using Postal's felicitous term 'remote representations', so as to make no claims about when the most remote, or underlying, representations are reached). The make-up of remote representations is guided by two further rules of thumb,

- (L) *Ceteris paribus*, two segments that distinguish morphemes are underlyingly distinct.

- (M) *Ceteris paribus*, two segments that never distinguish morphemes are not underlyingly distinct.

At each stage in the process of working, or arguing, back from the surface, rules are formulated as notations of the processes relating more remote representations to less remote ones. These rules and remote represen-

⁶ A strong, or substantive, form of this principle, requiring the underlying representation to be one of its forms, is attributed by McCawley (1967: 80) to William Dwight Whitney. As is well known, structuralist phonologists expressed doubts about 'fictitious' base forms; but the need for some cases of such representations is a commonplace of generative phonology.

tations themselves are subject to various conditions that have been proposed in the literature, among them:

- (N) Of the available alternatives, choose the remote representation with the most complex or least determined context, so as to obtain the simplest rules deriving the surface form.
- (O) Choose a representation from which the surface forms can be derived by rules that are natural, in the sense that they recur in many languages and have a phonetic basis.
- (P) Choose the least marked representation available (Schane 1968 b, opposed by Malone 1970 and Vennemann ms. 1971 a).
- (Q) Choose the representation that results in the longest derivations (take a 'free ride', in the sense of Zwicky 1970 b).
- (R) Choose representations and rules so as to minimize extrinsic ordering statements (Anderson 1969; Kisseberth ms. 1972; Norman 1972).
- (S) Choose underlying systems that are unmarked, in the sense that they recur in many languages and are symmetrical (Chomsky and Halle 1968: ch. 9).

Of these principles, only (N) is regularly used without comment. Principle (O) is widely applied, but presents difficulties when the most natural analysis (in the sense of this principle) is not the simplest one, or when facts make the most natural analysis unlikely (Davison 1971). The remaining principles are all controversial. Probably, none of them is a good guide to analytic or verificatory practice. Certainly they are contradictory as a set and contradictory to other principles,⁷ so that at the very least different principles must be assigned different weights; some must undoubtedly be discarded.

Further constraints on the choice of remote representations have been advocated by Kiparsky (ms. 1968; 1971 b). Expressed in terms of methodological principles, Kiparsky's constraint on abstract analyses splits into two conditions, the first a special case of (I):

- (T) Other things being equal, an occurrence of a segment not involved in alternations should be represented underlyingly in its surface form.
- (U) Wherever possible, avoid rules that neutralize completely some underlying distinction.

A condition that is similar in spirit to (T), but logically independent of it, is one that McCawley (1967 a: 79; 1967 b: 107) observes in the descriptions of William Dwight Whitney and Edward Sapir:

- (V) Every underlying segment should also occur as a surface segment.

⁷ The opposition of *Sound Pattern* naturalness, expressed by (S), and Postalian naturalness, expressed by earlier principles, is remarked upon in Zwicky (1971 b).

Generative descriptions do not hold to this principle absolutely, but analysts customarily feel obliged to defend violations of principle (V), and (T) as well.

Even in analyses in which the underlying segments constitute a subset of the surface segments, there is considerable room for non-patent steps. For example, underlying forms can be chosen by a sort of 'musical chairs' principle, so that surface x realizes underlying y, surface y realizes underlying z, and surface z realizes underlying x; the result violates Principle (T) and requires special justification. The *Sound Pattern* treatment of English vowels is very close to this paradigm. Also, intermediate steps in derivations may introduce elements that do not occur on the surface, even when the underlying inventory is impeccable; these 'false steps' (Zwicky 1972c) are constrained by a generalization of Principle (V):

(W) Every segment in a derivation should occur as a surface segment. Again, deviations from Principle (W) are reasonably common; they do need defense, however.

If principles (T) and (U) are promoted to theoretical principles prohibiting special underlying forms for non-alternating forms and prohibiting rules of absolute neutralization, then this position in combination with a strong form of principle (I) yields a particularly 'realistic' or 'non-abstract' view of underlying representations, one not held by anyone, to my knowledge. This extreme position would be opposed to a highly 'formalistic' or 'abstract' approach, which would seek the simplest lexicon and system of rules consistent with the data to be explained, regardless of other considerations. An extreme version of the formalistic position is again one not actually advocated by anyone, although as Vennemann (ms. 1971b) points out, there is a noticeable tendency in the literature towards a 'once a systematic phoneme, always a systematic phoneme' principle:

(X) If some occurrences of a segment x are derived from a remote representation distinct from x, then all occurrences should be derived from remote representations distinct from it.

That is, the existence of one or more sources for x allows us to eliminate it entirely from the underlying inventory. This principle is applied several times in *Sound Pattern*, as when the existence of a Vowel Shift rule deriving aj from i permits even non-alternating occurrences of aj, as in *light*, to be so derived. This principle has not been defended, except insofar as it promotes simple underlying systems, and it contradicts reasonably well-established principles like (I), hence it cannot be considered established.

4. Simplicity and Significant Generalizations. I provide here no discussion of formal simplicity and the evaluation metric, because I believe that these considerations have played virtually no role (beyond that in Principle (N)) in what generative phonologists have done in arriving at

and arguing for particular analyses. Consequently, although I find Botha's recent (1971) book on methodology in generative phonology stimulating at many points, I have not built on it in this paper, because its almost exclusive concern with the evaluation metric removes it from the domain of the working phonologist.

Simplicity in its informal sense is, of course, appealed to often in the generative literature, by implicit use of principles like

(Y) Write as few rules as possible.

and in the explicit attempts to capture (in Chomsky and Halle's phrasing) 'significant linguistic generalizations', an undertaking governed by the maxim

(Z) Whenever possible, apparently disparate facts which belong together should be described by identical means.

Here we have passed well beyond methodological assumptions peculiar to linguistics, into attitudes towards scientific inquiry in general. Principle (Z), which is appropriately the last of the set, amounts to an injunction to search out the hidden unities of nature.

5. Concluding Remarks. Methodological principles are established by success; they are valuable insofar as they lead us to choose those accounts of phonological phenomena we have independent reason to suppose are (relatively) correct. As a result, if there is a poverty of sustaining evidence for us to refer to, there is a high degree of indeterminacy in our descriptions. This indeterminacy—the existence of alternative descriptions all of which are possible theoretically—has manifested itself so often that there is a continuing effort to go beyond linguistic facts in the narrowest sense (those in (A) above) and to pursue other evidence (the lines of inquiry in (B), for instance). Consequently, the central methodological issues depend for their solution upon a decision as to the sorts of data germane to phonological analysis.

The title of this paper echoes 'The Strategy of Phonemics', an article of Morris Halle's setting out some of the methodological principles of phonemics at a time when he was wrestling with the foundations of the theory. I have tried to do something similar for generative phonology at a time when most of its practitioners⁸ are sceptical of some of the fundamental tenets of that theory. I close with an apposite quotation from Halle's earlier work (1954: 199): 'To us the major criterion for the applicability of a certain category to linguistic description is whether or not this category yields simple statements not only on the particular level for which it was introduced, but on all levels which are pertinent to descriptions of a language. It must always satisfy a multiplicity of criteria'.

⁸ See, for example, the discussion in Lightner (1971).

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Diskussion

Dressler: 1. While praising the lucid distinction between theoretical and methodical principles, I want to add that theoretical principles must also be hierarchized and interconnected among themselves.

2. It might be good to distinguish clearly between the level of description (i.e. construction of hypotheses) and the level of the facts themselves, cf. the work of Šaumjan.

3. In generative grammar methodical principles are often promoted to theoretical ones, in order to strengthen a claim and to make falsifiability easier.

4. As to poverty of sustaining evidence, it is found most of the time in historical linguistics: So one could think of resigning to use there such a demanding theory as contemporary generative grammar, such as Zwicky has portrayed it so admirably.

ad Dressler:

ad 1. I entirely agree.

ad 2. Some distinction along these lines is required, but the matter is not so simple as it might appear. Philosophers of science have pointed out again and again that much of what we take to be “facts” is actually colored extensively by our theoretical presuppositions. This is certainly true in linguistics; a statement like “The plural of [kæt] is [kæts]” already embodies quite a bit of theory.

ad 3. Certainly. But the issue is not falsifiability *per se*. Falsifiability could be achieved in quite absurd ways. The issue in promoting a methodological principle to a substantive one is whether the extension or contraction of theory implied is warranted.

ad 4. The problem here seems to me to be analogous to the contrast between the results obtainable by paleontology and those obtainable from the study of living organisms. We do not create a separate theory for cases

known to us only by reconstruction; rather, it is assumed that they are of similar type to existing cases, but that we may have to remain in ignorance of aspects of them, even though such aspects may be considered important in descriptions of existing cases. We demand as much as we can from a theory, and get along with less when we have to.

Anttila: Dressler pre-empted my question about theory and method. I do find your opening the kitchen doors very illuminating for the history of linguistics. You provide a point in a Disneyland General Electric type of display. Your paper is parallel to the lay-out of Bloch's kitchen (postulates). Also he was trying to establish a hierarchy of principles by weeding them down to the end. Theory and method were entangled in American structuralism, and in practice they still are.

ad Anttila: Theory and method are always entangled. My intent in this paper was to unravel some of the knots. I think, however, that Bloch's postulates, like Harris' methods, confuse the two domains and (despite their real value for students of linguistic methodology) shed no light on relationship between structuralist theory and method.

Drachman: With regard to the principle of 'homing in', an earlier illustration of yours (on Sanskrit) suggested clearly that there was in fact no principled way to know when to stop squeezing the data. On the other hand, your principle "L" is perhaps not to be dismissed so lightly as you suggest. Data for the acquisition of Modern Greek gives some reason to believe that a rule discoverable from a non-opaque alternation (e.g. voicing of word-initial stops following nasals in external sandhi) may be generalized to non-alternating stops (word medially).

DeArmond: It appears to me that all of these principles may occur as constraints on the theory of phonology, although some are directly in contrast with others, in which case one or the other must be selected. In this event the one which makes the largest claim and hence is the most restrictive is the one to be selected. I consider Postal's Naturalness condition to be one of the largest of the constraints, though by no means the only one, which has been proposed for phonology. I would appreciate your view on this point.

ad DeArmond: Although I have not systematically attempted in this paper to assign relative weights to the various methodological principles, I would agree that those encouraging a close relationship between phonetics and phonology are extremely important.

Haas: It has been explained to us by Mr. Zwicky that a Generative Phonology that would rely exclusively on 'theoretical principles' would admit many alternative descriptions, including absurd ones, and offer no good reasons for choosing among them. Reasonable choice requires an appeal to what he calls "methodological principles". The latter, then, are hardly less important than the 'theoretical'. Most, if not all, of them seem to be

familiar from the earlier descriptive or historical study of languages. Many of those principles, which are not theoretical within the framework of a generative *representation* of linguistic constraints, seem to be theoretical principles of another discipline—namely, of *Linguistic Analysis*. Have we not, in fact, been asked to rescue this discipline from years of neglect? Its theoretical significance has been obscured by a confusion about ‘discovery procedures’, a confusion of two kinds: (i) methods or operations which enter linguistic reasoning and the theory of description (just as an astronomer’s measurement of the altitude of a star enters astronomic reasoning) and (ii) others (such as the use of a telescope) which are ‘merely’ of heuristic value.

ad Haas: One of my secondary purposes in this paper was to indicate the extent to which generative phonology has not been constructed *ab ovo*, but instead continues (in one form or another) many principles of earlier linguistic study—as surely it must. However, I do not see Linguistic Analysis as a discipline separate from linguistics, each with its own “theoretical principles”; nor do I see Chemical Analysis as a discipline separate from chemistry, or Diagnostics as a discipline separate from Medicine. Nor do I think linguistic analysis has been neglected. I had hoped that my citation of exemplary analytical works and of many vigorous analytic controversies would have dispelled the idea that generative phonologists have spent fifteen years spinning out formalisms and advocating innate ideas. Unfortunately, these works (and my earlier articles concerning methodology) have not reached so wide an audience as the programmatic and broad theoretical works.

Chen: (1) You have rendered a brilliant and easy to follow account of what characterizes the *modus operandi* of today’s practicing generative phonologists. You have given a guided tour to the generative kitchen, and equipped the curious visitor with a ‘recipe’, so to speak, that tells him how to cook a generative meal. From the point of view of an outsider (or, for that matter, a neophyte), however, the rationale behind each of the strategies may not be transparent. E.g. the convenient, but ultimately sterile dichotomy between competence and performance and the idealized homogenous language system account for the general downgrading of the types of data in (B). The tendency to simulate computer operation (cf. Postal 1968 *Aspects* . . . p. 270: “Grammars are thus logically analogous to computer programs . . .”, cf. *ibidem* pp. 232, and 273. However, cf. disclaimer, SPE p. 60, fn. 4.) underlies the ‘free ride’ principle (Q). The Generative-Transformational model, in fact, hypothesizes a generating mechanism that has a powerful high speed central processing unit but an extremely limited random access storage capacity. That’s why the linguistic analyst is not penalized for long and complex derivations. The rationale is something like: minimize programming instructions (your principle Y), but maximize the use of existing instructions (looping, subroutine, your principle Q).

While we are on the topic of 'free ride' principle, it may be useful to recall SPE p. 63:

"Grammars should be selected in such a way as to minimize the amount of 'computation' that is necessary, and . . . 'length of derivation' is one factor in determining 'complexity of computation'."

(2) There are obvious hierarchical relations that hold among the principles A-Z. You could have brought out this fact more clearly — but that would mean, of course, radically extending the scope of your paper to include the 'philosophy', or the general theoretical framework of generative phonology. If I understand you correctly, your immediate concern was to describe the 'strategy' as against defining the 'goal' of generative phonology. Analogous to the goal/strategy distinction, we have pairs like theory/methodology, or, for that matter, statesmanship/politics, war/battle, etc.

(3) I think you played down the importance of the simplicity metric. I heartily agree with you that a simplistic feature-counting simplicity metric virtually plays no role in guiding the analyst's choice *in practice*. (cf. Zimmer 1969. On the evaluation of alternative phonological descriptions, *Journal of Linguistics* 6: 97-8 writes: "The fairly widespread assumption that feature counting will automatically lead us to choose the preferable description from two or more competing ones, . . . has never, to my knowledge, really been supported by detailed and convincing arguments".) It has been the practice, however, to appeal to some sort of simplicity metric in arguing for or against the merits of a particular analysis. Steve Anderson, for instance (dissertation mimeo, Indiana U. Ling. Club p. 77), contrasts McCawley's analysis of Finnish consonant gradation (using 24 features) with his own (14 features!).

More importantly, although rarely amounting to a crucial argument in practice, the simplicity metric has played a basic role in shaping the model of generative phonology and in determining the formal aspects of the descriptive apparatus. This is true especially of the earlier generative literature. If I understand correctly, the only linguistic proper argument for distinctive feature notation (which has been accorded a *theoretical* status, not merely pragmatic preference) against segmental notation is the necessity to formally reflect the naturalness or unnaturalness of phonological statements, and the naturalness is formally expressed in terms of the relative complexity of the rules, and the complexity of the rules, in turn, is measured in terms of the number of features and symbols used. The same need of a formal evaluation measure determines the use of certain formal abbreviatory devices (which again, in my understanding, are not mere matters of notation, but constitute substantive claims about the innate capability of man to manipulate and interpret such abstract devices).

I have discussed some of these ideas in a recent paper *On the Formal Expression of Natural Rules in Phonology* (preliminary version in *Project on Linguistic Analysis*, University of California, Berkeley, September 1972 no. 16: 1-29; to appear in *Journal of Linguistics*, vol. 9: 2).