D. TERENCE LANGENDOEN

REVIEW ARTICLE

C.E. Bazell, J.C. Catford, M.A.K. Halliday, and R.H. Robins (eds.), *In Memory of J.R. Firth*, Longmans, London, 1966. xi, pp. 500.

1. INTRODUCTION

These 27 essays on a wide variety of linguistic topics is indeed a fitting tribute to the memory of the man who more than anyone else has been responsible for the flourishing of general linguistic study in Great Britain today. Each of these papers save one deals with one or another of the aspects of linguistics with which Firth's name is most closely linked: prosodic analysis (nine papers), the non-phonological study of linguistic elements in terms of their co-text and context (fourteen papers), and the history of linguistics (three papers). The one paper outstanding is by W. Haas, and it deals with general linguistics as a whole. In this review I shall deal in turn with the papers in each of these four groups, concentrating most heavily on those of general or theoretical importance.[1]

2. PROSODIC ANALYSIS

Eight of the nine papers on prosodic analysis deal with phonological problems in individual languages: K.H. Albrow, 'Mutation in "Spoken North Welsh"', pp. 1–7; W.S. Allen, 'A Problem of Greek Accentuation', pp. 8–14 (this paper is a little gem); R.E. Asher, 'The Verb in Spoken Tamil', pp. 15–29; John T. Bendor-Samuel, 'Some Prosodic Features in Terena', pp. 30–39; Eugénie J.A. Henderson, 'Towards a Prosodic Statement of Vietnamese Syllable Structure', pp. 163–197; Judith Jacob, 'Some Features of Khmer Versification', pp. 227–241; H.L. Shorto, 'Mon Vowel Systems: A Problem in Phonological Statement', pp. 398–409; and R.K. Sprigg, 'Phonological Formulae for the Verb in Limba as a Contribution to Tibeto-Burman Comparison', pp. 431–453. The ninth, by T. Hill, 'The Technique of Prosodic Analysis', pp. 198–226, is an attempt to meet the need for a pedagogical introduction to prosodic analysis, 'with sufficient "worked examples", to make it easier for the student not merely to understand work published by

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others but to set about doing it himself' (p. 198). It is an entirely successful effort; Hill has provided an eminently serviceable pedagogical tool, not only for students, but also for professional linguists untrained in Firthian phonology who would however like to teach themselves what prosodic analysis has to offer. Moreover, the relationship of the data to the analysis is so clear and explicit, that one who has been trained in other phonological approaches can immediately determine for himself in what ways prosodic analysis is similar to or different from his own.[2] On this matter, Hill himself has some interesting and I think very important remarks to make (pp. 222–223). The first is that prosodic analysis should not be viewed simply as a reaction to or as a corrective for the neo-Bloomfieldian phonemic approach; that it is mistaken to think of some languages as being more amenable to phonemic analysis while others yield better to prosodic techniques (Hill interprets the statement to this effect in Lyons (1962, p. 132) as having possibly been intended simply as a courteous recognition on Lyons' part of the existence of American phonemic phonology). Prosodic analysis stands on its own legs as a phonological theory, and compares most favorably with phonemic theory when put to the test.

Hill's second point deserves to be quoted in full (I retain his original capitalizations):

> It will have no doubt struck the reader that, in those numerous and fundamental respects in which Transformational phonology has broken away from Phonemics, it is recapitulating, a decade or more later, the developments of Firth's theory. Polysystemicity is an essential element in it, as is the flexible approach to the relation of exponent and phonological item. It is still (like Firth in 1935) attached to the articulatory segment as a basic unit – but this is bound to change. It seems indeed especially unfortunate that a theory operating with the data of acoustic phonetics, in which the parameter rather than the segment is the natural basic isolate, should not have followed out the implications of this for analysis. (p. 223)

What is most staggering about this passage to me is not the monumental falsity of the assertion that generative phonology is "attached to the articulatory segment as a basic unit" (about which more shall be said below), but Hill's assurance that it is patently obvious that developments in generative phonology are recapitulating the developments in Firth's thinking about phonology ten or more years later. Degree of polysystemicity allowed is a relatively superficial criterion by which to compare the two theories, and

[2] One minor correction for Hill's summary of harmony in native Turkish polysyllabic words is in order. He maintains that rounding may occur only "over the earlier portion (possibly the whole) of the word" (p. 204); however in progressive aspect verbal forms, rounding may occur only over the latter portion of the word, as in *Kalıyordular/kalıyorlardı* 'they were staying'. Hill makes another minor correction for the rule also in his footnote 9, p. 224.

besides neither theory can be said to have really broken away from neo-Bloomfieldian phonemics in this regard, since many practitioners of that theory permitted it to some degree too; see for example Fries and Pike (1949). The relative flexibility of the Firthian and generative phonological approaches to the "relation of exponent and phonological item" as opposed to the relative rigidity of American phonemic phonology may perhaps be admitted, but here one must be very careful to separate theory from practice, and compare theory with theory and practice with practice. As I have argued elsewhere (Langendoen, 1968, Chapter 3), the theoretical constraint explicitly imposed by Firth upon prosodic analysis, that it be an allotment of phonetic data to phonological systems (phonematic and prosodic), is identical with the taxonomic constraint that phonemic analysis theoretically operates under. As I have also shown, most Firthian practice ignores this constraint. But similarly, much neo-Bloomfieldian analysis ignores constraint imposed upon it by theory too, sometimes sanctioning such practice by the procedure known as normalization (see Swadesh (1934), Hockett (1942) for example). On the other hand, generative phonology admits of no such constraints, either in theory or in practice.

Also, in order to substantiate his claim, Hill should have informed us what in fact was "the development of Firth's theory" over time. As I understand it, Firth's earliest phonological stance was essentially that of Daniel Jones, and that by 1935 he was propounding a position in all respects comparable with that of Twaddell (1935). Finally, and most importantly, I have shown that, despite their different starting points, Firth and Z. S. Harris had by 1948 developed phonological theories, prosodic analysis and long component analysis respectively, whose representations can be mapped one into the other (Langendoen, 1968, Chapter 3). However, by this time, Firth has repudiated the notion that the result of a phonological analysis should be a redundancy-free (or redundancy-minimal) representation of the phonetic system, which was of course the whole motivation on Harris' part for long-component analysis. Firth's motivation, rather, was to develop a phonological representation which is appropriate for the phonetic data.

This fact is what makes sound so strange Firth's own insistence that the phonological categories for each language should be set up on an *ad hoc* basis.[3] The only way I can make sense out of this is to assume that Firth felt he had to present an anti-universalist posture. That he however believed in phonological universals and was concerned to discover them is clear from an examination of his discussion of real language material in Firth (1948).

[3] Some contemporary neo-Firthian phonologists, notably Robert M.W. Dixon, have managed to adhere strictly in practice, as well as in theory, to the principle that phonological analyses be done *ad hoc* for each language. See, for example, Dixon (1965).

Hill's thesis that generative phonology is in certain respects recapitulating Firthian phonology can, however, be defended, although not for the reasons he himself gave (as I have just shown). Consider first the matter of the goals of phonological analysis in generative phonology. One of them, at least until very recently, has been to obtain a redundancy-free representation of the phonetic material, in which one takes into consideration all the material supplied in a surface structural description (the output of the transformational component). As work progressed, it was realized more and more just how difficult it is to achieve genuine freedom from redundancy – difficult that is if the phonological description is not to contain specious simplifications and phonetically unmotivated base forms.[4] Not it is becoming generally recognized that there is no point to aiming at elimination of redundancy; rather one should be concerned with obtaining as broad an inventory as possible of phonological universals (including implicational universals in the sense of Greenberg (1966)). Then the phonological representation should be set up such that the rules of universal phonology are maximally applicable to them.[5] Thus it could be argued that generative phonology is recapitulating Firthian phonology, in its current departure from concern with the elimination of redundancy.[6]

Firthian phonology may also be said to have anticipated the now classic argument of Halle and Chomsky concerning the irrelevance of a taxonomic phonemic level between a systematic phonological representation on the one hand, and a systematic phonetic representation on the other (Halle (1959), Chomsky (1964), Chomsky and Halle (1965)). If we admit for purposes of this argument the similarity between Halle and Chomsky's notion of a systematic phonological representation and Firth's notion of a combined prosodic and phonematic representation on the one hand, and systematic phonetics and Firth's 'phonic data' on the other, then we can see that the *form* of Halle and Chomsky's argument was in fact used by Firthian phonologists against neo-Bloomfieldian and particularly Harrisian phonology considerably earlier, for example in Allen (1957). Allen's argument was that even granted the ultimate similarity of Harrisian long-component analysis and a prosodic one, Harris' techniques involve setting up an irrelevant intermediate level, namely a taxonomic phonemic one, to which the long component

[4] For a very useful survey of the problems involved in achieving genuine redundancy-free representation, see Stanley (1967).

[5] Such a view of phonology is currently being developed by my colleague David L. Stampe, and others.

[6] But in so doing, it is not consciously recapitulating Firth; the original impetus to escape from concern with redundancy within the ranks of generative phonologists was supplied by their rediscovery of the Prague School theory of markedness. There is no evidence which would indicate that Firth was ever influenced by this aspect of Prague School theory.

analysis refers. Schematically, we can compare long component analysis and prosodic analysis as in Figure 1. Allen rejected the phonemic level in Harris' system because it was phonologically *inappropriate*. It sets up elements, phonemes, about which distributional statements at the level of long components have to be made. As Allen wryly observed, you do not need distributional

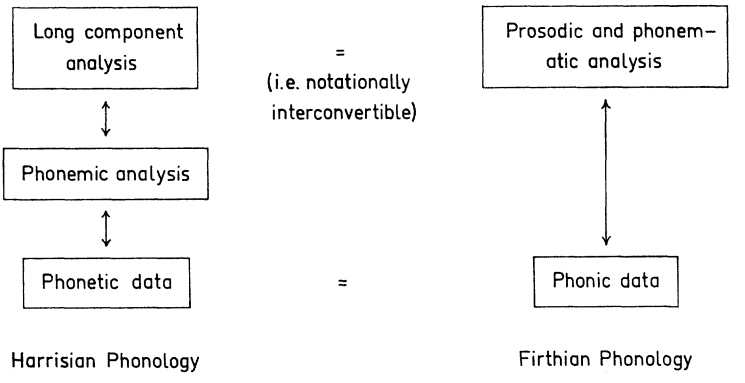

Fig. 1. Schematization of Harris' long component analysis and Firth's prosodic analysis according to Allen (1957).

statements, if you do not set up elements which require them in the first place.[7] The fact that the most important arguments used by the schools of prosodic analysis and of generative phonology against neo-Bloomfieldian phonology should have identical forms, I take to be quite significant.

In the paragraph following the one in which the preceding long quotation was taken, Hill observes:

> The essential difference between the two theories [sc. generative phonology and prosodic analysis] is probably the concept of a sequence of operations, found in Transformation phonology (p. 223).

He then goes on to discuss a problem in Turkish phonology, and compares the generative phonological treatment of it in Lees (1961) with its prosodic treatment in Waterson (1956). Lees' description makes use of "sequence of operations", or what in Bloomfield's terms is called "descriptive order" (Bloomfield, 1933, pp. 212–214), while Waterson's does not. The problem has to do with the Turkish possessive suffix, which has an initial *s* when suffixed to noun stems ending a in vowel, and is lacking that *s* when attached to a stem ending in a non-vowel. Lees accordingly has set up a rule to delete the initial *s* of the possessive suffix (which Lees takes to have a single under-

[7] This was Allen's version of Firth's argument against the relevance of redundancy in phonological analysis, and for phonological appropriateness as the aim of analysis.

lying phonological representation, namely with initial *s*). However, noun stems with an underlying final *ğ* lose that *ğ* when a vowel-initial suffix is added, and this rule must be applied after the rule which deletes the initial *s* of the possessive suffix when it is preceded by a non-vowel (*ğ* is such a non-vowel). Thus (ignoring stress and vowel harmony) the addition of the possessive suffix *sin* to the noun stem *köpeğ* (dog) results in *köpei* (his dog) – the final *n* of the possessive suffix is lost by another rule. If the rules were to be applied in the opposite order, then the *ğ* deletion rule would not apply, although the *s* deletion rule still would, resulting in **köpeği*.[8]

Waterson's prosodic treatment of the same phenomenon is to set up an "S prosody" for the possessive suffix, which after "base final C [meaning non-vowel] there is no exponent" (p. 584). The exponent of K^h (=standard Turkish orthography and Lees' *ğ*) in syllable-initial position is voice and absence of plosion (which renders it in fact inaudible). Following his statement of Waterson's treatment, Hill continues:

> It may well be argued that sequence is concealed in Waterson's characterization of S prosody, and contrariwise that Lees' sequence is an idiosyncratic way of setting out a structure. In any case, the parallelism between the separate assertions is obvious, and the two theories deserve close comparative study. (p. 223).

Indeed, descriptive order is concealed in Waterson's treatment; furthermore it is a treatment which exemplifies the general procedure for concealing descriptive order first developed by Harris (1951, pp. 237–238). The same concealment can be detected in any number of prosodic analyses, for example in Allen's treatment of the Hāṛautī nominal (cf. Langendoen, 1968, Chapter 4, Section 4; Langendoen 1964), or in Albrow's treatment of Welsh mutation in the present volume. But while two or three mutually ordered rules are easily expressible (or concealed) within a prosodic or long-component system of notation, matters generally become unbearably complex when five or more rules are involved in an ordering relationship. Since natural language apparently does not eschew requiring such sets of rules for a descriptively adequate account of it,[9] we may conclude that generative phonology provides a much better framework for the treatment of phonology than does either long-component or prosodic analysis.[10] Lees' "Sequence" is most definitely *not* "an idiosyncratic way of setting out a structure."

[8] Hill mistakenly asserts that the result of applying the rules in the wrong order would be **köpeksi*. The point that descriptive order in generative phonology, however, is still made.

[9] For an illuminating discussion of this point from an historical perspective see Kiparsky (1967). Recently, D.L. Stampe has shown me that relatively 'low level' phonetic facts about an English dialect which we share, concerning vowel length, nasalization, and dental flapping require five ordered rules for descriptive adequacy.

[10] Followers of prosodic analysis often have a handy 'out', namely by appeal to poly-

Another parallel between prosodic and generative phonology is the claim by both theories that syntax is relevant to phonological description, and once again the rediscovery of this principle came first to the Firthians (see for example Robins (1963)). However, Hill in his treatment of 'Phonology in relation to Grammar and Lexis' (pp. 220–222) curiously restricts the part of grammar that can play a role in phonology to lexical category membership. This is too severe a restriction; it would rule out for example Bendor-Samuel's (1962) brilliant prosodic treatment of stress in Terena (cf. Langendoen (1968, Chapter 4, Section 9), and Bendor-Samuel's article in the present volume), in which he uses grammatical information above word level to account for a bewildering array of stress and tonal phenomena in the language.

I would like to return now to consider Hill's misconception that generative phonologists take the articulatory segment as their basic phonological unit, and then to the problem of Turkish phonology that formed the basis of his discussion of the question of descriptive order. As is well known, the basic units of generative phonology are the Jakobsonian distinctive features, which *have* been set up with due regard paid to the finding of instrumental phonetics.[11] Perhaps what Hill finds objectionable in generative phonology is that these features are characteristically displayed in one-column matrices, the column being the speech segment, but the necessity for doing so is immediately apparent from the briefest inspection of not only the phonetic facts as revealed by instruments, but also the facts of perception and articulation, not to mention native-speaker intuitions that there "really are" speech segments. Furthermore very common phonological phenomena, notably metathesis and reduplication, can only be accounted for systematically on the assumption that there are segments which metathesize and which are reduplicated.

If we now reexamine the Turkish material used in Hill's discussion of the problem of descriptive order, we see that both Waterson's prosodic analysis and Lees' generative phonological analysis fail to tell us *why* it is that the *s* of the possessive affix is missing after non-vowels and present after vowels. Both analyses would be as simple as before if, instead, the *s* were missing after vowels and present after consonants; that is if the facts were that *his money* would be expressed **para-ı*, and *his dog* would be **köpek-si*. But clearly this latter state of affairs would be highly surprising. As it is, in Turkish, the *s* of the possessive affix provides a hiatus between two vowels, and its absence

systemy, but that will not do for the case mentioned by Stampe (see footnote 9) and for many others.

[11] Whether they can be defined in terms of acoustic parameters is another, and for purposes of this discussion, irrelevant question.

after non-vowels prevents the occurrence of a cluster of successive non-vowels whereas the hypothetical state of affairs would either give us a vowel sequence or a non-vowel sequence. This is relatively unexpected vis-à-vis the actual state of affairs; that is to say, the actual state is more expected *from the point of view of universal phonology* than is the hypothetical state.[11a]

Suppose we say that (ignoring again vowel harmony, stress assignment, and the final *n* of the possessive suffix) the underlying forms for the possessive suffix are both *sin* and *in*.[12] Then we may say that that form of the suffix is chosen which would render the word in which it appears the most expected from the point of view of universal phonology. Thus the addition of the possessive suffix to the forms *kıl* (lake), *köpeğ* (dog), and *para* (money) would be *kılı, köpei,*[13] and *parası* respectively. Under this interpretation of generative phonology we have obtained a description which not only accounts for the occurring forms but also one which tells us in principled fashion why other forms do not in fact occur.

3. CONTEXTUAL STUDIES

The second group of papers contains the only one in the entire collection which is explicitly devoted to a critical evaluation of a segment of Firth's work, and that is John Lyons, 'Firth's Theory of Meaning', pp. 288–302. It is, by and large, a fair and sensitive evaluation of that part of Firth's work which is the most difficult and exasperating to deal with. Lyons' major points are (1) that Firth consistently held that the study of meaning is an integral part of linguistic science during a period in which practically every other theoretically concerned linguist held the opposite view, but (2) that Firth's theory of meaning, while calling our attention to the significance of the study of language use for sociological study, is incapable of dealing with the classical problems of semantics: reference, meaning relations (for example, synonymy, antonymy, consequence – for further discussion see Lyons (1964), Langendoen (1968, Appendix to Chapter 3)), and the notion of "having meaning". Lyons, however, does accept as valid Firth's concern to eliminate a dualistic theory of semantics, or more precisely one which is formulated in terms of concepts in the mind (see especially Lyons' footnote 23, p. 301).

[11a] Classical Greek is a counterexample; however intervocalic *s* passes through *h* before being deleted, and the tolerance for obstruent clusters accounts for the retention of post-consonantal *s* in that language.

[12] It does not seem appropriate to try to set up, as does Lees, a single underlying phonological representation for this suffix, since, in his terms, the *s* deletion rule applies only to this morpheme and to no other in the language.

[13] Note that the decision to specify the form of the possessive suffix must still be made before the application of the rule which deletes *ğ* in intervocalic position.

I see nothing to recommend Firth's and Lyons' position; moreover for reasons which have been most effectively stated by Bierwisch (1967, pp. 3–4), it is clear that semantic analysis (what Bierwisch calls semantic markers, following Katz and Fodor (1963)) are mental entities, in fact innate mental entities.

Lyons makes one other important point, a point which is crucial to two other papers in this volume to which we shall turn presently, namely that the study of collocations (the co-occurrence of pairs or groups of linguistic items) is not facilitated by the postulation of a "collocational level" of linguistic analysis (p. 297). Lyons' reasons are that there are a variety of reasons why particular collocations may or may not be acceptable, and there are a variety of other perspectives, besides that of acceptability, from which they may be studied. Therefore,

> Rather than set up one structural level to handle the co-occurrence of particular lexical items, *presumably on a statistical basis*, [emphasis mine] it would seem preferable to distinguish these several factors and these various points of view, and to investigate them separately (p. 297).

Two papers in this volume, M.A.K. Halliday, 'Lexis as a Linguistic Level', pp. 148–162; and J.McH. Sinclair, 'Beginning the Study of Lexis', pp. 410–430, however, attempt to make and to develop the opposite claim, namely:

> ... that it may be helpful to devise methods appropriate to the description of these patterns in the light of a lexical theory that will be complementary to, but not part of, grammatical theory. In other words, the suggestion is that lexis may be usefully thought of (a) as within linguistic form, and thus standing in the same relation to (lexical) semantics as does grammar to (grammatical) semantics, and (b) as not within grammar, lexical patterns thus being treated as different in kind, and not merely in delicacy, from grammatical patterns. This view is perhaps implicit in Firth's recognition of a 'collocational level' (p. 148).

For both these authors, moreover, the study of lexical patterns is to be made on a statistical basis (Halliday, pp. 153, 159; Sinclair, pp. 414, 418–419, 428). Lyons' objections to the advisability of studying collocations by undertaking statistical analysis of the patterns of co-occurrence of lexical items therefore apply directly to the work of Halliday and Sinclair, and as far as I can tell, these objections are irrefutable. This is not to say that such statistical analysis of textual material is not useful for certain purposes, but rather that it is of no value for linguistic theory.[14]

Halliday and Sinclair are open to criticism from another perspective as well. Just as Katz and Fodor (1963) maintained that semantics equals lin-

[14] In the course of their discussions, however, Halliday and Sinclair both make some interesting points concerning idioms; for example Sinclair observes that certain idioms have both long and short forms, e.g. *fed up* and *fed up to the back teeth* (p. 424), and that idioms can be interrupted by certain non-idiomatic material, for example oaths (p. 421).

guistic description minus grammar, so Halliday, in the passage quoted above, holds (though not so explicitly) that lexical description equals linguistic description minus grammatical description. Recently, McCawley (forthcoming) has given extremely compelling arguments against the Katz-Fodor view of semantics as a residue; his point being that within the framework of generative grammar, deep (grammatical) structure is the same thing as semantic structure, and that a separate semantic component of the type envisioned by Katz and Fodor is not necessary. The same kind of argument can be advanced against Halliday's and Sinclair's position. Everything that they envision as part of the lexical level (except for the statistical statements) can be considered to be represented in the deep structures of sentences.[15]

To show this, it would be necessary to elaborate the view concerning deep structure which underlies McCawley's argument, and to discuss all the examples raised by Halliday and Sinclair (particularly by Halliday) as illustrative of the kinds of considerations which show the need for an autonomous lexical level in terms of this view. Such an undertaking would be out of place in a review article such as this; let me simply indicate that McCawley's view of deep structure is akin to that of George Lakoff's and John Ross', which is set forth in Bach and Harms (forthcoming). It is a view concerning deep structure which is considerably more abstract than that set forth in Chomsky (1965); hints of its abstractness can be seen in Lakoff (1965). And rather than take up all of Halliday's examples, I shall consider just one.

That example concerns the use of the adjectives *strong* and *powerful*; both may be collocated with the noun *argument*, but only *strong* can be acceptably collocated with *tea*, while only *powerful* goes naturally with *car*. Expressions such as *a strong car* and *powerful tea* are relatively unacceptable vis-à-vis *a powerful car* and *strong tea* (p. 150). For such facts to be accounted for in grammatical terms, one would presumably have to provide a subcategorization of the nouns *argument*, *car*, and *tea* such that members of the

[15] In fairness to Halliday, it should be pointed out that he actually believes that the existence of an autonomous lexical level is an open question, which can only be decided on the basis of much further theoretically-oriented research:

"One may validly ask whether there are general grounds, independent of any given model, for supplementing the grammar by formal statements of lexical relations, at least (given the aim of linguistics to account for as much of language as possible) until these are shown to be unnecessary. It may be a long time before it can be decided whether they are necessary or not, in the sense of finding out whether all that is explained lexically could also have been incorporated in the grammar; there still remains the question whether or not it could have been explained more simply in the grammar. The question is not whether formal lexical statements can be made ... The question of interest to linguists is how the patterns ... are to be stated with a sufficient degree of abstraction, and whether this can best be achieved within or outside the framework of the grammar" (p. 150).

set including *argument*, would collocate freely with both *strong* and *powerful*; *car* and members of its set only with *powerful*; and *tea* and its set only with *strong*. As Halliday points out, such subcategorizational feature specifications as abstract, concrete, inanimate, and mass, which have been set up by linguists such as Chomsky and others on independent grounds are unsatisfactory, in view of the acceptability of *a strong table* and *powerful whisky* as collocations and the questionable acceptability of *a strong device* (pp. 150–151).

But even if such subcategorization could be provided, there would be, according to Halliday, needless duplication in the grammar if matters were handled grammatically. The same collocational potentials as hold between *argument* and *strong* also hold between *argue* and *strongly*, *argument* and *strength*, and *argument* and *strengthen*. Since *strong*, *strongly*, *strength*, and *strengthen* all belong to different grammatical classes (adjective, adverb, noun, and verb), they will have to be recognized as different items, but with the same collocational potential. By setting up a lexical level, and by regarding items such as *strong*, *strongly*, *strength*, and *strengthen* as the same (lexical) item, then the collocational potentials need only be stated once. Moreover, and to Halliday this is the clincher, a collocational fact may be exhibited in a discourse without the elements of the collocation entering into a syntactic relationship at all, as in the sentences: *I wasn't altogether convinced of his argument. He had some strong points but they could all be met* (p. 151).

Halliday's arguments, however, do not go through as soon as one adopts a position concerning linguistic analysis similar to that of McCawley's. Halliday suggests that the kind of subcategorization found in Chomsky (1965) is insufficient to make the kinds of distinctions that are necessary at the putative lexical level, but his distinctions are in turn insufficiently delicate for purposes of incorporation into deep structure.[16] For one thing, at least two senses of the adjective *strong* must be distinguished, and hence two separate lexical items $strong_1$ and $strong_2$ must be set up; the first roughly the same in meaning as *sturdy*, and the second roughly the same as *concentrated*. The collocation *strong table* involves the use of $strong_1$, while *strong tea* that of $strong_2$. This distinction needs to be made if only to account for the relative semantic oddity of *This table is too strong* compared to *This tea is too strong*. Also note that $strong_2$ can only be predicated of material in a medium perceived by the senses of smell, taste, or sight; we speak of $strong_2$ *odors*, $strong_2$ *tea*, and $strong_2$ *light*, but not of *$strong_2$ *noises*, for example.

[16] The notion 'delicacy' actually cannot play any role in linguistic theory. All distinctions, no matter how delicate, which are made semantically must find separate representations in any descriptively adequate deep structure.

Strong$_1$, on the other hand, is predicated of material or abstract objects, particularly those objects which are capable of standing up under use or attack, for example a *strong*$_1$ *table*, *a strong*$_1$ *fort*, *a strong*$_1$ *argument*, *a strong*$_1$ *chain*, or *strong*$_1$ *health*. If *strong*$_1$ is predicated of an object, it is simultaneously predicated of its parts, thus *a strong*$_1$ *table* has *strong*$_1$ *legs*, *a strong*$_1$ *top*, etc.; *a strong*$_1$ *argument* has *strong*$_1$ *points*; and *a strong*$_1$ *chain* has *strong*$_1$ *links* (whence the source of a well-known maxim).

Two senses of *powerful* must also be distinguished: *powerful*$_1$ meaning *having considerable power*, and *powerful*$_2$ meaning *overwhelming*; thus *a powerful*$_1$ *car* is not like *a powerful*$_2$ *whisky*. Presumably *powerful*$_1$ is syntactically related to *power*, whereas *powerful*$_2$ is not (perhaps it is related to *overpower*).

Having made these distinctions (others presumably will have to be made, especially in the case of *strong*), we now indicate how they are to be expressed in linguistic terms. We say that all the semantic information contained in any predicate (verb or adjective – also apparently noun, according to the very convincing arguments of Bach (forthcoming)) is syntagmatic; we say that predicates impose interpretations on their arguments (subjects or objects). For example, *strong*$_2$ imposes on its subject that it be material conveyed by one of three senses having a relatively high degree of concentration, while *strong*$_1$ imposes on its subject that it be a physical or abstract entity fit to carry out a function and withstand damage or attack. If *strong*$_1$ is predicated of a subject which is already specified as meaning something different from what *strong*$_1$ imposes on it, then a contradiction ensues; following the terminology of Chomsky (1965), we say that a selectional restriction has been violated. Certain predicates may be highly specific concerning the range of arguments which can occur with them; to take well-known examples, *bark* selects subjects which are capable of making noises comparable to those characteristic of dogs, and *rancid* selects only subjects designating certain foodstuffs made from animal or vegetable fat.

The fact that derived forms of predicates enter into the same semantic relationships with arguments as the predicates themselves (recall Halliday's examples involving *strongly*, *strength*, *strengthen*) simply means that grammatical transformations apply to create these forms out of the underlying predicates. Halliday is right to group *strong* and its derived forms together, but he is wrong to call them all the "same items" (p. 151). They are in fact all different items which probably require separate listing in the lexicon of English,[17] but they are permitted to occur in sentences only if particular transformational rules have or have not applied (for general discussion of

[17] Saying that there is a lexicon is not, of course, saying that there is a lexical level of representation of a sentence.

this point, see Langendoen (forthcoming, Chapters 6 and 7); for detailed discussion of how forms such as *strengthen* are introduced into sentences see Lakoff (1965)). Notice in particular that a derived form need not have the same collocational potential as its base form; some interesting examples are presented by T. F. Mitchell, 'Some English Phrasal Types', p. 337 in the current volume. He points out, for example, that *heavy drinker* is not paralleled by **heavily drunk* or **heavy drink* (the latter collocation is of course possible with a different sense of *heavy*). Finally, Halliday's point that two items may enter into a collocation relation without entering into any apparent syntactic relation can be answered by observing that his point only holds for surface structure. In deep structure representations significant collocational relations will always appear as well-defined syntactic relations among bundles of semantic markers; this in fact is what defines a significant collocational relation.

This concludes our consideration of the attempt of Halliday and Sinclair to justify an autonomous lexical level of linguistic representation. As a parting shot at Sinclair, we may answer his complaint (reminiscent of Austin (1962)) that "grammar is hardly any help at all" (p. 425) in making distinctions among lexical items, by saying that he has not examined grammar deeply enough. Unfortunately, space limitations preclude our giving similar detailed consideration to the many other interesting contextual studies in this volume. T. F. Mitchell's article, for example, is packed with an enormous quantity of intriguing facts and formulations about English, plus a noteworthy challenge to generative grammarians:

> One cannot help but be puzzled by the refusal of American transformational-generative grammarians to incorporate in their valuable work collocational study of the kind envisaged here... (p. 354).

Angus McIntosh has supplied a very valuable study of the use of *will* and *be going to* as expressions of prediction in his article 'Predictive Statements', pp. 303–320. Raymond Firth has provided an autobiographical sketch of his experience as a linguistically oriented ethnographer in 'The Meaning of *Pali* in Tikopia', pp. 96–115, while G. B. Milner, 'Hypostatization', pp. 321–334, has taken up problems of meaning comparable to those raised by R. Firth in a more general fashion. Jeffrey Ellis, 'On Contextual Meaning', pp. 79–95 is a not-very-helpful attempt to expound some Firthian and neo-Firthian ideas about context. The remaining articles in this group deal with specific problems in specific languages: J. E. Buse, 'Number in Rarotongan Maori', pp. 52–65; Marjorie Daunt, 'Some Modes of Anglo-Saxon Meaning', pp. 66–78; Braj B. Kachru, 'Indian English: A Study in Contextualization', pp. 255–287; Randolph Quirk and David Crystal, 'On Scales of

Contrast in Connected English Speech', pp. 359–369; Natalie Waterson, 'Numeratives in Uzbek: A Study in Colligation and Collocation', pp. 454–474; and E.M. Whitley, 'Contextual Analysis and Swift's Little Language of the *Journal to Stella*', pp. 475–500.

4. HISTORY OF LINGUISTICS

The three articles in the collection devoted to problems and issues in the history of linguistics are G.L. Bursill-Hall, 'Notes on the Semantics of Linguistic Description', pp. 40–51; Roman Jakobson, 'Henry Sweet's Paths toward Phonemics', pp. 242–254; and Vivian Salmon, 'Language Planning in the Seventeenth Century; Its Context and Aims', pp. 370–397.

Bursill-Hall's paper is little more than an inventory of the technical terms of the Modistae grammarians; useful, but not particularly stimulating. Salmon's investigation of the flowering of linguistic inquiry in Great Britain in the seventeenth century, culminating in the great work of Lodowick, Dolgarno, and Bishop Wilkins, is mainly concerned to disprove the thesis advanced by DeMott (1955, 1958) that the fundamental inspiration came from the work of Comenius. In light of the great interest now being shown in 16th- and 17th-century linguistic thought (see Chomsky (1966), Lakoff (1966) for example), Salmon's article is quite timely.

Jakobson's investigation of the work of Henry Sweet is one of the highlights of the collection under review. Jakobson's major thesis is an extension of one advanced earlier by Firth (1934),

> ...that the theoretical foundations of phonemics are implicit in Sweet's exposition of the principles which underlie both the *Broad Romic* and the representation of speech sounds in a rational spelling (p. 245).

He supplements this thesis with other interesting observations concerning Sweet's use of *Broad Romic*, for example, that stress need not be marked on the first syllable of a word in a particular language if the majority of words in that language have initial stress, and that "binary oppositions requiring a symbol only for the mark but not for its absence were clearly viewed by Sweet" (p. 248).

5. W. HAAS, 'LINGUISTIC RELEVANCE', PP. 116–147

Haas is justly well-known for his articles which range far and wide over the domain of general linguistics (for example, Haas (1954, 1957)); the present article is another in that tradition. The article opens with a consideration of the aims of linguistic analysis. Interestingly, Haas cites as "fundamentally

the most puzzling characteristic of language", "its enormous productivity – that obvious ability we have... of saying what has never been said before, and understanding what we have never heard before. To explain how this is possible, in the root-problem of linguistic analysis" (pp. 116–117). Despite this acknowledgment of the truth of a position toward linguistic data reintroduced into general linguistics by generative grammarians, however, Haas is critical of the general aims of such grammarians. He is under the impression that they are advocating the complete elimination of methodology in favor of the formalization and logical evaluation of "various intuitive statements about language" (p. 119). Haas asks:

> Would anybody accept a 'generative meteorology' to replace the present search for effective methods and theories in this particular field? Would it not be ludicrous to suggest that a rigorous logical theory, *not* about the weather but about the very imperfect understanding we have of it; i.e. a theory about any such *statements* as might be derived from sundry methodological hints, hunches and guesses about the weather, could qualify as meteorological theory? (p. 119).

Quite obviously, Haas is laboring under a fundamental confusion concerning the place of native speaker intuitions for a generative grammarian. Such a grammarian would agree with Haas "that the objects of linguistic analysis are not just physical objects, not arbitrarily selected 'stretches of speech'" (p. 118). But if the objects of linguistic analysis are to be anything more than that, they *must* be native speaker intuitions about what counts as significant speech, and more importantly as grammatical speech. Native speaker intuitions, then, make up the *entirety* of the data available to linguists; the use therefore of such intuitions is not to replace rigorous methodology, but the "corpus" of grammarians such as Harris (1951). Sound methodology is just as important to a generative grammarian as to any other kind – any impression one may receive to the contrary stems from the fact that generative grammarians typically have not given prime consideration to methodological matters, and the reason for this is that they have not needed particularly sharp tools for finding data to work on. There is more than enough linguistic data which stares you in the face without having to go look for it with refined analytic tools.

The bulk of Haas' essay is devoted to showing that if linguistic analysis is to proceed significantly, one needs more tools than simply segmentation and identification of units; and the placement of units in paradigmatic and syntagmatic contrasts. One needs also to be able to talk about the relationship of units to higher levels, what Haas calls "functional relations" (pp. 124–125). Haas says simply that:

> To give a structural description of linguistic elements is to describe them in terms of those three [sc. paradigmatic, syntagmatic, and functional] relations (p. 125).

Haas then devotes some space to an exposition of the notion functional relation, a notion which he finds missing in the work of many recent linguists (e.g. A.A. Hill (p. 131); Jakobson, Fant, and Halle (p. 146)). If an item has some functional relation to a higher unit, then that higher unit is said to have a functional value; for example *The boys went home* is the value of such functions as *The ...s went home*; *... boys went home*, *The boys ... home*, etc. (pp. 126–127). Any part of the utterance then may play one of two roles, either that of a variant (so *boy* is a variant in *The ...s went home*) or of a constant (as in *The boys ... home*). In the first case, *boy* is said to have diacritical power, and in the second determinant power.

Although Haas uses syntactic examples to illustrate his concepts, he is mainly concerned to exemplify their usefulness for phonology. He points out that Trubetskoy's *Grenzsignale* and most of Firth's prosodies have only determinant power, and the fact that they have little or no diagnostic power does not mean that they should be relegated to the non-significant, non-phonemic, or redundant junkpile (p. 137). An element having determinant power must be recognized as such "for stating regularities in the distribution of lower-rank units entering it as variants," for example the syllable, and it may be needed "for stating regular structures of higher-rank units, into which its own values enter as variants," (p. 135), for example sentence structure is better stated in terms of phrases than in terms of words or morphemes.

In general, we can say that Haas' distinctions are useful, as far as they go, especially for phonology, but that they do not go nearly far enough. Their inadequacy is clearly seen in syntax; it is clear that Haas' general definitions have not utility in telling us what we do not already know about syntax – they are a means of coding the obvious. To a generative grammarian, they also are seen as coding only the superficially obvious; they fail totally in getting at deep structure relations (although given deep structure, they could be then applied, if anyone wanted to set himself the task of applying them). Even in phonology, though they do the service of calling our attention to the function of certain phonologically redundant elements, they do nothing more. This is true, despite the claims made for these notions by Haas in his concluding paragraph:

> To be continually productive of ever new 'acts of speech', a language must admit of individual choice; but for such acts to be shared (by a community and, indeed, by successive periods in an individual's life), choice must be contained within the limits of fixed ranges and patterns. Distinctive value reflects the freedom of choice we have in speaking a language; determinant value reflects the constraints to which we submit. It is the task of Linguistic Analysis to state the extent of that freedom, and the nature of those constraints (p. 143).

I would hold, rather, that a well-developed theory of language, involving

powerful claims concerning the nature of formal and substantive linguistic universals, is required to state the extend of a person's freedom in the use of language, and the nature of the constraints under which he must operate.

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