put us in their debt for a great deal of linguistic information about what was formerly the Indian Empire. D. L. R. Lorimer was one of these, and there will be no more from his pen.

Studies in linguistic analysis. [With an introduction by J. R. FIRTH.] (Special volume of the Philological Society.) Pp. vii, 205, with 5 plates. Oxford: Basil Blackwell, 1957.

Reviewed by D. TERENCE LANGENDOEN, *Massachusetts Institute of Technology*

This volume of collected papers is intended to provide a body of linguistic analyses of diverse languages using the 'general principles and methods' developed by J. R. Firth and his colleagues in Great Britain over the previous twenty-five years. The introductory article by Firth, 'A synopsis of linguistic theory, 1930–1955' (1–32), is intended as a summary of those general principles which he himself had developed during that period. The volume takes on greater significance in view of Firth's death in 1960, since it stands as the last body of linguistic work written under his direct personal influence, his article being among the last of his own publications. Recent linguistic work in Great Britain is already distinguishably different from the work summarized in this volume, and the term 'neo-Firthian linguistics' may be suggested to characterize the recent work of M. A. K. Halliday, R. M. Dixon, and others.*

In this review, I shall focus most of my attention on the articles by Firth (1–32), Allen (68–86), and Robins (87–104). Of the remaining papers, the one by Haas (33–53) is not at all concerned with exemplifying Firth's 'general principles', but with a criticism of the use of the term 'zero' in American linguistic literature. His conclusions are disregarded by at least one of the other contributors to the volume (Mitchell, 185), and I shall not consider Haas's paper further. All the remaining papers, except for Halliday's (54–67), deal with various phonological problems. To these papers I shall make only passing reference.[1]

As Firth himself acknowledged in a number of places, his understanding of the notion 'language' is a development of the notion held by the anthropologist Malinowski.[2] Malinowski held that language must be studied as a mode of human activity, as an activity directly interrelated with other bodily activities, and not abstractly as an entity of its own which functions, in Sapir's terms, as a 'method

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[1] The full contents of the volume follow: 'Introduction' (v); J. R. Firth, 'A synopsis of linguistic theory' (1); W. Haas, 'Zero in linguistic description' (33); M. A. K. Halliday, 'Some aspects of systematic description and comparison in grammatical analysis' (54); W. S. Allen, 'Aspiration in the Hāṛautī nominal' (68); R. H. Robins, 'Vowel nasality in Sundanese: A phonological and grammatical study' (87); R. K. Sprigg, 'Junction in spoken Burmese' (104); F. R. Palmer, 'Gemination in Tigrinya' (139); J. Carnochan, 'Gemination in Hausa' (149); T. F. Mitchell, 'Long consonants in phonology and phonetics' (182).

[2] R. H. Robins, 'General linguistics in Great Britain, 1930–1960' in C. Mohrmann, F. Norman, and A. Sommerfelt (eds.), *Trends in modern linguistics* 11–37 (Utrecht, 1963).

of communicating ideas, emotions and desires by means of a system of voluntarily produced symbols'.[3] Firth's claim that 'the object of linguistic analysis as here understood is to make statements of meaning so that we may see how we use language to live' (23) is seen to be merely a restatement of Malinowski's position, as soon as we understand that 'living' for Firth meant being able to make coordinated bodily movements, including speech.

The salient feature in Firth's definition of the objective of linguistic analysis is his insistence that language be studied so as to reveal how utterances of that language receive semantic interpretation. Again, following Malinowski,[4] Firth maintained that it is not really languages as such that the linguist studies, but utterances of languages which are appropriately recorded together with their contexts. The meaning of such texts is only seen as the relation between the texts and their contexts; the contexts being the physical situation, the behavior of the participants, and other such 'observable' phenomena. Thus, like Bloomfield, Firth maintained what may be called a behavioristic theory of meaning; he differed from Bloomfield in the emphasis with which he asserted that the study of meaning is central to linguistics.

The notion that the objects of linguistic study are not languages as such, but rather utterances in context, leads Firth to the bizarre position that the same utterance, spoken in two different situational contexts, constitutes two different linguistic entities. While in fact he never explicitly voiced this claim, he did at one point express it epigrammatically:[5] 'The use of the word "meaning" is subject to the general rule that each word when used in a new context is a new word.' It is clear however that in his actual linguistic work, Firth made use of the identifiability of words apart from situational context, and never actually raised the question of how he was able to identify two occurrences as the same word.

Firth claimed that the situational context provides only a part of the semantic interpretation of a sentence; its meaning is partially determined by its grammatical and phonological form, and also by the cooccurrence potentialities of the words which appear in it. This last aspect of Firth's semantic theory, the notion that the cooccurrence potentials of words in phrases is a dimension of the meaning of these words, is an especially noteworthy part of the theory; it generally goes by the name 'meaning by collocation'. I shall have more to say on all of these aspects of his semantic theory; but first a comment is in order concerning Firth's behavioristic assumptions.

In claiming that language must be studied as a mode of human bodily activity, and not as a phenomenon which reflects mental activity and human volition, Malinowski saw himself maintaining a position outside the mainstream of linguistics as represented, say, by Sweet, Sapir, and Jespersen.[6] In adopting essentially the same position, Firth saw even further that it was a position irreconcil-

[3] E. Sapir, *Language* 7 (New York, 1921); B. Malinowski, *Coral gardens and their magic* 2.59 (New York, 1935).

[4] Cf. *Coral gardens* 11: 'To us the real linguistic fact is the full utterance in the context of situation.'

[5] J. R. Firth, 'Modes of meaning', *Papers in linguistics, 1934–1951* 190 (Oxford, 1955).

[6] *Coral gardens* 59–60.

able with Saussure's. He rejected all distinction between langue and parole, and signifiant and signifié: 'My own approach to meaning in linguistics has always been independent of such dualisms as mind and body, language and thought, word and idea, *signifiant et signifié*, expression and content. These dichotomies are a quite unnecessary nuisance, and in my opinion should be dropped.'[7]

Returning to Firth's semantic theory, I shall consider first his claim that the meaning of utterances is determined in part by their contexts of situation. Clearly, if this claim is to make any sense at all, two conditions must be satisfied. First, one must have some way of identifying two instances of contexts as repetitions or partial repetitions of one another; and secondly there must be some reasonable upper limit to the number of elements of contexts of situation required for the semantic description of given languages. To discuss the possibility of meeting the second condition, it is first necessary to meet the first. This fact has long been recognized in phonology; one must have some criterion for determining when two utterances are partial or complete repetitions before one can discuss what the inventory of phonological entities is for a particular language. But Firth and his colleagues have not even attempted to satisfy the first condition for contexts of situations, and hence are to this day in no position to discuss even single instances of such contexts, much less to present a full account of their relevance to semantics. Consequently, Firth was being seriously misleading when he asserted that 'no linguist has yet set up EXHAUSTIVE systems of contexts of situation such that they could be considered mutually determined in function and meaning' (9; my emphasis). He had not even shown how a linguist can isolate a SINGLE element of a context of situation.[8]

Even if we grant that the first condition might be met, we find that Firth is still in no position to meet the second. To be able to place some sort of reasonable bound on the number and type of elements of contexts of situation one would need some sort of criterion of relevance, which would enable one to include certain components and exclude others. Granted that Firth realized the need for such a criterion, he made no attempt to supply one (29–30):

> In dealing with ... texts abstracted from the matrix of experience most of the environmental accompaniment in the mush of general goings-on must of necessity be suppressed. Nevertheless the linguist must use his nest [sic; net?] to catch and retain on his agenda such selected features or elements of the cultural matrix of the texts as may enable formal contexts of situation to be set up within which interior relations are recognized and stated.

One need only ask, on what principles is such a net to be constructed? to realize how far short Firth actually fell in attempting to solve this problem.

Firth's notion of context of situation thus cannot even be considered a subject for study.[9] But if somehow these problems were solved, and it were possible to

[7] J. R. Firth, 'General linguistics and descriptive grammar', *Papers* 227.

[8] Firth's claim that 'the context of situation in the present theory is a schematic construct for application to typical "repetitive events" in the social process' (8) can be interpreted as a realization on his part of the need to satisfy the first condition. Needless to say, realizing a problem is not solving it.

[9] The notion 'context of situation' is usually meant to include what is called context of culture—that is to say, the observable components of the culture which bear on the situa-

study contexts of situation, it is clear that since utterances generally are meaningful apart from context, it would make no sense to say that contexts of situation provide their meaning. It would probably be found that a context of situation generally had nothing at all to do with the semantic interpretation of sentences uttered in it. What relevance could the context of situation have for supplying the semantic interpretation of the following sentence, considering it to have been spoken by one man to another during their mutual coffee-break at half past ten in the morning: *I really believe that seat belts should be required in every new car sold on the market today*?

Yet both Malinowski and Firth sincerely believed that the study of contexts of situation would prove to be a powerful tool in semantic analysis; in particular, that it would enable us to understand how potentially ambiguous utterances are unambiguously understood in certain contexts. For example, Malinowski observed that a particular sentence in the Kiriwinian language of the Trobriand Islands was potentially ambiguous, but was unambiguously understood in the narrative in which it occurred. Malinowski attributed this to the cultural context in which the sentence was uttered, and observed,[10] 'Our ethnographic knowledge ... enabled us to solve this ambiguity.' Firth claimed at one point that the English sentence *I want every comfort* must, in certain contexts, be interpreted as *I lack every comfort*, whereas in others as *I desire every comfort*.[11] What he failed to show was HOW the context of situation in each case could be used to disambiguate the sentence.

At this point one may naturally ask how one knows in advance that these Kiriwinian and English sentences are, in fact, ambiguous. Malinowski had no answer; it had apparently never occurred to him as requiring explanation. Neither had Firth, although he might have answered that this advance knowledge is supplied by the other 'modes of meaning'. But it is precisely THIS information about sentences, their interpretation apart from context, that is the subject

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tion. This, of course, is no more a subject which can be studied than context of situation, and is, if anything, less well worked out than the latter. Yet Dixon, in his recent book *Linguistic science and logic* 101 ('s-Gravenhage, 1963), gratuitously assumes an analysis of the context of 'British Culture' already worked out to help supply a semantic interpretation for a particular conversation.

A fact which has apparently escaped the attention of Firthians is that the components of the context of situation and more particularly of the context of culture are generally not observable in their sense of the word. If one is talking to one's maternal uncle, in what sense is he observably one's maternal uncle? A theory of culture, like a linguistic theory, must be concerned not with making catalogs of 'observables' neatly arranged in tables, but rather with characterizing the representation of culture in the minds of individuals, and especially with providing an account of how individuals growing up in a particular society learn its cultural pattern and what is expected of them within it. Such a theory will be required to answer the questions: What are cultural universals? What is the form of social structure? How do children acculturate, or internalize a theory of their own culture? Once such a theory is proposed and particular cultures are described in terms of it, it will be an interesting problem to investigate relations between the semantics of the language(s) of such cultures and the cultures themselves.

[10] *Coral gardens* 42.

[11] 'Modes of meaning', *Papers* 205.

matter of semantics. The knowledge about the context or about the world in general that people use to disambiguate sentences, and the way in which they use this knowledge to perform the task, may ultimately be accessible questions for study, but are obviously beyond our present powers of inquiry.

It is worth noting that Malinowski and Firth often presented the problem of relating utterances and context in exactly the reverse fashion of their usual presentation. Both of them observed, for example, that given some utterance, it is generally possible to state contexts in which the utterances are permitted to occur. Thus Firth asserted,[12] 'If I give you one brief sentence with the information that it represents a typical Cockney event, you may even be able to provide a typical context of situation in which it would be the verbal action of one of the participants.' Malinowski still more bluntly attempted to 'recreate' contexts of situation for texts in his corpus, given no prior information about those contexts:[13] 'Take, for example, the second text in our collection ... Let us see ... whether this text can naturally be placed within some normal context of native life.' Neither Malinowski nor Firth happened to notice that this ability of humans to provide contexts of situation for utterances ad lib is an argument against, rather than for, the assertion that contexts are needed to provide their semantic interpretation.

It is amusing to observe that some of Firth's followers have, in their purely linguistic descriptions, felt obliged to supply hypothetical contexts of situation for certain of their citations in various languages, merely to satisfy the 'scientific need' of supplying them, when it was only the citations themselves that were of interest. One instance is provided by Carnochan's paper, where, in discussing the conjugation of a Hausa verb, he invents a dialog in which sentences with various forms of the verb are uttered in sequence. The dialog contains only three conjugated forms, whereas the complete conjugation has twenty-four forms; but rather than give dialogs containing them all, the author remarks,[14] 'This one instance of context of situation, in which extended collocations for the contexts of three forms of Aspect I have been given, appears sufficient without repeating the process for all twenty-four examples, as this would have little bearing on the present study.'

Other aspects of Firth's semantic theory are his notions of grammatical, phonological, and phonetic meaning, and of meaning by collocation. Although Firth was notoriously inexplicit about what he meant by 'grammatical meaning' (8), it seems that, if made explicit, the concept would roughly correspond to Bloomfield's notion of the episememe. In another discussion, Firth noted that the meaning associated with grammatical categories differs from one language to another, and he argued from this premise directly to the conclusion that no two categories in two different languages are the same. Their identification by name (e.g. 'noun', 'verb', 'particle') was for him a kind of convenience (21–2). One might consider this argument against universal grammar seriously if Firth had been explicit about what he meant by the meaning of grammatical categories.

[12] 'Personality and language in society', *Papers* 182.

[13] *Coral gardens* 49.

[14] J. Carnochan, 'Glottalization in Hausa', *TPS* 1952.83.

But the conclusion does not follow from the premise at all; there is no reason a priori why one cannot make a substantive identification of grammatical categories between two languages, and account for their meaning difference (as well as their syntactic difference in function) in terms of the rules of the two linguistic descriptions which interpret them semantically, which expand them into other constituents, and which rearrange them with respect to each other. Indeed, unless such substantive identifications are made, and a theory of universal or general grammar is developed, we shall be in no position to account for the human ability to acquire languages.[15]

A number of Firth's colleagues and students have repeated this antiuniversal argument in slightly different forms. Not much different from Firth's is the argument put forward by Halliday (57):

> The 'structural' linguist, while attempting to develop descriptive methods that are general, ... will be unwilling to claim universality for any formally established category; since, while, for example, it may be convenient in the description of all languages so far studied to give the name 'verb' to one class of one unit, this is not a universal statement: the 'verb' is redefined in the description of each language.

Robins has argued that the existence of universal grammatical categories depends upon our having universal methodological criteria for classifying taxonomically the forms of an utterance into such categories.[16] As Robins notes, it is impossible to have such a taxonomic methodology, and so he concludes that there are no universal categories. Obviously, again, this conclusion does not follow from the premise. Robins does have the feeling, however, that the identification of the categories 'noun' and 'verb' in different languages is more than a mere convenience; in fact, that it is the case that 'grammatical analysis in terms of a basic distinction of nominal and verbal categories succeeds in new fields and stands critical examination in the older areas of language study where so much else of traditional grammar has had to be abandoned.'[17] Even so, Robins does not see this as evidence for the universal character of the categories 'noun' and 'verb', but merely as evidence that 'the experience of different human societies is similarly ordered'.[18]

Firth's argument against universals of phonology, and in favor of giving ad-hoc treatment to each new language, followed the same pattern. As I shall argue in greater detail below, what led him to this extreme position was his failure to realize that the phonological component of the grammars of languages contains rules which operate upon universally defined categories (so that the realization of the same category in two different languages will be SYSTEMATICALLY different).

I omit consideration of Firth's ideas of phonological and phonetic modes of meaning, where either he had little of interest to say, or what he said was absurd in terms of any known sense of the word 'meaning', e.g. 'it is part of the meaning

[15] N. Chomsky, 'Explanatory models in linguistics', *Logic, methodology and philosophy of science: Proceedings of the 1960 international congress* 529 ff. (ed. by E. Nagel, P. Suppes, and A. Tarski; Palo Alto, 1962).

[16] R. H. Robins, 'Noun and verb in universal grammar', *Lg.* 28.289–98 (1952).

[17] Ibid. 296.

[18] Ibid. 297.

of an American to sound like one'.[19] His notion of meaning by collocation, however, deserves careful attention. Firth viewed this notion as an attempt to formalize within semantic theory Wittgenstein's thesis that 'the meaning of words lies in their use'.[20] Roughly speaking, the notion means that if a word typically appears in the environment of other words, that word takes on as part of its meaning its ability to appear in such environments, and carries this meaning with it, even when it happens to appear in an atypical environment. Thus,[21] 'one of the meanings of *night* is its collocability with *dark*, and of *dark*, of course, collocation with *night*.' It should be immediately apparent that this notion makes sense only if some criterion is given for being 'typical'; otherwise, the observation that a word potentially appears in infinitely many environments renders the idea hopeless. It is not possible to define the criterion in terms of probability of occurrence in such-and-such environments, because, for all practical purposes, the probability is zero for any word in any environment.

The trouble with the attempt to assign meaning to the collocability of words in phrases is that it attempts to solve the problem of assigning meaning to phrases backwards. Firth in effect claimed that, given the phrase *dark night* with some determinate meaning, we can say something about the meaning of each word. Rather, the proper solution of the problem would be to claim that the lexical items *dark* and *night* each have determinate meanings, and that the meaning of the phrase is determined by a rule which amalgamates the meanings of the individual words. In this way, we are able to speak of a general rule which amalgamates the meanings of nouns and adjectives to give the meaning of noun phrases.[22] Notice that Firth's claim does not enable him to take advantage of the regularities of the language, such as the grammatical composition of noun phrases; he is obliged to state meanings by collocation ad hoc for each collocation. Furthermore, he is burdened with excessive redundancy, even for those meanings which he may be able to state. In the example above, he must give essentially the same information twice. For more complicated collocations involving a greater number of lexical items, such redundancy becomes excessive.

It is perhaps possible to interpret Firth's view about meaning by collocation not as a serious proposal about semantics, but as a guide to stylistic analysis. Ordinary collocations may be considered to be stylistically neutral (clichés), while striking ones are stylistically prominent. This is the interpretation suggested, in fact, by Robins;[23] and indeed the examples from English chosen by Firth to illustrate the principle are typically clichés. Apparently one of his favorite examples was *You silly ass*, which he used in a number of places.[24] It must be noted, however, that Firth fails to make a distinction between clichés and idioms, where the latter are lexical items (having therefore a dictionary definition) composed of smaller lexical items. The distinction between them is

[19] 'Modes of meaning', *Papers* 192.

[20] It is not at all clear, however, that by 'use' Wittgenstein meant what Firth means, namely occurrence in a linguistic context.

[21] 'Modes of meaning', *Papers* 196.

[22] J. Katz and J. Fodor, 'The structure of a semantic theory', *Lg.* 39.170–210 (1963).

[23] 'General linguistics in Great Britain' 24.

[24] 11; 'Modes of meaning', *Papers* 195.

crucial for semantics; and Firth's failure to make it reveals a further weakness of his position.

This concludes my discussion of Firth's claims about semantics. He has failed to give an adequate explication of semantics at any of his 'levels' of semantic interpretation. Indeed, all of his suggestions for undertaking actual semantic inquiry have remained entirely speculative and programmatic. For example, although the claim has been persistently made that one of the modes of meaning is given by the context, it is notoriously true that no one has even yet given the semantic interpretation of a single English sentence in terms of contexts of situation. That a theoretical claim should have maintained itself for thirty years without bearing or showing any sign of bearing visible fruit is nothing short of astounding.

I can best approach Firth's ideas about grammatical and phonological analysis by considering in some detail his brief statement concerning the proper 'form' of a linguistic description (5), leaving out that part of the sketch which has to do with stating the relation between text and context, and the interior relations of the context.

Like essentially all other linguists, Firth proposed that a linguistic description contain a grammatical and a phonological part, but Firth's interpreters have been somewhat at a loss to understand whether these two components are to be arranged hierarchically or independently.[25] As we shall see below, the tacit assumption among Firth's colleagues, including Robins, has been usually that it is hierarchical. For both the grammatical and the phonological description, Firth maintained that an essential distinction must be made between what he called syntagmatic relations and paradigmatic relations. These terms are distinctly Saussurean, as Robins noted;[26] it is worthwhile considering whether the idea behind them corresponds substantively with Saussure's.

Firth's notion of the nature of grammatical syntagmatic relation was almost identical to Saussure's 'rapport syntagmatique'; for Firth, it amounted to the claim that elements of grammatical structure, or grammatical categories, have certain 'mutual expectancies'. In fact, Firth felt that these relations of mutual expectancy, which he called 'colligations', are in all respects parallel with (though not to be confused with) word collocations. The grammatical structure of each utterance or text, he felt, can be presented as an array of the categories appearing in it, the actual elements of the text being realizations (in Firthian terminology, 'exponents') of the categories. The only substantive difference between Firth's view and Saussure's is Firth's claim that the array of categories need not be linear or sequential. This difference seems to be crucial. It is clear that if an explicit grammatical theory were based on Saussure's notion of 'rapport syntagmatique', it would be a finite-state theory, and hence, immediately, that it would be inadequate as a linguistic model.[27] Firth's denial of Saussure's simplistic linear model therefore amounts to a rejection of a particular model that is

[25] Cf. 'General linguistics in Great Britain' 22–3.

[26] Ibid. 19.

[27] Cf. N. Chomsky, *Syntactic structures* 18 ff. ('s-Gravenhage, 1957).

linguistically inadequate. The natural question is, did Firth propose a linguistically adequate model?

One might have guessed that Firth, following the developments in the United States, would hit upon some sort of constituent-structure model; but if he did, he nowhere proposed it in writing. The most that he had to say about the structure of a grammatical model was this (17): 'elements of structure ... share a mutual expectancy in an *order* which is not merely a *sequence*', and '[grammatical] categories are not considered as having *positions* in *sequence*, but can be said to be placed in order.' From such remarks, one is unable to conclude anything. Fortunately, we are able to obtain a clue as to the form of linguistic description he actually had in mind by realizing that Firth proposed essentially the same syntagmatic-paradigmatic model for phonology, and here his ideas were more clearly formulated.

Phonological structure, for Firth, was composed of a linear sequence of segmental or 'phonematic' units, and a superimposed sequence (or, if you will, a parallel sequence) of 'prosodic' units, which have the characteristic property of distributing themselves over long ranges of phonematic material; but beyond the observation that prosodic units have a characteristic 'domain' or 'focus' in the phonematic sequence, no location can be found for them in that sequence. Each sequence, phonematic and prosodic, is itself made up of 'systems' and 'structures'; that is to say, of paradigmatic and syntagmatic relations.[28] Returning to Firth's proposed grammatical model, we can see that when Firth claimed that grammatical categories follow an order which is not a linear sequence, what he meant is that they are arrayed along two such sequences, one corresponding to the phonematic and the other to the prosodic sequence in phonology. Grammatical categories and features with 'discontinuous and cumulative exponents' (21) belong to the second category. It is difficult to talk about the 'generative capacity' of such a grammatical model precisely because of Firth's failure to be explicit, but if my reconstruction and interpretation of it is correct, it appears that Firth failed to get beyond a model with the generative capacity of a finite-state grammar.[29]

There is one tantalizing reference in all of Firth's work which indicates that he recognized the need of a transformational rule in English grammar (13):

> In order to state the facts of negation in contemporary English, it is necessary to set up a class of ... syntactical operators which function not only in negation but also in interrogation with front-shifting of the first nominal element of the verbal phrase.

[28] This description of Firth's views on phonology differs in one important sense from that given by Robins in 'General linguistics in Great Britain', where he associates phonematic units with paradigmatic relations and prosodic units with syntagmatic relations. While it may seem imprudent to quarrel with someone like Robins over the correct interpretation of Firth, he seems to be clearly in error on this point. Allen gives essentially my interpretation in his 'Retroflexion in Sanskrit: Prosodic technique and its relevance to comparative statement', *BSOAS* 16.556 (1954).

[29] This did not prevent other Firthians from producing grammatical descriptions essentially in terms of phrase structure. Robins's treatment of Sundanese grammar in 'Formal divisions in Sundanese', *TPS* 109–42 (1953), recognizes a hierarchy among the constituents phrase, clause, and sentence. Halliday works with a similar hierarchical system in 'Categories of a theory of grammar', *Word* 17.241–92 (1961).

Whether he would have claimed that interrogative sentences are derived from declarative ones by means of this device of 'front-shifting' is never clear. Furthermore, he nowhere told how the notion 'syntactic operator' fits into his framework, and what in general the properties of such operators are. His statement is in fact merely a way of saying what can be found in any traditional English grammar.

Firth's notion of paradigmatic relation, however, bears no close resemblance to Saussure's 'rapport associatif', which was merely the relation between words which enable them to be mentally associated. For Firth, a paradigmatic relation was one that holds between the elements which act as exponents in some syntagmatic structure; put more directly, these elements are identified as the 'fillers' of the 'slots' provided by the grammatical and phonological categories. The similarity of Firthian linguistics to American slot-and-filler grammatical description, notably tagmemics, has already been noted;[30] we see here that, in contemporary British linguistics, slot-and-filler description arises with Firth, not only for grammar but also for phonology.

Firth, like many others before and after him, noticed that the class of elements which fills one position (say a vowel position) may only partially resemble the class which fills another (vowel) position. A language may have a word structure exemplified by the phonological formula CV^5CV^7, where the exponents indicate the number of vowels which appear in the given position. In the first position, we may find the vowels *a*, *e*, *i*, *o*, *u*; in the second, *a*, *e*, *i*, *o*, *u*, *ö*, *ü*. While the 'same' five vowels appear in the second position, they bear, he claimed, a different relationship to one another, because there they are also opposed to the vowels *ö* and *ü*, whereas in the first position they are not. For this reason, Firth was led to the extraordinary conclusion that the five vowels in the first position are different phonological entities from the corresponding ones in the second position, and the phonological description is required to mark that difference. The literature provides a number of exemplifications of this position. Carnochan, writing about Hausa phonology, concludes that since five vowels potentially occur in verb stems and only three in verb affixes, the vowels in the two positions must be phonologically different. This in spite of the phonetic fact that vowels sound alike in the two positions:[31]

> ... in a stem a five term vowel system operates. In the ending a three term system. This does not mean that in listening to Hausa sentences the ear does not perceive similar vowel sounds in verb stems and in verb endings. At the phonetic level there may be similarity of sound in these two places, but this is not sufficient reason for considering these sounds to be necessarily forms of the same phonological unit.

Carnochan therefore proposed to designate the vowels of the stems as *a*, *e*, *i*, *o*, *u*, and those of the endings *y*, *w*, *ə*.

It is immediately apparent that this proposal amounts to the introduction of pointless redundancies into the description. The fact that there is a five-way vowel contrast in Hausa verb roots as opposed to a three-way contrast in affixes

[30] P. Postal, *Constituent structure: A study of contemporary models of syntactic description* 104 ff. (Bloomington, 1964).

[31] 'Glottalization in Hausa' 80.

is sufficiently noted by position; one does not need to create new entities to express the fact. The kind of redundancy which is introduced in this way is not the same as that noted by Postal for slot-and-filler description in general, where redundant slots are created. Carnochan's proposal creates, in addition, redundant fillers for these redundant slots.[32]

One may ask what linguistic (or other) motivation led Firth to propound this position. Apparently the major motivation was to avoid having to use distributional techniques in linguistic analysis. In phonology, specifically, the position arose as a reaction against having to say that phonemes which do not occur in statable positions have 'defective distributions'.

For Firth the problem of having to account for defective distributions does not arise. He himself testified that at one time he had maintained a distributional 'theory', but that he had felt obliged to abandon it, presumably at some time in the early 1940's (18):

> The interior relations of the elements of structure are, however, obscured by certain theories of *distribution*, which I held at one time but have since abandoned.

How is it that distributional analysis 'obscures' one's description? The argument, advanced by Firth and made explicit by Allen (83–5), is that the statement of the distribution of linguistic elements represents an 'amending' of the analysis, and if the analysis needs amending, it must be wrong. Perhaps they had in mind the analogy from astronomy: the need to posit amendments like epicycles to handle the 'facts' of planetary motion within Ptolemaic astronomy was evidence that the theory was wrong. One should have a linguistic theory in which 'the question of distribution does not arise, since one has manufactured nothing that requires it' (83).

It is difficult to understand this argument, since whatever is gained in simplicity of description is lost in the redundancy of the linguistic elements required, and in the statements needed to relate the various structures and systems. It is possible that some Firthian linguists, including Firth himself, missed the fact that these systems and structures even need to be related. Thus the 'simplicity' they attain vis-à-vis distributional analysis is not genuine; they have simply left important matters out of account. Indeed, Firth seemed to glory in this inadequacy, and indicated that it is a property of language itself to be irreducibly multistructural and polysystemic (30):

> Any given or selected restricted language, i.e. the language under description is, from the present point of view multi-structural and polysystemic.

There is at least one instance in the volume under review in which the appeal to linguistic polysystemy has the consequence of preventing the analyst from making an adequate descriptive statement, one which would apparently result in undermining the description given in the article. This is where Palmer insists that if he were to consider 'the entire data of the language' in a single set of statements, he would be obliged either to preserve his analysis intact but recognize

[32] For a grammatical example exemplifying this position, cf. 'General linguistics in Great Britain' 20–1.

three phonologically distinct consonant lengths, or to abandon his analysis in favor of another which he sketches briefly, but which, for a reason that is not altogether clear, he rejects. Since both claims are untenable, he claims instead (147),

> All these complications are avoided if the approach is polysystemic. It is not required that the ... phonological analysis shall be integrated with the analysis of other, unrelated [but also phonological—DTL] data.

Still, it must be admitted that Firth is somehow correct in insisting that there is something different about the vowels *a, e, i, o, u* when they alone occur in a given position, and the same vowels when they occur in a position which also admits the entities *ö* and *ü*. One has missed something if one insists that they are the same 'phonemes'. But to express this difference, it will not do to claim that the vowels are entirely different in the two positions; it is enough to notice that in one position but not in the other, the feature 'rounded' has been neutralized for the front vowels. Then if one claims that the real phonological entities in linguistics are not the phonemes, but the features themselves (rounded, front, high, low, etc.), one is able to express the fact correctly that in each position the vowels *a, o, u* consist of exactly the same combination of phonological (distinctive) features, but that the vowels *e* and *i* differ from each other in the two positions: in one but not in the other they must be classified distinctively as 'unrounded' (where they contrast with *ö* and *ü*).

One further observation about Firthian phonology in comparison with American taxonomic phonology. The former has consistently denied that a given sound is a member of the same phonological class in all the positions in which it occurs, and that a phonological contrast in one position is sufficient evidence for assuming a phonemic contrast everywhere. This claim has permitted some of the more astute Firthian phonologists to arrive at better descriptive analyses than would be otherwise possible within a strictly taxonomic framework. Thus, Carnochan is able to say, correctly it seems, that there is only a three-way vocalic contrast in Hausa verb affixes, even though five vowel qualities are found there phonetically—the same vowel qualities which are manifestations of the five-membered phonemic vowel system of verb stems.[33] The phonetic qualities are predictable on the basis of the three positional 'phonematic units' and certain 'prosodies' which affect them. I will discuss below the general nature of the effect of prosodies; here I simply note that if Carnochan were obliged to treat the five-way vowel contrast as everywhere phonemic, he would have to say that the five vowel qualities appearing in the suffixes are themselves manifestations directly of the five vowel phonemes. His own description, apparently more adequate than this one, would be impossible.

The phonological constraint here rejected by Firth and his colleagues may be called, following Chomsky, 'a particularly strong form of the biuniqueness and local determinacy conditions'.[34] The consequence of abandoning this constraint

[33] Cf. 'Glottalization in Hausa' 85.

[34] N. Chomsky, 'The logical basis of linguistic theory', *Preprints of the Ninth international congress of linguists* 540 (Cambridge, 1962).

is that partial overlapping is permitted; this is clear from Carnochan's analysis. Would Firth have wanted to go further? Would he have rejected the biuniqueness condition altogether, thereby allowing for the possibility of complete overlapping? The only approximation to a discussion of this theoretical issue known to me has been that of Robins; it apparently insists there on maintaining the biuniqueness condition:[35]

> ... when a language or any defined part of a language has been analyzed into sets of component elements, the interrelations of these elements in the structures of which they form part should be so stated and systematized, that from these statements we may be able to carry out the reverse or 'upward' procedure and describe the synthesis of the language, showing how these constituents are successively reintegrated in the utterances of native speakers.

In the body of the same paper, however, he violates the biuniqueness condition; for example, he writes 'phonemically' *pamanna* for phonetic [pamána], where 'phonemic' *pamana* would also be pronounced this way.[36]

There is at least one instance in the literature where complete overlapping is countenanced in a Firthian phonological analysis. In Bendor-Samuel's discussion of Tereno phonology,[37] we find that the homophonous forms [yeno] 'you walked' and [yeno] 'his wife' have different phonological representations, the second being simply *yeno* and the first *Y* + *yono*, where the symbol *Y* represents the second personal singular constituent, and has the property that it may condition certain changes in the first vowel which follows it; in particular it may change *o* to *e*. Bendor-Samuel's analysis is easily justifiable, as it leads to an elegantly simple description of certain aspects of Tereno phonology.[38]

Since Firthian phonology does not necessarily impose the constraints of biuniqueness and invariance upon its description, and since, at least for some practitioners, there is concern for over-all descriptive simplicity, one may ask to what extent this phonological theory resembles generative phonology, where these constraints are also not imposed, and where there is also concern for descriptive simplicity. The answer depends ultimately upon how one interprets the Firthian notions 'prosody' and 'phonematic unit' in generative terms. A prosodic feature has been defined as any feature which 'affects the analysis of more than a single segmental unit'.[39] For my purposes it is therefore enough to say that a prosodic feature is one involved in a context-sensitive phonological rule. Phonematic features are either unaffected by rule or are affected by context-free phonological rules. In making our interpretation we must also, of course, replace the slot-and-filler concept by a theory embodying the neutralization of distinctive-feature contrasts. The prosodic features themselves may be assigned to particular segments; there is no basis for Allen's claim that doing this is an 'indiscriminate co-

[35] 'Formal divisions in Sundanese' 141–2.

[36] Ibid. 136.

[37] J. T. Bendor Samuel, 'Some problems of segmentation in the phonological analysis of Tereno', *Word* 16.348–55 (1960).

[38] Bendor-Samuel's analysis would be considered morphophonemic by taxonomic phonologists.

[39] F. R. Palmer, 'Comparative statement and Ethiopian Semitic', *TPS* 1958.129.

ercion' (86). Firthian phonologists are always careful to provide a 'focus' for their prosodic features, and knowing this focus enables us to determine a natural place for locating a prosodic feature in the sequence of phonological segments.

This is a very natural and straightforward way of interpreting a prosodic statement, and I now proceed to such an interpretation of the statements provided by Robins and Allen in the volume under review, and to evaluate those statements in the light of that interpretation.

Robins is concerned with accounting for the nasalization of vowels in Sundanese, a language of Java, which he claims occurs when the vowel follows a nasal consonant within a word, and no other true consonant or liquid intervenes. We can account for this within generative phonological terms by means of a rule:

$$\text{A.} \quad \begin{bmatrix} + \text{ vocalic} \\ - \text{ consonantal} \end{bmatrix} \rightarrow [+ \text{ nasal}] \text{ in the env. } [+ \text{ nasal}] [- \text{ consonantal}]_0 \text{ ——}$$

where [-consonantal]$_0$ means zero or one or more nonconsonants. Robins, however, calls the feature of nasalization a prosody which has its focus in nasal consonants, and its domain the sequence of vowels which follow it and which appear before another consonant or word-boundary. But it will be seen that Rule A precisely expresses this description, whereas the notation used by Robins, namely the 'structural formulas' by which he states the nasalization of each word ad hoc, obscures this description. His notation is faulty because it fails to reveal clearly that it is a nasal segment which conditions the nasalization of the following vowels, and because the formulas do not distinguish *h*, which does not block nasalization, from true consonants and liquids, which do.

Robins goes on to observe that when the plural constituent *-ar-/-al-* is infixed into the nasalized form of the root,[40] the progressive assimilation of nasalization is blocked by the liquid in this plural constituent only for the vowel that immediately follows it, but picks up again on the second vowel after the infix. Therefore Rule A as it stands is not observationally adequate; we require a modified version of it:

$$\text{A}'. \quad \begin{bmatrix} + \text{ vocalic} \\ - \text{ consonantal} \end{bmatrix} \rightarrow [+ \text{ nasal}] \text{ in the env.}$$

$$[+ \text{ nasal}] (+ \text{ Plural} + [\text{x}]) [- \text{ consonantal}]_0 \text{ ——}$$

To see how Rule A′ works, we apply it to a Sundanese form containing the plural constituent, say *m+ar+oekɣn*. We see that only the vowel *e* is in the environment [+nasal] + Plural + [x] [−consonantal]$_0$—, and hence is nasalized, yielding *m+ar+oẽkɣn*. In addition, the vowel *a* is in the environment [+nasal] [−consonantal]$_0$—, and since that, too, is an environment which permits the application of Rule A′, it is also nasalized, yielding *m+ãr+oẽkɣn*. Neither of the vowels *o* or ɣ appears in the environment specified for the application of Rule A′, so they remain unnasalized, resulting in the pronunciation [mãroẽkɣn].

[40] It is curious that he has not attempted a prosodic description of the alternation *r/l* in this plural constituent, since it is obviously a result of dissimilation.

What we have achieved here is the statement of a rule that correctly and automatically specifies which vowels will be nasalized in Sundanese. The rule itself considers nasality to be a segmental feature, on a par with all the other phonological features of the language, and requires no special marking of the words of the language beyond that which is supplied by the syntactic component of the grammar. It would be superfluous to state specially the 'structural formula' of each word, as Robins is required to do.

Rule A′ is an exemplification of how, generally, one may state grammatical information in a phonological rule. The need to include grammatical information in any adequate phonological description is also noted by Robins (92):

> ... a full analysis of the *n* prosody, a full statement of the patterning of vowel nasality, in Sundanese cannot be satisfactorily made without reference to the grammatical status and structure of certain words characterized by *n*.

The ability to make use of grammatical information in the phonology in this way presupposes a form of grammar in which the grammatical analysis of sentences is provided in order that the phonological component can make use of it. In other words, the grammatical and phonological components of the linguistic description are hierarchically arranged. This proposal about the form of linguistic description is one which is also made in generative phonological theory. What the latter theory contains in addition, however, is an explicit proposal about how, in general, grammatical information about sentences is to be used to determine their pronunciation in any language.

If we now consider an interpretation of the phonological description contained in Allen's paper, we find that the following phonological rules are required:[41]

1. [+voiced] → [−tense] in the env. [x]——

2. $[x] \rightarrow [-\text{tense}]$ in the env. $\left\{\begin{matrix}[+\text{tense}] \\ [-\text{voiced}]\end{matrix}\right\}$ $[x]_0$——

where the feature 'tense' is used to distinguish the aspirate stops from the unaspirate, and where we consider the segment *h* to be phonemically a tense voiced segment, and where a 'nontense' *h* is rendered phonetically as zero. Since ordinary *h* is phonetically voiceless, we need also a rule

3. $\begin{bmatrix} - \text{ vocalic} \\ - \text{ consonantal} \\ + \text{ tense} \end{bmatrix} \rightarrow [-\text{ voiced}]$

[41] Rule 2, in particular, is a rule in which one segment has an effect on segments indefinitely removed in the sequence, provided no word boundary intervenes. Apparently one of the 'motivations' for prosodic analysis is to develop a notation that avoids having to say that a given segment influences others separated from it by arbitrarily many other segments. In 'General linguistics in Great Britain' (30) we find: 'prosodic restatement ... avoid[s] the rather tiresome concept of "action at a distance" of one sound on another in traditional accounts.' It strikes me as odd that phonological rules should be evaluated as 'tiresome' rather than as 'correct' or 'incorrect'. In any event, the formulation of phonological rules like Rule 2 is well motivated for any language that has ever been seriously studied.

Rule 1 says that voiced segments are necessarily unaspirated except initially; Rule 2 says that an aspirate does not follow either an aspirate or an unvoiced segment. Suppose now there is a morpheme *gha* in Hāṛautī, to which may be prefixed the morpheme *ka* without intervening word boundary. Applying the rules in the order given to the form *ka*+*gha*, we get a pronunciation [kaga], since by Rule 1 all word-medial voiced segments are deaspirated. Suppose, however, the pronunciation were actually to be [khaga]. Then Rules 1 and 2 would be insufficient, and we would require an additional rule to apply before Rule 1:

4. [− voiced] → [+ tense] in the env. —— [x]$_0$ [+ tense]

But Rule 4 is a precise formulation of one of the 'historical' rules of Hāṛautī phonology, discussed in Allen's article (86), and it is an interesting question whether the synchronically simplest and correct phonological rules would faithfully reflect the historical development of the language. The question is unanswerable for Hāṛautī from the information about the language given in Allen's paper, because his prosodic analysis fails to state the facts, and the crucial examples that would have to be discussed are not given. Allen has, however, gone on record elsewhere, in articles that appeared both before and after the publication of this paper,[42] as believing that in general this is true. Yet Allen's 'prosodic' description of Hāṛautī phonology is presented in such a way as to make it impossible to investigate the possibility. Strangely enough, he claims, with respect to the historical description of the phonology, that it is 'at once the most concise and least appropriate of all [the phonological descriptions]' (70). But his only argument that it is inappropriate is that it is historically correct! He seems to have missed the point that its very conciseness, so far as it is synchronically correct, is the main argument for believing it to be the most appropriate, i.e. true.

These interpretations of Allen's and Robins' description indicate, above all, that prosodic descriptions miss an essential fact: adequate phonological descriptions of languages take the form of ordered rules which apply to the abstract representations of utterances provided by the syntactical component of the grammar. The consequence of this failure is that such descriptions can adequately handle only phenomena which require no 'descriptive order'; when they attempt to handle phenomena which can be accounted for most simply by appealing to descriptive order, they are at best able to give ad-hoc descriptions of considerably greater complexity.[43] They would certainly be in no position to provide an adequate account of any phonological phenomenon, such as English stress, that requires for adequate description a set of cyclically ordered rules operating in a transformational cycle.[44]

Perhaps a more serious consequence of the failure of the proponents of prosodic analysis to recognize the place of ordered rules in a linguistic description is their claim that the grammatical and phonological features of each language must be

[42] W. S. Allen, 'Relationship in comparative linguistics', *TPS* 1953.85; *Sandhi* 24–5 ('s-Gravenhage, 1962).

[43] These same remarks apply, of course, to American taxonomic phonology.

[44] 'Explanatory models in linguistics' 544 ff.

described ad hoc for each language. Their only reason for believing that such features must be stated ad hoc is that each language treats these features in different ways. But this is merely to say that each language has different rules of pronunciation and of syntax, not that the features themselves are not universal. Lacking the notion 'rule of grammar' they are also prevented from asking what the formal universals of language are, such as the existence and form of the phrase-structure and transformational components of the syntax.