J. MILLER

# A NOTE ON SO-CALLED 'DISCOVERY PROCEDURES'*

The purpose of this paper is to examine some aspects of the writings of Bloomfield and Harris, with reference to the work of Wells and Hockett, in order to bring to the fore certain of their basic ideas which have been ignored or misrepresented in recent polemics. While it is true that many American linguists were involved in the description of unanalysed Amerindian languages and looked to linguistic theory for guidelines for fieldwork, it is undeniable that the leading theorists (with the exception of Pike) were not concerned with the development of discovery procedures. This is an important point, because it is precisely the notion of discovery procedures that has been belaboured in discussions of American pre-generative linguistic theory. I wish to argue that basic to the work of the genuinely theoretical linguists of the 'structuralist school' was what Lyons (1970:27) calls "a certain interpretation of scientific rigour and objectivity", and not the desire to develop discovery procedures.

In the course of the argument important points will be developed. The first is that American 'structuralism' was not an isolated growth stemming from work on Amerindian languages, but was essentially connected with the main movements in mathematics and the philosophy of science in the early part of this century. The second, less important, point is that what is called the "American structuralist school" embraced various theories and that there is a wide gulf between the work of Bloomfield, Harris and Wells on the one hand and the work of linguists such as Bloch and Gleason on the other, with Hockett standing between the two groups. It is, I think, indisputable that Bloomfield, Harris, Wells and Hockett were the leading theorists of the 'American structuralist school', and in connection with the development of discovery procedures it is instructive to note that in the work of these linguists a distinction is carefully drawn between the actual process of discovering the structure of a language and the business of describing a structure which has already been discovered. Wells, for example, in his classic paper 'Immediate Constituents', remarks:

[···] we do not propose our account as a mechanical procedure by which the linguist,

* John Lyons, John Christie, Gill Brown and Ron Asher have kindly read and commented on an earlier version of this paper. Throughout the paper the terms 'structuralist' and 'structuralist school', which are often used to refer to pre-1957 American linguistics, will be used but put into inverted commas to show that this narrow use is unsatisfactory.

*Foundations of Language* **10** (1973) 123–139. 

starting with no other data than the corpus of all the utterances of the language and a knowledge of the morphemes contained in each one, may discover the correct IC-system.

Wells further remarks that

The distinction between methods of discovery and methods of proof (or more generally, methods of exposition), and between the order in which certain facts are discovered and the order in which they are expounded, is familiar to logicians (Joos, 1957:197).

Hockett is not as explicit on the subject as Wells, but he does make the following observation in his 'Problems of Morphemic Analysis':

[···] in actually working with a particular language one has to skip back and forth, operating by trial and error (Joos, 1957:241.)

Exactly the same point is made by Harris, the most rigorous theorist of the 'structuralist' school. In his 1946 work, 'From Morpheme to Utterance', there is no mention of discovering structures but only of description, witness the opening sentence: "This paper presents a formalised procedure for describing utterances". In the introduction to his later work, '(Methods in) Structural Linguistics', Harris states explicitly:

These procedures also do not constitute a necessary laboratory schedule [...]. In practice, linguists take unnumbered short cuts and intuitive or heuristic guesses, and keep many problems about a particular language before them at the same time [...] they will usually know exactly where the boundaries of many morphemes are before they finally determine the phonemes. (Harris, 1951:1)

Since these operations of substitution and segmentation and the concentration on form to the exclusion of meaning were not discovery techniques but expository procedures, one is entitled to ask why it was thought desirable that structural descriptions be set out in this manner. The answer is to be found in a short passage in Bloomfield in which are set out the essentials of his philosophy of science: (1) "science shall deal only with events that are accessible in their time and place to any and all observers (*strict behaviorism*)"; (2) (science shall deal) "only with events that are placed in co-ordinates of time and space (*mechanism*)"; (3) "[...] science shall employ only such initial statements and predictions as lead to definite handling operations (*operationalism*)"; (4) (science shall employ) "only terms such as are derivable by rigid definition from a set of everyday terms concerning physical happenings (*physicalism*)". (Bloomfield, 1939:13.)

It is worthwhile mentioning that, whereas it is usually maintained that no small influence was exerted on Bloomfield's thought by the development of behaviourist psychology, the above four tenets are peculiar neither to behaviourist psychology nor to Bloomfield's linguistics but were typical of the

logical empiricist views propounded by the Vienna Circle.[1] In turn, these views owed much to the ideas of Ernst Mach and to developments in mathematics in the nineteenth century (cf. Blanshard, 1962:93–126) and I will argue later that these developments are important in that they influenced the most theoretical of the American structuralists, Zellig Harris.

For the moment, however, I wish to concentrate on points (1) and (3) above, because it is these particular tenets which were the source of the typical 'structuralist' emphasis on dealing only with observables and on the segmenting of utterances. With respect to segmenting, it is interesting to note that the American 'structuralists' adopted an American addition to logical empiricism. Whereas the insistence that the linguist deal only with observables is simply an application to language of Mach's dictum that the laws of physics must be conceived of as statements linking percepts and that the scientist must confine himself to what he can observe, the interest in segmentation techniques stemmed from the American physicist Bridgman's theory of operational analysis, whose basic proposition is that a scientific term applies to a particular case if and only if the performance of specified operations yields a certain characteristic result (cf. Hempel, 1956:52).

Taking into consideration the above-quoted statements from Harris, Wells and Hockett, together with the statement by Bloomfield and the fact that operational analysis was a rather special American development, it is clear to me that the segmentation techniques of the 'structuralists' were adopted from Bridgman as the proper form which scientific statements should take.

At this stage in the discussion, however, it must be pointed out that American 'structuralism', as mentioned in the introduction, was no monolithic theory and that the label subsumed various different approaches to the study of language. Whereas the four linguists cited in the preceding paragraph were the leading theoreticians of the 'structuralist school', there were many linguists, less theoretically minded, who did indeed interpret these techniques of segmentation as discovery techniques for use in the field. For example, Swadesh (1934) discusses an inductive procedure for discovering the phonemes of an alien language and Gleason (1955) talks about the problems inherent in "the scientific analysis of a hitherto unrecorded language". The fact of the matter is that many American linguists were busy with the task of analysing unknown Amerindian languages and theoretical statements couched in terms of operations did readily lend themselves to interpretation as dis-

[1] Even in the best histories of linguistics this fact is not mentioned. For example, Robins (Robins, 1967:207) writes: "American theory was conditioned by the rigorous positivism of the behaviourist or mechanistic psychologists", and Dinneen (Dinneen, 1967:240–243) devotes three pages to a discussion of behaviourism with no mention of the developments in the philosophy of science.

covery procedures.[2] However, this does not alter the fact that this form of theoretical statement was borrowed from Bridgman's version of logical empiricism and that the leading theoreticians did not regard the statements as descriptions of discovery procedures.

The adoption of operationalism in such a thorough-going manner meant that the inadequacies of that particular philosophy would be reflected in its application in linguistics, and operationalism was by no means free from faults. Apart from the lack of criteria for distinguishing between 'good' and 'bad' operations, there was one extremely serious flaw which has been discussed in Hempel (1956), namely the lack of any explanation of the logical relationships between theoretical and observational terms. This flaw was compounded for structural linguistics by the adoption of physicalism and by the imitation of certain advances in mathematics. I wish to consider first of all these advances, which consisted in the development of non-Euclidean geometries in the middle of the nineteenth century. While such geometries are of interest in themselves, they are all the more important in that they drew attention to the theory of axiomatics, to the importance of formulating explicitly all one's assumptions and of investigating what follows logically from these assumptions. These advances in turn led to the realisation that sets of axioms and the theorems which could be deduced from them did not necessarily relate to specific objects but could stand on their own as uninterpreted systems and be investigated and developed for their own sake (cf. W. and M. Kneale, 1962:379–390). This position is essentially that of the formalist school, whose best known representative is Hilbert. Hilbert's basic idea was

> that the transfinite notions of mathematics were ideal constructions of the human mind [···] He proposed to establish [···] consistency for ordinary mathematics by examining the language in which this mathematics is expressed. This language was to be formulated so completely and so precisely that its reasonings could be regarded as derivations according to precisely stated rules – rules which were mechanical in the sense that the correctness of their application could be seen by inspection of the symbols themselves [...] without regard to any meaning which they might or might not have. (Curry, 1963:11.)

Hilbert's approach is well illustrated in his treatment of numbers, which he regarded not as existing entities but as a system of objects derived from a postulated initial object by means of a specified operation which could reapply an indefinite number of times.

[2] The same point is made by Robins (Robins, 1967:208). He mentions the influence both of Bloomfield's interpretation of science and of the practical task of language analysis which faced many American linguists of the time, but on p. 209 he writes as though Bloomfield himself had been concerned with the development of discovery procedures "American linguists concentrated their attention on formal analysis by means of objectively describable operations and concepts, as Bloomfield had insisted that one should". But Bloomfield talked not about analysis but about description.

These new ideas in the fields of mathematics and the philosophy of science exercised a very obvious influence on both Bloomfield and Harris. I wish to consider Bloomfield's work first. The very title of his paper 'A Set of Postulates for the Science of Language' betrays the source of the author's inspiration. Bloomfield, although attracted to the axiomatic method, was well aware of the distinction between mathematics and the other sciences and states right at the beginning of this paper that linguistic 'theorems' cannot be deduced from a handful of postulates. Rather he uses the postulates to make his assumptions absolutely explicit in order to clarify his own thinking and to free linguistics from the fruitless controversies which dogged the psychological linguistics of Paul, Wundt and Delbrück.

At first sight it seems that Bloomfield broke with the psychological linguistics he condemned by insisting that the linguist work without reference to meaning, or at least with as little reference as possible to meaning, and that linguistic analysis should begin with the phonetic data and proceed 'upwards' through the phonology to the morphology and syntax. It is in this fashion that Bloomfield was interpreted by the more extreme 'structuralists' such as Bloch, who then added the restriction that no syntactic or morphological information could be taken into account when describing or discovering the phonology of a given language.

However, careful examination of Bloomfield's own work indicates that the above interpretation, which is widely accepted, is wrong. Firstly, as stated in footnote 2, Bloomfield talked not about analysis but about description. Secondly, he did not attempt to eradicate meaning. As Lyons points out, Bloomfield was certainly pessimistic about the possibility of constructing a comprehensive theory of semantics but he never suggested that the study of the phonology and syntax of a language could be carried out "in total ignorance of the meaning of words and sentences" (Lyons, 1970:33). After all, in Bloomfield (1926) one of the basic assumptions is that utterances may be alike or different, though Bloomfield believed that awareness of such similarity or difference was all that the linguist needed to describe the syntax and phonology of a language and that a full, scientific account of the meaning of an utterance was out of the question at that time.

Thirdly, in *Language* there is no question of a phonological description which does not involve syntactic or morphological considerations. For instance, Bloomfield not only states explicitly that "The word is not primarily a phonetic unit" (1935:181) but discusses 'words' in such a way that it is clear that he is postulating a unit called a 'word' in order to explain such phenomena as occurrence of stress, vowel-harmony and possible consonant clusters. Although this interpretation of *Language* is the product of reading between the lines, its correctness is borne out by a slightly later work,

'Menomini Morphophonemics', in which they following statement appears:

> The process of description leads us to set up each morphological element in a theoretical basic form, and then to state the deviations from this basic form which appear when the element is combined with other elements. If one starts with the basic forms and applies our statements [···] in the order in which we give them, one will arrive finally at the forms of words as they are actually spoken.

These words might well have been taken from an introduction to generative phonology, with the exception that the term 'rule' and not 'statement' would be used.

It must be mentioned, furthermore, that in 'Menomini Morphophonemics' Bloomfield is unconcerned with discovery procedures. As has been pointed out (Chomsky, 1964), discovery procedures are possible only if certain criteria are invoked, among them that of bi-uniqueness and its stronger version, local determinacy, according to which phones may be assigned to phonemes only on the basis of their physical characteristics and/or the physical characteristics of neighbouring phones. Adoption of this criterion, however, leads to the problem of how phonemic overlapping should be handled. As one classic paper on phonemic overlapping has shown (Bloch, 1941), strict adherence to this criterion produces counter-intuitive analyses. The latter paper displays, it must be admitted, a rather extreme form of bi-uniqueness. At the other end of the scale are papers like that by Householder (Householder, 1965), in which it is asserted that there are no genuine cases of phonemic overlapping because there usually is some distinction in the phones. At the end of Menomini Morphophonemics' Bloomfield simply states that

> Menomini phonetics, however, allows a great deal of latitude to some of its phonemes, and of some overlapping between phonemes. Thus, phonemic ɛ is rather widely replaced by *e*, [···] some speakers partially and some quite constantly replace *i* by *e*.

That is to say, in his actual work on language Bloomfield postulates an abstract unit, the 'word', his justification being that this unit enables him to account for a large number of phonological facts, and he also postulates in a very sophisticated way abstract morphophonemic forms in Menomini from which the phonemic forms are derived by means of ordered rules. (Not surprisingly, Bloomfield's work was rather disturbing to many of the 'structuralists'. As Joos remarks:

> When we look back at Bloomfield's work, we are disturbed at this and that, but more than anything else Bloomfield's confusion between phonemes and morphophonemes disturbs us. (Joos, 1957:92.)

It may now be asked how it came about that Bloomfield's theoretical principles were misinterpreted and what the exact nature of these principles was. Although the main reason for misinterpretation was probably the general climate of opinion, with a very strong emphasis on observable data, another

important factor was the extreme vagueness of the general statements in *Language*, which amount only to an assertion that linguistic description starts with the phonology "which defines each phoneme and states what combinations occur". Now this assertion need not be interpreted as implying that the grammar of a language can and must be derived from its phonology. Indeed, as was pointed out above, one of the characteristics of *Language* is the extent to which syntactic, morphological and phonological phenomena are treated as interrelating. A much duller interpretation is available, namely that since "Any utterance can be fully described in terms of lexical and grammatical forms" and since "any morpheme can be fully described (apart from its meaning) as a set of one or more phonemes in a certain arrangement" (Bloomfield, 1935:167) it is only sensible to state first of all the possible shapes of morphemes in a given language.

Lying behind this interpretation is Bloomfield's most fundamental assumption: that every abstract unit or form postulated by the linguist has some physical correlate in the utterance-continuum. For many such units physical correlates had not been discovered in Bloomfield's day but Bloomfield was confident that more and more correlates would be revealed as laboratory equipment became more and more sophisticated. This assumption is closely connected with the four ingredients of Bloomfield's philosophy of science, behaviourism, mechanism, operationalism, and physicalism. What is particularly interesting is that Bloomfield does not say that the linguist should be able to take the phonetic data of a language and derive the grammar from an analysis of that data. What he does say is that the linguist's theoretical terms must be "derivable by rigid definition from a set of everyday terms concerning physical happenings". This in no way implies that the 'set of everyday terms' is in some sense prior to the set of theoretical terms but only that the linguist must eventually be able to justify the postulation of abstract units and forms by pointing to the publicly observable data which they explain.

Of course, there are difficulties. The classic technique used in the justification of syntactic or morphological analyses is to appeal to the substitution of one form or sequence of forms for another form or sequence of forms. The nub of the matter for Bloomfield was the problem of deciding when two forms or sequences of forms were to be classed as the 'same'. It was not enough simply to use a phonemic representation of the forms. Since forms were sequences of phonemes, what Bloomfield really required was that every phoneme be associated with a constant physical correlate which would be present in every utterance for which the phoneme was postulated. That is, Bloomfield was looking for correspondence rules connecting sentences with utterances. In the final description of the language the linguist should be

able to describe utterances and state that such-and-such a physical feature was associated, in every utterance, with such-and-such a theoretical unit, but there is no question of devising procedures for discovering which physical features were associated with which theoretical units. Bloomfield was concerned with describing the known, not with discovering the unknown. It must be emphasised that the word 'observable' should not be taken literally. It is sometimes thought that 'structuralists' such as Bloomfield and Harris were concerned with the analysis of actual sounds. As was stated in the preceding paragraphs, Bloomfield was well aware of the fact that even the study of the relatively 'low' level of phonology involved theoretical units, although his hope was that constant physical correlates would be found for each theoretical unit. Harris, who shares many of Bloomfield's principles, has this to say:

> Whereas the logicians have avoided the analysis of existing languages, linguists study them; but, instead of taking parts of the actual speech occurrences as their elements, they set up very simple elements which are merely associated with features of speech occurrences.

For 'associated' read 'connected by correspondence rules'.

With respect to the relationship between syntax, morphology and phonology, Harris appears to agree with Bloomfield insofar as he accepts that linguists "[...] will usually know exactly where the boundaries of many morphemes are before they finally determine the phonemes." (Harris, *op. cit.*:1.) Harris, however, differs from Bloomfield in that he believes that there are two approaches to the analysis of a language. Granted that one can tell when two utterances are similar or dissimilar, then, using the technique of substitution, one can either determine the morphemes independently of the phonemes or the phonemes independently of the morphemes, and Harris in *(Methods in) Structural Linguistics* does attempt to devise procedures for discovering which physical features are associated with which theoretical units, and then for discovering which groups of phonemes are associated with which morphemes.

This portion of Harris' work has attracted, and rightly so, a great deal of criticism. If, however, one takes Harris' earlier paper, 'From Morpheme to Utterance', a different picture emerges, because in it Harris could not have been less concerned with discovery procedures, his goal being to seek "a clearer method for obtaining generalisations about the structure of utterances." (Joos, 1957:142.) This paper shows clearly the extent to which Harris' response to the developments in mathematics was different from Bloomfield's. Hilbert's view that numbers do not exist but are a system of objects derived from a postulated initial object by means of a specified operation which can reapply an indefinite number of times finds, if not an exact parallel, at least a very strong echo in Harris' approach to the descrip-

tion of utterances: 'The method described in this paper will require no elements other than morphemes and sequences of morphemes, and no operation other than substitution, repeated time and again' (Joos, 1957: 142).

It must be stressed that there is no reference to Hilbert in any of Harris' writings and that the parallel drawn above between the work of Hilbert and that of Harris is a supposition, which, however, acquires a great deal of plausibility from Harris' obviously extensive knowledge of mathematics and philosophy of science, not to mention the striking similarity between Hilbert's methods, as described by Curry, and those of Harris, as described in the last sentence of the preceding paragraph. From Harris' writings alone it is also not clear whether Harris borrowed these ideas directly from Hilbert or whether these ideas were simply 'in the air' or whether the immediate source of the ideas was in the work of Carnap, who applied Hilbert's ideas to language, in particular to the language of science, in an attempt to devise 'formation' rules which would determine whether a sentence was 'well-formed' and 'transformation' rules which would describe the manner in which one sentence would be derived from another.

Harris himself has stated, in a letter to the writer, that he does not know that there was any particular source for the search for constructional methods in linguistics but that he himself knew Hilbert's work better than Carnap's, though he was also interested in the intuitionist approach to mathematics. On the basis of this statement it is, I think, valid to assert that the source of Harris' methods does lie in Hilbert's work, though Harris was not *consciously* influenced by Hilbert. It also seems valid to suppose that Carnap's work was yet another source. In particular, I suspect – though this suspicion will not be substantiated here – that Carnap's notions of 'formation' and 'transformation' rules passed into linguistics in Harris' later work on transformational analysis and are the source of the distinction in Chomsky's generative grammar between phrase-structure rules and transformational rules. Again, there is no suggestion here that the borrowing was done consciously.

Interesting as it is to investigate Harris' philosophical antecedents, the main business of this part of the paper is to suggest that, just as Bloomfield foreshadowed generative phonology, so Harris, in his 1946 paper, foreshadowed generative syntax, not just in some vague way but with respect to the concept of a grammar as a set of absolutely explicit rules and with respect to the postulation of abstract structures rather different from the observed structures. In order to substantiate this thesis I will have to go into a certain amount of detail concerning 'From Morpheme to Utterance'.

Harris' account of the noun phrase uses the symbols $N^1$, $N^2$, $N^3$ and $N^4$. In addition, the symbol $A$ stands for the class of adjectives, $T$ for a class of

morphemes which includes the traditional articles, demonstrative adjectives and possessive pronouns as well as *some* and *any*, and *P* for the class of prepositions. There are a number of other symbols but these will not be used in the following discussion.

The statements about the structure of utterances are in the form of equations, for example $N^1 - s = N^2$. $N^1$ might be *paper*. The combination of *paper* and the plural morpheme *s* yields the form *papers*. *Papers* can be substituted for *paper* in the frame *I'll get my – out* but *papers* cannot be substituted for *paper* in the frame *– s*, that is, in the frame which consists of the plural morpheme. There is no form **paperss*, since the plural morpheme is added only once to a noun in English. The fact that *papers* cannot be substituted for *paper* in all the frames in which the latter occurs is captured in the raised number, *papers* being $N^2$, *paper* $N^1$.

Other equations are $TN^2 = N^3$, for which the frame cited by Harris is *I don't like* – in which *the orchestra* or *these pointless, completely transparent jokes* can be substituted for *butter*, and $N^3PN^4 = N^3$, for which the frame is *Who brought – here*? In this frame *this piece of junk* can be substituted for *the book*.

These equations are not at all self-explanatory. Consider, for instance, the equation $TN^2 = N^3$ and the examples given by Harris. Firstly, one might ask just what the significance of the '=' sign is. Secondly – and I will take up this point first – why are the three examples, *the orchestra, these pointless, completely transparent jokes* and *butter*, all labelled $N^3$? What is particularly disturbing is that *butter* is $N^3$, because the previous example of a noun form without any accompanying article or adjective, *paper*, was labelled $N^1$. The answer to this question is given by Harris himself:

> [···] the values of the symbols in the equations are not morphemes but positions, indicating whatever morphemes occupy these positions (irrespective of what other positions these morphemes may occupy in other equations). (Joos, 1957:152.)

That is, in the symbol $N^3$ the raised number does not necessarily refer to a sequence of morphemes more complex than that referred to by the raised number 1 in $N^1$. The raised number refers to a position which may be occupied by sequences of varying complexity, e.g. *butter*, *the orchestra*, *these pointless, completely transparent jokes*.

How then does one decide which raised number should be assigned to a given sequence of morphemes? As with so much of Harris' work, the answer is in the text, unexpanded and obscure. In this case the clue is in the statement that

> In general, a class may be considered as bound to the level indicated by the number with which it is associated; i.e. it is bound to whatever is substitutable for the symbol – and – number combination that accompanies it in the equations. (Joos, 1957: 151.)

If the raised numbers are thought of as referring to levels, then the pieces of the puzzle fall into place, the puzzle being the various equations given by Harris.

Consider first of all the raised number 1. Two equations are relevant, $A^2N^1=N^1$ and $N^1$ *-s*$=N^2$, the latter of which has already been discussed. The former equation is exemplified by the frame *Isn't he a* – in which *peculiar fellow* can be substituted for *Senator*. That is, the raised number 1 refers to the level on which adjectives or the plural morpheme or the possessive morpheme can be added to a noun morpheme. Although Harris had as his goal a completely explicit set of statements, his account of the structure of utterances is not, as it stands in this 1946 paper, absolutely explicit. For example, since any number of adjectives can be added to a noun morpheme, as shown by the equation $A^2N^1=N^1$ (that is, an adjective added to a noun yields a sequence which may stay on the first level in order that another adjective be added) but since only one plural or one possessive morpheme can be added, an absolutely explicit account would have to ensure that adjectives are added before a plural or possessive morpheme. When one of the latter is added the resulting sequence of forms is moved up, as it were, to the second level. This, I wish to suggest, is the appropriate interpretation of the equation $N^1$ *-s*$=N^2$. Of course, Harris does not discuss utterances in terms of morphemes being added together but in terms of positions or frames on which various sequences can occur. Nonetheless, to talk, as I am, of levels on which different morphemes are added, seems consonant with the spirit of Harris' paper and certainly clears up a certain opaqueness in the original.

On the second level, one of the following morphemes may be added: *the, a, his, my, your, our, their, this, that, those, these, some, any, each.* If one of these morphemes is added, the sequence of morphemes is moved up to the third level, on which are added morphemes and sequences of morphemes such as *all, both, each of, some of, any of, one of, all of.* As Harris himself points out, there are certain selection restrictions which his statements do not incorporate, since a complete statement would exclude such sequences as **both any boys*, **each of each student*, and so on. When one of the appropriate morphemes has been added, the sequence is moved up to the fourth level so that no more of these morphemes may be added. That is, sequences such as **all all the boys* are avoided.

With the addition of one of the third level morphemes the noun phrase sequence is closed (cf. Hockett, 1958: 189). What is added on the fourth level is not, therefore, another 'bit' of the noun phrase but the verb phrase.

That is to say, on levels one to three, no matter what forms are added, the resulting sequence still behaves syntactically like a noun, but on the fourth level a noun phrase and a verb phrase are added together to yield a sentence.

It should be noted that it is not obligatory for a noun morpheme to have other morphemes added to it on any of levels one to three, which explains why a single noun morpheme such as *paper* or *butter* may be $N^1$ or $N^4$. *Butter* may be moved right up to level four without having anything added to it but on that level it has a verb phrase added to it. The raised number 4 refers to a particular level, which level is interpreted in terms of positions or frames, the frame in this case being simply a verb phrase or $V^4$.[3]

In the preceding exegesis backing can be found for the assertion that Harris anticipated generative syntax with respect to the notion of a grammar as a set of explicit rules and with respect to the postulation of abstract structures.

Firstly, although his 1946 account is not absolutely explicit as it stands, it was certainly Harris' intention to set up an explicit set of equations such that one could take a given utterance and trace what would now be called its derivational history. For example, Harris takes the sentence *She made him a good husband because she made him a good wife* and traces the sequence of equations leading up to the final equation $N^4V^4$ & $N^4V^4$. He shows that although both halves of the sentence consist, at the topmost level, of the sequence *NVNAN*, at some point in the sequence of equations one comes across two different formulae which show explicitly that the two halves of the sentence are different in meaning and structure. What Harris himself says about this sort of analysis is that

> indication of the difference, in meaning and in construction, can be derived from the structure of the utterance. We proceed to analyze the utterance, going backward along the equations as far as may be necessary to reveal this difference.

What Harris is talking about, essentially, is a set of formulae which represent the structure of utterances and which can be mechanically inspected, and in this respect his investigation of language runs parallel to Hilbert's investigation of mathematics (cf. the passage from Curry quoted earlier). Secondly, if one takes Harris' set of formulae and starts from the top instead of from the bottom then one can easily make the sort of statement which is made by a Chomskyan generative grammar. Just as Chomsky's phrase-structure rules tell us what a noun phrase may consist of, so Harris' equations tell us what sequences may turn up in $N^4$ position. That is, if one regards $N^4$ not as referring to a position but to a category, then Harris' equations are easily converted into Chomskyan phrase-structure rules. Just as Bloomfield's

[3] Harris' formulae also indicate what the head of a construction is. This is an important point, because this type of information is not given by the rewrite rules of a generative grammar. Postal (Postal, 1964:27) recognises this but tries to show that Harris' formulae do not give this information and can therefore be considered equivalent to PSG. The flaws in Postal's argument have been exposed by Robinson (Robinson, 1970).

work on the morphophonemics of Menomini foreshadowed generative phonology, so Harris' work on syntax foreshadowed generative grammar (cf. Lyons, 1968: 157 and 1970:35). Thirdly, just as Bloomfield and Hockett (cf. the discussion of the normalisation of Latin forms in the latter's paper 'Problems of Morphemic Analysis') postulated rather ingenious abstract forms when dealing with the phonemic shape of morphemes, so Harris postulated rather ingenious abstract structures in his analysis of sentences. I am thinking in particular of that passage in his 1946 paper in which he says that

> English *Vv* suffixes (*-ed*) are best added not to *V* (verb morphemes) but to $V^3$ (verb phrases including object, etc). *-ed* may best be regarded as a suffix of the whole verb phrase.

This statement is elaborated in Harris' 1957 paper and is very like the approach to tense which was adopted by Chomsky.

It may further be noted that the notion of deep abstract structures is also anticipated in the idea that the structure of an utterance may be represented by a set of formulae. In Harris' later work, though not in the 1946 paper, the concept of rule ordering too is anticipated. Although Chomsky claims in *Current Issues* that Harris' rules are not ordered, in his 1957 paper Harris brings out and elaborates the idea that the formulae are indeed ordered. This is perfectly clear in his discussion of *May there be mentioned now a certain secret*?, in which he talks explicitly about the rules having to apply in a definite order (Harris, 1957:202–210).

Now that the work of Bloomfield and Harris has been discussed, the time has come to consider briefly the ideas of Pike. Indeed, no paper on any aspect of American linguistics before 1957 would be complete without mention of Pike because, although in the opening paragraph of this paper it was observed that Pike was the only leading theorist who made one of his chief goals the development of discovery procedures, these 'discovery procedures' were not a set of explicit operations but were rather a set of rough guidelines accompanied by detailed notes on the sorts of peculiar situations that occur in natural languages. Furthermore, at a time when, in America, the generally received theory of linguistic description was the Bloch-Trager interpretation of Bloomfield, Pike in his theoretical and descriptive work constantly and consistently reminded his readers that a phonological description of any language was impossible without taking into account syntactic and morphological information (cf. Pike, 1947).

Now that some twenty years have passed since the controversy of 'the way up' versus 'the way down' was at its peak, it is instructive to examine Pike's position vis-a-vis those of the other theoretical linguists of the time because such an examination reveals clearly the differences of opinion inside

the 'structuralist school' and changes of opinion within the work of individual linguists. For example, with respect to differences of opinion, while it is undeniable that Pike's theory and that of the Bloch-Trager-Gleason group were irreconcilable, it is equally undeniable that, granted the correctness of the above interpretation of Bloomfield's work, Pike's theory was rather similar to Bloomfield's.

These two approaches, that of Bloomfield and that of Bloch-Trager-Gleason, are the two extremes of the structuralist school between which other linguists vacillated. For instance, in the opening paragraphs of this paper a sentence was quoted from Hockett (1947) which shows that Hockett was well aware that linguists tackling hitherto unanalysed languages do not do all the phonology before proceeding to the morphology and syntax and indeed the whole spirit of Hockett (1947) is that of generative phonology (see in particular the discussion of the Latin nouns *rex*, *urbs*, *nix*), yet in Pike (1947) the statement (from Hockett, 1942) with which Pike begins his attack is: "No grammatical fact of any kind is used in making phonological analysis".

The crucial word in this quote is 'analysis', because it is one thing to assume that one can take an un-analysed language and find out its phonological system without having the least idea about the grammar of that language, but quite another thing to assume (a) that every theoretical unit has a physical correlate and (b) that in the final description of the analysed language one should begin with the physical correlates and work one's way up to the theoretical units. (The assumptions (a) and (b) are logically independent of each other. I have tried to show that Bloomfield held (a) but not (b).) If Hockett did in fact hold the former assumption, about analysis, then he was wrong, but if he held merely the latter assumption, about the form of the description, then he was not wrong but was providing a description which was not the simplest one available.

For the purposes of this paper it is not important to decide exactly what Hockett meant by 'analysis'. What is important is that it be recognised that there is a certain vacillation in Hockett's work and that there is at least one paper in which Hockett is not concerned with discovery procedures and in which his description proceeds from the phonemics to the phonetics (Hockett, 1947).

A comparison of Pike and Harris helps to reveal the complexity of and the confusion in Harris' writings. I think it fair to assume that Pike would have no quarrel with Harris' work on syntax, in which Harris explicitly rejects any connection between his methods of description and discovery procedures. What Pike did disagree with was Harris' earlier work on phonology, in particular with statements which implied that the phonology of a

language could be investigated without knowledge of its grammar (see especially the discussion of junctures in Harris, 1942). As with Hockett, it is not clear whether Harris means 'analysis' or 'description', but given his many disclaimers that his methods constitute laboratory schedules or discovery procedures, I incline to the latter interpretation. However, the resolution of this problem is not important for the present paper; indeed, the problem may not be capable of resolution. As has been said of *(Methods in) Structural Linguistics*,

> [···] everything is in it somewhere or other. And, like the Bible or Shakespeare, you can easily support both sides of any argument with quotations from it. (Householder, 1952: 268.)

In spite of the confusion with respect to fundamental theoretical principles which is revealed in the work of Hockett and the early work of Harris by the comparison with Pike, it still is true to say that the leading theorists of the American 'structuralist school', Bloomfield, Wells, Harris (and, to a lesser degree, Hockett), were not primarily concerned with techniques for discovering the structure of previously unknown languages, that they used techniques which are often supposed to be unique to generative grammar and that their solutions to particular problems in a given language were often penetrating and insightful.

These points are important, because many existing accounts of the structuralist school do not mention these interesting features of certain structuralist work, either because they are superficial, like Lepschy's (Lepschy, 1970), or polemical and tendentious, like the observations in Chomsky (1964) and Postal (1964). Certainly in Chomsky (1964) there is a footnote in which it is mentioned that the procedures of the taxonomic school were influenced by pragmatism and operationalism, but apart from this footnote Chomsky directs his fire at discovery procedures. Moreover, he attacks either the weak points of the best structuralists or the arguments of the lesser ones, a form of argument which quickly leads people to believe that before 1957 all was darkness.[4] Now it must be stressed that this paper does not constitute a denial of the insights and benefits of generative grammar but

[4] To be fair, it must be said that Chomsky has always acknowledged his debt to Harris, both in the preface to *Syntactic Structures* and in *Current Issues in Linguistic Theory*, footnote 1, and Harris in turn acknowledges his debt to Chomsky (footnote 1 in Harris, 1957). However, in spite of these acknowledgments, it is a rather one-sided account of pre-1957 American linguistics which is to be found in *Current Issues*. Perhaps one should not be surprised at this, since it must have been extremely galling to have developed a theory which put right the defects in earlier theories and yet to see it reviled and rejected by the linguistic establishment. The same bitterness, mixed with revolutionary zeal, pervades Postal (1964) and is responsible for the utmost lack of sympathy with, and subsequent misinterpretation of, certain earlier theories of syntax, in particular those of Harris (cf. footnote (3) above) and Pike.

rather a plea for a more balanced appraisal of the 'structuralist school' than has hitherto been available. Such an appraisal would be extremely useful to the community of linguists in general and salutary for practitioners of the generative school.

*University of Edinburgh*
*Department of Linguistics*

## BIBLIOGRAPHY

Blanshard, B.: 1962, *Reason and Analysis*, Allen & Unwin, London.
Bloch, B.: 1941, 'Phonemic Overlapping', *American Speech* **16**, 278–284. Reprinted in Joos.
Bloomfield, L.: 1926, 'A Set of Postulates for the Science of Language', *Language* **2**, 153–164. Reprinted in Joos.
Bloomfield, L.: 1935, *Language*, Allen & Unwin, London.
Bloomfield, L.: 1939, 'Linguistic Aspects of Science', in *International Encyclopedia of Unified Science* **1**, University of Chicago Press, Chicago.
Bloomfield, L.: 1939, 'Menomini Morphophonemics', *Travaux du Cercle Linguistique de Prague* **8**, 105–115.
Chomsky, N. A.: 1964, *Current Issues in Linguistic Theory*, in Fodor and Katz.
Curry, H. B.: 1963, *Foundations of Mathematical Logic*, McGraw-Hill, New York.
Dinneen, F. P.: 1967, *An Introduction to General Linguistics*, Holt, Rinehart and Winston, New York.
Fodor, J. A. and Katz, K. J. (eds): 1964, *The Structure of Language*, Prentice-Hall, Englewood Cliffs, N.J.
Gleason, H. A.: 1955, *An Introduction to Descriptive Linguistics*, Holt, Rinehart and Winston, New York.
Harris, Z. S.: 1942, 'The Phonemes of Moroccan Arabic', *Journal of the American Oriental Society* **62**, 309–318.
Harris, Z. S.: 1946, 'From Morpheme to Utterance', *Language* **22**, 161–183. Reprinted in Joos.
Harris, Z. S.: 1951, *Structural Linguistics*, University of Chicago Press, Chicago.
Harris, Z. S.: 1957, 'Co-occurrence and Transformation in Linguistic Structure', *Language* **33**, 283–340. Reprinted in Fodor and Katz.
Hempel, C. G.: 1956, 'A Logical Appraisal of Operationism', in Frank, P. (ed.), *The Validation of Scientific Theories*, Beacon Press, Boston.
Hockett, C. F.: 1942, 'A System of Descriptive Phonology', *Language* **18**, 3–21. Reprinted in Joos.
Hockett, C. F.: 1947, 'Problems of Morphemic Analysis', *Language* **23**, 321–343. Reprinted in Joos.
Hockett, C. F.: 1958, *A Course in Modern Linguistics*, Macmillan, New York.
Householder, F. W.: 1952, Review of Harris, Z. S., *Methods in Structural Linguistics*, *International Journal of American Linguistics* **18**, 260–268.
Householder, F. W.: 1965, 'On Some Recent Claims in Phonological Theory', *Journal of Linguistic* **1**, 13–34.
Joos, M. (ed.): 1957, *Readings in Linguistics*, American Council of Learned Societies, Washington, D.C. Republished in 1966 as *Readings in Linguistics* Vol. I, University of Chicago Press, Chicago and London.
Kneale, W. and Kneale, M.: 1962, *The Development of Logic*, Oxford University Press, Oxford.
Lepschy, G.: 1970, *A Survey of Structural Linguistics*, Faber and Faber, London.
Lyons, J.: 1970, *Chomsky*, Fontana/Collins, London.

Pike, K. L.: 1947, 'Grammatical Prerequisites to Phonemic Analysis', *Word* **3**, 155–172.
Postal, P.: 1964, *Constituent Structure, International Journal of American Linguistics*, No. 1, Part II.
Robins, R. H.: 1967, *A Short History of Linguistics*, Longmans, London.
Swadesh, M.: 1934, 'The Phonemic Principle', *Language* **10**, 117–129. Reprinted in Joos.
Wells, R. S.: 1947, 'Immediate Constituents', *Language* **23**, 81–117. Reprinted in Joos.