



1974

The dual structural criterion reviewed

Thomas Bearth
SIL-UND

Follow this and additional works at: <https://commons.und.edu/sil-work-papers>

Recommended Citation

Bearth, Thomas (1974) "The dual structural criterion reviewed," *Work Papers of the Summer Institute of Linguistics, University of North Dakota Session*: Vol. 18 , Article 1.

DOI: 10.31356/silwp.vol18.01

Available at: <https://commons.und.edu/sil-work-papers/vol18/iss1/1>

This Article is brought to you for free and open access by UND Scholarly Commons. It has been accepted for inclusion in Work Papers of the Summer Institute of Linguistics, University of North Dakota Session by an authorized editor of UND Scholarly Commons. For more information, please contact zeineb.yousif@library.und.edu.

The Dual Structural Criterion Reviewed

Thomas Bearth

The dual structural criterion (referred to henceforth as DSC) is a solidly established piece of discovery procedure in tagmemic methodology. Its basic usefulness and empirical adequacy is reflected in a great number of tagmemic descriptions, whether the authors explicitly refer to it or not.

There are, however, two major limitations to the usefulness of the dual structural criterion: the first pertains to its application as a practical tool of analysis, the second, to its theoretical status. I shall try to show that the two weaknesses are interrelated.

1) There is an insecurity as to the precise criteria which govern the application of the DSC. This fact has led to some controversy between Pike and Longacre, in spite of their agreement as far as the basic principle is concerned. Thus Pike (1962, p.231 f.) agrees with the fundamental idea as presented in an earlier paper by Longacre (1960, unfortunately I do not have this available here), and writes (1962, p.231): "Two constructions, to be in contrast, must differ in form and meaning." And: "Longacre has shown that the formal difference must be dual." The duality requirement is particular to minimal contrast between grammatical constructions. Pike stresses this particularity by contrasting it with the minimal condition for establishing a contrast between phonemes: "...a pair of phonemes minimally contrastive may differ by only one component; a pair of constructions must differ by two formal components." Nor does Pike seem to object to Longacre when he quotes his requirement, with regard to the dual contrast, of "a difference of two tagmemes, or of one tagmeme and a transformational potential". However, Pike regrets Longacre's "focus on the number of segments within a grammatical string". He states his specific point of view: "In my view a difference in the distribution of two constructions in higher-layered constructions may, like a transformational difference, count as one of two required differences, provided this distributional difference is paralleled by a substantial difference in structural meaning (such as "declarative" vs. "interrogative"). "(p.232) Longacre, in his Grammar Discovery Procedures (1964) explicitly excludes external distribution as a contrastive feature between constructions. His main contention is that this would obscure the distinctiveness of structural levels of the hierarchy; "external distribution of a syntagmeme may naturally be described in terms of the (usually higher level) tagmeme which that syntagmeme manifests" (p.21-22). In a footnote on p.21, Longacre mentions other points on which Pike holds a different view, particularly the need to appeal to semantics in some cases. We shall come back later to this latter divergence.

2) My second query is more fundamental and more theoretical and is the primary concern in this contribution. By focussing attention on this

2.

second issue, we shall hopefully be able to provide new insights into the first issue which is of a more procedural kind.

Nowhere in the literature, as far as I know, has a satisfactory explanation been given as to why the DSC should be the decisive factor for distinguishing between constructions. Tagmemicists have been satisfied to find that the duality principle seems to work by and large, in other words, that it is empirically correct. It holds its place as a useful concept in tagmemics, but on the other hand it stands there so to speak, as an erratic block. It does not appear to be in any logical or organic sense related to the rest of the theory, or derivable from basic tagmemic concepts. Yet if one studies for example the sections dealing with analytical procedures for clause and phrase level syntagmemes (Longacre, 1964, pp.47-61 and 74-84) he is quickly convinced of the strategic place which the DSC occupies in tagmemic analysis.

I believe that this lack of explanatory power in the concept of the DSC has its roots in the overall view which tagmemicists hold of grammar in general, and in particular in their view of the basic unit of grammar, the tagmeme itself. I do not think that these views are mistaken, but rather that they need complementation.

Having presented the problem, I hope to show that the DSC finds its theoretical justification and a consolidation of its conditions of application within the framework of a generalized theory of the linguistic sign, although not without undergoing some modifications.

By "generalized" I mean here a theory of the linguistic sign which accounts not only for lexical units of various sizes and complexity and their meanings, but also for constructions and their meanings.

Frei (1962) has argued that a syntactic relation is a linguistic sign. His argument goes as follows: The total meaning of a syntagmeme is not equal to the sum of the meanings of its constituents (e.g. *deux cent* vs. *cent-deux*). An additional semantic factor has to be accounted for, which is independent of the meaning of the constituents and is therefore a property of the syntagmeme itself, not of its constituents (multiplication vs. addition in the example just given). This meaning or *signifié* (using Saussurian terminology) has its formal counterpart or *signifiant*; the latter, according to Frei, is non-segmental and may be manifested by such features as order (as in the example given), intonation, complementary distribution of variants, and (maybe most important) selection.²

Most important to us is the view that a grammatical relation is a linguistic sign, constituted by the arbitrary and synchronically unalterable relationship between a *signifié* and a *signifiant*. In view of the particular (non-segmental) nature of the relational sign. Frei suggests a particular terminology: the total sign, i.e. the syntactic relation of a syntagmeme as such, he calls *catène* (which I shall call catena to make it more distinct from *catène*); the *signifié* is called catène (the pattern meaning), the *signifiant* catenant (the formal correlates of the pattern meaning).

In the above example, the catenas would be described as follows: in "deux cent" the caténé is "multiplication"; the caténant is a feature of order whereby the lower numeral precedes the higher one; in "cent-deux", the caténé is "addition", the caténant the order "higher numeral followed by lower numeral".

Once we accept to work with the hypothesis that a syntactic relationship is a sign which can be abstracted from the constituent signs, we can try to formulate the conditions under which two such signs, i.e. catenas, must be considered to be different. Note that this is tantamount to asking what the distinguishing features have to be in order to make two constructions different. For reasons which will become apparent soon, it is necessary to examine separately the conditions for the signifiant (caténant) and for the signifié (caténé).

What minimum requirement has to be fulfilled so that two caténants can be considered to be different, given their particular nature of non-segmental signs? Remember that segmental signifiants are perceived as different when they differ by at least one of their constituent phonemes. (We disregard at this point homophonous signifiants and allomorphic variation.) Since caténants have a definable, but not a segmental form, their minimum differentiating factor cannot consist of phonemes. Furthermore, since the catenant is different from the signifiants of the constituents, and since the catenant is the fundamental ingredient of the syntagmeme, formal differences within the signifiants of the constituents are of no bearing whatsoever on the catenant. This is trivial, since we know that the segmental composition of a syntagmeme is variable, it corresponds to the fact that its constituent terms are substitution classes, not individual signs.

In other words, a change in one of the constituents does not by itself signal a new pattern. Nor does a change in more than one constituent, or even a change of all constituents. Nor does a difference of one tagmeme alone indicate a change of construction, --because after all it is not a different tagmeme at all! This paradoxal way of putting it shows to which extent the tagmeme is a relational concept - not only because of the slot-class relationship, but also in a syntagmatic sense.

+ X + Y
+ X + Z

Z could only constitute a different tagmeme from Y if it was a member of a different set. A different set would be characterized by a different relational potential at one point or another in the language.

The basic condition, then, is not the number of differences, but the presence of a relational difference. A difference between two relations must be relational in nature. Now a relational difference is observable at least in two places in a linear arrangement. It implies some kind of a mutual constraint between two terms of an arrangement, to the effect that the choice of A limits the choice of B in a different way from the way in which C limits the choice of D (and vice-versa),

A, B, C and D being different, but potentially intersecting sets of elements.

The DSC then has its ultimate *raison d'être* in its usefulness as a means of diagnosing relational (or structural) differences. It reflects the fact that formal differences between constructions are basically non-segmental, although they may of course involve selections bearing on segments.

It would seem, then, that the formal structure of language is built around three types of contrasts: distinctive feature contrasts, (distinguishing phonemes), segmental or suprasegmental contrasts (distinguishing morphemes) and relational contrasts (distinguishing syntagmemes).

Going back to our initial hypothesis that a syntactic relation is a sign, I shall now trace the analogy further. It is clear that whereas one or more segmental (or suprasegmental) differences, are the normal, statistically predominant way of distinguishing morphemes, this is not true in an absolute sense. There are two cases in which a segmental structure does not signal a difference between morphemes: homonyms and allomorphs. What permits us to conclude in the first case that there is a difference in spite of the sameness of segmental structure and in the second that there is no difference in spite of the different segmental structure? The answer is simple: meaning. We can say, then, that two signs are different if and only if their significés are different. In the average, this is correlated with the manifestation of a phonemic difference in their signifiants, but not always. It must be stressed that the ultimate criterion is semantic. This also is true at the phonological level: The decisive criterion for establishing phonemes as contrastive units is their relevance to meaning.

The deviation from the norm as seen in homonyms and allomorphs is a consequence of the arbitrary nature of the relationship between signifié and signifiant. Now if syntactic relationships are in fact, as I believe they are, linguistic signs, then the relationship between the caténé and the caténant shares the general properties of the relationship between any signifié and its signifiant. Hence we would expect to find usually an overt relational difference, satisfying the DSC requirement, between two constructions, but not always. Just as two signifiants may be homonyms we might expect at least theoretically two caténants to be homonymous. On the other hand - and this is much more likely - we should be prepared to find cases where the DSC requirement is fulfilled and yet the constructions must be regarded as being the same. From the possibility of homonymous caténants and variant forms of caténants it follows again that the ultimate criterion for establishing two syntagmemes as contrastive entities is not formal but semantic; two caténas, and hence two constructions are different if and only if their caténés are different.

Admittedly this approach does not make things easier. Structural meanings tend to be, to quite a degree, more abstract than lexical meanings. Adopting semantics as ultimate in syntactic analysis brings

with it a considerable danger of ethnocentric projection (as illustrated by some analyses based on intuition). However difficult it may seem, I see no other way. The final touchstone of anything which is truly linguistic is semantic distinctiveness. Semantics, although being the most elusive part of the linguist's task, seems to be unavoidable.

The point I am arguing, then is that semantic distinctiveness and not formal differences per se, is the decisive diagnostic factor at all levels for establishing the contrastive identity of linguistic units, whether it be phonemes, morphemes or syntagmemes. By this, I do not mean to discard the usefulness of and the need for formal criteria. But while these may precede procedurally the semantic criterion, they are inferior to the latter in the hierarchy of decisive power. The DSC, as stated by Longacre, will work in most cases, but it needs ultimate confirmation on semantic grounds. On the other hand it should be expected that a correct semantic interpretation will always have some formal correlate which confirms it; just as homonyms are in most cases distinguished by some distributional feature, so homonymous catenas will be objectively distinguishable in some way, even if in some cases it is not by the DSC. E.g. declarative and interrogative clauses mentioned by Pike (1962) may be one such case, in which precisely external distribution is an indication of a correct identification of two types based primarily on semantic criteria in the absence of the DSC. Symmetry within the system may be another substitute for the DSC, and Longacre himself makes provision for this: "On occasion, however two similar sequences - not adequately contrastive in internal structure - may be considered to be separate because they belong to different classes which are otherwise separable (in that they are constituted by sequences adequately contrasting in internal structure)." (1964, p. 22-23.) Longacre himself does not seem to discard meaning from the analysis; "The background of meaning would seem to be essential to the analysis." Does he imply the use of meaning when he writes a few lines later: "Nevertheless, it seems necessary to insist that it is the formal side of the (form-meaning) composite which is amenable to initial systemic analysis"? (Parentheses and underlining mine.) However this may be, I have tried to show that meaning must be given a more definite place in the analysis than either Longacre or Pike envisage it, and where precisely this place is. At the same time this does not contradict in any way Longacre's insistence on optimal rigour on the formal side. Furthermore while fully recognizing the value of the DSC as formulated by Longacre as a methodological tool, I cannot maintain its absoluteness but, on a purely deductive basis, I must agree with Pike that there are cases which cannot adequately be handled with Longacre's criteria.

To summarize, I would like to draw the following conclusions:

- 1) The DSC is justified as the predominant formal way of signalling that two grammatical constructions are different.
- 2) The recognition of sign properties in syntactic relations gives a theoretical basis to the DSC, permits us to account for its range of

6.

variability, and explains the existence of cases of deviation.

3) The inclusion of the signifié of the catena in the discovery procedure integrates grammatical analysis into linguistic analysis as a whole and permits a unified approach and the application of the same kind of criteria at all levels.

4) The treatment of the formal properties of syntagmemes as signifiants of the catena permits us to define the precise nature of relational signs in contrast to other kinds of signs.

5) The sign approach to syntax brings into focus the need for studying seriously the semantic side of grammar rather than just giving semantic labels to constructions, not as an unavoidable evil but as a prerequisite to identifying the units of grammar and understanding the way they function.

I would also like to suggest the following areas for further exploration:

1) Procedures for exploring, isolating and describing the meaning of syntactic relations.

2) Theoretical implications of the postulated existence of "segmental" and nonsegmental signs.

3) A closer investigation of the nature of the catenante. Are Frei's features exhaustive? Are they necessarily non-segmental? (Note that the syntagmatic model taught at British S.I.L. comes relatively close to the catena approach but recognizes that morphemes themselves may be described as syntagmatic features characterizing a construction. Frei would presumably say that the characterizing feature consists of the selection of a given set of morphemes rather than of the morphemes themselves. Is this a purely terminological divergence?) It would be necessary, at this point, to reexamine Pike's criticism of Bloomfield's tagmeme and taxeme (which is akin to though not identical with Frei's catena) (1967, p. 286f.). Even if substantial changes have to be made regarding the features of the catenante, it does not invalidate the fundamental approach. It seems to me that there is something valid even in Bloomfield's approach which is missing in tagmemics.

4) The explicit approach to grammar through relationships, not in place of a unit-arrangement approach but rather complementary to it. Ultimately, this might lead to a reopening of the discussion about the fundamental unit in grammar. I consider the signifiant-signifié relationship to be the pivot around which everything else which is relevant in language gravitates. While it is true that the tagmeme is a form-meaning composite, it cannot be conceived as such apart from the structure of which it is a part. Hence the question: Is the tagmeme really the basic unit of grammar? Or does the tagmeme reveal part of the truth, the DSC another part, the catena by integrating the two, the whole truth (inasmuch as this is possible)? (The tagmeme would be handled as a feature or bundle of features of selection, in relation to structural meaning and order.)

Note, with regard to the status of relationships, Pike (1967): "...the units are treated as occurring only as points in a system of relationships with unity and relationship both relevant, but with attention focused on the first" (p.282); and: "I would now-1964-find less objection, however, to a view which gives equal priority to tagmemic class-in-slot and to construction - as a neither-the-hen-nor-the-egg-first view." (op.cit., p.477).

5) Relevance of the catena approach to dynamic models, particularly TG.

Bibliography

Frei, Henri, "L'unite linguistique complexe". Lingua XI, 128-140, (1962)

Bloomfield, Leonard, Language, London 1933.

Longacre, Robert E., "String Constituent Analysis", Language 36, 63-88
(1960)

Grammar Discovery Procedures, Mouton (1964)

Pike, Kenneth L., "Dimensions of Grammatical Constructions", Language 38,
221-244 (1962)

Language in Relation to a Unified Theory of the Structure
of Human Behavior, Mouton (1967)

Footnotes

1) Of course, it is consistent with the general concept of the feature mode and the notion of contrastive-identificational feature. However, this in itself does not account for the duality principle. Longacre's inductive presentation is valid but still leaves the basic theoretical questions aside. He writes (1964, p. 18): "One difference is insufficient in that it may be relevant to but one point in the patterns being contrasted and may not indicate that the patterns themselves are in contrast."

2) These features are of course reminiscent of Bloomfield's taxemes; Frei makes a reference to Bloomfield in which he criticizes an instance of Bloomfield's application of the taxeme of selection (p.138, footnote 34). I am not concerned here with the details, though, but with the principle.