MANFRED KRIFKA

HARMONY OR CONSISTENCY?

*Review of John A. Hawkins, Word Order Universals**

Hawkins' book is concerned with regularities of word order and word order change in the Greenberg and Vennemann tradition. In this review it is argued that H. offers some new data and ideas, but that his work does not constitute a substantial progress. My main points of criticism are that H.'s data do not warrant the conclusions he draws if one applies statistically sound methods, and that H. considers only a restricted set of possible explanations for word order regularities.

H.'s book (Word Order Universals, New York: Academic Press, 1983) is mainly a reformulation of thoughts developed in his earlier articles (1979, 1980, 1982). In the first two chapters, H. discusses the basics of the study of word order universals. In chapters 3 and 4, he develops his own theory of synchronic word order universals. In chapters 5, 6 and 7, he discusses some consequences for language change and historical reconstruction. Chapter 8 contains the data for his language sample.

It is difficult for me to do justice to H.'s work. The reason is that the statistics on which his hypotheses are based are not reliable. This shortcoming frequently leads to a confounding of statistically warranted facts and mere hypotheses. But I try to hold back my objections to the underlying statistics and take H.'s figures for granted until the last section. I think this is an adequate way to treat the more interesting parts of H.'s work.

1. Introductory considerations

In the first two chapters, H. summarizes the findings and theories of the study of word order universals. He starts with a short discussion of the

* I would like to express my thanks to Theo Vennemann, Thomas Becker and Michel Kefer for the discussion of this paper, and to Carin Föhr for correcting my English.

two approaches to language universals, the innatist approach (associated with Chomsky 1965 and subsequent publications) and the functional approach (associated with Comrie 1981 and others). H. tries to mediate these approaches by stressing that they are concerned with quite different problems. His book is mainly conceived in the functionalist paradigm, but he tends to explain the word order universals with a theory originating in the innatist paradigm, namely X-bar-theory.

A fundamental problem for any theory of word order typology in the Greenberg tradition is the determination of the basic word order of a given language, i.e. the normal serialization of subject (S), object (O) and verb (V); of adjective (A) and genitive noun phrase (G) relative to their head noun (N); and of the adposition (P) relative to the noun phrase (I use "N" to cover both nouns and noun phrases, and the term "adposition" to cover prepositions, postpositions, circumpostitions and ambipositions). There are at least two problems for the determinations of basic word order: (i) the problem of identifying S, O, V etc. in a given language and (ii) the problem of deciding which of the occuring orders of the thus identified elements is basic.

H. hardly touches upon problem (i); he simply refers to Mallinson and Blake (1981) and thinks that "semantic criteria will suffice to make the cross-linguistic equation" (p. 12). It is difficult to see how to apply semantic criteria in crucial issues like the determination of subject in ergative languages. The only way to identify a subject in these cases is to investigate the syntactic and morphological processes of the language in order to determine its primary grammatical relation (cf. Keenan 1976 for the definition of subject and Sasse 1978 for the notion of the primary grammatical relation).

In order to handle problem (ii), H. applies three criteria. He uses the term "doubling" to describe the situation in which there is a variable serialization of two constituents. The basic one is that which (a) is more frequent in text samples, (b) is more frequent within the grammatical system (e.g., if most lexemes of a category occur in serialization x, then x is basic), and (c) is grammatically unmarked (e.g., if serialization x, but not serialization ¬ x may undergo a certain rule, then x is basic). In cases where these criteria collide or are not sufficient to establish a basic word order, H. makes no basicness distinction at all (a case in point are prenominal and postnominal genitives in English.) In later chapters, H. extensively draws on doublings in the discussion of word order change.

I miss the inclusion (or at least the discussion) of free word order languages as a subtype of its own (cf. e.g. Hsieh 1977). It may be possible to determine with H.'s criteria a basic word order for languages like Latin or Warlpiri. But this basic word order would then be of a quite different status: It would mainly reflect pragmatical, not grammatical properties.

In the second chapter, H. gives a concise report of the works on word order by Greenberg (1966), Vennemann (1972, 1974, 1975, 1976) and Vennemann & Harlow (1977). He also discusses the theory of Keenan (1979). In a monograph like this, the inclusion of some other important works, like of Tesnière (1959) and Lehmann (1973), would have been appropriate, I think.

2. The Principle of Cross-Category Harmony

In the next two chapters, H. formulates his main theoretical contributions to the study of word order: In chapter 3, he pleads for a reformulation of universals, and in chapter 4, he develops an explanation for the cooccurence of certain word order properties. I will start my discussion with this chapter.

Based upon the possible combinations of the basic order of S, V, O, of A, G, N and of N, P, Greenberg (1966) defined 24 language types. For example, the combination of SVO, AN, NG and PN (i.e. Prepositions) constitutes one type, and the combination of SOV, AN, GN and NP (i.e. postpositions) another. Some of these types are represented by quite a lot of languages from different areas and families, others are less populated, and some have no instantiations at all in the samples used by Greenberg.

Bartsch & Vennemann (1972) and Vennemann (1972, 1974, 1975, 1976) tried to explain this unequal distribution by their "Principle of Natural Serialization" (NSP) (which would more perspicuously be called "Principle of Unidirectional Serialization"). This principle states that natural languages tend to either propose or postpose all modifiers ("operators") relative to their heads ("operands"). According to Vennemann (1972), O, A, G, N are operators to V, N, N, P, respectively; Vennemann (1976) and Vennemann & Hawkins (1977) included S as an operator to V. There are, then, two ideal types of languages defined by the NSP, namely the type of consistent postspecifying languages with VSO/VOS & NA & NG & PN and the type of consistent prespecifying languages with SOV/OSV & AN & GN & NP. Vennemann's principle was criticized by numerous scholars, mainly for two reasons: More than half of the languages listed in the Greenberg samples do not fit into the ideal types, and there are cases of word order change where a marked drift to one or the other ideal type is lacking (cf. Li & Thompson 1974 for a discussion of Chinese).

H. proposes a different pronciple to explain the distribution of word order types, namely his "Principle of Cross-Categorial Harmony" (CCH). The CCH states that there is a tendency for the ratio of preposed to postposed modifiers to generalize from one phrasal category to the others, i.e. from one head category to the other head categories. For example, it states that a language with SVO order (ratio preposed/postposed V modifiers = 1) tends to have AN/NG or NA/GN order (ratio of preposed/postposed N modifiers = 1). If two head categories have an equal ratio of preposed to postposed modifiers, they are said to be "harmonic" to each other.

It is important to note the theoretical relation between the CCH and the NSP. Both theories can be evaluated and compared by the sets of languages which are preferred according to them. Now, the NSP set clearly is a proper subset of the CCH set. This is because every type which is consistent according to the NSP is harmonic according to the CCH: Consistent post-specifying languages have a ratio of preposed to postposed modifiers equal to 0, and consistent prespecifying languages have a ratio equal to ∞ for all head categories.

Furthermore, the languages of the NSP set are the *only* maximal harmonic languages for the CCH if one considers the basic order of S, V, O, of A, G, N and of P, N. This is simply because there is only one type of adposition modifier, namely N. H. fails to make this relation between the CCH and the NSP clear enough.

Using the Greenberg samples and an expanded sample of his own with 336 languages, H. tries to test the CCH and the NSP. He measures the degree of harmony of a pair of head-modifier orders by counting the deviations from the nearest maximal harmonic pair. For example, PN & VSO exhibits 0 deviations, PN & SVO exhibits 1 deviation, and PN & SOV exhibits 2 deviations. With language types defined by word orders of more than two head categories, H. sums up the number of deviations of each combination of two head categories. Let me give three examples using H.'s handy abbreviations V_1 for VSO/VOS, V_2 for SVO/OVS, V_3 for SOV/OSV, and N_1 for NA & NG, N_2 for AN & NG/NA & GN, N_3 for AN & GN. Type PN & V_1 & N_1 is maximal harmonic, type PN & V_1 & N_2 exhibits two pairs of 1 deviation, totaling 2 (namely, PN & N_2 and V_1 & N_2), and type PN & V_1 & N_3 exhibits two pairs of 2 deviations, totaling 4 (namely, PN & N_3 and V_1 & N_3). Then the prediction is that if a language type T has fewer deviations from its nearest maximal harmonic type than a language type T' has from its nearest maximal harmonic type, then T has more instantiations than T'. To measure the success of the CCH, H. simply

determines with each pair (T, T') of a given typology which of the two types has more instantiations in the sample. If T has more than T', this counts as a point in favor of the CCH; if T has fewer, this counts as a point against the CCH.

Note that there is a certain asymmetry implicit in this procedure. If we have three types T, T' and T'' with decreasing harmony, then there are 3 elementary predictions (if we refer to the number of instantiations of a type T with #(T), these predictions are (i) #(T) > #(T'), (ii) #(T') > #(T'') and (iii) #(T) > #(T'')). Now (iii) follows from (i) and (ii) because of the transitivity of the >-Relation. H. decides to include (iii) in the evaluation because this procedure reflects the intuition of the CCH more accurately. This simply has the consequence that, if a very harmonic type (e.g., T) has very few instantiations (e.g., fewer than T' and T''), then this would have more disastrous effects on the CCH than if a medium harmonic type (e.g., T') has very few instantiations (e.g., fewer than T'').

H. calculates the number of correct predictions for different pairs and triples of head constituents and shows that the CCH allows for a high ratio of correct predictions. Then he tests the NSP by the following procedure: He counts the number of deviations of head-modifier orders from the serialization of the majority of head-modifier constructions, i.e. from the nearest consistent serialization. For example, type NP & SVO & NA & NG differs in two head- modifier orders from the nearest consistent serialization PN & VSO & NA & NG, namely in NP and SV.

The result is that the CCH does remarkably better than the NSP. In the most important assessment of the three-termed typology of verb, noun and adposition modifiers, out of 92 possible elementary predictions, 88 are correct with the CCH and 72 are correct with the NSP, using the data of Greenberg's Appendix II. Furthermore, most exceptions to the CCH (and to the NSP!) can be traced back to the well-known fact that verb-initial languages are comparatively rare because of the independent tendency to have sentence-initial subjects.

H.'s method of measuring the deviations from harmonic and consistent types strikes me as a bit arbitrary and not really comparable. There are equally, or even more, plausible methods in the same vein which will yield a rather different picture. For example, one could assume three serialization types for every head category: modifier-initial (i.e. V_1, N_1, and P_1), modifier-intermediate (i.e. V_2, N_2, and P_2), and modifier-final (i.e. V_3, N_3, and P_3). This would do away with the problem of adposition modifers, which constantly poses conceptual problems in chapter 4 (there is only one

adposition modifier, in contrast to two or more verb or noun modifiers). Of course, if one considers only the basic order of unilateral adpositions and neither doubling structures non circumpositions, the category P_2 has no members.

In this representation, a type in which all elements have the same indices would be maximally harmonic, and it is easy to measure the deviation of a given type from the nearest harmonic type. For example, type P_1 & V_1 & N_2 would be one step away from the nearest harmonic type (namely P_1 & V_1 & N_1), and type P_1 & V_2 & N_3 would be two steps away from the nearest harmonic type (namely P_2 & V_2 & N_2). To test the NSP, one would have to count the deviations from the nearest consistent types P_1 & V_1 & N_1 and P_3 & V_3 & N_3, of course.

Table I. Comparing CCH and NSP predictions using two methods.

Language type	CCH'	NSP'	CCH*	NSP*	Languages in Expanded Sample
	Number of deriations				
P_1 & V_1 & N_1	0	0	0	0	38
P_3 & V_3 & N_3	0	0	0	0	96
P_1 & V_1 & N_2	2	1	1	1	13
P_1 & V_2 & N_1	2	1	1	1	56
P_1 & V_2 & N_2	2	2	1	2	21
P_3 & V_3 & N_2	2	1	1	1	55
P_3 & V_2 & N_3	2	1	1	1	12
P_3 & V_2 & N_2	2	2	1	2	13
P_1 & V_1 & N_3	4	2	2	2	1
P_1 & V_3 & N_1	4	2	2	2	10
P_1 & V_2 & N_3	4	2	2	3	7
P_1 & V_3 & N_2	4	2	2	3	0
P_1 & V_3 & N_3	4	1	2	2	2
P_3 & V_3 & N_1	4	2	2	2	11
P_3 & V_1 & N_3	4	2	2	2	1
P_3 & V_2 & N_1	4	2	2	3	0
P_3 & V_1 & N_2	4	2	2	3	0
P_3 & V_1 & N_1	4	1	2	2	0

Table I shows H.'s calculations (the CCH' and NSP' columns) and the calculations according to the second method (the CCH* and the NSP* columns). The numbers of languages are from H.'s "Expanded sample".

For CCH', there are $(2 \times 16) + (6 \times 10) = 92$ predictions, out of which 90 are correct (97.8%). The same holds for CCH*; the two procedures have no effect on the measure of deviations. For NSP', there are 92 predictions, out of which 75 are correct (81.5%). NSP* is the strongest hypothesis; it makes $(2 \times 16) + (4 \times 12) + (8 \times 4) = 112$ predictions, out of which 105 are correct (94%). Thus, NSP* does quite a lot better than NSP', and the difference to the CCH'/CCH* should hardly be significant enough to prefer one or the other theory.

Actually, it is possible to show by standard means that H.'s data do not allow to prefer the CCH to the NSP. If one considers only basic word orders of the discussed types, it is just the ordering of N and V modifiers which plays a role in comparing the CCH with the NSP because only N and V modifiers allow an intermediate head position (i.e., N_2 and V_2). The CCH differs from the NSP in predicting that there is a positive interaction between N_2 and V_2, i.e. that type N_2 & V_2 has more than chance frequency. This allows a simple chi-square test, with the NSP (no interaction) as null hypothesis and CCH as alternative (positive interaction between N_2 and V_2). In Table II, I show the contingence tables for the Expanded Sample.

It is easy to see that these figures do not entitle us to reject the null hypothesis. We have a chi-square value of 0.05, which is far too little to reach any level of significance (with 1 degree of freedom, this would be 3.84 for the $\alpha = 0.05$ level). Compare this with the very clear result when we test the two

Table II

Contingency tables to test CCH's prediction of a positive interaction between V_2 and N_2 order against the null hypothesis. The chi-square-value is 0.05, far less than the critical value 3.84 for a level of significance of 0.95. The null hypothesis is not to be rejected.

(i) Actual scores

	V_2	$\neg V_2$	
N_2	34	68	102
$\neg N_2$	75	159	234
	109	227	336

(ii) Expected scores

	V_2	$\neg V_2$
N_2	33.1	68.9
$\neg N_2$	75.9	158.1

predictions which the NSP and the CCH share, namely that there is a positive interaction between V_1 and N_1 order, on the one hand, and V_3 and N_3 order, on the other. Tables III and IV show the relevant contingency tables.

Table III

Contingency tables to test NSP's and CCH's prediction of a positive interaction between V_1 and N_1 order against the null hypothesis. The chi-square-value is 38.68, much more than the critical value 3.84 for a level of significance of 0.95. The null hypothesis is to be rejected.

(i) Actual scores

	V_1	$\neg V_1$	
N_1	38	77	115
$\neg N_1$	15	206	221
	53	283	336

(ii) Expected scores

	V_1	$\neg V_1$
N_1	18.1	96.9
$\neg N_1$	34.9	186.1

Table IV

Contingency tables to test CCH's prediction of a positive interaction between V_3 and N_3 order against the null hypothesis. The chi-square-value is 69.0, much more than the critical value 3.84 for a level of significance of 0.95. The null hypothesis is to be rejected.

(i) Actual scores

	V_3	$\neg V_3$	
N_3	98	21	119
$\neg N_3$	76	141	217
	174	162	336

(ii) Expected scores

	V_3	$\neg V_3$
N_3	61.6	57.4
$\neg N_3$	112.4	104.6

To settle the question between NSP and CCH, it would be important to perform the calculations with more than just one middle position in the basic order of V and N modifiers. H. makes an interesting attempt to do so in taking into account four verb positions (namely V_1 (VSO/VOS), V_2 (SVO), V_3 (SOV non-rigid) and V_4 (the rigid SOV-subtype)) and four noun positions ($N_1 - N_4$, according to the relative ordering of adjective, genitive noun and relative clause to the noun). This test yields better scores for the CCH, but H. tests it only on the 30 languages of Greenberg (1966), which is too small to give reliable results. Another improvement would be to consider doubling structures. This could be especially instructive for adposition modifiers because by this way one gets intermediate positions for this phrasal category, too.

3. Greenberg's universals reformulated

The aim of chapter 3 is to formulate a set of universals which should hold without any exception for human languages. H. stresses that these universals are of a quite different nature than the CCH (cf. p. 163ff.). The CCH, he argues, only tells us which word orders are preferred to others, but it does not exclude any of them. H., therefore, calls it a "distributional" universal. The "non-statistical" universals, on the other hand, are of a different nature because they actually divide possible from impossible languages. As they are formulated as implications, they are termed „implicational" universals.

H. achieves a more rigid reformulation of many of the Greenberg implicative universals by two methods (cf. also Hawkins 1979, 1980): (i) He tries to find antecedents which yield as few exceptions as possible. For this, modifier-peripheral word orders like PN/NP or V_1/V_3 yield the best results, whereas an intermediate word order like V_2 (i.e. SVO) is shown to be practically devoid of consequences for other word order patterns. (ii) More important, H. mostly uses three-termed implications (i.e., $P \rightarrow (Q \rightarrow R)$), whereas Greenberg only uses two-termed implications (i.e. $P \rightarrow Q$). But note that at least Greenberg's universal 5 is formulated in the same manner: "If a language has dominant SOV order and the genitive follows the noun, then the adjective likewise follows the noun", i.e. SOV → (NG → NA). By the way, I think that Greenberg's formulation of the universal (i.e., as of the form $(P \& Q) \rightarrow R$) is more perspicuous than Hawkins' (i.e., as $P \rightarrow (Q \rightarrow R)$) because it doesn't suggest any asymmetry between the properties P and Q.

All in all, H. formulates some 22 universals. Consider one example (Universal I, p. 64): "If a SOV language has the order adjective-noun, then it also has the order genitive-noun", i.e. SOV → (AN → GN). If one restricts

the discussion to SOV languages, there are four logical possibilities for a language, the last one being excluded by the universal: (i) AN & GN, (ii) NA & GN, (iii) NA & NG and (iv) *AN & NG. If one considers language frequency, type (i) has the most instantiations (93 in the Expanded sample), type (ii) and (iii) have fewer (55 and 10, respectively), and type (iv) has no instantiations at all. The decreasing number of instantiations of types (i) to (iii) can be traced back to the CCH (or to the NSP). This is not possible for the lack of examples for type (iv) because, according to the CCH/NSP, it should be equally well represented as type (ii), and better represented than type (iii).

I think that H.'s conception of two quite different sorts of universals is interesting, especially for the theory of word order change (cf. below). But I doubt that H. has achieved showing that his "non-statistical" universals are really all "non-statistical" and relevant to the study of language (cf. Mallinson & Blake 1981: p. 411 for a similar criticism). In many cases, the difference between the excluded type (iv) and the possible, but disfavored type (iii) is very small or even nonexistent. For example, for Universal (IX), PN → (NG → NRel[ative clause]), type (iii), i.e. PN & GN & RelN, has only one instantiation (Amharic). And there are cases where both (iii) and (iv) have no instantiations at all, for example Universal (XVI), NP → (Num[eral]N → GN). This makes it doubtful whether H.'s distinction between statistical and non-statistical universals is really tenable for all his implicational universals. On the other hand, there are several occasions where H. finds instantiations of forbidden types. One reaction to this is to sharpen the formulation of the universal. For example, universal (III), PN → (NA → NG), has four counterexamples, namely Arapesh, Gitua, Karen and Kaliai-Kove. H. therefore strengthens the universals to (III'), (PN & ¬ SOV) → (NA → NG), which has no exceptions in the sample. But it is unclear whether such complex implications are of any interest for the study of language. One can easily conceive that they just describe the sample in an ad hoc manner and don't have any theoretical status at all.

4. Explanations

H. formulates some principles to explain the non-statistical universals as well as the CCH/NSP. Let's begin with the former.

There are two interesting patterns in the distribution of the order of noun modifiers: (i) Some noun modifiers show a tendency to be postposed when compared with other ones, independent of the overall direction of serialization predicted by the CCH/NSP. A relative clause is more commonly postposed than a genitive noun, a genitive noun more commonly than an adjective and an adjective more commonly than a demonstrative or numeral. (ii) Some noun modifiers show a tendency to be ordered in the opposite direction to the overall direction of serialization predicted by the CCH/NSP than others. The numeral, demonstrative and adjective are more commonly found on the opposite side of N than the genitive noun or the relative clause are. Note that (i) behaves asymmetrically to the CCH/NSP, whereas (ii) is symmetrical to it.

Observation (i) is easily explained by the well-known tendency of heavy constituents to occur late in the sentence. H. proposes a "Heaviness Hierarchy", namely Rel ≥ Gen ≥ Adj ≥ {Dem/Num}, and a "Heaviness Serialization Principle" (HSP), which says that heavier constituents exhibit more rightward positioning relative to the head noun across languages. Heaviness is defined in terms of length and quantity of morphemes, quantity of words, syntactic depth of branching nodes and inclusion of dominated constituents. H. offers for the HSP the standard explanation in psycholinguistic terms involving language processing.

To explain observation (ii), H. formulates a "Mobility Principle" (MP) which says that demonstratives, numerals and adjectives exhibit greater or equal mobility away from the adposition + NP serialization. H.'s arguments for the MP can be traced back to language change: H. claims that more mobile constituents have a greater tendency to acquire a new word order pattern than less mobile constituents, and this is reflected by synchronic word order patterns which are a snapshot of changing word orders. The reason for the behavior of mobile constituents is the following: The more mobile constituents are single (nonbranching) and nonphrasal, whereas the less mobile ones are branching and phrasal. Based on this, H. makes two assumptions: (a) As relative clauses and genitive NPs can dominate demonstratives, numerals and adjectives, the reordering of relative clauses and genitive NP presupposes the reordering of demonstratives, numerals and adjectives. (b) The reordering of demonstratives, numerals and adjectives causes fewer reorderings of surface elements and should therefore precede the reordering of genetive NPs and relative clauses. H. shows that this explanation can account for other cases, too. An example is the behavior of simple adverbial modifiers of adjectives in comparison to the behavior of PP and NP modifiers of adjectives (the former are more mobile than the latter).

I would like to consider another explanation for some aspects of the MP. As far as the order of adjectives and genitive NPs is concerned, the MP is based on universals (III), PN → (NA → NG), and (IV), NP → (AN → GN). (III) says that in prepositional languages, the order NG is a precondition for the development of the order NA. (IV) says that in postpositional languages, the order GN is a precondition for the development of the order AN. This can be interpreted by assuming a closer affinity between the order of adposition modifiers and genitive noun modifiers than between adposition modifiers and adjectival noun modifiers (and similar for demonstrative and numeral noun modifiers). This would reflect the well-known phenomenon that adposition phrases can often be explained as grammaticalizations of genitive constructions (cf. below).

Now we turn to the explanation of the CCH/NSP. H. suggests that this principle shows that there is a syntactic-semantic parallelism between the verb and its modifiers, the noun and its modifiers, the adposition and its modifiers and the adjective and its modifiers (p. 180). Especially for the CCH, he claims the existence of "some form of analogy principle", namely that "the operator preposing and postposing balance within one category generalizes to another as a result both of the operator-operand [i.e. modifier-modified] relation linking the two categories and of a natural tendency observable throughout languages for like elements to be treated in a like manner" (p. 182).

I understand this as a claim that the ultimate reason for the CCH/NSP is a semantic one, as is the ultimate reason for any proportional analogy (cf. Krifka 1983 p. 255ff. for the semantic motivation of phenomena of analogy). It is the semantic similarity between modifier-modified constructions of different head categories which leads to a similar encoding.

But H. switches without further discussion of this point to a syntactical-based explanation of the CCH/NSP. He introduces X-bar-theory (cf. Chomsky 1970, Jackendoff 1977, Lightfoot 1979), which makes a syntactical distinction between modifiers and heads, just like some other syntax theories like dependency grammar (Tesnière 1959) or categorial grammar (Vennemann 1976, 1977). X-bar-theory assumes two general phrase structure rules, namely (1.a) and (b), where "X" is a variable for a head category, "Spec X'" a specifier of X, and Comp a complement (argument) of X.

(1) a. X" → (Spec X') X' b. X' → X Comp

A harmonic language would allow very general phrase structure rules like (1.a,b) to cover the order of noun modifiers, verb modifiers, adposition modifiers and adjective modifiers. Its syntax would be simpler as and preferred to a syntax with different rules for the modifiers of different head categories. Furthermore, the distinction between two modifiers categories (specifiers and complements) might make it possible to describe languages with mixed serialization directions which do not obey the NSP, but do obey the CCH.

H. shows that it is not so esay to map the structural properties of the X bar theory immediately to the distributions of word order patterns in the languages in the sample. However, he comes to the conclusion that the prospects that X-bar-theory may explain the CCH/NSP are good enough to pursue this way further. Specifically, he thinks that, on the basis of X-bar-theory, one can at least distinguish between more marked and less marked word order patterns and that this distinction should be reflected in the frequency of languages exhibiting these patterns.

All in all, I found H.'s discussion of possible explanations of the CCH/NSP phenomena quite unsatisfactory. He only discusses some versions of X-bar-theory at some length. But there are more theories to explain the word order correlations. One example is Lehmann's (1973) principle that noun modifiers should not intervene between the object and the verb (see below). Another is the postulation of some sort of analogy which is mentioned, e.g., by Greenberg (1966). Greenberg also points to the well-known diachronic affinity between the order of genitive noun and head noun, on the one hand, and the order of the noun phrase and the adposition, on the other. There are many cases where the former construction changes to the latter (e.g. *back of the house*). There is a similar diachronic affinity between the order of object and verb, on the one hand, and the order of the noun to the adposition, on the other: In many languages, adpositions are derived from verbs (e.g. *concerning the proposal*). These affinities could play an important role in the development of cross-categorial word order patterns (cf. f.e. Lehmann 1972, Mallinson & Blake 1981: p. 383ff.)

I have some doubts as to whether a purely syntactical approach will lead to an explanation of the CCH/NSP. This is because there seem to exist a lot of serialization phenomena which do not belong to the core of syntax at all, but which, nevertheless, obey the CCH/NSP. For example, Lehmann (1978) discusses the order of family name and given name, of name and title and of additive numerals; Krifka (1983), furthermore, mentions the serialization of postal addresses. Let us look just at the serialization of personal names. There is a tendency that, in postspecifiying languages, the order

normally is given name – familiy name (cf. Gaelic *Seán MacGabgabb*), and in prespecifying languages, family name – given name (cf. Japanese *Nakasone Taro*). I assume that the order of family name to given name is too marginal as to be covered by the central principles which are proposed by X-bar-syntax. On the other hand, there is independent evidence that the family name is considered as a modifier to the given name (cf. the genitival construction of nobiliy names like German *Otto von Wittelsbach*). This semantic relation of modification could have been the basis for an analogical extension from more central word order patterns to the peripheral ordering of personal names.

It might be questioned whether the semantic similarity between, say, the relation of the object to the verb and the relation of the adjective to the noun is vivid enough to enable any analogical generalization. I think that the relation of modification is in fact quite evident and psychologically real. For example, a red house will be considered as a special case of a house, not as a special case of red. To hear a dog will be considered as a special case of hearing, not as a special case of a dog. And to be under a table will be considered as a special case of to be under something, not as a special case of a table. This is basically the argumentation of Vennemann (1972), who furthermore considers other modification relations, like the one between modal verb and main verb. The assumption of analogy as the reason of the distributional universals may have another explanatory advantage. As I have shown, the language samples of H. indicate that there is a correlation between the serialization of head-modifier orders of different phrasal categories in case these orders are strictly prespecifying or postspecifying, but do not warrant the claim that there is a correlation in case the serialization is not strictly prespecifying or postspecifying. The reason for this may be that only a strict prespecification or postspecification in one phrasal category yields a basis vivid enough to encode head-modifier relations by the same word order in other phrasal categories as well. On the other hand, according to X-bar-theory with different modifier types like "Spec X'" and "Comp", the serialization of head-modifier constructions of different phrasal categories should correlate even in languages which are not strictly prespecifying or postspecifying.

Another explanation can be obtained from Lehmann (1973). Lehmann introduces a principle that a language tends to have no intervening elements between the object and the verb. Thus, object modifiers tend to be placed on the opposite side of the verb, and sentence mood markers tend to be placed on the opposite side of the object. This yields structures like (2.a) and (b), which are typical word orders for prespecifying and postspecifying languages.

(2) a. Object Modifiers – Object – Verb – Sentence mood marker

b. Sentence mood marker – Verb – Object – Object Modifiers

It is possible to give a more general explanation by assuming that, whenever a complex constituent consisting of a head h and a modifier m is itself a modifier to a head H, then there is a tendency for m not to intervene between h and H. That is, the orders [m h] H and H [h m] would be preferred, and the orders [h m] H and H [m h] dispreferred. This principle can explain all the phenomena the NSP was intended to cover. It could by itself be motivated by the well-known tendency to avoid center embeddings because they pose processing difficulties (cf. Kuno 1974 for a similar explanation concerning relative clause order, and Mallinson & Blake 1981: 387 concerning adposition order).

5. The Laws of Language Change

In chapters 5 to 7, H. draws on the discussion of synchronic word order universals to gain principles for word order changes and the historical reconstruction of languages.

H. interpretes the non-statistical word order universals as giving strict limitations on possible word order types in the present as well as in the past. He explicitly formulates a principle of "universal consistency in history" (UCH), which says that "at each state in their historical evolution, languages remain consistent with implicational universals derived from current synchronic evidence" (p. 211). I think that this central and often neglected principle would have deserved a broader discussion than the one given by H. (cf. Christy 1983 for a history of "uniformitarianism" in the paleological sciences and in linguistics).

There are two lines of reasoning. If one assumes that some language universals are determined mainly by innate principles, then it seems to be clear that these universals should have undergone major changes during the biological evolution of man. Having no direct access to previous stages of human language in this sense, we can only speculate about this. But it would be important to restrict the UCH to the biologically late stages of mankind. On the other hand, if one assumes that some language universals are deter-

mined mainly functionally by the needs a language has to fulfill in a speech community, then it is easily conceivable that language changes with cultural development. Because cultural development is much faster than biological evolution, and because it exhibits quite a different pace in different geographical regions, we would have access to different stages of human language, and we would have to relativize the relevant language universals to those stages. There are some recent speculations about historical changes of preferred grammatical structures in these terms; cf., for example, Givón (1979) for the change from prespecifying to postspecifying word order, and Sasse (1978), Plank (1979) and Thomas T. Ballmer (pers. comm.) for the change from ergative to accusative languages.

The implicational form of non-statistical language universals leads to an interesting consequence for the study of language change: They constrain the possible pathways in the gradual transition from one historical stage to another. For example, a synchronic implicational universal of the form $P \rightarrow Q$ says that the occurrence of Q enables the occurrence of P, i.e. that a language first has to acquire Q before it can acquire P, or that it has to acquire Q and P simultaneously. And a three-termed implicational universal of the form $P \rightarrow (Q \rightarrow R)$ (equivalent to $(P \& Q) \rightarrow R$) says that the occurrence of R enables the joint occurrence of P and Q.

On the basis of these considerations, H. formulates hypotheses about the acquisition of doubling structures, i.e. about the development of mixed word orders like AN and NA. The strongest hypothesis is the "Frequency Increase Hypothesis" (FIH), which says that in case there is a synchronic word order universal $P \rightarrow Q$, then the following holds: If the frequency of Q structures is less than 100% (i.e., $\neg Q$ structures exist), and if there is an increase in the frequency of P structures relative to their doublets $\neg P$, then the frequency of Q structures will increase, too.

Note that the FIH does not exactly follow from the formulation of the synchronic universal $P \rightarrow Q$ because this formulation is only concerned with basic word orders and not with doubling structures. The FIH draws on the hypothesis that word order change proceeds by the acquisition of doubling structures, that is, a change from P to $\neg P$ is mediated by stages where P structures as well as $\neg P$ structures occure. Surely, this is a correct assumption. It would still be interesting to formulate explicit synchronic universals for doubling structures; in this way it should be possible to constrain the pathways of word order change even further.

H. tests the FIH successfully on data from early Indo-European dialects and from the development of English and German. To cite just some examples from the development of New High German, consider the fact that in Early New High German there was an increase of prenominal genitives and relative clauses, of postpositions and of SOV order (in dependent clauses) (cf. Lehmann 1972). These developments are consistent with the FIH and the synchronic universals RelN → GN and RelN → (NP ∨ SOV).

H. then criticizes the theory of word order change proposed by Lehmann and Vennemann. He interpretes Vennemann's conception as a trigger chain theory implying the violation of synchronic universals, and he gives some arguments opposing this view. I think that H. has fundamentally misrepresented Vennemann's intentions. H. simply considers the NSP – that languages tend to put all modifiers before their heads or to put all modifiers after their heads – as a universal of the implicational kind. Then the following scenario would hold: A language fulfilling the NSP is disturbed by some internal or external influence. It looses consistency, that is, violates the NSP. Therefore, a pressure is exerted to regain consistency and end the state of violation of a universal. This pressure causes the language to evolve consistent word order patterns once again.

I think it is obvious that the NSP basically describes which word orders are preferred and which are not preferred, and was not conceived as an implicational universal of the usual kind (cf. Vennemann 1983), in spite of the fact that some of Vennemann's formulations may invite a misrepresentation of it in this way. It should be applied to explain word order changes in quite a similar fashion to the way H. himself applies the CCH in chapter 6. That is, a language which is not in a preferred state as defined by the NSP is under certain pressure to change towards more preferred states, but does not violate a universal.

The major differences between Vennemann's theory and H.'s theory are, I think, (i) they assume different preference principles for word order, namely the NSP and the CCH, and (ii) Hawkins, furthermore, formulates restrictions for the possible pathways of word order changes by a set of non-statistical, implicational universals.

H. also discusses some possible triggers which may cause a language to deviate from a more preferred type in terms of the CCH/NSP to a less preferred type. He criticizes Vennemann's assumption that loss of case marking should be a major factor for a language developing from SOV to SVX (cf. also Sasse 1977 for a similar criticism). Then he lists some other possible triggers, for example, language contact, grammatical reconstrution and grammaticalization of afterthoughts (cf. Hyman 1975 for the latter). More importantly, he stresses that there may be a lot of possible causes (which is a

common situation in the historical sciences). H. considers the implicational universals just as some sort of "upper bound" which imposes a maximal frame onto possible historical changes.

Concerning the relation between word order change and the synchronic distribution of word orders, H. assumes a "Relative Time Hypothesis". It says that, given two types T and T' with T more preferred than T' by the CCH/NSP, then any single language (and all languages combined) will have been exhibiting type T for a greater time span than type T' in its (their) history.

I think that this hypothesis is just mere speculation. There is evidence that one of the languages with the longest recorded history, Chinese, has been oscillating during the last three thousand years between a mildly prespecifying and a mildly postspecifying type (cf. Li & Thompson 1974), and it is easily conceivable that this process is repeated again and again. Furthermore, there is some evidence (or at least discussion) that the consistent prespecifying type was even more numerous in the times of the past than it is now (cf. Givón 1979). Finally, word order is sensitive to areal influences, and therefore it is conceivable that the actual distribution of word order types is just a result of a self-reinforcing process caused by a few languages of high prestige (cf. P. Schmidt 1927, Hyman 1975 and Bechert 1976 for a discussion of the geographical distribution of word orders).

6. Historical Reconstruction

One major application of word order universals is the historical reconstruction of language. H. gives some thought to it in chapter 7, using examples from the reconstruction of Late Common Germanic, Proto-Indo-European and Proto-Bantu.

H. pleads to acknowledge synchronic universals in language reconstruction, a view that goes back at least to Jakobson's criticism of the reconstruction of Proto-Indo-European phonology. He develops some maximes for the application of synchronic evidence in the study of language change.

His first principle is called "deductive inference"; it says that given a universal P → Q and there is evidence that a language has property P (¬Q), then Q (P) can be assumed for the language by modus ponens (modus tollens). H. exemplifies this by reconstructing postnominal relative clauses (NRel) for Late Common Germanic out of the attested postnominal demonstratives (NDem) and possessives (NPoss) and the synchronic universals NDem → NRel and NPoss → NRel.

A second principle, the "reconstruction of doubling innovations", is concerned with the problem of determining the relic and the innovation of a pair of competing word orders. Given a universal P → Q, then if a language has P & Q/¬Q word order, ¬Q is the relic, and if a language has P/¬P & Q word order, then P is the relic. H. exemplifies this again with Late Common Germanic, which exhibits NPoss and NG/GN; by the universal NPoss → NG, the order GN is shown to be the relic and NG the innovation.

The next principle, called "the logic of competing variants", is to reconstruct those features for a proto-language which are most compatible (in terms of synchronic universals) with the attested daughter languages. Imagine a situation where some of the daughter languages have property P_i and the others its converse P_j (i.e., $P_j = \neg P_i$). Given a universal U_i permitting P_i to co-occur with the properties of all of the daughter languages and a universal U_j permitting P_j to co-occur with properties some of which occur in the daughter language and some not, then one should reconstruct P_i as the property most compatible with the daughter languages.

H. illustrates this principle with the controversial question of Proto-Indo-European word order (cf. Lehmann 1974, Friedrichs 1975). I consider here only the reconstruction of adposition order. There is a universal which allows prepositions to co-occur with types (i) to (iv) on the following scale, and postpositions with types (ii) to (iv).

(3)

i	AN &	NG &	NRel	
ii	NA	NG	NRel	prepositional languages
iii	AN	GN	NRel	
iv	AN	GN	RelN	
v	NA	GN	RelN	postpositional languages
vi	NA	GN	NRel	

Most of the early Indo-European dialects (9 out of 12) have prepositions, and only some have postposition. Furthermore, all 12 dialects are of a type between (i) to (iv), none being of type (v) or (vi). H. constructs this as an argument in favor of the preposition analysis of Proto-Indo-European.

There is a further precondition for the application of the principle of the logic of competing variants. It says that one should be able to motivate the acquisition of P_j out of P_i by the properties of the daughter languages which exhibit P_j, but not vice versa the acquisition of P_i out of P_j by the

properties of the daughter languages which exhibit P_i. H. illustrates this in his example by trying to show that an acquisition of postpositions out of prepositions in the postpositional dialects is better motivated than an acquisition of postpositions out of prepositions in the postpositional dialects. His strongest argument is that the four VSO/SVO-dialects are all prepositional, which is the normal situation, whereas among the seven SOV dialects, there are four prepositional and three postpositional ones, which is a quite unexpected distribution. If one assumes prepositions for Proto-Indo-European, this would be readily explainable: Under the pressure of SOV, some (but not yet all) dialects developed postpositions. On the other hand, if one assumes postpositions, it would be easy to explain why prepositions developed in the VSO/SVO dialects, but it is difficult to explain why most of the SOV languages should have developed prepositions. According to H.'s samples, only 7% to 8% of the SOV languages have prepositions, that is, there is a clear tendency against prepositions in SOV languges. Furthermore, prepositions are unlikely to be the spearheads of word order change.

With arguments like this, H. furthermore argues for VSO/SVO and NA/AN & NG & NRel in Proto Indo European. He does not think that these arguments "prove" Proto Indo European to have been of this type because there is the counterevidence that the two most early attested languages, Sanskrit and Hittite, are of type NP & SOV & AN & GN & RelN. But he thinks that these are good arguments in the controversial question of Proto Indo European word order. I agree with H. on this point.

H.'s last reconstruction principle is called "inductive inference". Given two word order pairs which differ in frequency, it says that the more frequent one is more likely the one represented in the proto-language. Furthermore, it should be exemplified in more of the daughter languages, and it should have been represented for a greater period in each single daughter language as well as in all the daughter languages combined.

As an example, H. uses the reconstruction of Proto-Bantu word order. Bantu languages are typically of word order type (i), SVO & PN & NA & NG & NRel. There are a few languages closely related to Bantu proper and situated on the periphery of the main Bantu area in the Cameroon grasslands (H. mentions Tunen and Bandem) which are of type (ii), SOV & PN & NA & NG & NRel. Heine (1976), impressed by the number of SVO languages, reconstructs SVO as the Proto-Bantu word order, whereas Givón (1971) and Hyman (1975) reconstruct SOV because of morphological and geographical evidence. I have argued for the latter reconstruction in Krifka (1983).

Led by his principle of "inductive inference", H. argues for the reconstruction of type (i): First of all, this type is about five times more frequent than type (ii), in the languages of the world, secondly, it assumes that only two of the daughter languages are of the rare type (ii), and thirdly, under this analysis the type (ii)-period of each single daughter language as well as the time span of all the daughter languages combined would be smallest.

I think it is problematic to use the principle of "inductive inference" in this schematic way. The first argument is by itself very weak. By applying it, one could "reconstruct" any unknown language to be of the most frequent type. Any argument pointing in another direction should overrule this one. For the second argument, it is important to use the concept of a "daughter language" in a controlled manner. Bantu proper consists of several hundred languages, whereas Grassland Bantu consists of some fifty languages; both are considered to be equally ranked branches of Bantu. If Grassland Bantu would have some rare property P and Bantu proper the more frequent property $\neg$P, then one should count only one P daughter language and one $\neg$P daughter language (and not 50 P daughter language and several hundred $\neg$P daughter languages). A similar objection holds for the third argument.

7. Statistic matters

I would now like to come back to my main point of criticism, namely [illegible] unsatisfactory employment of statistical arguments. I found it quite surprising that the editors of a series called "Quantitative Analyses of Linguistic [illegible]ucture" let a text with such serious statistical flaws pass by. I will mention [illegible] three point which are, I think, most problematic.

The first is the evaluation of the CCH against the NSP. I have shown [illegible] H. uses quite an arbitrary and obscure metric in defending the CCH [illegible]inst the NSP, and that it seems that the CCH cannot be argued for by [illegible]ndard statistical means on the basis of H.'s sample.

The second is the substantiation of the claim that the implicational [illegible]ersals are exceptionless, whereas the distributional universals only de[illegible]e statistical tendencies. There are many cases where a possible, but not [illegible]rred word order co occurence has only very few instantiations, and the [illegible]sible co occurence has none. The difference between very few occur[illegible] and none is in these cases without any statistical significance. There-

fore it seems impossible to found deep theoretical distinctions upon such slight differences.

Thirdly, H. gives virtually no thought to sampling problems. He uses three samples, namely the two well-known samples of Greenberg (1966) (that is, the 30 language sample and the sample of Appendix II with 142 languages and language families) and a sample of 338 languages compiled by himself on the basis of Greenberg's sample and additional information. H. admits that this sample is mostly a convenience sample, and he discusses some genetical biases: Altaic, Caucasian and Paleosiberian languages seem to be grossly over-represented, and Sino-Tibetan and most African and American language families grossly under-represented. (One should appreciate at least that H. names his sources and presents the facts in a clear manner).

I think H. would have done better to be satisfied with a smaller sample and to put some more work into the construction of a more representative one instead. This is because H.'s hypotheses bear heavily on the relative frequency of language types, and therefore the sample must be selected very carefully. Furthermore, it would have been important to have some more word order data for the sample languages, for example, whether a language has basically free word order, or whether an SOV language is of the rigid or of the non-rigid subtype. And it would have been interesting to have some figures of text frequencies. For example, in Krifka (1983) I have argued that both English and the Bantu languages are SVO, but in Bantu languages the frequency of verb-intitial sentences is much higher than in English (due to the marking of anaphoric subject on the verb). This could exert a higher pressure to have postposed modifiers in other categories, and indeed, we have a higher degree of postspecification in Bantu than in English.

In sampling methods, I can see no progress at all if one compares H.'s work with the pioneering and explorative study of Greenberg (1966). This strikes me as a very serious shortcoming of H.'s work, especially if one considers the growing sensibility for sampling problems among researchers in the field. For example, Bell (1978) developed some first guidelines for sample construction in the study of language universals, and Maddieson (1984) and Kefer (to appear) pay close attention to possible sample errors and are apt to restrict their samples where they suspect genetical or areal biases.

REFERENCES

Bartsch, R., Vennemann, Th. (1972), Semantic Structures. Frankfurt/M.: Athenäum.

Bechert, J. (1976), Bemerkungen zu Greenbergs 'Basis Order Typology'. Papiere zur Linguistik 10, 49–66.

Bell, A. (1978), Language samples. In J. H. Greenberg (Ed.), Universals of Human Language 1, 123–156. Stanford: Stanford University Press.

Li, Ch. N. (1975) (Ed.), Word Order and Word Order Change. Austin: University of Texas Press.

Chomsky, N. (1965), Aspects of the Theory of Syntax. Cambridge, Mass.: MIT Press.

Chomsky, N. (1970), Remarks on Nominalization. In R. A. Jacobs & P. S. Rosenbaum (Eds.), Readings in English Transformational Grammar. Waltham, Mass.: Blasdell.

Christy, C. (1983), Uniformitarianism in Linguistics. Amsterdam: John Benjamins.

Comrie, B. (1981), Language Universals and Linguistic Typology. Chicago, Oxford: University of Chicago Press.

Friedrich, P. (1975), Proto-Indo-European Syntax. Journal of Indo-European Studies, Monograph 1, Montana College of Mineral Science and Technology, Butte.

Givon, T. (1971), On the verbal origin of the Bantu verb suffixes. Studies in African Linguistics 2, 145–163.

Givon, T. (1979), On Understanding Grammar. New York: Academic Press.

Greenberg, J. H. (^{2}1966), Some universals of grammar with particular reference to the order of meaningful elements. In J. H. Greenberg (Ed.), Universals of Language (2nd ed.), 73–113. Cambridge, Mass.: MIT Press.

Hawkins, J. A. (1979), Implicational universals as predictors of word order change. Language 55, 618–648.

Hawkins, J. A. (1980), On implicational and distributional universals of word order. Journal of Linguistics 16, 193–235.

Hawkins, J. A. (1982), Cross-category harmony, X-bar and the predictions of markedness. Journal of Linguistics 18, 1–35.

Heine, B. (1976), A typology of African languages based on the order of meaningful elements. Berlin: Dietrich Reimer.

Hsieh, H.-I. (1977), Noun-modifier order as a consequence of VSO order. Lingua 42, 91–109.

Hyman, L. (1975), On the change from SOV to SVO: Evidence from Niger-Congo. In Ch. N. Li, 113–148.

Jackendoff, R. (1977), X-bar-Syntax: A Study of Phrase Structure. Linguistic Inquiry Monograph 2.

Keenan, E. L. (1976), Towards a universal definition of Subject. In C. N. Li (Ed.), Subject and Topic, 305–333. New York: Academic Press.

Keenan, E. L. (1979), On surface form and logical form. Studies in the Linguistic Sciences, Special Issue, Vol. 8, 1–41.

Kefer, M. (to appear), Satzgliedstellung und Satzstruktur im Deutschen und in der Universalgrammatik Tübingen: Niemeyer.

Krifka, M. (1983), Zur semantischen und pragmatischen Motivation syntaktischer Regularitäten. Eine Studie zur Wortstellung und Wortstellungsveränderung im Swahili. München: Wilhelm Fink.

Kuno, S. (1974), The position of relative clauses and conjunctions. Linguistic Inquiry 5, 117–136.

LEHMANN, W.P. (1972), On the rise of SOV patterns in New High German. In K.G. Schweisthal (Ed.), Grammatik, Kybernetik, Kommunikation (Festschrift für Alfred Hoppe), 19–24. Bonn: Dümmler.

LEHMANN, W.P. (1973), A structural principle of language and its implications. Language 49, 47–66.

LEHMANN, W.P. (1974), Proto-Indo-European Syntax. Austin: University of Texas Press.

LEHMANN, W.P. (1978), The great underlying ground plans. In W.P. Lehmann (Ed.), Syntactic Typology: Studies in the Phenomenology of Language, 3–55. Austin: University of Texas Press.

LI, Ch.N., & S.A. THOMPSON (1974), Historical change of word order: a case study of Chinese and its implications. In J.M. Anderson & C. Jones (Eds.), Historical Linguistics, 199–217. Amsterdam: North Holland.

LIGHTFOOT, D.W. (1979), Principles of Diachronic Syntax. Cambridge: Cambridge University Press.

MADDIESON, I. (1984), Patterns of Sounds. Cambridge: Cambridge University Press.

MALLINSON, G., & B.J. BLAKE (1981), Language Typology: Cross-Linguistic Studies in Syntax. Amsterdam: North Holland.

PLANK, F. (1979), Ergativity, syntactic typology and universal grammar: some past and present viewpoints. In F. Plank (Ed.), Ergativity. Towards a Theory of Grammatical Relations, 3–59. New York: Academic Press.

SASSE, H.-J. (1977), Gedanken über Wortstellungsveränderungen. Papiere zur Linguistik 13/14, 82–142.

SASSE, H.-J. (1978), Subjekt und Ergativ: Zur pragmatischen Grundlage primärer grammatischer Relationen. Folia Linguistica XII, 219–252.

SCHMIDT, P.W. (1927), Die Sprachfamilien und Sprachenkreise der Erde. Heidelberg: Winter.

TESNIERE, L. (1959), Eléments de syntaxe structurale. Paris: Klincksieck.

VENNEMANN, TH. (1972), Analogy in generative grammar: the origin of word order. Proceedings of the 11th International Congress of Linguists, Bologna 1972. Bologna 1975: Mulino.

VENNEMANN, TH. (1974), Topics, subjects and word order: from SXV to SVX via TVX. In J.M. Anderson & C. Jones (Eds.), Historical Linguistics, vol. 1, 339–376. Amsterdam: North Holland.

VENNEMANN, TH. (1975), An explanation of drift. In Ch.N. Li (Ed.), 269–306.

VENNEMANN, TH. (1976), Categorial grammar and the order of meaningful elements. In A. Juilland (Ed.), Linguistic Studies Offered to Joseph Greenberg on the Occasion of His Sixtieth Birthday, 615–634. Saratoga, Cal.: Anma Libri.

VENNEMANN, TH. (1981), Überlegungen zu einer Theorie der linguistischen Präferenzen. Klagenfurter Beiträge zur Sprachwissenschaft 9, 262–292.

VENNEMANN, TH. & R. HARLOW (1977), Categorial grammar and consistent basic VX serialization. Theoretical Linguistics 4, 227–254.