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# PROBLEMS OF METHODOLOGY AND EXPLANATION IN WORD ORDER UNIVERSALS RESEARCH*

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## 0. INTRODUCTION

Ever since the publication of Greenberg 1963, word order typologists have attempted to formulate and refine implicational universals of word order so as to characterize the restricted distribution of certain word order patterns, and in some cases have also attempted to develop general principles to explain the existence of those universals. The starting point of much of this work is the fact that the attested word order patterns are skewed in such a way that all the attested word orders comprise only a small fraction of the mathematically possible word orders, and among the attested word order patterns there is a preponderance of just a few patterns, while the others are rarely attested (cf. Hawkins 1983:2). Hawkins (1979, 1980, 1983, 1988, 1990a), building on the work of Greenberg, Lehmann (1973), and also the early work of Vennemann on the subject (1974a, 1974b, 1975), has attempted to make a set of universals that are exceptionless by incorporating three or more terms (variables) into the statement of the universals, as in (1) (Hawkins 1983:64):[1]

(1) If a language has SOV word order, then if the adjective precedes the noun, the genitive precedes the noun; i.e. SOV ⊃ (AN ⊃ GN).

These universals are statements of patterns observed in the database used. It is also possible to take the universals to be hypotheses about the nature of word order in all the languages of the world, and of possible human languages. As empirical hypotheses they are falsifiable, though since a universal with only a few exceptions is still significant, they are falsifiable to different degrees. If such a hypothesis is not falsified by the data, it is considered exceptionless and a true universal; if it is falsified by only a few languages, then it is considered a statistical universal. The former are used to define all and only the attested word order patterns, while the latter have been used in determining statistical preferences (frequencies) among attested word order patterns. For Hawkins and Vennemann the difference between the two (statistical vs. non-statistical) is of great theoretical importance.

Once the word order patterns and their frequencies are known, the next step is to attempt to explain why these particular patterns appear exclusively or with much greater than chance frequency in the languages of the world. The explanations for the word order patterns are based first of all on the exceptionlessness of at least some of the universals, and the near-exceptionlessness of the others. In the rest of this paper I will present, using data mostly from Sino-Tibetan languages, a number of problems with the databases used in many studies on word order universals, with the universals derived thereof, and with the explanations founded upon those universals. I will focus mainly on the work of John A. Hawkins because of the scope, complexity, and influence of his work.

## 1. COMPARABILITY

While recognizing that possible incomparability of the categories compared is a problem in typological studies, Hawkins states that ‘We are

going to assume that the categories of subject, object, verb, adjective, genitive, noun, adposition, etc. whose basic ordering we are going to study, are comparable across languages' (1983:11). It is assumed (following Greenberg) that 'semantic criteria will suffice to make the cross-linguistic equation' (ibid.). This means essentially using translation equivalents, regardless of the form of the actual manifestation of the meaning in a particular language. Much work in typology and description (e.g. Nichols 1986, Dryer 1986, C. Lehmann 1986, Van Valin 1985, 1986, Dixon 1980, LaPolla 1993a, 1995, 1997) has shown that comparability IS a problem, as the differences in marking type, semantics, finiteness, and information structure, etc. do influence word order. For example, a language that has not grammaticalized the syntactic category of subject will often have a relatively free word order based on pragmatics (usually information structure and/or animacy); a language that has grammaticalized a subject such that it is the patient of a transitive clause which is the unmarked subject may have a different word order from one which has grammaticalized a subject such that it is the agent of a transitive clause which is the unmarked subject. Assuming agent is equivalent to subject in all languages then is very problematic.

Most studies on word order take a sentence type with two full lexical NP's as the basic word order, and from this draw conclusions about the entire grammar of the language, but it has been shown that in natural discourse this type of sentence is relatively rare and marked (e.g., DuBois 1985, 1987; Lambrecht 1987, 1994; Hopper 1986, Jacobsen 1993). For example, Jacobsen (1993:267) reports that in counts of clause types in a set of Nootka texts, only 5.6% of all clauses had two or more arguments, the most common (non-quotative) clause types being V (42.6%), VS (31.2%), and VO (18%). Conclusions about the entire grammar based on an infrequently occurring word order pattern are then suspect. As Hopper has argued, 'there is no alternative in typological studies to a careful language-by-language study of textual occurrences of word order . . . Isolated and decontextualized sentences . . . have only a limited validity in typological studies . . .; without textual analysis, the data base for a typology is suspect' (1986:125).

There is also the question of how different types of information structure affect word order, either in terms of basic order or in terms of doubling. Principles such as Tomlin's (1986) 'Theme-First' principle are only valid for languages that have initial themes. It is the fact that the unmarked information structure in the sentences of a particular language is of a particular type that causes the theme to be in sentence initial position, and the fact of theme being in sentence initial position that causes the subject (if there is one) to be in initial position. This is not the case for all languages. Some languages (see for example Tomlin & Rhodes 1979, Tomlin 1986:130ff) have the theme at the end of the sentence and the focus at the beginning of the sentence. In this case the language will generally be verb initial and subject final. Other patterns of information structure are also possible. Aside from the position of the subject generally corresponding to that of the theme, the unmarked object position generally corresponds to the unmarked focus position for NPs, and a change in information structure can bring about a change in word order (LaPolla 1993b). In terms of doubling, very often the two variants of, for example, relative clause, adjective, or demonstrative position, are conditioned by information structure, or by semantic or phonological (e.g. length, 'emphasis') factors, or by finiteness, and these factors should be considered in doing cross-linguistic studies, and are particularly important to general theories of word order preferences. For example, Jordan (1969:27) mentions that in Cho (Hko) Chin the demonstrative can either precede or follow the noun, and says that demonstratives follow the noun when they 'refer to some object already mentioned'. In short, information structure has important consequences for the order of subject, object, verb, and other constituents, and sentence patterns in languages with different information structures may not be

directly comparable.[2]

Using the semantic equivalence criterion leads to two problems of classification: making distinctions where there may be none, and not making distinctions where there are differences. An example of the first type involves the nature of attributives in Sino-Tibetan. For many of the languages in the family there are no noun-like adjectives, only stative verbs. The word order (and morphology) of these stative verbs when modifying a noun is often the same as that of a relative clause and/or genitive. Universals treating them as two (or three) separate patterns then are questionable (see Dryer 1988a, Van Valin 1986 for discussion). In a number of Tibeto-Burman languages, there are two patterns for attributive 'adjective' placement: post-nominal and nominalized prenominal, the latter being equivalent to a relative clause. Often the former is used for simple adjectives, while the latter is used for more complex adjectivals. Compare for example (2a-b), from the Qiang language (LaPolla 1996).

(2) a. ɕtɕimi nɑ-tɕ mi
heart good-G person
'(a) good hearted person'

b. mi nɑ
person good
'(a) good person'

The relative formation itself is actually a reflection of the fact that in Tibeto-Burman, all N-N compounds where one N modifies the other have the order modifier-modified. So the relative-noun form of adjectival modification is based on a different principle than the N-ADJ order. Treating them as the same is then problematic.

The second type of problem is more serious, as it blinds us to valuable information we might be collecting on the types and distribution of the differences we are otherwise glossing over (see for example Nichols 1984, C. Lehmann 1986, Van Valin 1986). I will give two examples here. The first is the nature of relative clauses and adjectives as discussed in Van Valin 1986 and C. Lehmann 1986. Looking at the relationship between the finiteness of the 'adjective' or relative clause and its position vis-à-vis the noun, Van Valin and Lehmann come to similar conclusions: '(1) adjectives which are highly nominal in nature precede the noun they modify in OV languages, while those which are highly verbal in nature follow the noun they modify; (2) relative clauses which are non-finite and nominalized precede the noun they modify, while those which are fully finite and not nominalized follow the noun they modify' (Van Valin 1986:5). The structural parallel between phrase internal order and clause internal order is striking: noun-like adjectives and relative clauses precede the noun, as do plain noun modifiers, while finite adjectives and relative clauses follow the noun, as do predicative verbs in simple clauses with a stative verb.

The second example is from a comparison of Dryer 1988b and 1992. In his 1988 study of the position of negative morphemes in relation to the verb, Dryer did not differentiate among negative particles, negative affixes, and negative verbs. His conclusion was that negatives correlate significantly with the order of verb and object. In his 1992 study, he differentiated the three types and found that while the position of negative verbs and negative affixes correlates with the order of verb and object, the position of negative particles does not (see Dryer 1992:97-8, Dahl 1979). Using the semantic criteria alone would cause us to lump negative particles together with negative verbs and affixes. In Tibeto-Burman this would have serious consequences for word order studies, as negative particles almost always precede the verb, while negative auxiliary verbs invariably follow the main verb (and generally derive historically from negative adverb + auxiliary verb constructions).

The insights from these studies would be lost to those who lump all relative clauses, all adjectives, and all negatives together on the basis of translation equivalents, with serious consequences for the validity of their results. We can see from this that semantic criteria will not suffice to make the cross-linguistic equation, therefore in-depth work on individual

languages and detailed typologies of attested variations are necessary for any study of language universals.[3]

## 2. THE UNIVERSALS

Of Hawkins' twenty independently numbered implicational universals,[4] eighteen apply to Sino-Tibetan languages.[5] We will discuss each one in turn:

Universal I, SOV ⊃ (AN ⊃ GN), and the more general Universal I´, OV ⊃ (AN ⊃ GN), hold true for all of the verb-final languages of Sino-Tibetan (i.e., all of Tibeto-Burman except Bai and Karen), as all Sino-Tibetan languages have GN order. This universal is only a statistical universal, though, due to the existence of Tigre (SOV/PREP/NG/AN) (Campbell, Bubenik, & Saxon 1988:224). German and Dutch may also be exceptions to this universal (Hawkins 1985: 580; Campbell, Bubenik & Saxon 1988:215).

Universal III, PREP ⊃ (NA ⊃ NG), is only statistical for Hawkins, there being four exceptions, including Karen (Jones 1961), a prepositional Tibeto-Burman language, in his database. His non-statistical version of this universal, Universal III´, PREP & ~SVO ⊃ (NA ⊃ NG), is not relevant to Sino-Tibetan, as there are no languages that are PREP & ~SVO in the family, though Dryer (1991:450) gives Kilivila (PREP/VOS/NA/GN) and Garawa (PREP/VOS/NA/GN) as exceptions to this latter version of the universal.

Universal IV, POSTP ⊃ (AN ⊃ GN), holds true for the postpositional languages in Sino-Tibetan for the same reason that Universal I held for these languages: all Sino-Tibetan languages have GN order. This universal is violated by a non-Sino-Tibetan language, Nomatsiguenga (VSO/POSTP/NG/AN; Payne 1985:465), though, so is only statistical.

Universal V, PREP ⊃ (NDem ⊃ NA), finds a counterexample in Bai, a Sino-Tibetan language that has PREP/NDem/AN (Xu & Zhao 1984), and the more general Universal V´, NDem ⊃ NA (equivalently AN ⊃ DemN), finds counterexamples in Manipuri (Meitei; Chelliah 1997), Ao Naga (Gurubasave 1980), Langsu (Dai et al. 1991), as well as more than a dozen other languages within Tibeto-Burman that have NDem and doubling of AN/NA order.

Universal VI, PREP ⊃ (NNum ⊃ NA), again runs into trouble with Bai, which also has NNum order (Xu & Zhao 1984:24), and the more general Universal VI´, NNum ⊃ NA (equivalently AN ⊃ NumN) is violated by at least eight Tibeto-Burman languages, including Garo (Burling 1961), which was in Hawkins' database (the Expanded Sample).

Universals VII and VIII, two statistical universals Hawkins derives by transitivity from Universals III, V, and VI, are PREP ⊃ (NDem ⊃ NG), and PREP ⊃ (NNum ⊃ NA) respectively. The former finds exceptions in Bai (PREP/NDem/NNum/AN/GN; Xu & Zhao 1984) and Karen (PREP/NDem/NNum/NA/GN/NRel; Jones 1961, Solnit 1986, Dai et al. 1991), while the latter finds an exception in Bai. The non-statistical versions of these universals, which exclude SVO languages, given in a footnote by Hawkins (p. 128-9), (VII´) PREP & ~SVO ⊃ (NDem ⊃ NG) and (VIII´) PREP & ~SVO ⊃ (NNum ⊃ NG), are not relevant to Sino-Tibetan for the same reason Universal III´ is not relevant, though Dryer (1991:450) gives Kilivila (PREP/VOS/NA/GN) as an exception to Universal VII´.

Universal X, PREP ⊃ (NA ⊃ NRel), which is derived by transitivity from Universals III and IX (PREP ⊃ (NG ⊃ NRel), holds true for Karen, the only Sino-Tibetan language with PREP and NRel.

Universal XI PREP ⊃ (NDem ⊃ NRel), which is derived by transitivity

from Universals III, V, and IX, does not hold true for Bai, which has NDem and RelN (Xu & Zhao 1984:24, 52). Its more general form in Universal XI´, NDem ⊃ NRel (equivalently RelN ⊃ DemN), is violated by 29 other Tibeto-Burman languages, including such well-known languages as Tibetan (Jin 1983, Beyer 1992), Jingpho (Dai & Xu 1992), Lahu (Matisoff 1973), Yi (Gao 1958, Ma 1951, Chen et al. 1985), Lisu (Hope 1974), and Manipuri (Meitei; Chelliah 1997).

Universal XII, PREP ⊃ (NNum ⊃ NRel), which is derived by transitivity from Universals III, VI, and IX, also does not hold true for Bai, which has NNum and RelN (Xu & Zhao 1984:24, 52). The more general Universal XII´, NNum ⊃ NRel (equivalently RelN ⊃ NumN), is violated by 58 other Tibeto-Burman languages and dialects, including those mentioned as violations of Universal XI´.[6]

Universal XIII, PREP ⊃ (~SOV ⊃ NRel), finds counterexamples in both Chinese (Chao 1968) and Bai, which are SVO, but have RelN. It is also violated by a non-Sino-Tibetan language, Asia Minor Greek (Campbell, Bubenik & Saxon 1988:215), which has PREP/VSO/SVO/RelN order.

Universal XIV, PREP ⊃ ((NDem ∨ NNum ⊃ NA) & (NA ⊃ NG) & (NG ⊃ NRel)), was created by collapsing Universals II, V, VI, and IX into one universal. This is Hawkins' Prepositional Noun Modifier Hierarchy (PrNMH). As Universals II, V, and VI are all merely statistical, so is this universal.

Universal XV, POST ⊃ (DemN ⊃ GN), and Universal XVI, POST ⊃ (NumN ⊃ GN), are true for all of Sino-Tibetan for the same reason Universal I and Universal IV were true for Sino-Tibetan: all Sino-Tibetan languages have GN order, no matter what the order of the other constituents.

Universal XVII (POST ⊃ ((AN ∨ NA) & (RelN ∨ NRel)) is a non-universal, as none of the logically possible combinations of these elements is excluded.

Universal XVIII, POST ⊃ ((AN ∨ RelN ⊃ DemN & NumN) & (DemN ∨ NumN ⊃ GN)) is similar to Universal XIV, though it was created by collapsing Universals IV, V´, VI´, IX´, XI´, XII´, XV, XVI, and XVII into one universal. This is Hawkins' Postpositional Noun Modifier Hierarchy (PoNMH). As four of the universals that were collapsed into this universal are statistical, and one is a non-universal, then this universal is also statistical at best.

Universal XIX, PREP ⊃ (ADJADV ⊃ AMS), is relevant only to (Kayah) Karen, which has PREP/AdjAdv/AMS order, the only Sino-Tibetan language to have PREP/AdjAdv order. In his discussion of the universal, Hawkins (p. 87) says there were no examples of PREP/AdvAdj/SMA order in his sample, yet if what is significant is the order of standard and attribute (cf. Dryer 1991:446), then Mandarin Chinese and Bai are both examples of this type.

Universal XX, POST ⊃ (ADVADJ ⊃ SMA), holds for all of the postpositional Sino-Tibetan languages, regardless of the order of adverb and adjective, as all have SMA order in comparative constructions, though Payne (1985:465) gives Yagua (AdvAdj/ASM) as an exception to this universal.

Of the two universals not relevant to Sino-Tibetan (II and IX), Universal II, V-initial ⊃ (NA ⊃ NG), is made statistical by the existence of Kilivila (VOS/NA/GN), Garawa (VOS/NA/GN), Yagua (VOS/NA/GN), and Guajajara (VSO/NA/GN) (Dryer 1991:450; Payne 1985:465). Out of the twenty universals, then, fifteen are merely statistical and one is not even a statistical universal (Hawkins calls it a 'non-universal'). Four of the five

universals that are exceptionless for the verb-final Sino-Tibetan languages (Universals I, IV, XV, and XVI), are all exceptionless for the same reason: all Sino-Tibetan languages have GN order (see discussion below, Section 3).

Hawkins uses the Prepositional Noun Modifier Hierarchy (PrNMH; Universal XIV) to define permissible co-occurrences of these word order patterns, and the result is that only seven of the 32 mathematically possible word order patterns are permissible according to the PrNMH. It specifically excludes the types we find in Karen (PREP/ND/NNum/NA/GN/NRel; Jones 1961), even though Karen was in Hawkins' database, and Bai (PREP/ND/NNum/AN/GN/RelN; Xu & Zhao 1984). The predictions of this hierarchy in terms of Hawkins' view that doubling of orders must be of adjacent subtypes of those orders defined by the hierarchy, and in terms of his Doubling Acquisition Hypothesis (1983: Ch. 5),[7] are also not in accord with what we know about Old Chinese word order (PREP/DN/NumN&NNum/AN&NA/GN/RelN—see LaPolla 1990, Chapter 5; LaPolla 1993b), which combines elements of his subtype 2 with subtype 6.

The Postpositional Noun Modifier Hierarchy (PoNMH; Universal XVIII) also defines a set of permissible and non-permissible co-occurrence types, with the result being that only eight of the 32 mathematically possible word order patterns are permissible according to the PoNMH. Adding the Sino-Tibetan data, we find that four of the universals that went into the PoNMH are statistical, and one is a non-universal. This allows for more than just eight co-occurrence possibilities. In fact one word order co-occurrence pattern that is not permissible according to the PoNMH is so common in Sino-Tibetan it must be reconstructed to Proto-Sino-Tibetan (LaPolla 1993b): POSTP/NA/RelN/DemN/NNum/GN. According to this hierarchy relative clause is said to be a relatively unstable operator (see also Hawkins 1983:85), but this is not the case in Tibeto-Burman languages, where relative clause is the second-most stable operator (after genitives), change generally occurring only due to language contact (borrowing of the Indian-style post-nominal relative construction).

It might be argued that we can simply add more implicantia[8] to the universals to make them exceptionless, but as Hammond, Moravcsik and Wirth (1988:13) point out, the more implicantia we add, the less useful the universals become, as the number of languages for which the universals will be relevant is correspondingly reduced. The problem of whether all or any of the implicantia have any true causal or conditional relationship to the implicatum (see discussion below, Section 3) is also exacerbated.

## 3. The Significance of the Universals

Hammond, Moravcsik and Wirth (1988:1), argue that 'language typology lies at the very center of linguistics in that it serves directly the goals of the discipline itself: to explain, as well as describe, the facts of natural language structure.' The non-statistical universals developed in typology are said to be general principles of language that determine language structure: 'If it is assumed that a universal generalization that is identified is a true principle governing possible natural language structure, then the relevant facts observed in any particular language are rendered necessary by virtue of it' (p. 2). Yet this line of thinking is circular: we observe a particular distribution, characterize the distribution in a certain way, then say that the distribution is the way it is because of our characterization. Linguistic universals are not explanatory. They simply describe the distribution of the particular phenomenon in the languages that have come to the attention of the linguist writing the universal up to that point in time. They say nothing about what is possible or not possible in human language, only what has or has not been observed.

Hammond, Moravcsik and Wirth are quite right about typology being

central to linguistics, but typologies and the universals derived therefrom only give us the data we are to explain. The universals may be used as hypotheses about languages we have not seen, but they are not explanations for what has been observed. Hammond, Moravcsik and Wirth (1988:13) make the very important point (p. 21, footnote 4) that given universals of the type $X_1 \supset (X_2 \supset Y)$, then 'if it happens that the predicted property Y in a universal is widely distributed in languages that do not have the properties identified by the added implicantia [i.e. $X_2$—RJL], then we risk losing sight of that undescribed distribution of Y.' They suggest that instead of using universals of the form "If $X_1$ and $X_2$, then Y" we use universals of the type '"If $X_1$; then $X_2$ if and only if Y", which would guarantee that there would be no instances of ($X_1$ -possessing) languages that had property Y but not property $X_2$' (ibid.). We find a problematic case like this in those universals that hold for Sino-Tibetan languages simply because all Sino-Tibetan languages have GN order. In those languages that satisfy the implicantia we can say it is because of the universal that those languages have GN order, but the languages that do not satisfy the implicantia also have GN order. Why do they have GN order? The argument that it is that particular set of implicantia that determines the word order then is seriously weakened because we have no way of ruling out historical accident (coincidence). As far as the truth functions of propositional logic are concerned, the material implication is true, but without some non-truth-functional tie like causality or logical consequence it has little significance. In the case of Hawkins' universals, we cannot rewrite them in the form of 'if and only if' statements, as there is no necessary relationship between the implicantia and the implicatum. This is an extremely important point. Using propositional logic I can write a material implication such that 'If someone is an American, and parts his/her hair on the right, then he/she is not bald' (i.e. American $\supset$ (part on right $\supset$ ~bald)). This would then be an exceptionless universal, as anyone that has hair to part of course is not bald, yet it is of no significance, as being American and parting one's hair on the right as opposed to the left have no direct causal or conditional relationship to not being bald. An even more extreme example of the problems of relying on truth functions of material implications is the implication 'If Chomsky is a linguist, then the earth is round'. This is of course true, but the truth of the implicans has nothing to do with the truth of the implicatum.

Aside from the problem of causal or conditional relationship between the implicantia and the implicatum, there is also the problem, discussed by Dryer (1989:278ff), that a universal, even an exceptionless one, may not be a statistically significant statistical universal. He uses the example of Greenberg's (1963) Universal 5 ('If a language has dominant SOV order and the genitive follows the governing noun, then the adjective likewise follows the noun'—equivalent to Hawkins' Universal I), and shows that using his methodology of counting language families (genera) divided into geographic areas 'the evidence for a preference among SOV&NGen languages to be NAdj rather than AdjN falls short of statistical significance, since in only three areas is SOV&NGen&NAdj more common than SOV&NGen&AdjN, there being no languages of either sort in North or South America in my sample' (Dryer 1989:279). This being the case, then whether the universal is exceptionless or not is a moot question.

Vennemann (1985:865) argues that 'linguistic universals are generalizations about all historical languages'. By 'historical languages' he means 'attested languages'. He goes on to say that these generalizations are part of a general descriptive theory of human languages, and that 'a general descriptive theory of historical languages does exactly what Chomsky demands of such a theory: It provides a characterization of the concept "possible human language"' (p. 865-6). The crux of the problem here can be seen in the first quote from

Vennemann: not all historical languages have been examined, in fact only a small percentage have been recorded, and of those records we have, most are poorly done and incomplete. This then rules out the possibility of universals based on small imperfect samples having any legitimacy in claiming to characterize the set of 'possible human languages'. This does not mean we have to wait until all languages are described before we can make any generalizations, we simply need to recognize that all typological studies are based essentially on convenience samples, no matter what steps are taken to insure the representativeness of the sample, as so few of the world's languages have been described,[9] and that all of our universals are at best statistical, therefore we must be more circumspect in making claims about what our universals show. That is, even if we can come up with a set of universals that hold for all attested languages, we should still take them to be what Vennemann (1985:867, 1984:595) calls 'linguistic preference laws', telling us what is 'usual', 'normal', or 'unmarked' cross-linguistically (cf. Campbell, Bubenik, & Saxon 1988:224). Rather than seeing preference theories as distinct from descriptive theories (those based on supposedly non-statistical universals), as Vennemann (1985:869) and Hawkins do, I would see them both as simply stronger or weaker preference theories, as we have no way of knowing what language types may have been lost.[10]

Hawkins (1983:61 and passim) mentions several times the significance of the difference between the mathematically possible word order combinations and the small number of combinations we find attested in typological studies. Using his own figures, if there are up to 8000 languages in the world, even if each of the 8000 languages had a unique set of word order patterns (given 20 varying word order pairs), they would still only represent .76% of the mathematically possible word order sets ($2^{20}$=1,048,576). Recognizing that a large mathematical discrepancy must exist due to the small number of languages in the world, Hawkins asks the questions: 'So how can we be sure that today's languages are not just a limited set of 8000 word order co-occurrences, with so-called co-occurrence regularities merely a mathematical artifact? Can we with any confidence rule out some word order co-occurrences as impossible, rather than just contingently absent? And are the actual co-occurrences sufficient to make the projection onto possible co-occurrences?' (1983:61). His answer is that 'we cannot define possible versus impossible human languages with absolute certainty, . . . But . . . we can be FAIRLY confident that today's languages provide a data base of sufficient size for extracting the universals of word order variation' (ibid., emphasis in original). In this I think he is much too optimistic. Given that there are 1,048,576 possible word order types, his sample of 350 languages, even if each one had a unique set of word order patterns, would at best instantiate .03% of the total number of possible word order patterns. It is therefore no surprise at all that many of the possible word order patterns are not attested in his database. In reality the languages in his database are not each unique. Even if we do not say that all languages began with a common source, we have to admit that there were no more than a few sources for all of the languages on earth today, and if we think only in terms of the languages in Hawkins' database, then the number of independent sources is certainly very small. The amount of change that languages can go through, even given a very long time span, is limited, and principles or tendencies can be determined that govern those changes (see discussion below, Section 5). We would expect only a limited number of types to be produced given the few sources and common tendencies for change (especially if Sapir's (1921) concept of 'drift' has any reality). There is also the factor of language contact and convergence (see for example Emeneau 1956, Masica 1976, Henderson 1965, Dixon 1997, LaPolla 1998). Careful typological studies have found considerable geographic clustering aside from the well-known *Sprachbund* (see e.g. Nichols 1984, 1986, 1992; Dryer 1988a, 1988b, 1991). It would be surprising if we did NOT find a large number of

shared patterns. Therefore Hawkins' database is NOT sufficient for extracting all the possible variations of word order. Proof of this and that his unattested patterns really are 'just contingently absent', can be found very simply in the fact that so many exceptions have been found for his supposedly nonstatistical universals (see Section 2, above), to the extent that one supposedly unattested pattern is actually the most common pattern found among the 130+ languages and dialects surveyed within Sino-Tibetan for this study. To answer Hawkins' questions, I would say the so-called co-occurrence regularities are merely a mathematical and historical artifact; we cannot rule out any word order co-occurrences as impossible, as they are probably just contingently absent; and the few actual co-occurrences we have recorded are not sufficient to make the projection onto possible co-occurrences. We can see this in the change in the significance of certain co-occurrence patterns as methodology has improved and the databases have increased in size. For example, Dryer (1992:95) lists five dependent-head pairs that have in the past been thought to be correlated with the order of object and verb, but are now shown, according to Dryer's database and methodology, to NOT correlate significantly with the order of object and verb: adjective-noun, demonstrative-noun, intensifier-adjective, negative particle-verb, tense/aspect particle-verb. We cannot say for sure that newer methodologies or better databases will not show that even more pairs are non-correlating, or show that the pairs Dryer says are non-correlating are in fact significant correlation pairs. It is an act of faith, not science, to assume that those languages we have not seen will not differ greatly from those we have seen.

Hawkins argues against the Chomskyan innateness approach to language universals that involves the study of single or few languages rather than large numbers of languages (1983: 5ff), stating that 'A single language can attest to the POSSIBILITY of some combination, but only large language samples can motivate its IMPOSSIBILITY' (p. 10, emphasis in original). The first part of this statement is of course true, but the second part is incorrect. Bell (1978:143) argued long ago that 'A sample of languages obviously cannot establish that a language type is impossible'. Hawkins suggests that the larger the sample, the more representative it will be, but 'errors of bias cannot be remedied by increasing the size of the sample' (Bell 1978:127). In a later paper, Hawkins (1988:324), again arguing that the generativist use of small language samples is problematic, says, 'The danger in looking at just a limited number of languages is that false inferences are easily drawn about impossible co-occurrences of linguistic properties, when these are merely contingently absent from one's sample; while accidental correlations of properties are elevated into principled ones'. I would say here the difference between Hawkins and the generativists is just a matter of degree. It is certainly correct to say that an attempt to establish universals based on English and one or two other languages is destined to fail, but even much larger samples can not inform us about every pattern possible in human language. This is not to deny the usefulness of sampling, or to deny that the results of sampling can reveal certain tendencies, but to warn against taking the results of sampling as absolute. Even if we could survey every language in use today, we would still not be able to say that some unattested pattern is impossible. It may just be that the few languages that had that pattern died out before we could record them. It is also possible a currently attested type will develop into a hitherto unattested type at some point in the future. There is simply no way to rule out the possibility that the patterns we find attested in the current languages of the world are the way they are simply because of genetic history or historical accident.

## 4. EXPLANATION IN TYPOLOGY

Based on the universals derived from his database, Hawkins develops several theories for explaining word order and word order change. He

emphasizes the importance of the non-statistical nature of the universals that go into these theories, as this is what sets his theories apart from those that use only statistical universals. Yet because his universals are in reality only contingently non-statistical (see arguments above, Section 2), the theories he develops on the basis of the non-statistical nature of the universals are open to the same problems he associates with the Chomskyan single-language methodology. The situation that arises here is similar to that which arose in early generative grammar. Chomsky's (1965) original view of substantive vs. formal universals, and the entire syntactic component of his view of universal grammar, fell apart with the discovery of Walbiri (Hale 1979) and other non-configurational languages, because the theory was based on the assumption that non-configurational languages do not exist. This discovery forced Chomsky to develop the 'principles and parameters' (Chomsky 1981a, 1981b) view of language (see Foley & Van Valin 1984:16ff for discussion). In the case of Hawkins' Cross-Category Harmony theory (and all the subtheories related to it) too we have a theory based on the assumption of universality on the part of certain word order patterns (or lack thereof), so if we disprove the universality of the assumed universals, as we have above, we not only falsify empirical hypotheses (the universals), but throw into question the theories of explanation which are built upon those hypotheses.[11]

Aside from the danger of taking the universals themselves as explanations, mentioned above (Section 3), there is a similar and very problematic tendency in studies of word order universals to define more general principles on the basis of the individual observed patterns, then claim that it is the generalizations that explain the patterns. This methodology is no less circular than taking the universals themselves as explanations. An example of this is Hawkins' two interacting principles of 'Heaviness Serialization' and 'Mobility'. The former states that the heavier the constituent, the more likely it will be to the right of the verb: Rel $\geq_R$ Gen $\geq_R$ Adj $\geq_R$ {Dem/Num} (Hawkins 1983:90). This only holds for prepositional languages, though, so to account for the exceptions the second principle is invoked, which states that adjectives, demonstratives, and numbers are more likely than relative clauses and genitives to disobey the Heaviness Serialization Principle (HSP) and move around the head (i.e., they are more 'mobile') (p. 93). There is also a 'Mobility and Heaviness Interaction Principle' (p. 94) which determines which principle will hold given a particular set of co-occurrences. These principles, which are simply restatements of the observed distributions, are then said to have explanatory power.[12] For example, Hawkins claims 'all of the implications of the PrNMH follow from HSP' (p. 91), and says that both principles 'force noun modifiers to the right of their heads' (p. 95). There are then three problems with these principles: (1) they are not absolute, as the data from Karen (which is in Hawkins' Expanded Sample) are ignored;[13] (2) they are simply restatements of the observed patterns, so cannot be explanations or motivational principles for those patterns; (3) the use of the two conflicting principles that have no independent justification explains everything and nothing, as when one principle does not hold, we just have to say the other principle holds. Hawkins (1983:98ff) argues that the heaviness of the constituents being related to their ordering ultimately involves a general psycholinguistic explanation based on considerations of ease of processing which predicts that head-initial order allows faster head recognition and therefore easier processing (see also Hawkins 1990b). He mentions, for example, 'the extreme nature of the processing difficulty caused by prenominal relatives' (1983:101). If this were true, it would predict that verb initial languages should be most common, yet they are not, and that if a language had both orders, the more complex the constituent, the more likely it would follow the head, yet in Garo (Burling 1961), a language that has both orders for modifiers, there is a preference for complex relative clauses to precede the head, and simple adjectives (of

the same form as relatives) to follow the head. This is the opposite of Hawkins' prediction. If valid, Hawkins' principle should hold for all languages with equal frequency, yet it does not. The fact that some languages, such as Chinese, can violate such a rule quite happily for thousands of years shows it has little validity in predicting word order patterns.[14]

## 5. An Alternative Type of Explanation

When I mentioned principles of language change above (Section 3), I was not thinking of grand principles such as Hawkins' Cross-Category Harmony principle, but of simpler principles, some structural, some semantic, some pragmatic. An example of the structural type is the fact that adpositions have two main sources: they grammaticalize out of either verbs in serial verb or participial constructions or nouns that are the heads of genitive constructions (Givón 1979, 1984; Anderson 1979; C. Lehmann 1985; Aristar 1991). This gives one reason for the correlation between verb position and adposition position: if a verb in a verb-object phrase grammaticalizes into an adposition, it becomes a preposition, whereas one in an object-verb phrase will become a postposition; if a noun in a noun-genitive phrase grammaticalizes into an adposition, it becomes a preposition, whereas one in a genitive-noun phrase will become a postposition. We see this very clearly in Chinese, a prepositional language where all of the prepositions have developed out of verbs in verb-object constructions, whereas the language is now in the process of grammaticalizing certain locative postpositions out of locational nouns in genitive constructions because of the fact that genitives in Chinese are prenominal. An example of this is *zài wūzi lǐ* (在屋子裡) [at room inside] 'in the room' or simply 'inside', where the first morpheme is a preposition or locative verb followed by 'room' followed by the 'locational noun' *lǐ*, which derives from a noun which means '(inner) lining of a garment'. The opposite situation holds in Yi (Chen et al. 1985:131, 134), where the postposition $ta^{33}$, as in $tsh\eta^{33}\ o^{21}dz_{\downarrow}o^{33}\ ta^{33}\ la^{33}$ [s/he Xichang from come] 'S/he comes from Xichang', is derived from the verb $ta^{33}$ 'to put, place'. Another postposition derived from a verb in Yi is the instrumental $si^{21}$ from $si^{21}$ 'to lead, bring along'.

Aristar (1991) argues that there is a correlation between the order of genitive and head, relative clause and head, and adjectival and head because genitive and relative constructions can have the same diachronic source, and genitive and relative constructions are a common source of adjectivals. In Chinese all three of these constructions take the same form and marking. Aristar (1991) further argues that the order of verb and argument influences the order of noun and modifier. The point here is that change is not random; there are identifiable cause-effect relations between word order pairs. As Bybee has argued, 'synchronic states must be understood in terms of the set of factors that create them. That is, we must look to the diachronic dimension to learn how the conventions of grammar arise if we are to know why they take the particular form that they do' (1988:351). Hawkins objects to this line of explanation (1983:131-2) because he says the postulation of these diachronic links between word order patterns is not general enough and cannot explain the patterns that are predicted to be attested and unattested by his multiterm implicational universals. He also questions the validity and frequency of these diachronic links between word order patterns. My response is that recognizing that in a particular language or commonly in a group of languages there is a diachronic relationship between (for example) genitives/verbs and adpositions is not meant to be a general theory of word order change, but the fact that these diachronic relationships do exist is very well documented.[15] It is true they cannot define unattested word order patterns, but that is not what they are intended to do. The universals are descriptive statements about a particular database, while the grammaticalization relationships document commonly found patterns of diachronic development. They do not say that all adpositions must come

from one of those two sources (verbs and genitive constructions), just that it is common for this to be the case. There of course are other possibilities. Grammaticalization theory is still very much in its infancy, so is not as yet a well developed general theory,[16] but it can already explain quite of few of the regularities of grammatical structure and grammatical change.

## 6. Summary and Conclusions

We have seen that relying on multiterm implicational universals which are based on a limited database and involve questionable assumptions of comparability in attempting to define and explain attested word order patterns and predict historical change is fraught with difficulties. This methodology may also lead us to neglect the important work of doing detailed typologies of the differences between forms that would otherwise be lumped together in the search for universals. Instead we should assume that all so-called universals that can be shown to be valid (i.e., contingently non-statistical or statistically significant, and involving truly comparable constituents) are statistical, and possible evidence of preferences or tendencies of language development. We should then not assume that these tendencies, or circular restatements of the observed facts, are explanations themselves, but look to other factors of structure, semantics, pragmatics, or psycholinguistics to explain the tendencies. The explanations should also be in the form of empirically falsifiable hypotheses (based for example on genetic history, contact, or patterns of grammaticalization), and not simply non-empirical assumptions of innateness or pairs of conflicting principles that cannot be disproved.

## Notes

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[1]Abbreviations used: A/ADJ adjective; AMS adjective-marker-standard order in comparatives; ADV adverb; Dem demonstrative; G genitive; O object; N noun; Num number; POSTP postposition; PREP preposition; Rel relative clause; S subject; SMA standard-marker-adjective order in comparatives; V verb.

[2]See Lambrecht 1987, 1994; Herring 1989, 1990; LaPolla 1995, for more on the relationship between information structure and word order.

[3]This may seem unrealistic, but as Tasaku Tsunoda (p.c.) suggests, it is no longer possible for a single linguist to master all the information necessary for valid studies of universals; it is necessary for specialists in different language families to work together on such studies.

[4]The numbered universals with primes (I´, III´, etc.) are counted together with the numbered universals without the primes, as they involve the same word order patterns. I have also not included Universals XXI-XXIII, as they involve possessive adjectives, and I was not sure what qualified as a possessive adjective. If the pronominal genitive prefixes found in some Tibeto-Burman languages are considered possessive adjectives, then they pattern exactly like unbound genitives, from which they developed.

[5]There are factual errors with the word order patterns ascribed to some of the Sino-Tibetan languages used by Greenberg and Hawkins (who uses Greenberg's 30-language sample and Appendix II) in their samples. In Greenberg's 30-language sample there is only one Sino-Tibetan language, Burmese, and the order of noun and number is incorrectly given as Number-Noun. In Greenberg's Appendix II, Chinese is also listed, though incorrectly as type 15, SVO with postpositions (it has prepositions). Hawkins' does not include Chinese in his Expanded Sample (Hawkins 1983: Chapter

8) due to his analysis that Chinese has both prepositions and postpositions, and so does not fit neatly into either category. In this Expanded Sample, Ladakhi, a Tibetan dialect, is incorrectly listed as type 21, SOV/POSTP/NG/NA. It has only GN order for genitives, and so should be included in type 24. In Hawkins' Table 42 (1983: 289), Chinese is excluded, so what is represented, if anything, is Tibeto-Burman, not Sino-Tibetan. Given that the Sinitic languages are VO while the Tibeto-Burman languages are mostly OV, this is a major omission. Keenan (1979:411) also gives SOV as the word order for Sino-Tibetan as a whole. (See also Payne 1985; Campbell, Bubenik, & Saxon 1988; and Dryer 1991 for other corrections to the Greenberg and Hawkins databases, and Mallinson and Blake 1981:12-8 and Givón 1988 for discussion of other problems with the data involved in typological studies.)

[6]The actual figures for NNum+RelN within Sino-Tibetan are as follows: out of 76 single pattern OV (POSTP) lgs, 58 (76%) have NNum+RelN; out of 5 single pattern VO (PREP) lgs, 2 (40%) have NNum+RelN; out of 20 OV (POSTP) lgs with doubling, 15 (75%) have NNum+RelN as a possibility; out of 96 OV (POSTP) lgs, 73 (76%) have NNum+RelN as a possibility. To avoid problems with deciding which order is 'basic' in languages with doubling, I have considered all languages with more than one relative clause position, no matter how 'basic' one of them might be, as languages with 'doubling'.

[7]The Doubling Acquisition Hypothesis states that if two patterns, P and Q, are related in a universal (if P, then Q), then if a language acquires P, it must already have Q or it must acquire it at the same time as P.

[8]Following Hammond, Moravcsik and Wirth (1988:4), I will use 'implicans' (plural: implicantia) for the antecedent of a universal, and implicatum (plural: implicata) for the consequent. E.g., in $X \supset Y$, X is the implicans and Y is the implicatum.

[9]The methodology used in recent work by Dryer (outlined in Dryer 1989) of counting language families rather than individual languages is certainly an improvement over the methodology of Greenberg and Hawkins, but his results are still open to what Bell (1978) referred to as 'bibliographical bias', as so few languages have been well described. This may be inevitable in studies of linguistic universals, but it is no less real.

[10]The value of these preference theories has also been called into question:

> Such statements of likelihood, tendency, or numerically unspecified relative markedness or naturalness can be used at most to roughly describe certain observed property preponderance relations in certain observed sets of languages. They are too vague and unnecessarily elaborate and abstract to be really useful even as statements of mere description. They have no possible predictive or explanatory uses at all . . . What the scientist wants to know is thus not, for example, why eighty-seven percent of all logs float in water, or why it is likely, natural, or the unmarked case for a log to float in water. What he wants to know, rather, is why those particular logs that float in water do float in water, and why those that don't float in water don't float in water. Similarly, a scientist dealing with human language will not want to know why subjects precede their unfocussed objects in eighty-seven percent of a sample of known languages, or why subject precedence is more common, natural or unmarked than object precedence in the set of all known languages. What he wants to know instead is why English, for example, has subject precedence, and why Ibang, for example, doesn't. (Sanders 1975:394-95)

[11]As Aristar (1991) has pointed out, Hawkins attempts to formulate exceptionless universals, but three of his theories that are supposedly responsible for these universals (the Mobility Principle, the Heaviness Serialization Principle, and Cross-Category Harmony) are only statistical generalizations, and so 'we have the odd situation of exceptionless, nonstatistical dependencies being proposed as an effect of principles which are both statistical and subject to many exceptions' (p. 3).

[12]Greenberg's (1963) use of his principles of 'harmony' and 'dominance' in explaining the observed word order patterns is also epistemologically circular, as the two principles are merely generalizations of the observed phenomena themselves. This is true also of Keenan's much quoted 'Subjects Front' principle, which is simply a restatement of the observation that most languages have initial subjects, and is not based on any independent criteria, such as the semantics or pragmatics of subjects. It

was made into a principle to explain why SVO and VSO languages are so common even though they do not conform to Keenan's other two principles (the Serialization Principle and the Dissimilation Principle) (Keenan 1979:408ff).

[13]Hawkins also ignores the Karen data when he makes statements such as 'Prep cannot co-occur with NA & GN' (1983:205, and a similar statement on p. 246), even though Karen is Prep/NA/GN. (Matthew Dryer (p.c.) tells me that at least fourteen other languages in seven different families have this pattern as well.) In a similar way Hawkins (1990a:95) states that 'prenominal relative clauses occur productively only in languages with verb-final word order in the sentence or clause', regardless of the fact that he gives an example in an earlier paper (Hawkins 1988:337) from Chinese, which is verb-medial, of a prenominal relative clause.

[14]This brings us back to the comparability issue as well, as Hawkins ignores the fact that for many types of prenominal relative clauses, identification of the nature of the clause as a relative clause can be made very quickly because of a particular clause initial particle, or, in the case of post-verbal prenominal relative clauses, because of semantic incompatibility of the initial constituent of the relative clause with the verb (e.g., it can be immediately identified as not being the object). Again, we can see that only in-depth analysis of the individual languages can aid us here, and broad cross-linguistic assumptions of processing ease or difficulty based on the marked vs. unmarked sentences of a particular language have little validity.

[15]See for example C. Lehmann 1985, Heine & Reh 1984, Heine, Claudi & Hünnemeyer 1991, Hopper & Traugott 1993. See also Bybee 1985, 1988 for evidence that the historical source of a morphological form determines its type and position, and Bybee 1988 for further discussion of the relevance of these facts to explanations in typology.

[16]In one way at least grammaticalization theory is more general than Hawkins' theory of word order, as it involves much more of the grammar than just word order, including semantics, pragmatics, morphology, and even phonology. See LaPolla 1997 for the beginnings of a general theory of communication and grammaticalization.

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