Book Reviews

Yan Huang, *Anaphora: A Cross-linguistic Study*. Oxford: Oxford University Press, 2000, 396 pages, ISBN 0-19-823529-1, £ 69.50 (hardback); ISBN 0-19-823528-3, £ 21.99 (paperback).

Reviewed by ANDREJ A. KIBRIK, Russkaja Akademija Nauk

There are two major contemporary traditions in theoretical studies of reference and anaphora that can be distinguished as "formal" and "functionalist". The former is represented, in the first place, by Chomskyan generative analysis, along with a number of less widespread approaches. Those working in this tradition customarily focus on issues of formal description, look at isolated, made-up examples, often quite complex in syntactic structure, confine their interest to intrasentential coreference, and tend to take into account only the world's best-known languages, such as English, Italian, Japanese, or Hebrew.

The functionalist tradition is opposed to the formal one in all respects. Different brands of functionalists are typically interested in cognitive or pragmatic or discourse explanations, analyse samples or corpora of natural discourse, look into coreference across sentence boundaries, and often take into account "exotic" languages or sizable language samples.

The research interests of Yan Huang lie in between. Along with Stephen Levinson, he is responsible for the development of the so-called neo-Gricean approach to anaphora. His publications of the 1980s and 90s, summarized in the book under review, mostly focus on pragmatic conditions of intrasentential anaphora (although there are exceptions, see below), and he looks at constructed examples in a wide variety of languages (giving most attention to English and other European languages, as well as to Chinese, Japanese, and Korean). Thus, Huang shares something with both the formal and the functionalist approach, and he is in a good position to give a general overview of the field. This is apparently the central goal he set for himself, and the book certainly lives up to this goal. Huang took on the immense job not only of popularizing his own views but of discussing other people's ideas, and as a result the book

422   *Book Reviews Linguistic Typology 8–3*

provides a thorough retelling and critical analysis of a variety of approaches to anaphora.

The book is primarily devoted to intrasentential anaphora. Chapter 1, "Syntactic approaches to anaphora", and Chapter 2, "Semantic approaches to anaphora", almost exclusively deal with constructed sentence examples. Here Huang reviews the major formal theories, such as binding and control theories, and the main kinds of phenomena usually discussed in the formal literature, such as null arguments, long-distance reflexivization, VP-ellipsis, logophoric pronouns.

Huang is very critical of formal treatments of anaphora, and hardly any receives his approval, as arguably failing to embrace relevant facts or as being actually falsified by known facts. For example, Huang discusses in detail the generative theory of the so-called "null subject parameter", according to which null subjects are allowed in exactly the same languages that also have subject agreement on the verb – because otherwise the structure would become redundant. This kind of naïve functionalism could have arisen only among linguists who do not appreciate the extent of variation permissible in human languages. Huang rightly demonstrates that linguistic reality is far from being that simple; as an example he cites Icelandic (pp. 59–60), which used to allow null subjects several centuries ago, but does not do any longer, even though the inflectional type of the language has remained largely the same.

Huang is a very conscientious critic. When discussing a given theory, he often assumes the role, not of an external judge, but of an interested discussant who attempts to fit the data into the framework but, alas, finds that the theory itself does not fit. For example, in his discussion of long-distance reflexivization in Section 2.3 he adopts the terminology common among generativists and demonstrates the inadequacy of this approach in its own terms. Likewise, he incorporates the entire conceptual system of formal semanticists in the discussion of VP ellipsis, and again arrives at the conclusion that the system does not quite work. The difficult side of this admirable tolerance towards competing frameworks is, for the reader, that after every several pages one has to learn a new kind of conceptual system, a new jargon, and a new set of complex abbreviations. As far as I can ascertain, opinions of formal linguists on how accurately Huang represents their treatments of anaphora differ significantly, but in any case this is a rare example in modern linguistics when the whole gamut of approaches originating in one theoretical camp is rather minutely overviewed by a representative of a competing camp.

From my perspective, the main problem with formal analyses is the quality of the data. Too many of the examples (re-)cited in this part of the book are so marginal and so unlikely to occur in normal language use that one wonders what kind of analyses one can construct on the basis of such data. Unfortunately, this often applies to Huang's own examples too. For instance, on p. 115

he quotes a Chinese example from one of his earlier publications that translates into English as 'Mr Wang thinks that Mr Li suspects that Miss Xu looks down upon self'. The problem is that no human language is designed to deal with this kind of structures, and very little of useful and relevant information, if any, can be derived from this kind of questionable data.

The full name of Huang's own proposed approach is "A revised neo-Gricean pragmatic theory of anaphora" (for which I will use "neo-Gricean approach" as shorthand). It is described in Sections 4.1 and 4.2 of Chapter 4, "Pragmatic approaches to anaphora". The neo-Gricean approach relies on a subset of the Gricean maxims, partly rephrased. (The fact that someone whose native language is apparently Chinese and who focuses on communication in Chinese finds this framework appropriate may suggest that Gricean pragmatics is not as ethnocentric as it has sometimes been claimed to be.) The neo-Gricean principles, or maxims, are formulated in a technically complex way (p. 207), but the essence is this: the speaker provides the amount of information that is necessary and sufficient for the addressee to reconstruct the speaker's intention. The application of this idea to anaphora (p. 215), in a nutshell, goes as follows. Preferably, reduced referential expressions are interpreted as locally coreferential, if this locus is greater than one predication. In other words, coreference inside the clause and across clause boundaries is marked differently. Regular pronouns normally tend not to refer to antecedents inside the same clause; this function is served by a specialized marked device, namely reflexives. Thus, regular anaphoric pronouns corefer with close antecedents, but not with ones too close. Full NPs in proximity are by default taken as non-coreferential.

Massive data is provided to justify and illustrate the neo-Gricean approach, and most of this is quite convincing. However, some details of how Huang works out this approach seem to me problematic. My main questions are the following four.

First, in the general formulation of the neo-Gricean principles (p. 207), the speaker's maxims and the recipient's corollaries are distinguished. However, in application to anaphora Huang discusses only the interpretation principles, thus leaving out the question of how the speaker chooses an appropriate referential device.

Second, the concept of "locally coreferential interpretation" does not explicitly account for the linearly asymmetrical nature of anaphora. Pronouns in Huang's examples *Mozart$_1$ adored his$_{1/2}$ music* and *He$_1$ adored Mozart's$_2$ music* (p. 214) behave differently, although they are equally locally related to the full NP *Mozart*.

Third, the interpretation procedure for anaphoric devices is presented as being dependent on a number of very general variables, such as "background assumptions", "contextual factors", linguistic meaning, and semantic entailments. Conversational implicatures, derived from the neo-Gricean principles,
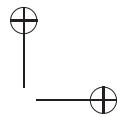
424   *Book Reviews Linguistic Typology 8–3*

are said to be cancellable by these factors. I am convinced that this is correct, but the problem is that these overriding factors include almost everything under the sun. Understanding anaphora means to sort out and predict precisely how these factors interact.

Fourth, the formulation of anaphoric procedures is so general that it is hardly falsifiable, therefore unprovable. Being unprovable does not mean being wrong, but there is a lot that remains to be done in unravelling the pragmatic basis of anaphora.

Huang's personal contribution is iconically located between the extreme formal and the extreme functionalist pole, which makes perfect sense. The continuation of Chapter 4 contains surveys of other "pragmatic/cognitive/functional" approaches (including Relevance Theory, Accessibility Theory, and the Prague School Functionalism), and this naturally flows into Chapter 5, "Switch-reference and discourse anaphora". The distribution of information between Chapters 4 and 5 is not exactly clear to me. Sometimes very similar kinds of approaches and phenomena are treated in either one or the other chapter. For example, the three well-known approaches of Giv\'on, of Ariel, and of Gundel, Hedberg, & Zacharski share a lot (despite some differences), but whereas the first and the third are treated in Section 5.2, "Discourse anaphora", the second already appears in Section 4.3.

Chapter 5 consists of two parts: Section 5.1 covers switch-reference phenomena, which Huang rightly views as an intermediary between intrasentential and discourse anaphora, and Section 5.2 is devoted to discourse anaphora as such. Section 5.1 contains a useful summary of switch-reference studies of the last two decades. Huang formulates several typological generalizations – for example, the number hierarchy which explains the allowances for using same-subject markers when the two subjects are not exactly referentially identical (p. 285), or with the adjacency hierarchy stating that if a language allows for marking switch-reference in non-adjacent clauses, it must also allow that for adjacent clauses (p. 293). Huang discusses the crosslinguistic tendency to use same/different-subject markers to code relationships between events, rather than (non-)coreference per se, and provides a neo-Gricean interpretation of this tendency: the default understanding of switch-reference markers is the referential one, but when it is contradicted by other considerations, the hearer tests other hypotheses, such as temporal or spatial disruption (p. 301).

In Section 5.2 Huang briefly reviews several models of discourse anaphora, including the topic continuity approach, several hierarchical models (especially that proposed by Barbara Fox), and several cognitively-based approaches. At the end of this section, he presents a neo-Gricean account of discourse anaphora. This analysis is mostly based on the data of Chinese conversations, but is assumed to be of a general applicability. The neo-Gricean account of discourse anaphora consists simply of the two mutually constraining principles: the prin-

ciple of sufficiency for recognition and the principle of minimal necessity, or economy. To illustrate the working of these principles in conversation, Huang looks at repairs in Chinese and English. He uses the classification of repairs developed in Conversation Analysis. He also gives some comments on the pragmatic reinterpretation of the phenomena discovered in discourse-oriented and cognitive studies, such as coreference to remote antecedents and the sensitivity of reference to discourse episodic structure. Since this account of discourse anaphora proposed by Huang comes separately from his analysis of intrasentential anaphora, this gives the impression that he views these two phenomena as separate, even though deducible from the same set of pragmatic principles. Probably Huang does not share the idea of some functionalists that intrasentential anaphora is simply a result of grammaticalization or routinization of discourse anaphora.
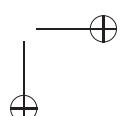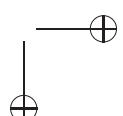
The degree of critical assessment is uneven across the book. In the "formal" part (Chapters 2 and 3), a very detailed and profound criticism of Chomskyan and other types of formal analysis is provided. In the "functionalist" part (Chapters 4 and 5), Huang is generally more agreeable to the suggestions of the approaches under discussion. Probably, this difference is due to the author's own functionalist preferences. I can only agree with Huang's suggestion at the end of Chapter 5 that "the interaction and division of labor between the cognitive and pragmatic constraints are not well understood and need to be further studied" (p. 329).

Besides the main chapters just reviewed, the book also has an introductory Chapter 1, "Typologies of anaphora", and a conclusion (Chapter 6).

The book has a significant crosslinguistic and typological dimension. Hundreds of languages are cited in connection with many phenomena discussed, and the index of language names occupies over nine pages: this wealth of linguistic diversity is very impressive. The majority of the examples actually cited come from relatively well-known languages, but more "exotic" languages also show up both in illustrative examples and in numerous bibliographical items found in the 35-pages list of references. There are occasional errors in quotations from lesser known languages, which is typical of publications with a large crosslinguistic data base. For example, "Somoan" occurs several times; it is probably a typo for "Samoan". In the Russian example (2.155) on p. 92 the verbal perfectivizing prefix *pro-* has apparently been taken for PRO, the generativist notation for subject omission in infinitival constructions.

Huang also advances a new typology: in Section 4.4, he proposes a distinction between "syntactic" and "pragmatic" languages, developing ideas of Charles Li and Sandra Thompson's on subject vs. topic prominence from the 1970s. Typical representatives of the two types are European and East Asian languages, respectively. "Pragmatic languages" are supposed to have several features: abundant use of zero anaphora, pragmatically controlled "empty cat-
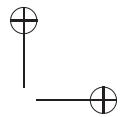
426   *Book Reviews Linguistic Typology 8–3*

egories", long-distance reflexivization, topic prominence, and some others. Of course, Huang does not suggest that European languages do not have pragmatics or that East Asian languages lack syntax. His idea is that the division of labor is in favor of syntax in "syntactic" languages and in favor of pragmatics in "pragmatic" languages. This typological opposition deserves further investigation, but right now it is based on just a handful of languages on each side. It is far from clear that the features listed co-vary outside this small set of languages. Also, I wonder if this opposition may be at least partly inspired by the difference in descriptive traditions: the overemphasis on grammar in mainstream Western linguistics and the downplaying of the role of grammar in traditional Chinese linguistics.

In conclusion, Yan Huang's book makes an important contribution to the understanding of anaphora and related issues. It is also a very good reference source, enlightening those who have always wanted to know what, for example, "sluicing", "pseudo-gapping", or "return pop" means, but have not dared to ask. This book also comes handy for those functionalists who are insufficiently familiar with formal theorizing about anaphora: at a relatively low cost, it gives them a fairly good idea of what they are rejecting. Especially if coupled with a recent comprehensive treatment of a psycholinguistic orientation (Garnham 2001), *Anaphora: A Cross-linguistic Study* can serve as a veritable encyclopedia of anaphora.

Correspondence address: Institut jazykoznanija, Russkaja Akademija Nauk, B. Kislovskij per. 1/12, Moskva 125009, Russia; e-mail: kibrik@comtv.ru

Reference

Garnham, Alan (2001). *Mental Models and the Interpretation of Anaphora*. Hove: Psychology Press.

Nicholas Evans & Hans-Jürgen Sasse (eds.), *Problems of Polysynthesis*. Berlin: Akademie Verlag, 2002, 280 pages, ISBN 3-05-003732-6, EUR 69.80.

Reviewed by ANDREW SPENCER, University of Essex

1.   Introduction

This book consists of an introduction by the editors and ten articles. There is a map showing where the languages mentioned in the book are spoken and a list of contributors but no index (not even a language index). A number of the
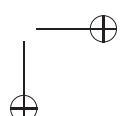
papers were presented at a conference in 1998, and some were published in the journal *Sprachtypologie und Universalienforschung*.

The influence of the two editors is seen to some extent in the choice of contributors in that all but four of the eleven authors have (had) institutional connections with Melbourne or Cologne. To some extent this seems to have narrowed the scope somewhat, as the editors themselves point out (p. 5). For instance, there is no discussion of whether, say, the noun and verb incorporation structures of Indo-Aryan languages admits them to the fold of polysynthetic languages, nor is there any treatment of Oceanic and New Guinean languages. On the other hand, the wealth of descriptive and typological expertise at those two centres mean that we see a refreshing variety of languages discussed, including some such as Nivkh and Ket, which, while well-known to the Russian-reading typology audience, are virtually unknown to the wider linguistic public.

Several of the articles in this collection are directed towards the question of characterizing the notion of "polysynthesis" in the context of theoretical models of morphosyntax or of typology. However, most of the articles are more descriptive in scope. For instance, Boeder, describes pronominal affixation/agreement in Georgian, arguing for the existence of a special zero-marked reflexivization process. Launey compares noun-head compounds with noun incorporation in Classical Nahuatl. Jung surveys clause combining in Apache. Drossard provides an overview of the notoriously complex verb system of the Siberian isolate Ket, while Fortescue charts the rise and fall of polysynthesis in Eskimo-Aleut, with some interesting speculations on how this might have come about. In addition, Sasse provides an interesting, and potentially important, discussion of the status of "words" in Cayuga (Iroquoian) and whether it makes sense for a dictionary of a polysynthetic language of this kind to be based on morphemes or on full word forms. He argues convincingly in favour of words, a conclusion that many morphologists will welcome, given the scepticism about the status of the morpheme concept.

The other articles, while having an important descriptive goal, are more focussed on the question of what constitutes a polysynthetic language, and for that reason I shall devote a little more attention to them. First, however, it is worth asking what polysynthesis is.

2. Defining "polysynthesis"

The term "polysynthesis" has been used to refer to a variety of types of language (and specific construction within languages). My impression is that the term has generally come to mean "incorporation of elements of meaning into a verb which would be expressed by a separate word in Standard Average European". Mithun (1988) stresses the importance of morphological complexity

in the verb as opposed to the noun and she explicitly contrasts Hungarian case usage with verb morphology in various North American languages. Of course, we cannot say that a language with a rich case system is ipso facto not polysynthetic. The Daghestan language Archi has a richer case system than Hungarian but also a richer (more polysynthetic) verbal morphology.

Drossard (p. 226) adopts essentially the characterization given above: For him a necessary property of polysynthesis is that the verb system have some semantic category that is expressed exclusively by means of a bound morpheme (e.g. "adverbial concepts"). Clearly, it is desirable for a definition of polysynthesis to exclude languages such as Chinese. Mandarin has very little morphology but it does have a number of aspectual and aktionsart markers (see Smith 1991 for detailed analysis). Assuming we can compare the semantic categories expressed by bound morphemes and lexical adverbs, Drossard's definition seems admit Chinese as polysynthetic, unless there is some clear and unequivocal way of excluding "aspectual" and "aktionsart" markers from the list of "adverbial concepts".

Another tradition (going back to Humboldt) stresses the idea that a single verb form can function as a whole sentence. An important part of this characterization is "polypersonalism", the idea that the verb form itself picks out, cross-references, incorporates or otherwise specifies the arguments of the verb. This idea is at the heart of innumerable descriptive grammars which speak of "incorporated pronominals" on verbs, in which subject and object markers are taken as realizing the verb's arguments, rather than just agreeing with (covert or overt) arguments (this idea is incorporated into generative grammar in Jelinek 1984). Polypersonalism is sometimes taken also to be part of the nominal system, so that the noun 'tree' is interpreted along the lines 'it is a tree' (see, for instance, Andrews 1975, for very detailed and explicit defence of such an analysis for Classical Nahuatl).

There are certain difficulties with the criterion of polypersonalism. First, what happens when a language has subject and direct object marking but no marking of indirect objects (as is the case in Chukotko-Kamchatkan languages)? Second, what exactly constitutes "object marking"? In certain Uralic languages, notably Hungarian, subject markers alternate depending on whether the direct object is definite. Is this "object marking", and if so is Hungarian polysynthetic (compared with, say, Finnish)? Third, what exactly constitutes "marking with a bound morpheme"? In Macedonian so-called pronominal clitics cluster around the finite verb and obligatorily cross-reference definite direct objects and all indirect objects. The clitics actually show most of the properties of affixes and virtually none of the properties of clitics, except that they are found on finite auxiliary verbs in some constructions. Does this make Macedonian polysynthetic? Bulgarian is very similar except that doubling of an overt definite object by a clitic is only optional (depending on information structure

and other aspects of discourse structure). Does this mean that Bulgarian is not polysynthetic? Many Bantu languages are similar to Macedonian/Bulgarian in that they obligatorily cross-reference the subject but the object marker only appears under certain discourse situations. Bresnan & Mchombo (1987) show that in Chichewa the object marker prefix behaves in much the same way as a clitic pronominal, in that an overt direct object noun phrase has the freedom of placement in the sentence associated with dislocated topics. Is Chichewa polysynthetic? Finally, what exactly is the point of singling out languages in which verb arguments can be covert but in which they are signalled by affixes bearing pronominal features. There are many languages in which arguments can be dropped if they can be inferred from the contexts, including Korean, with very complex verb morphology and Chinese, with very little morphology. Arguably this is a more important typological property than the rather slippery notion of cross-reference arguments with pronominal affixes. But we certainly do not want to make Chinese polysynthetic.

A somewhat more focussed characterization of polysynthesis pinpoints so-called "noun incorporation" as a defining feature: a polysynthetic language permits (encourages) a finite verb to form a morphological compound with its direct object, as an alternative to the analytic expression of that argument, generally with slight differences in focus, referentiality and so on: 'Man killed bear' vs. 'Man bear-killed'. The term "noun incorporation" is somewhat misleading, in that such languages will sometimes permit incorporation of adverbs and verbs (*pace* M. Baker 1988): 'Girl quick-runs' ∼ 'The girl runs quickly' or 'Warrior die-fell' ∼ 'The-warrior fell, dying' (see Spencer 1995 on Chukchee (Chukchi)). Given this variety of X-V compounding it would be much more appropriate to use a term such as "lexeme incorporation". But again there are serious questions about what exactly constitutes "noun incorporation" in this context. Frisian is said to have productive noun incorporation (Dijk 1997). Does that make it a polysynthetic language (as opposed to Dutch or English)?

   In their (excellent) introduction the editors attempt to invest the traditional, vague, characterizations of polysynthesis with typological import, arguing that the indicators of polysynthesis point to a "clustering" of properties that "call out for a unified explanation" (p. 4). They characterize a prototypical polysynthetic language (note the qualifying "prototypical") as one in which information about the core grammatical properties of the predicate and its arguments can be coded on the verb itself, so that the verb form can serve as a free-standing, contextually-independent utterance (p.3). Evans and Sasse then turn to the problems enumerated above, listing several open questions:

(i)   is (direct object) incorporation criterial for polysynthesis?

(ii)   must all argument positions be filled even in non-finite constructions?

(iii)   is it criterial that pronominal affixes be completely independent of overt nominal arguments?
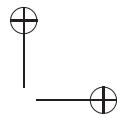
430   *Book Reviews Linguistic Typology 8–3*

(iv)   is the existence of agreement/pronominal affixation a necessary property, where a language shows other properties of polysynthesis (such as incorporation)?

The point of asking these questions is that none of the possible criteria for polysynthesis picks out either a necessary or a sufficient property.

M. Baker (1996), in an interesting theoretical study, has attempted to codify the concept of polysynthesis by deriving (some of) the typical properties of polysynthetic languages from more abstract underlying grammatical primitive notions, especially agreement. As a consequence some of the languages which are traditionally thought of as polysynthetic turn out not to be (e.g. the Eskimo group, despite showing some variety of noun incorporation). Neither the pre-generative descriptive tradition nor Baker's typological study has a great deal to say about the relationship between N-V compounds and compounds headed by nouns (this issue is directly addressed for Classical Nahuatl in Launey's contribution). This is partly because N-N compounding is common in languages which seem to be completely lacking in classical noun incorporation. However, there are languages such as Chukchee and Nahuatl in which nouns incorporate their modifiers (including verb stems and determiner-like elements such as possessives and numerals in the case of Chukchee, as discussed in Mattissen's contribution). Such constructions have not been the subject of much detailed study, apparently, and so it is difficult to know whether such languages reflect a stronger tendency towards word-based information packaging or whether attribute incorporation is entirely independent of argument incorporation. Nonetheless, there is one respect in which N-N compounding comes to have more significance for definitions of polysynthesis. I refer here to "synthetic" or "verbal" compounds of the type *train driver*, *train driving* in which a deverbal noun is compounded with a noun denoting the object of the verb root. Constructions of this sort can be extended to finite constructions in a number of Germanic languages (especially Frisian; see Dijk 1997).

But enriching the definition of polysynthesis with reference to noun incorporation (or lexeme incorporation) will not be of help unless we can safely characterize some notion of productivity for incorporation. Verb stems may sometimes include elements with some kind of identifiable lexical meaning, irrespective of whether such stems have significantly lexicalized meanings and irrespective of whether such a process is in any sense productive. If such lexicalized formations were admitted into the definition German would certainly be a polysynthetic language. Given occasional backformations such as *to baby-sit* or *to noun-incorporate*, English is also polysynthetic. Perhaps we should reserve "polysynthetic" for languages in which incorporation is productive, to the extent we can define "productive".

But in any case, would we really want to claim that, say, English and Frisian belonged to different language types because Frisian but not English has pro-

ductive noun incorporation? Conversely, a number of languages which used to have productive incorporation (such as Classical Nahuatl and nineteenth century Chukchee) have largely lost it or are showing signs of losing it (cf. Campbell 1985 on Pipil, a descendant of Nahuatl). But Pipil retains many of its other inherited polysynthetic characteristics. Would we want to say that Pipil belonged to some significantly different language type from Classical Nahuatl by virtue of losing noun incorporation, in the way that SVO Italian belongs to a different word order type from SOV Latin? (Cf. also my remarks about polypersonalism and clitic systems above.)
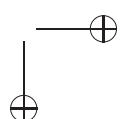
All this raises a crucial question, touched upon but not elaborated, in Evans and Sasse's introduction, namely, what could possibly hinge on characterizing a language as polysynthetic. As far as I can tell it is only M. Baker (1996) for whom the term has any theoretical content or consequences. Investigating a clustering of properties, none of which seem to be criterial, is less likely to lead to an uncontroversial characterization of the problem. In particular, a prototypical polysynthetic language will tend to have a majority of the typical properties, but what if a language only shows each of those properties to a limited degree, or only has, say, half of them? If it can be shown that such intermediate cases are very rare and historically unstable that would lend support to the idea of a polysynthetic type (whether M. Baker's or the more traditional notion). But I am sceptical that such a type can be motivated.

M. Baker links polysynthesis to polypersonalism, in that for him, polysynthetic languages always have incorporated pronominals and not agreement markers. Evidence to distinguish the incorporated pronominals from agreement hinges on subtle, often rather covert, properties. For instance, M. Baker suggests that polysynthetic languages lack subject-object asymmetries, because it is the pronominals that are the real arguments. Evans' piece looks at semantic properties, referentiality and definiteness, in the pronominal elements in Bininj Gun-wok. Pronouns are prototypically referential and definite, but this is not the case with Bininj Gun-wok object pronominals. These can cross-reference a variety of indefinite, generic or even non-referential objects, though the inflected verb forms still does not have as many degrees of freedom as a verb + free pronoun construction in non-polysynthetic languages. As Evans notes there is much need for further work on this neglected aspect of the problem, and Evans' meticulous and carefully-argued piece provides an excellent model for such research.

Brett Baker argues that another Gunwinyguan language, Ngalakan, has both genuine agreement markers and also bound pronominal affixes, but that there is also a third construction, in which the pronominal marker has generic reference and need not show formal agreement. He observes that a "disagreement" rule for certain noun classes further highlights the differences between the pronominal affixes and pronouns proper. In this construction a noun bearing the MASC

or the VEG class marker takes a NEUT agreement on the verb. (B. Baker's description of this also provides a neat illustration from field work of how native speakers can be natural linguists.) Both B. Baker's and Evans' papers therefore illustrate the need to be very careful when characterizing the status of pronominal elements in the verbal complex.

Nordlinger and Saulwick discuss the question of whether it is possible for a polysynthetic language by any definition (including M. Baker's) to have true infinitives. They provide a detailed analysis of Rembarrnga, an Australian language which is cited by M. Baker as a case of (his version of) polysynthesis. Their point is very simple: according to Baker's theory, polysynthetic languages cannot have infinitives because predicates in such languages have to identify their arguments by an agreement process and infinitives lack agreement. Nordlinger and Saulwick demonstrate that Rembarrgna, contrary to Baker's claims, has very clear instances of infinitives (as opposed to "nominalizations"). The result for Rembarrnga therefore complement the facts of Chukchee (another language discussed explicitly in M. Baker, 1996), which also has true infinitives (Spencer 1999).

For M. Baker incorporation is criterial for polysynthesis, but what exactly counts as "incorporation" anyway? Mattissen presents a good case for regarding Nivkh (Gilyak) as a polysynthetic language, in the sense of permitting lexeme incorporation. Mattissen enters an old debate played out on the pages of Soviet linguistics publications over whether Nivkh exhibits genuine lexeme incorporation. Nivkh is famous principally for its initial consonant mutation, triggered by specific grammatical constructions, notably when a direct object immediately precedes the verb. The object-verb complex not only triggers mutation but forms a single accentual unit, suggesting that the complex is a single phonological word. By these criteria Nivkh has a better claim to polysynthesis than most Oceanic languages, for instance. However, Mattissen points out several differences with the "standard" picture of noun incorporation: adjuncts, interrogative pronouns and proper names, and inflected nouns can all get incorporated. In addition, Nivkh permits verb-verb compounding (somewhat like Ket).

Mattissen explicitly considers the question whether Nivkh is polysynthetic by any of the several characterizations available in the literature. On one criterion Nivkh fails dismally because it lacks "polypersonalism". On the other hand, nouns in Nivkh regularly incorporate their modifiers, and Mattissen explicitly compares this property with similar facts in Ainu and Chukchee.

3.   Conclusions

This collection contains a number of interesting descriptive papers together with a smaller number of theoretically-oriented pieces which more directly ad-

dress what I would regard as the central "problems of polysynthesis": what is it and does it matter? In some respects, this is a little disappointing. It would have been good to see more chapters devoted to the theoretical and conceptual issues and more chapters addressing the implications for the theory of morphosyntax and, of course, the implications for models of typology. In principle, at least, polysynthesis is topic on which morphologists, syntacticians, lexicologists and typologists could fruitfully work together. I would hope that the unresolved questions raised by this volume will serve as an impetus to such collaboration.
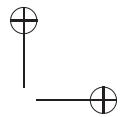
Correspondence address: Department of Linguistics, University of Essex, Colchester, CO4 3SQ, UK; e-mail: spena@essex.ac.uk

References

Andrews, J. Richard 1975. *Classical Nahuatl*. Austin: University of Texas Press.

Baker, Mark, C. 1988. *Incorporation. A Theory of Grammatical Function Changing.* Chicago: University of Chicago Press.

Baker, Mark, C. 1996. *The Polysynthesis Parameter*. Oxford: Oxford University Press.

Bresnan, Joan and Mchombo, Sam 1987. Topic, pronoun and agreement in Chichewa. *Language* 63: 741—82.

Campbell, Lyle 1985. *The Pipil Language of El Salvador*. Berlin: Mouton de Gruyter.

Dijk, Stefan 1997. *Noun Incorporation in Frisian*. Ljouwert: Fryske Akademy.

Jelinek, Eloise 1984. Empty categories, case, and configurationality. *Natural Language and Linguistic Theory* 2: 39—78.

Mithun, Marianne 1988. System-defining structural properties in polysynthetic languages. *Zeitschrift für Phonetik, Sprachwissenschaft und Kommunikationsforschung* 41: 442—452.

Smith, Carlota, S. 1991. *The Parameter of Aspect*. Dordrecht: Kluwer Academic Publishers.

Spencer, Andrew 1995. Incorporation in Chukchi. *Language* 71: 439—89.

Spencer, Andrew 1999. Chukchee and polysynthesis. In: Ekaterina V. Raxilina and Jakov G. Testelec (eds.). *Tipologija i Teorija Jazyka – ot opisanija k ob"jasneniju. K 60-letiju Aleksandra Evgen'evicha Kibrika*. Moscow: Jazyki russkoj kul'tury, 106—113.

Walter Bisang (ed.), *Aspects of Typology and Universals*. (Studia typologica, 1.) Berlin: Akademie-Verlag, 2001, 240 pages, ISBN 3-05-003559-5, EUR 69.80.

Reviewed by GILBERT LAZARD, Ecole Pratique des Hautes Etudes

This is the first volume of a new series called *Studia typologica*, which is edited by Thomas Stolz as a companion series of the journal *Sprachtypologie und Universalienforschung.* Its publication is the result of the wish of participants in the 1998 Summer School of the German Society of Linguistics (Deutsche Gesellschaft für Sprachwissenschaft) in Mainz, whose topic was language typology. It is not a coherent book on this subject, but it presents ten papers
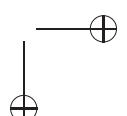
which illustrate how language typology works and are representative of both functional and formal linguistics.

Guglielmo Cinque (pp. 13–19) examines the status of "mobile suffixes". Verbal suffixes encoding notions of mood, modality, tense, aspect, and voice sometimes show variable ordering. The author considers whether this should be taken as evidence against an invariant universal order of such suffixes. He surveys, for instance, the Turkish suffix *-(y)Abil-* expressing possibility, which may appear before or after the negative suffix *–mA-*. But this variation depends on the meaning to be expressed: the suffix has not the same scope in both positions. Other examples in other languages call for similar interpretations. The conclusion is that those facts, "if they do not show conclusively that all 'mobile' suffixes are only apparently mobile, at least invite some caution in drawing conclusions from them which are against the assumption that grammatical markers come in a rigidly fixed order" (p. 19).

Bernard Comrie (pp. 21–35) considers some of the ways in which linguistic typology can be used in historical linguistics, more specifically in historical reconstruction. He first critically examines examples in the reconstruction of the Indo-European proto-language. It has been claimed that the reconstructed system of consonants with voiced aspirates and no voiceless aspirates is typologically unacceptable. However, he says, such a system is attested in Kelabit (Austronesian) and Mbatto (Kwa). On the other hand, he approves of the argument against the possibility of an ergative alignment in the Indo-European proto-language, but he partly refutes his own argument against the idea of an active–inactive system, although he sees no cogent evidence either in its favour or against it.

The last part of the article is about "reconstruction, grammaticalization, and the Uniformitarian Hypothesis". Referring to Comrie (1992) and considering that grammaticalization proceeds from simpler to more complex structures, the author hypothesizes that earlier languages may have been simpler than modern ones. An objection is that this scenario violates a basic principle of science called the Uniformitarian Hypothesis, which says roughly that things were in the past more or less as they are now. The answer is that typology enables us "to reconstruct earlier stages of human language that differ qualitatively from those spoken today in being less complex, while nonetheless maintaining the Uniformitarianism Hypothesis that the historical processes posited for such distant past developments must be the same as those we observe in the more recent period" (p. 34). This position is obviously right, for there must have been a time when grammar was born and progressively developed from simpler shapes to more complex ones. The question is that that time is certainly very remote, and we may doubt that reconstruction and typology, in their present state, enable us to look usefully so far back.

Hubert Haider (pp. 37–52) presents rather abstract considerations on func-

tionalist and cognitive explanations of why grammars are what they are. The following few passages will illustrate the author's theses: "The explanation of a design that results from natural evolution does not involve reference to a future utility." (p. 38) – "The utility of (a grammar of a) language does not determine its design, because completely different designs can serve the same utility." (p. 38) – "The regiment of grammar is stronger than the putative shaping force of its conditions of use." (p. 39) – "Acquisition and use of grammar are facets of a specific cognitive capacity. This capacity rests on brain functions that are the result of evolution. If so, there must be a genetic basis identifiable in the genome. That this is the case indeed, has been verified by Fisher et al. (1998: 68), who identified a gene defect on chromosome 7 that correlates with a well-studied case of developmental dysphasia." (p. 41) – "The luxury of grammar systems of natural languages is by far underdetermined by the conditions of use" (p. 41). – "Function does not determine the form deterministically, and the WHEREFORE is independent of the WHY. The answer to the latter is found in a theory of the genome and its expression in the phenotype." (p. 43) – "Languages are as they are because UG is as it is." (p. 51)

Lars Johanson (pp. 53–62) presents his theory of aspect with examples from English, Russian, Turkish, Swedish, and Hungarian. I think he is right in distinguishing aspect proper and what he calls actionality, i.e., *Aktionsart*. He is also right in building a conceptual framework, – namely the "three perspectives" called intraterminal, postterminal, and adterminal, – as a *tertium comparationis* for comparing language-specific systems, and in excluding the vague current notions of perfective and imperfective. His theory is assuredly interesting. However, I personally prefer the theory of Cohen (1989), whose initial framework consists of a combination of two oppositions, completeness vs. noncompleteness (roughly equivalent to Johanson's adterminality and intraterminality) and concomitancy vs. non-concomitancy (cf. Lazard 2001: 445–459).

Jaklin Kornfilt (pp. 63–82) studies variations of the constructions of nonfinite clauses in Turkish. She shows that adverbial clauses have different properties from subject and object clauses. On the other hand, she shows that nonfinite clauses, which are left-branching and head-final and as such resemble noun phrases, are nevertheless to be distinguished from derived deverbal nominals by specific properties. She formulates that difference by saying that "these clauses are indeed clauses with the basic properties of sentences, rather than phrases with lexically derived deverbal nouns or adverbs", and she proposes explanations in terms of generative grammar. A last section is devoted to a type of relative clause which seems to be at variance with the status of "adjunct".

Christian Lehmann (pp. 83–97) proposes a program for language documentation. This task is different from description: "DOCUMENTATION OF A LANGUAGE is an activity (and, derivatively, its result) that gathers, processes and exhibits a sample of data of the language that is representative of its linguistis

436   *Book Reviews Linguistic Typology 8–3*

structure [...] DESCRIPTION OF A LANGUAGE is an activity (and, derivatively, its result) that formulates, in the most general way possible, the patterns underlying the linguistic data" (p. 88). Documentation involves not only a text, but all the particulars of the speech situation, speech act participants, etc. Lehmann's reflexions on the subject are reasonable. I would like to make a remark only about one sentence in his introduction. He says that "linguistics is a historical discipline". This is true of documentation and description, but it is not true of the whole of linguistics, for linguistic typology is not a historical discipline.

Ignazio Putzu and Paolo Ramat (pp. 99–132) study articles and quantifiers in the Mediterranean languages. The authors survey those expressing totality on the basis of the rich English system with five terms (*whole*, *all*, *every*, *each*, *any*) taken as a framework for comparative description. They also investigate the development of the definite article in a number of old and modern languages of different families. They make many interesting points. But why the Mediterranean languages? Do they form a *Sprachbund*? We are only told in the conclusion that "[t]here appears to be a geographical East-West gradient in the sequence in which the definite article is attested" (p. 129).

Anna Siewierska (pp. 133–152) discusses two universals which have been proposed concerning the order of constituents in the possessive phrase. Those alleged universals express implicative relations between the order Genitive–Noun and Pronoun–Noun and between Prefix and order Genitive–Noun respectively. She tests them in a sample of 284 languages. She concludes that they are on the whole valid as "statistical universals" and she adds a third one. She draws therefrom some conclusions about likely pathways of evolution.

Thomas Stolz's article is entitled "Comitatives vs. instrumentals vs. agents" (pp. 153–174). It is admittted that there is an affinity between these notions and it is a fact that in many languages there is some kind of syncretism between them. Stolz "seeks to provide empirical evidence in support of the hypothesis that one ought to keep two types of languages apart, i.e. to distinguish those with a high degree of affinity between instrumentals and agentives from those with a low degree of affinity" (p. 153). He posits five *a priori* possible patterns: three forms for the three functions, syncretism of all three (Comitative-Instrumental-Agentive = CIA-syncretism), syncretisms of two of them (CI-syncretism, IA-syncretism, CA-syncretism). The investigation bears on 122 languages including 50 European languages and 72 African, Asian, Oceanian/Australian, and American languages. The main result (p. 171) is that "there seems to be something in comitatives and ergatives that renders them incompatible conceptually. They partake in syncretistic patterns with the instrumental, but never both of them at the same time." Other interesting conclusions concern the difference between European and extra-European languages, and between ergative and non-ergative languages.

In his introduction and conclusion Stolz tackles the fundamental problem

of the approach to the comparative study of languages. He asks "which, if at all, of the two competitors, form and function, takes precedence over the other when the contours of a category is at stake". He chooses "to adopt a form-oriented (or semasiological) point of view" for his study, because it is impossible to "be sure about the range and internal structure of the ontological (or onomasiological) component". He however writes: "Most likely, the right solution is a compromise between form-orientation and function-orientation" (p. 155). I think the solution is indeed a combination of the two orientations, but it is not a compromise. It is to do what he actually did, namely first to follow the onomasiological way in positing a conceptual framework, which inevitably is intuitive, then to proceed exclusively in the semasiological way, i.e., to investigate languages by observing forms and inferring meanings only from the observation of forms (Lazard 2002).

Walter Bisang's 49-page article (pp. 175–223) is made of thoughts "on the explanatory power of functional criteria and the status of Universal Grammar", tested by means of examples in the domains of areal relations, grammaticalization, and typology. In the introduction he says (p. 175): "[M]y general line of reasoning will be as follows. Since I take the development of human language as a result of evolution, I expect that there are linguistic structures which are beyond functional explanations and I do not see why it should not be possible in principle that some structural constraints are innate. However, many findings from language contact, areal typology and cognitive networks [...] suggest that the influence of functional factors on the selection of linguistic structures can be fairly considerable." Then he surveys a variety of questions, with examples in a number of different languages, American, African, and especially South-Asian. One conclusion is that those examples "suggest that the cognitive equipment of man has more explanatory power than is admitted by formalist approaches" (p. 218).

Correspondence address: 49 av. de l'Observatoire, 75014 Paris, France; e-mail: gilzard@wanadoo.fr

References

Cohen, David (1989). *L'aspect verbal*. Paris: Presses Universitaires de France.

Comrie, Bernard (1992). Before complexity. In John A. Hawkins & Murray Gell-Mann (eds.), *The Evolution of Human Languages*, 193–211. Redwood City, Cal.: Addison-Wesley.

Fisher, Simon E., Faraneh Vargha-Khadem, Kate E. Watkins, Anthony P. Monaco, & Marcus E. Pembrey (1998). Localization of a gene implicated in severe speech and language disorder. *Nature Genetics* 18: 168–170.

Lazard, Gilbert (2001). *Etudes de linguistique g\'en\'erale: Typologie grammaticale*. Leuven: Peeters.

— (2002). Transitivity revisited as an example of a more strict approach in typological research. *Folia Linguistica* 30: 141–190.