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Whats in a grammar: A reply to Stokhof

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WHAT'S IN A GRAMMAR: A REPLY TO STOKHOF¹

It is gratifying for an author to receive attention in scholarly book reviews, but alarming when a review focuses as sharply on criticism as does the recent article by Professor W.A.L. Stokhof (Stokhof 1988). This spirited review article offers a long list of deficiencies in my Acehnese grammar (Durie 1985a), both in theoretical approach and in the adequacy of the data presented. I assume that Stokhof felt the book was significant enough to devote so much attention to it, and in the same spirit I am responding to his article.

In this response I offer clarification of questions that arise from the review. I do not attempt to cover every single minor point, but instead focus on places where significant misunderstandings may arise, or where I have a serious disagreement with Stokhof. The bulk of this article is organized as a list of topics, treated in the same sequence as they appear in Stokhof's article. However, first there is the matter of a concern about Stokhof's general approach.

1. **A reference grammar.** In a reference grammar, information is presented systematically according to grammatical topics. For a query about syllable structure the reader will need to look at the section on syllable structure. For a query on intonation the reader will need to look at that part of the grammar which deals exclusively with intonation. Such queries arise in the most diverse contexts as one uses a reference grammar. The advantage of this approach is a completeness, consistency and concision which is not possible in a pedagogical grammar. However, a reference grammar can be challenging for the reader: at various points Stokhof seems to expect that needful facts about Acehnese should be listed at the places where he as a reader had need of them. This leads him to suggest that important information is not provided in the grammar when in fact it is. Here I list some examples.

¹ I would like to express my thanks to the Department of Languages and Cultures of Southeast Asia and the Pacific, of Leiden University, for their hospitality to me while I prepared this paper, and to the Netherlands government for the travel grant which removed me from the distractions of my own institution.

1.1 Stokhof wrongly reports (1988:331):

'We cannot tell from Durie's description whether [sic] *lôn* in (3-11) is a cliticized word or a clitic. The word here is enclitic and proclitic at the same time, but exact rules about the position of such unstressed monosyllables relative to the stressed word are not given. Nor is much information offered about their relative positions.'

(3-11) asee=nyan hana=lôn=poh'=ji=lê
 dog that not I hit 3 any more
 'I won't hit that dog any more'.

The problem here is that example sentence (3-11) discussed by Stokhof is taken from a section devoted solely to discussing epenthetic *h*, which is attached to certain enclitics. This sentence was intended to illustrate a point about the use of this *h*. Understandably, general information concerning the interpretation and ordering of clitics is not provided here, but under relevant headings. The information Stokhof sought was available in the grammar. First, in section 8.6 *Argument Cliticization*, the grammar states that there is no difference in form between the clitic and the independent pronoun forms of *lôn* 'I, polite': the difference is neutralized (see Durie 1985a:206). It is thus misleading to criticize the grammar for not providing criteria to distinguish between these two.

Second, it is not true that no rules are given about the position of clitics relative to the stressed word. From the grammar's treatment of verbal morphology it is clear that *poh* 'hit' as a transitive verb must take an Agent argument, moreover this argument must be referenced by a pronominal proclitic placed directly before the verb (Durie 1985a:125, 201). So *lôn* 'I' is proclitic to *poh* 'hit' and it cross-references the Agent.

1.2 Stokhof claims 'Durie does not inform us whether two {w} may occur between labials' (1988:334). This information is provided, but again, not in the section (3.3.1.3) that Stokhof is discussing. Questions involving the distribution and cooccurrence of phonemes are dealt with in 2.4 *Phonotactics*, and especially in 2.4.1 *Syllable and Word Structure* and 2.4.2 *Distribution of Phonemes*. In these sections it is established that each vowel must belong to a separate syllable, and each syllable must begin with at least one consonant. Thus any two vowels must minimally occur in the disyllabic structure CVCV, where C represents consonants and V repre-

sents vowels. So no two vowels can be adjacent, and two /u/ cannot occur between two labials.²

1.3 Stokhof reports that 'no information is provided about whether ... the enclitic is optional or obligatory' (1988:335). But the information is provided in section 8.5.1 *Undergoer Clitic Referencing*: 'Undergoers are optionally cross-referenced by enclitics'.

1.4 When discussing sentence (4-37), repeated below, which appears in a section on 'locative Undergoer incorporation', Stokhof writes of 'the fact that we are not informed about possible cliticization (my informant rejected, e.g., *ujeuen ka = keunöng = jih bak=lôn* but not *ujeuen ka = ji = keunöng bak= lôn!*)' (1988:337).

(4-37) *ujeuen ka = keunöng bak = lôn*
rain IN strike at I

'The rain came in on me'.

I disagree with Stokhof's report that his second example sentence is acceptable: in my account *keunöng* 'strike, coincide' takes an Undergoer, but not an Agent, so it cannot take an Agent proclitic to give *ji = keunöng*. As for the first example, which Stokhof correctly reports is unacceptable, in this case the Undergoer, the rain, is inanimate and as such cannot be cross-referenced on the verb *keunöng* by the enclitic = *jih*, see section 5.5.5.3 *Inanimate use of the third person clitics*. Here again Stokhof has taken an example out of context. Not finding grammatical information in the same section as the example, he does not seek it under the appropriate headings but instead concludes that the grammar does not present it at all.

1.5 Stokhof claims (1988:343) that I do not mention the optional pause after a Core Topic phrase, but see section 3.1.2 *The Phrase*: 'A phrase can be separated from other phrases by pauses'. This is not a distinctive characteristic of the Core Topic, because it is true of any preverbal phrase that it can be followed by an optional pause.

1.6 Stokhof suggests (1988:343) that 'prosodic phenomena are probably

² Note that *peuumpung* 'to place in a nest' (Durie 1985a:80) is probably my error for *peueumpung* where two /u/ vowels are separated by a glottal stop (Durie 1985a:26[iii]), i.e. [puʔuɪmpuŋ^o]. Also the alternation reported by Stokhof (1988:335, footnote 2) of /u/ ~ /uɪ/ in *peusuuem* 'to heat' from *seuueu* 'hot' is almost certainly a root alternation between *suuem* [suʔuəm] and *seuueu* [suʔuəm], i.e. a type of non-derivational vowel alternation involving /u/ and /uɪ/. Contra Stokhof, this phenomenon is described in the grammar in section 3.4.2 *Vowel Alternation*. It seems that in the dialect Stokhof worked with, this alternation, which is most often attested across dialects, or by variant forms of the root in a single dialect, has been lexicalized as an irregular quirk of derivational morphology.

decisive for subject and predicate function assignment', and evidence is provided from Indonesian. The inference the reader must draw is that the grammar does not address this question. But it is addressed, in, as might be expected, the sections on intonation (8.4) and word order (8.3).

1.7 Stokhof queries (1988:346) whether enclitic cross-referencing with certain nominal and prepositional predicates is obligatory. The discussion of these predicates makes it clear that it is optional (sections 5.4, 7.2).

1.8 Stokhof claims that 'no specific diagnostic features have as yet been discovered' (1988:347) to distinguish intransitive verbs which take proclitics from those which take enclitics. Yet the grammar devotes no less than 17 pages (Durie 1985a:55-71) to motivating this distinction on semantic grounds, accumulating a large body of evidence which is reinforced by extensive discussion of morphemes which trigger switches between these verb classes (see sections 4.5.1 and 4.5.3). In all, about 10% of the pages in the grammar is devoted to this topic.

2. A point by point refutation. In this section I treat a list of topics, in the same sequence as they appear in Stokhof's article.

2.1 *The question of the dialect standard.* I agree with Stokhof that no official decision has been reached about what should be standard Acehnese. However, I disagree that my statements on this topic are 'quite at variance' (Stokhof 1988:323) with those of Cowan (1981) or the Indonesian sources Stokhof quotes (Akbar et al. 1985 and Ismail 1983). The crucial point, which I make in the grammar, is that the sociolinguistic situation described by Snouck Hurgronje just under one hundred years ago no longer exists today.

In Snouck Hurgronje's time there was no large metropolis at the site of the present-day Banda Aceh. Rather, there was a cluster of wealthy and powerful *gampôngs* (villages) close to the palace (Snouck Hurgronje 1906, I:24). Because of their association with the royal court, and especially because of their economic dominance in the Greater Aceh region, the so-called *banda* dialect spoken in these villages was prestigious and set a local standard. These villages were not located within the *banda*: Snouck Hurgronje reports that their customs and the dialect were styled *banda* because of their proximity to the *banda* proper. The so-called *banda* dialect was not the metropolitan vernacular of a city, but rather the regional variant typical of the Lowlands (*Barôh*) dialect area, within which the *banda* was located. Cowan (1981) makes no distinction between the prestigious *banda* dialect and the Lowlands dialect area in general. My own dialect research in the Lowlands confirms this: I found that the former *banda* dialect, as described in Snouck Hurgronje's writings, and codified in Djajadiningrat's dictionary (1934), corresponds most closely to the dialects of

gampôngs found in the vicinity of present-day Banda Aceh (see Durie 1985a:4-5, where this research is reported).

We have no evidence that this former standard was acknowledged in the more distant parts of Aceh, Snouck Hurgronje's pioneering work being restricted to Greater Aceh. It probably was never a standard over the whole region: 'In the more distant of the lowland districts [i.e. outside Greater Aceh] the influence of the Banda Aceh is scarcely traceable' (Snouck Hurgronje 1906, I:25). The recent description of Cowan (1981), who speaks of the *banda* standard, does not refer to the contemporary situation, with which he had no familiarity, but to that described by Snouck Hurgronje.

So what has happened in the intervening years? Most importantly, Banda Aceh has become a bustling metropolis, in which many inhabitants are first- or second-generation newcomers from other parts of Aceh and wider Indonesia. Some of the original villages have been swallowed up by urban growth, and the Acehnese spoken in Banda Aceh is no longer the characteristic Lowlands dialect. In fact, it is not possible to speak of 'the' Acehnese of Banda Aceh, since the city includes people from so many regions, and the younger generation, especially in the last 10 years, is not acquiring Acehnese with any fluency. The result is a blend of dialects, strongly influenced by Indonesian. Some younger city-dwellers, born and raised in Banda Aceh, cannot speak Acehnese, except perhaps enough to buy some bananas in the market. Increasingly, fluency in Acehnese is associated with being from a *gampông*, and for some the most prestigious form of Acehnese is Bahasa Indonesia. At the fascinating PKA III (Pekan Kebudayaan Aceh III, or Third Acehnese Cultural Festival), held in Banda Aceh in August of 1988, I observed an Acehnese play in which one of the characters, a middle-aged Acehnese woman, insisted on speaking only Bahasa Indonesia. She had, she said, gone on the *ranto* and forgotten her Acehnese. When pressed, she revealed that by a '*ranto*' she meant she had moved a few kilometres down the road. This evoked a howl of recognition from the thousands of spectators. At a seminar held as part of the Cultural Festival I heard someone complain that Acehnese youth who go to study at Universitas Syiah Kuala, in the vicinity of Banda Aceh, forget their mother-tongue in favour of Indonesian within a week or two.

These circumstances do not suggest that the Acehnese of Banda Aceh is a likely candidate for a prestigious standard. Nevertheless it is true that for Acehnese speakers who settle in Banda Aceh there is a noticeable adjustment, an elimination of those features which are considered to be recognizably dialectal. A kind of 'dialectless' metropolitan vernacular has developed – by no means a universal standard – in which identifiably dialectal elements are suppressed. A question arises: which of the traditional dialects does this emergent vernacular most resemble? Or in other words, natives of which region have to adapt their dialect the least when in Banda Aceh? My tentative answer was and is: North Aceh, for the

reasons given in the grammar (1985a:6-7), but a fuller answer would require an extensive socio-linguistic survey. Certainly the original *banda* dialect is now far removed from the current Banda Aceh vernacular.

2.2 The question of what works to refer to. Stokhof upbraids me (1988:324) for not referring to more works by Acehnese scholars. His valuable list of references, although not by any means complete, does much to fill the gap. In all fairness one can point out that half of these became available after my grammar went to press. Of the rest none compare in quality with the works of Budiman Sulaiman and Abdul Gani Asyik, which I cite. Many are reports of team projects which are uninspired and plagued with typos (not the fault of the authors in most cases). This problem has even affected the examples Stokhof takes from these sources (see section 9, footnote 6 below). I did not attempt to list every source on Acehnese known to me, whether European or Indonesian, but only those which made a substantial contribution to the field.

2.3 The Acehnese oral stops. My treatment of the Acehnese oral stops is unrecognizable in Stokhof's review (Stokhof 1988:325). Some points of confusion are: (i) Voice onset time sets up a fourfold contrast in my description, not the twofold contrast between [p^h] and [p] that Stokhof reports. (ii) Contra Stokhof, 'glottal stricture' is not a parameter I use. The contrast between [p] and [b], for example, is not one of glottal stricture but voice onset timing, voicing beginning earlier for [b] than for [p]. (iii) Contra Stokhof (1988:326-327) my phonemic analysis of [b̥] as /bh/ is based primarily on distinctive features (Durie 1985a:12, [iv]). These criteria are reinforced by phonotactic patterns and informant judgements. (iv) My analysis does not result in Stokhof's Table I (1988:325), but rather in my Table I below, which is more reminiscent of Stokhof's Table 3.

Voice onset lead (voiced)	b̥	b
No voice onset lead (voiceless)	p ^h	p

- glottal noise source
 - delayed voice onset time
 - lowered F₀
 - delayed vowel intensity peak
- Absence of the features listed for [b̥] and [p^h]

Table 1

Stokhof's speculative treatment of the Acehnese stops in terms of register, or Tongue Root Advancement, is puzzling. Register is not 'a most conspicuous shared feature' (Stokhof 1988:326) of Acehnese and mainland language groups: it is not an obvious feature of Acehnese. As an articulatory account of murmur and aspiration in Acehnese this suggestion cannot be taken seriously. Significantly, although Lee (1977) and Burnham (1976) have posited both aspiration and murmur for Proto-Chamic, register has been excluded from the reconstruction. In Chamic languages the subsequent development of register (which was restricted to only some members of the family)³ is correlated with voicing: Haroi and Western Chamic tongue root advancement, for example, developed in the context of voiced stops (see Burnham 1976:46-51 and Friberg and Hor 1977:20). The 'noise source' that Stokhof refers to from my description is simply the sound of air rushing through the glottis, i.e. the 'h' sound, and needs no special explanation.

2.4 The nasal stops. A terminological clarification is in order. The symbol [°] after a stop represents the absence of an oral release, not the presence of a glottal release (see Stokhof 1988:327). This is standard usage (see Abercrombie 1967, Ladefoged 1982).

2.5 The nasal vowels. The grammar clearly states that a 'plain' nasal stop always precedes a nasal vowel (Durie 1985a:20). In other words, an oral vowel cannot follow a 'plain' nasal stop. Stokhof believes that the following vowel may or may not be nasalized. Despite his disclaimer – 'I therefore refrain from any comment on the phonetic aspects of the description' (1988:328) – this disagreement about the phonetic facts is fundamental to resolving the phonological issues raised by Stokhof in his discussion of Acehnese nasalization. It is misleading to write that neutralization of a contrast between [ma] and [mã] is 'not mentioned by Durie' (1988:328) when in the passage referred to above I clearly exclude [ma] as a possible sequence, only allowing [mã]. This question was investigated very carefully, with speakers of several dialects from all over Aceh, using spectrographs and nasal-oral airflow measurements. See also Durie 1987b.

2.6 The diphthongs. First some corrections of points of detail (see Stokhof 1988:330): (i) Regrettably there are two typographic errors on p.27 of the grammar: *ě* should represent [ɣ], and *ö* [ʌ]. (ii) The *aě* vowel letter sequence of *baě* 'howl' does not represent a sequence of vowel phonemes. A glottal stop intervenes between the two vowels (see Durie 1985a:26), thus [baʔɣ]. (iii) [aə] is not a possible diphthong, as it is excluded from the vocalic inventory (Durie 1985a:17 Table 2-4).

³ This restriction is important, because, compared to any of the Chamic languages, Acehnese has not progressed very far down the road of assimilation to the mainland type.

The question of whether the diphthongs are biphonemic or single phonemes is an interesting one. Any gain in simplicity from eliminating the diphthongs is at least balanced by the required additions to the syllable structure description. Stokhof underestimates this factor. His suggestion (1988:331) that any two-place vowel sequence ending in /ə/ is permissible does not work. /e/, /ɜ/, /o/, /ʌ/, /ĩ/, /ã/ and /a/ never form diphthongs in this way. The syllable structure descriptions would have to specify that these sequences are excluded. Also the suggestion that in C(C)V(C), V can symbolize 'any vowel or two-place vowel' (Stokhof 1988:331) is a formal trick that defeats the purpose of this type of representation. The point of using a single 'V' is that it represents a single vowel.

2.7 Pronominal clitics. (See Stokhof 1988:332-333.) In my grammar the clitic pronominals are not treated as cliticized words because in most cases they are phonologically distinct from the cliticized pronouns (thus *geu* = 'he/she/they' differs from *gopnyan* 'he/she/they') and there is no predictable synchronic relationship between the form of the clitics and their stressed counterparts. There is in addition the epenthetic /h/ which is applied to clitics but not cliticized words.⁴

Stokhof (1988:333) prefers to regard the Agent proclitics as inflectional affixes rather than distinct words. My criteria for treating these as lexical words should be clear from the discussion of affixation (1985a:32). These are: (i) word-internal phonological processes (section 3.3.1) do not affect proclitics, and (ii) proclitics can be replaced by incorporated words or even phrases (section 8.6).

Contra Stokhof, my treatment of the proclitics is not 'derivational' (Stokhof 1988:333) but syntactic. The phonological clitics are described as words, not derivational affixes.⁵

2.8 Reduplication. Stokhof writes: 'the author employs the confusing and opaque concept "emphatic" *without further elucidation*' (1988:333, emphasis mine). This is inaccurate, for I add: 'In the most general sense reduplication implies something other than the usual, with the emphasis being on something *more* than the usual' (1985a:39-40). This further elucidation covers plurality and the other specific meanings Stokhof cites (1988:334), with the exception of 'similarity'. In his example (3), intended

⁴ Stokhof rightly concludes (1988:333) that /h/ is restricted to some dialects, and even where it occurs its distribution varies. In Djajadiningrat's forms there is no /h/ because the old *banda* dialect did not have this feature.

⁵ Also, contra Stokhof, I do not report that the pronominals signal 'number'. See the opening paragraph of section 5.5 *Pronouns* and also section 5.5.2 *Plural Reference*, where I suggest that 'The pronouns do not systematically mark number' (Durie 1985a:116). The only pronouns which might be argued to mark number are first person: 'I', 'we inclusive' and 'we exclusive'. But even these are best distinguished in terms of person alone. Thus 'I' is 1, 'we inclusive' is 1+2, and 'we, exclusive' is 1+3, where the numbers represent first, second and third person. See the discussion of pronominal number in Greenberg (1988).

to illustrate similarity, *neu-neuijô-ijô* is a mistake⁶ in his source for *meu-neuijô-ijô* 'greenish (literally: having greenness)', where it is not the reduplication but the double affixation *meu-* + *-eun-* which derives the meaning 'a bit greenish' from *ijô* 'green'. This derivation pattern for colour terms was first described by Abdul Gani Asyik (1972). It conforms to general affixation patterns described in my grammar, where the relevant sections contain colour term examples: see the passages on *meu-* and *-eun-* in Durie 1985a:87,148.

Again on p.333 Stokhof misleadingly mentions 'Durie's (?semantically invariant) "emphasis"' without acknowledging my disclaimer: 'Unfortunately I cannot give a single semantic characterization of reduplication' (1985a:39). Here, as elsewhere, the review criticizes the grammar for a limitation which the grammar itself acknowledges. Drawing attention to the limitation is a good thing, but the reader should also be informed of the original source of the critical insight. In this particular case the descriptive problem is a very subtle and difficult one and the literature is full of cases of over-description of reduplication, where authors are too specific in their general claims. Although, as it happens, my account copes well with Stokhof's additional data, I can echo his words: 'Reduplication in Acehnese awaits further detailed investigation' (1988:334).

2.9 Agent and Undergoer. Stokhof's most extensive and involved criticism of my grammar concerns these grammatical functions. It is not feasible to provide a simple response to Stokhof's objections, because this discussion contains fundamental misunderstandings, which I will try to clarify here, treating each point separately. (See also Durie 1985b, 1986, 1987a, 1988a for additional discussion of this topic.)

2.9.1 In his section 6, Stokhof reports that I used three 'semantic' functions in my description: Agent, Undergoer and Dative. He remarks:

'What strikes the reader here is the quite vague definition of the semantic functions, and one wonders whether they are defined in concord with the semantics of the verb with which they appear or completely independently' (Stokhof 1988:335).

This is a misrepresentation of my analysis. Agent, Undergoer and Dative are defined in the first place as grammatical functions, in terms of their coding in the clause (by their cross-referencing and case marking). They are not presented as semantic functions. It is, however, one of the substantive claims of the grammar that the Acehnese grammatical functions, in particular Agent and Undergoer, defined in grammatical terms, have a

⁶ Also *limeueng-limeueng* 'five' in Stokhof's (2) is a mistake in the source for *limong-limong* (in my orthography *limöng-limöng*).

coherent semantic characterization, defined compositionally in terms of the verb's meaning⁷, and not independently of it. Chapter 4, on verbs, is organized to reflect this insight. In section 4.4, *Verbal Semantics*, having already defined the formal properties of the grammatical functions in section 4.2, I proceed to describe their semantic characteristics, in terms of verbal lexical semantics. It is worthwhile repeating the introduction to section 4.4 here:

'The most fundamental semantic characteristics of Acehnese verbs are those which relate to their argument semantics. It is in terms of argument semantics that this section is organized. It needs to be kept in mind that the terms AGENT and UNDERGOER refer to grammatical categories defined in terms of the cross-referencing they take (4.2).' (Durie 1985a:55.)

2.9.2 Contra Stokhof (1988:335), in Foley and Van Valin's (1984) approach the inherent lexical content of the actor NP is not the 'meaning ascribed to the Actor ... as notion'. Inherent lexical content has to do with an NP's reference; 'actor' has to do with its semantic role in the clause. Stokhof seems to have confused the crucial distinction between interpretation and definition. While the semantic role of an NP is *defined* in terms of a semantic decompositional representation of the predicate, the reference of the NP may in particular cases be an important clue to the correct *interpretation* of the meaning of the predicate, and to the semantic role the NP is playing. For example, in *I fired the gun* vs. *I fired the employee* the reference of the object phrase gives a crucial clue to the interpretation of the meaning of the verb, and thus to the semantic role played by the object.

2.9.3 In Foley and Van Valin's framework, semantic types of predicates are represented by a universal, abstract, decompositional *logical structure*. This logical structure represents predication at the level of the clause, not just the lexical meaning of the verb. As such it can include the contribution of aspectual markers and adverbials.⁸ For example, the semantic component of volition is represented in logical structure, and note that in English sentences such as: *Max instinctively smiled* vs. *Max intentionally smiled* volition is coded by the adverb rather than the verb. Interestingly, a component of logical structure which is lexicalized in verbal semantics in one language may be expressed through adverbials or by some other means in another language. In English, volition is not systematically lexicalized as part of the verb's meaning, but in Acehnese it is. On the other

⁷ I cannot understand why Stokhof reports that I have 'not ventured upon lexical decomposition' (1988:336).

⁸ The determination of the appropriate logical structure for a clause (e.g., by the linguist) is sometimes dependent, as I have noted, upon the reference of arguments, but argument reference is not a constituent part of the logical structure.

hand, the contrast between event vs. state is usually lexicalized in the English verb (e.g., *die* vs. *dead*), whereas in Acehnese it is not, thus *rhët* means 'fall' or 'fallen' and *matê* means 'die' or 'dead'. So when Stokhof writes (1988:336): 'In Foley and Van Valins's work, Agent status is apparently not part of the semantics of the verb per se', he is confusing a language-specific property of English for a general property of the theory.

2.9.4 Stokhof (1988:336) writes: 'Durie concedes that his decisive criterion for A[ctor], "control, intention", is not very effectual, but more convincing parameters are not provided'. I have never made any such concession. Stokhof seems to be referring to my discussion of *êk* 'feel the urge to, feel up to' and *têm* 'want to, choose to' (Durie 1985a:63), and has confused the tasks of semantic interpretation and decomposition. The point I made for these two verbs is that the component of control/volition is inherent in their meaning, but not inferable from other semantic components of the meaning. There is an important distinction here between what the speaker knows about the meaning of a verb and what the linguist can infer, given partial information about the meaning. Some examples should make this clear. If the linguist knows that *pajôh* is a verb used to refer to eating, he can (correctly) infer that it is a volitional verb, taking an Agent. This is because eating is normally a volitional act. On the other hand, if he knows that *matê* means 'die, dead', he can (correctly) infer that it is a non-volitional verb, taking an Undergoer, because people usually think of dying as non-volitional, something that happens to you. If, however, he is told that *êk* and *têm* both mean 'be inclined to (do something)', that *keunöng* and *poh* both mean 'strike', or that *teungeut* and *êh* both mean 'sleep', he cannot guess which of each pair is volitional, because the situations described lead equally well to a volitional or non-volitional interpretation: An inclination can result from a wish or an urge; an event of striking could be a man beating a dog or a coconut hitting the ground; sleep can be regarded as something one does when one goes to sleep, or the involuntary state of being asleep or falling asleep. In each case I would claim that the native speaker *knows* which verb describes a volitional situation, and which one a non-volitional situation, but this distinction cannot be *predicted* from the partial understanding of the meaning which is imparted by an English gloss. In many cases an English translation is insufficiently precise to reveal this aspect of the meaning.

How then can the linguist tell what the verbs mean? Through normal methods: by interviewing native speakers, and by careful observation of texts. After years of doing this for Acehnese verbs I came to the conclusion that verbs which take proclitics have a volitional meaning, while those which do not have a non-volitional meaning. Only now that I am confident enough of this result can I use the presence or absence of a proclitic as a clue to interpreting the meaning of the verb. Stokhof disagrees with this result, but I cannot understand why, for he presents no evidence of the

requisite kind, neither reports of speaker intuitions nor extensive text-based studies. The only substantial problems he raises are already noted in the grammar, where a lengthy account is offered to deal with them. The two most important issues are discussed immediately below.

2.9.5 To Stokhof's mind (1988:337), the argument of *keunöng* 'strike, coincide' takes an agent-like meaning. Later on down the page he reports that this verb's argument is 'endowed *by the author* with agent-like characteristics' (my emphasis). Nothing could be farther from the truth. The state : event contrast which is sometimes associated with the term *agent* is of no account at all here. The semantic feature claimed to be essential to Agent, as defined in the grammar, is volitional control whether in respect to a state or event, and this is not a property of *keunöng*'s argument.

The reader may ask then why *ujeuen* 'rain' is coded as an Agent in (2) but as an Undergoer in (1):

- (1) *ujeuen ka=keunöng bak=lon*
rain IN strike at me

'The rain came in on me'.

- (2) *ka=ji=tôh ujeuen*
IN 3 excrete rain

'It is raining'.

In the grammar I argue that (2) is a kind of animate weather metaphor, where 'rain' is coded as an Agent of the verb *tôh* 'excrete', which is otherwise used to refer to excretion and urination (and laying eggs). As one says in Australia of the rain: 'It's pissing down' or 'It's spitting'. Pissing and spitting are things people do volitionally. In Acehnese the metaphor can even be extended to addressing the rain in the second person, using the form of (2) (see Durie 1985a:70).⁹ In contrast, (1) is a rather more mundane example. *Keunöng* 'strike, coincide' describes a coincidence in time and place of two things, with physical contact, in this case raindrops and me. The same predicate could be used when bullets hit targets, and coconuts land on the ground. No animacy is involved, and the argument of *keunöng* is semantically a very typical Undergoer.

What kind of evidence might make me revise this analysis? If the verb *keunöng* was used to describe volitional striking by a human protagonist my account would be in trouble. However, I am confident that, although an Acehnese person might translate 'The stick struck the child' using

⁹ Some kind of grammatical weather metaphor involving animacy is not uncommon in the world's languages, as I report in the grammar.

keunöng, the same verb would not be used to translate 'The man struck the child', except perhaps to mean that the man was flying through the air and collided with the child!

2.9.6 It is a mistake to claim that no Agent-Undergoer opposition exists for verbs other than 'variable controlled/non-controlled' verbs: 'It is precisely the absence of such an opposition in the case of the other (in)transitive verbs which in my opinion makes the A[gent]-U[ndergoer] dichotomy look rather far-fetched' (Stokhof 1988:337). This opposition in fact exists for the vast majority of transitive and intransitive verbs. See the discussion in the grammar (1985a:57, 63), and the extensive treatment of the relevant derivational affixes *meu-* and *teu-* in chapter 4. I quote one crucial passage:

'Besides the possibility for some words like *galak* of directly taking either an Agent or an Undergoer, the various derivative verbal morphemes can alter the argument roles of a particular root. The prefix *meu-* (4.5.3.2) can add the element of control to a basically uncontrolled root. Other morphemes delete or reduce the element of control, leaving an intransitive verb with an Undergoer.' (Durie 1985a:57.)

2.9.7 Perhaps the most controversial aspect of my treatment of Agent semantics is the account of inanimate 'agent-metaphors'. The exceptional nature of these examples, one of which was sentence (2) discussed above under 10.5, is noted in the grammar, and considerable attention is devoted to trying to account for them within the general framework provided by my characterization of the Agent as an animate, volitional, controlling participant. So when I make the generalization that independent pronouns take animate referents (1985a:70), I immediately note that proclitic third person pronominal forms *ji=* and *geu=* 'he/she/they' are, exceptionally, used with an inanimate referent in certain specified contexts. When Stokhof writes that, in spite of my general characterization of pronouns as animate, 'obviously the proclitic forms *ji=* and *geu=* occur in a non-animate, non-volitive Agent sense' (1988:338), he is just reporting my own description.

Here, and elsewhere in the review, Stokhof seems to have trouble coming to terms with the implications of Sapir's famous dictum 'all grammars leak'. That is, generalizations have their exceptions. It is unfair to criticize a grammarian for positing rules with exceptions when the exceptions are carefully documented, and misleading for the reviewer to present the exceptions to the rule without acknowledging that the grammar reports them together with the generalization. The onus is on the reviewer, not just to report the exceptions as a problem - since the grammar does that already - but to come up with a more elegant account, either of the exceptions, or

of the general case, in such a way that the exceptions are eliminated. Unfortunately Stokhof provides the reader with no viable alternative analysis. His suggestion (1988:338) that the category 'animacy' be neutralized for the third person proclitic forms is unworkable, because the cases when proclitics can have inanimate reference are very restricted, as the grammar argues at some length. There is, for example, no way of saying 'The rock hit the tree' where 'rock' is cross-referenced by a proclitic.

2.10 Syntactic or semantic relations? I could agree with Stokhof (1988:340) that the same *syntactic* relation, manifested by prosody and linear order, exists between head and modifier in *sadeuep=keumeukoh* 'harvesting knife' and *ureueng=keumeukoh* 'harvesting person': 'harvester'. However, the *semantic* relation is distinct. In the former case, the head (*sadeuep*) is the instrument, in the latter case the head (*ureueng*) is the agent. Here Stokhof confuses these two levels of description (or perhaps he rejects such a distinction altogether).

2.11 The notion of 'subject'. Stokhof (1988:340ff) provides an extensive critique of my arguments against the notion 'subject' as applied to Acehnese. Rather than argue for my claim here, I refer the reader to Durie (1986, 1987a, 1987b, 1988a, 1988b), where the relevant issues are discussed at length. I will however consider Stokhof's alternative account of *subject*:

'I personally wish to define Subject as: "the element x selected by the Speaker as the starting-point for his (lingual) reconstruction of the projection in his mind of a situation or situations in the real and/or imaginary world". Consequently, it is the central element of the situations/events or states of affairs which are the appropriate referents of the meaning as expressed by the pertinent (co-occurring) predicate.' (Stokhof 1988:342.)

This definition is opaque and unworkable. Although Stokhof mentions four diagnostics for subject in his sense - word order, intonation, agreement, and NP marking (1988:342) - we are not told how they relate to the definition, nor how they can be used to determine the subject in Acehnese. Some questions need to be addressed: How do we know what is projected in the speaker's 'mind'? How can we tell what is the 'starting-point' for the 'reconstruction' of this 'projection'? For example, in (4-101) as annotated by Stokhof (1988:344), how can we know that the clitic *ji=* represents this starting-point?

(4-101) *parang=tumpôy*(Topic) *ta=teumeutak* *h'an=ji*(Subject)=*pajôh*
 machete blunt INC chop NEG 3 eat
 'A blunt machete won't cut no matter how much you chop.'

How do we know that 'chop' is not the starting-point, or the machete, or bluntness, or something else? Stokhof's definition may be of interest as a philosophical notion, but how does it apply to Acehnese?

Likewise, Stokhof's definition of Topic: 'THE x which is in an unspecified relation with y', where x is 'an x which occupies a more central position in the frame of reference of the speech situation than all other possible x's'. What does 'central' mean? How can we tell when x occupies it, or what x is? What is y? In what sense is *parang=tumpôy* in (4-101), which Stokhof annotates as Topic, in 'an unspecified relation with y'? These definitions are useless as descriptive tools. On the other hand, my criteria, which Stokhof dismisses as 'mechanistic', are accessible to verification and as such are substantive.

2.12 The grammar's layout. Stokhof's criticism of the KITLV for the layout of the book is misdirected. I supplied camera-ready copy in laser-printed form in order to keep the cost of the book down. At the time (1984) I had little choice about which letter-type to use.¹⁰

3. Concluding remarks. It is unusual for an author to receive the detailed review that Stokhof has given *Grammar of Acehnese*. As a critical article it deserves the serious response that I have tried to give it. Although I disagree with most of the points he makes, Stokhof does have some cogent criticisms: a map would have been a useful inclusion; an exhaustive list of clitics would have been desirable; a systematic account of how loan words are incorporated into Acehnese is yet to be undertaken. However, these useful remarks are overshadowed by many inaccuracies, some of which stem from a misunderstanding of the principles guiding the organization of grammatical information in a reference grammar, and others from a misunderstanding of the necessary treatment of exceptions to grammatical generalizations.

For less controversial responses to the book I can refer the reader to other reviews, listed in the references. Also, some major limitations are noted in the introduction to the grammar itself (Durie 1985a:8).

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¹⁰ Note that of Stokhof's corrigenda, those for p.120 and p.124 are not errors.

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