Book Reviews

Daniel Nettle, *Linguistic Diversity*. Oxford: Oxford University Press, 1999, xix + 168 pages, ISBN 0-19-823858-4, £ 35.00 (hardback), ISBN 0-19-823857-6, £ 12.99 (paperback).

Nettle's *Linguistic Diversity* sets out to provide an account for the number, distribution, and variation of the languages of the world. His approach is broad and cross-disciplinary, and coming from a linguistic background I never felt myself to have crossed completely into alien territory: comparative and typological linguistics and sociolinguistics inform all the arguments made in the book. Beyond linguistics, Nettle appeals to anthropological and geographical/human ecological arguments, and makes use of computer modeling to illustrate processes of linguistic change and diversification (see comments to Chapter 5, below). In line with a recent resurgence of interest in the parallels between (biological) genetic processes and linguistic change Nettle also keeps the biological-evolutionary metaphor clearly in view.

The first chapter of the book justifies the interdisciplinary approach taken and sets out the aims of the book. The major aim is to account for the sources of three types of linguistic diversity: (i) LANGUAGE DIVERSITY, the absolute number of languages which exist in a given geographical area; (ii) PHYLOGENETIC DIVERSITY, the number of genetic groupings of a particular level in an area; and (iii) STRUCTURAL DIVERSITY, the variation in typological parameters encountered in an area. These three types of diversity can vary independently of one another.

Chapter 2 describes the basic mechanisms of language evolution. Variation arises from discontinuities in the transmission of language between generations of speakers. The neutral evolutionary model assumes that this variation is random, although in language evolution this is not entirely the case, since acquisition can be biased both structurally (e.g., tendencies towards regularization, typological naturalness), and socially. The AVERAGING and THRESHOLD

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PROBLEMS (respectively, that random/pseudo-random change should level itself out to an accepted average, and that a preexisting form would always be acquired preferentially to an innovation due to the predominance of the former) can be overcome in a number of ways. SOCIAL SELECTION is the main mechanism, whereby the speech model of a learner is selected from the range of available models on the basis of social factors, as in Labov's classic study of dialect variation in Martha's Vineyard (Labov 1963). The place of FUNCTIONAL SELECTION in language diversification is also discussed; certain values of linguistic parameters are cognitively simpler, more iconic, more contrastive or perceptible than others. Functional selection would be the motivation behind such phenomena as chain shifts in phoneme inventories and the grammaticalization of word order in languages where phonetic erosion has obscured case markings. GEOGRAPHIC ISOLATION has traditionally been considered an important amplifier of variation, but the assumption that non-industrial cultures normally existed in isolation is now questioned, and in any case, social selection gives a mechanism whereby cross-cultural contact can lead as often to linguistic dissimilation as assimilation.

In Chapter 3 Nettle carries out computer simulations of the mechanisms of linguistic change described in the preceding chapter. From the perspective of traditional approaches to the study of linguistic change this is probably the most unusual undertaking in this book, and Nettle spends several pages (pp. 35-39) justifying it and outlining its strengths and limitations. The latter are obvious: any simulation is by practical necessity greatly simplified. Nettle recognizes that there is a risk of circularity, since the parameters chosen by the experimenter for simulation influence the results of the simulation in a way which is highly likely to correlate with the experimenter's own preliminary theorizing. The strength of evolutionary simulation in linguistics, as in biology, is that otherwise impossible experiments can at least be approximated with finite resources in a finite amount of time. Nettle shows that if the mechanisms of social and functional selection are incorporated into a computer simulation of the neutral evolutionary model, variation in (simulated) linguistic parameters occurs in a way similar to that observed in the real world. From these simulations, Nettle was able to support the following generalizations about linguistic variation (p. 54):

(i) Geographical isolation and random variation ("noise") in acquisition can produce change in continuous variables (such as vowel quality), but are insufficient to produce change in discontinuous variables unless the level of noise is very high.

(ii) In the absence of social selection, even very low levels of contact between groups destroys local diversity.

(iii) A mechanism of social selection (through which there is a bias towards acquiring the variety of some arbitrarily determined prestige model) dra-

matically increases the amount of diversity which evolves, and buffers the diversification against inter-group contact.

(iv) Functional selection can amplify or reinforce linguistic variation, but not initiate it.

These results nicely highlight the primary importance of social factors in linguistic change, and contribute to a more sophisticated understanding of the mechanisms involved.

In Chapter 4 Nettle proposes the ECOLOGICAL RISK HYPOTHESIS, which states that for non-industrial societies of a given size or population, the greater the ecological risk, the fewer languages there will be (p. 83). This seems a common-sense idea, that where people have to form large subsistence networks to guarantee survival a certain degree of linguistic uniformity is important, whereas where small, self-sufficient networks can exist there is no constraint against linguistic diversification between groups. Informal statements of this principle are found in the literature on the social motivations for linguistic change (Fortescue 1998: 18 is a recent example of this), but Nettle's approach has the advantage that it formulates the principle in a testable way. The hypothesis is tested using statistical techniques to show that in general there is a valid correlation between ecological risk and linguistic diversity. Nettle talks about pre-industrial societies as being either of the farmer-pastoralist type or the hunter-gatherer type. While the classification of populations into formal subsistence types is certainly simplistic (Ellen 1994: 41–46), it can perhaps be forgiven in this type of task. An interesting result is the generalization that hunter-gatherers have a generally greater degree of linguistic variation than farmer-pastoralists. This can be exemplified by the Australian case. Nettle's estimates of linguistic diversity based on ecological type seems to predict a total number of languages in pre-colonial Australia in the range of 17 (for a population of 251,000) to 129 (population of 750,000) languages. This can be compared to the usual conservative estimate, which is that there were more than 200 (e.g., Dixon 1980; Nettle himself comes to a figure of 266). Nettle hypothesizes that hunter-gatherers respond to ecological stress by mobility rather than by maintaining social networks. This is a reasonable hypothesis (although the greater mobility of nomadic pastoralists seems to be a force for linguistic uniformity; cf. spread zones vs. retention zones, Nichols 1992), but it does weaken somewhat the claim that ecological risk is the most important single factor.

In Chapter 5 Nettle investigates linguistic change over time. He makes use of the terminology of EQUILIBRIUM and PUNCTUATION, which has been recently adopted for linguistic purposes (by Dixon 1997) from a branch of (biological) evolutionary theory (Eldredge & Gould 1972), popularized in various works by Gould.

Nettle is careful to point out that equilibrium and punctuation are situated on a cline, and that whether a language/culture is in a state of punctuation or

equilibrium can be considered a matter of perspective. By way of illustration, Nettle describes three major periods of linguistic punctuation. The first of these, the Neolithic period, basically consists of the spread of farming (Renfrew 1998) throughout many cultures. This of course began at different times in different places. The changes of the (various) Neolithic period(s) brought about widely differing rates of expansion of populations and population density, with the concomitant increase of a relatively small set of languages. The NEOLITHIC AFTERSHOCK is a linguistic punctuation caused by the spread of certain Eurasian languages in a number of imperial and colonial expansions. The Neolithic aftershock encompasses most of the cultural and linguistic expansion of the historical period. Nettle's INDUSTRIAL PUNCTUATION could be considered the aftershock of the Neolithic aftershock, the process over the most recent few centuries in which virtually no culture has remained uncontacted, and the vast majority of cultures are integrated into a world-wide economy (although this process is by no means complete). Nettle's model permits some informed speculation about the fate of the world's languages as the industrial punctuation (presumably?) runs its course (cf. Krauss 1996 for treatment of this topic). As illustration, these case studies are sketchy, as they conflate a large number of different periods drawn out over space and time as single examples of punctuation. Nettle is certainly aware of this, but it still leaves the notions of punctuation and equilibrium inadequately exemplified and lacking a more formal definition.

Chapter 6 contains discussion of genetic (Nettle prefers the term "phylogenetic") diversity. This chapter uses the work of Nichols (1992) as its methodological starting point (as well as the major source of data), although Nettle is quite critical of some of Nichols' specific results. The main point of the chapter is to explain the well-known unevenness in the distribution of phylogenetic diversity worldwide. Phylogenetic diversity is necessarily a more abstract measure than language diversity; the distant relationships between languages do not significantly influence language use by speakers, and an explanation for differences in phylogenetic diversity cannot be sought in sociolinguistically motivated changes. Nettle gives evidence that the major parameter is the time since settlement, and that the greater the time since settlement, the lower the phylogenetic diversity can be expected to be. This is perhaps a controversial position, but Nettle presents a model in its support which could account for observed differences in the phylogenetic density of different continents, e.g., Nichols (1992: 13) who shows that "the colonized areas differ systematically from the Old World: they show greater genetic and typological diversity and distinctive typological profiles". Nettle bases his model on the assumption that phylogenetic diversification is only the norm during the initial period of settlement of a continent (a period of linguistic punctuation in Dixon's term), and that once the continent reaches its stable population (an equilibrium) then nor-

mal processes of linguistic change and interaction (areal effects and the occasional extinction) would cause a decrease in the apparent number of language stocks.

Nettle's model of typical changes in phylogenetic diversity (p. 121) seems to assume geographic isolation as an amplifier, although the role of isolation was minimized in Chapter 3. However, geographical isolation does seem to have a role in the increase of phylogenetic variation; for example, phylogenetic density tends to be higher in mountains (Nichols 1992: 13–16).

The final chapter of the book deals with structural diversity. Typology can show links between certain structural parameters within languages, and some structural features have been shown to be conservative whereas others are easily diffused. However, a general theory of structural diversity is lacking, and Nettle does not attempt to treat this topic as thoroughly as the other topics in the book. Despite this, some interesting results of computer simulation are foreshadowed (Nettle forthcoming), showing that minor structural types which are generally selected against functionally have a greater chance of establishing themselves in languages with small populations of speakers (note that this phenomenon is well attested in biological evolution). While by definition typological "exotica" occur in fewer languages, the link between minor structural types and languages with small populations, although commonly observed, has not been previously explored in a rigorous manner. This has implications for descriptive linguistics; the most theoretically interesting languages can be expected to be the ones which are most likely to disappear.

The amplifiers of linguistic change discussed in Chapter 2 produce structural diversity in a variety of ways. LINGUISTIC DRIFT is Nettle's term for diversity produced by social selection and geographic isolation; as the name suggests, linguistic drift is from a language-internal point of view essentially a random process. Functional selection, the third of Nettle's amplifiers, is particularly important for processes of structural change since it tends to give directionality to linguistic change. Languages have particular communicative problems which they have to solve, and there may be a limited number of ways of doing this. That is not to say that there is only one solution; functional selection produces diversity because within a language there are MULTIPLE OPTIMA for solutions of communicative tasks and there are COMPETING MOTIVATIONS for functional selection. A small change due to linguistic drift can have far-reaching structural consequences, such as when phonological erosion causes the loss of case markings, and subsequent functional selection favoring a new system of indicating syntactic roles, such as fixing word order.

This chapter also contains a case study, which compares size of the phonological inventory to the average size of lexical items. An increase in the size of the phonological inventory is often motivated by truncation of words. The number of vowel qualities in a language is often increased to compensate for

the loss of previously contrastive consonants: a nasalization contrast in vowels commonly comes about where following nasal consonants were lost, and likewise tone distinctions can come into being due to the loss of consonantal voicing distinctions. This study is once again carried out statistically. For ten unrelated languages the size of the segmental inventory is compared to the mean word length. The functional motivations for the determination of these two parameters are in conflict. An optimal segmental inventory is one in which the members are maximally discriminable; this means that functional selection would tend to work in the direction of reducing the size of segmental inventories. In terms of economy of expression, functional selective principles generally favor the truncation of words. The larger the segmental inventory, the smaller the mean word length a language can maintain without losing too many phonological distinctions between words. With a small segmental inventory the mean word size needs be larger to avoid non-viable levels of homophony. Nettle shows that in the sample set there really is a regular inverse relationship between these parameters, such that the mean word length in languages with larger segmental inventories is significantly shorter than the mean word length in languages with smaller segmental inventories. The important point is that there are many possible balances between the needs of economy and discriminability, and so there are many different possible functional optima.

Although not expressly addressed in the text, the back cover of the book claims that the book is intended for readers across the natural and human sciences, as well as "the informed general reader" (what used to be called "the educated layman"). This audience could certainly find much of interest, particularly readers interested in allied fields such as anthropology, archaeology, and population genetics. The writing style is engaging and accessible, and the layout is good. A proofreading error on p. 134 has "SOV" for "SVO" (in paragraph 2, line 2 of Section 7.2.2), but this is exceptional. Although the book is not aimed at specialists in the field of linguistics, it contains many interesting ideas and new approaches to issues. *Linguistic Diversity* is likely to be sympathetically received by typologists since Nettle's methodology in diachronic linguistics is always informed by typological principles, and the book would be very appropriate for students to illustrate the connections between diachronic, typological, and social-cultural aspects of linguistics.

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Simon Kirby, *Function, Selection, and Innateness: The Emergence of Language Universals*. Oxford: Oxford University Press, 1999, xvi + 156 pages, ISBN 0-19-823812-6, £ 12.99 (paperback); ISBN 0-823811-8, £ 37.50 (hardback).

Kirby's book does not discover any new universals or other typological facts, but it does make an important contribution by asking rarely-asked questions and proposing unusual answers. The genre could be called "computer-aided philosophy of typology" if there were much other work fitting this category. Kirby's main focus is on showing with computational models and simulations how functional explanations of universals actually work, but he also devotes some energy to defending Chomsky's idea of an innate component of our grammatical knowledge. In this respect, too, the book is highly unusual, and I do not know of any other work that is equally comfortable with both functionalist and generative theories and parlance. Clearly, such an approach is not likely to please everyone, and I will have some criticisms below, but I feel that linguistics needs more such works if we want to get closer to resolving some of our perennial issues.

In Chapter 1 ("A puzzle of fit"), Kirby starts out with the observation that many language universals can be shown to match some feature of language use, so that we are faced with a puzzling fit of form to function, or "appearance of design", similar to adaptation in biological organisms. He then notes that Chomsky's innatist program is not well equipped to solve this puzzle, but he also stresses that it was not designed for this purpose to begin with (an important point missed by Newmeyer 1998, cf. Haspelmath 2000): "The richly structured, innate U[niversal] G[rammar] ... posited by generative syntax is not proposed in response to the ... universals uncovered by typological research. Instead, the prime concern is the problem of language acquisition in the absence of necessary experience" (p. 15). Kirby clearly sees an important role for functional explanations, but he points out a "big flaw" (which is really only an omission) with the functional approach: Functionalists typically assume that by demonstrating a match between form (i.e., language universals) and function (especially constraints on processing), they have explained the form. But Kirby points out (following Bybee 1988 and a few others) that the puzzle of fit is left unexplained if the link between the explanans and the explanandum is not made explicit, and that functionalists have not generally addressed this "problem of linkage".

In Chapter 2 ("The impact of processing on word order"), Kirby adopts Hawkins's (1994) parsing theory of word order regularities, which is based on the idea that the central parsing preference is the preference for early immediate constituent recognition, and then proposes his solution for the problem of linkage. Like Bybee (1988), he crucially relies on language change, and he proposes that language change can be characterized as a "complex adaptive system", similar to biological evolution. The form-function fit can be explained by a model of linguistic selection, corresponding to natural selection in biological evolution. Ultimately, language universals "emerge" from this process as "emergent properties" (unfortunately, Kirby never discusses this fashionable but notoriously vague term, although it even appears in the book title). In this respect, Kirby is in line with a recent trend that stresses the similarities between language change and biological evolution (e.g., Keller 1994, Nettle 1999, Croft 2000, Haspelmath (1999b)), but what makes his model special is that he locates the source of linguistic selection (or "functional selection", in Nettle's 1999 terms) exclusively in the parser in language acquisition. For example, disfavored word order patterns tend not to be acquired by children (even though they hear them) because the parser filters them out from the "primary linguistic data" that are actually used for language acquisition. Thus, Kirby follows Lightfoot (1989) in assuming that the "trigger experience" is only a proper subset of the total linguistic experience, because passages difficult to parse are simply disregarded for acquisition purposes. I find Lightfoot's general approach implausible in many ways (cf. Haspelmath 1999a), and the very

idea of language change originating in language acquisition is of course rejected by many (cf., most recently, Croft 2000). As Kirby notes himself, a readily available alternative is to locate functional selection in the speaker, by assuming that disfavored word order patterns tend to disappear because they are avoided by speakers in order to facilitate parsing by hearers. Kirby seems to think that such an assumption of "speaker altruism" is problematic and should be avoided, but that is almost certainly an error. For the speaker to take into account the hearer's needs is not necessarily "altruistic" (in the everyday sense), because the speaker wants the hearer to understand her words, so serving the hearer's needs fits perfectly with the speaker's egoistic goals. There is of course overwhelming evidence that speakers constantly monitor their speech to adapt their phonological, morphosyntactic, and lexical choices to the hearers' needs.

Kirby then uses his model of linguistic selection for computational simulations of language change resulting in crosslinguistically preferred patterns. For instance, a VO language with overwhelming postpositional word order (with prepositions in only 10 % of the cases) turns into a stable VO&Prep language after 35 "generations" (i.e., iterations of the simulation). More complex simulations involve multiple branching structures (e.g., RelS Adj N vs. RelS N Adj vs. Adj N RelS vs. N Adj RelS, etc.) and Hawkins's "prepositional noun-modifier hierarchy". In each case, a set of simple assumptions yields the predicted patterns, illustrating the model's correctness nicely. For such relatively straightforward cases, computer simulations do not add a great deal to what we knew before, and many linguist readers may not fully understand the mathematical formulas, but the potential value of such simulations seems undeniable to me. If considered necessary, they can be enriched gradually, e.g., by building in further factors such as changes introduced by adult speakers or sensitivity to social factors. Computational simulations are widely used in evolutionary biology, and the study of language change would profit from more such studies as well, I feel.

In Chapter 3 ("Hierarchies and competing motivations"),[1] Kirby extends the simulation to implicational hierarchies, which can only be explained in terms of competing functional motivations. The specific example is Keenan & Comrie's relativization accessibility hierarchy, and Kirby shows that this, too, can be modeled by computer simulations when a few additional assumptions are built into the simulation. Most importantly, Kirby here also takes selection by the speaker into account in the case of one of the two competing motivations.

In Chapter 4 ("The limits of functional adaptation"), Kirby makes strikingly clear what was only adumbrated earlier: That he is committed not only to the functional-typological agenda, but also to the Chomskyan program. In short, he argues forcefully that the functional adaptation of Chapters 1–3 is only one type of constraint on possible grammars, and that in addition there are immutable constraints imposed by the innate Universal Grammar. His main ex-

ample comes again from the grammar of relative clauses: While psycholinguistic studies have indeed shown relativization on more deeply embedded positions (direct object, indirect object, oblique, etc.) to be more difficult to process, such studies have also tended to show that "parallel-function relatives" (i.e., relative clauses where the syntactic function of the head noun is the same as the syntactic function of the relativized element, e.g., (1a) and (1d)) are easier to process than non-parallel-function ones (e.g., (1b) and (1c)).

(1)

		head function	relativized function
a.	*The man who found me saw Ruth.*	subject	subject
b.	*The man whom I found saw Ruth.*	subject	object
c.	*Ruth saw the man who found me.*	object	subject
d.	*Ruth saw the man whom I found.*	object	object

Kirby is absolutely right to point out that psycholinguistic evidence should not only be adduced in post-hoc explanations of observed universals, but should also allow us to make predictions to be tested on crosslinguistic data. In this case a prediction would be that there should exist, for instance, languages that do not permit object relative clauses unless they modify a matrix oject. Kirby notes that this prediction does not seem to be borne out by our data, and he jumps to the conclusion that such grammars are simply not possible due to the makeup of UG: From the way relative clauses are constructed, it follows that the permitted function of the relativized element can never depend on the syntactic function of the head noun. Even though the parser probably tends to filter sentences like (1b) and (1c) from the trigger experience, speakers are forced to acquire these structures because they come in a package with the structures for (1a) and (1d). Since Kirby uses Chomskyan jargon in the way he phrases his concrete proposals, many non-Chomskyans will not be convinced by this example.[2] And it may well be that the predicted language type does indeed exist, only that it has escaped our notice so far because it is not found among the well-studied languages.

But the general point is no doubt valid, and it is one that is not sufficiently appreciated by functionalists: Even though perhaps all grammatical constraints can ultimately be given a functional explanation, there are in all likelihod also constraints on what a (functionally-motivated) grammatical constraint can be, and these meta-constraints are perhaps not themselves functionally motivated. Quite generally, syntactic processing complexity is a gradient phenomenon which "counts" numbers of words, whereas grammars seem unable to count words in this way – they can place constraints only on positions of syntactic categories. A very nice example of what this can mean in practice is Kirby's discussion of English prenominal genitives. As a prepositional VO language, English would be expected to have postnominal possessors, and the prenominal possessors (e.g., *Simon's book*) survive only through functional differenti-

ation: Animate possessors are generally prenominal, whereas inanimate ones are postnominal. From a parsing perspective, we expect only very short modifiers to be allowed prenominally, but the grammar seems unable to incorporate a rule such as "maximally two-word possessors are allowed prenominally". Instead, it restricts prenominal possessors to animate ones, which yields a very similar effect, because animate NPs tend to be significantly shorter than inanimate NPs.

Once it is recognized that there are both functionally-motivated grammatical constraints and (possibly directly innate) meta-constraints on possible grammars, we can look for criteria for telling them apart. In the past, the search for functional constraints and the search for innate meta-constraints have proceeded as if only one type of constraint existed. This situation has sometimes been lamented (e.g., Newmeyer 1998), but only by actually combining both approaches, as Kirby attempts to, can real progress be made.

Kirby adds a fifth chapter on "Innateness and function in linguistics", where he discusses the possibility of the innate constraints themselves being functionally adaptive at the phylogenetic level, as suggested by Pinker & Bloom and Newmeyer. Kirby does not come to any firm conclusion here, and he notes that the possibility of functional explanation at the level of Universal Grammar makes it more difficult to tell the glossogenetically adaptive universals apart from the phylogenetically adaptive universals. But he cites some work that attempts to tackle this problem through computer simulations, and he suggests that ultimately only an approach of "co-evolutionary linguistics", in which both kinds of adaptation interact, can succeed.

Kirby's program for explaining language universals is thus a fairly ambitious one, but despite occasional problems, I find the book very successful. It adopts an unusual (Chomskyan-functionalist) perspective, points to crucial weaknesses of both mainstream approaches to explaining universals, and comes up with concrete answers and interesting simulations. For someone who probably sees his main contribution in the philosophical and computational domains, Kirby's discussion of grammatical issues is amazingly sophisticated. I recommend this book to every typologist.

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1. Much of this chapter was published as Kirby (1997) in this journal.
2. This applies even more to Kirby's discussion of a potential counterexample, the case-matching effect in German free relatives (*Ich muss wen du mir empfiehlst*

nehmen 'I have to take whoever you recommend to mé' vs. **Ich muss wer einen guten Eindruck macht nehmen* 'I have to take whoever makes a good impression'). His strategy for explaining away this counterexample is as hermetic as the worst kind of Chomskyan work in syntax.

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Nicholas D. Evans, *A Grammar of Kayardild: With Historical-Comparative Notes on Tangkic.* (Mouton Grammar Library, 15.) Berlin: Mouton de Gruyter, 1995, xxv + 837 pages, ISBN 3-11-012795-4, DM 378.

This excellent grammar provides everything (and more!) which one has come to expect of grammars in the Mouton Grammar Library series: maps, a 546-page grammar, 60 pages of transcribed texts, a 164-page Kayardild–English dictionary with illustrations, photographs of language consultants, author index, language index, and subject index. Even for this series, however, *A Grammar of Kayardild* provides an extremely rich account of the language. It goes well beyond the requirements of a descriptive grammar in its detailed discussions of supposed prehistory of the language and the language group (Tangkic). As explained by the author, the Grammar core of the book derives from a 1985

A.N.U. dissertation. Additional textual and lexical material were collected after that and incorporated into the exemplary Texts and Dictionary sections of the present book.

Kayardild was spoken traditionally in the South Wellesley Islands, located at the southern end of the Gulf of Carpenteria, just off the coast of Queensland, Australia. By 1948 the community of Kayardild speakers had been relocated to Mornington Island in the neighboring Wellesley Islands, where 150 Kayardild speakers with varying degrees of fluency were reported at the time of writing. Evans (pp. 15–50) gives an informative account of the ecological, social, and linguistic forces which have greatly, and adversely, affected this community in the course of the twentieth century, resulting in a variety of social and political problems and some loss of language abilities. Kayardild belongs to the Tangkic family which is classified as non-Pama-Nyungan (on the basis of the pronoun set, retention of initial apical stops and nasals of proto-Australian, and miscellaneous morphological facts). Tangkic represents the easternmost group of non-Pama-Nyungan languages and contact with Pama-Nyungan languages has presumably contributed to the assimilation of some Pama-Nyungan features such as an ergative/absolutive distinction, subsequently lost in Kayardild, dependent-marking, and suffixing.

There are 12 chapters in the Grammar section of the book. Chapter 1, "The language and its speakers", introduces the reader to the socio-cultural history of the Kayardild language community, as well as containing some introductory remarks on the language and the language family. Chapter 2, "Phonology", reviews the phoneme inventory, the phonotactics, and a collection of morphophonemic rules. The morphophonemic rules include Delaminalization in nouns, whereby a stem-final laminal (dental or palatal) becomes an apical in the nominative: *nith-i* 'name-LOC' versus *nid-a* 'name-NOM', *ngij-inja* 'wood-OBL' versus *ngid-a* 'wood-NOM'. (The related Tangkic language Lardil has a similar alternation in which the apical alternant is arguably underlying, fn. 9, p. 74.) The neutralization of the apical/laminal contrast to apical in the nominative forms would appear to support Dixon's (1980: 185) claim that apical is the unmarked coronal, a claim which is not uncontroversial in the context of Australian languages (cf. Evans 1995a: 728). Chapter 3, "Descriptive preliminaries", introduces the descriptive terms to be used in the remainder of the book, with an extended discussion of the way the term "case" is being used. In previewing the terms to be used in the book, together with illustrative examples, this chapter serves also as a succinct and reader-friendly introduction to the structure of the language. Then follow the chapters describing the grammar proper: Chapters 4, 5, and 6 cover nominals and NPs; Chapters 7 and 8 cover verbals and the verb group; Chapter 9, "Syntax of the simple clause"; Chapter 10, "The modal case system"; and Chapters 11 and 12 deal with subordinate clauses. For the purposes of this review, I shall focus on a few key features

which have particular significance for our understanding of case systems and inflectional systems: complementizing case, modal case, and verbal case. It is in these areas that Kayardild and, in varying degrees, the other Tangkic languages have something very special to offer linguists. In fact, Evans begins his book (pp. 1–8) with an overview of the language's most unusual features, a considerate and helpful gesture which is particularly appreciated in a volume of this size.

Complementizing case refers to a system of suffixation, not just on nouns, but on all words, including verbs, inside certain (typically, subordinate) clauses (cf. the discussion and additional references in Nordlinger 1999: 147–148). For example, the complement clauses of verbs such as 'know', 'be ignorant that', 'think (feelingly)', 'find out', 'see', and 'hear' require this kind of case-marking, reflecting the syntactic relationship of the whole subordinate clause to the verb of the matrix clause. Thus, in (1), a complementizing case marker *-ntha*, subject to truncation in the utterance-final position, indicates the object-hood of the embedded clause. (1) also illustrates some of the possibilities for chaining together nominal suffixes in this language, e.g., GEN-INSTR-MABL-COBL:

(1) *ngada* *mungurru,* [*maku*-ntha *yalawu-jarra*-ntha
1SG.NOM know woman-COBL catch-PST-COBL
yakuri-naa-ntha *thabuju-karra-nguni-naa*-ntha
fish-MABL-COBL brother-GEN-INSTR-MABL-COBL
mijil-nguni-naa-nth]
net-INSTR-MABL-COBL
'I know that the woman caught the fish with brother's net.' (p. 5)

The complementizing case marker here is glossed as COBL, indicating a complementizing suffix based on an oblique case form. Another situation where complementizing case is required in a subordinate clause involves complex sentences in which the pivot is not the subject of both clauses. Complementizing case can also appear in various types of main clauses, including the type where one can pragmatically supply a "higher verb" like 'hear' or 'see'. Evans gives the example of people sitting at an airport waiting for a plane when someone suddenly exclaims (2):

(2) *dan*-kurrka *ri-in*-kurrka *thardawankawuru*-ntha
here-LOC.COBL east-from-LOC.COBL aeroplane-COBL
burri-jurrk
appear-IMMED.COBL
'(I can hear) the aeroplane coming in just now, here from the east!' (p. 6)

Here, the suffixes in roman are the appropriate forms of complementizing case. It is as though the utterance is embedded under an 'I hear' clause, bringing it into line with the subordinate clause use described above. The person of the subject of the clause attracting complementizing case plays a part in determining which case suffix is to be used: a 1st or 3rd person as subject selects the complementizing case based on the OBLIQUE suffix; a 1st + 2nd (inclusive) subject selects the suffix based on the locative; a 2nd person subject occurs with the locative when the speaker identifies with the addressee and the oblique otherwise.

Modal case is a system of suffixes attached to the constituents of NPs in the VP and indicate tense, aspect, and mood. These suffixes are formally similar, or even identical, to ordinary case markers of the language. For example, the suffix *kinaba* (with many phonological variants, including *-na*) functions as an ablative suffix as well as a modal case suffix with the meaning 'prior'. Thus, in (3) below, *-na* is functioning as the modal ablative marker, as is *-naa* in (1) above.

(3) *dangka-a* *raa-jarra* *bijarrba*-na *wumburu-nguni*-na
man-NOM spear-PST dugong-MABL spear-INSTR-MABL
'The man speared the dugong with a spear.' (p. 2)

The tense, aspect, and mood categories encoded by these modal cases are not identical with those found with the verbs and are not simple copies of the form or meaning of the tense inflections on the co-occurring verbs. The meaning of the modal ablative *-na* in (3), for example, is 'prior', which is not a category of verb inflection. Rather, the modal ablative is used in conjunction with either the past, ALMOST, or precondition verb inflections. A complication is that non-subject NPs which have a "subject orientation" escape modal case. Subject-oriented NPs include body part instruments (e.g., the Kayardild phrase corresponding to 'with my foot' in 'I kicked the dog with my foot') and other NPs which are semantically linked in particular ways to the subject. Evans (pp. 414–416) makes interesting comparisons between this kind of "modal blocking" and the use of ergative marking on some non-subject phrases in other Australian languages.

Verbal case refers to a system of complex suffixes appearing, like modal case, on the words in certain NPs of the VP. Each suffix complex contains a relational case marking followed by a tense suffix, based on the tense suffix of the verb, as shown in (4):

(4) *ngada* *warra*-jarra *dathin*-kiiwa-tharra *ngilirr*-iiwa-tharr
1SG.NOM go-PST that-VALL-PST cave-VALL-PST
'I went to that cave.' (p. 163)

VALL stands for verbal allative. The verbal case forms are not cognate with the ordinary case suffixes, but derive from suffixes or free verbs. The verbal allative *(k)iiwa*, for example, is a locative + inchoative complex (p. 165).

Space considerations prevent me from reporting on the interesting complexity and subtleties of these phenomena here, but Evans gives a full and lively discussion of all these matters, including detailed analyses of the forms and semantics of each of the case-marking systems and historical-comparative discussions of the phenomena. The material is a rich source of data for linguists (especially cognitive linguists) interested in the polysemy of grammatical categories, (e.g., *kinaba* marks ablative and as a modal case marker PRIOR; *kiya* indicates locative and as a modal case marker INSTANTIATED; *kuru* indicates proprietive and as a modal case POTENTIAL) and Evans offers insights into the motivations for each of these connections (pp. 407–411). I have followed Evans' terminology in referring to these suffixes as "case markers", though this is not unproblematic. Modal case suffixes may be relatable historically and synchronically to garden-variety case suffixes, but their function is to mark tense, aspect, and modality. As for the verbal case suffixes, Evans (p. 89–91) construes them as verbalizing (but inflectional!) suffixes, transforming any word to which they are attached into verbals. Labelling such suffixes as "case markers" might be stretching the term "case" a little bit too far for some linguists and I would have appreciated more discussion of this point by Evans.[1] Whether or not one agrees with his deeper analyses of the data, however, one can be grateful to him for the care he takes in describing the categories and pulling all the data together into some feasible systematization.

The historical-comparative discussions, tracing the evolution of the modern Tangkic languages from proto-Tangkic, are substantial. Ideas about the prehistory of the languages are advanced in a scholarly and balanced way, with fair consideration given to alternative hypotheses. Predictably, perhaps, it is the evolution of the case-marking systems which occupies Evans most (pp. 423–446). Again, I can not possibly do justice to the intricacies of Evans' arguments. Very briefly, the order in which I have discussed the three systems here reflects the reconstructed diachronic development. One starts with a proto-language which had a system of complementizing case utilized primarily in subordinate clauses; in the next stage, a process of "insubordination" (p. 438) transfers aspects of subordinate clause marking to main clauses, with the case markers changing somewhat in their meanings, giving rise to modal case (other factors come into play which I omit here); in the last stage, the existence of modal case facilitates the rise of verbal case in Kayardild (and the related language Lardil) (p. 183).

In the context of such a *tour de force* on the part of the author it seems almost churlish to point out some minor irritations I felt in reading the book. For one thing, I did not find the type sizes for body text, examples, and sub-

headings entirely pleasing, compared with, say, the more uniform type sizes found in another excellent book in the same series, Merlan (1994). Commas positioned after italicized words consistently have a delinquent space before them, something which surely should have been picked up before publication stage. Attempts to combine the abbreviation and the full name in forms such as "PROPrietive", "LOCative" etc. (occurring throughout the book) seem distracting rather than helpful to me. Also, a couple of mistakes in headings caused me brief moments of confusion: "Retention of initial laminals" (p. 35) should be "Retention of initial apicals" (as described on p. 32) and "Direction of motion with transitive verbs" (p. 168), as a main use of the verbal allative, should be "Direction of motion with intransitive verbs".

In the Preface (p. ix) the author points out that it was the wish of the Kayardild community to have a linguist record their language, prompting Evans' first field trip to the community in 1982. He also explains that "grammars of little known languages should be presented in as straightforward language as possible, and furnished with a generous set of naturally-occurring example sentences and texts" (p. ix). *The Grammar of Kayardild* achieves both these goals, impressively and on a grand scale, and many generations of linguists of all theoretical persuasion stand to benefit from it. My only concern is that the cost of the book may be a factor in preventing wide dissemination of its contents and this would be a truly unfortunate result. Thankfully, the cost is substantially lower for members of the Association for Linguistic Typology and the Society for the Study of the Indigenous Languages of the Americas.

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Notes

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The following abbreviations are used: COBL complementizing oblique, GEN genitive, IMMED immediate, INSTR instrumental, LOC locative, MABL modal ablative, NOM nominative, PST past, SG singular, VALL verbal allative.

1. If one rejects the idea that these tense-aspect-modality suffixes are case markers, then Kayardild will have less of the multiple case marking than has been claimed for it (p. 4, pp. 114–117; Dench & Evans 1988: 33–35; Evans 1995b: 400–409; Plank 1995: 29). Even putting aside the modal and verbal cases, however, it is still possible to have three relation-marking suffixes on a noun (cf. ex. 3–52, p. 116.) That is, even with a more restrictive definition of "case marker", Kayardild still presents a challenge to Nichols' (1986) claim that the maximum number of (relation-marking) affixes in noun morphology is two.

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