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REVIEW ARTICLE

Three book-length studies of language evolution

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Andrew Carstairs-McCarthy, *The origins of complex language: an inquiry into the evolutionary beginnings of sentences, syllables, and truth*. Oxford: Oxford University Press, 1999. Pp. xi + 260.

Simon Kirby, *Function, selection, and innateness: the emergence of language universals*. Oxford: Oxford University Press, 1999. Pp. xvi + 156.

Daniel Nettle, *Linguistic diversity*. Oxford: Oxford University Press, 1999. Pp. xi + 168.

If I find that a book I am reading for review is interesting, then I count myself lucky. If it turns out to be insightful, then I am fortunate indeed. The fact that I was given three books to review, each of which is not only interesting and insightful, but downright ENJOYABLE, makes me feel like a winner of the Linguistic Lottery. The books are the first three to appear in a new series on language evolution published by the Oxford University Press.[1] Given the quality of the first entries, my advice to all linguists is to put yourselves down for standing orders for the ones to follow.

While the three books all deal with language evolution in the broad sense, each does so in a way markedly different from the other two. *The origins of complex language* (OCL) is about the biological evolution of language, putting forth the remarkable thesis that syntactic structure was modeled evolutionarily on syllable structure. *Function, selection, and innateness* (FSI) and *Linguistic diversity* (LD) treat language evolution over historical time, FSI addressing the shaping of grammatical form by external function and

[1] As John Davey, the OUP editor for linguistics, explained to me, this isn't a formal series 'just a way of badging a group of similar titles'. Each of the three books has an identical cover design that artistically highlights the word *evolve* from a page of the *Shorter OED*. However, in keeping with the informality of the series, neither the covers nor the front matter of any of the books contains text that suggests that OUP has grouped it thematically with any of the others.

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LD the evolution of the spatiotemporal diversity of languages and language groups. Given these differences, I have no choice but to discuss each book separately.

Of the three volumes under review, OCL is by far the most ambitious, and by virtue of the territory that it stakes out, the most vulnerable to critical commentary and to alternative interpretations of the facts. Indeed, one might wonder whether a book on language origins, even one by such a respected scholar as Carstairs-McCarthy, deserves a review in any journal devoted to the scientific study of language. After all, a long tradition in linguistics considers any discussion of the origins and evolution of language to be disreputable. In 1866 the Société de Linguistique de Paris issued an outright injunction against speculation on the topic at its conferences and in its publications. Rumor has it that the Linguistic Society of America considered the same ban upon its founding in 1924, but settled instead for a 'gentlemen's agreement' (as such things were then known) prohibiting papers on language origins. Whether this story is true or not, no article in the Society's journal *Language* has ever been devoted to the topic, and even in our times a prominent theoretical linguist (Lightfoot 1991) has endorsed a ban.

The topic has been mired in ill-repute for the simple reason that good evidence for fueling sensible theories has been hard to come by. There are no archeological digs turning up specimens of the language of 100,000 years ago. While the fossil record has given us a reasonably clear picture of the evolution of the vocal tract, grammatical structure, needless to say, is not preserved in geological strata. And, most seriously, a major tool of evolutionary biology, the comparative method, is inapplicable to the study of the origins of language. This method depends on the identification of homologues to the relevant trait in some related species. From examination of the differences the trait manifests in each species, it is often possible to build plausible conjectures about its evolution. Yet the central aspects of language – syntax and phonology – have no homologues, even for our most closely related species. In other words, language is an emergent trait (or 'key innovation') and poses, along with all such traits, particularly difficult problems for evolutionary biology.

As a result, a lot of what we find written about the origins of language has the flavor of a 'just-so' story, not much more sophisticated than the bow-wow, heave-ho, and ding-dong theories reported in the introductory texts.

Nevertheless, as the volumes under review attest, I am not alone in the conviction that it is time to put the question back on the theoretical linguist's research agenda. Several developments have made it possible to remove at least some of the wooliness from the admittedly hirsute speculation that has always surrounded it. First, several decades of research in generative grammar have led to hypotheses about which aspects of language are innately determined, and therefore germane to the question of the biological evolution of language. Before we can know HOW language evolved, we need

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to be pretty sure about precisely WHAT evolved. Second, from attempts to teach signed language to chimpanzees and gorillas, we have a fairly clear understanding of the linguistic capacities of higher apes, which, in turn, provides some measure of understanding the capacities of pre- and proto-hominids. And finally, new findings in paleoneurology have led to surprising discoveries about the evolution of the brain, in particular to those areas dedicated to language.

I also feel strongly that generative grammarians have an OBLIGATION to address the question of the evolution of language. A persistent criticism of the idea of an innate UG has centered on the absence of any account of its phylogenesis. What forces, it is often asked, could have led these hypothesized universal properties of language to become incorporated into the human genome? One must concede that the absence of even the rudiments of an answer to this question has conferred a rhetorical advantage to those who reject a UG perspective entirely. For this reason, we must welcome a work like OCL, which meets the obligation head on.

Carstairs-McCarthy takes three essential, yet seemingly unrelated, properties of human language, and argues that they are inextricably linked evolutionarily. The first is the enormous number of words with distinct meanings possible in any given language. The second is the principle of language organization known as 'duality of patterning', in which languages are describable in terms of two combinatory systems – one involving meaningless sounds and syllables and one involving meaningful words and phrases. The third is the universal distinction between the syntactic categories S and NP.

As Carstairs-McCarthy notes, only the second of these has traditionally been considered a 'design feature' of language organization that distinguishes it from systems of animal communication (see Hockett 1960) and therefore posing a special problem for researchers of language origins and evolution. But, as he argues persuasively, the reasons for the existence of the first and the third are by no means self-evident. Our hunter-gatherer ancestors were 'biologically equipped to learn languages appropriate for talking about quantum physics, literary theory, and chemical engineering as well as hunting and gathering' (12). Why so, given that a biological requirement that vocabulary be small would free up brain capacity for more pressing tasks? The lengthiest and most interesting discussion in OCL concerns the S/NP distinction. Carstairs-McCarthy argues that the needs of communication could be well served without the distinction, and, to make the point, constructs several imaginary languages lacking it. In one of these languages, 'Monocategoric', simple expressions combine with one-, two- and three-place operators with no distinction between phrases and clauses. As Carstairs-McCarthy shows, Monocategoric has the same possibilities for expressing propositional content as do actually occurring human languages. He devotes an entire chapter (chapter 3) to the now classical distinction

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between truth and reference, which one might suppose to be at the bottom of some necessary distinction between sentences and noun phrases. In a sweep of the philosophical literature on the topic that takes us from Frege to Wittgenstein to Strawson (followed by a glance at Plato), Carstairs-McCarthy argues that the reverse is true. That is, it is the fact that we have evolved the grammatical distinction between S and NP that is at the root of the putative ontological distinction between truth and reference.

In a breathtaking tour-de-force of anatomical determinism, Carstairs-McCarthy ties the three properties of language ultimately to the descent of the larynx! During the *Homo erectus* period the larynx began to lower from its standard mammalian position – high up in the mouth – to its anatomically modern one. It appears that pressures unrelated to communication were responsible for the lowering larynx, in particular the adoption of a bipedal gait (Aiello 1996). Now, as is well known, the L-shaped configuration of the supralaryngal air cavities that arose with the lowering of the larynx vastly increased vocalization potential. How would our ancestors be likely to make use of this potential? Given the (not unreasonable) assumption that they had a fixed call system governed by a principle, 'synonymy-avoidance', that dictated that calls be semantically distinct,

> … a lower larynx and a more agile tongue would have triggered pressure for the brain to accommodate a larger call vocabulary. What's more, this pressure would have been independent of our ancestors' cognitive abilities or social structures at the time. It could have operated so as to facilitate the learning of more calls than were immediately needed … to achieve advantage over competitors … (131)

In other words, in one of many reversals of what even Carstairs-McCarthy admits is the common sense position (132), he posits that the availability of forms drove the creation of meanings, rather than the reverse.

Duality of patterning now follows as a matter of course. Given an expanded call system, it would not be long before individual calls would be strung together, preserving their component meanings, thereby creating complex calls. At this point, calls are now analyzable into recurring constituents at two levels and Hockett's design feature has come into being.

Now, what about the S/NP distinction? Again, human anatomy shaped human destiny. In a nutshell, this syntactic distinction was modeled on the structure of the phonological syllable, whose structure, in turn, is largely based on an acoustically-characterizable hierarchy of sonority. While theories of the syllable differ markedly among each other, virtually all phonologists today agree that they are composed of a syllabic nucleus and two 'marginal' elements, namely, an onset (to the left) and a coda (to the right). According to Carstairs-McCarthy,

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> [E]veryone agrees on the existence of three asymmetries, which are what matters to us:
> - between nuclei and margins;
> - between the two kinds of margins; and
> - between the syllable itself…and its constituents. (140)

Crucially, Carstairs-McCarthy sees SYNTACTIC structure as reflecting the same three asymmetries, and constructs the following checklist of characteristics that syntactic structure shares with syllable structure:

> (i) Every text is analyzable into sentences such that each sentence obligatorily contains a nucleus-like position.
> (ii) This nucleus-like position is filled by a class or classes of words that are substantially but not completely distinct from the classes of words that fill constituents occupying the margin-like positions.
> (iii) Substantially the same classes of words are found in all constituents occupying margin-like positions.
> (iv) Some non-nuclear constituent or constituents are privileged in onset-like fashion.
> (v) A sentence cannot occupy the nucleus-like position in a larger sentence. (151)

Characteristic (i) is the requirement that sentences reflect syllables in their need to be headed. But just as a sound of relatively low sonority may occupy the nuclear position, the head need not be a verb – characteristic (ii). (Carstairs-McCarthy gives as examples predicate nouns and adjectives in languages in which the copula is non-present.) Characteristic (iii) is the fact that the same category, NP, occurs both pre- and post-verbally, just as onsets and codas contain the same (nonsonorous) items. Characteristic (iv) draws a parallel between the existence of features particular to onsets in syllable structure and the many features of syntax particular to pre-head position. (Carstairs-McCarthy includes here a seemingly disparate group of phenomena including the properties of verb-final and V2 languages; the fact that many languages have a pre-head focus position; and the syntactically distinct nature of subjects.) And characteristic (v) is simply the inability of a sentence itself to be a head.

According to Carstairs-McCarthy's scenario, the parallels between syntactic structure and syllable structure are not accidental:

> [I]t is reasonable to conclude that the neural organization underlying syllable structure was co-opted to provide a syntax for strings of 'words' when the need became pressing. It was natural, therefore, that syntactic structure should possess features reminiscent of syllable structure. The resemblance was neither accidental nor analogical but rather homological in the evolutionary sense; that is, it came about because sentence structure

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> had originally the same biological basis in neural organization as syllables structure had. (148)

The S/NP distinction, then, is no more than a syntactic reflection of the distinction between syllables as a whole and their marginal elements.

Is the above scenario believable? I would like to answer affirmatively, but each step of the argument left me feeling that language could have developed quite differently from the way outlined in OCL. Due to space limitations, I will focus on his account of the origins of the S/NP distinction. As Carstairs-McCarthy notes, the received position (if there is such a thing in language origins research) is that syntactic structure is ultimately derivative of conceptual structure and that the S/NP distinction is therefore ultimately derivative of the proposition/argument distinction. Such a position is congenial to the basic assumptions of such otherwise disparate work as Bickerton (1990), Pinker & Bloom (1990), Newmeyer (1991), and Wilkins & Wakefield (1995). In its most general form, the received position is based on the following premises: first, the higher apes have conceptual abilities more advanced than those of other animal species, and it stands to reason that the earliest hominids were even more human-like in this respect; second, we know that in human language there is a strong homomorphism between syntactic structure and conceptual structure. Therefore, one concludes that at the time that whatever combination of brain and vocal tract development enabled the first spoken language, the existing conceptual structures were called in to serve as models for syntactic structure. In particular, since there is a rough correlation between the semantic notions 'predicate', 'argument' and 'proposition' and the syntactic categories 'V', 'NP' and 'S', respectively, it seems reasonable to hypothesize that, as language evolved, the latter were grammaticalizations of the former.

Carstairs-McCarthy addresses the received position, but in my view unsatisfactorily. By way of rebuttal to an argument of the above general structure, he demonstrates with made-up languages that 'sentences are not the only syntactic vehicles through which predicate-argument structure can be expressed' (99). Such is of course true, but is it relevant? If sentences (and their semantic counterpart propositions) ARE the vehicles through which predicate-argument structure is expressed – and all semantic theories agree that they are – then it is most parsimonious to assume that the same was true for our ancestors. In other words, if Carstairs-McCarthy is going to overturn semantics as a model for syntax evolutionarily, then his first task has to be to overturn mainstream ideas in current semantic theory.

Now Carstairs-McCarthy might reasonably object that the received position only pushes back the problem, substituting the question of why conceptual structure is the way it is for the question of why syntactic structure is the way it is. And, of course, nobody knows why. It is not 'logically necessary' that semantic representations should have the syntactic

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nature that most semantic theories (however else they mutually disagree) say that they have. So, if it is the case that the origins of syllable structure are explicable (and I will grant Carstairs-McCarthy that point) and also the case that syntactic structure is like syllable structure in enough crucial respects, then the syllable-to-sentence scenario would be very attractive.[2]

However, I was not convinced by Carstairs-McCarthy's attempt to attribute a wide variety of syntactic asymmetries to asymmetries in syllable structure. Most seriously, his theory provides no account of the origins of syntactic categories. The terms 'classes of words' and 'constituents' appear in several of the characteristics on the above checklist. If these terms are just another way of referring to phrasal categories, then how did THEY arise evolutionarily and, in particular, what are the origins of the lexical categories (N, V, P and A) that head them? The simplest assumption, I feel, is that they have their roots in meaning (nouns being grammaticalizations of objects, verbs of actions, and so on). But then the obvious conclusion is that phrasal categories are simple elaborations of the (semantically-determined) lexical categories. Another problem is that, at best, the checklist accounts only for the internal structure of SENTENCES. Phrases have internal structure too, though not all meet the characteristics in the checklist. For example, PPs and APs do not manifest characteristic (iii) – their 'onsets' and 'codas' do not contain the same types of grammatical elements. So, what would Carstairs-McCarthy say about the internal structure of these phrasal categories (and of VP and NP as well)? If he lets their semantic structure be a (partial) determinant of their internal syntactic structure, then he would have a hard time holding the line on S. But if not, then why were they not also modeled on syllable structure?

In closing my discussion of OCL, I should mention that Carstairs-McCarthy points to a sixth characteristic that one might expect of sentences if they were modeled evolutionarily on syllables:

(vi) A sentence cannot occupy a margin-like position in a larger sentence. (151)

Now, as he notes, such is manifestly NOT a property of (modern) human language – sentences can be embedded inside other sentences. So he is compelled to posit that embedding came along at an evolutionarily later period. He may be right, of course, but here too he flies in the face of conventional wisdom, which sees the ability of being able to conceptualize – and 'grammaticalize' – discrete infinity as the hallmark of the transition from *erectus* to *sapiens* (see Bickerton 1990).

[2] Carstairs-McCarthy still has the problem of accounting for the nature of conceptual structure. Possibly he would take the position that it is modeled on syntactic structure, rather than the other way around. Such an idea has 'Whorfian' implications that I am very uncomfortable with, though I will not pursue that matter here.

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But despite the above critical commentary, I must say that I was dazzled by the level of erudition manifested by OCL and its uncanny ability to marshal an astonishingly diverse and seemingly unrelated set of facts in support of its controversial hypothesis. Whatever its flaws, there is nothing like it in the language evolution literature. This book, I predict, will take over from Bickerton's *Language and species* the status of point of departure for all linguistically informed studies of the biological evolution of grammar.

Simon Kirby's *Function*, *selection*, *and innateness* is the latest, and in my opinion, the most creative, of an increasing number of works designed to demonstrate that there is no incompatibility between autonomous generative syntax and a functional explanation for why grammars have the properties that they have (see also Newmeyer 1998). FSI is an attempt to solve what many have taken to be the fundamental problem for generative grammar, namely the 'puzzle of fit'. The puzzle is the apparent fact that many language universals appear to be designed with the language user in mind. As the rich literature of functional linguistics has stressed, language structure seems to reflect to a significant degree properties of language users' minds and behaviors. Among these are their cognitive representations of the conceptual relations among the elements that make up a sentence, their strategies for successful communication, and their strategies for processing language in real time. To give an example of the first-named, the fact that derivational morphology virtually always occurs 'inside' inflectional morphology seems to reflect the fact that the former is, by many independent criteria, conceptually closer to the stem than the latter (Bybee 1985).

Kirby feels (as do I) that the most compelling extant theory pertaining to the shaping of form by function is the parsing theory Early Immediate Constituents (EIC) of Hawkins (1994). The essence of EIC is the common sense idea (and one that has a number of antecedents in the parsing literature) that language has been shaped in part to facilitate the rapid identification of phrasal constituents (and therefore to allow rapid assignment of meaning to the sentence as a whole). As Hawkins demonstrates, the preference for quick constituent structure assignment is reflected both in performance and in the grammar itself. The former is instantiated by the statistical preference for speakers of VO languages to postpose 'heavy' constituents when they have the opportunity to do so. For example, text counts demonstrate that they are more likely to say *It is plausible that it will rain today* than *That it will rain today is plausible*. The latter is reflected in the dozens of typological generalizations that form the bulk of Hawkins's book. For example, it has long been known (Greenberg 1963) that VO languages tend to have prepositions and that OV languages tend to have postpositions. Hawkins demonstrates that this generalization is a consequence of EIC.

Kirby hypothesizes that EIC makes its presence felt in the process of language acquisition by influencing the variability of word orders that the

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child learns. As a consequence, 'the frequency of use of a particular ordering by one generation is some function of the frequency of use of that ordering by the previous generation and the EIC metric of that ordering' (37). Unless some countervailing force intervenes (a force which could be sociological as much as grammatical), the orderings most favored by EIC will ultimately become grammaticalized. Perhaps the most noteworthy feature of FSI is the set of EIC-incorporating computer simulations that Kirby carries out to show that such must be the case. In one simulation (47–50), for example, he starts with a hypothetical speech community in which verb-object order and adposition order are 'dysfunctionally' associated. In the course of the simulation, the populations come to converge on the orders preferred by EIC, namely, VO / P-NP and OV / NP-P.

Complications arise when the universals to be accounted for take the form of hierarchies. For example, Keenan & Comrie (1977) demonstrated that the accessibility of noun phrases to relativization depends on the grammatical function of the gap or resumptive pronoun within the relative clause. In the following hierarchy of grammatical functions (the Accessibility Hierarchy or AH), relativization is possible in a given language for a given grammatical function only if every item higher on the hierarchy is also relativizable:

> Subject > Direct Object > Indirect Object > Oblique > Genitive > Object of Comparison

Hawkins proposes a parsing-ease account of the AH, demonstrating that it follows from EIC that the more inaccessible the position on the hierarchy, the greater the processing demands on the language user. But the first simulation that Kirby carried out that plugs in the relevant data did not lead to the AH. Rather, it led to the population having no relative clauses at all! The problem, then, is to explain why structures that appear to be functionally dispreferred have not been over time 'processed out of existence', rather than arranging themselves in a hierarchy representing their processing difficulty. Borrowing a page from the functionalist literature, Kirby suggests that 'competing motivations' are at work. That is, some countervailing force is at work that leads to structures that are dispreferred by EIC to be preferred for some other reason. Kirby suggests that subject relatives need, in many cases, to be more morphologically complex than object relatives. Building a dispreference for morphological complexity into a new simulation, Kirby's virtual populations end up manifesting the AH.

Chapter 4, 'The limits of functional adaptation', is, in my view, the most creative contribution to linguistic theory in the book. Kirby begins by demonstrating that not every experimentally-determinable functional preference is reflected in the typological properties of human language. Continuing on the topic of relativization, he reviews literature that demonstrates an experimental preference for 'parallel function relatives', in which the matrix function of the head matches the function of the gap inside

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the clause, over relatives in which the functions do not match. Interestingly, parallel function relatives are no more common cross-linguistically than the dispreferred variety. Why not? Kirby provides a compelling explanation. It is generally assumed that there is a predication relationship between the head noun of a relative and the relative clause itself. In order for the parallel function preference to be expressed grammatically (and thereby to be reflected in typology), constraints on predication would have to be sensitive to information about *wh*-movement, or vice-versa. But universal grammar does not make available such information. In other words, we have a case where the distribution of grammatical forms results from the 'competition' between parsing ease and the innate language faculty.

The final chapter of FSI addresses the question of whether functionally-motivated principles could form part of an innate universal grammar. One's first thought might be to dismiss the entire idea as absurd. However, there is nothing a priori absurd to the idea at all. A well-accepted functional mechanism, namely natural selection, can lead to the incorporation into the genome of that indispensable for survival. Kirby, in a lengthy and fascinating discussion centering around the ontological status of the principle of Subjacency, rejects (quite rightly) the strict adaptationist account of the origins of that principle presented in Newmeyer (1991), but suggests that its biologization could have been effected by means of the Baldwin effect. In other words, even an innate universal grammar might be functionally motivated.

In a more critical vein, I feel the need to call attention to a problem that Kirby himself recognizes as endemic to functional explanation. That problem derives from the need to appeal to motivations in conflict in order to explain some particular complex typological distribution of grammatical elements. Unfortunately, most of the time we have little independent means for judging the relative strength of one motivation compared to another. The danger then in computer simulations of the effects of function upon form is to unconsciously 'cook' the simulations by hypothesizing relative motivation strengths that lead to the desired results.[3] Kirby might possibly be guilty of this in one or two places. If so, however, that hardly detracts from what is, overall, a magnificent demonstration of the adaptation of language structure to external pressure.

As the title of the third volume under review suggests, Daniel Nettle's goal is to explain linguistic diversity. To be specific, he has three types of diversity in mind: LANGUAGE DIVERSITY – the number of different language spoken in a given geographical area; PHYLOGENETIC DIVERSITY – the number of different lineages spoken in an area; and STRUCTURAL DIVERSITY – the structural variation found among languages spoken in a given linguistic area. Nettle

[3] For general discussion of the problem that competing motivations pose for functional explanation, see Newmeyer (1998).

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cites concepts and results from a number of different fields of inquiry in his attempt to account for these types of diversity, including anthropology (his own area of specialization), linguistics, geography, archaeology and evolutionary biology. His conclusions, while by no means earth-shaking in their implications, are eminently reasonable and manage to tie together a number of strands of thought in an original and creative way.

LD begins, quite reasonably, with a discussion of why diversity should exist at all. Even assuming that language transmission from generation to generation is imperfect, why don't the departures from the norm average themselves out over time, leading to stasis? The answer is that not all changes have the same status SOCIALLY and not all have the same status FUNCTIONALLY. Like Kirby, Nettle constructs computer simulations to model the set of interacting factors, in this particular case those that tend to promote and those that tend to retard diversity. One of the many results that emerged from the simulations were that social selection hugely increases both the amount of diversity which evolves and its stability against inter-group contact.

As Nettle notes, it follows that 'where individuals have large and dispersed social networks, we should expect linguistic uniformity over a wide area. Where social networks are small and tightly self-contained, many distinct languages will ultimately evolve' (59). Since the driving force behind the size of a social network is based ultimately on factors that derive from the environment (in the broad sense) in which the social groups must function, LD is squarely within the tradition of ecological determinism, recently revived and popularized by Jared Diamond's wonderful book *Guns, germs, and steel: the fate of human societies* (Diamond 1997). Why did a huge number of languages, each with a small number of speakers, develop in interior New Guinea? Because the self-sufficiency afforded by the abundance of food resources kept the primary social networks small and localized. Why is Hausa the largest linguistic group in tropical Africa? Because the seasonal rainfall in the north of Nigeria encourages migrations, leading in turn to the creation of links between distant households and therefore the language being spread over a wide area. In general, then, we 'predict a general correlation between the degree of ecological risk of the environment people live in and the average size of language groups' (81). This prediction appears to be largely confirmed.

The most fascinating – and at the same time most speculative – material in LD is found in chapter 5, 'Changes in time'. Nettle attempts to calculate the number of languages likely to have been spoken at various times in human history. Given that there were between 5 million and 9 million people towards the end of the paleolithic period (around 10,000 years ago), and given the average size of hunter-gatherer communities, Nettle calculates the global language diversity of the period as being between 1667 and 9000 languages. The Neolithic societies that followed, based on farming and

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herding, would have spread out to cover much larger areas than their Paleolithic predecessors, leading to the disappearance of their languages. However, 'as the farming communities became established, they split up into units whose size depended on ecological and geographical conditions, and diversification began again' (104–105). It is only with the Eurasian expansion of the 'Neolithic aftershock' that the number of languages began to decrease rapidly. 'Most of our human heritage is disappearing before our eyes' (114).

Chapter 6, 'Phylogenetic diversity', is in large part a critique of the idea put forward in Nichols (1990) that the number of stocks (essentially, reconstructable language families) increases with time. Nettle, appealing to evidence from a variety of sources, puts forward a much more plausible scenario in which the number of stocks increases at the onset of the colonization of a new area, but then declines. The final chapter, 'Structural Diversity', is, as Nettle himself acknowledges, sketchier in its proposals than the others. Its most noteworthy suggestion is the idea that functionally disfavored structural patterns should be more frequent in languages with small numbers of speakers. Since 'the effects of random drift are greater when the population is small, … the idiosyncrasies of one influential individual can spread through [the population] very easily' (139). Presumably, this suggestion is a testable one.

In short, Nettle's book, like the other two under review, is a valuable contribution to our understanding of how and why languages have changed over time.

REFERENCES

Aiello, L. C. (1996). Terrestriality, bipedalism, and the origin of language. In Runciman, E. G., Maynard-Smith, J. and Dunbar, R. I. M (eds.), *Evolution of social behaviour patterns in primates and man* (Proceedings of the British Academy 88). Oxford: Oxford University Press. 269–289.

Bickerton, D. (1990). *Language and species*. Chicago: University of Chicago Press.

Bybee, J. L. (1985). *Morphology: a study of the relation between meaning and form* (Typological Studies in Language 9). Amsterdam: John Benjamins.

Diamond, J. (1997). *Guns, germs, and steel: the fate of human societies*. New York: W. W. Norton.

Greenberg, J. H. (1963). Some universals of language with special reference to the order of meaningful elements. In Greenberg, J. (ed.), *Universals of language*. Cambridge, MA: MIT Press. 73–113.

Hawkins, J. A. (1994). *A performance theory of order and constituency* (Cambridge Studies in Linguistics 73). Cambridge: Cambridge University Press.

Hockett, C. F. (1960). The origin of speech. *Scientific American* **203**. 88–96.

Keenan, E. L. & Comrie, B. (1977). Noun phrase accessibility and universal grammar. *Linguistic Inquiry* **8**. 63–99.

Lightfoot, D. W. (1991). Subjacency and sex. *Language and Communication* **11**. 67–69.

Newmeyer, F. J. (1991). Functional explanation in linguistics and the origins of language. *Language and Communication* **11**. 3–28.

Newmeyer, F. J. (1998). *Language form and language function*. Cambridge, MA: MIT Press.

Nichols, J. (1990). Linguistic diversity and the first settlement of the New World. *Language* **66**. 475–521.

Pinker, S. & Bloom, P. (1990). Natural language and natural selection. *Behavioral and Brain Sciences* **13**. 707–784.

https://doi.org/10.1017/S0022226700008197 Published online by Cambridge University Press

Wilkins, W. K. & Wakefield, J. (1995). Brain evolution and neurolinguistic preconditions. *Behavioral and Brain Sciences* **18**. 161–226.

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