Diachronic Typology and Changing Paradigms in Historical Linguistics: A Review Article of *Linguistic Diversity in Space and Time**

Malcolm Ross
Australian National University

Linguistic Diversity in Space and Time. By Joanna Nichols. Chicago and London: University of Chicago Press, 1992. xv + 358pp.

Only rarely does a book appear which prompts the reader to change his research paradigm – and even more rarely does the reader have the chance to review it. *Linguistic Diversity in Space and Time* (*LDST*) is such a book. Because it is, I crave the indulgence of LLM's editor and its readers, and instead of writing a 'standard' review I shall not only summarise and discuss some of the book's proposals but seek to show how *LDST* has the potential to reorient our understanding of linguistic prehistory. The scope of *LDST* is worldwide, but I will focus on its bearing on Melanesia and adjacent areas. I will also draw attention to some inaccuracies in Nichols' treatment of Pacific languages.

Nichols' stated aim was to investigate the grammatical, genetic and areal stabilities of certain structural (typological) features, the distributions they display, and possible universal determinants of these patterns. This investigation was apparently sparked by the work of the Russian linguist Georgiy Andreyevich Klimov, who claimed that certain structural features in a language could be used as predictors of others; that is, it is possible to devise a typology of whole languages, not just of certain aspects of structure. Nichols tests Klimov's claims and finds them somewhat wanting, but her research has carried her a great deal further. She finds that the features she has examined are in most cases not randomly distributed across the world's languages, nor is their distribution solely or even mainly determined by the stock to which a language belongs. Instead, there are some striking correlations of structural features with geography, and Nichols argues that these are the reflexes of events which took place early in the history of human settlement of our planet.

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of the two interpretations to Australia/New Guinea. Under the second (population-typological) interpretation, 'the input to the Australia-New Guinea divergence was a local segment of an already-formed cline of stabilization' (279; also 213-215), but under the third interpretation the settlement of Australia and New Guinea are depicted as separate events (228-229).

In chapter 8 Nichols reconstructs a three-stage spread of human language, and the scenario of the third interpretation forms part of stage B:

a) From the origin of humankind, 'probably in Africa and over 100,000 year ago' (274), until expansion out of the Old World tropics. This stage is inferred from the findings of other disciplines. It is not reflected in modern linguistic distributions.

b) Expansion from the Old World tropics across Eurasia, around the Pacific Rim and through the New World, dated to 'approximately 60,000 to 30,000 years ago' (275). The global clines arose during this expansion.

c) The post-glacial 'rise of complex societies and large-scale economies, the spread of languages driven by economic and political prestige, and consequent reduction in linguistic diversity' (275).

Note that the spreads that have occurred during stage C have actually not all been associated with complex societies. The spread of Pama-Nyungan speakers across Australia is often said to have been associated with an improvement in their pre-Neolithic technology.

This three-stage model introduces an unclarity, or perhaps simply a pair of alternate possibilities. 'The spread of a few lineages and a single structural type (verb-final, dependent-marking, accusative) over most of Eurasia, beginning with the Indo-European spread (275) obviously belongs to stage C. Eurasia (other than south and southeast Asia) displays unity, however, not only with regard to its structural type but also with regard to the clinal features. It is unclear to me why this latter unity should not also be attributed to the Indo-European spread, i.e. to stage C, rather than to stabilisation at stage B. A similar consideration applies to much of Africa (the Niger-Congo stock), Australia (as is noted on p.236), and the Pacific (the Austronesian stock).

For me, however, there is a greater problem which affects each of the above alternatives, and this is the question of **mechanism**. All three of Nichols' interpretations rest on the inference that the global clines reflect events which took place early in human linguistic history, an inference it is difficult to reject since it is hard to think up other reasons why such clines should occur. What all three interpretations lack, however, is a mechanism whereby structural features, which, by Nichols' estimation, were in place at least 8,000 and in some cases more than 50,000 years ago might continue to be reflected in the modern

languages of the planet. Two mutually exclusive and obvious mechanisms are available: contact and retention.

By contact, I mean the transfer by bilinguals of features from one of their languages to the other. Nichols implies such a mechanism at a couple of points (212-213, 224-226), and there is ample evidence from research on 'language alliances' (*Sprachbünde*) like the syntactic alliance in the Balkans and the tonal alliance in southeast Asia that this kind of contact-induced change is common. Indeed, its frequency is much greater than practitioners of the comparative method have been wont to assume (see Thomason and Kaufman 1988 for a survey). But contact-induced change can hardly be the means of long-term survival for the features which contribute to the Old World < New World < Pacific cline (inclusive pronouns, inalienable possession, and plurality neutralisation), as their very distribution speaks **against** it. Of the features sampled in *LDST*, the one which is known to change easily as a result of contact is constituent order. Its resulting distribution, as the analysis confirms, is on a scale smaller than Nichols' 'areas' and it lacks genetic stability. Inclusive pronouns and inalienable possession, on the other hand, have macroareal distribution and are genetically stable (see my discussion above). These properties are evidently the outcome of grammatical stability, not of contact-induced change.[2]

This leaves us with the alternative mechanism, i.e. common inheritance. Nichols' scenario does imply that all languages are ultimately related to each other, and I find myself forced to the inference that the macroareal variation in inclusive pronouns, inalienable possession, and plurality neutralisation is due to the fact that languages which are geographically closer to each other are likely to be more closely related than languages which are geographically further apart. This is a startling conclusion, as it says that the macroareally distributed features are genetically more stable than the phonemes and morphemes with which the comparative method operates.[3]

It must be stressed that this conclusion is mine, and differs from *LDST*. But further consequences flow from it. The first of these is that the dialect-geographical interpretation of the global clines will not work, as it is based on contact-induced change (at least, that is the

[2] I have considered the possibility that macroareal distribution might result from the very slow contact-induced diffusion of grammatically stable features, but this scenario does not guarantee a macroareal outcome. Note that I am claiming that global distribution of the clinal features cannot be explained by positing contact-induced change as the main mechanism of spread. I am not saying that they are never spread by contact-induced change: a few (but only a few) Austronesian languages in Papua New Guinea have lost the inclusive/exclusive distinction apparently as the result of contact.

[3] Note that this does **not** mean that inclusive pronouns, inalienable possession, and plurality neutralisation can be used as innovation defining subgroups. Their binary nature means that they lack the individual-identifying status required of an innovation (Nichols in press).

only mechanism I can see which would spread innovations from an Old World centre to a Pacific/New World periphery).[4] But this does not matter, as Nichols does not press this interpretation. More worryingly, it seems to me that the population-genetic interpretation must also depend on contact-induced change to achieve feature stabilisation (again, I cannot see how else a feature can spread from language to language). It follows that if the distribution of the clinal features speaks against a contact-based explanation, then the population-genetic interpretation is also unworkable for them.

This brings us to Nichols' historical inference from the cluster analysis. Although she interprets it in terms of population typology, it also works well if we assume that the global clines are mainly due to the retention of structural features, with occasional innovations. On this scenario, Old World languages were characterised from the start by absence of the inclusive/exclusive opposition, of inalienable possession, no plurality neutralisation, and dependent marking. Somewhere in southeast Asia a restructuring took place, resulting in languages with the inclusive/exclusive opposition, inalienable possession, plurality neutralisation, and head marking.[5] The centre from which circum-Pacific colonisation took place happened to include languages of both the 'old' and the 'new' kinds, resulting in the mix that Nichols finds in New Guinea, Oceania and the New World.

The careful reader will notice that I have just lumped two Pacific areas, New Guinea and Oceania, together with the New World, whereas Nichols' global cline Old World < New World < Pacific place the Pacific at the extreme. I have done this for the following reason: if we set aside the Australian languages, which are clearly *sui generis* and largely the result of a quite recent spread, and the Austronesian languages, which represent settlement somewhere around 1600 BC (both groups are strong on the three globally clinal features), then we are left with a profile in New Guinea and Oceania which is quite similar to that of New World languages. There is in any case no reason to assume that there was only one wave of immigration from southeast and east Asia into the Pacific and the New World. Nichols assumes multiple migration into the New World (229), and Don Laycock (pers. comm.) pointed out some years ago that the language map of the Sepik region of Papua New Guinea reflects multiple migrations into New Guinea.

[4] Another reason for doubting the dialect-geographical interpretation is that it requires innovations to spread an immense distance, and there is no particular reason why they should continue inexorably to do so.

[5] Note that this inference assumes that modern humans appeared in the west of the Old World and colonised the globe from there. Proponents of the theory that modern humans emerged from earlier forms of *Homo erectus* in several different areas of the globe, one of them being southeast Asia, could legitimately argue that the 'new' languages simply represent separate geneses of language in diverse regions (although no one has to my knowledge actually argued in this way).

How Australia fits into the scenario above is an interesting question. On Nichols' cluster analysis its languages preserve affinities with the Old World. But if we limit our attention to the three globally clinal features (my supposed markers of inheritance), then their affinities are with the 'new' languages, and their dependent marking, along with their ergativity, is attributed to innovation after they became isolated. This could imply that Australia was initially settled at the same time as New Guinea (rather than representing a first lonely split) and that its peculiarities arose after the end of glaciation and the submerging of the land bridge.

This scenario also raises questions of chronology, and these can only be answered by matching linguistic and archaeological event sequences. Archaeologically, the foundation settlement of New Guinea and Australia could represent a single wave, dated to perhaps 50,000 years ago. The post-glacial sea rise occurred between 16,000 and 6,000 years ago, and Australian linguistic innovations would have occurred since that time. Peter Bellwood and Matthew Spriggs (pers. comm.) tell me that a majority of archaeologists working in North America put the earliest settlement of the New World somewhere between 10,000 and 20,000 years ago, i.e. around the end of glaciation, suggesting that Nichols' date of 35,000 years ago (228) is too early. Others, however, defend a date closer to Nichols'. This means that the period of circum-Pacific colonisation may have lasted an immensely long time, beginning perhaps 50,000 years ago and continuing until the dispersal of the Austronesian stock from perhaps 6,000 to less than 2,000 years ago. One can imagine a linguistic history of huge complexity over such a time span.

Chapter 7 lays the foundations of a linguistic theory of geographic diversity. Among other things, the chapter makes considerable reference to a pair of concepts introduced in chapter 1. These are the 'spread zone' and the 'residual zone'. They are like sub-continents in size, but their definitions are based on criteria such as relative diversity, centre, periphery, and internal stability.

A proto-typical spread zone is the Eurasian steppe (15-17). Throughout traceable linguistic history, it has displayed little linguistic diversity because it has been dominated by a single language or language group: in succession western Indo-European, Iranian, Turkic, and Mongolian have spread across its openness.[6] These have all been similar in structural type: dependent marking, verb-final, accusative. Each new arrival has wiped out much of the evidence of its predecessor. Each spreading group was probably a dialect network with an innovating centre and archaisms at the periphery. A major dialect, the language of the political/military/cultural elite, served as the *lingua franca* of the steppe.

[6] Indo-European may have been preceded by the Kartvelian stock, which survives in the southern Caucasus, and by other stocks of which nothing survives.

On the periphery of the steppe is the Caucasus, a proto-typical residual zone (13-15, 21-22). Its languages are structurally and genetically diverse because the Caucasus has served as a refugium, attracting intrusive languages from the lowlands.[7] The newcomers have sometimes acquired speakers from earlier arrivals but have not generally replaced them. Hence some of the language stocks represented in the mountains are more ancient than those which have come and gone from the steppe, and there has been a steady increase in genetic and typological diversity over time. The groups in the Caucasus have remained outside the political and cultural hegemony of the steppe cultures, but have maintained links with them such that innovations have entered at the periphery. Thus, unlike the network of a spread zone, in the Caucasus it is the periphery that has been the source of innovations, and the highland interior which has remained archaic. There is no centre of linguistic diffusion, but there are areal features. Before the introduction of Russian there was no single *lingua franca*; instead there were various local bilingualisms, whereby residents of highland villages knew the language of a lower village, but not vice versa.

The reader who is familiar with Melanesia will have no difficulty in applying these characterisations. New Guinea, New Britain and Bougainville are obvious residual zones. The language maps of New Guinea and Bougainville clearly show evidence of multiple incursion, and a careful consideration of the New Britain map with its Papuan isolates and (by local standards) relatively distantly related Austronesian groups shows a similar pattern. Thurston's (1989) account of west New Britain sociolinguistics shows a pattern of bilingualism just like that described by Nichols for the Caucasus. The Pacific to the north and east of New Guinea is a potential spread zone – potential, in the sense that most of its has only been settled once, by Austronesian speakers. But the fact that the Austronesian stock gained only a very limited hold in New Guinea reinforces the latter's status as a residual zone (although for most purposes it is treated as a continent in *LDST*; see pp.21, 26).

The crucial importance of the concepts of the spread zone and the residual zone is that they totally reorient our perspective on comparative linguistics. 'The standard unspoken assumption in linguistics is that the spread zone is some kind of norm and the residual zone a curiosity just because of its diversity' (272; also 4-6). The comparative method is by definition applicable only to the languages of a stock, and these are usually the languages

[7] Roger Blench (pers. comm.) points out that the term 'refugium' is rather unfortunate. In the tropical zone people **like** living in mountains, where the soil is often very fertile and they can escape coastal and lowland diseases like malaria. This is certainly true of New Guinea, New Britain and Bougainville. However, this observation does not affect the usefulness of the concepts of spread zone and residual zone (although it suggests the latter could be better named) in reshaping the traditional view of linguistic prehistory.

of a spread zone, with Indo-European serving as the methodological prototype. But Nichols shows that the residual zone is not simply a linguistic waste-basket to which we can make no principled approach, but that it is as definable as a spread zone. Indeed, residual zones have a behaviour all of their own, which evidently follows from their sociolinguistic history. Their frequencies of head/dependent marking, alignment, word order and complexity depart from the profiles of their host continents and assume a standard profile similar to the entire sample (23, 190-191, 208), whereas, as we would expect, each spread zone departs markedly from that profile (248-249). Nichols argues that a residual zone will naturally revert to this profile if left undisturbed, because there is a universal tendency for certain features to spread at the expense of others in a high diversity, high contact, multilingual situation. The profile tends towards verb-final order, head marking, accusative alignment, and increase in complexity (251).

Nichols assumes that these parameter values are 'more likely to be borrowed in a contact situation' (251) than other values, but I am not convinced that borrowing is necessarily the mechanism by which these values reassert themselves. A likely mechanism for increase in complexity is what Thurston (1989:556-557), explaining the linguistic history of northwestern New Britain, calls 'esoterogeny', i.e. the birth of an 'esoteric' lect. An esoteric lect is 'the lect of a strong community which is valued by its speakers as an emblem of group identity and is not shared with outsiders' (Ross and Durie, in press). As Milroy (1993:219) says, 'the maintenance of *social identity* may in some circumstances be important enough to devalue the message-oriented function: to put it crudely, some groups in society may not particularly want to be understood by other groups.' Esoterogeny occurs when speakers adopt innovations which make their lect less regular, more complex, and less easy for outsiders to understand or learn. Several kinds of innovation contribute to the process (see Thurston 1987, 1989, and Anderson 1988 for examples). These include phonological processes of elision and assimilation, one of whose effects is to bond functors and pronouns to roots (a Papua New Guinea example is Maisin, described in Ross (in press)), resulting in an increase in complexity in Nichols' terms. Since esoterogeny occurs where there are numerous small and relatively independent communities, i.e. in the very circumstances encountered in a residual zone, it seems the most probable mechanism for the increase in complexity noted by Nichols.

Nichols mentions a phenomenon which may be part of this process. She suggests that the acquisition of clitic pronouns in Balkan languages was not originally a diffusion phenomenon but a spontaneous response of contact (272-273). This cliticisation can be interpreted as one reflex of the esoterogenic bonding processes I have just mentioned. It also has the effect of increasing head marking. If it could be demonstrated (and this is a matter for research) that esoterogenic bonding was more likely to increase head marking than de-

pendent marking, then we would also have an explanation for the tendency towards head marking in residual zones.

The other two features of residual zones are accusative alignment and verb-final order. It would be convenient if I could also explain these as the result of esoterogeny, but I cannot. They are, however, the unmarked values worldwide, and we have already seen that ergative (and perhaps stative/active) alignment is unstable and liable to revert to accusative. On Nichols' results, word order is also likely to revert to verb-final, a finding which has taken me by surprise, as there is evidence from pidginisation and from typological studies (Steele 1978) that natural change is likely to result in subject-verb-object order.

If the considerations above are roughly correct, then increase in complexity and in head marking are the results of esoterogeny, whilst accusative alignment and verb-final order are the unmarked structures to which languages incline. In this connection it is worth noting that New Guinea as a whole, perhaps the world's largest residual zone, does not show the results of esoterogeny (mean complexity is moderate, head and dependent marking evenly divided) but does show predominantly accusative alignment and overwhelmingly verb-final order. One could speculate about the reasons for this, but I will not do so here.

An interesting finding is that the standard profile of a residual zone does **not** include the globally clinal features (inclusive/exclusive opposition, inalienable possession, plurality neutralisation and noun classes). These behave in the same way in a residual zone as they do in the macroarea to which the zone belongs (196-197, 200-202). This, however, is in keeping with what we might expect: the high grammatical stability of these features protects them from contact-induced change. In any case, the increase in complexity and in head marking which occurs in residual zones would presumably favour these features rather than erase them.

The concepts of spread zone and residual zone represent polar opposites, of course, and there are areas of the world which can be classed as neither. In some cases Nichols seems to class a region as a residual zone on the grounds of diversity rather than of its sociolinguistic history. Such a case is northern Australia. It is well known that the area from which a stock has spread is, other things being equal, the area of greatest diversity within its spread zone (Sapir 1916, Dyen 1956). Normally, such an area is easily recognised because of the genetic relatedness of its languages, but where a stock is sufficiently ancient, that relatedness may become unrecognisable. This may be the case with northern Australia: that is; its languages may be anciently related both to each other and to the Pama-Nyungan stock. A small piece of evidence for this is that northern Australia has the same skew in head/dependent marking as the rest of the continent (190), and has not reverted to the standard residual zone profile of head marking.

An important outcome of Nichols' description and analysis of residual zones is that it provides a research framework for investigating the prehistory of such areas which the standard comparative method cannot provide, and this is of particular importance in New Guinea and the surrounding larger islands.

Nichols points out, however, that there is another limitation on the application of the comparative method: its applicability is affected by whether languages are head marking or dependent marking. Dependent marking morphology probably assists the detection and reconstruction of stocks, whilst head marking probably complicates comparative reconstruction at stock-like time depths. This means that where there are fairly consistently head marking stocks, we may have failed to detect them (169-170, 266-270). Traditionally, morphological paradigms like personal pronouns have been used as genetic indicators, but these are prone to erosion, replacement and analogical restructuring in head marking languages; in plurality neutralising languages, the paradigm may be halved (267). Head marking languages often have monomorphemic verb roots, fewer citation forms, and oft-collected body-part terms taking inalienable possessive markers which make them susceptible to processes which hinder cognate recognition. Hence we should be less skeptical about proposals of genetic relatedness in regard to head marking languages than we might be with regard to dependent marking. This does not of course exempt the comparative linguist from careful investigation of head marking languages; it simply means that we may have to work harder to demonstrate that certain languages belong to the stock, as Lynch (1978) found with the Austronesian languages of southern Vanuatu and the present reviewer with Maisin, also an Austronesian language.

I began this review article by saying that reading *LDST* has changed my research paradigm. Given that I have been critical of Nichols' interpretations at some points, the reader may well question how serious I was in making this assertion: my answer would be that I would probably never have addressed these issues at all, had I not read *LDST* and acquired a framework within which to address them. My paradigm was the tree diagram and the comparative method – together with expansions to make it work with Oceanic Austronesian languages (Ross 1988: chs.1-2). I was aware of contact-induced change, and had analysed some cases, but they remained problems to which I struggled to find a principles approach. The recognition that a residual zone has regular historical patternings which are different from but just as principled as those of a spread zone is an important key to devising new methods. It also changes the status of the traditional comparative method from that of the major method in historical linguistics to that of just the method used to investigate a spread zone.

Because many languages have histories which have been determined partly by the factors which obtain in a spread zone, partly by those of a residual zone, it is important to integrate the methods used to investigate the two situation. The traditional comparative

method works with the form/meaning correspondences which arise in the history of a stock or family. Correspondences also arise between genetically unrelated languages in a residual zone, but these are structure/meaning correspondences where forms do **not** correspond. Thus the Austronesian language Takia (Karkar Island, off the north coast of Papua New Guinea) enjoys form/meaning correspondences with numerous other Austronesian languages. But because it has been intimate contact with its Papuan neighbour Waskia and has evidently been influenced by it, there are structure/meaning correspondences between Takia and Waskia (e.g. both languages have subject-object-verb constituent order, both make the typically Papuan three-way distinction between independent, coordinate dependent, and subordinate [dependent] clauses, both use postpositions and indeed have structurally corresponding postposition paradigms, and so on). Since form/meaning and structure/meaning correspondences of this kind are in complementary distribution, it is possible to achieve some integration of method in the investigation of the two sets of historical factors (Ross in press).

Nichols draws our attention to the fact that certain structural features are relatively immune from change, apparently because of their grammatical autonomy, whilst others are interrelated and presumably change in tandem. She also points out that certain feature values have low grammatical stability and are prone to revert to an unmarked value. These parameters not only apply in language change at large, they also have specific effects on what happens in contact situations. There seems, for example, to be a complex interaction between low grammatical stability and contact among similar languages, such that contact may in certain circumstances result in the **maintenance** of an unstable feature. At the same time, there are changes which take place as the result of contact itself: esoterogeny is just one category of these. On most of these matters there has been no more than a sprinkling of case studies.

Clearly we will be much better placed to understand language change, and especially change in residual zones, if we are able to understand the interactions of structural, functional and sociolinguistic factors that lie behind the generalisations presented in *LDST*. There are a number of current research directions which are beginning to contribute to this understanding, represented by the literature in functional and cognitive grammar (e.g. Croft 1991). A diachronic perspective is offered by the growing pace of work on grammaticalisation (e.g. Heine, Claudi, and Hünnemeyer 1991; Hopper and Traugott 1993). One of the things that *LDST* does is to provide us with a framework within which we can place some of these findings, yet leaves us realising that there are huge areas of language change about which we have little understanding. And we urgently need an increase in that understanding if we are to interpret data relating to the prehistory of New Guinea and the surrounding larger islands.

I have dwelt here on Nichols' findings, her interpretations of these, and their importance for historical linguistics. There are, however, a few technical matters which require comment.

A consequence of the fact that the sampled population consists of stocks and families, not languages, is that even large language stocks are represented by a maximum of six languages (the world's largest stock, Niger-Congo, has only five representatives). Whilst this is consistent with Nichols' sampling intentions, it behooves the representative languages to be 'typical' of the family. However, where a stock includes several families, Nichols endeavours to include 'known structural diversity' in the representatives she selects. For the Austronesian stock, which is spread across her south and southeast Asia, New Guinea, and Oceania areas, the result of Nichols' sampling method is rather odd. Typologically, most Austronesianists would probably regard the languages of the Philippines, and many in Taiwan, Borneo, Sulawesi and other parts of western Indonesia as 'typical' of western Austronesian languages. But these mostly fall into an unsampled zone seemingly to keep south and southeast Asia discrete from New Guinea.[8] The southeast Asian representative of Austronesian is the atypically stative/active Aceh. Nearby Toba Batak (also on Sumatra) would have been a more typical choice. Ironically, the only 'typical' western Austronesian language in the sample is Chamorro (Mariana Islands), which falls into the Oceania area, of which it is decidedly atypical![9] The stock fares a little better with its other representatives. The 'Western Melanesian' family[10] is adequately represented by Tawala, Polynesian by Futuna, and Remote Oceanic by Ponapean, but the choice of Drehu (Loyalty Islands) to represent 'nearer' Oceanic is unfortunate. If 'nearer' refers to what is usually called 'Near Oceanic', then Drehu is wrongly assigned: it belongs to Remote Oceanic (but is atypical of it).

The sample includes languages representing twenty-five 'Papuan' (non-Austronesian) families in New Guinea and Oceania.[11] Only four are attributed to a stock. The attributions of two of these, Awtuw and Yessan-Mayo, are inconsistent with each other, whilst the **lack** of an attribution is also inconsistent in several cases. According to Laycock (1981), Awtuw and Yessan-Mayo belong to the Sepik sub-phylum of the Sepik-Ramu phylum. I tabulate

[8] I write 'seemingly', because this unsampled zone is missing from the listing on page 26, although shown on Map 1 on page 28.

[9] The presence of a western Austronesian language in the Oceania area sample is required by the fact that there are two such languages (the other is Palauan) in the area, and the sampling procedure requires that the 'family' be represented.

[10] This should be the Western Oceanic family (Ross 1988: ch.10).

[11] There are two representatives of the Chimbu family. This, as Nichols (313-314) points out, is a mistake.

below the five languages in Nichols' sample which Laycock attributes to the Sepik-Ramu phylum. Clearly it is difficult to determine which of Laycock's grouping levels – family, stock, and sub-phylum – meet Nichols' criteria for 'family' and 'stock' (although on the evidence of Laycock 1965, his Ndu family meets her criterion), but this does not explain why Awtuw is attributed to the 'Sepik-Ramu stock' (a phylum in Laycock's terms) but Yessan-Mayo to the 'Upper Sepik stock' (indeed a stock in Laycock's terms, but not the one in which he places Yessan-Mayo!).[12] Nor does it explain why Abelam, Alamblak and Yimas receive no stock attribution in *LDST*.

Language	Nichols' Family	Nichols' Stock	Laycock's Family	Laycock's Stock	Laycock's Sub-phylum
Awtuw	Ram	Sepik-Ramu	Ram	Ram	Sepik
Yessan-Mayo	Tama	Upper Sepik	Tama	Tama	Sepik
Abelam	—	?	Ndu	Middle Sepik	Sepik
Alamblak	Sepik Hill	?	Alamblak	Sepik Hill	Sepik
Yimas	Lower Sepik	?	Pondo	Lower Sepik	Lower Sepik

Similarly, Nichols attributes Hua (Gorokan family) and Kobon (Kalam family) to the East New Guinea Highlands stock in accordance with Wurm (1981), but omits this attribution for Kewa (Engan) and Ku Waru and Salt-Yui (both Chimbu).[13]

These considerations also raise the possibility – although it is not strong – that more than six families in the New Guinea sample could eventually prove to belong to the same stock, breaching one of Nichols' sampling criteria and skewing the sample.

It remains for future research to determine whether these flaws undermine Nichols' findings. The problem is that – in our present state of knowledge – any sample with a genetic base will be flawed, and one should be wary of using her data to draw any genetically based conclusions. Happily, even major sampling flaws leave her ideas untouched, and these are the book's most significant contribution.

This outstanding book is not easy to read. There are several reasons for this. One is endemic in the subject matter: Nichols' findings are a multidimensional network of interrelated facts which will place a strain on any intellect, and their presentation is complicated –

[12] Laycock's higher level groupings are controversial, as Foley (1986) has pointed out, but this does not affect the points I am making here. Nor do Nichols' attributions seem to owe much to Foley's classification.

[13] Recent research indicates that Kobon belongs to the Adelbert Range stock, not the East New Guinea Highlands stock, but this information postdates the publication of *LDST*.

justifiably so – by the fact that some of them permit more than one interpretation. Another is that the level of abstraction is generally high, and at times presupposes knowledge which the reader may not have. A mundane example of this is that Silverstein's (animacy) hierarchy is referred to on a number of occasions, but its nature and significance are not explained. A second is that the reader needs to know more about population genetics than the present writer does.

The technical presentation of *LDST* is good. I found only one typo, a mildly amusing one, where 'high count' is replaced by 'high court' (top line of table 88, page 233). One reference, Mel'čuk (1974), on page 49, is not in the references.

But it is all too easy to criticise even the greatest book. *LDST* has changed the way I look at historical linguistics, and it belongs in the library of every historical linguist and typologist.

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Research School of Pacific and Asian Studies, Linguistics
Australian National University
Canberra, ACT 0200
AUSTRALIA