

Das Thema des Autors und sein Wissen darüber hätten mehr als die vorhandenen 175 Seiten Text verdient, und es ist zu hoffen, daß er in Zukunft noch einige Aspekte seiner jetzt vorliegenden Arbeit vertiefen wird.

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Nichols, Johanna: Linguistic Diversity in Space and Time. Chicago: The University of Chicago Press, 1992. 358 pp. ISBN 0-226-58056-3. Price: \$ 45.95

In this book, Nichols proposes to investigate human prehistory not with the traditional tools and methods of comparative linguistics, but with a new approach called "population typology." Her goal is "detecting affinity at great time depths and describing early linguistic prehistory" (1). The justification for this new approach is that "the comparative method does not apply at time depths much greater than about 8000 years" (2). Thus, in place of cognate words – all of which have allegedly disappeared after 8000 years –, Nichols uses just ten grammatical features ("head/dependent marking, complexity, alignment, word order, PP's, inalienable possession, inclusive/exclusive pronouns, plurality neutralization, noun classes, and numeral classifiers" [259f.], which she herself notes are not independent of one another. A population biologist might wonder about the feasibility of using just ten interdependent features from a single area of language structure to reconstruct all of human prehistory. Most linguists will wonder about the feasibility of using typological traits at all in the investigation of genetic affinity, after Greenberg's demonstration of their absurd consequences in Africa.

Putting aside for the moment these qualms, what are the results of Nichols' study? Does she confirm the validity of Greenberg's African classification? What is the import for the Nostratic, or Eurasiatic, families that seek to connect Indo-European with other Eurasian families? And what about Amerind, Greenberg's recently proposed family uniting all Native American languages, save Eskimo-Aleut and Na-Dene? Actually, Nichols' book does not explicitly discuss any of these matters, and the results of her study are so nebulous and imprecise that families as sharply delineated as Eurasiatic, Amerind, or Nilo-Saharan are invisible to her methods. So, too, would be Indo-European, Uralic, Sino-Tibetan, Austronesian, Afro-Asiatic, or Niger-Congo for that matter, but Nichols makes no attempt to demonstrate that population typology is capable of identifying even such obvious and universally recognized families. Apparently population typology only works before 8000 B.P., while the comparative method only works after that date.

Nichols' principal discovery is that "four features – inclusive/exclusive oppositions, plurality neutralization, adpositional phrases, and inalienable possession – have distributions which take the form of global west-to-east clines, and noun classes may be a fifth" (206). However, the reader's excitement over this discovery will certainly be tempered by Nichols' admission that "these distributions can be called west-to-east only in the sense that

the Pacific behaves as though it were east of the New World. The areas of the Pacific are separated from the Old World by more miles of open ocean than the New World is, and it would be possible to weight the distance of a water mile so as to derive a more eastern adjusted geography for the Pacific" (206f.).

Are there really any clines at all? I think not. Let us take a closer look at the putative inclusive/exclusive cline, which Nichols considers "a garden-variety instance of stabilization." According to Nichols' theory, the origin of this particular cline is roughly as follows. She assumes an initial population in Sahulland (New Guinea and Australia, before they were separated by a rise in the sea level) that had differing frequencies of the inclusive/exclusive feature, somewhat higher than 50% in the south, which became Australia, somewhat lower than 50% in the north, which became New Guinea. Subsequent to the isolation of New Guinea and Australia, the frequency of this feature reached an equilibrium of 100% in Australia and 0% in New Guinea. This, in turn, is part of a worldwide cline, with highest frequencies of the inclusive/exclusive in the Pacific (which, because of the water miles, is to be thought of as in the Atlantic), the lowest frequencies in the Old World, and frequencies intermediate between the two in the Americas.

There are, however, some problems with the above explanation. First, I know of no linguist who considers the Austronesian inclusive and exclusive pronouns to be historically connected with the Australian inclusive and exclusive, much less these two systems with the other inclusive/exclusive patterns found around the world. All the evidence indicates that these are *historically independent* creations of the same structural type that have nothing whatsoever to do with one another. This is after all what typology is all about: structural similarities that are historically independent. Second, there is a very good reason – natural selection – why gene frequencies stabilize at different values in different regions, but natural selection does not operate on language. What force, then, is responsible for the stabilization of the inclusive/exclusive feature at different rates in different regions? That topic is never broached.

The global clines are not, however, the only discoveries of population typology. Other revelations include: (1) The presence of noun classes in Niger-Kordofanian is due not to the process of grammaticalization proposed by Greenberg, but to "location in or near a hotbed" (139). "... geographical factors suffice to explain the fact that gender exists at all in the Niger-Congo stock: Africa is a class hotbed, and we would expect to find noun class systems in this family" (140). (2) "For the New World areas, affinities are mostly within the New World, except that eastern North America is peripheral and isolated, with more affinities to New Guinea than to the rest of New World" (224). If this were true, then the Algonquian languages would be closer to New Guinea languages than to the Wiyot and Yurok languages located on the northern California coast. Yet Wiyot, Yurok, and Algonquian are universally recognized – except by Nichols, apparently – as forming a

valid family. (3) "... rapid circum-Pacific colonization ... including the first colonization of the New World ... was underway by about 35,000 years ago" (228). It would appear, however, that these first Americans were extraordinarily neat, and left no trace in the archaeological record of their first 20,000 years in the Americas. (4) The ancient Indo-European homeland is to be located neither in Anatolia nor north of the Black Sea – the two favored homelands of traditional comparative linguists – but at some unspecified Asian location "east of the Urals" (236).

There are problems with this book that go beyond its substantive fantasies. Despite the assertion on the cover jacket that this book "will be of critical interest to linguists, archaeologists, [and] population specialists" the book is essentially unreadable to anyone without a thorough background in linguistics and linguistic typology. Terminological jargon proliferates on every page, much of it of Nichols' own creation, and few archaeologists are likely to comprehend casual references to such linguistic terms as "pro-drop," "Aux," "neutral dominant alignment," or "Aktionsarten," much less Nichols' own creations of "macrogender," "hotbeds," and "spread zones." Indeed, in her exuberance to coin new linguistic terms, she has even added three new words to the English language: "dispreference," "taxonomized," and "taxonimization."

Inasmuch as one must wade through over 200 pages of such typological jargon before the subject of human prehistory even appears, it seems more than likely that most, if not all, archaeologists and molecular biologists will skip these pages and jump to the book's conclusions. There is a peril here too, however, for Nichols eventually concludes (274f.) that there were three stages in the spread of modern humans over the earth: (1) expansion out of Africa around 100,000 years ago, (2) a second stage of expansion to Australia, New Guinea, and the Americas between 60,000 and 30,000 years ago, and, finally, (3) a third stage of expansion beginning with the end of the glaciation around 10,000 years ago. The reader who skips the typological sections (which is most of the book) will assume that these sections provide some evidence for this three-stage dispersal theory. In reality, there is not a scintilla of evidence for any of it, and the whole scenario seems to have simply been grafted onto the rest of the book because it fits the emerging archaeological and genetic perspectives.

The book is really unreadable even for linguists, and every review I have seen of this book – all by linguists – mentions its "difficult" nature. Virtually, every page contains passages that are inscrutable. For example, to explain why Greenberg was so successful in his African classification ("a paradigm case of scholarly success" [5]), but not in his New World classification, Nichols proposes that "the languages of Africa prove to have been underanalyzed raw data for comparative work, while those of the New World prove otherwise" (5). Or consider: "Diversity of a particular kind may even be regarded as the state to which a group of languages will naturally revert if left undisturbed"

(23). Or: "Both noun classes and numeral classifiers are hotbed phenomena, and their hotbeds are smaller than continental in size. But the propensity to contain hotbeds has a geographical scale of its own" (199). Or: "the Old World has less internal diversity than either of the other macroareas. In such cases it is sometimes difficult to tell active areality on the part of the Old World from default unity of the Old World where diversity is greatest in the colonized areas" (205).

This book is based on an absurd assumption – that the comparative method in linguistics is only capable of discovering the obvious: Indo-European, Uralic, Dravidian, Bantu, Algonquian, and other families of recent origin. This initial absurdity is compounded by additional absurdities on almost every page, but couched in such obscure – and obscurantist – jargon, buttressed by no less than 96 tables, and validated by numerous chi-tests, that to an outsider the work appears one of great erudition, rather than the pretentious nonsense it really is. This is a work, not of science, but of science fiction, and it tells us nothing whatsoever about human prehistory.

Merritt Ruhlen

Nichter, Mark (ed.): *Anthropological Approaches to the Study of Ethnomedicine*. Langhorne: Gordon and Breach Science Publishers, 1992. 259 pp. ISBN 2-88124-529-3. Price: \$ 24.00

The book under review includes a broad range of ethnomedical studies first organized for a special edition of the journal *Medical Anthropology* (13 [1–2], 1991)! As stated in the preface, the "objective of this volume is to broaden appreciation of ethnomedicine as a multidimensional subject of inquiry contributing to and benefiting from contemporary anthropological theory and methods" (vii). Ethnomedicine is seen not just as a subfield of medical anthropology, but as the "study of the full range and distribution of health related experience, discourse knowledge, and practice among different strata of a population; the situated meaning the aforementioned has for peoples at a given historical juncture; transformations in popular health culture and medical systems concordant with social change; and the social relations of health related ideas, behaviors, and practices" (ix).

The collection is comprised of ten essays, an introduction, and a critical commentary (the latter two by the editor, M. Nichter). They include articles on the efficacy of traditional medical forms of treatment and the biomedical evaluation (Anderson), an epidemiological description of the culture-bound syndrome *empacho* in Guatemala (by Weller, Ruebush II; and Klein), several articles on discursive practices in the context of healing (with examples from Haiti, Peru, and South India [Broxwin, Glass-Coffin, and Nuckolls]), symbolic approaches to the study of traditional medical systems (example from Jamaica and Rwanda [Sobo, Taylor]) and on the personalities of healers and patients (examples from Tibet, Malaysia, and South India [Adams, Laderman, Trawick]).

Instead of discussing all chapters, I concentrate