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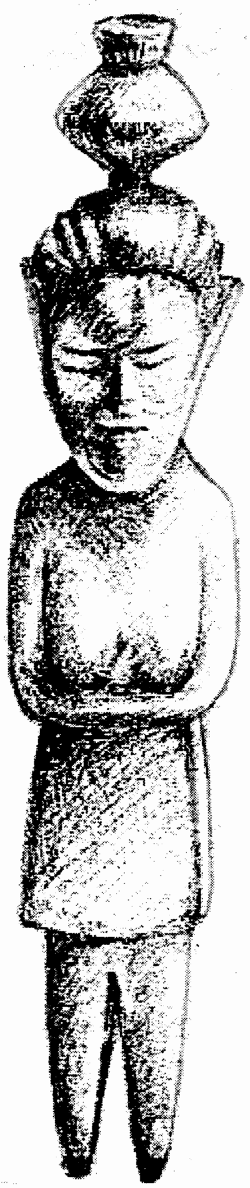

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AIM & SCOPE

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LINGUISTIC METHODOLOGY AND DISTANT LINGUISTIC COMPARISON

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Distant (or long-range) Linguistic Comparison seeks to investigate the possibility that certain languages or language families, not previously thought to be genetically related, at least not "closely" related, might indeed be part of still larger groupings, which may be called "macrofamilies".

At the present time, there is a handful of scholars in various countries devoting serious study to long-range comparison. Furthermore, two organizations, namely, the Language Origins Society and the Association for the Study of Language in Prehistory, have been founded for the specific purpose of furthering the cause of investigating distant linguistic relationship. Some of the work being done is of very high quality, adhering quite strictly to the methodological principles established by the founders of Indo-European comparative linguistics, while other work is quite speculative and less methodologically rigorous. Moreover, there are two main approaches being utilized: using terminology coined by Hal Fleming, the first approach may be called "taxonomy first", which seeks first and foremost to classify languages into valid groupings, that is, into language families and/or macrofamilies, while the second approach may be called "reconstruction first", which, as the name implies, emphasizes reconstruction. The first approach is reminiscent of the beginnings of Indo-European comparative linguistics, where relationship was first established by the early pioneers such as Rasmus Rask, Franz Bopp, and Jacob Grimm, and it was only much later, beginning with August Schleicher, that actual reconstruction took place, though the need for reconstruction had been recognized as early as 1837 by Theodor Benfey. The two approaches are actually not mutually exclusive, but, rather, properly used, they can inform and further one another. I, personally, would give the edge to "taxonomy first". After all, one cannot successfully reconstruct until one has first established which languages might have a reasonable chance of being genetically related, that is to say that one must know which languages to compare.

The early founders of Indo-European Comparative Linguistics placed great importance on the comparison of grammatical forms, and this bias continues to the present day in Indo-European Studies and has even been carried over into the study of other language families. However, this overemphasis on the comparison of grammatical forms is far too restrictive and was the reason that the Celtic languages, which have developed many unique features, were not immediately recognized as Indo-European. As noted over sixty years ago by Holger Pedersen (1931:245):

> That agreement in the inflectional system is an especially clear and striking proof of kinship, no one denies. But it is only an anachronism in theory, which has no significance in actual practice, when such an agreement is still designated as the only valid proof. No one doubted, after the first communication about Tocharian..., that the language was Indo-European, though at that time virtually no similarities in inflection had been pointed out. Such similarities have since been shown, but even where they are almost obliterated, proof of kinship could be adduced from the vocabulary and sound-laws. Hardly any one will assert that it would be impossible to recognize the relationship between, say, English and Italian, even without the help of other related languages or of older forms of these two languages themselves, although agreements between the inflectional systems are practically nonexistent.
>
> From the modern point of view it must be said that proof for relationship between languages is adduced by a systematic comparison of languages in their entirety, vocabulary as well as grammar. The reason why earlier scholars felt they should disregard the vocabulary was that they knew of no method of systematic comparison in this field.

In 1957, Joseph Greenberg laid out a set of principles for establishing genetic relationship among languages, and these bear repeating. Greenberg notes that the simplest way to establish genetic relationship is by identifying a large number of similar morphs (or allomorphs) — especially irregularities — in similar environments in the languages being considered. Another significant indicator of probable genetic relationship is the presence of similar rules of combinability. Unfortunately, and this is significant, historical processes over the passage of time bring about the gradual transformation and eventual elimination of such similarities. The longer the period of separation, the lesser the chances will be that similarities of morphological forms and rules of combinability will be found. Fortunately, there are other factors that can be helpful in determining possible genetic relationship. One significant factor is the semantic resemblance of lexical forms. Here it is important to be able to establish recurrent sound-meaning correspondences for a reasonably large sample of lexical material. Lexical forms with identical or similar meanings have the greatest value. Next in value come forms that, though divergent in meaning, can convincingly be derived, through widely-attested semantic shifts, from earlier forms with identical or similar meaning. The chances that lexical resemblances indicate genetic relationship increase dramatically when additional languages are brought into the comparison and when these new languages also exhibit a very large number of recurrent sound-meaning correspondences with the other languages. Greenberg considers the comparison of basic vocabulary from a large number of languages from a specific, wide geographic area to be the quickest and most reliable way to determine possible genetic relationship. To be meaningful, however, comparison must strive to eliminate chance resemblances and to separate borrowings from native elements. This is often easier said than done; however, Greenberg lays out two main techniques for detecting borrowed lexical items. First, he notes that borrowing is commonly confined to certain grammatical spheres (for example, cultural items) and certain grammatical categories (nouns far more often than verbs). Second, borrowed words can be distinguished from native vocabulary by expanding the range of comparison to include additional languages.

It is only after these preliminary steps have been undertaken that meaningful comparison can begin. That is to say, and to reiterate, we must first have a good sense of which languages are likely candidates for comparison.

The basic principles underlying the Comparative Method may be summarized as follows: The first step involves the arduous task of data gathering. Once a large amount of lexical material has been gathered, it must be carefully analyzed to try to separate what is ancient from what is an innovation and from what is a borrowing. Once the native lexical elements have been reasonably identified within each phylum, the material can be compared across phyla to determine sound correspondences. Not only must the regular sound correspondences (that is, those that occur consistently and systematically) be defined, exceptions must also be noted and explained. Here, widely-attested sound changes (palatalization, metathesis, assimilation, dissimilation, syncope, etc.) provide the key to understanding the origin of most exceptions. In other cases, analysis of the influence that morphology has exerted will provide an understanding of how particular exceptions came into being. Some exceptions, however, though clearly related, simply defy explanation. All of these must be noted. The final step involves the reconstruction of the ancestral forms and the formulation of the sound laws leading to the forms in the descendant languages, identifying the laws that have produced the regular sound correspondences as well as the exceptions. The same principles apply to the reconstruction of the grammatical forms and rules of combinability and to the identification of the modifications leading to the systems found in the descendant languages. Invariably, it takes the dedicated efforts of several generations of scholars to work out all of the details. Here, we may cite the case of Indo-European — as even the most casual reading of Lehmann's new book (1993) on the *Theoretical Bases of Indo-European Linguistics* shows, after nearly two full centuries of investigation of what must surely be the most thoroughly-studied language family on the face of the earth, there still remain many uncertainties about the reconstruction of the Indo-European parent language.

It was necessary to discuss these issues in order to address concerns that have been raised about the applicability of traditional methods of comparison to long-range comparison. It must be made perfectly clear that the same principles are just as applicable to long-range comparison as they are to any other type of linguistic comparison.

Furthermore, claims that these methodologies break down when one tries to apply them beyond a certain time limit, say, 5,000 to 10,000 years ago, can be shown, without a shadow of doubt, to be false. One can cite, for example, the case of the aboriginal languages of Australia. Archaeological evidence indicates that Australia has been inhabited by human beings for approximately 40,000 years. Though there remain many unsettled questions, such as exactly when Proto-Australian was spoken (probably at least 30,000 years ago), or about how the different languages should be subgrouped, and so on, there is no question that all extant languages belong to the same language family (cf. Ruhlen 1991:188), and comparative work on these languages is continuing apace. Another example that can be cited is the case of the Afroasiatic language family. Due to the extremely deep divisions among the six branches of Afroasiatic (Semitic, Egyptian, Berber, Omotic, Cushitic, and Chadic), which are far greater than those found, by way of comparison, among the earliest attested branches of Indo-European, the Afroasiatic parent language must be placed as far back as 10,000 BCE, or perhaps even earlier, according to some scholars. This extremely ancient date notwithstanding, the major sound correspondences have been determined with great accuracy, excellent progress is being made in reconstructing the common lexicon, and scholars are beginning to piece together the original morphological patterning, though progress here lags behind other areas.

One last point needs to be made: Reconstructed languages should be thought of as real languages in every sense of the term. This means that we should be very careful not to reconstruct anything that is not characteristic of language in general: our goal should be to strive for reality in our reconstructions, and we should not hesitate to use every means at our disposal to help us arrive at realistic reconstructions. It goes without saying that we must be fully cognizant of the work of our predecessors and adhere closely to the time-honored methodologies — the Comparative Method and Internal Reconstruction — that have served Comparative-Historical Linguistics well since the days of Bopp, Rask, and Grimm. However, we must not stop here — we must also make full use of recent advances in phonological theory that have broadened our understanding of sound change and of new insights gained from typological studies, and our proposals must be consistent with the data. And, finally, we must learn to practice a little humility, realizing that every theory has its advantages and disadvantages: some theories will have one advantage, some will have another, some will be patently silly, and so on.

One large-scale grouping that has been proposed at various times and by various scholars is the so-called "Nostratic" macrofamily — the name "Nostratic" was first suggested by Holger Pedersen in 1903 (it is derived from Latin *nostras* "our countryman"). Though the "Nostratic Hypothesis" has occupied the efforts of a handful of scholars from time to time, for the most part, it has been ignored by most scholars — the early work done was simply not of high quality and, therefore, was not convincing. However, beginning in the early 1960's, interest in the Nostratic Hypothesis was revived by the work of two Russian scholars, namely, Vladislav Illič-Svityč and Aaron Dolgopolsky, who first started working independently and, at a later date, through the efforts of Vladimir Dybo, cooperatively. Their work, though not without its own shortcomings, was the first successful demonstration that certain language phyla of northern and central Eurasia, as well as the ancient Near East, might be genetically related. Following Pedersen, they employed the name "Nostratic" to designate this grouping of languages. In particular, Illič-Svityč, in the course of several publications, culminating in his posthumous comparative dictionary, which is still in the process of publication, included Indo-European, Kartvelian, Afroasiatic, Uralic, Dravidian, and Altaic in his version of the Nostratic macrofamily. From his very earliest writings, Dolgopolsky also included Chukchi-Kamchatkan. After Illič-Svityč's untimely death in 1966, this work was continued by Dolgopolsky as well as several other Russian linguists.

The first question that should be addressed is: What is the basis for setting up a Nostratic macrofamily? First and foremost, the descendant languages can be shown to share a large common vocabulary. In an article published in 1965, Illič-Svityč listed 607 possible common Nostratic roots, but only 378 have been published to date in his posthumous comparative Nostratic dictionary. It should be noted that there are differences between the etymologies proposed in 1965 and the items included in the later dictionary: first, some of the items listed in 1965 do not appear in the dictionary; next, minor changes have been made to several of the earlier etymologies. Dolgopolsky currently claims to have approximately 1,500 common Nostratic roots, but none of this material has been published as yet. I have a great deal of lexical material (approximately 25,000 cited forms) from the Nostratic daughter languages to support 601 common Nostratic roots. This material will appear shortly in a joint monograph by myself and John C. Kerns, entitled *The Nostratic Macrofamily: A Study in Distant Linguistic Relationship* (Mouton de Gruyter). It should be mentioned here as well that Greenberg is currently preparing a book entitled *Indo-European and Its Closest Relatives: The Eurasiatic Language Family* (Stanford University Press) in which a large amount of lexical material will be discussed, though Greenberg's Eurasiatic is not the same as Nostratic (see below). As is to be expected, the various branches of Nostratic investigated to date exhibit regular sound correspondences, though, it should be mentioned, there are differences in interpretation between Illič-Svityč and Dolgopolsky on the one hand and myself on the other. Finally, a moderate number of common grammatical formants have been recovered.

Notable among the lexical items uncovered by Illič-Svityč, Dolgopolsky, and myself is a solid core of common pronominal stems. These pronominal stems have particular importance, since, as forcefully demonstrated by John C. Kerns (1985:9-50), pronouns, being among the most stable elements of a language, are a particularly strong indicator of genetic relationship.

The conclusion seems inescapable that the consistent, regular correspondences that can be shown to exist among the Nostratic descendant languages as well as the agreements in vocabulary and grammatical formants that have been uncovered to date cannot be explained as due to linguistic borrowing and can only be accounted for in terms of common origin, that is, genetic relationship. To assume any other possibility would be to stretch credibility beyond reasonable bounds. This does not mean that all problems have been solved. On the contrary, Nostratic studies are still in their infancy, and there remain many issues to be investigated and many details to be worked out, but the future looks extremely exciting and extremely promising.

The next question that needs to be answered is: Which language phyla have been shown with probability to belong to the Nostratic macrofamily, and what subgroupings can be established? As noted earlier, Illič-Svityč included Indo-European, Kartvelian, Afroasiatic, Uralic, Dravidian, and Altaic within the Nostratic macrofamily, and Dolgopolsky added Chukchi-Kamchatkan as well. Greenberg includes Indo-European, Uralic-Yukaghir, Altaic (Mongolian, Chuvash-Turkic, and Manchu-Tungus), Japanese-Korean (Korean, Ainu, and Japanese-Ryukyuan), and Chukchi-Eskimo (Gilyak, Chukchi-Kamchatkan, and Eskimo-Aleut) in his Eurasiatic macrofamily. He does not include Kartvelian, Afroasiatic, nor Elamo-Dravidian — not because he believes that they are unrelated, but because he believes that these three language phyla are more distantly related to Indo-European than are the others, which, along with Indo-European, form a natural taxonomic subgrouping. My own opinion is close to that of Greenberg, though I would exclude Japanese-Ryukyuan and Ainu. We may note here that Paul Benedict (1990) has recently presented a large body of evidence to support his view that Japanese-Ryukyuan is genetically related to Formosan and other Austronesian languages, which is not to deny that there are Altaic elements in Japanese. Indeed, Japanese appears to be a mixed language, containing both Austronesian and Altaic elements, with the Austronesian being the most ancient. Korean has, perhaps, the best chance of ultimately being an Altaic language, but much work still has to be done before this can be proved beyond a reasonable doubt. As I see the situation, Nostratic includes Indo-European, Kartvelian, Afroasiatic, Uralic-Yukaghir, Elamo-Dravidian, Altaic, Chukchi-Kamchatkan, Gilyak (also called Nivkh), Eskimo-Aleut, and possibly Sumerian. Afroasiatic stands apart from the rest as an extremely ancient, independent branch. Younger are Kartvelian and Elamo-Dravidian. Indo-European, Uralic-Yukaghir, Altaic, Gilyak, Chukchi-Kamchatkan, and Eskimo-Aleut appear to be more closely related as a group than any one of them is to Afroasiatic, Kartvelian, and Elamo-Dravidian. Finally, Sumerian, if it really does belong here, is a separate branch, probably closest to Elamo-Dravidian. To be sure, there remain numerous problems to be resolved here as well, such as, for example, whether or not Altaic is even a valid taxonomic entity, but, in due course, as more and more scholars turn their attention to these issues, solutions will begin to emerge.

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Ruhlen, Merritt. 1991. *A Guide to the World's Languages*. Volume 1: Classification. Stanford, CA: Stanford University Press. (This is a reprinting, except for the addition of a postscript on recent developments, of the original 1987 edition.)

The following review of *A Guide to the World's Languages* by Merritt Ruhlen appeared in *Diachronica* IV (1987), pp. 159-223; it is reproduced here with the permission of the publisher. Though no revisions have been made to the text of the original, it has been lightly edited to correct typographical errors and several minor inconsistencies.

TOWARD A DEFINITIVE CLASSIFICATION OF THE WORLD'S LANGUAGES

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What a densely packed, what a marvelously rich, what a stimulating, what a useful book Merritt Ruhlen has produced for the people of the world! There is so much information about Language and the languages of the world in it. Anyone who can read the common Roman letters can look up their own language and its kin and locate them on the great genetic bush of human language — without English. Historians of science should benefit even more. Scientists, other scholars, writers, even journalists, now have not only a definitive reference work on the classification of human languages but also one with state-of-the-art freshness and great clarity.

This is most definitely NOT a warehouseman's guide to world languages, pragmatic, limited, and easily defended. It does not mix typological, genetic, and geographical criteria like Meillet and Cohen's dreadful *Les langues du monde* (1924; 1952). It is thoroughly genetic in its approach, and it is bold, i.e., it seeks to carry phylogeny as far as it will go. Ruhlen has worked closely with Joseph H. Greenberg for years, and the book, as a product of cognition, shows evidence of that. I would suppose that Thomas Huxley comes most to mind when thinking of Ruhlen's role in this. All his own intelligence and effort has been channeled into the elaboration and defense of another man's theories, which he has incorporated totally in his own mind. Yet, Ruhlen's own evident intelligence and knowledge show through very clearly. And his own efforts have been great. For example, in working out the specific histories of various phyla and the details of their internal classifications and attendant controversies, he consulted 105 specialists in various linguistic groups or areas. The number actually was greater than that because some people did not respond to his inquiries.

The book has some of the attributes of a textbook. Those aspects which are localized in parts of Chapter 1 and Chapter 7 make a useful introduction to genetic classification, methodologies, naming taxa, and the origin and evolution of (human) language. Those useful and stimulating sections will be ignored here, although the book is to be recommended as a text or supplement for courses in Historical Linguistics.

The main focus of this review will be on the actual state of the art of genetic classification as seen from another person's perspective. So much do I agree in fact with Ruhlen's basic set of 17 major phyla plus a bunch of small ones or isolates that it will be useful to expand the discussion of some particular phyla to see what other significant viewpoints are around. Rarely do I disagree flatly with Ruhlen about major matters, but there are some differences of opinion. In those cases, it is often enlightening to see where varying opinions on sub-grouping or in extensions of phyla can take us. Of course, in the cases of 3 proposed phyla — Amerind, Austric, and Altaic — controversy is severe and ought to be discussed. It will also be of help to insert some of the points in controversy made at the recent (1987) NSF/LSA-sponsored workshop on historical methodology at Stanford. To the extent possible, I will appeal to common knowledge and keep literature citations to a minimum.

INDO-HITTITE: Some would object immediately that its proper name is INDO-EUROPEAN (IE) (German *Indogermanisch*) and that Hittite does not have a special status as a coordinate branch. But all the problems are in sub-grouping and reconstruction, not in accepting the validity of this phylum, which is the most solidly established in all the world. This is also the phylum whose study is widely regarded as the model for historical linguistics at large, especially the so-called "comparative method" and phonological reconstruction. (I say "so-called" because Indo-Europeanists often point out that there are many comparative methods in science generally and that one should specify which comparative method one is talking about in any given case.) But there are distinct limitations to the use of Indo-European as the model for our methods, and sometimes the advice of sages trained in that discipline is erroneous.

First, IE studies are fundamentally inward-looking; the system of IE languages is the universe of the inquiry, and said inquiry is dominated by centripetal forces. Thus it becomes difficult to think of larger entities to which IE may belong, and one finds it onerous to cope with problems involving masses of unclassified languages.

Second, IE studies is a den of antiquity, either through a preference for old written languages or reconstructed old languages like Proto-Germanic, Proto-Slavic, etc. Yet, just this virtue of abundant past records, which give IE studies so much of their strength, shows how special and partially irrelevant IE procedures are for phyla not so blessed. In its devotion to hoary written languages, IE resembles Semitic greatly. In that branch of scholarship, the inattention to Modern South Arabian languages, due to the belief that only their supposed ancestor was important, delayed access to valuable data which eventually forced revisions in Proto-Semitic.

Third, in their profound fixation on ten or so old written and fairly closely-related languages, IE students must get an extraordinarily distorted view of what language relationships are in a "normal" or model phylum. Yet, it is their phylum which is aberrant; most other phyla do not rest upon ten or so closely-related languages. They often have hundreds of languages and/or great differences among them and/or no written antiquities at all.

Fourth, IE sages give bad advice to their colleagues in other regions with poorly-established phyla because they have forgotten what their ancestors did when IE itself was being established. As Ruhlen argues persuasively, there are two distinct comparative methods (CM) of IE studies: CM-1 is what early scholars did to set up the whole IE system, and CM-2 is what current IE workers seem preoccupied with, namely, phonological reconstruction. It could indeed be argued that 20th-century Indo-Europeanists, were they to follow their own advice, would be too cautious to create IE itself de nouveau. They would first require the reconstruction of Proto-Celtic, Proto-Albanian, and other problematic groups and then "maybe in a hundred years when all sub-groups have been reconstructed" would they venture to propose Proto-IE. Of course, IE seems to me, as an Africanist, to be an obvious phylum, and I cannot believe that Indo-Europeanists would have been hobbled by their own irrelevant advice. Yet, this seems to be the kind of advice which the IE sages have been giving their Americanist colleagues for some time now, and it is undoubtedly related closely to the remarkable timidity on display in the New World.

In his African chapter, especially section 3.6 (pp. 120ff.), Ruhlen devotes much time to the criticisms of Greenberg's African classification. The heart of the critique was that "although he has discovered substantial numbers of apparent cognates for the groups he postulates, these sets of cognates do not exhibit REGULAR SOUND CORRESPONDENCES...that many linguists have come to regard as 'the only real proof of genetic relationship' (Welmers 1973:5)." That methodological viewpoint is attacked by Ruhlen and more recently by Greenberg in his *Language in the Americas* (1987). It is, in my opinion, the simple result of an over-emphasis on IE studies in the education of most historical linguists of the period since World War II. To a degree, it does not even accurately reflect the mature viewpoints of some IE sages but, rather, is a textbook phenomenon — something repeated often and automatically in the introductory textbooks. Most of them are also written by IE scholars. (We will come back to this methodological point later.)

URALIC-YUKAGHIR: The Uralic hypothesis is nearly as well established as the IE one and with no apparent disagreements about the major sub-classes of Samoyedic (North, South) and the rest (Ugric, Finnic). The final divisions of Finnic cannot be agreed upon, however. Ruhlen follows Austerlitz (1968) in dividing Finnic into (I) Permic, (II) Volgaic (Mari and Mordvin), and (III) North Finnic ([A] Saamic or Lappic, [B] Baltic Finnic). Ruhlen's four other authorities (Collinder 1956, Sauvageot and Menges 1973, Harms 1974, and Voegelin and Voegelin 1977) list from 2 to 5 primary sub-divisions of Finnic. Permic is always one of them. Volgaic is the most controversial, not being proposed 2 times out of 5. Another well-known opinion would be that of Raimo Anttila (1972:301), who entirely agrees with Austerlitz. Uralic was traditionally linked with Altaic in a Ural-Altaic phylum, but current linguistics largely refuses to accept that linkage. Uralic has also been tied to IE and/or Dravidian, but those theories have either been silenced or exist as part of the Nostratic or Boreal or Eurasiatic hypotheses. Although the contents of those larger efforts differ from each other, still Uralic is always in them along with IE and Altaic; so, to a remarkable extent, Uralic is at the core of those hypotheses. Thus, it is quite a surprise to me to find that Uralic has a distant relative which Collinder, Harms, Greenberg, and Ruhlen regard as very certain, namely, Yukaghir (plus Chuvantsy and Omok, both now extinct) in extreme northeastern Siberia. While this grouping of Uralic and Yukaghir cannot be said to have achieved general acceptance, it seems not to have aroused opposition either. Like the other so-called Paleo-Siberian languages, Yukaghir's classification simply is not a matter of interest to many linguists. However, the Uralic-Yukaghir hypothesis has been confirmed or independently discovered by Václav Blažek (personal communication, July 1987).

CAUCASIAN: Either under this name or perhaps the more familiar CAUCASIC, this phylum is a traditional one. Its primary sub-divisions of SOUTH CAUCASIC or KARTVELIAN and NORTH CAUCASIC are very firmly established things. Ruhlen accepts a further division of North Caucasic into Northwest and Northeast, following others but particularly Gamkrelidze and Gudava. Northeast has an additional division into Nax and Dagestan. But the real issue with Caucasian lies in the very phylum itself because it well illustrates Ruhlen's disapproval of "binaristic" approaches, although in the opposite direction from what he intended. With these heavily consonantal and pervasively glottalized languages locked up together in the Caucasus mountains and associated with similar cultures and physical appearances, it is natural for scholars to keep trying to relate them to each other genetically. (The situation is very analogous to that of Hadza and Sandawe in Tanzania.) As Ruhlen says, "Whether or not all Caucasian languages derive from a single source has never been resolved to the satisfaction of most linguists." Many linguists, especially "most Soviet linguists," believe the two constitute a phylum with "a common ancestor."

Others, including some Russian linguists, reject the Caucasian phylum. I would count myself too as one of the opponents because, while it appears that Kartvelian does ultimately relate to North Caucasic, it probably shares a more immediate ancestor with IE or Afroasiatic (AA) and Nostratic BEFORE it shares one with North Caucasic. It is also interesting that of the SIX versions of Nostratic reported by Ruhlen on page 259, including Greenberg's 1986 Eurasiatic, four of them include AA, and three of those also put Kartvelian alongside AA. None of the six EVER include North Caucasic, nor do the current Russian and Israeli revitalizations of Nostratic ever include North Caucasic along with Kartvelian. Sergei Starostin, on the other hand, supported by some, believes that North Caucasic in fact relates to Sino-Tibetan and possibly to Na-Dene before it relates to Kartvelian! So I feel supported in my rejection of the Caucasian phylum as the primary or "next higher" genetic grouping to which Kartvelian and North Caucasian each belongs.

Although I have not seen Gamkrelidze and Gudava's evidence for the Caucasian phylum, my own efforts to relate the North and South produced very few lexical resemblances. With their greater knowledge and access to data, they may have found more, of course. In addition, however, Bomhard has amassed a fair number of lexical links among Kartvelian, IE, AA, and others. Dolgopolsky has shown rather convincingly that Kartvelian pronouns clearly belong to the so-called "Mitian" (cf. French "moi" / "toi") or Nostratic group. So the issues seem clearly drawn: either (A) Kartvelian is a Mitian language but North Caucasic is something else (e.g., Sino-Caucasic), or (B) Kartvelian and North Caucasic are both Caucasian languages which show incidental or irrelevant resemblances to outside languages, possibly just due to borrowing from powerful IE neighbors like Persian, Armenian, etc., or (C) Kartvelian, as a member of the Mitian larger phylum, shows genetic connections with North Caucasic as a member of the Sino-Caucasic larger phylum BECAUSE Mitian and Sino-Caucasic are themselves genetically related in an even higher level super-phylum. If such an entity can exist by hypothesis, then it ought to be called "Eurasian" because very little of the great Eurasian land mass would not be associated with it. Then, the best existing evidence for "Eurasian" would have to be Gamkrelidze's evidence for CAUCASIAN.

A somewhat more inclusive super-phylum than my hypothetical "Eurasian" was postulated twenty years ago by Morris Swadesh as VASCO-DENE (Spanish version) or BASQUE-DENEAN. Except for having Basque but lacking IE, it is just about the same as Mitian plus Sino-Caucasic-Dene. It has received no support among linguists, at least that I know of or that Ruhlen mentions, but it may possibly have stimulated some of the recent Russian work. We will return to Vasco-Dene later.

AFROASIATIC: A more apt short form used by Diakonoff and his associates is AFRASIAN, which I would recommend to everyone. Hereinafter, I will call it AA. In Europe, it is frequently called HAMITO-SEMITIC or SEMITO-HAMITIC. Old and solidly grounded in Semitic and Ancient Egyptian, AA continues to grow in its southern branches, and its overall dimensions now far exceed the original Sem of Arabia and Ham of Egypt. As Paul Newman has argued recently (at Stanford), the phylum's biggest problem is everyone's preoccupation with Semitic and the bias inherent in the belief in the antique, hence primeval, quality of Semitic morphology, especially the tri-consonantal verb roots and conjugational affixes. AA now has six families or sub-phyla, which are usually viewed in the IE manner, as equal in status officially but with possible reduction to fewer major branchings. The official roster of families nowadays contains Semitic, Egyptian, Berber, Cushitic, Chadic, and Omotic. The last results from a splitting in twain of traditional Cushitic (e.g., the Moreno classification of 1940). Chadic is nearly 40 years old, in the usage of American Africanists, but older still for German scholars. It was glued onto AA in Greenberg's extremely influential African classifications of 1948-53 and 1963. But many Semiticists have never accepted Chadic as part of AA, a stance which tends to amaze students of other AA sub-phyla and of African languages in general.

Although counting the numbers of languages in any particular group is always chancy because of the problem of dialects, in cases where I feel more sure about the basic facts, I will indicate differences with Ruhlen. Still trying to reckon that a cluster of closely related dialects should be counted as one language, as he does, I count about 48 more AA languages than he does. He actually has counted one, Birale, whose membership in AA is not certain because the field data are so poor and confusing. Otherwise, within Omotic, Cushitic, and Semitic, I count 18 more than he does, and, in Chadic, my sources tell me "at least 150" not 123 only. In the case of the Ometo group of Omotic, Wallamo or Wallaita, and the dialects close to it exceed 40 in number, but all are very close. Nevertheless, one of them, Dorze, ought to be called a separate language. My general point is that the counting will always be arbitrary but that the numbers counted in any African phylum will probably be too low because new varieties are continuously being found. Ex Africa semper novo!

Ruhlen has a useful listing of the recent proposals for modifying the basic six family structure. His proposers include Greenberg (1981), Ehret (1979), Bender (1981), Hetzron (1982), Newman (1980), Fleming (1981), and Voegelin and Voegelin (1977). Newman believed that Omotic was too different to be included in AA, but he no longer believes that. Three proposers think Omotic is coordinate to all the rest. Taking the essence of Newman's belief, the number really is four. Also, four of them (but a different foursome) agree that Beja or North Cushitic is either coordinate to the rest of Cushitic or a separate stock within AA. Two of them propose that Berber has a special relationship to Chadic, while two believe that it is Semitic which is close to Berber. So, perhaps Berber is the most uncertain branch at the moment. Or, as Ruhlen says: "It is apparent that there is as yet little consensus on the internal relationship of Afro-Asiatic." That statement applies, of course, to the attempts at higher lever branchings, not to the basic six sub-phyla. There have also been alterations in the internal states of all the sub-phyla except Egyptian. Hetzron on Semitic, Newman and Jungraithmayr on Chadic, Militarëv on Berber, Bender and Fleming on Omotic, and almost everyone on Cushitic represent improvements on older sub-groupings. Indeed, the appearance of uncertainty on AA sub-grouping is a sign of great intensification of effort within the phylum rather than chaos. A lot of work on AA is being done in Europe and Russia, including most of the reconstructing outside of Chadic. Ruhlen has committed sins of omission with respect to that work, although not mortal ones, but the omission of Jungraithmayr on Chadic is unfortunate. He, like several other German Chadicists who think about higher-level branchings, believes that Chadic is closer to Berber.

While some Africanists in good standing (e.g., Hodge and Dolgopolsky) believe that AA has external relations with other phyla, especially IE and Kartvelian, still, the great mass of their colleagues have not been interested in such things. Few, if any, believe anymore in the old Hamitic theories of relations between AA and Nilo-Saharan or Niger-Congo or Khoisan, nor are the special partly Hamitic entities like Fulani, Nubian, Masai, or Hottentot given much credence. A few want to excise the Saharan branch of Nilo-Saharan and attach it to AA. Above all, the great increase in both scholars and field data in AA has resulted in a widespread urge to put our house in order — first. External relations come second. Part of the reason for that lies also in the realization that AA is a big phylum with a great deal of internal diversity, exemplified by Chadic, Cushitic, and Omotic, rather than a tidy arrangement of eternal and unchanging entities like Akkadian and Egyptian. It dawns on us too that the time depth must be quite great within the phylum and that the achievement of a true Proto-AA will be difficult because it will probably not be the same as Proto-Semitic.

NIGER-KORDOFANIAN or **N-K:** Also widely known by its earlier name of NIGER-CONGO (N-C), so called before Greenberg added Kordofanian to N-C. The name chosen by Greenberg, CONGO-KORDOFANIAN, has not been adopted as much, nor has it become popular, and it should simply be discarded. N-C has also been called SUDANIC, WEST SUDANIC, and NIGRITIC. Just as Khoisan and Eskimo have been associated with a physically distinct set of populations, so too has N-C. In its case, the association with the archetype of African Negro led to taxonomic distortions in West Africa. Those varieties of West Atlantic spoken by Fulani (Peul, Fula) people were mistaken for AA varieties, while most of the Chadic languages were resisted as AA varieties for the same reason — the bodies did not fit the archetypes, so the languages could not be classified genetically! There IS, of course, some correlation in the world between physical types — either phenotypic or genotypic — and genetic linguistic stocks, but it is so far from being 100% (1.00) that in any given relationship, it has to be discovered, not assumed. Yet there remains among European linguists an unrepentant and stubborn racism that insists that one knows the most important thing about a people when one knows their physical type.

N-C is a huge affair, and N-K is even larger. The 1,064 N-K languages that Ruhlen counts are second only to the 1,175 Austric in number. As I argue below, Austric ought to be listed as a super-phylum. It was characteristic of Greenberg's final African scheme that it reached for the maximum in phyletic linkages, so that N-K probably ought to be seen as a super-phylum too. However, the other aspect of this scheme was that sub-grouping was a more pragmatic matter and that final judgments were to come later. In N-K, the labors of many scholars have produced sub-grouping that stresses things not seen in the original. For example, West Atlantic and Mande (Mende) are now formally classed as more separate or distinctive than the rest, much the same as Kordofanian itself. Where Greenberg has six branches, to wit, West Atlantic, Mande, Gur or Voltaic, Kwa, Benue-Congo, and Adamawa-Eastern (Ubangian) in N-C, to which Kordofanian was attached as a coordinated sub-phylum, the present scheme has three primary sub-phyla. Following Bennett and Sterk (1977), which has been the most influential sub-grouping, Ruhlen now proposes this scheme for N-K: (I) Kordofanian, (II) Mande, and (III) Niger-Congo. Group III, in turn, is divided into: (A) West Atlantic and (B) Central Niger-Congo, which contains all the rest. Central Niger-Congo, in turn, divides into (1) North

(Kru, Gur, Adamawa-Ubangian), and (2) South (Western, Ijo, Eastern). The last, "Eastern," contains 9 sub-groups (Central Niger, Yoruboid, Edo, Lower Niger, Jukunoid, Delta-Cross, Efikoid, Eastern Cross, and Benue-Zambesi). The last, "Benue-Zambesi," has two primary branches: (I) Cara and (II) Nyima. Nyima divides into (A) Plateau and (B) Wel. That last divides, in turn, into (1) Bendi-Bokyi and (2) Bantoid. Bantoid itself divides into (a) Non-Bantu and (b) Broad Bantu. Most of what used to be called "semi-Bantu" now is called Bane, a half of Broad Bantu. The other half is called Narrow Bantu, half of which is Northwest Bantu, mostly meaning the northwestern Congo and Central Bantu, which has 249 languages or most of those known to the outside world as BANTU. They pretty much cover the southern 40% of Africa, and their speakers constitute a substantial part of the physical and cultural diversity found among N-K speakers. Yet Bantu is a mere twig on the N-K bush. It is extraordinarily analogous to Polynesian vis-à-vis Austronesian.

N-K as a whole also resembles Austronesian as a whole in not being especially controversial. What there is of that tends to involve the major sub-phyla like Kordofanian, Mande, and West Atlantic. It is usually possible to get a vigorous discussion going about the relationships found within Bantu or between Bantu and its more proximate relatives. The sheer size of the N-C part tends to inhibit over-confidence in sub-grouping. In Bantu, the large expanse of closely-related languages and dialects, which seem to ooze into each other in all directions in an infinitely clinal manner, virtually guarantees that anybody's sub-grouping will be wrong, especially if s/he uses a Stammbaum model. Ruhlen quotes a facetious observation that Bantu is 500 dialects of a single language; there is much to that.

There have been a few attempts to connect N-K to N-S (e.g., Bender, Gregersen, Homburger). While it is not fair to say that those attempts are wrong, they are not accepted generally among Africanists; nor do they seem to have a handful of vigorous supporters. Rather, it is the case that most workers in both phyla have not yet confronted these hypotheses because they literally have not heard about them or have heard them dismissed in conversation as speculative. My own opinion is that the conjoining of any two of the African phyla would be a major step above the level of a super-phylum. To link N-K and N-S would be something more venturesome than Amerind or Nostratic; if it involved Khoisan or AA, something even bolder.

NILO-SAHARAN or N-S: It too was once called Sudanic and East Sudanic and so forth. It has also been called "Greenberg's waste basket," hence a collection of hard-to-classify languages and a very unreliable entity as a phylum. Vis-à-vis AA or N-K, N-S is widely viewed as the most shaky of the three, but it no longer gets the kind of stubborn opposition that Khoisan receives in South Africa and Britain. When Greenberg finished his first classificatory sweep of Africa, he ended up with fourteen phyla. Of those, one was AA. One was N-C, which then had Kordofanian joined to it. The fourth was Khoisan. All the rest, or 10 phyla of the first classification, were put together as Nilo-Saharan. It represents far far less consensus, far less agreement on sub-grouping, and very little progress on reconstruction. Yet, it has held together for the past 24 years because its critics, principally British Africanist linguists, have been honest and unconvincing. In their honesty, they have produced more and more pieces of evidence which link various of the old ten phyla together.

Ruhlen follows Greenberg's views as modified several times recently by Bender. The present scheme has nine sub-phyla: Songhai, Saharan, Maban, Fur, East Sudanic, Central Sudanic, Berta, Kunama, and Komuz (= Koman plus Gumuz). The old Chari-Nile node, which embraced East Sudanic, Central Sudanic, Berta, and Kunama, was abandoned in the face of numerous criticisms. Ruhlen quotes Bender as recently proposing a simpler scheme of six sub-phyla, namely, Songhai, Saharan, Maban-Fur-East Sudanic-Central Sudanic, Kunama-Berta, and Komuz. I do not know if others have accepted Bender's scheme, there being so few people who work on the "big picture" in N-S. Christopher Ehret, who has been doing such work, will probably not agree. It is widely regarded that Songhai is the hardest group to keep in the phylum because it is so remote and because several scholars (e.g., Mukarovsky, Creissels) see Songhai as related to Mande, a member of N-C otherwise. Before his unfortunate and relatively recent demise, Karel Petráček was trying to excise Saharan from N-S, while Thilo Schadeberg was ADDING some Kordofanian languages to N-S. These proposals, it must be said, have not gained adherents, despite the fact that they are known among Africanists and the proposers respected. However, I at least believe that Schadeberg is correct to remove the Kadugli group of Kordofanian from N-K to put it in N-S. It is only the second case I know of where parts of one of Greenberg's phylum have been moved to another, the other being in Southeast Asia, where Greenberg himself moved Miao-Yao (see below). Generally, Africanists grumble about a detail here or there but remain satisfied with Greenberg's classification in its gross outlines — four African phyla and most of the internal classification.

The numbers of languages for N-S are too high in spots and too low in others. Ruhlen's East Sudanic sub-phylum in its Eastern branch and Surma sub-branch includes a language — SHABO — which I do not believe is even N-S, much less fairly close to Majang. His opinion was obtained from Bender, who, in this case, seems to be mistaken; if it is so difficult just to show that Shabo should be included in N-S, then how can it be in the same sub-branch with neighboring Majang or any other language? If Shabo is N-S, then it is a major branch with a status like that of Furian. In the famous Nilotic branch of East Sudanic, there are too few in the Bari and Lotuxo groups, too many in the Teso-Turkana and Kalenjin, except for Okiek (or Dorobo) and Datooga, where there are too few. And so it goes. As Ruhlen says, everything is more difficult in N-S, which has always been a "literature-poor" phylum. An important part of the African literature nowadays is produced in Germany, and Ruhlen has surveyed that too. Just in the case of Nilotic, his network of sources missed the contributions of Franz Rottland, Rainer Vossen, and their colleagues.

Were N-S to be located in the Americas, it would be regarded as a congeries of 10 or 20 phyla which might someday be related to each other, but only after each phylum had been properly reconstructed and all the borrowings and areal influences filtered out. Were it found in Eurasia, it would be an exciting and venturesome entity like Nostratic. Because it is found in Africa, where Greenberg's boldness has been domesticated, hence accepted, N-S does not seem extraordinary. Yet, it has the general attributes of a super-phylum in its deep diversity among sub-phyla and the common feeling that the whole enterprise is a bit shaky. For outside comparativists who seek to determine if N-S is related to other phyla, the great danger is the strong separateness of the individual families. One must be careful to distinguish between an item which links Songhai, for example, to an outside group from an item which links Songhai and Komuz to an outside group. The two items have radically different import.

KHOISAN: Also called CLICK LANGUAGES and more loosely the BUSHMEN languages. It has never been properly named to everyone's satisfaction. Ruhlen uses Khoisan, and correctly so, because that is the name which has slowly asserted itself among Africanists over a period of years, aided no doubt by the continuous ethnographic references to the Khoi and San peoples. Khoi is the Nama (Hottentot) name for "person," while San is the Nama word for Bushmen. One might propose Zhu, since some of the San languages have that for "person." Or Khoi-Zhu for the phylum. Despite the reverence with which some ethnographic types use San, as a new word to replace the derogatory old word Bushman, San has distorting effects which are serious. One is that all the non-Khoi are in one group and the Khoi in another. That is true CULTURALLY only in the sense that the Khoi are cattle people and the San are hunters. Linguistically, the hunters are found in all the sub-divisions, while Khoi shares one division with some of them. There is no true linguistic moiety of Khoi, or San.

Ruhlen's discussion of this phylum is one of the most important in his book, and it led him to important methodological questions. The African section of the book is where he chooses to present the many criticisms of Greenberg's methods and classifications. It is where he mounts his general defense, particularly on (1) the felt need to have reconstructions and (2) binarism or binaristic approaches. We will return to those points at the end.

Khoisan is the one African phylum where strong and continuing opposition exists. Let us look at what Greenberg proposed and then see what his opponents still reject. Ruhlen has found an 1847 source, John Appleyard, who proposed the basic unity of Bushman and Hottentot. That was reinforced and expanded by the several Bleeks, beginning in 1858. But the present phylum was proposed "in the early 1920s" by Albert Drexel, who added the two Tanzanian languages, Hadza and Sandawe, to the South African Khoisan. For nearly a century, the Khoi and San were separated by many scholars, the most noteworthy being Lepsius and Meinhof, for reasons of typology, Khoi having grammatical gender and San lacking it. For that vitally important attribute, the Hottentots (Khoi) also got themselves included in the select circles of the Hamitic cattle people of Africa, along with Fulani, Nubian, Nandi, Masai, and most of AA. Hamitics and typology were jointly arrested by Greenberg's frontal assault from 1948 to 1963, so that Khoisan is actually a very young phylum, having attained its modern shape and being an object of international discussion only in the past 30 years.

Khoisan has three sub-phyla: South African Khoisan (SAK), the Sandawe language of Tanzania, and the Hadza language of Tanzania, the last two almost contiguous. SAK, in turn, has three branches: Northern, Central, and Southern. Khoi belongs to Central. Opinion on Khoisan seems sharply divided by national styles in linguistics. Most Americans following Greenberg and Germans following Oswin Köhler accept Khoisan in the above form. Most British and South African linguists are skeptical, chary, or looking for more data. Key influences on both have been the writings and opinions of E. O. J. Westphal (above all) and Archie Tucker, mitigated or corroded by German and American influences. Westphal neither accepts Sandawe and Hadza in the same group, nor does he accept SAK as a valid entity. Close reading of his opinions, however, suggests a tacit acceptance of Sandawe and Khoi as related. Much key data on Hadza and Sandawe remain unpublished. There has been considerable field research on SAK by South Africans, some of it still unpublished, but the most astounding data on a SAK language has become widely known through its publication in*Language*. There, the primary linguist, Tony Traill, in collaboration with the well-known phonetician, Peter Ladefoged, displayed a language with over 100 consonant phonemes, plus tones and several vowel series, thus setting a world's record for phonological complexity and causing Kabardian to seem ordinary!

My own research on Khoisan (cf. Fleming 1987) was motivated by the scarcity of good Hadza cognations with Sandawe. Greenberg had shown that SAK was related to Hadza and to Sandawe, but he was unable to produce more than 11 Hadza-Sandawe etymologies. Undertaking to check that relationship and the biological data involving all so-called "Bushmen" people, I increased the cognation count threefold, found why this particular binary comparison was so hard, and discovered that the biological data did not seem to support the concept of Khoisan, if it included Tanzania. The last was a surprise, given the manifest phenotypic resemblances among Hadza, Sandawe, and the San. It is likely that binarism in this case would have produced even more cognations had the databases been larger. This leads to the conclusion that single languages which are also clearly very remote from their kin certainly require a large dictionary to maximize their chances of being related. Words which are lost, in the ordinary sense of having wandered semantically from their original meanings, can be retrieved in a larger lexicon. One example would be the case of She, an Omotic language (of the Gimojan branch), which had ordinary words for "tooth" which were not connected to other AA forms. But in the specialized vocabulary for body part terms, we find /san/ for "canine tooth," which is cognate with Semitic, Chadic, and Berber "tooth" as /sinn/, /san/, and the like.

ALTAIC: This possible phylum might be described as the Belfast of genetic linguistics because nearly every bit of it is bitterly contested ground. Altaic as a genetic hypothesis has had the same fate that the large Amerind hypotheses have had — it has RECEDED under intense criticism from splitters, while it has EXPANDED in new directions through the efforts of lumpers. Some people are sure that Altaic, now a larger entity than the traditional Turkic-Mongolic-Tungusic, is a part of, nay a core element of, Nostratic or Eurasiatic. Yet others seem bent on reducing even the traditional concept to rubble, with little clumps of Turkic languages here, Mongolian languages there, and Tungusic over in a different pile. Ruhlen's discussion of Altaic is very valuable in its own right as a short description of the development of the present kaleidoscope of views. It appears that a stringent critique of received theories has been fruitful in Altaic studies because some earlier typological excesses (e.g., those of Max Müller) have been swept away. And if the older versions of Altaic were oriented towards the west, towards the Turkic languages and whatever was related to them, then a splitters' residue has now appeared in the east, oriented towards Japanese and Korean and whatever is related to them. In effect, Japanese, Korean, and Tungusic have become the nucleus of a renascent Altaic, while Turkic is now the isolate looking for kin, and Mongolic is the focus of dispute. These last two observations are based on my notes, taken at the Altaicists' summary of their discussions at Stanford University on 1 August 1987. However, it is clear that a slanted view was, perforce, presented because the panel consisted only of splitters.

A vital part of the splitters' argument was played by the conclusion that massive borrowing by Mongolian from Old Turkic had created the false impression that Mongolian was related to Turkic. Similarly, it was contended, those same borrowings in evolved (altered) forms were passed on from Mongolian to Tungusic, creating once more the false impression that Tungusic was also related to Mongolian and Turkic. Exuberant cultural and social growth among the Mongolian peoples also affected both the Turkic and Tungusic peoples, resulting in more loan words, reinforcing the false impression of genetic relationship. It was a very forceful argument!

In concluding his Altaic chapter, Ruhlen adopted the essential classification of Street (1962) and Patrie (1982), which has the following membership and sub-divisions: (I) Altaic Proper: Turkic, Mongolian, and Tungusic; (II) Korean-Japanese: Korean, Japanese-Ryukyuan, and Ainu. What is surprising to me is the almost casual inclusion of Ainu, not only as related to Altaic but also as relatively close to Japanese and Korean! However, Ruhlen follows Patrie, who "has adduced considerable evidence linking Ainu with the rest of Altaic, including both Japanese and Korean." This classification is similar to those of other authorities on Altaic, except that Nicholas Poppe (for example) links Mongolian and Tungusic more closely and excludes Japanese, while Roy Andrew Miller puts Japanese and Korean in the same branch with Tungusic, separates Mongolian from them in that same branch (Eastern), but makes Turkic a distinct coordinate to the rest as Western. Neither Poppe nor Miller include Ainu within Altaic. Moreover, the Altaicists' summary at Stanford was explicit in rejecting a place in Altaic for Ainu. Unfortunately, I have not seen Patrie's arguments for an Altaic Ainu, but from some limited inquiry I made into the subject years ago, I remain skeptical that Ainu belongs in the same branch of anything with Japanese and Korean. Greenberg in his Eurasiatic 1986 splits Korean-Japanese from Altaic, making each a primary branch of the whole super-phylum, the other branches or sub-phyla of Eurasiatic being Indo-Hittite, Uralic-Yukaghir, and Chukchi-Eskimo. But he too puts Ainu in with Japanese and Korean.

There are other opinions on the subject both of Altaic and of Ainu. Traditional Altaic plus Altaic as a part of Nostratic are strongly supported by many Russian linguists. Some Finnish linguists are, however, strongly opposed to both. American physical anthropology has a firmly established tradition of treating the Ainu as a special problem because they do not appear to be very much like Japanese and Koreans. Recent dental studies (Turner 1986) separate the Ainu from Japanese quite smartly, connecting the Japanese with Southeast Asian populations via the Ryukyus, while equally recent and authoritative serogenetic studies (Masumoto 1984) (particularly Gammaglobulin) lace the Japanese and Ainu firmly in the northern "Mongoloid" group alongside the Mongols, Tibetans, Eskimos, and Amerinds, including the north Chinese. (In Gammaglobulin, China is very unusual for a supposedly homogeneous population. North China belongs near the Mongol-Tibetan-Eskimo group, South China is part of the Southeast Asia plus Indonesia group, and the rest of China is clinal between these two points.) So it appears that extreme eastern Asia will have enough controversy to satisfy all of us for some time to come! Of course, it must be reiterated that biological affinities prove NOTHING about genetic linguistic affinities but are valuable heuristically.

So far as I can tell from Ruhlen's history of Altaic, none of the modern workers in Altaic include GILYAK within Altaic's range. Greenberg includes it in his fifth or Chukchi-Eskimo sub-phylum of Eurasiatic as an independent sub-branch, alongside the other two sub-branches, Eskimo-Aleut and Chukchi-Kamchatkan. My own impression from reading Karl Bouda (1960) is that Gilyak is distantly related to Ainu. (More on Gilyak below.)

CHUKCHI-KAMCHATKAN: This is the fifth large linguistic grouping, either a phylum in itself or an "isolate" (small phylum) or a major part of a phylum (sub-phylum) to be located in the most improbable location for old human habitation — the frozen expanses of Siberia and Arctic Europe. The isolates (Ket-Kot and Gilyak) and the phylum (Chukchi-Kamchatkan) and sub-phylum (Yukaghir) are usually found listed in encyclopedias as "Paleo-Siberian", a grouping whose genetic validity is always denied and whose geographical convenience is always asserted. Most of Uralic could logically be included if it is the frozen northlands which are the heart of Paleo-Siberian. Indeed, Eskimo-Aleut of Arctic North America could be included by extension. A not inconsiderable part of traditional Altaic (most of Tungusic plus some Turkic languages like Yakuts) shares the same domain. Thus, it is not surprising on geographical and cultural grounds that all of these parts of Paleo-Siberian — except Ket-Kot — should be related to each other and to Uralic and to Altaic in a super-phylum called Nostratic or Eurasiatic. Those versions of Nostratic which exclude Kartvelian and AA are strongly focused on the Arctic and Sub-Arctic lands of Eurasia. The implications for the earlier origins of IE itself become very interesting.

Ruhlen divides Chukchi-Kamchatkan into "...two basic, and deep divisions." — Southern or Kamchadal and Northern or the rest ([A] Chukchi and [B] Koryak: Kerek, Koryak, Alyutor). Under the rubric LUORAWETLAN, which is still preferred by Russian and some European linguists, this phylum has been known since 1775 and included its present membership by 1798! Bogoras' famous study*Chukchee*, which is the first comparative study of Luorawetlan, was done in 1922. There seems to have been no serious dispute about the membership or the relationships among the five languages. The idea that they are also related to Eskimo-Aleut seems natural to anthropologists because of the close physical and cultural similarities between the two groups, including the physical presence of Eskimos in extreme eastern Siberia on the western shores of the Bering Straits right next to the Chukchi.

DRAVIDIAN or **ELAMO-DRAVIDIAN:** The first of these two entities has been one of the most stable phyla in the history of linguistics — one of the verities, so to speak. Dravidian has had a sub-grouping which was essentially correct for a century. In it, Brahui of western Pakistan is either a coordinate branch or recognized as the most divergent, while the main mass of Dravidian languages in India constitute a second branch. Ruhlen has adopted the McAlpin classification of 1981 for purposes of sub-grouping. It differs only somewhat from that of Andronov (1978) and earlier classifiers, mostly with respect to Telugu and the Gondi-Kui group. In Ruhlen's scheme, Dravidian consists of (A) Northwest: Brahui and (B) Dravidian Proper: (1) Northeast: Kurux-Malto, (2) Central: Kolami-Parji and Telugu-Kui, (3) South: Tulu and Tamil-Kannada. Some scholars (e.g., Zvelebil 1970, following Bloch 1946) join Kurux and Malto to Brahui in a larger northern branch, over against a central and a southern branch.

For a number of reasons, it is possible to infer an old Dravidian language or branch, spoken in the 2nd millennium BC in most of the Indus River valley and that of the western Ganges. Initially, Sanskrit was located in western and northern India-Pakistan, where it was replacing the local languages in the 2nd millennium BC. Secondly, Sanskrit showed the effects of intense contact with some Dravidian language, as do most of its daughters, not only in vocabulary but also in phonology. Third, the basic coordinate branches of Dravidian, to wit, Brahui and Dravidian Proper, embrace the Indus River valley between them, thus making it more likely than not that the language(s) of the Indus Valley Civilization was/were Dravidian. Most of the Punjab and the lowland Ganges can also be included in the same statement. It would, of course, not be surprising to Indologists to hear such hypotheses because they are fairly traditional views of the prehistory of greater India.

Fourth, nevertheless, the archeological roots of the Indus Valley Civilization are seen nowadays as lying in cities of Baluchistan, Afghanistan, and ultimately southwestern Persia and Iraq, rather than being the complete mystery they were previously. (See particularly the work of Lamberg-Karlovsky.) Thus, the brilliant Harappan Civilization is ultimately an offshoot of Mesopotamia. If diffusion, rather than local invention, was to be the explanation for the Harappan cities, then the west was always the most likely source of it all. Archeological connections to Mesopotamia are not, in themselves, good linguistic evidence for a Dravidian language. Obviously, something is missing. That crucial evidence seems to be provided by three more things. Fifth, two groups of scholars, the one Finnish and the other Russian, earlier announced that the Indus Valley script had been deciphered, using the assumption that it was based on a Dravidian language. Since this exciting discovery seems not to have been pursued, or perhaps it was actually quietly abandoned by its proponents, it is a weak part of this argument. No one would disagree, I think, with Zvelebil's (1970:195-96) conclusion, after reviewing the arguments of the two teams, that "A proof that the readings and translations of the Harappa inscriptions as Dravidian are correct can be offered only if (a) either a bilingual inscription will confirm the validity of a 'Dravidian hypothesis' or (b) if, in the absence of a bilingual, a much greater amount of material would be read, translated and interpreted, and such large amount of data will form a meaningful and consistent corpus of texts." It really comes down to one problem — reading and translating the corpora — and that problem frustrates us all in the case of Meroitic and Easter Island, as well as the Indus Valley.

Sixth, however, is Lamberg-Karlovsky's finding that the archeological cities linking Susa to Mohenjo Daro were specifically Elamite in writing and presumably in speech. And, seventh, McAlpin "rediscovered and elaborated" the hypothesis of the 1850's that Elamite was related to Dravidian distantly. Russian scholars have also stated their belief in this Elamo-Dravidian in recent years. Thus, with Elamite linked genetically to Dravidian, and the Elamite cities linked to their cousins in the east, it becomes possible to see Dravidian as part of a larger entity focused as much on greater Iran as on greater India.

My own inquiries into this topic fifteen years ago caused me to believe that Elamite was related to Dravidian and to Sumerian. However, at that time, it was evident that good data on Elamite were not easy to obtain (a problem of references and library sources more than anything else) and that much of later Elamite was positively awash in Persian. Hence, my acceptance of Elamo-Dravidian is not based solidly on good textual data from Old Elamite. My impression that Elamite as related to Sumerian was much stronger than the feeling for Elamo-Dravidian. There seem to be few scholars who agree with me on this, however, but Zvelebil mentions R. S. Vaiyanatha Ayyar (1929), H. S. David (1954), and A. Sathasivam (1965); the latter claims to have "501 cognate sets drawn from some or all of the nineteen Dravidian languages and from Sumerian, the twentieth member of the Dravidian family proposed here." (Quoted in Zvelebil 1970:21-22, fn. 32.)

Dolgopolsky (1986) linked Elamite and Dravidian and included them in his Nostratic, which is basically Mitian plus AA. Bomhard does just about the same. Dolgopolsky does not include Sumerian, however, but Bomhard cautiously does. Three of the versions of Nostratic mentioned in Ruhlen do not include Dravidian in that super-phylum or Elamite or Sumerian either for that matter. However, Illič-Svityč, Menges, and Birnbaum DO include Dravidian, but in the "Eastern" branch alongside Altaic and Uralic. Reports that "Dravidian has been related to Uralic" are part of the stuff one hears repeatedly at conferences. However, recently Stephen Tyler, a well-known Indologist and cognitive anthropologist, proposed de nouveau that Dravidian was related to Uralic!

Finally, despite this host of inclusions in Nostratic, a serious southern alternative for Dravidian has been proposed. Not only does Greenberg not include Dravidian in his Nostratic (Eurasiatic) but also he has been saying informally that Nilo-Saharan shares more than 60 cognates with Dravidian. Since this observation is not yet published, it is not clear whether Nilo-Saharan is truly NEXT of kin or whether it is related to Dravidian as part of a larger entity. Since AA and Kartvelian were not present in Ruhlen's version of Eurasiatic because Greenberg had not yet included them, they may be co-members of said larger entity along with Dravidian and Nilo-Saharan. One needs to see evidence produced, of course. Otherwise, from what I have seen of the data from the respective phyla, it would not occur to me to propose any relationship between Dravidian and Nilo-Saharan.

SINO-TIBETAN: Of all the old established phyla, this one has the greatest uncertainty about its sub-groups. It also has held the record for controversial inclusions and exclusions, that is, until the recent civil war over Altaic began. Ruhlen's review of the history of this variable concept is superb, and the reader is urged to peruse it directly. In brief, the entire phylum began in the 19th century, thoroughly entangled with many of the groups which now make up Austric (see below). As the other elements fell off one by one, Tai and its kin (Daic or Thai-Kadai) plus the small Miao-Yao group remained embedded, more often than not in the Chinese (Sinitic) part of the family. It is probably the case that many linguists are still being taught that Tai (Thai) and Miao-Yao belong in Sino-Tibetan. But, in the 1940's, Paul Benedict began the challenge which has resulted in the present predominant view that Daic is an independent phylum or it relates to some of the members of Austric and that Miao-Yao is the same. The current Russian views in this respect are virtually identical to the American, except that the Austric hypothesis seems to have more adherents in Russia than in the U.S.A.

Assuming that the contemporary views are more accurate than their predecessors, there are some interesting things to learn about Sino-Tibetan. Why has there been so much confusion? One factor seems to have been the predilection towards typology in the 19th century. Languages with tones and short words but little inflectional morphology, spoken by physically similar people who lived in or near China, seemed to be akin to each other. The assumption is not unreasonable on the face of it, and to a great extent just that set of typological assumptions WORKED in West Africa. Unfortunately, it did not work in Southeast Asia, anymore than it worked in central and eastern Africa. That is what one would expect of a genetic strategy not based on genetic criteria. A second factor is areal linguistics. As Gerard Diffloth has observed, the Austroasiatic languages in India are very different from those near China in phonology and morphology. Sino-Tibetan, Daic, and Miao-Yao languages have been influencing each other profoundly, undoubtedly for millennia. The influences extend to the lexicon too. The "facts" that Archaic Chinese borrowed very heavily from Old Daic and that later Daic languages in turn borrowed heavily from Tibeto-Burman as well as Chinese contributed to an unusually deep and pervasive pattern of lexical similarities between Sino-Tibetan and Daic languages. Finally, the overwhelming linguistic power of Chinese civilization and the prestige of its culture and the great numbers of its people have made everyone from Japan to Burma to Xinjiang (Sinkiang) at least partly Chinese!

There is also the possibility that the current views are mistaken, that when one has allowed for all the borrowings and influence, Daic and Miao-Yao are nevertheless still related to Sino-Tibetan. Ruhlen quotes the view of a Thai scholar, Prapin Manomaivibool, to that effect — for Daic. For Miao-Yao, we have the strange case of Greenberg's changing views: in 1953, while supporting Benedict's general theses, he thought that Miao-Yao was, despite the Sinitic borrowings, really related to Chinese; later he changed his mind, telling a few colleagues that Miao-Yao was his greatest mistake and that Benedict (and the Russians) were right; but most recently, he has re-re-examined the question and has expressed thoughts that Miao-Yao might after all be related to Sino-Tibetan. Since such indecision is extraordinarily unlike Greenberg, it would seem that Miao-Yao is a tough nut to crack! Even Benedict has had his troubles with Miao-Yao. With respect to the lower numbers "3" and "4", he first derived them from Tibeto-Burman or Chinese as loan words (1975:83-84) and later saw them as Austro-Thai native cognates (1975:211-17).

Even when Sino-Tibetan is reduced to its "true" components, those languages grouped around the three foci of Chinese, Tibetan, and Burmese, a large uncertainty about sub-grouping still exists. The problems are the status of the Karen-type languages and how many groups to propose for the combined Tibetan and Burmese groups, or Tibeto-Burman. Some of the Himalayan varieties are problematic also because they are poorly known. At least one of them, Kusunda, does not appear even to be Sino-Tibetan but rather an isolated language. The divisions of the phylum produced by Ruhlen, drawing upon Benedict, Shafer, and some recent work, is probably as good as anything we have or can anticipate in the near future. Its major sub-phyla are: (I) Sinitic and (II) Tibeto-Karen. Sinitic is the Chinese languages plus Bai or Minchia. (However, Benedict [1976] made Minchia a major sub-phylum coordinate with Sinitic and Tibeto-Burman.) Tibeto-Karen in turn splits in two: Karen and Tibeto-Burman. The latter consists of Tibetic, Baric, and Burmic, each with many languages in it. Shafer's and Miller's "Bodic" is basically the same as Tibetic. Baric is Garo and some other languages spoken north and east of Bangladesh or a bit west of the main mass of Naga languages along the India-Burma border.

The question arises about a large and diverse phylum like Sino-Tibetan: what are its external relations? Especially, since it sits between the massive Nostratic super-phylum to the north and the equally large Austric super-phylum to the south, its history of having been untangled from one might encourage us to look towards the other. Yet, there is no Sino-Altaic nor Sino-Nostratic nor similar hypothesis in the literature that I know of or that Ruhlen mentions. The massive Sinitic component in the Japanese lexicon, virtually all the Japanese numbers, for example, seems to fool no one at all. Yet there are persistent hypotheses, all directed at what is left over in the north after the Nostratic languages are taken away. Sino-Tibetan is said to be related to Ket-Kot of the Yenisei Valley (e.g., Gray 1939:389; Shevoroshkin 1986). Edward Sapir is associated with a suggestion that Chinese is related to Na-Dene, while Robert Shafer, Heinz-Jürgen Pinnow, and Sergei Nikolaev are in agreement. Sapir was primarily an Americanist, while Shafer and Pinnow are Asianists. This opinion, shared among the four of them, is not to be considered trivial. Finally, there is an even grander suggestion, associated with Starostin and Nikolaev, that Sino-Tibetan is related to both North Caucasic (plus Hurrian) and Na-Dene. It would seem almost given that this proposed grouping of old Southwest Asian phyla with Sino-Tibetan and the second oldest phylum in the New World, assuming that Amerind is one phylum and the first there, must be older in its occupancy of northern Eurasia and the Bering Sea area than any version of Nostratic. Indeed, that is exactly what the relatedness of Ket-Kot and Yenisei would mean. (More on Caucaso-Sino-Dene below.)

AUSTRIC (AUSTROASIATIC; MIAO-YAO, DAIC, and AUSTRONESIAN): Austric is not to be dashed off lightly, and Ruhlen treats it cautiously, giving the Austronesian portion another chapter in its own right because of its enormous size (= 1,175 languages) and tremendous geographical spread across 205 degrees of longitude (Madagascar to Easter Island) or 57% of the earth's surface in its wider equatorial zones.

Each of the potential sub-phyla of Austric can stand, and in two cases have stood, by itself as an independent phylum. Both Miao-Yao and Daic as new-born foals, so to speak, are not entirely accustomed to being separated from their previous mother, Sino-Tibetan. But Austronesian is one of the oldest linguistic phyla around and certainly one of the largest. It was first proposed in its Indonesian form in 1606, and again in 1702, quite a long time before Jones made his famous remarks which supposedly began the IE hypothesis and hence historical linguistics! Ruhlen's pleas against "Euro-centric bias" find a more telling argument in his statement that a fairly complex and reasonably accurate version of Austronesian was presented two years before Jones' speech. Perhaps more convincing in general terms is his calculation that 40% of all human languages are found in Austric, Indo-Pacific, and Australian!

Let us consider the sub-phyla first and then the question of Austric's validity. Beginning in the west in India, the AUSTROASIATIC family is a solid entity and has been for several generations. It has gone into and out of proposed Austrics with some regularity but always stayed intact. Its key anchors in Munda, Khasi, Mon, Khmer, Nicobarese, and (usually) Vietnamese held it together. As is more often the case, sub-grouping has been the source of disagreements (rather than Altaic-type questions of genetic relationship). Ruhlen's authorities speak now of 150 Austroasiatic languages, and some find as few as two basic branches, while others get 10 or so. Munda seems always to be one branch of whatever scheme is proposed, so the hard problems consist of the relations among the southeast Asian members (plus Khasi). Diffloth and Pinnow favor a basic east-west split, with the eastern branch having several equal members (rather like IE); this is the scheme Ruhlen adopts. In some ways, a much bolder scheme is that which sub-divides the eastern part into a northern tier which puts Vietnamese together with Khasi of India, as opposed to a southern tier which links Nicobarese with Aslian (Semai, et al). Pinnow also puts Nahali of central India, usually seen as an "isolate," along with Munda in the western branch. Most Austroasiaticists do not include Nahali however.

MIAO-YAO is solid. There is a scattering of Miao varieties from south-central China to Thailand; they are always isolate in someone else's context. Since this is a common pattern around the world for old remnant languages, it is surprising that the Miao varieties are only dialects. However, Benedict includes the "Pateng group" as a distinct Miao language. Yao consists of at least three languages. The distribution of Miao and Yao then becomes an interesting problem in culture history. Both are quite singular and definitely not too similar to each other. Within the structure of the Austric hypothesis, the experts give Miao-Yao a status equal to both Daic and Austronesian joined together or Austro-Tai. Clearly, then, the relatively small Miao-Yao group has a large phylogenetic status.

At least two reconstructions of Proto-Miao-Yao (PMY) have been made, the one by Herbert Purnell (reported by Ruhlen and earlier by Benedict) and the other by A. Pejros (reported by Shevoroshkin). Benedict reported on Chang's tonal reconstructions (from 1947 to 1972) but seems also to have made some of his own. All this work is particularly valuable in sorting out the loan words from various sources which have made the classifications of Southeast Asian languages so vulnerable.

DAIC may also be called KADAI, following Benedict's usage. The original conception of a Thai-like group, which was so often included in Sino-Tibetan, was based on Thai and its close relatives (e.g., Laotian, Shan, Ahom, etc.), which collectively showed the maximum amount of Sinitic borrowing. With the inclusion of such languages as Li, Lati, Lacqua, and Kelao, Benedict was able to make headway with his Tai-Austronesian hypothesis because they often showed crucial archaic forms. For example, in the meaning of "eye," Benedict's matching of the Thai group's /ta/ against Common Indonesian /mata/ was underwhelming, indeed made weaker by Li forms like /sa/ and /cha/, until he was able to show Lati /mcu/ with stress on /-cu/ and a general tendency for the Daic languages to lose the unstressed initial consonants.

Kadai was much increased and strengthened by the contributions of Chinese scholars, especially Fang-kuei Li, who published data on the Kam-Sui languages and Ong Be from 1943 to 1967. The 16 Thai languages and dialects were now matched by 8 non-Thai or "para-Thai" languages and dialects, which tended to confirm earlier reconstructions of Proto-Thai, and 20 languages and dialects in the other half of Daic, namely, in the Li-Lacqua and Kelao-Lati groups.

The present Daic sub-grouping reported by Ruhlen but reflecting Benedict's 1983 opinion show interesting changes of the picture seen above. The preference now is to set Lati-Gelao (Kelao) apart as a full half of Daic (which now has 57 members), while the other 55 languages are put into Li-Kam-Tai, which is itself divided into Li-Lacqua-Laha and Be-Kam-Tai. Be (Ong Be) is half, and the other half divides into 6 Kam-Sui languages on the one hand and the 44 Tai on the other. Many of the latter are still found in China, from which it is usually reckoned that all the Tai of Thailand, Laos, and Burma are derived.

AUSTRONESIAN has been linked to Daic, as the Austro-Thai hypothesis, more persistently than Austric has been proposed. Benedict was more sure of Austro-Thai than he was of Austric, and Greenberg in 1953 accepted Austro-Thai, even as he tried to re-connect Miao-Yao to Sino-Tibetan and as he rejected Schmidt's Austric of Austroasiatic and Austronesian (but not Miao-Yao nor Daic). Yet, I would guess that, so vast is the Austronesian realm, that most students of it have not been able to concentrate their energies on peripheral matters like Austro-Thai and that some are in fact opposed to such a linkage (e.g., Dyen).

Malayo-Polynesian, the old name for Austronesian, has enormous trouble with its internal genealogy. What is very striking about this phylum, after the large numbers and geography have been appreciated, is the consistent agreements about membership. What belongs is not a matter of controversy. That Austronesian, with nearly a thousand members, is a valid linguistic taxon is not disputed at all. The attributes of Proto-Austronesian (PAN) are reasonably well agreed upon; indeed, the reconstruction of PAN is far advanced over the proto-languages of phyla of comparable size like Australian, Indo-Pacific, Niger-Kordofanian, or Amerind or smaller phyla with great internal diversity like Afrasian, Nilo-Saharan, or Khoisan.

What troubles Austronesian is the strong controversy over the relative status of the Formosan and the Melanesian languages in its membership. It is also troubled by outright contradictions between two different methods of sub-grouping, and it has problem areas (e.g., Melanesia) where traditional methods of reconstruction appear to produce impossible or stupid results, even when the task is undertaken by one of the most competent practitioners.

Using lexicostatistics, careful mathematical criteria for clustering, and hundreds of Austronesian word lists, Dyen and his associates created the most complex, exhaustive, and definitive internal classification that the phylum has ever seen. Among other things, it proposed that the center of diversity of the phylum was in Melanesia and that the traditional notion of a homeland in south China or Formosa was mistaken. The Formosan languages had failed to show as much distinctiveness as the Melanesian languages had.

Yet, in the next twenty years, Dyen's colleagues failed to accept his conclusions. Using traditional methods which stressed reconstructions, they came to accept a different internal classification represented by that proposed by Robert Blust (1978) and several others. This is the one which Ruhlen chose to support. Interestingly enough, it also reflects Benedict's old emphasis on the separateness of the Formosan languages. The scheme adopted by Ruhlen goes like this: (I) Atayalic, (II) Tsouic, (III) Paiwanic, (IV) Malayo-Polynesian. The first three sub-phyla are found only among the 14 "aboriginal" languages of Taiwan (Formosa). All other Austronesian languages (945) are in the fourth sub-phylum; it has two primary branches: (A) Western and (B) Central-Eastern. Western has 11 sub-branches, with 4 being Philippine, 4 being Indonesian, and 3 (Chamorro, Palauan, and Yapese) being Micronesian. I would suggest that it simply be called "Indonesian." Central-Eastern has a Central branch with 89 languages focused on Maluku, Timor, and Flores of eastern Indonesia and an ill-named Eastern branch of 482 languages. Besides the 56 languages of South Halmahera and Northwest New Guinea in one branch, we find the well-named Oceanic branch of 426 languages, which contains the greater part of Melanesia, most of Micronesia, and all of Polynesia. The Dyen scheme has been stood on its head! But, from an ethnological point of view, one of the great virtues of Dyen's classification, namely, the fact that Polynesian is a mere twig on a great bush, has been retained.

The new scheme chosen by Ruhlen has striking similarities to Niger-Congo, if the Kordofanian sub-phylum is left out, and argues emphatically for an old Austronesian settlement in Sunda land or Formosa, followed by secondary occupation of ethnological Indonesia, followed then by an invasion of the Indo-Pacific realm (Melanesia) and then a more rapid surge into the unoccupied Pacific. It is a scheme which ought to attract much anthropological attention because of the long-standing interests of both biological and cultural anthropologists in the peoples of Oceania. What is also interesting is that, even if Blust and his followers are wrong and Dyen is right, the homeland would still ultimately have to be in the west by force of the Daic linkage or the Austric hypothesis, of course. If Dyen is right in his scheme, and also in rejecting ties to Daic, then the history of the peopling of the Pacific becomes quite different.

The strange case of reconstructions which follow correct IE methods and produce cockeyed results is found among some Melanesian languages; it was reported by George Grace at the recent Stanford conference. The general conclusion seemed to be that a prevailing but highly unusual social situation was responsible for extraordinary amounts of code-switching, bilingualism, and gender-based dialects. In fact, a similar situation had been reported in the northern Amazon by Ward Goodenough in a well-known article and from Papua on occasion in the ethnographic literature. Although some scholars were excited by Grace's discovery and tried to start a rampage of classificatory destruction, the Austronesianists refused to draw the conclusions either that Austronesian should be broken up or that IE methods did not work well — usually.

What remains is the question of the validity of Austric. I believe that some scholars have been relaxed about Austric because they saw it as a large phylum but not as something as hair-raising as Nostratic or Amerind. Yet Austric should indeed be regarded as our first viable super-phylum. Numerically speaking, it is the largest entity around, having nearly one fourth of the entire human roster of "roughly 5,000 languages" counted by Ruhlen. It is supported by a number of linguists. Much of the lexical evidence produced for it has been quite sophisticated, taking advantage of the advanced state of reconstructions in the area generally, and there has been a lot of it. BUT, much of the lexical evidence has been interpreted by Benedict as derived from sub-stratum effects, i.e., Austroasiatic and Austro-Thai have borrowed from each other at an early date, and thus the question of genetic kinship is delayed until the borrowing problems can be solved. Some linguists are very critical of Benedict for posing the sub-stratum problem, and Greenberg has recently cut the Gordian knot by treating the evidence as genetic rather than sub-stratum. Many of Benedict's presentations in both Austro-Thai and Austric have been reduced considerably in effectiveness by his reliance on reconstructed forms whose resemblances to modern forms seem truly tortured. Much can probably be settled if Benedict's (eventually) powerful arguments can be rescued from the forest of starred forms and tangled underbrush of extremely unconvincing proposed similarities (e.g., *li* = *sa* = *ma* "lick, tongue"). Benedict has argued, however, that the Austric members centered on mainland Southeast Asia, as opposed to either India or the Pacific, have had most of their morphemes systematically reduced to monosyllables and inherited affixes lost because of the areal linguistics of that area — short words with no visible affixes and many tone phonemes. Hence, the need for reconstruction to recover much of what was lost. Pinnow has also shown that, as between the Indian sector and the Austronesian, a number of common grammemes can be found. My own belief is that the longer one looks at the Austric hypothesis, the better it looks.

INDO-PACIFIC: Another of Oceania's vast phyla, basically, Indo-Pacific or I-P, is a phenomenon like Australian or Niger-Congo. It is a very large number of languages, 731 according to Ruhlen, which is strongly associated with one geographical area and one prevailing physical type. In this case, New Guinea (plus eastern Indonesia and the Melanesian Islands) is the area and Papuan the physical type. Indo-Pacific has some very distant outliers whose physical types have as often been associated with Australia as with New Guinea and whose locations in the Andaman Islands, Timor, Halmahera, Bougainville, Santa Cruz (near Fiji), and Tasmania strongly suggest that Indo-Pacific was resident in most of the southwest Pacific when Austronesian began occupying Indonesia and realms to the east. The distribution of Indo-Pacific seems to justify the traditional ethnological assumption that so-called "Australoids" were the first inhabitants of the insular near-Pacific and Asian lands near by.

Although an unspoken notion, that all non-Austronesian languages in and around New Guinea were probably related to each other, has been around for a while, Indo-Pacific was invented by Joseph Greenberg in 1960, with formal proofs offered in 1971. Ruhlen argues that by 1950 "the common belief was that the New Guinea area contained innumerable small families (only a few of which had been identified), that displayed no relationship either among themselves or to languages outside New Guinea." The unspoken notion was found among anthropologists and was primarily based on theories of racial sub-strata, in my opinion. Even against this background, Greenberg's hypothesis was breath-taking. Soon, however, the data on long-neglected Papuan languages began to pour in, mostly through the efforts of Australian linguists like Arthur Capell and Stephen Wurm and the Summer Institute of Linguistics. A large part of I-P has been independently confirmed, and many hundreds of new languages have been placed in sub-categories, some of them very new.

The full reach of Greenberg's I-P has not been independently supported by many scholars, but the major reasons are caution rather than controversy. Many of his outliers have been confirmed, most importantly the Santa Cruz group (William Davenport 1962), Bougainville (Allen and Hurd 1965), and the Timor-Alor-Pantar (Watuseke and Anceaux 1973). Two aspects of criticism might be that none of the Papuanists seems to be ready to include Tasmania and the Andamans in the same phylum and that some Papuanists reckon that there are several independent phyla in I-P. It is almost certain that this huge phylum has enormous time depth in it, relative to most linguistic phyla, and that the "critics" are not so much critical as simply unwilling to connect languages which seem so remote from each other and which seem to have so little in common. In the case of Tasmanian, the criticisms have been hotter, especially those of Dixon and Crowley, but there the condition of the languages is a major consideration. The ten Tasmanian languages have been extinct for generations now, and the critics maintain that the data recorded cannot be trusted much. Some others disagree with that assessment.

That Tasmanian should be thought, albeit incompetently, to be related to Papuan languages, instead of Australian, strikes anyone with access to a map as anomalous and incredible. Greenberg indeed supposed that Australian would join Tasmanian or Papuan at some level but found that he was unable to propose such a linkage. Recently, as Ruhlen reports, Blake (1981) suggested a small number of links between one Australian language and one Tasmanian. But, so far as I can tell, Pater Schmidt never proposed that Tasmanian (which he wrote a book about) was related to Australian. Morris Swadesh, in his final reduction of the world's languages to a dozen phyla, did not put Tasmanian in his Australian phylum either; he linked it to Austric and Papuan.

While the diversity and time depth in I-P might inhibit one's support for Greenberg's hypotheses, the very numerous languages offer an advantage; one should be able to construct a large number of etymologies linking the various branches to each other. From my own lengthy inspection of Greenberg's proposed etymologies, I conclude that I-P is a viable genetic grouping and that it will become stronger as more scholarship is applied to it. An additional advantage is found in the fact that I-P has 13 branches, a situation like IE, where reconstruction is enhanced because the chances of an ancestral form surviving in some branches are much better. Were the ten wobbly Tasmanian languages to constitute one coordinate sub-phylum, or the four surviving Andamanese another, access to Proto-I-P would be much more difficult, as well as the task of constructing a network of etymologies.

AUSTRALIAN: The thirty branches of Australian, fifteen of which are single languages, are largely concentrated in the northwestern part of the continent, but most of Australia was occupied at the time of European contact by the Pama-Nyungan branch — hundreds of fairly similar languages and dialects. No doubt, this has contributed greatly to the ease with which the phylum has been accepted. Had contact begun in the north, we would have another Altaic! Perhaps the most convincing buttress to this argument is the relatively brief and mild controversy about the number of separate phyla in the northwest. Pater Schmidt, surely a master lumper, was unable to accept the membership of all of them in the same family. Having spent an important part of his life working on Australian and being the first to detect the vast southern branch, his conclusion in 1919 that there were many separate phyla in the north stimulated the scholars following him. In 1923, Kroeber proposed that all Australian languages were in the same family. Capell concurred in 1937, and Greenberg in 1953. Since then, the matter has not been much in dispute, although Dixon has some doubt about two northerners, Tiwi and Djingili. Australian linguists present at the Stanford workshop were unmoved by the surge of phylum bashing which occurred. Their own concerns were internal classification and reconstruction.

Australia as a language area nonetheless has unusual characteristics. Phonological homogeneity is one. The pervasive presence of laminal consonants is another. With phonetic change having much less tendency to disguise cognates than elsewhere, particularly in Austric, the lexical change tending to be simple loss, Australia is unusually kind to lexicostatisticians. Finally, more than any other area, this one got vital help at a crucial time from amateurs, in particular a sheep rancher named Edward Curr, who gathered data on 500 varieties of Australian and published them in a four volume work in 1886-87. Schmidt, Kroeber, Greenberg, and everyone else until the 1960's used Curr for their main database.

External relations seem to concern nobody. It is rare to hear of any hypothesis linking Australian to any other phylum, except the very occasional mutterings about the absence of linkages to Tasmanian and I-P. Blake's suggestion, mentioned above, that one Pama-Nyungan language has ten resemblances to one Tasmanian dialect is actually the only one I can remember. It is entirely possible that the reason that kinfolk are hard to find for Australian is very great time depth. Between 30 and 40 millennia are the usual archeological estimates for the human invasion of Australia, and this is probably the minimum age of separation of the Australian phylum from any purported kin.

Yet, it is extremely likely that southern Australia was first occupied by some language group different from the present Australian phylum. That earlier southern group I will call "Victorian" because that is a famous place name applied to much of the south. It is tempting to call it "Murrayian" after the anthropological tradition of Carpentarians in the north and Murrayians in the south. The reason for invoking such a hypothetical entity is that Australia as a phylum is very unlike I-P with its 13 evenly distributed sub-phyla; rather, it is lopsided like Austronesian — only more so. If 75% of the sub-phyla of Austronesian are on one island, while the other 25% occupy half the world, Australian has 97% of its primary branches concentrated in the northwestern sixth of the continent, while 3% occupy the vast remainder — say 2.5 million square miles. It seems truly obvious that Australian spread out from its confines in the northwest much more recently than 30,000-40,000 years ago. Since the south did in fact have people in it most of that time, I reckon that "Victorian" acquires validity by implication.

The southerners might indeed be related to Australian, as a large and dispersed set of additional sub-phyla or one major group coordinate to all the rest of Australian. Or "Victorian" could have been what Swadesh chose to call "lost languages" of unknown or unknowable genetic affiliation. But, in my opinion, the better likelihood is that "Victorian" was still in existence until the 19th century, albeit confined to the island of Tasmania. As a major branch of I-P, Tasmanian might have been spoken by Papuans with a mighty flair for sailing, but it seems easier to suppose that they traveled down across the Australian continent to get from (say) the Torres Strait area to Tasmania.

ESKIMO-ALEUT: Sometimes called Eskimoan or Macro-Eskimo. Although this well-known and solid group is spoken in North America, it is not a factor in the hot disputes about native American linguistic phyla. Its primary divisions are into: (A) Aleut and (B) Eskimo: Yupik (in the west) and Inuit (over most of the north to Greenland). Three of the five Yupik languages are spoken in eastern Siberia next to Chukchi-Kamchatkan. Inuit's three languages are spread over a vast area and represent a fairly recent expansion from the west. The genetic link between Aleut and Eskimo had been known since 1818 (Rask), but unpublished until 1918, according to Ruhlen, quoting Pedersen. Its solid status has been apparent for about a century now. However, because Eskimo was an archetypical example of polysynthetic morphology, the Eskimo-Aleut may well have been viewed as merely the most northerly of the vast array of American language clusters showing polysynthetic morphology. It is really in the attempts to link it with Asian and/or Eurasian phyla that we can see its differentiation from American families most clearly. Some of the motivation for that may have sprung from the ordinary ethnological tradition that the Eskimos were not Indians, that their deepest links must be with the Siberians and other Circumpolar peoples.

External relations and the dates of its presumed arrival from Asia constitute the main points of interest in Eskimo-Aleut. (The dates are not discussed here.) More than half of the Nostratic proposals include Eskimo-Aleut, beginning with Pedersen in 1931 and ending with Dolgopolsky and Greenberg in 1986. Swadesh's huge Basque-Denean in the 1960's also included it. While Illič-Svityč did not include Eskimo-Aleut in his very influential Nostratic publications, his students and colleagues have done so. Thus, we may regard at least an Asian connection, but more particularly a Nostratic one, as the dominant view of Eskimo-Aleut during the 20th century. Generally speaking, those who have an important AA presence in the Nostratic west have tended to leave Eskimo and Chukchi-Kamchatkan off at the eastern end (Menges 1977, Birnbaum 1978, Hodge 1986, Bomhard 1987), except for Pedersen (and

Bomhard in his most recent writings), or chop AA off in the west if they have Eskimo and Chukchi attached in the east (Collinder 1965, Greenberg 1986). The suggestion is thus strong that Pedersen's original Nostratic was a higher level genetic unit than those that followed, although Swadesh's Basque-Denean was even larger but somewhat different.

Two more remarkable things about Eskimo-Aleut need mentioning. Ruhlen says that Martin Frobischer in 1576 "proposed a relationship between Uralic and Eskimo." I suppose that is the earliest known date for some version of the Nostratic hypothesis. One could argue, of course, that there have always been foolish people and there always will be. However, the persistency of the proposed linkages among various purported Nostratic languages indicates, to me, that there is evidence available that individual scholars do see from time to time. Rather than this showing foolishness, it shows independent confirmation. The second thing is that the conservative splitters of America see part of the Nostratic phenomenon and tend to accept it, but they would be very loathe to admit to a belief in Nostratic. Campbell and Mithun (1979:39) say: "The proposal of a genetic relationship between Eskimo-Aleut and Chukotan in Asia (Chukchi-Koryak-Kamchadal) is worthy of more research. It appears promising, but not yet sufficiently documented to embrace uncritically. It is the only proposal of connections between New World and Old World languages which at present appears to be worthy of attention. (See Krauss 1973a.)" When something is "worthy" to that lot, it must be fairly obvious!

NA-DENE: The history of classification in this phylum is genuinely interesting, involving some of the great pioneer linguist-anthropologists and some crucial theoretical disputes. Since most of this is very well known and often taught in university courses on historical linguistics, and considering how good Ruhlen's summary of it is, we will mention only a bit of it here. Perhaps the most telling point mentioned by Ruhlen, but first unearthed by Michael Krauss, was that "By the mid-nineteenth century Russian linguists had arrived at an understanding of the Na-Dene phylum that differed little from that reached by Americans a century later (see Krauss 1976:334)." Via their occupation of Alaska and much of the Northwest Coast down to Spanish California, the Russians had access to the most divergent Na-Dene languages, to wit, Eyak, Tlingit, and Haida, plus so-called "Pacific Coast Athabaskan" of Oregon and California, by 1805. As early as that, Rezanov, the Russian who recorded the first Eyak, was attributing similarities among Eyak, Athabaskan, and Tlingit "to borrowing rather than common origin." This was to be Boas' stance and later the famous "Diffusionist" position a century later.

The Na-Dene which Ruhlen and Greenberg accept has two primary branches: (I) Haida and (II) Athabaskan-Tlingit, which divides into (1) Tlingit and (2) Athabaskan-Eyak. (Athapaskan has always been an acceptable alternative.) Haida, Tlingit, and Eyak are each one language. Athabaskan is 31 others, not the least of which are the four Apachean (including Navaho) far to the south in New Mexico, Arizona, and Texas. It is the Haida connection which causes virtually all of the modern controversy. No one doubts that Haida is very remote from any other language. Levine (1979) and others influenced by him feel that the very small number of cognates which link Haida to the Na-Dene group actually disappear on close examination. Greenberg examines Levine's examination even more closely and manages to put back some 14 of 30 Haida-Athabaskan comparisons, some 17 out of 30 three-way comparisons (Haida, Tlingit, and Athabaskan), and finds that 17 cognates are four-fold, i.e., they are also found in Eyak. This is better than the six that Greenberg is able to find within IE, using Levine's criteria and comparing some 37 supposed cognates among Albanian, Armenian, and Keltic. One should point out that Greenberg found 20 cognates between Tasmanian and I-P (figures for I-P from Ruhlen, p. 180). But it is also important to note that the principal Na-Dene hypothesis, i.e., the relationship among the 32 non-Haida languages, is not itself controversial nowadays.

Unlike the Eskimos, the Na-Dene peoples were not normally distinguished from Indians in the ethnological tradition. Indeed, the warlike Apache and their Navaho cousins were the epitome of Indian-ness to many Americans. Hence, for external relations, one could look in all directions. Given the difficulties with Haida, it would not appear easy to generate etymologies with other phyla. Indeed, relations with Amerind languages are proposed by some people, according to Ruhlen, but no names are mentioned. What are mentioned are the several hypotheses linking Na-Dene to Sino-Tibetan (Sapir 1925, Shafer 1952 and 1957, Swadesh 1952). To these, one must add the Russian proposals to link both of them to North Caucasic, Hurrian-Mitannian, and Yeniseian (Ket, Kot), as well as Basque (Shevoroshkin 1987). The latter is also Swadesh's opinion. Since I have not seen the relevant evidence nor arguments presented, other than Swadesh's, I cannot evaluate these hypotheses.

AMERIND: A phylum, or perhaps super-phylum, embracing all of the native or Indian languages of North and South America, except for Na-Dene and (naturally) Eskimo-Aleut, has been proposed by Greenberg. The book presenting the evidence is entitled *Language in the Americas*, which was published in 1987, though announcements about his findings go back as far as the 1950's. No other prominent linguist has ever gone this far. But the furor aroused by the hypothesis, and the fact that the proposer was Joseph Greenberg himself, has become intense and promises to become even more so. While some Americanists seem to regard the hypothesis as too bold and clearly irresponsible, it will not strike an Africanist or Oceanist that way. We are used to him being bold, but we are used to him being RIGHT. We are used to very large numbers of very diverse languages with great geographic reach, so the whole proposition is not so startling or remarkable to us. The real question is whether the Amerind hypothesis is right, or not.

This review will confine itself to a brief summary of Ruhlen's history of Amerindistics, a quick glance at the main outlines of the classification, a discussion of contrary arguments, and finally an evaluation of the evidence Greenberg presents in his book. This from someone who has never studied the languages of the New World, hence an Old World viewpoint.

Ruhlen describes a kind of vertical triangle of history — from A to B to C. Herein A is equal to C in large measure, while B is a wholly different point in opposition to both of them. Much of the history was Point A, where much data were recorded and scores of phyla were proposed. The high point of A may be the Powell classification of 1891, with 58 phyla in North America alone but South America basically untouched. Some movement towards phyletic reductionism involved Boas and others, but it was Sapir who took the whole field to Point B, where the phyla in North America were reduced to 6. Usually starting immediately after the Sapirean proposal, but in any case gathering strength by 1964, came the attacks on Sapir's classes and increases in the number of proposed phyla. This Point C culminated in the Campbell and Mithun book of 1979, which proposed 63 phyla for North America, plus Campbell's ten separate Central American, for a total of 73 for North America, including Central America. At the same time, Loukota had increased the South American phyla to 117. Truly, Point C (which Ruhlen calls Phase III) represented a dramatic advance towards the wisdom of our grandmothers. All of the Old World has far less than 100 phyla, while the New World, which is commonly supposed to have been settled from the Old World "at a later date," has almost 200 phyla! It has been the most remarkable achievement in historical linguistics, indeed in all of prehistory, for a very long time!

Although Ruhlen tends to neglect and play down his work, Swadesh has to be part of Point B too because he got North and South America down to four phyla in the 1960's. Ruhlen does quote him saying in 1960 that "recent research seems to show that the great bulk of American languages form a single genetic phylum going far back in time... Eskimo-Aleutian and Nadenean seem to stand apart, and may therefore represent later waves of migration..." It was the Sapirean high point. Besides Na-Dene and Eskimo-Aleut, which he put into Basque-Denean, Swadesh had Macro-Mayan (which embraced the rest of North American), Macro-Chibchan, and Macro-Arawakan, or two largely South American phyla versus one wholly North American. Not only the ratios but also the general membership of each reminds one of Greenberg's scheme.

The internal classification of Greenberg's Amerind represents the Point C phyla being brought together and sorted into categories but all explicitly related to each other. He postulates six primary sub-phyla, named Northern Amerind, Central Amerind, Chibchan-Paezan, Equatorial-Tucanoan, Andean, and Ge-Pano-Carib. The most startling, interesting, and troubling thing about the gross classification is that two-thirds of the sub-phyla are in South America or partly in adjacent areas like Central America and the Caribbean. So one-sidedly North American are our usual perspectives that I expected the center of diversity to be in California and Mexico. I got that perspective from listening to the preoccupations of Americanist teachers and colleagues. Algonkian, Zuni, and Mayan are not the centers of the Amerind universe! Tarascan, Miskito, and Yanomami are much closer to it, although still a bit north of center. The anomaly is that we are confronted with the opposite situation in Australia; here, it is generally assumed that Amerind is northern in origin, yet its greater diversity is in the south. As in the case of "Victorian," one can suppose that some of Amerind which was in the north has been lost. That is the opposite of Swadesh's supposition that the lost languages were in South America. And here I propose that Na-Dene and Eskimo-Aleut are the villains in the piece. Or Na-Dene is the undetected closest relative of Amerind.

Americanist counter-arguments started before *Language in the Americas* came out. They gathered strength at the Stanford Conference. As they have been variously directed at Kroeber, Sapir, Swadesh, and Greenberg, they have been consistent, reiterating a set of themes which can fairly be called the "splitters" mentality. There is also an unstated central premise or Weltsanschaung or visceral state which is much harder to demonstrate but which can surely be called "conservative." Do not change things but, if you must, do it slowly and grudgingly. Some of the actual arguments are good. The reader is directed to Lyle Campbell and Marianne Mithun, eds., *Languages of Native America: Historical and Comparative Assessment*, 1979, especially the 67-page Introduction. For example, one should work with adequate data, one should test relationships against large word lists and have basic grammars at least on hand — not work with word lists consisting of 20 words and no grammar like Kroeber and Dixon did in California. One ought to be very concerned with borrowings and areal linguistics, i.e., structural influences (phonological, morphological, syntactical), and circulating cultural words. The Americanist splitters stress grammatical borrowing as part of their concern for Sprachbund phenomena. In this, they derive directly from Boas. Of course, I find that refreshing after long contact with Semiticists, whose belief in the primacy of grammatical evidence is so strong! So, the first key arguments are that one should use good data and one should watch out for borrowings and influences.

Unfortunately, the flip side of the argument is false, even though it is one dear to methodologists everywhere. Hypotheses which are generated by those who use poor data and neglect borrowings are false. Or poor methods lead to bad results, ergo, results based on poor methods must be false. The conclusion is a non-sequitur, and the history of science does not support it. In the subtler prose of Campbell and Mithun, we read at the Conclusions to their Introduction that sadly enough American Indian linguistics had seen "...the perpetuation of the hypotheses of influential scholars without regard to the rigor of their methods or the weight of their evidence. It is hoped that a recognition of this history as a perpetuation will halt the momentum of the cumulative view so that oft-repeated but poorly founded proposals will be reconverted into empirical hypotheses worthy of subsequent research." They and their colleagues then proceed to demolish most of the "Lumper" hypotheses of the past and replace them with safe little ventures more worthy of consideration. This also sounds to me like the renascent voice of Leonard Bloomfield and the Operationist/Behaviorist stance in American social science.

There are two more key arguments which have been hurled at the Amerind hypothesis. One is associated with Campbell and Ives Goddard, although it is an old one often used in the Old World. The second is found commonly among historical linguists; it was highlighted by Campbell and Mithun via a long quotation from Ives Goddard. It is reported, and disputed, by Ruhlen at great length. The first says that anyone can pile up a bunch of similarities between two languages and a bigger bunch if there are more languages involved. Therefore, many similarities between or among languages proves nothing. Campbell gave a public demonstration of this point at Stanford by producing many similarities between Finnish and the Penutian etymologies proposed by Greenberg. It was very impressive. (Perhaps the model for this exercise was Dyen's display of similarities between Proto-IE and Proto-AN, which was designed to mock Benedict's Austro-Thai.)

The remaining argument, or the second of the above, stresses the comparative method and the need for reconstruction. Why? Because through the comparative method, one can establish the precise sound and meaning correspondences between two languages. Therefore, one can eliminate borrowings and areal influences. One will then not be fooled by any bunch of similarities. Indeed, both distant genetic relations and close ones operate out of and require the same comparative method. If proposals of distant relations are not to be spurious ones, as seen above, then they have to be based on good, solid similarities to begin with, i.e., the kind of etymologies one would want to begin reconstructing with: P = P, except after E, etc.

Ruhlen treats this argument as the crucial one, as it does seem to underlie the others, and attacks it repeatedly throughout the book. If I may recast his rebuttal in my own terms, it makes two points: First, rigor and reconstructions did NOT actually give us the old solid phyla like IE and Uralic or any others in fact. Scholars with hypotheses have been the sources of our phyla. The rigor and reconstruction people have distorted the history of historical linguistics and effectively block our further progress. Second, reconstruction has not actually been so successful as its advocates argue, and the so-called proofs of phyla offered by reconstructions are not really the reasons that scholars believe in various phyla. It is the accumulation of convincing evidence that causes scholars to see languages as members of some phylum; after that, they may start working on reconstruction, but the evidence has already persuaded them that it will not be a waste of time to reconstruct.

Then is the evidence presented by Greenberg for Amerind convincing? No, say some American Americanists with great intensity. But some of their fellows do think the evidence is convincing. Some Russian scholars find it convincing. My own opinion, rooted in my experience in African phyla is that Amerind is not only convincing, but it is also a robust hypothesis. Although some proposed etymologies do not provoke belief, others are so unlikely to be due to anything but genetic connection that they could carry the entire hypothesis by themselves. For example, the 1st person marker (usually a pronoun) **n-/*-n* contrasting with the 2nd person **m-/*-m* is far too widespread to be due to chance or borrowing. Widespread here means from north to the south, among most major branches, and altogether more than a hundred times. An alternative 1st person marker **i-* is "indeed very common in Amerind." Its alternation with **n* is exciting to an Africanist. While pronouns are not the only good evidence in the world, I agree with the Semiticists and Nostraticists that pronouns really do not get borrowed very much, nor do they change easily. This is an empirical matter to me, not a matter of faith in one kind of evidence. When pronouns have changed, by replacement, or seriously disguised phonetic change, as in the Chadic and Omotic sectors of AA, then everything becomes more difficult.

Lexical evidence, other than pronouns or grammemes, is abundant, either to tie specific sub-phyla together as innovations and unique retentions or to tie various sub-phyla to each other. One of Greenberg's appendices lists the number of links between any two sub-phyla or among larger numbers. Taking a Northern stock, Almosan-Keresiouan, compared with a southern, Andean, or a central, Chibchan-Paezan, we find 34 etymologies with both Northern and Chibchan in them and 21 with both Northern and Andean in them. This probably reflects the fact that there are 43 Chibchan-Paezan languages but only 18 Andean. Between the two South American sub-phyla, there are 32 etymologies. It is hard to do any better than this in most of the African phyla.

As a footnote to the Amerind question, which will most assuredly be a continuing and bitterly controversial problem within the mind of American historical linguistics, Russian linguists, more or less independently of Greenberg, decided that America had three phyla, Eskimo-Aleut, Na-Dene, and Amerind. Their point man or pioneer is Sergei Nikolaev, who has reconstructed some of Proto-Amerind already, including two pronouns — **nV-* "I" and **mV-* "thou." Their independence, to some extent at least, can be shown by Nikolaev's Amerind "nose" (**sVn*). That is absent in Greenberg's etymologies.

LANGUAGE ISOLATES (SMALL PHYLA): Basque, Burushaski, Ket, Gilyak, Nahali, Sumerian, Etruscan, Hurrian, and Meroitic. All are defined as having a "reasonable amount of documentation that has been evaluated by scholars for a sufficient period of time to know that the language is not closely related to any other known language or group." Yet, in the cases of Meroitic and Etruscan at least, these criteria are not met, both explicitly lacking sufficient documentation. A reasonable amount of documentation is debatable in Hurrian, according to my colleague Paul Zimansky, and sufficient period of time is not the case in Nahali, where good and full data have only been available for a decade or so. Kusunda of Nepal and Shabo (Mekeyir) of Ethiopia are borderline cases where the data have remained insufficient because neither is close to any other language but where a great increase in data could lead to successful linkage with an existing phylum; these also suffer from lack of scholarly attention. Some of Ruhlen's "Isolates" could be better left as "Unclassified."

Let us examine these small phyla one by one. This is basically not done in the book and strikes me as the greatest fault of Ruhlen's whole endeavor. The following discussion is entirely mine, except for the first sentence about Basque.

BASQUE: Literally hundreds of years have passed since Europeans and European scholars have recognized the separateness of the Basque dialects. A complete summation of all the attempts to link Basque to other languages would surely be beyond anyone's competence. Everyone seems to have tried. Not everyone can be said to have failed, however. In our time, there have been three serious efforts by trained comparativists to put it somewhere. In sequence, they are Swadesh, who featured Basque as the western end of his Vasco-Dene, Hans Mukarovsky and his colleagues in Vienna, who see it as connected to AA, and Cirikba, who links it to Sino-Caucasic-Dene or Dene-Caucasic. It is immediately apparent that two are quite close to agreement, viz., Swadesh and Cirikba. Shevoroshkin also includes some of the other "Isolates" in Dene-Caucasic (see below). Mukarovsky's ideas about an AA-Basque relationship seem inherently likely in principle — both are likely to have been near or around the Mediterranean long ago. His proofs are, however, bedeviled by the massive borrowing problem which exits between Basque and the Berber sub-phylum of AA. There is absolutely no doubt that a large amount of lexicon is shared. Having never seen proofs of Cirikba's argument, I cannot assess it. But Shevoroshkin's support is noteworthy. Mukarovsky is very doubtful that Basque is related to Caucasic; indeed, as Ruhlen reports, efforts to link Basque to Caucasic are famous but have won few converts.

BURUSHASKI: The /-aski/ of the Burusho people in the Vale of Hunza in extreme northern Pakistan. Maybe also the language of Shangri-La, if that mythical place has a language in its Himalayan valley. Burushaski and its very close sibling, Werchikwar, have been spoken in the past in what is now called Dardistan and Nuristan. Loan words in nearby IE languages show this. Yet no one sees Burushaski as a possible candidate for the language of the Harappans of the Indus Valley, as far as I know. My own efforts, brief and spread out over two decades, lead me to believe that this "complete mystery", as Ruhlen calls it, will finally end up in or near the larger grouping which Swadesh called Vasco-Dene. It shows bits of resemblance to various languages, primarily of western Eurasia, part of the realm of Vasco-Dene. It may indeed furnish a key linkage between north Caucasic and the Sino-Tibetan which lies over the mountains from the Vale of Hunza. Most of all, the mystery of Burushaski is founded on a lack of prolonged scholarly attention.

KET: Also includes a related but not close cousin, KOT. It is probably better known in Europe and the former Soviet Union as Yeniseian. Everyone who has had an opinion, not many scholars, seems to point to Sino-Tibetan. Whatever its kin turn out to be, and again Vasco-Dene is the best bet, this small phylum will continue to appear remote from all of them. Russian linguists have generated a Proto-Yeniseian which should be a big help in classifying it; their preference is Sino-Caucasic.

GILYAK: Also known as NIVKHTSY in the former Soviet Union, based on /nivx/ "person, Gilyak." Mentioned before as a member of Greenberg's version of Nostratic in the same branch with Chukotian and Eskimo-Aleut. Nevertheless, the Finnish expert on northern Eurasian languages, Juha Janhunen, denies that Gilyak has any external relations. Robert Austerlitz, another expert on the same kind of languages, refused to include Gilyak in any outside group — this at the Stanford conference. My only opinion is that Gilyak may be remotely related to Ainu.

NAHALI: Also called NIHALI, NEHARI, NAHARI, NIHARI, etc. Pinnow is the primary advocate of external relations for Nahali, as a co-member with the Munda group as the western half of Austro-Asiatic. Norman Zide, an expert on Munda, objects that Pinnow's evidence is largely morphological, especially verb conjugations, but that lexical evidence is lacking. Lexical borrowings from Munda are not at all lacking. Since Nahali must have the world's record for borrowing, verb conjugations are no more sacrosanct than the lexicon because they are subject to area influence. Nahali's borrowings come from Sanskrit, other Indic, Dravidian, and Munda. As THE resident native phylum in the heart of India, it has great historical significance, particularly since every other phylum in India has ties to the outside or is extremely northern, western, or eastern. Nahali has an ample lexicon that cannot be derived from borrowing, and that core is the one that so far has resisted all attempts to discover its lost kin. Alas, again we must say that, in this case too, hardly anyone is actually working on the problem.

SUMERIAN: Like Basque, Sumerian has probably been compared with everything in the world at some time or other. And again like Basque, Sumerian may be a prime example of the fruitlessness of binary comparisons, i.e., this one language is almost always compared against another one or a single phylum. Nevertheless, binaristically, several of us reached the conclusion that it was related to Elamite (myself) or Dravidian, as discussed above. We also know some other things about Sumerian. It is universally rejected as a relative of Semitic or AA by Semiticists and Afrasianists, so far as I know. And this despite the very close geographical proximity of the Semitic epicenter in Arabia to southern Iraq (Mesopotamia). The recent archeology of things relevant to Sumeria strongly suggest that the Sumerian homeland was in the hills and/or mountains of Iran, northern Iraq, and eastern Turkey. Sumerians had something to do with the land of Dilmun (Persian Gulf) and both the Arabian and Persian sides of the Gulf. This is all very close to the Elamite realm and the archeological roots of Dravidian. Russian linguists, as of this date at least, have not proposed that Sumerian has any external kinfolk. Morris Swadesh had classified virtually all the world before his death, yet he left Sumerian in a class by itself. Somehow that seems appropriate for humanity's oldest known language!

ETRUSCAN: It is usually said that the Etruscan database is too slender for any valid genealogical work to be done. The reasons for that are the lack of translations for most of the abundant Etruscan texts. "We can read the texts but we do not know what most of them mean." This is the same as Meroitic, but only half as difficult as Harappan. Yet there is an additional assumption lurking here, it would appear, namely, that a very large amount of data are required for classification. There does seem to be some data; grammemes and some basic vocabulary.

Furthermore, people do make suggestions based on that data. Dolgopolsky, for example, thinks that Etruscan is Nostratic because it has *mi* for "I," the first half of the Mitian marker pronouns. Shevoroshkin believes that it has been shown that Etruscan belongs with Basque to the Dene-Caucasic "macrofamily" (super-phylum). Only Swadesh would believe that that difference was unimportant because both would mean Vasco-Dene, but to our contemporaries, the difference between Nostratic and Dene-Caucasic IS important. The rest of us, no doubt, would like most of all to see the data and hear the arguments one way or the other.

HURRIAN: Sometimes presented in its MITANNI avatar, a later stage which had some Indic or Indo-Iranian loan words in it. Hurrian is often said to be related to "Caucasic," no longer a delightfully ambiguous term, and to URARTEAN of Armenia and to HATTIC/KHATTIC of central Anatolia. I will consider each of them separately for formal purposes, although there is reason to believe that the lot of them are related to each other. Most previous discussions were trivialized by the small but powerful book by Diakonoff and Starostin, which tried to show that Hurrian was not only related to North Caucasic but more exactly was simply a member of the eastern or Nax-Dagestan half of that phylum. Their book also presents much Proto-North Caucasic as well as Proto-Northeast Caucasic. Equally valuable, and tremendously daring from a specialist's perspective, they presented the equivalent of a Hurrian dictionary. After reading their book quite carefully, I concluded that they were right, even though some of the reconstructed North Caucasic seemed as seriously tortured as Benedict's Austro-Thai did and despite the presence of too many culture words for my taste. They also present Urartean convincingly as close to Hurrian.

MEROITIC: Also called MEROEAN. Archeologically attested in the northern Sudan but with no known daughters. Meroitic has a serious problem of decipherment. Since it is written in Egyptian hieroglyphs or something close to that, the problem is only one of knowing what the textual meanings are. Some scholars have labored patiently to pry loose a brick here, a brick there, from the house of mystery. So there is a very small Meroitic corpus. Unfortunately, Meroitic is not close to any other language, hence the easy solutions are blocked. The small corpus is mildly controversial, some believing it AA and some N-S. My authority on Meroitic, Bruce Trigger, is inclined to think it is N-S. It probably is.

To Ruhlen's list of so-called "Isolates," which I consider to be small phyla until such time as they get related to another phylum, let us add a few more which he did not know about or did not have the time to think about. Some of them can be accounted for, but the rest not. All of them are interesting.

KUSUNDA: One language buried in the mass of Himalayan languages reported by Grierson in the famous *Linguistic Survey of India.* It is spoken by a very very small number of people in western Nepal and should be regarded as moribund. Being reported in the midst of a group of Tibetic languages, which as a lot have been grossly neglected, did not do Kusunda any good. Recently, new data have been collected, and the authors drew the same conclusion I drew years ago. Kusunda has no relatives, or, if it is remotely related to some group, that group is presently unknown. Ruhlen has classified Kusunda as Sino-Tibetan and in his second volume will present data on it. I think he is mistaken, but I have no idea what is related to this rapidly disappearing language. The whole matter ought to be treated as an urgent linguistic problem and a great deal more information obtained.

SHABO: Also called MEKEYIR. This case is very straight forward. We have a fair amount of data, a 200-300 item lexicon and some grammar, reported by Lionel Bender and Peter Unseth, from the field work of others. Bender thinks it is N-S, possibly a member of Surma or East Sudanic, and Unseth tends to agree with him. Ruhlen cites Shabo as a member of Surma, but solely on the advice of Bender. I think that Shabo has not yet been accounted for. Since the Shabo people are nomadic hunter-gatherers in the forests of extreme south-western Ethiopia, and since they live in the general territory of the Majangir, who are hunters and marginal farmers themselves, Shabo is a matter of great historical significance too. Not the least of the possibilities is that they might be connected with the inexplicable mystery of the pre-Bantu and pre-Mangbetu language of the African Pygmies. It is also not clear who lived in Ethiopia before AA came to dominate it so completely.

HATTIC: Also known as KHATTIC. Supposedly the underlying people and source of the name Hittite. They join Hurrian and Urartean in Diakonoff and Starostin's hypothesis as related to North Caucasic. However, they are not placed so explicitly in North Caucasic as Hurro-Urartean is; they may form a distinct group, possibly even on the Kabardian (Northwestern) side of North Caucasic.

URARTEAN: Named after URARTU, which in Armenian and scholarly opinion is the same as Ararat. No doubt the language of the people around Lake Van when the IE-speaking Armenians intruded into the area. As discussed above, Urartean joins Hurrian in North Caucasic, according to Diakonoff and Starostin. What is much harder to figure out is how Kartvelian came to sit in the sweetest valleys of the Caucasus, right in between both branches of North Caucasic and their kindred languages in Anatolia and northern Iraq. Did Kartvelian intrude or was it sitting there all the time before the others got there?

MINOAN: Also known as LINEAR A, that epigraphic language strategraphically below Linear B on Crete. Linear A has roughly the same problem as Harappan. Since Linear B has for some time now been known to be archaic or Mycenaean Greek, then it is obvious that the language underlying the Greek strata must be Minoan itself. That language is interesting in its own right because of the famous Minoan civilization, but it is also likely to give us an important clue to old Neolithic Anatolia. Archeologically, Minoan civilization is a Bronze Age climax of older Cretan culture(s), which was/were derived directly from the Neolithic of Anatolia. I would think it likely to be related to Hattic, its neighbor of old, and possibly to be a link to Etruscan or Basque. In this, it need not be assumed necessarily that any of these are related to an unknown or an undemonstrated Dene-Caucasic, of course. Nevertheless, all of these lie in the path of the Anatolian Neolithic. which did sweep across the Mediterranean to and through western Europe (Whitehouse 1977:88).

CYPRIOT: The pre-Greek epigraphic language of the island of Cyprus. It is supposed to be unreadable like Minoan and Harappan, but there exist theories that it is "really Semitic" or such like. I do not know if anything has been done with Cypriot lately. Geographically, Cypriot has an equal chance of being related to Minoan, Hattic, Semitic (e.g., Ugaritic of the nearby Levant), or Egyptian.

KASSITE: The hill men of Iran were always important in the affairs of civilized Mesopotamia. Not the least of them were the Kassites. It is said that a corpus of data exists but that no one can classify the language.

GUTIAN or QUTIAN: Another hill-men's language. Diakonoff says it too is related to East Caucasic.

UNCLASSIFIED: "An unclassified language is typically that of a recently discovered ethnic group. In such cases, little or nothing is known of the language of these people, or what is known has not yet come to the attention of someone who could classify it on this basis... This list of unclassified languages merely represents those that have come to my attention. There are still uncontacted tribes in South America, and peoples whose distinct languages have escaped notice elsewhere in the world, especially in Southeast Asia, New Guinea, and Africa." In South America, Ruhlen's list includes Carabayo, Guaviare, Yari, Mutus, Yuwana, Kohoroxitari, Arara, and Chiquitano. In New Guinea, it includes Warenhori, Taurap, Yuri, Busa, Nagatman, Porome, Pauwi, and Massep. It is important to reinforce this point. Thus, just in the past decade, the following have been discovered in Ethiopia, Somalia, Kenya, and Uganda: Mao languages (Sezo, Hozo, Madegi, Bambeshi-Diddesa, Ganza), Birelli, Shabo, Oropom, Omotik (a Nilotic language, not related to Omotic of Afrasian), Sogoo, Boni languages (other than the one already known), south Somali languages (barely known before, not just dialects of Somali). Most of these have been classified fairly easily, usually in Afrasian, but two (Shabo and Birelli) continue to resist our efforts, and one (Oropom) has been judged BOGUS by a leading East Africanist linguist (Bernd Heine). New African languages, finally classified as Khoisan, Niger-Kordofanian, or Nilo-Saharan in the usual case, continue to be recorded, while there still exist languages whose name and whereabouts are known but nothing recorded about them except some local opinion that they are related to some known group (e.g., Dorsha in southwestern Ethiopia) or just a bit is known, but it has led to a shaky, albeit probably accurate, classification (e.g., Guba and Ganza of Ethiopia-Sudan borderlands).

A final note on Unclassified languages. In the contemporary and most valuable Russian hypotheses about the small number of "macrofamilies" in the world, they list Khoisan, I-P, and Australian as possibly to be joined to the others or just as maximal groups themselves — for the moment. Yet, the two large African phyla, which I have argued are really super-phyla, have no places in the Russian scheme. They are completely neglected. Thus, in this peculiar way, N-K and N-S can also be added to the list of Unclassified languages.

PIDGINS and CREOLES: Ruhlen has an interesting section on those languages which have arisen from contact situations and begin as languages "belong nobody." While these are of interest to the followers of the discussions initiated by Derek Bickerton, they do not concern me here because they can be accounted for readily enough and do not help us reach back into the remote past. Hypotheses do arise from time to time about various languages being ancient creoles, like Germanic being an IE creole in western Europe or Omotic being an AA creole in Ethiopia. But these hypotheses seem never to prove themselves or to gain strong enough adherents to be felt seriously in matters of classification, even though it is likely in theory that such could happen.

REMAINING ISSUES: METHODOLOGICAL

Much time is spent fighting methodological battles in Ruhlen's book. It is a necessary chore for anyone in the social sciences and especially in historical linguistics. Given the persistent claim that reconstruction via the comparative method is the only reliable procedure, Ruhlen could hardly do otherwise. A preoccupation with methodology is not necessarily required of all historical science, of course. All sciences have methods, but many sciences have not freaked out on methods and techniques as much as linguistics has during some of its periods. No doubt sociology and psychology were smitten by the operationist bug as well, but the same is not true of geology, astronomy, or evolutionary biology. Even linguistics threw off most of its methodological hang-ups during the dramatic expansion of the Chomskyan paradigm and afterwards. Then theory became central, not methods.

Why is reconstruction so important? Let us grant that few phyla have been established by scholars preceding step by step up the reconstruction ladder. This does not involve theory or method; it is an empirical and historical question. The people who insist that we do not know anything before reconstruction is done, or that nothing is true without reconstruction, have assertion as their only argument. We of the mid-20th century, the heyday of hyper-methodology, actually inherited most of our phyletic map of the world from our "sloppy" predecessors of the 19th and early 20th centuries. Most of that phyletic map has been retained, i.e., it has been tested and largely found true. Much of what was rejected stemmed from the gross misuse of typology, especially in Africa and Southeast Asia (e.g., Sudanic, the many half Hamitics, the Thai and Vietnamese problems). Few of the major genetic groups established by the traditional comparison of grammatical and lexical morphemes have been overturned. Some of them have been gathered together in larger groups which I call phyla.

In the Old World, one can count thirteen phyla which are still standing (IE, Uralic, Altaic, Eskimo-Aleut, North Caucasic, Kartvelian, Dravidian, Austroasiatic, Sino-Tibetan, Austronesian, Australian, AA, and Khoisan), although Altaic and Khoisan are disputed. Three of the four new phyla established in the Old World since World War II, namely, N-K, N-S, I-P, and Austric, were set up by Greenberg, whose use of traditional 19th century methods is labeled "mass comparison" and criticized as a departure from the tried and true methods of our predecessors. (This is actually amusing.) Although Austric was first set up by traditional methods (Pater Schmidt), it, as a bold venture, had been mostly rejected before Paul Benedict revitalized it and then partly rejected it again. The use of reconstruction in Austric and Austro-Thai HAS been salient. Also Thai-Kadai or Daic owes its existence partly to reconstruction, although the flood of new data on non-Thai languages helped too. The use of Proto-Chinese is a considerable help in Sino-Tibetan. Perhaps the case where reconstruction helps most of all, however, is the western half of North Caucasic (Kabardian, et al), where phonological problems are extreme.

In the world's most extreme phonological case, Khoisan, we may see the opposite effect. Classification and sub-classification may proceed and have proceeded on the traditional bases of "matching" of morphemes, mostly lexical. Although Khoisan has abundant morphology (e.g., 21 pronoun distinctions in some SAK languages), a lack of published grammars on crucial languages (e.g., Hadza) and insufficiency in others (e.g., Sandawe) frustrates what will probably turn out to be a very convincing morphological argument. Yet the extreme number of PHONEMES in some SAK languages makes reconstruction quite a dubious undertaking. Some very competent people (e.g., Tony Traill, Derek Elderkin, Rainer Vossen, Christopher Ehret) are now struggling with these Khoisan problems, and they may have great success, but the phylum does not stand or fall because of the reconstruction. The reason is that so many of the etymologies which support the Khoisan hypothesis are not involved in the severe phonological (mostly click releases) problems. This is absolutely true of the potentially powerful morphological etymologies.

Despite the flamboyant claims by some Americanists about what can be done, what must be done, and what they will have to disallow, they can truly point to few scientific accomplishments. They have destroyed hypotheses, more by insistence than by demonstration, but they have built almost nothing. They have established an extraordinary atmosphere of timidity, oddly combined with aggressive skepticism. It is not that there are no viable hypotheses that can be advanced for genetic relationships among languages in the Americas. Rather, it is the case that nowadays scholars are AFRAID to venture hypotheses because they do not wish to be attacked. Such an atmosphere smacks more of metaphysical puritanism than it does of healthy and exuberant historical science!

Yet, I have to disagree with Ruhlen and Greenberg on reconstruction. It does not find its value in the early stages of inquiry when setting up the whole structure of the house, so to speak, is more important than finishing off one room. But it does have value, and great value, in later stages of the inquiry when one has elaborated a whole structure and would like to test it. That is accomplished by the discovery and control of borrowings. And that is aided by sound laws. All historical linguists have presumably been trained in good IE methods of reconstruction or have figured out for themselves how it was done. That is useful training, if it is governed by a reasonable strategy. As we have seen, reconstruction can be difficult and may indeed require the work of several generations of scholars. One does not reject a hypothesis just because it is difficult to test. If that were the case, Einsteinian physics would have been thrown out by 1930. (Theories which are impossible to test do get thrown out of science, e.g., divine creation of Man.)

Calvert Watkins has argued that etymologies can be seen as having levels of credibility or truthfulness. Preliminary etymologies are the heart and soul of phylum building or setting up genetic hypotheses to begin with. At that point, he argues, it is wise to be tolerant of them. Some of them surely will be clinkers, but some may be true. Since the preliminary etymologies are embedded in a hypothesis, they do not have to be proven by sound laws or the like in order to be there. Like any scientific hypothesis, they do not have to be known to be true BEFORE they occur. Testing, or the determination of their truth value, begins AFTER they occur. Other levels of etymological credibility occur before some etymologies are "proven," i.e., tested and found innocent of being borrowed or an accidental similarity. At those other levels, any given etymology may be falsified, by being shown to be a borrowing or something else. If all the preliminary etymologies embedded in a genetic hypothesis are falsified at those secondary levels, then the genetic hypothesis itself is probably false. That is the history of the "Thai is a Sinitic language" hypothesis. If some, even many, etymologies can be falsified but some others withstand testing, then the genetic hypothesis is probably not false. This seems to be the case of the "Haida is a Na-Dene language" hypothesis. (I have elaborated on Watkin's ideas, which were presented at Stanford. He may not agree with everything, or indeed anything, said above.)

Many genetic hypotheses involve few or no etymologies at all. For example, consider these from Africa: (A) Ari (Omotic) is either a N-S or a Sudanic language because the Ari people are Negroid; (B) Hottentot is a Hamitic language because it has grammatical gender; (C) Moru and Madi are Sudanic languages because they are found in central Africa; (D) Peul/Fula is a Hamitic language because the Fulani are cattle-herders and fervent Moslems and tall and Caucasoid. Testing of these hypotheses demands first that the pitiful handful of preliminary etymologies be examined; type (C) had none at all. Secondly, the search for more etymologies may reveal nothing, i.e., there is no basis in etymologies for proposing the hypothesis, and so it is probably false. Lack of credible etymologies is bad news for a genetic hypothesis.

Nevertheless, the converse is doubtful or has become doubtful recently. If two or more languages are linked by some etymologies, the genetic hypothesis may be rejected BEFORE the credibility of the etymologies has been determined. How can that be? One reason is that scholars just do not want to believe the hypotheses. A second reason is that the geography of the hypothesis is incredible. Who would believe that Hottentot was related to Arawakan? Even if 100 ostensibly good etymologies were presented? A third and more significant reason would be that the NUMBER of etymologies is too small, i.e., this many ostensible etymologies can occur by accident. In fact, these are usually called "chance resemblances." A fourth reason is a further development of the logic of the third. It holds that anyone can pile up ostensible etymologies between two languages, or more if there are more languages, just because of chance resemblances. Or, to put it more pictorially, as one Americanist has, "Anyone can throw a bunch of mud at a barn, and some of it is bound to stick (to the barn)." As this logic unfolds, it leads to breath-taking conclusions. Since anyone can pile up etymologies, then any pile of ostensible (or purported) etymologies proves nothing, i.e., the implied genetic hypothesis will turn out to be false anyway, so that there is no point in having piles of etymologies, and, therefore, any argument in favor of a genetic hypothesis is false or unworthy if it involves someone finding a pile of alleged (ostensible) cognates (etymologies). Phew! They have not yet drawn the full logical conclusion to this point — none of the established phyla have any validity because they were once piles of alleged cognates, they were "faux pas" at the outset.

This kind of logic is already circulating; one can hear it at conferences, and it tends to stun proposers of new genetic hypotheses — and their audiences. Yet, it is basically ridiculous. Oddly enough, it is based in large part on one of Greenberg's old arguments, although the major targets of this logic nowadays are Greenbergian proposals. Before examining this problem, one should say that it disappears at once as a problem if one adopts the views of Calvert Watkins. All preliminary sets of etymologies are tolerated, indeed treated with respect, because some of them lead to truth, and it cannot be predicted beforehand which one will be false.

How many good etymologies must there be for two languages to be said to be related? Repeat the question for ten languages. And again for 1,103 languages. How does one begin to answer these questions? One obvious tack is to determine how many etymologies can be found between two languages that are NOT related. That is what Greenberg tried to do in 1953. He assumed that Thai and Jur were not related to each other and counted about 7% of the lexicon as similar when he compared them. Let us re-state that as: in any 100 words or morphemes of ordinary Thai vocabulary, one can find 7 that resemble their counterparts in Jur. Or 7 ostensible etymologies per 100 comparisons. Even this much is wobbly. Was he proposing 7 with the same meaning, as would be required by lexicostatistics? Or did he mean 7 cognate-type items, like English *fowl* and German *Vogel*? It makes quite a difference, but it is very doubtful that a cognate searcher would ever abide by the same-meaning rule. But, if we let someone start with a Swadesh-type item like German *Rauch* and poke around in an English dictionary until he finds *reek*, then we have lost all statistical control over the question. Not only will the database of the language make a difference, because a word like *reek* might not show up in a small lexicon, but also things become hard to calculate. "What are the chances that the word for 'bird' in X will begin with a labial consonant? And what are the chances that that will happen in Y too?" is a question different from "What are the chances that a word semantically similar to 'bird' in X...?" In one case, we assume that there is one word for "bird;" in the second, the number is unknown.

Greenberg's efforts to settle this question led him to seek empirical answers, and eventually he had a range from 7% to nearly 20% for "unrelated" languages. At the higher level, he was finding numbers that exceed those between languages known already to be related (through the transitive or chain principle), like between Cushitic and Semitic languages. The original 7% figure itself already exceeded known percentages found between distantly related N-C, N-S, and AA languages, if one followed lexicostatistical rules. The whole endeavor appeared to be fruitless. However, no distinction was ever drawn between similarities and good etymologies, even though the difference was seen as important to glottochronology. The lexicostatistical work on African languages relied to the extent it could on good etymologies, i.e., many similarities got thrown out in the process, but Greenberg's control studies relied on similarities. So there never was an answer empirically to our question of how many GOOD etymologies are needed. And the control studies were not satisfactory for yet another reason.

How do we know that Thai and Jur are not related? Or to review Dyen's famous spoof of IE versus Austronesian, or Campbell's of Finnish and the Penutian group, what would we say if we believed that Finnish was ultimately related to Mayan, or English to Samoan? Dyen's whole point was based on the TACIT belief that his audience would think it ridiculous for IE to be related to Austronesian — presumably for reasons of geographical distance. But, if all human languages are ultimately related, any comparison of two human languages would be expected to produce some similarities, and a comparison that involved 1,103 languages, like IE versus Austronesian, should produce many similarities. Of course, Dyen did not compare 1,103 languages. Rather, he compared two ancestors — as retrieved by reconstructors — of 1,103 languages. Those two existed 5,000-6,000 years ago, if we take the IE dates. (PAN dates are very controversial.) English compared with Samoan would produce many fewer matches than PIE versus PAN, I would wager. But this should not be the case if neither of them is related. There is a general expectation that the older the pair compared, the closer they seem to be because they are much closer to the ancestor or they still retained what they would later lose.

In sum then, I think our notions of how many good etymologies, or even how many similarities, are needed to show genetic relationship are totally unfounded. The African calculations mentioned above suggest that, as between remotely related languages, the numbers of good etymologies get very low, even in cognate hunter terms, when just two languages are compared. Furthermore, it is quite possible that we will have to re-think entirely the question of how many similarities two or more unrelated, really unrelated, languages might have. The assumption that Finnish and Penutian are unrelated is the one which has not been examined. It is in fact a hypothesis itself.

Three genetic hypotheses were not mentioned in the above discussions. One states that all human languages have a common ancestor or all human languages have been carrying bits and pieces of one common ancestor, along with later innovations, since the time of first language. Call this MONOGENESIS. A second states that human languages have several or many ancestors. This is usually called POLYGENESIS. Further research must determine which particular languages share which ancestors, of course, but in any comparison of two or a thousand languages, it must be assumed that they may be related, or it cannot be assumed that they are unrelated. Some of them are (related or unrelated), but we do not know which ones — yet. A third hypothesis states that, while the basic design features and preconditions of language exist in our genes and/or brains, language is created anew whenever it is needed. Therefore, we do not have to assume that languages are related. Call this the LAMARCKIAN hypothesis. It is very unlikely that the Lamarckian theory has any supporters, but it is the only one which permits us to make the assumptions Greenberg made to begin his control studies. Since it is widely known that most linguists believe in either monogenesis or polygenesis, and the Lamarckian notion is contrary to everything historical linguistics has discovered in the past two centuries, it is truly remarkable that unrelatedness is so often taken for granted.

REMAINING ISSUES: BINARISM

Ruhlen devotes much time passim to criticisms of "binarism" or "binaristic approaches." They have been alluded to above to some extent. Basically, his objections are well founded. If someone compares two languages, s/he is likely to produce fewer similarities than if s/he compared ten. Ruhlen also tries the same idea at the super-phylum level, i.e., if one hears that Nostratic has been proposed (for example), one may compare two of the families in it against each other. Such can be done, but it is not a fair test of the larger group, which may include five or more phyla. Some of the etymologies of the proposed Amerind super-phylum may bind Algonkian to Mayan, for example, while others may bind Mayan to Andean, and still others bind Algonkian to Zuni, etc. Someone with only binaristic vision might argue that s/he was unable to classify Mayan because sometimes it seemed related to Algonkian, yet sometimes it seemed related to Andean; and anyway, it is easy to get together a pile of etymologies, especially when one compares American Indian languages with each other. Ruhlen would argue that the trouble with binarism was well exemplified in the collective myopia of Americanists.

In a more formal and statistical vein, Greenberg has produced an excellent discussion of the advantages of using more languages in comparisons. In Appendix A of *Language in the Americas* (pp. 341-44), there is a brief presentation of the amount of retention of ancestral vocabulary ("recoverable vocabulary") and its likely age to be expected when one uses various numbers of languages. The argument is not polemical, not addressed to binarism, and it is very encouraging because it suggests why giant phyla like I-P, N-K, and Austro-Thai are so valuable — morphemes of great time depth have a good chance of being preserved. A few calculations will suffice here. Suppose we compare two languages: then, after 10,000 years, we can expect to recover only 5.1% of (basic) vocabulary and, at 20,000 years, only 1%. But 20 languages, if compared, will produce 61.8% after 10,000 years and still have 22% after 20,000 years. This depends on using the "Joos function" or the "dregs effect," instead of a "homogeneous replacement rate," as used in early glottochronology. If one did not make Joos corrections, then one could calculate that two languages would retain ZERO at 20,000 years, while, from 20 languages, only 2.2% would be recovered after 20,000 years. Since most phyla have more than 20 languages, it is easy to see how hopeful the situation is for Amerind with its 583 languages. For just the same reason, it is obvious why Basque and Sumerian have been resistant to taxonomic efforts.

There is reason to believe that the situation with two languages is a bit more hopeful than that. If one applies the glottochronological formula invented by Kruskal, Dyen, and Black (1973), which has no homogeneous replacement rate and out-Jooses Joos, then one could expect 10% recovery for two languages at around 10,000 years and about 2% at 20,000 years.

HIGHER LEVEL GROUPS OR SUPER-PHYLA

Morris Swadesh had the bad luck to die before he finished setting up the structure in detail of the human language network and in cranking out the numerous etymologies to go along with it. He ended up with 11 phyla for the globe, or 12 if you count Sumerian, but with the notion that they were all linked to each other. It was Monogenesis with 12 secondary nodes. Disregarding for now the question of monogenesis, we can ask how many nodes we today think there are underlying the phyla proposed by Ruhlen. Or how many super-phyla are there, if any, into which we can put Ruhlen's phyla?

A group of Russian linguists has renewed work on Nostratic, which, as we have seen, is very close to Greenberg's Eurasiatic, except that theirs includes Kartvelian, AA, and Elamo-Dravidian. They too propose Amerind, but they also put Eskimo-Aleut in with Nostratic. Na-Dene is put with Sino-Tibetan, Yeniseian, North Caucasic, Hurrian, Urartean, Hattic, Basque, and probably Etruscan in a super-phylum called Dene-Caucasic. Beyond that, which would be quite a lot, there are no signs of any hypothesis on the Russian side. Greenberg has suggested Dravidian and N-S, as mentioned above. In fact, the old tropical world stands about the way it did when Swadesh died. Australian, I-P, Austric, N-S, N-K, and Khoisan are rarely joined to others and indeed have some trouble being accepted themselves, although Swadesh had linked I-P to Austric. Burushaski, Nahali, and Sumerian are still isolated. But the major questions before the house of historical linguistics right now are surely whether Amerind, Nostratic, and Dene-Caucasic will survive testing. I think they will, if it depends on piling up etymologies rather than metaphysics.

ADDENDA (1987)

Since this review was written, a number of additional points have come up. Two were transmitted to me by colleagues. A third is a speculation which has been partly published as a genetic hypothesis elsewhere.

SUMERIAN: Claude Boisson recently sent me a manuscript whose locus of publication is presently uncertain. In it, he reviews the hypotheses concerning Sumerian origins. There are four items of interest to us here. First, the large amount of unproductive speculation about the genetic affiliations of Sumerian have left a literature which is in itself a detriment to progress. Secondly, after mentioning Greenberg's informal idea that Dravidian may relate to N-S (which we discussed above), he also mentions that Ivanov, Diakonoff, and Starostin have proposed that Sumerian belongs in Dene-Caucasic (Blažek 1987). This is a formidable troika of proponents, perhaps most of all because of Diakonoff's long-term work in Near Eastern prehistory. Thirdly, Boisson himself has very carefully compiled a set of correspondences linking Sumerian to Dravidian. The quality of his work is superb, most of the etymologies are convincing, and it seems likely to me that future testing of Boisson's set will strengthen it. Nevertheless, given the considerable irritation of Sumerologists and Dravidianists fed up with speculations, Boisson takes pains to stipulate that he is not proposing a genetic link as such but only a working hypothesis!

Fourthly, a re-check of Zvelebil shows that each of the above current hypotheses about Dravidian has an older binaristic counterpart. Thus, relations with Korean (Altaic and Mitian) have been proposed by H. B. Hulbert (n.d.), Ch. Dallet (1874), and A. Eckardt (1966). A linkage with Mitanni (Hurrian or Dene-Caucasic) was proposed by C. W. Brown (1930). Nor is the African connection a new idea either. Zvelebil lists J. Mayer (1924), E. H. Tuttle (1932), and four publications by L. Homburger. Tuttle specifically links Nubian, hence N-S, while Homburger seems to prefer N-K, i.e., Peul (West Atlantic) and Mande. The reader is referred to Zvelebil 1970:21 for references. I have not read them. Mme. Homburger also proposed an Afrasian-Dravidian linkage, as have others, of course, as part of Nostratic.

Does a possible membership of Dravidian in Nostratic preclude its being related to Sumerian, which may be a member of Dene-Caucasic? And how could Dravidian also be related to N-S? Surely someone is mistaken here. If D is related to S and M and N, and S is related to D and C, but C is NOT related to M or N or D, then by the transitivity principle, we are in a logical impasse. Perhaps all of these ARE related to each other. Or perhaps there is an error in here somewhere. Not necessarily such an error as mistaking bad etymologies for good but such an error as finding a higher level linkage before a lower level one. All these problems are raised by Boisson's marvelous paper.

NIGER-KORDOFANIAN: Roger Blench was kind enough to give me a preview of some thoroughly up-to-date sub-classification within N-C. With the understanding that the main frame of Bennett and Sterk's sub-grouping is still intact, Blench pointed out that Ijoid-Defaka has been raised in status to being a full coordinate half of N-C. The major coordinates now are: (I) Kordofanian versus (II) Atlantic-Congo, within which we find (IIa) Mande versus (IIb) Niger-Congo. With that are (A) West Atlantic and (B) Niger-Congo plus Ijoid-Defaka or (B.1) Niger-Congo proper versus (B.2) Ijoid-Defaka. Within N-C proper, there are four branches, to wit, Kru, Kwa (which has been re-defined), Benue-Congo, and Gur plus Adamawa-Ubangian. The Bantu belong to Benue-Congo, which has been much revised internally. The reader is advised to be on the lookout for John Bendor-Samuel's forthcoming book on N-C, which should include the recent revisions. He is the editor.

SPECULATION: Within a decade, someone will formally propose a higher-level mega-super-phylum which will include both Mitian and Dene-Caucasic plus some other phyla — probably AA, Kartvelian, and Dravidian. We have already seen three cases where classifiers have put one language into both camps or have related to each other languages which are said to belong to the different camps. Etymologies common to North Caucasic and Kartvelian could be connections of Mitian and Dene-Caucasic. Etruscan has been seen as either Mitian or Dene-Caucasic. The confusion over Sumerian and Dravidian relates to the same thing, because Dravidian is most typically related to Nostratic (Mitian), especially Uralic, as we have seen. Swadesh's Vasco-Dene supports the general conclusion and could indeed be a model for it, except that Swadesh did NOT include either AA or IE in his Vasco-Dene. His exclusion of IE does not make any sense in terms of the common Nostratic proposals and probably resulted from his setting up the equivalent of Shevoroshkin's Dene-Caucasic plus Basque and then adding most of the Nostratic membership to it, starting on the east.

Beyond that, of course, lie the possibilities that MOST of the world's phyla will be connected genetically to each other, not as they are now by general presumption but rather in explicit etymologies. Some scholars, not the least of whom are Ruhlen himself and Greenberg, have already produced "global" etymologies. Mark Kaiser (Illinois), John Bengtson (Minnesota), Vitaly Shevoroshkin (Michigan), and F. Seto (Tokyo) are perhaps the leaders in these efforts. These phenomena have hardly been studied since Trombetti's day, and most linguists dismiss them casually, seemingly without reflection on what they mean. Some global etymologies HAVE been explained away, e.g., common human BABA, MAMA, and DADA in kinship terms are arguably the products of human infants trying out their phonetic inventories, and these three are the easy ones. But other global etymologies cannot be explained away as baby speech or as onomatopoeia, certainly not as borrowings, and they cannot be derived from universal linguistic processes or general theory. They are a PROBLEM!

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PAMA-NYUNGAN II AND TASMANIAN

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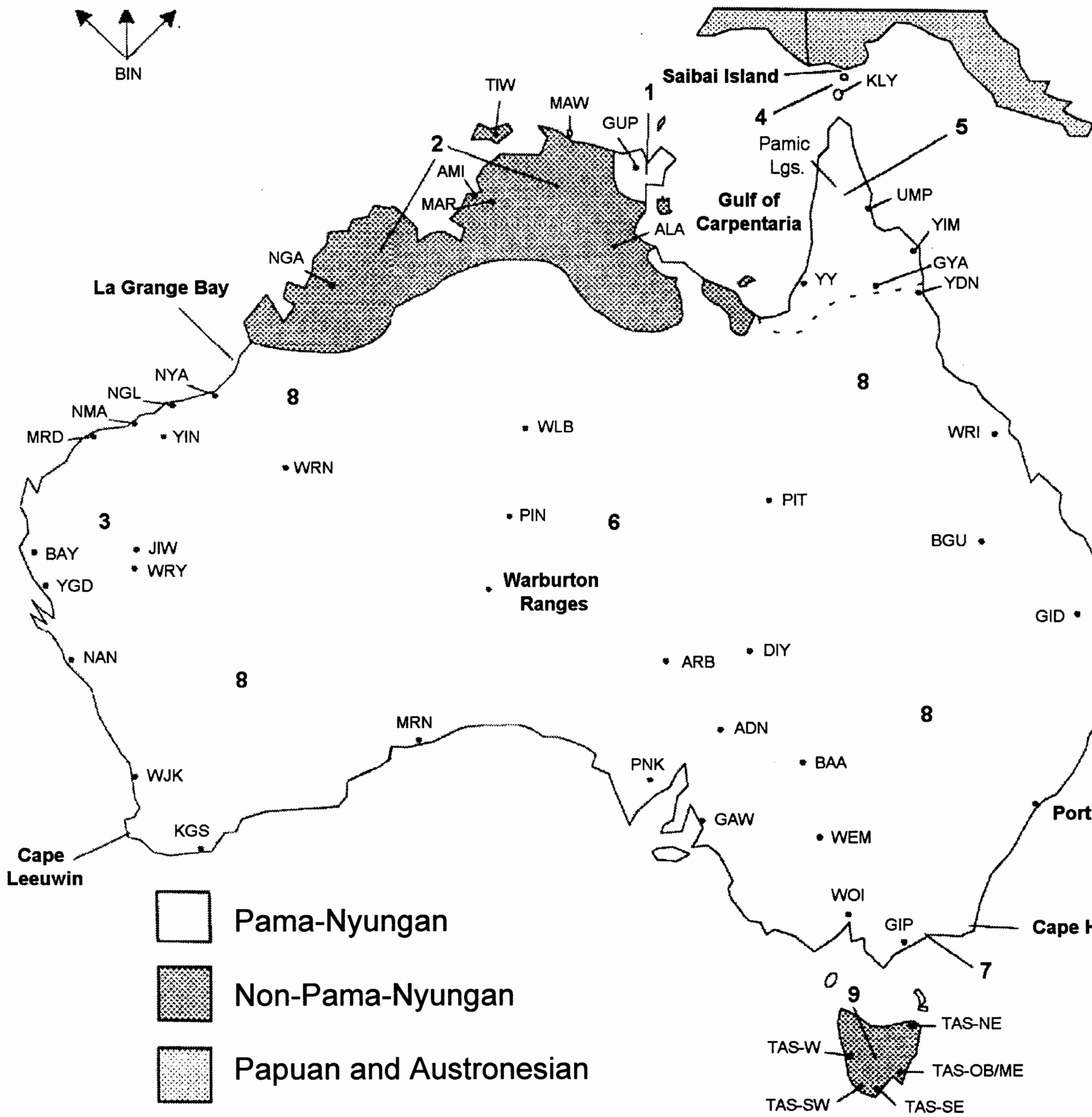
In the Spring 1993 issue of *Mother Tongue*, O'Grady presented a summary of comparative work on Australian languages up to 1964, and a data set of 114 items which is intended to demonstrate several aspects of the genetic relationships existing among the Australian, and in particular, the Pama-Nyungan languages. In this paper, we provide a key for that data, along with discussion of the proposed cognate sets, which illustrate some of the important issues in Australian comparative linguistics. In addition, we present evidence for the genetic relatedness of Tasmanian with Australian in the form of 29 putative cognate sets. These suggest that further work on this question is called for, especially in view of Greenberg's claim of 1971 that Tasmanian belongs in his Indo-Pacific grouping. A survey of work done since 1964 is in preparation for possible inclusion in a future issue of *MT*, unless some other Australianist wishes to do this.

The map below (p. 31) includes the approximate locations of the languages referred to in both papers, as well as relevant geographical information. The numbers indicate the areas referred to in the previous paper.

The 114 forms cited in the Spring 1993 issue of *MT* resolve themselves into 34 reconstructed roots.[2] Of these, eighteen are relatively straightforward from, say, an Indo-Europeanist's point of view.

A. CLEAR CASES OF COGNATION

1. Proto-Nyungo-Yuulngic (PNYY) **jirri(l)* 'frightened' (4, 14, 15; **-i* > GUP Ø; for **j* > *th* in GUP (and other languages), see Dixon (1970); **rr* > NYA *t*).
2. Proto-Nyungic (PNY) **kapi* 'water' (6,17).
3. PNY **kurra* 'short' (7, 8, 27, 29; stem accretion in WJK, GUP, MRN; vocalic apocope in WJK; **rr* > NYA *t*).
4. Proto-Pama-Nyungan (PPN) **jalany* 'tongue' (9, 10, 93; final consonant masking with *-pa* extension in WLB (diachronically) and PIN (synchronically); for **j* > BGU *th*, see above).
5. PPN **jinang* 'foot' (12, 13, 96; **-ng* > PIN, WRY Ø).
6. PPN **kupa* 'stooped' (24, 25; stem accretion in NGL; vowel assimilation in BGU — where *thana-* is 'stand').
7. PPN **kurun* 'eye' (28, 30; **-n* > PIN Ø; **r* > UMP ' (glottal stop [IPA ʔ])).
8. Proto-Eastern Oceanic (PEO, a subgroup of Austronesian) **malu* 'shade' (see 33) borrowed and reanalyzed in PPN as **malung* (34-35; **-ng* > NAN Ø).
9. PNYY **mangu* 'face, eye' (36, 38-39; stem accretion in GUP).
10. PPN **mara* 'hand' (40, 44; **r* > UMP '. Given evidence from additional Pama-Nyungan languages, this root is reconstructable as **marang*).



Map 1: Approximate Locations of Languages

11. PNY **mina* 'face, eye' (45, 50, 56; stem accretion in ADN, WJK).
12. PPN **mina* 'true, right, good' (47-48, 51-52 ('the true, correct, straight language'), 54; stem accretion in KGS twice over; vowel assimilation in Proto-Pamic (PP)).
13. Proto-Desert Nyungic (PDN) **ngaju* 'I' (62).
14. PPN **ngapu* 'water' (64-65; **u* > NYA, DIY *a*).
15. Proto-Austronesian (PAN) **payung* 'shelter, protection, shade, cover' (see 83), borrowed as PPN **payung* (82, 84; *-*ng* > BAY *Ø*; **u* > BAY *a*, GUP *uu*).
16. PPN **pinang* 'ear' (86-88; *-*ng* > YY, PIN*Ø*; **a* > YY *Ø*).
17. PPN **taru* 'ankle' (92, 95; **u* > PIN *i* (sporadic innovation); **t*- > UMP *th*; **r* > UMP ').
18. PNY **wirri* 'play' (102-103; **rr* > NYA *t*).

B. ANTONYMS AS COGNATES

The existence of a secret "Upside Down Language" (*Jiliwirri*) among the Warlpiri people of northwestern Central Australia was documented in Hale (1971). It turns out that this tradition of antonymy has strong diachronic secular echoes as well.

A goodly number of additions to the cognate sets given above now come into focus. No attempt is made to reconstruct the referent ranges of the four new protoforms offered (except for 19, below). Work on this problem is still ongoing.

19. PP **Taja* (1, 91; initial dropping in UMP; stem accretion in GYA. Given *all* available Pama-Nyungan evidence, this root is reconstructable as PPN **raja* 'shadow').
20. PNY **jama* (5, 94; vocalic apocope in WJK).
21. Add #26 to set 3, and update reconstruction to PPN

*kUrran (with *U, since YDN is not diagnostic for PPN vowel length).

22. PNY **kartu* (19, 20).
23. PPN **tuungku* (100, 101).

C. 'EAR' AND 'GROUND': ANTONYMS TOO?

An overwhelming array of evidence attests to a semantic relationship between 'ear' and 'ground' in Pama-Nyungan. In the case of WJK, this has clearly been effected through a reciprocal trade-off in the semantic roles of ancestral **pUju* and **jungka*: **pUju*, an old term for 'ear,' has come to mean 'ground' in this language, while the modern reflex of **jungka* 'ground' has come to mean 'ear.'

Since 'ear' and 'ground' both appear in the Swadesh 100-item lexicostatistic test list, such a semantic switching of roles has the immediate effect of lowering the percentage of cognates shared by WJK and other *Pama-Nyungan languages* by two percent at one fell swoop. Such an innovation, multiplied several times over, could bring about a drastic lowering of cognate percentages between languages which are, in fact, quite closely related. Certainly the minuscule cognate percentage of eight percent shared by the two Pama-Nyungan languages WJK (in the extreme southwest of the continent) and UMP (in the extreme northeast) belies the strong *grammatical* evidence for genetic relationship between these two languages!

The 'ear' : 'ground' connection in Pama-Nyungan, as evidenced in the comparative data, is seen, then, in the following:

24. PNYY **pUju* 'ear' (2, 3; stem accretion in WJK, GUP).
25. PNY **jungka* 'ground, dirt' (16, 97, 99; stem accretion in BAY).
26. PDN **langa* (31, 32).
27. PNYY **mu(r)na* (57, 58; stem accretion in GUP, WRN).
28. PPN **yampa* (104, 105; arguably also 106-108; stem accretion in NYA, WLB; **m* > YY *Ø*; *-*a* > YY *Ø*).

Any *one* of the above reconstructions, considered alone, would no doubt elicit total skepticism from any reader of *MT*. That *five* roots should all independently point to an 'ear' : 'ground' connection puts us into an entirely different ballgame, however. And PDN **langa*, moreover, is phonologically highly marked: initial **l* is a relatively *rara avis*, as is intervocalic **ng*, so that the likelihood of two **langa* roots coexisting as homophones is extremely remote.

Even though the case for an 'ear' : 'ground' semantic link would seem unassailable on the above evidence, there are linguists — Nick Evans and David Wilkins, at least — who insist that one uncover evidence of polysemy within a given language before they will accept such a connection. (We would submit, however, that the chance that *each* of the sets 24-28 involve *two* distinct etyma must be counted as being astronomically small.) Evidence of polysemy involving 'ear' and 'site, place..., country...' (cf. 'ground') does, in fact, appear in YY (see item #86 in the database), so that the connection which we established on the basis of comparative evidence is indeed vindicated.

Our own feeling, then, is that the comparative evidence can be so compelling that it alone can engender 99 percent or more confidence in a semantic connection. Such is the case with 'egg,' 'brain,' and 'water' (O'Grady 1990), where the evidence of *nine* separate roots is used to show conclusively, we feel, that these three concepts are indeed linked in the minds of speakers of Pama-Nyungan languages.

Thus, we are able to extract with full confidence several more cognates from the database:

29. Add #18 to set 2.
30. Add #63, 66, and 76-78 to set 14; prenasalization (sporadic) in PIN, PIT; stem accretion in PKA; **ng* assimilates to **p* in PIT, YGD, ARB; **u* > YGD *a*.
31. PPN **nguku* (69-70; stem accretion followed by final consonant masking in WLB). Item #22, from TIW, propels us out of the Pama-Nyungan arena altogether, and into the rarefied atmosphere of Proto-Australian (PA). TIW *kukuni* (#21) (the *-ni* being a masculine suffix, with *ng-* putatively assimilated to *-*k*-) may be one of the very few lexemes in that exceptionally divergent tongue which has cognates in *any other* Australian language. Another may be item #109 in our database, where the *-tarla* portion of *yimitarla* titillates us with the *possibility* that it may represent a stage of almost unimaginable antiquity (15,000 years?) in the evolution of Australian languages, when 'tongue' had an initial apical **t* not yet shifted to mainland non-Pama-Nyungan laminal **j*. A later shift was to merge non-Pama-Nyungan initial **t* with **j* in PPN (Evans 1988), which in turn was host to yet another such shift (Hendrie 1990) in a number of its daughter languages (shades of the three successive Slavic palatalizations!).
32. PNYY **nurrku* (73-74)

D. POTENTIAL : ACTUAL

One of us (GNOG) published in *Current Anthropology* in 1960 a squib on an all-pervasive Australian semantic phenomenon, whereby a notion such as 'animal' is referenced by the same root as 'meat' (an animal is, after all, potentially meat). Similarly, 'firewood' and 'fire,' 'hit' and 'kill,' and 'seek' and 'find' each are typically represented by a single root in the lexicon of an Australian language.

To set 6, therefore, we can confidently add #23, since a 'stooped' person or animal, seen from a distance, can be thought of as POTENTIALLY 'drinking.' This brings us in turn to #71, which shows complete semantic agreement with #23 but an unexpected initial *ng* for the expected *k*. Abandon ship? Indeed not. We choose to set up a category of Residue for such cases, involving what we take to be sporadic nasalization of initial (and occasionally intervocalic) stops. Conceivably, this **k*- > *ng*- shift came about at the interface between child language and adult usage in WOI. In the same sense, Lockwood (1969) refuses to regard Lithuanian *lìzdas* 'nest' as being unrelated to English *nest* (< PIE **ni-sd-os*), preferring to "assume an exceptional shift of **n* to *l*, for some reason unknown."

We'd very much appreciate input from Robert Blust and other Austronesianists on their approach to such problems, as well as sporadic prenasalization, in that language family.

E. OTHER SEMANTIC CHANGE

The database attests also to the following:

'flame' > 'eye':

33. PPN **tili* (11, 98; **t-* > YDN *j*).

'eye' > 'jealous':

34. Add #55 (with *-B* suffix) to set 11.
35. Add #111 (with *-mpaya* suffix[es]) to set 7.

'tru(ly)' > 'emphatic' suffix:

36. Add #112 (with **a* > *Ø*) to set 12.

'ear' > 'leaf':

37. PPN **jAlpa* (110, 113-114, with putative and problematic shifts of *-*a* to *i* and **j* to *ny* in PIN and shift of both vowels to *I* in WJK). For semantic confirmation, see #108 in set 28.

'eye' > 'good':

38. Combine sets 11 and 12 under PPN **mina.*
39. Add #37 to set 9.
40. Note also BAY *kurunyarru* 'good,' which, although not included in the database, can be added to set 7.

The unexpected appearance of a nasal for an initial stop in #71 (set 6) and #113 (set 37) has further echoes in the database:

41. PNY **piki* 'moon' (46, 85).
42. PNY **purtu* 'in vain' (59, 90).
43. Add #60 to set 15.

F. CREATING BRAND NEW PRONOUNS

Death in Aboriginal Australia leads to the tabooing of the name of the deceased and of any word of similar phonetic makeup, at least for a period. What if the deceased's name sounded like a pronoun? That was exactly the situation at Warburton Ranges in the Western Desert in the 1950s, when *ngayu* 'I' (cf. NYA, WLB *ngaju* at #62) was proscribed because a man named *Ngayunya* had died. The replacement, *nganku*, was itself later proscribed because of a further death, and was replaced by *mi*, a borrowing from English *me* (Dixon 1980).

This is what happened also in the not-too-distant past in MRD on the northwest coast of the continent — a language surrounded on three sides by neighbors having *nyinta* for 'thou.' MRD's *kartu* (item #21, set 22) is manifestly cognate with GAW *KARTO* 'wife' and NMA *kartu* 'man, male...,' which clearly show the manner of genesis of a new second person singular pronoun in MRD.

Again, in WRN, spoken well to the east of MRD, where we would expect **ngayu* for 'I' because of WRN's very close affinity with Western Desert dialects, we get instead *parra* 'I' and *parra-ngku* 'thou' (*-ngku* 'thy') (#79-80). The root common to these two forms is cognate with GIP *prra* 'man, person' (#89) and with KLY *PARU* 'forehead, face; front' (#81). So in WRN we have putatively 'FACE' > 'PERSON' > 'MY PERSON' > 'I'. It is clear that this is the direction of the shift, not 'I' > 'PERSON' > 'FACE', since the root *ngayu* (*ngaju* in #62, cf. TIW *ngia* at #67) must be of truly immense antiquity in Australia.

In the case of WOI, we take the initial *m* in *marram* 'body' (#41) to have replaced an earlier **p* (cf. WOI *ngupa-* 'drink' (#71) < **kupa-*). Thus the re-creation of first person singular pronouns in WRN and WOI followed almost identical paths. In WOI, *marrambik* 'I' (#42) is *marram* 'body' + epenthetic *b* + *-ik* 'my,' and the second person singular pronoun (#43) is built on the same root. Note also, far to the north, YY's *pam orto* 'thou' (*pam* 'person, human body, male'; *orto* synchronically < *nhorto* 'thou').

This widespread mechanism for the re-creation of pronouns is certainly ancient in Australia, and its genesis could well antedate the physical separation of Tasmania from the mainland. It therefore seems appropriate to examine pronouns in Tasmanian languages,[3] bearing in mind at the same time (1) Greenberg's Indo-Pacific hypothesis of twenty years ago, by which Tasmanian would belong in the same phylum as "Papuan," and would stand apart from Australian, and (2) Crowley and Dixon's 1981 study of Tasmanian, which finds no evidence for Tasmanian : Australian genetic relatedness.

We feel that Crowley and Dixon have uncovered some crucial evidence and then downplayed it unnecessarily:

1. Western Tasmanian (TAS-W) *TULLAH*, *TULLANA* 'tongue,' arguably */t(h)alana/*, vs. PPN **jalany* 'tongue.'
2. Southeastern Tasmanian (TAS-SE) *BOULA* 'deux,' taken to be */pula/*, vs. PPN **pula* 'two.'

'Tongue' and 'two' indisputably belong in the very forefront of basic vocabulary — e.g., witness the cognation of German *Zunge* and Panjabi *jíib*. The evidence cited by Crowley and Dixon thus constitutes an extremely promising point of departure for further cognate search.

3. Turning now to pronouns, we find, again, unnecessary caution on their part in the face of TAS-SE *NEENA*, evidently */nina/*, 'you' *[SG]* (#61), which has definite

echoes in some non-Pama-Nyungan languages, where Tryon (1974) documents */nina/* 'you'*SG* in the Daly Family languages Maranunggu (MAR) and Ami. (More work is clearly needed in the reconstruction of second person singular pronouns in Australia, as Capell (1956) reconstructs "Common Australian" (=PPN) **nyun* (#75), Blake (1988) Proto Non-Pama-Nyungan **nginy*, and Dixon (1980) PA **ngin* (#68).

4. The third person singular (and plural?) pronoun in Tasmania is universally *NARA*, probably */nara/*. This has clear echoes in PPN **nyarrang* 'there.'

5-6. We turn now to first person singular pronouns, which are *MEENA* (*/mina/*) (#49) in the southeast of Tasmania and elsewhere and */manga/* in the north. We hypothesize that these replaced, because of taboo, an earlier TAS root for 'I,' which conceivably was a reflex of Dixon's Proto-Australian **ngay* 'I.' As has been seen above, the mechanism for the creation of a new pronoun 'I' has repeatedly involved, in mainland Australia, the pressing into service of an old term for 'person, face, eye,' or 'body.' Why should this not have happened in Tasmania also? Thus TAS-SE */mina/* 'I' can be seen as a plausible cognate of #45, and #47-56, bridging the 12,000 year gap by way of pre-pre-Proto-Pama-Nyungan. And northern TAS */manga/* 'I' belongs, correspondingly, as a cognate of #36-39.

If it be taken as given that the four TAS pronouns cited above have plausible cognates in mainland Australia, it behooves us to extend the search to the most basic realms of the vocabulary. Where to turn? If Capell (1956) and Dixon (1980) have taught us anything, it is that one of the true "Devonian rocks" of comparative Australian is the monosyllabic verbal root **pu-m* 'hit/do.' (In fact, Foley [1986] posits cognation of this root with Papua New Guinea's Proto-Eastern Highlands **pu-* 'hit.')

So what of TAS? Forms like Northeastern Tasmanian (TAS-NE) *LAANE* 'hit' provide not a glimmer of hope that they descend from Proto-Australian **pu-m*. What if we assume an ultra-conservative phonological history for the Tasmanian languages — as seems justifiable on the evidence so far. We could then simply seek out any TAS forms with shapes such as *pu(m)...* or *po(m)...*, momentarily ignoring semantics completely, and then only *subsequently* bringing semantics into razor-sharp focus.

7. This strategy *immediately* brings us face-to-face with TAS-NE *POMA-LI* 'make, create,' which is entirely compatible with the secondary meaning of Proto-Australian **pu-m*, above, namely 'do,' Note, too, that the shape *pum(V)* is phonotactically very much in disfavor both in Tasmanian and Australian languages (vs., say, *pun(V)*) — making cognation even more plausible.

8. Another Devonian rock of comparative Australian is Capell's **ka-* /Dixon's **kaa-ng* 'carry.' It would therefore make every kind of sense to look for a cognate in TAS. We do, in fact, find that TAS-SE was host to the form *KANA* 'bring' — indeed a likely candidate for cognation, especially if the *-na* can be shown to have been a suffix.

In the following summary of the remaining more or less plausible candidates for cognation, Pama-Nyungan looms quite large on the Australian side of the equation. This should not be taken as necessarily indicative of a closer relationship between TAS and Pama-Nyungan than between TAS and non-Pama-Nyungan Australian languages. Rather, it is probably an artifact of the authors' much more detailed knowledge of Pama-Nyungan. We call upon Australianists steeped in non-Pama-Nyungan languages to help push back the curtain of ignorance surrounding TAS still further.

9. TAS-SE *ANYAA* 'what': PPN **ngaana* 'what.'
10. TAS-NE *KÖNTA* 'earth, ground': PPN **kunang* 'faeces.'
11. Mid-eastern Tasmanian (TAS-ME), etc. *KRA-KANA*, *KRA-TABEE* 'sit, remain, be': GAW *KURA-* 'to approach, touch, feel,' WEM *kuroe.ja* 'to step on, squash.'
12. TAS-ME *LÖ-GANA* 'weep': PPN **rung(k)a-* 'cry, weep.'
13. TAS-SE *LÖGA(NA)* 'foot': PNYY **luku* 'ankle.'
14. Northern Tasmanian (TAS-N) *MOKA* 'rain': PA **nguku* 'water.'
15. TAS-ME *MUNA* 'lips': PIN, MRN *muni* 'lip.'
16. TAS-ME *NGAMELE* 'when, where': NYA *ngapi* 'whatchamacallit.'
17. TAS-SE *NÜ-KARA* 'drink': GYA *nuka-l* 'to eat, drink.'
18. TAS-SE *PAA* 'man, husband' (< 'scarred with cicatrices'??): PPN **para* 'scar, scarred.'
19. TAS-ME *PAALIERE* 'to extinguish': PPN **palu-* 'to die, be extinguished.'
20. TAS-SE *PAYI(NA)* 'tooth' (< 'biter'?): PPN **paja-l* 'to bite.'
21. TAS-ME *PYA(WA)* 'two': NGL *piya-* 'they two.'
22. TAS-NE *RAPA* 'bad, angry': PIN *rapa* 'confident, unafraid, bold,' *rapanyju* 'very confident or defiant, [having] no fear.'
23. TAS-NE *RIA(NA)* 'song, game, dance': PPN **rirrang* 'tooth' (whose reflexes include YIN *yirra-ma-* 'to sing').
24. TAS-SE *ROÁTA* 'to spear': PPN **ruwa-* 'to twist, to hit with missile.' Cf. GID *tuwa-* 'to dig [with spear-like digging stick].'
25. TAS-SE *TÁTUNE* 'frog': PPN **jarrany* 'frog.'
26. TAS-SE *TIÉNA* 'give': PPN **jAya-l* 'give, send.'
27. TAS-NE *TIGANA* 'eat': JIW *thika-* 'eat,' PIN *jiki-rnu* 'drink, smoke.'
28. TAS-NE *TOKANA* 'foot': BAY *juka.rra* 'foot,' UMP *thuki-* 'to follow the tracks of...'
29. TAS-SE *TRU-KERA* 'coire': UMP *thuka-* 'copulate with.'

Many of these putative cognates seem more plausible than those found in Greenberg (1971), although the lack of TAS data and the great time depth involved make any proposed genetic relationship which includes the Tasmanian languages extremely difficult, if not impossible, to verify. At the very least, however, the sets proposed here indicate that the possibility of a relationship between Australian and Tasmanian languages deserves serious consideration.

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NOTES

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2) See key, in which parentheses indicate the source of a borrowing, and square brackets are used to signify that the form presents either phonological or semantic problems. Data recorded before the use of standard transcription systems are presented in upper case letters.

3) The Tasmanian data presented in the following discussion are taken from Schmidt (1952). Unfortunately, Plomley (1976) is presently unavailable to us.

APPENDIX

A	1	UMPila	aja	shallow
B	2	WadJuK	BUDJOR	ground
B	3	GUPapuyungu	buthuru	ear
C	4	GUP	dhirr'thirryu-n	frighten
D	5	WJK	DJAM	water
E	6	WJK	GABBI	water
F	7	WJK	GORAD	short; stunted
F	8	GUP	gurriri	short
G	9	Warlpiri(WLB)	jalanypa	tongue
G	10	PINtupi	jarlinypa	tongue
H	11	YiDiNy	jili	eye
I	12	PIN	jina	foot
I	13	GIDabal	jinang	foot
C	14	WaRiYangka	jirril	afraid
C	15	NYAngumarta	jiti-rni	flush (bird from cover)
J	16	NYA	jungka	ground, dirt
E	17	PIN	kapi	water
E	18	DIYari	kapi	egg
K	19	GAWurna	KARTO	wife
K	20	NgarluMA	kartu	man, male (as of animal)
K	21	MaRDuthunira	kartu	thou
L?	22	TIWi	kukuni	water
M	23	WEMbawemba	kupa-	drink
M	24	NGarLa	kupapirri	stooped posture
M	25	Bidyara-GUngabula	kupu thana-	bend, stoop
F	26	YDN	kurran	long, tall
F	27	MiRNiny	kurrartu	short
N	28	PIN	kuru	eye
F	29	NYA	kuta	short
N	30	UMP	ku'un	eye
O	31	WLB	langa	ear
O	32	WaRNman	langa	ground, dirt
(P)	33	Proto-Eastern Oceanic	*malu	shade, shadow
P	34	GID	malung	shadow, shade

P	35	NhANda	malu	shade
Q	36	ADNyamathanha	mangu	face
Q?	37	YinGgarDa	mangu	good
Q				
Q	38	Proto-KAnyara	*mangu	cheek
Q	39	GUP	mangutji	eye, seed,...sweetheart
R	40	ARaBana, PIN	mara	hand
[S]	41	WOIwurrung	marram	body
[S]	42	WOI	marrambik	I
[S]	43	WOI	marrambinherr	thou
R	44	UMP	ma'a	hand
T	45	PaNKarla	MENA	eye
[U]	46	WJK	MIKI	moon
T	47	Kala Lagaw Ya	MINA	true, real, good, perfect
			mina	good
			mina geth	right hand (*geth* 'hand')
T	48	JIWarli	mina	right hand
T	49	TASmanian	MEENA /mina/	I (SE and Oyster Bay)
T	50	ADN	minaaka	eye
T	51	"King George Sound"	MINAM	truly
T	52	KGS	MINANG	(Name of language at KGS)
?	53	KGS	MINANG	the south
T	54	Proto-Pamic	*mini	good
T	55	WJK	MINOB	to be jealous
T	56	WJK	MINYT (= /minaj/?)	the countenance
V	57	GUP	munatha	ground
V	58	WRN	mu(r)narta	ear
[W]	59	WJK	MURDO	in vain
[X]	60	WJK	MY-A /maya/	a house
Y	61	TAS	NEENA /nina/	thou (SE and Oyster Bay)
Z	62	NYA, WLB	ngaju	I
AA	63	PIN	ngampu	egg
AA	64	NYA, DIY	ngapa	water
AA	65	PITta Pitta	ngapu	water
AA	66	PKA	*ngapuru	brains
Z?	67	TIW	ngia	I
Y?	68	Proto-Australian	*ngin	thou (Dixon)
L	69	BAAgandji	nguku	water
L	70	WLB	ngukunypa	brain
[M]	71	WOI	ngupa-	drink
Y	72	GUP	nhu-na	thee
BB	73	WJK	NURGO	egg
BB	74	GUP	nurrku	brains
Y	75	PPN	*nyun	thou (Capell)
AA	76	PIT	pampu	brain, egg
AA	77	YGD	papa	water
AA	78	ARB	papu	egg
S	79	WRN	parra	I
S	80	WRN	parrangku	thou
S	81	KLY	PARU	forehead, face; front
			paaru	face
X	82	BAYungu	paya	deep wooden baby tray
(X)	83	Bahasa INdonesia	payung	umbrella
X	84	GID	payuung	sling for carrying a child
U	85	GAW	PIKI	moon
CC	86	Yir-Yoront	pin	(1) ear, (2) site, place... country...
CC	87	PIN	pina	ear
CC	88	GID	pinang	ear
S	89	"GIPpsland"	prra (sic)	man, person
W	90	PIN	purtu	[in vain]
A	91	Gugu YAlanji	tajali	deep water
DD	92	PIN	tari	inside ankle bone
G	93	BGU	thalany	tongue
D	94	YINdjibarndi	thama	fire
DD	95	UMP	tha'u	foot
I	96	WRY	thina	foot
J	97	BAY	thungkara	ground, dirt
H	98	NYA, PIN	tili	flame
J	99	WJK	TONGA (arguably /thungka/)	ear
EE	100	PIN	tungku	short
EE	101	UMP	uungku	long
FF	102	YIN	wirri	play
FF	103	NYA	witi	play
GG	104	UMP	yampa	ear
GG	105	Wirri (WRI)	yampa	...(on the) ground, place...
GG	106	NYA	yamparra	single person
GG	107	WLB	yampirri	single men's camp
GG	108	YY	yap	(1) leaf, (2) bush, shrub
G?	109	TIW	yimitarla	tongue
HH	110	WJK	DILBI	leaf
N	111	WEM	kurumpaya	to be jealous
T	112	WEM	-min	EMPHATIC particle
[HH]	113	PIN	nyalpi	leaves
HH	114	DIY	thalpa	ear

C. C. UHLENBECK AND DENE-CAUCASIAN

W. WILFRIED SCHUHMACHER
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1. Being a Bascologist and an Americanist, in addition to his contributions to Indo-European linguistics, C. C. Uhlenbeck

(1866-1951) might be supposed to have had the best equipment to make an early contribution to what today is called "Dene-Caucasian", also taking into account his interest in "lumping" in other areas; cf. his Eskimo-Indo-European hypothesis or the relationships of Basque discussed by him. (I have benefited from the obituaries by Hammerich, Bouda, and Thalbitzer in tracing life and linguistics of C. C. Uhlenbeck; Hammerich, Bouda, and Thalbitzer 1953; and have also taken his publications from there.)

As pointed out by Bouda, "from his treatise 'Baskische Studien', which appeared in 1891, to the end of his life, [Uhlenbeck] had followed, loved, and promoted the study of the Basque language" (Hammerich, Bouda, and Thalbitzer 1953:75). As an Americanist, Uhlenbeck started in 1905 with Eskimo to be followed by Algonquian, especially Blackfoot, due to his stay in Montana among the Piegans in 1910-11. Later, most interested in Algonquian, he also tried to acquire a general view of the North American Indian languages, which even is present as late as in a work from 1948.

2. Despite all his knowledge, in his genealogical research concerning Basque, he never did enter the American continent but stayed in Asia (Ural-Altaic, Chukchee, Paleo-Caucasian). Also, evidence from Basque might have been helpful when tackling American Indian Problems.

3. In his "Blackfoot-Arapaho Comparisons", Uhlenbeck (1827:227) is faced with the problem of Blackfoot *kóni, -ko* "snow" ~ Arapaho *hí* "id.", supposing "a case of vocalic intermutation". He, therefore, as far as Arapaho is concerned, thinks that *o* and *i* had been interchanged; that in **kino* the final syllable was lost as was **k*; and, to complete the process, that *h* was added word-initially, resulting finally in *hi* (neglecting here vowel quality).

4. V. Shevoroshkin (e.g., 1990) has defined Almosan-Keresiouan as a member of Dene-Caucasian (though not being Na-Dene). Bengtson (1991:102) lists as one of his Dene-Caucasian etymologies "FROST" **čiG-(Vr-)*, to be reflected, for instance, by Basque *(in-)tzig(ar)* "frost", Sino-Tibetan: Garo *-ćik-*, Lushei *śik*, Kachin *-si* "cold".

As Arapaho *-h-* < Proto-Algonquian **s* (and **h*), *hi* might well be interpreted as a reflex of a proto-form (with **-s-*) which goes back to the Dene-Caucasian reconstruction of "fronts" listed above (whereas the Blackfoot word might reflect another proto-form).

5. As for C. C. Uhlenbeck, it seems, then, his time had not come... It was only in the last years of his life and in the year following his death (1951) that the works of Karl Bouda, Robert Shafer, and Morris Swadesh appeared representing the first steps into today's Dene-Caucasian.

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Language in the Americas. By Joseph H. Greenberg. Stanford, CA: Stanford University Press, 1987. Pp. xvi, 438, maps, tab. Price: $45.00

Reviewed by STEFAN LIEDTKE

Joseph H. Greenberg, Professor of Anthropology and Linguistics, emeritus, of the University of Stanford, California, has undertaken in this book, on the basis of extensive linguistic material, to assign the multitude of Indian languages of the Americas to only three genetic groups. They are the Eskimo-Aleut and Na-Dene (Athabaskan, Tlingit, Haida) groups related to linguistic families in Eurasia and, finally, Amerind, in which Greenberg classifies all the remaining North, Central, and South American languages. Greenberg also correlates this linguistic threefold division with population-genetic and archaeological data, which likewise indicate three waves of Asiatic immigrants. The immigration of speakers of the Amerind group represents, according to this model, the oldest wave. The drastic reduction of linguistic groups also indicates a relatively late settlement of the New World in comparison to the other continents. From this point of view, it does not seem to make sense to distinguish precisely on this continent a disproportionately high number of linguistic families (up to two hundred). Thus, rather recently, Campbell and Mithun, in the volume edited by them *The Languages of Native America* (Austin, 1979), indicate for the United States alone 95 language families which are independent of each other. Against this "splitting" tendency, by which all languages are classified separately unless their relationship is obvious, the present work with its variably well-attested results, which are therefore only convincing in certain respects, does however in its overall tendency present very interesting opposite views.

According to Greenberg, the "Amerind" group is composed of eleven subgroups, each of which is outlined by lexical comparisons (63-180). I will only, because of considerations of space, outline a few groups by way of example. In the "Amerind Etymological Dictionary," lexical material is presented which is common to at least two subgroups (181-270). Here also Greenberg applies consistently his method of lexical mass comparison, which, as is well known, dispenses with laborious reconstructive work and the application of the structural comparative method. This procedure has its justification and advantages especially when compared to the fundamental weakness of pairwise comparison (4, 25). It should not, however, either by defenders or opponents, be seen as a substitute for the comparative method. The advantage of mass comparison clearly lies in this, that it draws attention to the larger picture and, in this way, the investigation avoids being mired in details. Broad linguistic connections from time to time require a correspondingly broad point of view and not a narrow focusing. But reconstructions and bilateral comparison should not be omitted but must be included, supplemented, modified, and extended. Where with overhaste and disregard for other possible procedures the work is too superficial, which is unfortunately often the case, the comparisons suggest a closer relationship than actually exists, or its quality repels someone who applies stricter standards. Both taken together unfortunately give the impression that further search is no longer fruitful.

The material presented for the subgroups must be considered under three aspects: together with the quality and quantity of lexical comparisons, also the number of languages considered, which for some subgroups is very large. Hence, when more languages are brought in and the citations are smaller in number, it happens, in some instances, that there is a considerable reduction of evidence for the individual bilateral connections and, along with this, the cogency of the classification.

Given the inaccuracy and the small amount of data, which is especially true for South America, although it is excusable, and to some extent expressly justified by the pioneer character of the work, one must make an exception in the case of Quechua. Here the material and the state of research are so favorable that the striking poverty and fair number of errors, which I need not go into here, are not justified. In addition, on pages 274-316, as evidence for the membership of Quechua in Andean, about ten grammatical elements are cited which Quechua has in common with ten different languages. As a control test, I would like to cite parallels I observed already two years ago between Wintun (W), a California Penutian language, and Quechua (Q) (for the evidence, cf. H. Pitkin, *Wintu Grammar*, UCPL 94 [1984], and G. J. Parker, *Ayacucho Q. Grammar and Dictionary*, the Hague [1969]; first W, then Q): (1) *-ya* 'iterative' / *ya* 'id.'; (2) *-ra* 'repetitive, continuative' /*-ra* 'continuative-durative'; (3) *-na* 'reflexive' / *na-* 'reciprocal,' mostly combined with plural *-ku*; (4) *-ca* 'transitive' / *-a* 'id.'; (5) *-p'ur* 'reciprocal (mutual participation)' (*p' ~ p* in W and Q evidenced internally) / *-pura* 'interactive-intersociative'; (6) *-paq* 'benefactive' / *-paq* 'id.'; (7) *man-* 'miss, lack, be missing,' *min-* 'to not be, not exist, negative' (*a ~ i* is recurrent) / *mana-* 'negation,' *mana ka-* 'falter' (*ka-* 'to be'); (8) *-kuy-* 'desiderative' / cf. *kuya-* 'love; affection for.' This means: Quechua has essentially more grammatical elements in common than with ten different Andean languages. From this, it follows that the classification must have a preliminary character until the possibility of a newly interpreted and more comprehensive and methodically more precise mass comparison have been exhausted. Until then, in any case, and also on the basis of other indications, Quechua, according to my reckoning, must definitely be placed nearer to the Penutian group.

The situation in regard to etymologies which argue comprehensively for the first time the unity of the heterogeneous Penutian phylum is gratifying. The similar attempts of Sapir, Whorf, Swadesh, and Hymes were hardly ever backed by significant lexical evidence, much less through the inclusion of all the linguistic groups: Tsimshian, Chinookan, Plateau, Oregon, Californian, and Mexican (with Mayan, etc.). Especially interesting for the reviewer as a confirmation of his own studies, is the inclusion of the Gulf phylum (Atakapa, Tunica, Chitimacha, Natchez, Muskogi), which is ethnologically highly interesting, if one thinks of the cultural relations of this region with Central American peoples. A further enrichment is Greenberg's convincing attempt to show a genetic relationship between Yuki (hitherto isolated) and Penutian. He points to a specially close connection between Yuki-Gulf, which also diminishes the weight of the argument that many Yuki-Californian Penutian parallels might be the result of areal diffusion. In view of the large number of Penutian languages, a comprehensive taking stock and extending of the material as well as establishing closer bilateral connections of the individual groups is a pressing desideratum and, in view of my previous studies, a task which promises success.

Likewise, my own unpublished investigations have led me to the surprising conclusion that the languages of Greenberg's Almosan group (especially Wakashan, Salishan, Algonkian) may possibly have as close a connection with the Penutian languages as with the Keresiouan group, particularly the more southern groups (Californian, Zuni, Mexican Gulf) and not quite so much with the partially-neighboring Penutian languages, with which areal influences could have played a role. This fits indirectly also with the proposal (by Mary Haas, 1958, in the *Southwestern Journal of Anthropology*) of an Algonkian-Gulf connection which in this way appears in a new light. On this point, Greenberg's subclassification should most likely also be modified for North America. Greenberg's "Amerind Etymological Dictionary" (181-270), indicated on the inside front flap to be, along with the grammatical section, the basic part of the book, unfortunately builds, as have all its predecessors, to a great extent an unmotivated hodge-podge of combinations which are phonologically and semantically too loose, and which cannot be gone into here in detail. Just one example: comparisons like those listed under 'bite' (192-193) (Guamaca *kaka* 'tooth,' Kiowa *k'o* 'knife,' Yanoama *koa* 'drink,' Ticuna *či* 'sting') are numerous and obviously lead nowhere. Also, the sound correspondences are seldom recurrent and show to some extent an unexplainable irregularity. Further, as can already be seen in the section on subgroupings (for example, Penutian 'feather' = 'wing 1,' 'leaf 2' = 'wing 2,' 'fire 2' = 'burn,' 'take' = 'arm' = 'touch'), many entries are completely or partially identical, so that they must therefore be seen as single items, thereby producing a decrease in the number of examples. On the other hand, obvious additional items are missing: in 'carry 1' (202),Yahgan *apa* is missing in Andean, as well as Californian **apa*. In regard to 'cut 1' (148): Costanoan *wal* has nothing to do with the Wintun and Natchez forms but is identical with Coos *walwal* 'knife.' Concerning 'cut 2' (148): Natchez top^h does not belong naturally with Wappo (!) *cipu* but with Wappo *t'oph-*, etc. Connections between the subgroups are therefore — in the lexical area — hardly discernible; in particular, the connections among South American and North American languages are practically undocumented.

The situation is entirely different in Greenberg's "Grammatical Evidence for Amerind" (271-320). This part of the work is brilliant and of incomparable value for his argument. To be sure, the comparisons are to a certain extent too narrow. For example, there is a *k*-negative (315) not only in three but at least six groups; the *w*-plural (295-296) not merely in one (!) but in at least five groups. Likewise, Almosan-Keresiouan *s*-causatives have parallels not only in Wakashan, besides those mentioned, but in Penutian (Yakuts, Tsimshian, Mayan), etc. Moreover, the treatment of the reflexive-reciprocal *-na-* (Penutian, Uto-Aztecan, Quechua) and the third person *u-/o-/w-* of Nootka, Algonkian, Yurok, Miwok, Mayan, and Tunica are completely absent.

Nevertheless, in spite of the above criticism of the lexical portion, Greenberg's book, through its treatment of grammatical elements, constitutes a milestone in the investigation of the genetical connections in North, Central, and South America. Merely as an example, we may mention here the excellent analysis of the pronominal *k*-element (probably originally first person inclusive) and its various reflexes. These results certainly provide the justification for Greenberg's other extensive conclusions. Only for this part, therefore, there holds without restriction: "...the strength of Amerindian studies is simply the vast number of languages. The synchronic breadth becomes the source of diachronic depth" (Preface, x). Greenberg's work provides, therefore, not only outstanding individual results — one often has to search thoroughly for these — and quite a few new insights regarding the new division of subgroups, but also for the first time a solid foundation for a comparison of Indian languages, which is not purely lexical.

The above review appeared in *Anthropos* 84 (1989), pp. 283-285. It was translated from the German by Joseph Greenberg.

IN THE PUBLIC MEDIA

The following report appeared in *Science News,* vol. 144, no. 2, July 10, 1993, p. 28:

LATE DATES IN EAST POLYNESIA

"A review of 147 radiocarbon dates obtained from mmaterial sites throughout East Polynesia, an area bordered by Hawaii, New Zealand, and Easter Island, suggests that humans lived in that part of the world much later than some scientists had thought.

"The earliest human presence in East Polynesia occurred in the Marquesas Islands between A.D. 300 and A.D. 600, assert Matthew Spriggs of the Australian National University in Canberra and Atholl Anderson of the University of Otago in Dunedin, New Zealand. Settlement began between A.D. 600 and A.D. 950 on most other islands and not until A.D. 1000 or shortly thereafter in New Zealand, the two archaeologists contend.

"Other researchers cite radiocarbon evidence for colonization of the Marquesas during the first millennium B.C. and Hawaii and Easter Island by A.D. 400. But these dates prove unreliable for several reasons, including the likely contamination of some samples before analysis and the inability to associate other samples with human-made relics, Spriggs and Anderson argue.

"Humans apparently spread throughout East Polynesia relatively quickly, possibly hopping from one island to another as they depleted easily obtained food sources, such as reef fish and turtles, the researchers propose in the June ANTIQUITY."

A GENETICIST MAPS ANCIENT MIGRATIONS

(Summary by Hal Fleming)

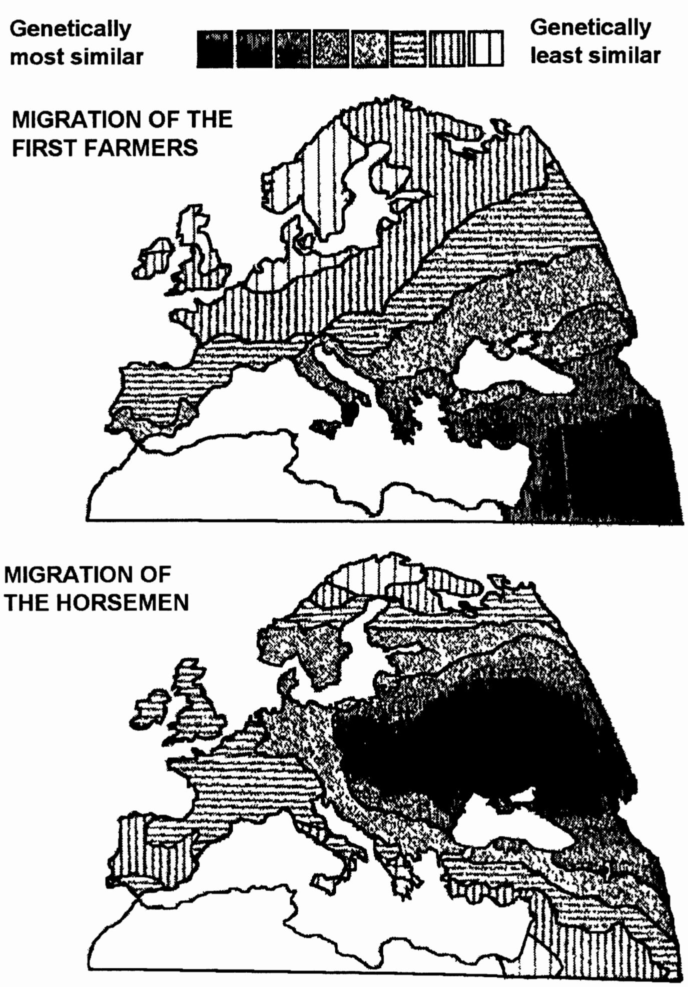
The *New York Times* ran a feature story on our honorable Fellow, L. L. Cavalli-Sforza, dated Tuesday, July 27, 1993, written by Louise Levathes, and entitled "A Geneticist Maps Ancient Migrations". Most of the massive data and conclusions of his team (Luca along with Paolo Menozzi and Alberto Piazza) will be published soon in a huge 1000-plus pages (Princeton University Press) and reviewed here in *Mother Tongue*, probably by two people representing the other two foci of our yet-to-emerge synthesis — an archeologist and a linguist. There will obviously be many reviews by "biologicals".

One small set of conclusions from the book can be mentioned here (words from Ms. Levathes). There are three maps which show genetic distributions which imply directionality, not necessarily migrations but also expansions and gene flows. When plotted on maps, the distributions show interesting patterns, some of which look very much like movements of populations.

Sticking close to Europe now, we find one map called "Migration of the First Farmers" (reproduced on p. 40). "The top map traces a migration out of Anatolia starting about 10,000 years ago. The people, the first farmers, gradually replaced indigenous European hunter-gatherers." Then we find a second map called "Migration of the Horsemen" (reproduced on p. 40). Levathes goes on to say: "The second map traces a movement of people from the Russian steppes 6,000 to 4,000 years ago; they had domesticated the horse and used it in armies."

Elsewhere she says: "These early farmers replaced the nomadic hunter-gatherers, and their sole direct survivors are believed to be the Basques, who are genetically and linguistically far removed from other Europeans." (Does "their" refer to the farmers or the hunters?) One must say that the maps show quite clearly that the area of likely survival of the hunters is **not** Iberia but northwest Europe, especially north Germany, Scandinavia, and north Britain. Similarly, the southern fringes of Europe (southern Balkans, south Italy, Moorish Spain) are much more like the Anatolian farmers and least like the Russian horsemen.

Ms. Levathes also credits Marija Gimbutas for having been "given a genetic foundation for a controversial theory," viz., that Indo-European speakers and their domesticated horses spread out from southern Russia to conquer old matrifocal Europe and its farmers. Cavalli-Sforza is quoted himself as saying: "We discovered an area of population expansion that almost perfectly matched Gimbutas' projection for the center of Kurgan culture." For the many who have found it fashionable of late to denigrate Marija's hypothesis — take note! The fad has moved elsewhere.



Source: Princeton University Press

BRIEF COMMUNICATION

SEEKING THE TRACES OF THE INDO-EUROPEAN HOMELAND

VÁCLAV BLAŽEK
Příbram, Czech Republic

The paper is divided into two parts: (1) a methodological scenario of the search of the homeland traces of an arbitrary group of related languages; (2) examples illustrating the homeland mapping in space and time, taken from the Indo-European languages.

The main axioms and points of the scenario are as follows:

1.0. The proto-language represents a hypothetical projection of all daughter languages in a single homogeneous language continuum, verified by the set of regular phonetic responses.
1.1. To map the homeland of a group of related languages means to define it in space and time including its consequent spreading.
1.2. The speed of changes in phonology, morphology, and lexicon varies in various languages, especially due to the influence of substratal, adstratal, or superstratal languages.
1.3. The most probable model of language spreading, especially in neolithic or paleolithic periods seems to be the sequential diffusion of mostly unnumerous populations. Their language was accepted by the original inhabitants for its prestigious role corresponding with higher culture, economic level or military power, or it became the language of business ("lingua franca").
1.4. The dissemination of cultures known from archeological searches also means diffusion of technologies rather than migration of a numerous population.
2.0. The location of the homeland of a group of related languages is based on a confrontation of the consequent sources of information present in the language, and with the extra-linguistic information from archeology, ethnography, paleo-ecology, etc.
2.1. Genealogical classification of the group of related languages, supplemented by relative or absolute chronology (lexicostatistics, glottochronology).
2.2. Ecological lexicon (especially flora, fauna).
2.3. Lexicon of material and spiritual culture.
2.4. Analysis of myths, folklore texts.
2.5. Identification of substratal lexicon and toponyms.
2.6. Analysis of proper toponyms.
2.7. References and borrowed proper names in texts and of other ethnic groups.
2.8. Analysis of mutual borrowings between the languages of the investigated group and their neighboring ones.
2.9. Hypothetical distant relationship of the proto-language and the proto-languages of other language families.
3. According to this scenario, we can present the following

conclusions: The oldest Indo-European homeland can be localized in the Near East. We do not know for sure if the ancestors of the Indo-Europeans were the authors of the neolithic civilization of Çatal-Hüyük, but the post probable roots of their spreading in the Balkan peninsula come from Asia Minor. The definitive formation of Indo-European culture and disintegration of Indo-European languages can be localized in the North Balkan - North-Pontic area. Most probably, this region was the epicenter of their migrations, including back migrations (the Asia Minor, the South Balkan, etc.). The Indo-Europeans were probably among the first to get acquainted with the domestication of the horse and the invention of the wheel, including the use of vehicles.

The above is an English summary of an article published in *Slovo a slovenost* 54 (1993). The full article, written in Czech, appears on pp. 31-39, while the English summary appears on pp. 39-40 of that journal.

SOFTWARE

Gamma Productions, Inc., the company that developed *Multi-Lingual Scholar*, has just released a new product, *Gamma UniVerse for Windows*. The following is a "description" of this package (from promotional literature supplied by the vendor):

Gamma UniVerse for Windows

Complete multi-language word processing system for the world's languages

With *Gamma UniVerse for Windows*, even the most complex languages with ligatures and overstrikes are easy to write and format. Arabic, Hebrew, and Persian format right-to-left; and Chinese and Japanese format horizontally or vertically. Mix and match any language combination. Spell check multiple languages in one pass.

Easy to learn and use

Gamma UniVerse takes full advantage of pull-down menus, mouse support and the features of the Microsoft Windows environment.

Easy transition from single language Windows word processing applications to Gamma UniVerse.

All screen fonts, printer fonts, and drivers are included. Just select print and it prints! No special complicated set-up procedures are required for supporting languages written in non-Latin alphabets.

Complete language support

Gamma UniVerse supports mixing languages in any document or on any line. UniVerse includes full support for mixing bi-directional text and contextual analysis. Overstrikes in all supported languages are completely WYSIWYG.

Cut and Paste multi-language text between multiple windows of the same or different documents.

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LETTERS TO THE EDITOR

Dear Hal,

Thanks for the kind words about my Nilo-Saharan work in MT-19 (can't find the page reference right now) but not for your comments on my comments on the Ringe test.

It seems that you and others have not really read Ringe's paper ("On calculating the factor of chance in language comparison", *Transactions of the American Philosophical Society* 82.1, 1992) or his reply to Greenberg ("A Reply to Professor Greenberg" — reference not at hand).

Contrary to your statement that Ringe's test shows that Indo-European "is the limit" (whatever that means), Ringe shows that IE indeed stands up as a phylum (at least insofar as his results for English, German, Latin, French, and Albanian go) but he does not look at anything beyond IE — the challenge is up to Nostraticists, etc. to give it a try. It is only a pre-test of what might be worth looking at, not a proof of anything.

As for Nilo-Saharan, your facetious comment "there goes Nilo-Saharan too" is uncalled for. In fact, my results so far support the idea of N-S as a deep phylum. Interestingly, the strongest positive test so far is for East Sudanic vs. Kado (in particular Krongo). For those who think Kado is part of Niger-Kordofanian, this could count as a "long-ranger" result. Why not drive over to Ohio State at end of July and get the complete story when I present my findings on all of N-S?

Let's face it: **global etymologies and multilateral comparisons are dead**. Contrary to Greenberg's assertion that looking at a whole lot of languages reduces the role of chance, the way others in that camp do it raises the role of chance vastly by creating networks of things "related" by chance phonological and semantic similarities. It is essentially irrefutable because it is completely uncontrolled. This is why Ringe's test is so vital: it provides a means of refutation. This is the scientific method, not repeated and ever more desperate listings of the products of too-imaginative minds and unsupported claims that the test is wrong.

I am sure I just lost several friends. But I've been expressing these criticisms for years and my views are based on seeking the right answers, not pleasing friends.

M. Lionel Bender
Southern Illinois University at Carbondale

Dear Hal,

Thank you very much for sending me MT 19, which I received yesterday. The article by Saul Levin seemed very unconvincing to me, and that of C. Hodge seemed open to criticism, but let that pass. Chacun à son goût. I would love to contribute something substantial myself; however, at my advanced age, this is not always feasible. Therefore, I will limit myself to some critical remarks to your "Brief Editorial", pp. 100-101. This concerns statements which you consider **false**, viz. the following:

1) "It is necessary to have a complete grammar and a lexicon of at least 2,000 words of a language in order to classify it." Desirable but not necessary. A judiciously selected 100-words list will suffice.

2) "One cannot classify a language on the basis of a short word list or poor field data": Cf. no. 1: poor field data **are** fatal.

3) "Two or more languages cannot be classified as related unless 'exact' sound correspondences can be established between them." What is meant by "exact"? "Regular" is the word. An "x" phoneme in language A, given identical phonetic surroundings, must always render a "y" phoneme in language B if they are actually related; introduction of a "z" phoneme for the same compared "x" phoneme in similar phonetic surroundings is certainly to be rejected.

4) "'Mere lexical similarities' cannot serve as a basis for classifying two or more languages as related": This is true, not false. Also, one must try to keep borrowings out.

5) and 6) — no comments.

7) "You can always find similarities between two or more languages just by accident...So seeking similarities is silly." Accidental similarities do occur often enough. But seeking **series of regular** similarities is not silly.

8) - 12a) require no comments. I simply do not know the answers.

N.B.: The plight of Mykolas Palmaitis is shared by all scholars and scientist is the former Soviet Union. My own salary amounts of $40; the prices in the shops are about the international level.

Sincerely yours,
Igor M. Diakonoff

HAL RESPONDS: Hal chooses not to respond to good colleague Igor's points. At least not at this time. He is grateful for Igor's letter and wants to get some more before answering. Two quick comments cannot be resisted. Re no. 2: more than one African language has been **accurately** classified by just 10 or 11 numbers recorded by an explorer or missionary. Re no. 3: we got the notion of "exact" correspondences from Shevoroshkin and other Moscovites. We were ourselves trained to say "regular". Live long and prosper, honored Fellow!

Juha Janhunen wrote on 16 August 1993 from Helsinki:

(he hopes that Hal will stay alive)...so that the Newsletter will not develop into an insider forum between a few convinced long rangers sharing the same apprioristic view of the world: I do not want to read about **material comparisons** between language families, since they are, in my opinion, just a continuation of the same old nonsense. What I do want to read about is news about **general issues** relevant to language comparison: new evidence of ancient population expansions, new methods of diachronic dating, new information about previously little known languages, etc. I would like to see *Mother Tongue* developing into the direction of a general ethnolinguistic periodical dealing with large issues of global significance.

So, the moment you die, I am very likely to quit, for I do not think those eager to continue your work are interested in keeping the doors open to criticism from non-believers. This is also evident from the solemn and serious style used by some of your colleagues: long range comparisons should not be so serious, should they? Therefore, take care of yourself.

I hope that, meanwhile, you have received my paper on Japanese. I find it extremely interesting, and not at all "dull and lifeless", that we can approach the problem concerning the origin of Japanese in terms of areal and typological comparisons. This **is** exactly the kind of **prehistoric linguistics** that someone has to do. I don't understand how anyone could think that this approach is less **vigorous** than that of the 19th century comparativists.

Best wishes,

Yours,
Juha Janhunen

HAL RESPONDS: The last paragraph of Juha's letter reminds one a great deal of Egerod's writings on southeast Asia, with one crucial difference — Egerod thinks that genetic classification is vitally important, although one may have to work towards it rather than starting out with it. Juha seems to have the same Horror Geneticus that the young Americanist priesthood has. Use all means and methods to work on interesting prehistoric problems but don't get involved with genetic taxonomy because it is not reliable, and it is too limited chronologically. Their teachers have mentally castrated a whole generation of young historians. Forgive them, Mother, for they know not what they lack!

EDITORIAL

Beginning with this issue, responsibility for the production of *Mother Tongue* is being shifted from Hal Fleming to Allan Bomhard. This will give Hal a much-needed break and allow him to concentrate his energies on other areas. This does not mean, however, that Hal will no longer be actively involved in editorial decision-making — indeed, he will remain very much involved. Given this change, manuscripts for publication in *Mother Tongue* should now be sent directly to me. General correspondence regarding the newsletter may be sent to either me or Hal. My address is:

Allan R. Bomhard, *Vice President*
Association for the Study of Language in Prehistory
73 Phillips Street
Boston, MA 02114-3426
U.S.A.

Also beginning with this issue, *Mother Tongue* is being produced in a new format. More than anything else, this change is being implemented to take advantage of the more sophisticated computer hardware and software available to Allan Bomhard. It is hoped that the readers will be pleased with the new design — comments/suggestions are welcome.

Keeping in mind that the purpose of *Mother Tongue* is to serve as a vehicle for the exchange and synthesis of ideas relating to the study of language in prehistory, in all of its aspects, members are encouraged to submit papers for publication, to comment upon work being done in the field, regardless of whether it was or was not published in *Mother Tongue*, to report on work in progress, to recommend books for review, and to keep the readers informed about forthcoming symposia, conferences, seminars, and the like that may be of interest to the members of ASLIP.

Allan R. Bomhard

NOTE: Previously promised news in archeology has been held back to give us a chance to contact people just back from the field. Therefore, *Mother Tongue* 21 (December 1993) will carry more archeological news. Also, some important articles in human genetics are just now coming out or are quite recent. So these too will be held back until December.

ASLIP BUSINESS

1. The persons honored by your votes have each individually agreed to be Fellows on the Council. Each acknowledged the honor warmly. They are now formally permanent Fellows.
2. One member of the Board of Directors is seriously ill and another has left ASLIP (Bender). Our quorum is now threatened. Therefore, ASLIP will now accept nominations for members of the Board of Directors. According to our By-Laws, the Directors are elected by the Annual Meeting. However, it will be useful to have nominations in hand at that meeting, especially those from several people. One surely may nominate oneself for Director. A willingness to attend the Annual Meeting will count for a lot!
3. For those who listen to the Canadian Broadcasting Company's (CBC) radio "Talk Show", a program on the "Mother Tongue" was broadcast recently. Besides the

incredible Merritt Ruhlen and your doddering old President, the voices of Noam Chomsky, Derek Bickerton, Colin Renfrew, and other interesting people were heard. If you want to hear the show, write to CBC Radio or our Pittsburgh office. We'll give it to you for nothing plus the cost of reproducing and mailing. It is not permitted to sell it.

4. While the Board of Directors will have to grapple with this question in April, we solicit your opinions on the following problem:

We have two Fellows who refuse (apparently) to participate in any ASLIP activity or even to answer letters in any way, other than what they are communicating already — some disdain. The problem has existed for more than five years. The question before the house is: should we throw them off the Council, or are ostensibly brilliant linguists excused from normal courtesy and cooperative effort? It's up to all of you.

BOOKS FOR REVIEW

The following books are available for review in *Mother Tongue*. If you would like to review one of these books, please write to Hal Fleming. Reviews are due 6 months after you receive the book. Please send 2 copies of your review, either single- or double-spaced with at least 1 inch margin on all sides to Allan Bomhard.

Shevoroshkin, Vitaly, ed. 1989. *Reconstructing Languages and Cultures.* Bochum: Brockmeyer.
Shevoroshkin, Vitaly, ed. 1989. *Explorations in Language Macrofamilies.* Bochum: Brockmeyer.
Shevoroshkin, Vitaly, ed. 1990. *Proto-Languages and Proto-Cultures.* Bochum: Brockmeyer.
Shevoroshkin, Vitaly, ed. 1991. *Dene-Sino-Caucasian Languages.* Bochum: Brockmeyer.

The following books are available for review in *Word.* If you wish to review a book, please write to Sheila Embleton, Department of Languages, Literatures, and Linguistics, South 561 Ross Building, York University, 4700 Keele Street, North York, Ontario, CANADA M3J 1P3. E-mail embleton@yorkvm1.bitnet or embleton@vm1.yorku.ca.internet. Telephone (416) 736-5016 at York and (416) 851-2660 at home. FAX (416) 736-5412 or (416) 736-5735. Books are available on a "first come, first served" basis. Graduate students are welcome to participate under supervision of a faculty member. Reviews are due 6 months after you receive the book. Please send **3** copies of your review, **double**-spaced with at least 2 cm margin on all sides. If possible, please **also** send your review on computer disk, specifying whether you used IBM or MAC, and which software programme you used. It may not be possible to return your disk to you. If your review will be less than one journal page or more than four journal pages, please check with the Review Editor before submitting your review. One journal page is roughly 1.5 double-spaced typed pages.

Books marked with * are appearing on this list for the first time. If you wish to write a review, this is your last opportunity. If there is somebody who would like to receive that book, but not for review, let me know — if nobody requests it, I might be able to send it to you (as a "gift").

Date of this list: August 24, 1993.

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January 6-9, 1994. Linguistic Society of America. Sheraton Hotel, Boston, MA, USA.
April 16-17, 1994. ILA. New York, NY, USA. Theme: Grammar in the Classroom.
August 9-14, 1994. LACUS. Vancouver, BC, CANADA.
December 27-30, 1994. MLA. San Diego, CA, USA.