

**Greenberg's American Indian classification:  
a report on the controversy\***

Paul Newman

Indiana University

It is now commonplace in theoretical linguistics for bitter disputes to rage between the adherents of this school or that. By contrast, over the past quarter of a century, comparative/historical studies have managed to progress in a solid, quieter manner; that is, until recently when the huge controversy erupted surrounding Joseph Greenberg's American Indian classification. The controversy concerning this classification, which has been viewed as everything from a stroke of genius to an intellectual scandal, has not only upset the tranquility of American historical linguistics, raising anew fundamental questions of methodology and principles of classification, but it has also spilled over into the general scientific and popular press.

The purpose of this report is first to summarize the events of the past four or five years, so that the reader can be brought up to date on what has transpired. Then I would like to discuss the issues involved, hopefully getting past the acrimonious argumentation and personality conflicts, in order to clarify the real linguistic issues that are in dispute. Before beginning, let me say that the battle lines between the pro-Greenberg and anti-Greenberg forces have been drawn so rigidly that it is difficult to find anyone familiar with the controversy who could be considered impartial enough to describe it in an objective way. Not being an Americanist with a vested interest in the outcome of the debate, I believe that I am in a position to provide a balanced and informative report; nevertheless it is only fair that I acknowledge that I do consider myself a disciple of Greenberg in the African linguistics sphere and thus am personally and intellectually sympathetic to him.

**The background**

The major thesis of Greenberg's classification, expounded in his book, *Language in the Americas*, published in 1987 by Stanford University Press, is that the languages of the New World, which previously had been classified conservatively into some 150 or so distinct families, fall into three stocks or macro-families. The first is Eskimo-Aleut, a non-controversial family made up of a group of more-or-less closely related languages spread from the Bering Straits to Greenland. The second is Na-Dene. This family, which consists of the widespread Athabaskan group plus the Eyak, Tlingit, and Haida languages of the Canadian northwest coast, was proposed more than half a century ago by Edward Sapir (1915). Although not all linguists accept the validity of Na-Dene — perhaps a majority do not — the proposal is hardly radical. Greenberg's third stock is Amerind, a huge group consisting of all the other North American Indian languages plus all the languages of Middle and South America. As should be obvious, it is this Amerind family that has caused the greatest furor.

The debate over Greenberg's new classification began not with the publication of his book, but rather with an article that appeared a year earlier in *Current Anthropology*, an article in which correlations between Greenberg's three macro-families and new findings about the genetics and dentition of New World populations were discussed (Greenberg, Turner, and Zegura 1986). This article was significant in two respects. First, it initiated a chain of events in which the evaluation of Greenberg's linguistic hypothesis became inextricably mixed with anthropological questions about the prehistoric peopling of the New World. Specifically, the classification became tied to a theory of a threefold migration from Asia across the Bering land bridge to account for the settlement of the Americas. Second, in the accompanying commentary it contained the unfortunate remark by Lyle Campbell (p. 488) that Greenberg's classification should be "shouted down", thereby setting the confrontational tone of the ensuing debate.<sup>1</sup> The image of outraged American Indianists shouting down seventy-five year old Joseph Greenberg was encouraged by the popular scientific press, which, for journalistic ends, chose to portray the differences between Greenberg and his critics not as a normal scholarly disagreement, but as a scientific war between opposing camps (see Lewin 1988, for example).

It is worth noting, by the way, that although the present controversy could be said to have begun with the 1986 *Current Anthropology* article, the broad classificatory scheme is much older. In the period around 1960, Greenberg (1960), Sidney Lamb (1959), and Morris Swadesh (1960) each suggested — presumably independently — that the New World languages all fell into just three stocks. In all three papers, the proposal was simply mentioned as a likelihood without supporting data. In a long footnote (p. 47, n. 13), Lamb went on to point out that the three stock idea was not original with him (nor with Greenberg or Swadesh), but rather had roots going back to the early 1800s.<sup>2</sup>

Greenberg's book appeared in 1987 and almost immediately received a *Current Anthropology* book review (Greenberg and respondents 1987), which consists of a number of short reviews by different authors.<sup>3</sup> Some of the reviewers were positive, but many were not, including Ives Goddard, an eminent Americanist, who "rejected it angrily", to use Greenberg's words (Greenberg 1990:7). Greenberg's characterization of Goddard's review is indicative of the general climate of the ongoing debate, since in my reading of it, I find it deprecatory and dismissive, rather than angry. If one wants to find anger and outrage, one need only turn to Lyle Campbell's diatribe, which was published as a review article in *Language* the following year (Campbell 1988). Whatever the validity of Campbell's specific criticisms, the tone of his review manifested a stridency that went far beyond the norm of academic linguistic journals. Another unfortunate event in coloring the nature of the intellectual exchange was the contribution in *Language* a few years later by James Matisoff, a renowned scholar in the Southeast Asian field (Matisoff 1990). According to the editor's note, this review commentary was invited to provide a neutral perspective on the dispute. However, in place of an insightful analysis, which one would have expected from a scholar of Matisoff's stature, the author provided a cutesy piece in which Greenberg and all other scholars who do long range comparison were subjected to ridicule. As one can imagine, harsh criticism has not been entirely one-sided. Greenberg has also not minced words in pointing out his opponents' flaws and in questioning their abilities, the gist of which is that they

know nothing at all about classification and that with their stated methodological rules of evidence, they wouldn't even be able to establish a family as clear-cut as Indo-European!

Two other publications around this time can be said to have indirectly added fuel to the fire, namely Ruhlen (1987), a basic reference work on the world's languages, and Cavalli-Sforza et al. (1988), an innovative study by a highly respected geneticist comparing linguistic and genetic data around the globe. What is striking about both these works is that they take Greenberg's new and still hypothetical American Indian classification as a given. This leads us to an understanding of the intense feeling of Greenberg's critics and the rationale for it. The essence of the objection is not just that the classification is all wrong and that the book is fundamentally unsound, factually and methodologically. Equally important is that the book was written by Greenberg and consequently would acquire an instant credibility it didn't deserve, if not among linguists, then by anthropologists and prehistorians. If *Language in the Americas* had been written by some unknown scholar without an established reputation, or if Greenberg were known to be a crackpot, then the book would probably have been ignored, with perhaps a minor review appearing here or there. But Greenberg is not a crackpot; he is a world-class scholar—in my opinion one of the major linguists of the 20th century—whose impressive contributions to such fields as African linguistics and universal typology are indisputable. Moreover, when one looks at other controversial, innovative proposals of his in the past, he almost always has turned out to be right. Thus we see the sense of frustration on the part of Goddard and Campbell, and many other serious, data-oriented Americanists, such as Wallace Chafe, Terrence Kaufman, and Marianne Mithun, who feel that they are now going to be obliged to waste their valuable time disproving what they consider to be a totally unsubstantiated piece of hogwash.

In March, 1990, Allan Taylor of the University of Colorado, Boulder organized a conference, sponsored by the National Endowment for the Humanities, to discuss Greenberg's classification and the controversy it had engendered. Many of the major players were there, including Greenberg and Ruhlen on one side and Campbell and Kaufman, along with other Americanists, on the other.<sup>4</sup> Among the participants were general comparative linguists who had a special interest in

substantive or methodological issues concerning matters of classification and also many non-linguists—archaeologists, physical anthropologists, and human geneticists. It was among the non-linguists that Greenberg found the strongest backing for his three migration theory and by implication for his three-way linguistic classification. Ironically, as I needled Greenberg in a recently published interview (Newman 1991), the success of his African classification had depended critically on ignoring the racial and historical factors that had misled previous scholars, and now the major support for his American Indian classification was coming not from an examination of the linguistic data per se but from its fit with migration scenarios.

The intermingling of Greenberg's language classification with the work of non-linguists had an important consequence: the issue moved beyond the world of specialists in American Indian linguistics and became of interest to a broader scientific and popular community. Thus, following the Boulder meeting began what one could call the media blitz. The controversy surrounding Greenberg's classification and its implications for New World settlement theories was covered by stories in *Science* (Morell 1990), *Science News* (Bower 1990a, Bower 1990b), *Nature* (Diamond 1990), and the *New Scientist* (Lewin 1990), as well as by scattered newspapers from San Francisco to Dallas. In a large sense the fears of Greenberg's critics had proved justified: whether Greenberg was right or wrong, his classificatory scheme was being disseminated widely.

As if this weren't enough, Greenberg's classification and his particular methodological approach acquired extensive coverage in a second, apparently unrelated, media event: sudden attention to the reconstruction of Mother Tongue (or Proto-Human), the establishment of large families such as Nostratic, and the scientific and personal status of long range comparison in linguistics. In a one year period, these topics were the subject of feature articles in three quite different mass circulation magazines: *U.S. News and World Report* (Allman 1990), *Scientific American* (Ross 1991), and *The Atlantic Monthly* (Wright 1991). The *Scientific American* article contained a large photo of Greenberg whereas *The Atlantic Monthly* went one better and included a prominent three-quarter page line drawing: Greenberg had clearly become a scientific celebrity. The negative side of the coin, however, is that Greenberg was portrayed not as the solid linguist

that he is (leaving his American Indian work aside for the moment) but as an iconoclast whose approach put him somewhere to the left of the crazies.

### **The issues**

As should be clear by now, streams of words have been exchanged about Greenberg's American Indian classification, some spoken in haste by pro- or anti-Greenberg forces, others penned by non-linguistically trained staff writers. In such circumstances, a lot of misinformation has been bandied about. What I would like to do now is try to clarify some of the real issues as I see them, avoiding, if possible, hyperbole and rhetoric. In the interest of conciseness, I shall limit myself to three general areas of disagreement. I won't offer a firm opinion as to the correctness of Greenberg's classification per se, but I will take the liberty of offering my views about the validity of specific arguments that have been raised.

1. The first issue is the soundness of Greenberg's method of mass comparison (or multilateral comparison, as he now prefers to call it), whereby classification is based on a broad inspection of basic vocabulary in a large number of languages. This issue actually breaks down into two sub-issues: (a) methodological and (b) epistemological.

- (a) Greenberg's critics argue that his method is patently unsound since surface similarities cannot be the underpinnings of a serious classification, the main reason being that there is no way to distinguish real cognates from loanwords on the one hand or areal features or accidental similarities on the other. They argue instead for the application of the Comparative Method, i.e. the establishment of regular correspondences and the systematic reconstruction of proto-forms, as the only solid methodological means of setting up language families.<sup>5</sup> By ignoring these strictures, Greenberg's methodology has been characterized in the general scientific press as unorthodox, innovative, and very radical. I would suggest that whereas Greenberg's American Indian classification as such may deserve to be characterized as audacious and heretical, his methodology, in principle at least, is really commonsensical. (In the case of his earlier African classification, it led to remarkably successful results, see Newman Forthcoming.) In the *Scientific American* article, mass comparison, viewed as a new, radical approach, is contrasted with the

Comparative Method: “If the Nostraticists bend the rules of the game, Greenberg and his group break them” (Ross 1991:145). But where did these supposed rules come from? As Greenberg has correctly pointed out, none of the important work on Indo-European over the past two centuries in which the Comparative Method was employed was designed to prove or thought essential to the establishment of Indo-European as a phylogenetic family. Similarly, the relationship of Bantu languages to one another preceded by a long time the beginning of systematic phonological and lexical comparison of languages in that family, and the same could equally be said for Semitic. The fact is, the Comparative Method exists primarily for non-classificatory purposes, namely to reconstruct aspects of a proto-language and to come to an understanding of linguistic changes and developments that have taken place in a family throughout the course of its history.<sup>6</sup> One could argue that Greenberg’s method, rather than being radical, is really a throwback to the old-fashioned, and sometimes mistaken, approach of late 18th and early 19th century philologists, whose assertion of relationships, such as between Sanskrit, Latin, and Greek or between Hebrew and Arabic, derived from the simple observation of *surface* similarities in vocabulary and grammar.

The criticism of mass comparison for not being able to distinguish true cognates from loanwords or accidentally similar forms is also due to a lack of understanding of the essence of Greenberg’s method. Greenberg would agree that in comparing lists of words from two disparate languages, one couldn’t always tell what was significant and what was not; but that is not the way he proposes that one should operate. The reason one compares many languages at once, rather than proceeding in pair-wise fashion, is just so that one can see patterns of similarities and configurations of interlocking similarities that one group of languages exhibits as opposed to some other language or group of languages. (Specialists in bats or elephants or dolphins might not see that these beings share a family in common — they might not even think of asking the question — but the kind of broad-brush approach Greenberg advocates is supposed to allow him to recognize such relationships.) All historical linguists are of course aware that in comparing individual languages, regular phonological correspondences are more valuable than scattered surface

resemblances, which can be accidental and misleading. The essence of Greenberg's approach, which traditional comparativists often overlook, is that when enough languages are taken into account, surface similarities may show up to an extent greater than one would expect by chance and thus provide a key to historical linguistic connections. Mass comparison is powerful and effective, so Greenberg would contend, because when the data from a broad array of languages are assembled, the languages almost separate themselves into groups.

But do they really? Given a comparative word list of some 50 items for some 100 American Indian languages, are the natural groupings really so obvious that an Eric Hamp or an Ives Goddard would come up with exactly the same classification that Greenberg would? If not, one has to ask the question whether a method that can only be applied by one person really qualifies to be called a method. In short, one wonders whether Greenberg has deluded himself into thinking that he has a rigorous method when instead he has something that most of us would much rather have, namely insight and genius! The key question that the opponents of Greenberg's American Indian hypothesis should be asking is not whether mass comparison *as an ideal* is sound, but rather whether his classification really derives from an application of the method in its pristine form. As best as I can determine, Greenberg has not presented unequivocal evidence of this in *Language in the Americas*. One does not, for example, find a multilateral basic vocabulary table for New World languages comparable to the one he provides for European languages (table 7, p. 24). Instead, as evidentiary support for his classification, he offers lists of cognate sets, and in so doing leaves himself open to the critical standards normally applied to such lists. We all know that if you look at enough words in enough languages with an allowable range of phonological and semantic variation, you cannot help but find matching items. (There is a good-sized list of supposed Hamitic etymologies at the back of Meinhof's *Die Sprachen der Hamiten* (1912), which, with hindsight, we now know to be totally worthless.) But this is hardly what Greenberg intends by the method of mass comparison. A major difficulty in evaluating Greenberg's hypothesis — whether one's personal leaning is in the direction of confirmation or refutation — is that the supporting data, especially the etymological sets, probably do not reflect the real evidence that led

Greenberg to classify the languages the way that he did. While the idea of mass comparison of vocabulary conjures up some kind of quantitative method, it is really an immersion technique in which after looking at huge quantities of data from language after language, one begins to develop a sense of what is diagnostic for one group as opposed to another. Although vocabulary is inevitably given prominence, by Greenberg and others, in methodological discussions on mass comparison, in practice Greenberg has always accorded great importance to grammatical similarities in his works on classification. This was true in his successful African linguistic classification, and his American Indian classification is no exception (see Liedtke 1989). The diagnostic items — which might be three detailed grammatical features and five specific lexical items — are what really matter if one wants to think in terms of a discovery method. All the other stuff—the long lists with, according to his critics, elementary transcription errors, mistaken morphological analyses, and false cognates—are simply there to try to convince the reader of what Greenberg is already sure of (and which, given his track record, may be so).

(b) This takes us to the epistemological question which lies at the heart of the dispute between Greenberg and his critics. The opponents insist that Greenberg's classification, especially his putative Amerind family, lacks proof. Some assert — erroneously I would contend — that relationships that go back further than 7,000 to 10,000 years can never be proved satisfactorily, and for Amerind, if it exists, one would be dealing with a time depth of double or triple that amount.<sup>7</sup> Be that as it may, the critics all seem to agree that Greenberg has failed to provide the essential proof to substantiate Amerind as a valid group. Greenberg's response is that the notion of proof is specious and the assumption that languages should automatically be treated as unrelated unless demonstrated otherwise is unwarranted. The objective in classification is not to prove this or that, but rather, given all the available evidence, to evaluate two competing hypotheses: (a) that languages x, y and z are related, or (b) that languages x, y and z are not related. As Lamb (1964:107) commented many years ago, “[I]t is not safe to assume that any two languages are unrelated. For a truly meaningful classification, it is just as bad to leave apart two related groups as it is to put together unrelated ones.” Whereas the opponents talk about proof, hard evidence,

and so on, Greenberg, in a disarming way, talks of his revolutionary Amerind theory as simply “the best guess going”. Clearly his method cannot provide proof of the kind that would stand up in court or would suffice to convince a skeptical specialist, but that’s not what it is supposed to do. It is simply supposed to lead to classificatory hypotheses that could be said to be more probable than the alternative proposals. In my opinion, Greenberg could be said to have won the first round of the epistemological debate.

There is another level, however, at which his position is not unassailable. Scientists cannot spend their time testing every crazy hypothesis that comes along. If someone claims to have discovered a cure for stammering, but has not tested it using normal procedures and controls, responsible speech pathologists have a right to ignore the theory as scientifically baseless. In order for scientists to have their theories treated seriously, they must first meet a certain evidentiary “threshold” (to use a legal term). The scientist doesn’t have to “prove” that his or her theory is right, but the claim cannot simply be asserted devoid of appropriate supporting data. Many of Greenberg’s critics insist that his “best guess hypothesis” on the Amerind stock is really a factually empty assertion constructed on meaningless look-alikes, faulty analyses, and misunderstood or mistaken data, whereas Greenberg and his supporters feel that the evidence he cites, while imperfect, is still quite compelling. I personally am not qualified to venture an opinion as to which side is factually right; but I would contend that the proper epistemological way to view the matter is as a threshold question. The arguments going back and forth about proof have been and will continue to be totally fruitless.

2. The second issue concerns migration theory and related matters concerning the peoples and peopling of the New World. The question is how relevant is the non-linguistic evidence to the evaluation of Greenberg’s American Indian classification. In some theoretical sense it shouldn’t count at all—from the time of Boas, American anthropologists, at least, have accepted the dictum that race, language, and culture are independent variables. In another sense, however, it does matter. Because the ancestors of separate language stocks represent separate language communities, the time, place, and number of these possible ancestors do place limits on the

reasonableness of specific linguistic proposals. As mentioned earlier, striking, if imperfect, correlations have been reported between Greenberg's three-way linguistic classification and work being done in archeology, genetics, and physical anthropology that supports the three-migration theory, e.g. Cavalli-Sforza et al. (1989), Rogers (1985), Schurr et al. (1990), Turner (1983), Williams et al. (1985), Zegura (1984). What is particularly significant about these works is that they were published independently of and based on research carried out prior to the appearance of Greenberg's book.<sup>8</sup> Even if, for sake of argument, one were to ignore the specific findings of these non-linguistic studies—although it is hard to imagine how one could do so—it is clear that just raising questions about possible settlement scenarios throws serious doubt on the reasonableness of the “conservative” view of Greenberg's critics in which American Indian languages are classified into some 145 or more totally distinct stocks. Not only would there had to have been at least 145 independent migrations across the Bering land bridge into the New World — Greenberg has joked about the need for a traffic controller there—but even more unlikely—and this, surprisingly has seldom been commented on — these 145 linguistic communities would had to have been spoken by totally distinct and unrelated languages *while still in Asia*. Whether Greenberg's Amerind phylum holds together or is eventually broken down into a larger number of separate families, the particular situation regarding the geography and human settlement of the Americas makes the assertion of myriads of independent stocks numbering between one hundred and two hundred the truly radical proposal. Were it not for the unwarranted rule that languages not proved to be related should be assumed on a priori grounds to be independent and unrelated, the proponents of the conservative consensus regarding American Indian classification would themselves have a major threshold problem.

3. The third issue concerns the factual, substantive basis of Greenberg's classification. I would contend that Greenberg's method is neither so flawed as to necessitate failure nor so powerful as to guarantee success. Thus the verification or refutation of Greenberg's classification is going to depend ultimately not on debate and rhetoric, but on a careful analysis and interpretation of the linguistic evidence itself. For Greenberg's position to be sustained, it is not necessary that all of

his proposed etymological sets and supposedly shared grammatical features be truly cognate, but his book must produce some comparisons that hold up under close scrutiny. Conversely, it is not incumbent on Greenberg's critics to *prove* that he is wrong (any more than he has to *prove* that he is right): refutation can be achieved either by providing a convincing demonstration that Greenberg has failed to make a threshold case for his classification (as some reviewers have already claimed), or by postulating a coherent, empirically better supported alternative classificatory model. Whether a linguistic proposal is to be accepted really depends not on formal proof, but on whether the proposal bears fruit or not. As Greenberg put it some 35 years ago with regard to his more limited classification of Central and South American languages, "The ultimate test is a pragmatic one.... If the present classification is correct, it will prove its usefulness in future more advanced comparative investigations.... By the same token it cannot be saved by the most ingenious argumentation if it fails the crucial test of practice" (Greenberg 1960:793).

In addition to objections about the historical interpretation of the cited forms, which relates to what is viewed as excessive semantic and phonological latitude in matching forms, Greenberg's opponents have raised damning complaints about the reliability of the basic data themselves. Goddard, e.g., has attacked Greenberg for "his use of data that are erroneous or misanalyzed" (Greenberg and respondents 1987:657) and other critics have similarly pointed out what they consider to be error after error in his citations. These charges, if true, are clearly serious and one can certainly understand how they worked to undermine the American Indian specialists' confidence in Greenberg's work as a whole. It has been contended by Greenberg that the method of mass comparison has a built-in tolerance for error. Maybe so, but I think that there is greater validity in the view that mass comparison or not, a comparison of non-words can produce nothing but nonsense. Does this mean, then, that the Amerind family, to focus on Greenberg's most tenuous proposal, is a classificatory castle built on sand, ready to tumble the next time an expert on Hopi or Zapotec or Mohawk finds a mistake? Greenberg's response to his critics has repeatedly been along the following lines: "Of course there are mistakes in a book of this scope; but, taken in the aggregate, their extent has been overexaggerated. In any case, even if you took

every single citation out of the book that you regarded as factually incorrect, there would still be ample evidence remaining to support my three-fold classification in a clear and convincing manner.” So, while the unreliability of basic data in a comparative linguistic work has to be regarded as an extremely serious matter, it still remains an open question whether the errors found in Greenberg’s book are such as to be fatal to his enterprise.

## **Conclusion**

It is probably too early to predict how Greenberg’s specific proposals will fare, although if I had to venture an opinion, I would bet that the number of distinct families will ultimately turn out to be much closer to three than to a hundred and forty-five. When subjected to careful scrutiny over a period of time, it is possible that some of his higher-level groupings will have to be dismantled and that a somewhat larger number of independent phyla will have to be recognized. What we can say without hesitation, however, is that Greenberg’s classification has set a fire under the Americanist community and that this field, which can already boast a venerable tradition in descriptive and historical studies, is likely to see a major renaissance in the coming years. By stimulating an increased interest in American Indian linguistics and culture history, which is likely to lead to new research and far-reaching discoveries, Greenberg, the villain, might end up being the best friend this field has ever had.

## NOTES

- \* I am grateful to the Dean of the College of Arts and Sciences, Indiana University, for providing the travel funds to Amsterdam that enabled me to attend the conference. Detailed comments on the draft of the paper originally presented were kindly provided by Lyle Campbell. Clearly I have not accepted all of his suggestions, and just as clearly he cannot be said to endorse the points of view reflected here.
- 1 Campbell and others (personal communication) have objected that his remark has been unfairly quoted out of context. Campbell’s comments begin: “This article is distressing” and ends with the sentence: “Indeed, the linguistic classification should be *shouted down* in order not to confuse nonspecialists or detract from the real contribution linguistics can make to prehistory” (p. 488, emphasis mine). I personally do not see the basis of the objection. Whereas Campbell probably regrets his exact choice of

- words, his complaint about being quoted unfairly rings hollow, especially since all the evidence since then suggests that the phrase accurately represented (and still represents) his sentiments on the matter
- 2 The most direct and influential precursor would, of course, have been Sapir, whose broad approach to classification (cf. Sapir 1929) led him to the postulation of distant linguistic relationships
  - 3 Subsequent reviews include the following: Bright (1988), Campbell (1988), Golla (1988), Adelaar (1989), and Liedtke (1989).
  - 4 For reasons that I am unaware of, Ives Goddard did not attend the conference
  - 5 Scholars such as Lyle Campbell have been outspoken in their criticism of Greenberg's methodology. The question that one needs to ask is, if their method is so superior, why is it that their own results have been so obviously wrong? To my way of thinking, a classificatory methodology that leads to the establishment of 62 totally separate linguistic stocks for North America alone (Campbell & Mithun 1979) can hardly be taken as an advertisement for its validity!
  - 6 In support of this conception of the nature of the Comparative Method, consider the remark by Calvert Watkins (1990:292), a solid comparativist with impeccable credentials: "As to the mystique of sound laws . . . Greenberg is right to quote with approbation the Africanist Paul Newman (1970[:39]): 'The proof of genetic relationship does not depend on the demonstration of historical sound laws. Rather, the discovery of sound laws and the reconstruction of linguistic history normally emerge from the careful comparison of languages already presumed to be related'."
  - 7 At the Boulder meeting, Greenberg, who had endorsed a time depth of approximately 12,000 years for his Amerind family (Greenberg, Turner, and Zegura 1986), was bombarded with recent, but still controversial, evidence suggesting that human settlement of the New World went back over 30,000 years. Although this evidence seemed embarrassing to Greenberg at the time, I would suggest that it actually is helpful to his position since it allows a much greater time period to account for the tremendous geographical spread and linguistic diversity of his Amerind phylum.
  - 8 As Greenberg (personal communication) has correctly pointed out, although it is a serious error to base a linguistic classification on non-linguistic evidence, a convergence of results between linguistic and non-linguistic studies carried out independently *does* have probative value.

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