

The Settlement of the Americas: A Comparison of the Linguistic, Dental, and Genetic Evidence

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A CONSIDERATION OF THE BIOLOGICAL evidence regarding the hominid settlement of the Americas leads to the following basic conclusions: (1) It did not occur prior to the terminal Pleistocene. (2) It was carried out by evolutionarily modern *Homo sapiens sapiens*. (3) It had its origin in migration from Asia via the Bering Strait. These views have had wide acceptance and

rest mainly on the absence of human skeletal remains earlier than the terminal Pleistocene, the biological resemblances between Amerindian and Asian populations, and the fact that human dental variation in the Americas is less than that in Asia, suggesting the relative recency of American settlement.

If the foregoing statements are taken as a set of working hypotheses, a series of major questions of greater specificity immediately arises. These include the number of migrations to be postulated, the ethnic and archaeological identity of each, and their relative and absolute chronology. In developing hypotheses regarding these questions, we will turn to the linguistic, dental, and genetic evidence, in that order. The three lines of evidence agree that the Americas were settled by three separate population movements whose identity can be most precisely expressed in linguistic terms as Amerind, Na-Dene, and Aleut-Eskimo.

These three types of evidence are logically independent in the sense that there are no *logical* constraints operating across any pair of them. It is obvious, for example, that the markers *n* for the first person singular and *m* for the second person singular, both of which are, in fact, very widespread in the Amerind linguistic stock, have no necessary connection with the presence of A and B genes at the same genetic locus or the presence or absence of incisor shoveling. There is, however, one source of bias that needs to be taken into account. If the investigator in one field is aware of the conclusions proposed in another, he or she may be influenced by this knowledge in developing a theory. It need not, of course, have this effect. The ultimate test is whether the scientist is able to justify the conclusions on the basis of data drawn exclusively from the field itself. In the present instance the linguistic researcher, on the one hand, and the genetic and dental investigators, on the other, carried out their work independently. Since dental and genetic studies belong to the same basic field, physical anthropology, it is not surprising that each was aware of relevant theories in the other's domain. That their conclusions were reached exclusively on the basis of internal evidence within the field itself is, however, sufficient indication of their independence.

THE LINGUISTIC EVIDENCE

There is only one comprehensive classification of the indigenous languages of the Americas (Greenberg 1960, n.d.). The major alternative and one which has become increasingly in-

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fluent in the recent period would involve the acceptance of something like 200 independent linguistic stocks (Loukotka 1968, Campbell and Mithun 1979a). What is being tacitly claimed by the proponents of this point of view is that, if we take any three of these stocks at random, we will never find that two of them are significantly more similar to each other in regard to traits relevant to a historical genetic classification. If this claim is accepted, two alternative types of nonlinguistic historical conclusions are possible. One is that each of these represents a separate migration, thus requiring a traffic controller at the Bering Strait. The other is that there are relatively few migrations or even a single one but that the time elapsed is so vast that all traces of affinity among any of the groups have been effaced.

A brief survey of the general trends in the classification of American languages will help to put the present proposal in historical perspective. Initially, it may be noted that, except for the work of Greenberg and that of Swadesh which preceded it but did not result in a precise classification, linguistic classification has proceeded separately in North and South America, with some Central American languages being included in either instance when relations across the border were obvious.

For North America the point of departure has been Powell (1891), which distinguished 58 stocks in North America, some of which extended into Mexico. A number of American anthropological linguists, including Harrington, Dixon, Kroeber, Whorf, Trager, and Sapir, beginning early in the 20th century included many of Powell's stocks in more extensive groupings of which the most important were Hokan, Penutian, and Azteco-Tanoan. This period of consolidation reached its climax in Sapir (1929), a classification in which six basic stocks north of Mexico were distinguished, with, in some instances, members south of the United States border. Sapir's six families were I, Eskimo-Aleut; II, Algonkin-Wakashan; III, Na-Dene; IV, Penutian; V, Hokan-Coahuiltecan; and VI, Azteco-Tanoan.

In the period following its publication and up to about 25–30 years ago, some investigators, who accepted the basic premise that there was a very limited number of stocks north of Mexico and also the validity of at least a substantial portion of Sapir's classification, began to publish studies in which evidence was presented for relationships cutting across Sapir's groupings. Instances of these are Haas (1958), linking a portion of Sapir's Hokan-Coahuiltecan, namely, the Gulf languages, to the Algic group (Algonkin, Wiyot, and Yurok), belonging to Sapir's Algonkin-Wakashan; Shipley (1957), comparing Yukian in Group V of Sapir's classification with Penutian (IV); and Newman (1964), seeking to prove the affiliation of Zuni (put in VI with a query) to the Penutian group.

It is perhaps partly, at least, because of these and similar hypotheses that the last two decades have witnessed a skepticism among a substantial portion of specialists in American Indian languages even of groupings which had hitherto been considered secure, e.g., Hokan in the narrower sense. There have been negative articles, notably Levine (1979), which seeks to disprove the affiliation of Na-Dene with Haida, its most distant branch. This trend reaches its climax in Campbell and Mithun (1979a), which, in spite of its title *The Languages of Native America*, embraces only North America with some extensions into Mexico and Central America. In their introductory contribution the editors list 62 separate stocks, a larger number than that of Powell. In several instances doubts are expressed concerning the validity of even some of these, so that the number is probably even larger. It is clear, however, that some of the contributors to the volume do not espouse this general point of view.

As indicated earlier, the classification of the languages of South America has been carried out in isolation from those of North America. Certain low-level stocks of extensive membership were early recognized, e.g., Arawakan, Carib, and Ge. However, the virtually exclusive tradition has been the

cataloging of numerous supposedly independent stocks without any attempt at synthesis. The most recent work in that tradition is Loukotka (1968), which lists 118 independent families. Some of these are extinct and have left no linguistic traces except, in some instances, in place names, and a few are only known from a handful of lexical items and are now extinct. Even subtracting these, the number of distinct families remains very large.

In a paper delivered in 1956 (Greenberg 1960), Greenberg tentatively advanced the hypothesis that all the languages of South America fall into three groups, Ge-Pano-Carib, Andean-Equatorial, and Chibchan-Paezan, the last of which, in accordance with the views of earlier scholars, even Loukotka, had a large extension into the neighboring areas of Central America. This was part of a larger hypothesis that all three of these South American groupings along with a major portion of the languages of North America belonged to one large Amerind family. Details regarding the membership and the internal subgroupings of the newly proposed South American families were given. The only groups not belonging to Amerind were stated to be Na-Dene and Aleut-Eskimo, so that all the languages of the Americas fell into three groups. The paper itself was a brief one in which the classification of South American languages was presented in tabular form without supporting evidence and other groups were merely named.

This paper was not published until four years later. In the meantime Lamb (1959) independently, but without including South America, arrived at a classification identical with that of Greenberg, namely, that the languages of North America could be divided genetically into three groups: Aleut-Eskimo, Na-Dene, and the rest. As with Greenberg, no supporting evidence was presented.

In a later paper (1979), Greenberg presented the same basic thesis in a somewhat revised form insofar as it related to Amerind and included details on North America. Some supporting grammatical evidence was presented in regard to Amerind. Moreover, Aleut-Eskimo, for which various relationships in northern Asia had often been suggested, was asserted to belong to a Eurasiatic grouping consisting of Aleut-Eskimo, Chukotian, Gilyak (Nivkhi), Japanese, Korean, Ainu, Altaic, Uralic-Yukaghir, and Indo-European.

Beginning about 1960, Greenberg began to compile a vast data base on the vocabulary and grammar of Amerind languages. The vocabulary data are contained in 23 notebooks reproduced in the Stanford Library and available on interlibrary loan. The detailed results in Greenberg (1986) include etymologies for each of the 11 subgroups of Amerind, totaling approximately 1,900, and 300 involving two or more of these 11 subgroups. A chapter on grammatical markers contains approximately 100 items.

The 11 subgroups are (1) Macro-Ge, (2) Macro-Panoan, (3) Macro-Carib, (4) Equatorial, (5) Macro-Tucanoan, (6) Andean, (7) Chibchan-Paezan, (8) Central Amerind, (9) Hokan, (10) Penutian, and (11) Almosan-Keresiouan. These groups may be briefly outlined in the following manner: Macro-Ge is essentially as in Greenberg (1960) with the inclusion of Nambicuar. Macro-Panoan is also virtually identical with that subfamily as outlined in the same source, as are Macro-Carib and Andean. The former Equatorial is split into two groups, one consisting of the languages most closely related to Tucanoan and the other of the remainder. Chibchan-Paezan includes some additional members, notably Huarpe in the south, Tarascan in the north, and Timucua, an extinct language of Florida included in the Paezan branch and most closely related to the languages of Venezuela. Central Amerind has three coordinate branches: Oto-Mangue, Uto-Aztecan, and Kiowa-Tanoan. Hokan is essentially "traditional" Hokan-Coahuiltecan, Sapir's V.1. Penutian embraces, besides the languages generally reckoned as Penutian (including here

Mexican Penutian), Yukian and the Gulf languages. Northern Amerind has two branches: one of these is identical with Sapir's Algonkin-Wakashan and the other is Keresiouan, consisting of Keresan, Siouan, Yuchi, Caddoan, and Iroquoian.

There are some clear subgroupings among the 11 groups. One consists of Macro-Ge, Macro-Panoan, and Macro-Carib and can be equated with the Ge-Pano-Carib of Greenberg (1960). A second consists of Equatorial and Macro-Tucanoan, thus forming a substantial portion of Equatorial-Andean of the former classification. A northern group consists of Hokan, Penutian, and Keresiouan. There may be a special connection between Andean and Chibchan-Paezan. Central Amerind seems to stand somewhat apart from the rest.

The following historical inferences concerning the settlement of the New World may be derived from the above classification: There were three migrations, or at least only three left linguistic traces. The oldest is probably Amerind, since it centers farther to the south than the others and shows greater internal differentiation. On the other hand, the absence of sharper linguistic differentiation in the north suggests a relatively rapid spread allowing no opportunity for the development of one or more strongly divergent subgroups. Should it, however, prove valid that Central Amerind forms a separate subgroup from all the rest, there would have to be a period relatively far to the north during which this divergence took place.

Na-Dene has deeper internal divisions and is geographically less peripheral than Aleut-Eskimo. In particular, the break between Haida and the remainder of Na-Dene requires a considerable time depth. The internal divisions of Na-Dene are (1a) Athapaskan-Eyak, (1b) Tlingit, and (2) Haida. The spread of the Athapaskans from interior Northwest Canada and Alaska to California, Oregon, and the Southwest must be very recent, probably within the last 1,000 years. The largest number of distinct subgroups of Athapaskan is in the northwest, and its nearest relative is Eyak, spoken now or formerly in two southern Alaskan coastal areas, the Copper River Delta and Yakutat. The geographical locations of Tlingit and Haida further reinforce the thesis that the origins of Na-Dene are to be sought in the general area of southeastern Alaska and northern British Columbia.

Aleut-Eskimo is probably the most recent. Its primary internal division is less deep than that within Na-Dene. One indication is that no one has publicly doubted the relationship between Aleut and Eskimo whereas, as has been seen, the affiliation of Haida to the rest of Na-Dene has been seriously questioned. Both the internal and the external evidence point to the original habitat of the Aleut-Eskimo as being on the southwestern coast of Alaska. Within Eskimo itself, the vast Inuit distribution from Central Alaska to Greenland with shallow internal differences suggests a very recent migration from the far western end of the present distribution. The other branch, Yuit, is found in the central and southwestern coastal areas of Alaska. It is also spoken in Siberia. If Sirenik, which in certain respects is drastically different from the rest of Eskimo, should prove to have separate genetic status, it even becomes plausible that Eskimo or even Aleut-Eskimo originated in northeastern Siberia. However, the position of Aleut suggests rather an origin east of Bering Strait with Siberian Eskimo as a subsequent reflux. The external evidence, with Aleut-Eskimo on the extreme eastern end of a vast Eurasian distribution, likewise supports an origin in either the extreme northwest of the Americas or northeastern Siberia. Although it seems most probable linguistically that the Aleut-Eskimo migration was the most recent, it is defensible to hypothesize that the Proto-Aleut-Eskimo community arrived in the extreme northwest as a still internally undifferentiated unit before the coming of the Na-Dene.

We have hypothesized that the three linguistic stocks represent separate migrations. They differ greatly, and there is little

likelihood that they are branches of a single linguistic stock. The determination of still deeper relationships regarding the affiliations of Eskimo-Aleut and Na-Dene would, of course, constitute important evidence concerning the point of Old World origin of these groups. There are some indications that Eurasian and Amerind are closer genetically than either is to Na-Dene. On the basis of a suggestion of Sapir, Shafer (1952) presented some evidence for a connection of Na-Dene with Sino-Tibetan. Both of these proposals require further investigation. If they should prove to be true, they would point to a more southerly ultimate place of origin in Asia.

Three lines of evidence which can be used to arrive at absolute dates are prehistory (by C^{14} and other means), phylogenies based on genetic data, and glottochronology, a purely linguistic technique. The last will be considered here in connection with the linguistic evidence, but, for reasons which will appear, it is the least cogent of the three.

Glottochronology, the one available method for dating the separation of linguistic stocks in the absence of written records, was first devised about 1950 (Hymes 1960). By examining the rate of retention of a specific list of 200 words it was determined that slightly more than .80 of this list was retained over 1,000 years. If two languages diverged from a single ancestral language and evolved independently, after 1,000 years they would be expected to have related forms in approximately $.80^2$ of the list. The process was hypothesized to follow a decay function so that after n millennia the proportion of cognate forms between two languages would be $.8^{2n}$.

Unfortunately, there are several major problems with this method. To begin with, the documented cases suggest that the probable error is quite high. Second, the separation dates for long periods are seriously underestimated by the assumption of a homogeneous rate of retention of the entire list; it is well established that certain words such as personal pronouns have a far higher retention rate than others. An alternative inhomogeneous rate was proposed by Joos (1964). This was arrived at by splitting the original list into eight sublists with differing retention rates. Because of incomplete data, a shorter list of 100 words with a retention rate of .86 per millennium was more often used, and even this list was often incomplete. The rate constant has also been revised and recalculated a number of times. Moreover, when the time of separation is great (i.e., approaches 10,000 years) differences of judgment regarding even one cognate will produce substantially different results. Because of these and other problems, the method has been almost completely abandoned for purposes of the dating of times of origin of ancestral languages, though it is still occasionally used for arriving at the subgroupings of a linguistic stock (Hattori 1973, Miller 1984). For the linguistic families with which we are concerned here, no substantially new counts have been published during the last two decades, although old data and reinterpretations have been discussed and reworked (Krauss 1973).

With all these reservations in mind, we may consider the relevant results. Dates for the Aleut-Eskimo divergence have ranged from 2,900 to 5,600 B.P. and have tended to cluster about 4,000 B.P. This variability depends for the most part on changes in the mathematical assumptions of glottochronology rather than a more extended or reliable data base. In regard to Na-Dene, Swadesh arrived at 9,000 B.P. as the date of Proto-Na-Dene by comparing Haida, the most distant language, with other languages of the group; however, in subsequent publications he drastically reduced the date to 4,700 B.P. (1959, 1962). The earlier date would seem more probable. Krauss (1973) estimates 5,000 B.P. for the Tlingit-Athapaskan divergence, a much closer relationship than that of Haida to the other Na-Dene languages. We are even less secure regarding dates for Amerind, which would have to be based on com-

parisons of languages that belong to different subgroups in Greenberg's classification. Here again the work has been done by Swadesh, but results subsequent to Swadesh (1958) are unusable because the actual cognate percentages and the sizes of the lists used are not given. Moreover, these unpublished lists have been subject to mathematical manipulations which have not been specified in detail. In addition, Swadesh tended to see more related forms than other linguists. His figures for Amerind languages belonging to different subgroups in the Greenberg classification cluster at about 7,000–10,000 B.P., which when adjusted by the Joos function becomes 9,000–11,000 B.P. Our opinion is that for Amerind we are dealing with a time period probably greater than 11,000 years and beyond the limits of glottochronology (Greenberg, Turner, and Zegura 1985).

THE DENTAL EVIDENCE

Excellence of preservation, the existence of numerous independent traits, genetic determination, evolutionary conservatism, between-group variation, and ease of observation are some of the qualities that make teeth a major source of direct and diachronic information on past and present population histories and relationships. Depending on the taxonomic level under consideration, differing sets of dental morphological traits can be considered. At the macroevolutionary level, that is, above species, size of canine, cingulum form, and other traits are useful for assessing affinity and descent within the primate order. At the microevolutionary level, that is, within a narrowly defined population such as the Aleut-Eskimo system, other traits like lower first molar root number and degree of incisor shoveling are valuable for recognizing changes over very short periods of time. It is at the mesoevolutionary level, by which we mean taxonomic units above the regional group and below the species, that a battery of 28 key crown and root traits has been developed to address specifically the question of Native American origins. Examples of this battery include incisor shoveling and Carabelli's trait, as well as less well-known features such as the Uto-Aztecan premolar variant (Morris, Hughes, and Dahlberg 1978). Anatomical variation within each of these traits is scored with a standard reference plaque or other strict observation criteria. The full battery of traits can be found in Turner (1985), along with dichotomized frequencies of each for approximately 9,000 variously complete prehistoric Native American crania examined by Turner between 1975 and 1984.¹

Dental variation within and between the Americas and Eurasia. Table 1 provides a ranked ordering of multivariate mean measures of divergence (MMDs) for the entire New World and comparative Old World population samples. These newly calculated MMDs differ slightly from previously published values because of minor changes in the dichotomizing breakpoints for some traits and increased sample sizes for some groups, particularly Eskimo. All but 23 of the 276 MMDs are statistically significant, and 18 of the 23 nonsignificant values are caused by the Athapaskan sample, which has the smallest mean number of individuals per trait and the least certain

¹ The dental information presented here was gathered and analyzed with help from the National Geographic Society, National Science Foundation, IREX (International Research and Exchanges Board), U.S.S.R. Academy of Science, and Arizona State University. Some of the archaeological information is from Turner's excavations in Alaska and museum studies throughout the Americas and in the U.S.S.R. Linda Nuss assisted with data processing. This is contribution no. 25 to his Peopling of the Pacific Basin and Adjoining Areas Series. A major dental report is in preparation, in which all findings and acknowledgments will be given.

provenience of the 24 series. Table 2 and figure 1 summarize sample information for each series.

Four observations are noteworthy: (1) All New World groups resemble each other more than they do most Old World populations (a small MMD indicates greater similarity than a larger value). (2) Dental variation is greater in the north than in the south. (3) New World groups are more like Asians than like Europeans. (4) Aleut-Eskimos, Greater Northwest Coast Indians (Na-Dene), and all other Indians (Macro-Indian) form three New World dental clusters.

From these four observations and other information, a dental hypothesis has been developed for the peopling of the Americas that envisions three distinct late Pleistocene migrations from Siberia (Turner 1971, 1983a, 1983b, 1985, 1986). Starting with the first observation, it is clear that all Native American groups had their ancestral origin in one Northeast Asian population system, since all are similar and possess what Turner has termed the Sinodont dental pattern rather than the Southeast Asian Sundadont or the European dental pattern (Turner 1983a). This interpretation is consistent with other biological evidence such as broad facial form, straight black hair, Mongoloid sacral spot, and numerous other anatomical and biochemical features that indicate a common Asian ancestry for all Native Americans (Hrdlička 1913, Laughlin 1963, Stewart 1973). Dental morphology is in perfect accord with all other biological evidence pointing to an Asian origin. The teeth are even more geographically specific, as they reject Southeastern Asia as a possible homeland. Additional dental data point to North China as the ultimate ancestral homeland for all Native Americans (Turner 1985).

Because New World dental variation is greater in the north than in the south (for example, the Aleut-Eskimo MMD of 0.040 is greater than the 0.011 MMD for South America-Mesoamerica), it is evident that the peopling of the Americas proceeded from Alaska southward. Variation should be greatest where populations have resided the longest period of time. This finding is consistent with archaeological (Jennings 1978, West 1981, Wormington 1983) and paleoenvironmental findings (Hopkins 1979, Hopkins et al. 1982) that indicate human colonization of the New World by way of the now-submerged Bering land bridge. Only in Alaska have all three of the earliest well-founded New World stone tool traditions been discovered: (1) bifacially chipped basally thinned or fluted points, absence of microblades (early component of Dry Creek [Powers, Guthrie, and Hoffecker 1983]); (2) bifacially chipped but unfluted points, presence of microblades (later component of Dry Creek, Groundhog Bay, various Denali sites, and others [West 1981, Ackerman 1983]); (3) macro- to microblades, absence of bifacially chipped tools (Anangula, Aleutians [Laughlin 1963]). Outside of Alaska and western Canada only the first tradition has been found (Dumond 1980, Stanford 1982, Bryan 1983).

The third observation, namely, that New World teeth are more like those of eastern Asia than like those of Europe, provides a solid basis for challenging the archaeological view that Paleo-Indians originated in Europe because their methods of stone tool manufacturing were like those used by late Pleistocene Cro-Magnon hunters such as the Kostienki or Sunghir tribes. The four European samples of table 1 and figure 1 are very similar among themselves and least like the New World groups. There is no support in this genetically sensible spatial pattern for theorizing that Native Americans originated in Europe or that they are some form of European-Asian hybridization. Examining the MMDs between South America and all other groups neatly shows that divergence is substantially correlated with geographic distance. The same is generally true for all other New World groups, even the Athapaskans.

This geographic-distance/dental-divergence relationship also indicates the chief probable cause of dental microevolution. Although regularity in the MMD distribution is sugges-

TABLE 1
RANKED ORDERING OF MEAN MEASURES OF DIVERGENCE (28 TRAITS)

<i>South America</i>	<i>Mesoamerica</i>	<i>Southwest U.S.</i>
.011 Mesoamerica	.011 South America	.027 E U.S. & Canada
.016 California	.025 California	.032 NW U.S. & Canada
.032 E U.S. & Canada	.037 E U.S. & Canada	.033 Archaic Canada
.043 SW U.S.	.048 SW U.S.	.037 California
.056 Archaic Canada	.070 Archaic Canada	.043 South America
.058 NW U.S. & Canada	.081 Gulf Alaska	.048 Mesoamerica
.062 Gulf Alaska	.086 NW U.S. & Canada	.048 Athapaskan
.082 Athapaskan	.115 NE Siberia	.049 Gulf Alaska
.100 NE Siberia	.117 Athapaskan	.064 NE Siberia
.117 Amur	.142 Aleut	.080 Japan
.120 Japan	.151 Amur	.095 Aleut
.122 Aleut	.152 Japan	.104 Eskimo
.132 Eskimo	.159 Eskimo	.114 Hiogo Japan
.152 Hiogo Japan	.189 Hiogo Japan	.127 Amur
.164 Japan recent	.196 Japan recent	.128 Japan recent
.177 Urga & Mongol 2	.216 Urga & Mongol 2	.132 Urga & Mongol 2
.199 An-yang	.255 An-yang	.136 An-yang
.250 Recent Thai	.273 Recent Thai	.173 Recent Thai
.298 Early Thai	.320 Early Thai	.213 Early Thai
.461 NW Europe	.466 NW Europe	.372 NW Europe
.594 Poundbury	.609 Poundbury	.494 Poundbury
.632 Danish Neolithic	.651 Holland	.533 Danish Neolithic
.637 Holland	.652 Danish Neolithic	.555 Holland
<i>California</i>	<i>E U.S. & Canada</i>	<i>NW U.S. & Canada</i>
.012 E U.S. & Canada	.012 California	.013 Gulf Alaska
.016 South America	.027 SW U.S.	.023 Archaic Canada
.025 Mesoamerica	.032 South America	.024 Athapaskan
.037 SW U.S.	.035 Archaic Canada	.032 SW U.S.
.050 Archaic Canada	.037 Mesoamerica	.046 E U.S. & Canada
.054 NW U.S. & Canada	.046 NW U.S. & Canada	.049 Eskimo
.064 Gulf Alaska	.058 Athapaskan	.054 California
.094 Athapaskan	.064 Gulf Alaska	.056 NE Siberia
.101 NE Siberia	.096 NE Siberia	.058 Aleut
.111 Japan	.102 Japan	.058 South America
.138 Hiogo Japan	.129 Aleut	.081 Japan
.143 Aleut	.141 Urga & Mongol 2	.086 Mesoamerica
.145 Eskimo	.144 An-yang	.112 Urga & Mongol 2
.151 Amur	.146 Eskimo	.113 Hiogo Japan
.158 Japan recent	.146 Hiogo Japan	.113 Amur
.169 An-yang	.155 Amur	.118 Japan recent
.178 Urga & Mongol 2	.168 Japan recent	.125 An-yang
.233 Recent Thai	.193 Recent Thai	.170 Recent Thai
.286 Early Thai	.219 Early Thai	.225 Early Thai
.467 NW Europe	.420 NW Europe	.409 NW Europe
.593 Poundbury	.544 Poundbury	.516 Poundbury
.616 Danish Neolithic	.551 Danish Neolithic	.539 Danish Neolithic
.662 Holland	.612 Holland	.571 Holland
<i>Archaic Canada</i>	<i>Athapaskan</i>	<i>Gulf Alaska</i>
.023 NW U.S. & Canada	.024 NW U.S. & Canada	.013 NW U.S. & Canada
.033 SW U.S.	.031 Aleut	.024 NE Siberia
.035 E U.S. & Canada	.047 Gulf Alaska	.032 Eskimo
.050 California	.048 SW U.S.	.037 Aleut
.056 South America	.058 Archaic Canada	.047 Athapaskan
.057 Gulf Alaska	.058 E U.S. & Canada	.049 SW U.S.
.058 Athapaskan	.070 Urga & Mongol 2	.057 Archaic Canada
.069 NE Siberia	.071 Japan	.062 South America
.070 Mesoamerica	.072 Amur	.064 E U.S. & Canada
.083 Eskimo	.074 NE Siberia	.064 California
.092 Aleut	.084 South America	.073 Japan
.096 Japan	.090 An-yang	.081 Mesoamerica
.129 Amur	.093 Eskimo	.093 Amur
.144 Hiogo Japan	.094 California	.093 Hiogo Japan
.145 An-yang	.100 Hiogo Japan	.098 Japan recent
.157 Urga & Mongol 2	.117 Mesoamerica	.101 Urga & Mongol 2
.158 Japan recent	.120 Japan recent	.145 An-yang
.188 Recent Thai	.141 Recent Thai	.190 Recent Thai
.222 Early Thai	.180 Early Thai	.240 Early Thai
.396 NW Europe	.379 NW Europe	.366 NW Europe
.513 Poundbury	.481 Poundbury	.484 Poundbury
.524 Danish Neolithic	.520 Holland	.506 Danish Neolithic
.570 Holland	.527 Danish Neolithic	.531 Holland

TABLE 1 (Continued)

<i>Aleut</i>	<i>Eskimo</i>	<i>NE Siberia</i>
.031 Athapaskan	.031 NE Siberia	.024 Gulf Alaska
.032 NE Siberia	.032 Gulf Alaska	.031 Eskimo
.037 Gulf Alaska	.040 Aleut	.032 Aleut
.040 Eskimo	.049 NW U.S. & Canada	.056 NW U.S. & Canada
.058 NW U.S. & Canada	.083 Archaic Canada	.064 SW U.S.
.092 Archaic Canada	.090 Hiogo Japan	.069 Archaic Canada
.095 SW U.S.	.093 Athapaskan	.072 Japan
.107 Japan	.101 Japan	.074 Hiogo Japan
.112 Hiogo Japan	.102 Japan recent	.074 Athapaskan
.114 Amur	.104 SW U.S.	.096 E U.S. & Canada
.115 Japan recent	.106 Amur	.096 Japan recent
.118 Urga & Mongol 2	.132 South America	.100 South America
.122 South America	.145 California	.101 California
.129 E. U.S. & Canada	.146 E U.S. & Canada	.106 Amur
.142 Mesoamerica	.159 Mesoamerica	.115 Mesoamerica
.143 California	.165 Urga & Mongol 2	.140 Urga & Mongol 2
.161 An-yang	.179 Recent Thai	.141 An-yang
.203 Recent Thai	.190 An-yang	.148 Recent Thai
.248 Early Thai	.250 Early Thai	.190 Early Thai
.381 NW Europe	.352 NW Europe	.309 NW Europe
.479 Poundbury	.431 Poundbury	.401 Poundbury
.519 Holland	.472 Danish Neolithic	.421 Danish Neolithic
.521 Danish Neolithic	.500 Holland	.472 Holland
<i>Amur</i>	<i>Urga & Mongol 2</i>	<i>An-yang</i>
.062 Japan	.023 Japan	.027 Japan
.072 Athapaskan	.031 An-yang	.031 Urga & Mongol 2
.090 Hiogo Japan	.067 Hiogo Japan	.066 Hiogo Japan
.093 Gulf Alaska	.070 Athapaskan	.090 Athapaskan
.095 Japan recent	.084 Japan recent	.105 Japan recent
.106 NE Siberia	.101 Gulf Alaska	.125 NW U.S. & Canada
.106 Eskimo	.112 NW U.S. & Canada	.131 Recent Thai
.113 NW U.S. & Canada	.118 Aleut	.136 SW U.S.
.114 Aleut	.126 Recent Thai	.138 Early Thailand
.117 South America	.129 Amur	.141 NE Siberia
.127 SW U.S.	.132 SW U.S.	.144 E U.S. & Canada
.129 Urga & Mongol 2	.134 Early Thailand	.145 Archaic Canada
.129 Archaic Canada	.140 NE Siberia	.145 Gulf Alaska
.151 Mesoamerica	.141 E U.S. & Canada	.161 Aleut
.151 California	.157 Archaic Canada	.169 California
.155 Recent Thai	.165 Eskimo	.169 Amur
.155 E U.S. & Canada	.177 South America	.190 Eskimo
.169 An-yang	.178 California	.199 South America
.241 Early Thailand	.216 Mesoamerica	.255 Mesoamerica
.354 NW Europe	.335 NW Europe	.441 NW Europe
.484 Poundbury	.410 Danish Neolithic	.508 Danish Neolithic
.511 Danish Neolithic	.424 Poundbury	.519 Poundbury
.514 Holland	.476 Holland	.614 Holland
<i>Japan</i>	<i>Hiogo Japan</i>	<i>Japan recent</i>
.000 Hiogo Japan	.000 Japan recent	.000 Hiogo Japan
.018 Japan recent	.000 Japan	.018 Japan
.023 Urga & Mongol 2	.054 Recent Thai	.058 Recent Thai
.027 An-yang	.066 An-yang	.084 Urga & Mongol 2
.062 Amur	.067 Urga & Mongol 2	.095 Amur
.071 Athapaskan	.074 NE Siberia	.096 NE Siberia
.072 NE Siberia	.090 Eskimo	.098 Gulf Alaska
.072 Recent Thai	.090 Amur	.102 Eskimo
.073 Gulf Alaska	.093 Gulf Alaska	.105 An-yang
.080 SW U.S.	.100 Athapaskan	.115 Aleut
.081 NW U.S. & Canada	.101 Early Thai	.118 NW U.S. & Canada
.096 Archaic Canada	.112 Aleut	.120 Athapaskan
.101 Eskimo	.113 NW U.S. & Canada	.123 Early Thailand
.102 E U.S. & Canada	.114 SW U.S.	.128 SW U.S.
.107 Aleut	.138 California	.158 California
.111 California	.144 Archaic Canada	.164 South America
.112 Early Thai	.146 E U.S. & Canada	.168 Archaic Canada
.120 South America	.152 South America	.168 E U.S. & Canada
.152 Mesoamerica	.189 Mesoamerica	.196 Mesoamerica
.293 NW Europe	.233 Europe	.227 NW Europe
.390 Danish Neolithic	.303 Danish Neolithic	.322 Danish Neolithic
.407 Poundbury	.304 Poundbury	.329 Poundbury
.473 Holland	.376 Holland	.380 Holland

TABLE 1 (Continued)

<i>Recent Thai</i>	<i>Early Thai</i>	<i>NW Europe</i>
.013 Early Thai	.013 Recent Thai	.019 Danish Neolithic
.054 Hiogo Japan	.101 Hiogo Japan	.024 Poundbury
.058 Japan Recent	.112 Japan	.028 Holland
.072 Japan	.123 NW Europe	.123 Early Thai
.126 Urga & Mongol 2	.123 Japan recent	.135 Recent Thai
.131 An-yang	.134 Urga & Mongol 2	.227 Japan recent
.135 NW Europe	.135 Danish Neolithic	.233 Hiogo Japan
.141 Athapaskan	.138 An-yang	.293 Japan
.148 NE Siberia	.171 Poundbury	.309 NE Siberia
.155 Amur	.180 Athapaskan	.335 Urga & Mongol 2
.165 Danish Neolithic	.190 NE Siberia	.352 Eskimo
.170 NW U.S. & Canada	.213 NW U.S.	.354 Amur
.173 SW U.S.	.219 E U.S. & Canada	.366 Gulf Alaska
.179 Eskimo	.222 Archaic Canada	.372 SW U.S.
.188 Archaic Canada	.224 Holland	.379 Athapaskan
.190 Gulf Alaska	.225 NW U.S. & Canada	.381 Aleut
.193 E U.S. & Canada	.240 Gulf Alaska	.396 Archaic Canada
.197 Poundbury	.241 Amur	.409 NW U.S. & Canada
.203 Aleut	.248 Aleut	.420 E U.S. & Canada
.233 California	.250 Eskimo	.441 An-yang
.250 South America	.286 California	.461 South America
.263 Holland	.298 South America	.466 Mesoamerica
.273 Mesoamerica	.320 Mesoamerica	.467 California
<i>Poundbury</i>	<i>Holland</i>	<i>Danish Neolithic</i>
.004 Holland	.004 Poundbury	.009 Poundbury
.009 Danish Neolithic	.028 NW Europe	.019 NW Europe
.024 NW Europe	.046 Danish Neolithic	.046 Holland
.171 Early Thai	.224 Early Thai	.135 Early Thai
.197 Recent Thai	.263 Recent Thai	.165 Recent Thai
.304 Hiogo Japan	.376 Hiogo Japan	.303 Hiogo Japan
.329 Japan recent	.380 Japan recent	.322 Japan recent
.401 NE Siberia	.472 NE Siberia	.390 Japan
.407 Japan	.473 Japan	.410 Urga & Mongol 2
.424 Urga & Mongol 2	.484 Urga & Mongol 2	.421 NE Siberia
.431 Eskimo	.500 Eskimo	.472 Eskimo
.479 Aleut	.514 Amur	.506 Gulf Alaska
.481 Athapaskan	.519 Aleut	.508 An-yang
.484 Gulf Alaska	.520 Athapaskan	.511 Amur
.484 Amur	.531 Gulf Alaska	.521 Aleut
.494 SW U.S.	.555 SW U.S.	.524 Archaic Canada
.513 Archaic Canada	.570 Archaic Canada	.527 Athapaskan
.516 NW U.S. & Canada	.571 NW U.S. & Canada	.533 SW U.S.
.519 An-yang	.612 E U.S. & Canada	.539 NW U.S. & Canada
.544 E U.S. & Canada	.614 An-yang	.551 E U.S. & Canada
.593 California	.637 South America	.616 California
.594 South America	.651 Mesoamerica	.632 South America
.609 Mesoamerica	.662 California	.652 Mesoamerica

NOTE: Traits and scoring procedures are in Turner (1985). Some of the dichotomizing breakpoints differ in the present paper.

tive of clinal variation, which is commonly attributed to graduated selection, there are no identifiable potential dental selective pressures from north to south in the Americas or east to west in Eurasia. Moreover, no convincing selective advantage has yet been identified for these dental traits, certainly none that gives reproductive advantage proportional to the magnitude of the MMD range. None of these traits is known to be associated with any other physical or biochemical trait that selection might have been working on. Given the small band sizes that must have been involved in the colonizing of Siberia and the Americas and the fact that all must have been rather closely related, population structure must be the underlying condition for the distance-divergence relationship. The small Asian-American MMDs support a rapid expanding-front colonization model like that proposed by Martin (1984).

New World dental variation forms three clusters: (1) Aleut-Eskimo, (2) Greater Northwest Coast or Na-Dene (in table 1 this division is made up of Southwest United States and Canada, Gulf of Alaska, and Athapaskan), and (3) all other

North and South American Indians (Macro-Indian). This can be appreciated by inspecting the dental dendrogram (fig. 1) based on the 28-trait MMD matrix clustered with the unweighted-pair-group, arithmetic-averages method. Aleuts, Eskimos, and Northeast Siberian Eskimos, Chukchi, and Koryak form one division. The clustering procedure has linked the Athapaskans with the Bering Sea Mongoloids even though the smallest Athapaskan MMD (0.024, table 1) is with the Northwest Coast Indians. A second cluster is the Greater Northwest Coast group shown as Gulf of Alaska and Northwest U.S. & Canada in figure 1. Most other North and South American Indians make up the third division. Archaic Canada and Southwest United States cluster anomalously, probably because both are pulled northward by having some "Na-Dene" crania in each series. This is almost certain for the Southwest, since it includes crania from Pecos and San Cristobal Pueblos that had Apache and Navajo contact, trade, and intermarriage (Gunnerson 1979). Archaic Canada is weighted numerically towards a greater interior-western than eastern representation.

TABLE 2
DENTAL GROUP COMPOSITION AND SAMPLE SIZES

GROUP	SAMPLE SIZE	MEAN	S.D.
1. South America: Panama 1 and 2, Ecuador (Ayalan, Cotacollao, Santa Elena, Valdivia/Chanduy), Peru (Preceramic, Paloma, Peru 1 and 2), Bolivia, Chile (Patagonia, Herradura, Cuchipuy, Punta Teatinos), Brazil (Lagoa Santa 1-3, Corondo, Sambaqui north and south, Minas Gerais)	254-1,203	689.1	259.0
2. Mesoamerica: Mexico (Coahuila, Tlatelolco, Tehuacán, Cuicuilco, Chichén Itzá)	66-367	222.1	90.0
3. Southwest United States: Utah (Cottonwood Canyon, Grand Gulch), Arizona (Canyons de Chelly and del Muerto, Kayenta, Chevez Pass, Point of Pines, Grasshopper, Mogollon), New Mexico (San Cristobal, Pecos)	172-628	429.1	104.0
4. California: Southern, Humboldt County, Sacramento County, Alameda County, Archaic (Tranquility, SJO-68)	40-292	137.5	65.5
5. Eastern United States and Canada: Iroquois (Toronto, Roebuck), Maryland (Nanjemoy/Juhle, Ossuaries 2 and 4), Arkansas (Quapaw, Togo, Golightly, Wapanoca, Vernon Paul, Nodena), Alabama	140-708	453.8	140.2
6. Northwest United States and Canada: Northern Maritime 1-4 (Namu), Central Maritime 1-3, Gulf of Georgia and Puget Sound 1-3, Lower Columbia River	57-475	250.0	127.1
7. Archaic Canada: Saskatchewan, Quebec	18-124	59.9	26.9
8. Athapaskan: Yukon, Inter-Mountain Fraser, Apache	0-100	41.9	27.9
9. Gulf of Alaska: Kachemak, Kodiak (Uyak), Alaska Peninsula	42-170	109.0	30.7
10. Aleut: Eastern, Western	17-273	122.9	71.8
11. Eskimo: St. Lawrence, Point Hope, Point Barrow 1 and 2, Mackenzie, Southampton, Smith Sound, East and West Greenland	117-786	370.4	197.3
12. Northeast Siberia: Ekven; Uelen, Chukchi; Eastern Siberia ("Koryak")	22-264	103.5	65.0
13. Amur: Ulchi, Goldi, Orochi, Negedal, Tungus, Gilyak	14-111	51.9	25.4
14. Urga and Mongol 2	14-150	81.5	34.3
15. An-Yang (North China)	8-224	127.6	46.2
16. Japan	20-138	84.7	35.9
17. Hiogo Japan	13-96	60.4	25.6
18. Japan recent	43-110	82.4	16.5
19. Thai recent (Bangkok)	43-133	90.4	24.0
20. Early Thailand: Don Klang/Ban Tong, Ban Kao, Non Nok Tha, Ban Na Di, Central Thailand, Ban Chiang	43-237	139.9	46.1
21. Northwest Europe: (U.S.S.R.) Lapp, Reindeer Island, Karilian Peninsula, U.S.S.R. Upper Paleolithic	19-136	82.2	33.1
22. Danish Neolithic	11-68	42.2	15.5
23. Holland: Dorestad de Huel, Lent	22-96	53.8	20.7
24. Poundbury (England)	34-119	82.1	22.7

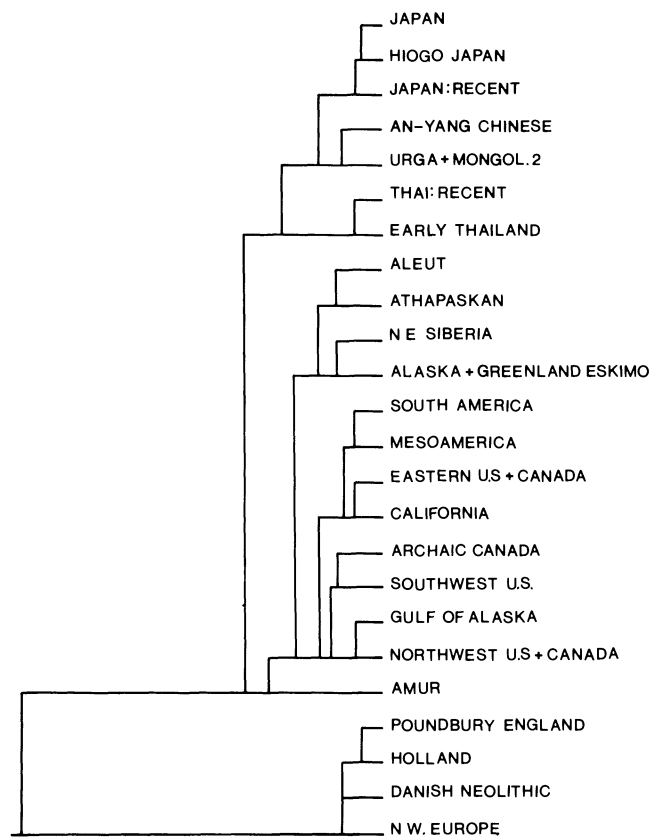


FIG. 1. Dental dendrogram.

Na-Dene-speakers may have been ranging well outside Alaska in Archaic times (Dumond 1969). An important dental quality of the Greater Northwest Coast group is the intermediate frequencies of traits when compared with Aleut-Eskimo and Macro-Indian. This quality has been discussed in detail elsewhere (Turner 1985). It is thought to be due to the ancestral Siberian condition instead of some form of New World clinal selection, past hybridization between Paleo-Indian and ancestral Aleut-Eskimo, or sampling error.

Dental correspondences with language and prehistory. From the similar amount of divergence between Southeast Asia and Northeast Asia (Recent Thai/Recent Japan, 0.06 MMD), within Europe (Holland/Danish Neolithic, 0.046 MMD), or within Macro-Indian (South America/Archaic Canada, 0.056 MMD), it is clear that the peopling of the Americas was a relatively recent event (see also Turner and Bird 1981). Moreover, it has been proposed that the worldwide rate of dental microevolution is about 0.01 MMD/1,000 years (Turner 1985, n.d.), and inspection of table 1 shows that when this rate is applied to the Macro-Indian samples, they have been separated from Northeast Asian, on the average, for 14,000 years. This corresponds well with the widely held view that the first Americans were the Clovis-culture big-game-hunting Paleo-Indians who reached the southwestern United States 12,000 years ago. While there are claims for pre-Clovis occupation in the New World going back to 30,000 or more years, most archaeology (Owen 1984) and the dental divergence rate are unresponsive. It is possible that a pre-Clovis group entered the New World which did not leave any descendants among recent Indians, but this is not a parsimonious way to explain away the Clovis-dental-time relationship. Given the proven reproductive success of the Paleo-Indian colonizers and their dogs, let alone that of the many subsequent successful animal and plant colonizers of the New World, what mechanism would have to

be proposed to explain the reproductive failure of the hypothetical pre-Clovis people? The point to be made here is that there is no body of agreed-upon evidence that requires a pre-Clovis migration, and the small amount of Macro-Indian dental divergence supports a relatively late initial peopling of the New World by an absolutely small original founding group from which all later Macro-Indian languages and cultural systems evolved in situ.

Ancestral Aleut-Eskimo entered as a distinct population already differentiated in Siberia and pursuing an ecologically isolated coastal lifeway. This proposition has been fully developed by Laughlin (1975, 1980, and elsewhere) and need not be repeated here except to emphasize that Macro-Indian and Aleut-Eskimo must have had a common ancestor, suggested to have lived in North China about 20,000 years ago (Turner 1985 and elsewhere). Given the hundreds of derived Macro-Indian languages and the relatively few proposed for Aleut-Eskimo, it would appear that the latter reached North America much later than the former. However, the known severity of the arctic environment, the many diverse archaeological cultures of Alaska, small population size, and the pattern of linear coastal settlement equally suggest that fewer languages could evolve and the language extinction rate was greater in the far north than elsewhere in the New World. In addition, Aleut-Eskimo teeth are just about as divergent from Chinese and Japanese as are Macro-Indians—an unlikely condition if Aleut-Eskimo reached Alaska around 5,000 years ago as was thought before it was appreciated that the Hypsithermal rise in Holocene sea level must have destroyed most evidence of Alaskan coastal occupation before that time (Turner 1985).

There is a remarkably good fit between Greenberg's Macro-Indian and Aleut-Eskimo linguistic divisions and the dental clusters so far proposed. However, the fit between Na-Dene-speakers and the Greater Northwest Coast or Na-Dene dental group is not as precise. The Na-Dene-speakers have a much more limited geographic distribution than does the dental cluster. There are several possible explanations, although it should not be overlooked that Greenberg and Turner both envision the Gulf of Alaska coast as the center of Na-Dene language and dental groups. First, the Na-Dene language distribution may have shrunk a great deal in the last three or four millennia. The teeth, particularly those from Namu (Carlson 1983), are well dated as prehistoric and provide a diachronic basis for proposing a more southerly limit for the Na-Dene speech community than exists today on the Northwest Coast. In this regard it is noteworthy that Athapaskan-speaking Indians had reached northern California by at least protohistoric times (Gould 1978). Second, the great amount of Northwest Coast Indian trading, slaving, and exogamous social organization may have spread Na-Dene-sourced dental genes well beyond the language boundary. This has been suggested for the more northerly link-up of the Southwest United States and Archaic Canada dental groups. Whatever underlies the lack of precise fit between the Greater Northwest Coast dental and Na-Dene language units, it hardly detracts from the basic fact that both Greenberg and Turner recognize three geographically correlated divisions of Native Americans. Supportive of this dental and linguistic correlation is the distribution of the three previously mentioned early New World archaeological stone tool traditions—Paleo-Indian, Denali or Paleo-Arctic, and early Aleutian. Paleo-Indian is distributed throughout the Macro-Indian dentition and language area. Paleo-Arctic occurs chiefly in Na-Dene territory. Early Aleutian is limited to this island chain, the Alaska Peninsula, and possibly one site north of the Brooks Range. This archaeological, linguistic, and dental distribution correspondence is simply too substantial to attribute to chance.

Since the New World dental and linguistic divisions are at-

tributed to three distinct Siberian migrations, a brief review of Northeast Asian prehistory will help show the robustness of this hypothesis. Turner (1983*b* and elsewhere), following Laughlin (1963), proposed that the Aleut-Eskimo maritime tradition began about 15,000 years ago in the region of the lower Amur River basin including Hokkaido because of the similarities of this region's blade tools (Derevyanko 1969; Vasilievsky 1973; Yoshizaki, personal communication) to early Aleutian blade artifacts and some dental similarities to later Aleut teeth. The route from the lower Amur to Anangula in the eastern Aleutians was by way of the Bering land bridge's southern coast, as proposed by Laughlin (1963).

The ancestors of Paleo-Indians probably exited Siberia by way of the Lena River basin, crossing to Alaska in search of the few large or small animals that could survive on the cold, dry steppelike land bridge. No archaeological or dental evidence for ancestral Paleo-Indian has yet been found in the Lena drainage system, but this route can be proposed on the basis of the many significant dental trait frequency differences between Aleut-Eskimo and Macro-Indian (16/27 [59.3%]; Turner 1985:78). For these differences to evolve requires keeping these two major populations separate after their original departure as a single northward-expanding North China population. A Siberian exit for ancestral Paleo-Indians cannot be proposed any farther west than the Lena Basin, say, the Ob or Yenisei drainages, because it would put these Sinodonts well into European Cro-Magnon territory as established by the 18,000-year-old Mal'ta teeth and artifacts (Turner 1983*b*). European Cro-Magnon people and culture could have extended east to about Lake Baikal on geographic grounds alone, since much of northwestern Eurasia has relatively low topographic relief, whereas northeastern Eurasia is characteristically mountainous. During the same period at the Upper Cave of Zhoukoudian, Sinodont peoples were present, and North China tribes had already begun to develop the North China generalized "microlithic" tool tradition from which American stone tool types can be derived (Turner, Gai, and Stanford n.d.).

Mochanov (1978) has recognized late Pleistocene Diuktai people between the Lena and Amur Basins whose stone tools were the same types as those used by the American Paleo-Arctic or Denali people just slightly later in time (West 1981). Thus, it is in the Northeast Siberian region bounded by the Lena and Amur Basins that the Na-Dene dental group differentiated its pattern of intermediacy between Aleut-Eskimo and Macro-Indian. These riverine-forest Diuktai/Na-Dene people entered Alaska just before the final flooding of the land bridge as boreal forest replaced arctic steppe vegetation (Colinvaux 1981, Hamilton 1982) and seemingly stimulated the small numbers of Macro-Indians to move southward out of eastern Beringia.

The majority of the 14,000-year-old and younger Kamchatkan Ushki artifacts found and reported by Dikov (1979), some of which have been examined by R. Ackerman, R. Carlson, P. Hobler, Turner, and others, seem to belong to the Diuktai assemblage and bear no convincing resemblance to Clovis artifacts. This is another reason for proposing the Lena Basin as the exit route for ancestral Paleo-Indians. Thus, there is some Siberian archaeological and dental evidence supporting the three-migration hypothesis for the peopling of the New World.

In sum, New World dental variation matches the North Asian Sinodont pattern, is greater in the north than in the south, has a divergence schedule corresponding to Clovis, and forms three clusters which correlate highly with linguistic and archaeological distributions. These facts, taken together, strongly suggest three late-Pleistocene migrations from Siberia to Alaska.

THE GENETIC EVIDENCE

Wiley (1981) credits the German entomologist Willi Hennig with codifying a series of ideas that began a major revolution in systematic biology, the field of phylogenetic systematics or cladistics. Some of the more important Hennigian principles which underlie the reconstruction of population history at all evolutionary levels are as follows (Wiley 1981:1):

1. The relationships leading to the cohesion of living and extinct organisms are genealogical ("blood") relationships.
2. Such relationships exist for individuals within populations, between populations, and between species.
3. All other types of relationship (i.e., phenotypic and genetic) are phenomena correlated with genealogical descent and thus are best understood within the context of genealogical descent with modification (quite literally "evolution").
4. The genealogical relationships among populations and species may be recovered (discovered) by searching for particular characters which document these relationships.

In general, organisms are similar biologically because they are related; however, organisms are not necessarily closely related just because they are similar. Genealogical descent or common ancestry causes biological similarity. Unfortunately, evolutionary biologists and all scientists interested in organic evolution are faced with an enormous problem. One actually judges similarity (whether phenotypic or genetic), and from these judgments about biological similarities and differences between individuals and populations inferences must be made concerning genealogical relationships. Sometimes these inferences turn out to be wrong because of evolutionary convergence, unrecognized gene flow, or incorrect character polarity assessment. In other cases the history of a particular allele in a population may confound any predictive association between genetic similarity and relatedness, as Schwartz and Armitage (1983) have clearly demonstrated in marmot colonies. Likewise, microevolutionary components of human population structure such as genetic drift, inbreeding, and gene flow can interact with natural selection to obscure the genealogical relationships of Native Americans. Amerinds have probably been in the Americas for somewhere between 500 and 1,000 generations. Trying to decipher these 12,000+ years of population history is a very complex undertaking, especially with a data base of 20th-century genetic data.

From a biological viewpoint there are at present few alternatives. The advent of C¹⁴ accelerator mass-spectrometry dating has sounded the death knell for all the major claims of great antiquity for human skeletons in the New World (Taylor et al. 1985, Zegura 1984). Even Bada (1985) now admits that the skeletons originally thought to be representative of Upper Pleistocene humans in the New World are more likely all Holocene in age. We are left with a sobering realization: "it now appears that currently the oldest human skeletons directly dated by C-14 analysis based on an organic fraction are from the Wilsall (Anzick) site in Montana with an age of 10,600 ± 300 C-14 years B.P." (Taylor et al. 1985:138). An unknown number of millennia are thus unaccounted for in the New World skeletal record (Irving 1985). Bonnichsen and Bolen (1985) reported finding the oldest human hair in the Americas at False Cougar Cave, also in Montana. These hair specimens occur stratigraphically below charcoal dated at 10,530 ± 140 B.P. (which could make them roughly contemporaneous with the Anzick skeletons), although the hair may actually be as old as 14,590 years B.P.

Perhaps the most exciting recent development in Amerind biological research has been the isolation of mitochondrial DNA from the brain tissue of 8,000-year-old skeletons from the Windover archaeological site in Florida (Agee et al. n.d.). It appears that DNA can be recovered from nearly any tissue preserved through rapid drying. In addition, tissues preserved in saturated environments like the Windover "muck" pond can

also preserve DNA under conditions of anaerobiosis, neutral pH, and high ion concentrations. The possibility that direct genetic comparisons of past and present human populations could help clarify major population shifts such as the initial peopling of the New World would be yet another unexpected dividend of basic molecular biological research and recombinant DNA technology (Agee et al. n.d.). In a similar vein, Lowenstein (1985) reviews the accomplishments of protein analysis using the radioimmunoassay method in deciphering taxonomic relationships of fossil, extinct, and living organisms. Since 1977 the woolly mammoth, Stellar's sea cow, the Tasmanian wolf, the quagga, the Piltdown hoax remains, various plant remains, bloodstains, and shrunken human heads have all yielded molecular data useful for species identification. In many cases new information on dates of evolutionary divergence also resulted. One clear implication of these applications is that what might be called molecular paleontology, molecular paleoanthropology, or molecular archaeology promises to provide crucial tests of hypotheses based on genetic data gathered from living populations or on morphological data gathered from skeletal remains and fossilized material (Lowenstein 1985).

One interpretation of the present genetic evidence for the peopling of the New World involves a tripartite grouping of Native Americans concordant with the groupings derived from linguistic and dental data: Amerind, Na-Dene, and Aleut-Eskimo. This represents a hypothesis that can be tested with more and better genetic data. The total body of genetic data reviewed here included gene and/or genotype frequency distributions, haplotype frequencies, restriction-fragment-length polymorphism frequencies (RFLPs), genetic distance matrices, dendrogram representations, oligovariate plots, synthetic gene-frequency maps, phylogenetic trees, and microevolutionary scenarios based on the primary data. Most of the data are serological in nature, involving blood-group antigens, serum proteins, erythrocyte enzymes, immunoglobulins, and leukocyte antigens. Additional genetic data come from restriction endonuclease analysis of mitochondrial DNA and from the genetic epidemiological analyses of disease data associated with the New World Syndrome. Data and/or interpretive material were taken from the following sources: Agee et al. n.d., Alekseyev (1979), Constans et al. (1985), Crawford (1984), Crawford and Enciso (1982), Crawford et al. (1981), Dykes, Crawford, and Polesky (1983), Eriksson, Lehmann, and Simpson (1980), Ferrell et al. (1981), Fitch and Neel (1969), Greenberg, Turner, and Zegura (1985), Harper (1980), Harper and Laughlin (1982), Lampl and Blumberg (1979), Laughlin (1963, 1966), Mourant, Kopec, and Domaniewska-Sobczak (1976), Neel (1976), Neel and Salzano (1966), Nei and Roychoudhury (1982), O'Rourke, Suarez, and Crouse (1985), Rychkov and Sheremet'eva (1980), Schell et al. (1978), Scott (1979), Scott and Wright (1983), Spuhler (1979), Suarez, Crouse, and O'Rourke (1985), Suarez, O'Rourke, and Crouse (1985), Sukernik et al. (1981); Szathmary (1977, 1979a, 1979b, 1981, 1983, 1985), Szathmary and Auger (1983), Szathmary and Ossenberg (1978), Szathmary, Ferrell, and Gershowitz (1983), Williams et al. (1985), Wallace, Garrison, and Knowler (1985), Weiss (1985a, 1985b), Weiss, Ferrell, and Hania (1984), and Zegura (1984).

It should be emphasized that we view the interpretation of the genetic data as secondary support for the primary inferences based on linguistic and dental data. Other authors have often drawn very different conclusions based on portions of these same data (Crawford et al. 1981; Ferrell et al. 1981; Nei and Roychoudhury 1982; Spuhler 1979; Szathmary 1979a, 1979b). There is, in fact, little agreement among those most closely involved with the actual data collection and analysis regarding the number or identity of the major groupings of Native Americans based on genetic data (Crawford 1984; Szathmary, personal communication; Weiss 1985b). With these caveats in mind, we will now review what the genetic

evidence reveals about the grouping or clustering of Native American populations.

Historically, as genetic data began to accumulate, the traditional position of two major groups (American Indian versus Aleut-Eskimo) became the paradigmatic interpretation of the genetic diversity in the Americas (Laughlin 1963). Then in 1978 Szathmary and Ossenberg used both genetic and skeletal data to question this conventional wisdom in a paper entitled "Are the Biological Differences between North American Indians and Eskimos Truly Profound?" In the meantime, other investigators had taken up the challenge of trying to decide if and how American Indian genetic variation could be partitioned (Mourant, Kopec, and Domaniewska-Sobczak 1976, Neel 1976). Those who emphasized a Pan-American Indian genetic identity saw no need for partitioning the American Indians because American Indians were all members of one "race." Others regarded local partitioning as essential but pointed to the many examples of presumed genetic drift in South American Indian gene pools, which, in turn, led to the questioning of the phylogenetic utility of such partitioning. These doubts have subsequently received a general theoretical endorsement from Relethford and Lees (1982:124-25), who state: "In sum, it appears that the basic difference between monogenic and polygenic systems is their rate of responses to changes in gene frequencies, such that serological data show drift and recent migration to a greater extent, while metrics tend to show long-term historical relationships." The gene-frequency distributions compiled by Mourant et al. (1976) present a confusing genetic kaleidoscope. On the one hand, there is a marked change in the frequencies of the A, B, and Diego antigens at approximately the latitude of the United States-Mexican border, which argues against any simplistic Pan-American genetic identity. For other genetic systems, however, Central America seems to represent a transitional zone between North and South America rather than a disjunction or bottleneck (Greenberg, Turner, and Zegura 1985, Mourant et al. 1976).

Between the two classificatory extremes of Pan-American unity and grouping by extensive local partitioning come those theories which propose a major dichotomy of possible microevolutionary import within the American Indian population system. For example, Nei and Roychoudhury (1982) see the dichotomy as North American Indians versus South American Indians, Crawford (1984) sees it as Middle and South American Indians versus a later-arriving North American Native population system, and we see it as the Na-Dene versus all the rest of the Amerinds (whether North, Central, or South American). Nei and Roychoudhury's (1982) results can easily be reinterpreted to agree with our framework once the linguistic affiliations of their study groups are made explicit (Greenberg et al. 1985). Unfortunately, the data on serum vitamin D binding protein (the Gc system) presented by Constans et al. (1985) graphically illustrate that this is not always the case (although the combined Gc and PGM1 results in Dykes, Crawford, and Polesky [1983:143] are in accord with our trichotomy).

Spuhler's (1979) study is the most extensive in the literature restricted to Native North American population affinities. When allocated by gene frequencies into language phyla in a stepwise discriminant-function analysis, 82% of the Aleut-Eskimo groups (9 of 11), 67% of the Na-Dene groups (8 of 12) and 52% of the non-Na-Dene Indian groups (15 of 29) were correctly assigned to the proper language phylum. Interestingly, no non-Na-Dene Indian language group had a higher percentage of correct allocation than the Na-Dene, underscoring the genetic distinctiveness of the Na-Dene with respect to other North American language groups.

Szathmary (1977, 1979a, 1979b, 1981, 1985) and Szathmary and Ossenberg (1978) have presented detailed evidence for the position that the biological differences between North Ameri-

can Indians and Eskimos are not as great as the previous literature suggests. Szathmary (1979b) has also proposed successive waves of migration as a possible explanation for her dendrogram results. The non-Na-Dene subarctic Algonkin groups in her analyses generally form a cluster of their own, separate from the Na-Dene cluster and far removed from the majority of the Eskimo populations, while the Na-Dene are the American Indian group with the closest relationship to the Eskimos based on current genetic similarities (Szathmary 1977, 1979a, 1979b, 1985). In addition, Szathmary has opined that the Na-Dene may have been closer to the groups that gave rise to the Eskimos than any other Indian or Siberian population. As a cautionary note, it should be pointed out that Ferrell et al. (1981), using almost the same genetic data as Szathmary and Ossenberg (1978), make a case for a much cleaner separation between Aleut-Eskimos and North American Indians. Indeed, it was an attempt to integrate Szathmary's (1979b) and Szathmary and Ossenberg's (1978) results with those of Spuhler (1979) and to reconcile this new information with preceding genetic data and interpretations of Native American population history that led Zegura to formulate a tripartite-partitioning hypothesis and to explore possible scenarios for the peopling of the Americas consistent with an Aleut-Eskimo/Na-Dene/non-Na-Dene Indian trichotomy.

Meanwhile, Harper (1980) presented a model for the origins and divergence of Native American populations based on erythrocyte enzyme and serum protein data. His model includes ancestral connections and hypothetical divergence dates for the Athapaskans (Na-Dene) and the Aleut-Eskimos but does not explicitly include information on non-Athapaskan Indians. According to his scenario, the organization of the gene pool responsible for the Native American population system began about 19,000 years ago. The Athapaskans (Na-Dene) and the Aleut-Eskimos are depicted as having had a period of common ancestry, with the eventual divergence of the Athapaskan lineage from the seminal population occurring approximately 15,000 years B.P. By about 10,000 years B.P. the non-Athapaskan branch had evolved into an entity called the Bering Sea Mongoloids. This population quickly bifurcated into the Aleuts and Eskimos, the subsequent bifurcation of the Eskimo lineage into Yuit (Yupik) and Inuit (Inupiaq) taking place about 5,000 years ago (Harper and Laughlin 1982). Although these dates do not fit the chronologies developed in the linguistics and dental anthropology sections, the omission of any discussion of the non-Athapaskan Indians renders direct comparisons futile. We are encouraged, nevertheless, by the definite concordances between linguistic diversity and genetic diversity in modern Eskimo populations. For instance, Inuit communities show a high degree of genetic homogeneity which parallels their linguistic homogeneity, while the Yuit show much greater genetic and linguistic diversity (Crawford and Enciso 1982, Scott and Wright 1983). These linguistic/genetic concordances corroborate theories about the relative recency of the eastward Thule (Inuit) migration and the greater time depth of Eskimo habitation in southwestern Alaska based on archaeological data.

From a genetic perspective the hypothesis of three separate migrations leading to a tripartite division of modern Native Americans is still without strong confirmation. This is why we regard the genetic evidence as supplementary rather than primary. The best direct genetic support comes from the Williams et al. (1985) study of immunoglobulin G (antibody) allotypes. According to these researchers, when current Gm distributions are analyzed with respect to the three-migration hypothesis, there are three distinct Gm distributions for the postulated migrants: Gm^{1;21} and Gm^{1;2;21} for the Paleo-Indians; Gm^{1;21}, Gm^{1;2;21}, and Gm^{1;11,13} for the Na-Dene; and Gm^{1;21} and Gm^{1;11,13} for the Aleut-Eskimo. Wallace et al. (1985) have also

provided an experimental design to test the three-migration hypothesis against a plausible alternative using mitochondrial DNA. Unfortunately, the necessary Na-Dene and Aleut-Eskimo data have not yet been collected, but they will be. Likewise, when more data are available from Na-Dene and especially from Aleut-Eskimo and Siberian groups on their genetic susceptibility to the New World syndrome of metabolic diseases (adult-onset or non-insulin-dependent diabetes mellitus, obesity, cholesterol gallstone formation, gall bladder cancer, and other middle-digestive-system tumors), migratory hypotheses incongruent with these data can be discarded (Weiss 1985a, Weiss et al. 1984).

Although Suarez, O'Rourke, and Crouse (1985:226) warn against "fanciful interpretation" of their synthetic gene-frequency maps based on Native North American data, by their own admission the overall topography of two of their figures preserves the Eskimo-Indian dichotomy as well as the distinct relationship between Northern and Southern Athapaskan-speaking (Na-Dene) groups. The maps also support the notion that the Eskimo populations are relatively distinct from the Na-Dene groups (Suarez et al. 1985:223). Their maps certainly do not disconfirm our three-migration hypothesis, but neither do they strongly confirm it in the opinion of the authors as expressed in a companion article (Suarez et al. 1985:238) on the relationship between heterozygosity and cultural complexity. Perhaps Weiss (1985b:491) has summarized the present state of genetic knowledge most succinctly:

Markers such as the Gm system do tend to discriminate to a reasonable extent among Eskimo-Aleut, Paleo-Indian, and Athapaskan people. . . . For less definitive markers, the cumulative evidence of many loci supports somewhat similar relationships. . . . Of course, Arctic peoples are not species, and they are not so easily treated phylogenetically. However, there seems to be some truth in the polychotomized description of Arctic peoples, and this may imply aspects of the *historical processes* which generated their differences.

CONCLUSIONS

The three lines of evidence, linguistic, dental, and genetic, lead to closely similar divisions of the indigenous New World population into three groups; Amerind, Na-Dene, and Aleut-Eskimo. In our opinion, the most reasonable historical interpretation is that these three represent three migrations from Asia. The order and chronology remain, however, less certain, especially the relative chronological priority of the Na-Dene and the Aleut-Eskimo.

Comments

by LYLE CAMPBELL

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This article is distressing. I object to much but concentrate on (1) Greenberg's classification of American Indian languages and (2) nonlinguistic correlations.

Most at issue is Greenberg's hypothesized "Amerind," including all American Indian languages save Eskimo-Aleut (established long ago) and Na-Dene (where Athapaskan is clear, others less so). "Amerind" is discounted by nearly all specialists.

Greenberg et al. assert that I and others, as proponents of the "major alternative" classification, with ca. 200 independent linguistic groups, have "tacitly claimed" that "we will never find that two . . . are significantly more similar to each other in . . . genetic classification." Not so; this "alternative" represents present knowledge, a healthy recognition of limita-

tions. It corrects such past excesses as the routine proposal of remote connections as hunches for further investigation to reduce linguistic diversity. Regrettably, many such connections became frozen in the subsequent literature without investigation. Methods for establishing distant linguistic genetic relationships are clear (Bright 1984; Campbell and Kaufman 1981, 1983), and when they have been applied to former proposals many have had to be abandoned (Campbell and Mithun 1979b). Greenberg's "Amerind" will fall among these, given the evidence and methods he has used. It is unfortunate that in his "lumping" he has not heeded the demands for proof that led to the "splitting" tendency that characterizes current Amerindian linguistics. The widespread first-person *n* and less widespread second-person *m* markers, the only evidence mentioned, have been recognized from the beginning without significant impact on classification (consider the many "Amerind" languages lacking one or the other or both and the non-Amerind language containing them, and consider the questions of sufficient proof and of other explanations for these). Many proponents of the "major alternative" may be sympathetic to the belief that (many) American Indian languages may have a common origin; however, they opine, currently accepted methods and evidence cannot demonstrate it. Given the audacity of this "Amerind" proposal, neglect of important recent work relevant to the claims is not to be excused lightly (cf. Bright 1984; Campbell and Kaufman 1981, 1983; Campbell and Migliazza 1986; Klein and Stark 1985; Suárez 1981, plus the extensive work within individual language families and geographical areas).

The dental and genetic correlations are unconvincing—at worst irrelevant, at best consistent with other interpretations. Repetition of the obvious seems required: there is no deterministic connection between language and gene pools or culture. A single language can be spoken by a genetically and/or culturally diverse community; a culturally and/or genetically homogeneous population can speak more than one language. That is, language shift and multilingualism are facts of linguistic (and cultural) life; genes neither cause nor cater to them.

Thus, the "Aleut-Eskimo" dental cluster fits linguistics, but, as the authors note, the "Greater Northwest Coast" (called misleadingly "Na-Dene") cluster does not match "Na-Dene" language geography. The Northwest Coast has few attested Na-Dene groups and many others. It is a notorious linguistic (and cultural) diffusion area, with multilingualism, borrowing of linguistic traits, slaving, and intermarriage. Here, language and genetic traits should not be expected to match.

Similarly, the genetic data they regard as "still without strong confirmation" and so as "supplementary." Specialists disagree, with conflicting interpretations from the same evidence—hardly strong support.

Conclusion: neither their linguistic classification nor its dental/genetic correlation is supported, the conclusions about migrations are unwarranted, and the whole speculative venture should be abandoned. Indeed, the linguistic classification should be shouted down in order not to confuse nonspecialists or detract from the real contributions linguistics can make to prehistory.

by JAMES A. FOX

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Greenberg has been working on the general classification of American Indian languages for over 25 years, latterly with NSF support, but publication of his findings has been limited to brief summaries and an invitation (Greenberg 1979) to consult his microfilmed notebooks through interlibrary loan. It is expectable that this should be so; anyone who has struggled with the linguistic and bibliographic complexities of even a single Amerind stock will appreciate the magnitude of Green-

berg's task. That we now have a complete work in the offing (Greenberg n.d.) is an exciting development in American historical linguistics, as the inferences drawn by Greenberg, Turner, and Zegura suggest, and in Old World linguistics as well, in view of the proposed inclusion of Aleut-Eskimo in a revolutionary "Euro-Asiatic." To anticipate the debate before the evidence is in, could Greenberg be right?

He is, first of all, in good company; Sapir (1968 [1916]:454–55) proposed long ago that his Eskimo-Aleut and Na-Dene stocks represent separate, and late, migrations into North America (though he did not group the remaining languages into a single stock). Indeed, as Sapir argued, if (as it seems) the time depth of American settlement is not great enough to account for its linguistic diversity, the case for a multiple-migration theory of Amerind origins is compelling.

Greenberg is also a superb historical linguist. His pioneering and monumental work on language universals and their dynamic explanations and the success of his general classification of African languages (after initially virulent criticism) warrant an expectation of sophisticated and telling arguments. So far, he has emphasized two comparative principles: grammatical correspondences and mass comparison. Both are critical to the comparative method, and neither has been widely applied to American Indian languages.

His casual reference in the present article to a "widespread" presence of first-person *n*- and second-person *m*- pronominal prefixes in General Amerind, along with his insightful and more detailed treatment (Greenberg 1979) of the proposed General Amerind third-person preconsonantal/prevocalic *i*-/*t*- alternation as an explanation for problems arising from internal reconstruction in certain South American languages, suggest that we may expect much of this in his forthcoming book. The latter correspondence, by the way, figured prominently in Teeter's (1964:1029) acceptance of the special relationship of Wiyot to Algonquian, but I (Fox 1978:35–36) have pointed out that it is so widely found in Latin American Indian languages that it can only support the Wiyot-Algonquian *relationship*, not the subgrouping. Incidentally, while the most straightforward reconstruction of the Proto-Mayan third-person preconsonantal/prevocalic pronominal prefixes is **u-/*r-*, evidence of a previous third-person prefix **i-* is found in the many Mayan kin and body-part terms beginning with *i*, obviously by fossilization of the pronoun in cases of inalienable possession (Fox 1985:406); a pre-Proto-Mayan system **i-/*r-* is remarkably similar to the system reconstructed by Greenberg.

Greenberg's insistence on mass comparison is also valid. Since the explanation by Verner of certain exceptions to Grimm's Law by reference to data from related but non-Germanic languages, the value of external comparison, though it places extraordinary demands on scholarship, has not been disputed. The chances of survival of a reflex of some etymon and of evidence pertaining to its reconstruction increase with the number of surviving stocks and with the genetic distance of such stocks from each other; mass comparison dramatically increases the chances of identifying such a reflex and of properly assessing the relevance of borrowing.

As Greenberg points out, Amerind-stock specialists' common demands for detailed lists of sound correspondences and complete comparative analyses as proof of proposed relationships and classificatory schemes are strongly correlated with a virtual cessation of the classificatory generalization begun by Sapir. Such demands are extravagant even for one stock; indeed, in Mayan, a venerable focus of linguistic attention, they have not yet been met. Amerind historical linguists tend to stay "safely" within the confines of obvious stocks and postpone looking afield, Verner's example notwithstanding. Attempts to explore the special relationship of Mayan to its patently nearest related stock (Mixe-Zoquean), for example, have been vigorously attacked—not without justification, but to the exclusion of pursuing this potential source of explanations for infra-

Mayan problems. Ironically, this "disputed" relationship is so shallow that it is not even mentioned in the summary outlined by Greenberg.

Such mechanistic and unrealistic expectations ignore the history and scientific value of classification. Once comparable data became available to scholars, many of the most significant classifications (Finno-Ugric, Indo-European, Austronesian, Sino-Tibetan) were made almost immediately, *on inspection* of lexical lists and grammatical paradigms. The Mayan language family, for example, was recognized by the brilliant Spanish missionary linguists of the 16th century long before even cursory systematic attention to correspondences began, and subsequent scholarship has neither dramatically upset the membership nor achieved consensus on subgrouping within Mayan. Most Mayanists still accept a subgrouping based more on quantity of lexical resemblances than on systematic positive innovations. This is not to advocate shoddy comparison but to emphasize that wider classification, rather than being some final product that must await perfection within the known subgroup, is an integral and dynamic part of the comparative method itself. It is reasonable to expect that at least some wider relationships should become apparent on inspection in the first significant application of mass comparison to the Amerind data. Therefore, while even preliminary judgment must await the evidence and argumentation, I do not view Greenberg's proposal as either rash or improbable, and I expect American Indian historical linguistics, including my own work, to profit from both the hypotheses and the example.

While I pretend no special competence in the dental or genetic aspects of the issue, I do wish to point out a discrepancy in the authors' culture-historic interpretations of linguistic and dental variation. Greenberg (presumably) notes the enormous diversity of his General Amerind as compared with Na-Dene and the relatively greater diversity of Na-Dene compared with Aleut-Eskimo and concludes that the General Amerind migration was first, Na-Dene probably second, and Aleut-Eskimo last. This follows logically from principles pioneered in linguistics by Sapir—degree of variation is generally proportional to the amount of time available for the variation to evolve. Turner, however, citing the same principle, interprets precisely the opposite pattern of dental variation (greater in the north than in the south) as evidence that the north has been populated longer than the south. Granted the threefold clustering, why the opposite patterns of variation? One suspects genetic mixing and language extinction in the north (Boas warned us!), but if that is the case, the dental variation is not primarily a function of microevolution in place over a long period of time.

Finally, the equation of Na-Dene with the Greater Northwest Coast, an area of great linguistic, cultural, and presumably genetic diffusion, is simplistic; it may be right, as that is the center of Na-Dene diffusion, but the point will have to be demonstrated.

by W. S. LAUGHLIN

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Three knowledgeable scholars present an informative discussion of the peopling of America in which they argue for three divisions in America and for three separate migrations from Siberia. This paper usefully focuses attention on the central point in contemporary studies—whether the single gene pool diverged before the migrants left Siberia or after they reached America.

The discussion of glottochronology correctly indicates a high degree of ambiguity and is neatly summed up with the observation that the relevant time period is beyond the limits of glottochronology. This accords well with the fact that many

linguists have searched for connections in Siberia but have found no counterparts. Compounding 11 subgroups into three, with a separate migration for each, Greenberg cogently notes that it is defensible to hypothesize that the Proto-Aleut-Eskimo community arrived as an internally undifferentiated unit *before* the coming of the Na-Dene. He discards the possibility of one migration with elapsed time so great that all traces of affinity among any of the groups have been effaced.

The dental evidence is displayed in a dendrogram that carries no hint of a triple division but rather is eloquent evidence of a single migration, with minor subdivisions in America. Clearly, dental evidence comprehends greater time depth than linguistic evidence. Turner proves the Asiatic affinities of Indians and also disproves a European component. He is also realistic in suggesting a time depth of some 20,000 years for a seminal population in North China, a date in good agreement with Harper's 19,000 years for "unmigrated American Indians." Thus, his dental, geographic, and archaeological evidence focuses on the central problem of whether divergence took place in Siberia or in America.

The genetic data cover a broad span of kinds of genetic loci, ways of grouping them, and migration hypotheses. Zegura recognizes that the hypothesis of three separate migrations leading to a tripartite division of Indians is still without strong confirmation and therefore views the genetic evidence as supplementary rather than primary. Another reason for this view is found in his treatment of Spuhler's (1979) study, for which he cites the fact that when allocated by gene frequencies into language phyla, 82% of Aleut-Eskimo groups (9 of 11), 67% of Na-Dene groups (8 of 12), and 52% of non-Na-Dene groups (15 of 29) were correctly assigned to the proper language phylum. Interestingly, a chi-square test reveals no significant difference between right and wrong assignments for these three groups.

Alaska, not the Bering Strait, was obviously the gateway into the New World, with the Bering land bridge a fundamental part of Alaska. The glacial maximum occurred some 18,000 years ago, and the bridge was broached by rising sea level some 14,000 years ago. The bridge itself was one of the lowest, flattest, and vegetationally most forlorn areas in the world, ranging from depauperate tundra in the south to polar desert in the north, except for the southern coast, with marine resources and driftwood. Fish, seals, walrus, white whales, drift whales, birds, and caribou were the primary resources for the presumably small (300) migration that followed the coast. Descendants of the small group that trickled up the Yukon River 15,000 years ago, when its mouth lay between St. Lawrence Island and the Pribilof Islands, reached the Magellanic area some 11,000 years ago. The other small division of the single migration continued on the coast, and subsequently their descendants occupied the entire coast from Attu to Angmagssalik.

The differences between American populations are not large enough to postulate more than one migration; the taxonomic category of American Indian easily embraces all of them. From the standpoint of evolutionary biology they appear appropriate to a 15,000-year time depth with divergence in America, including the now liquidated land bridge. A single small migration some 16,000 years ago appears most parsimonious. Researchers who flirt with trinities should be reminded that Eskimos have walked on water for 10,000 years. They wait for it to freeze, and when on thin ice they avoid creating unnecessary waves.

by EMÖKE J. E. SZATHMARY

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What is useful about this article is that it brings together the viewpoints on the peopling of the Americas of three individuals known for their work in linguistics, dental anthropology,

and arctic biology. The authors think the evidence in their fields supports the three-migration model for the settlement of the Americas and agree that the descendants of the founding groups are most precisely defined "in linguistic terms as Amerind, Na-Dene, and Aleut-Eskimo." Although their perspectives are offered in sections subtitled "The Linguistic Evidence," "The Dental Evidence," and "The Genetic Evidence," very little *evidence* is presented. Rather, we are given conclusions based on research, their own and that of others, published elsewhere. This is legitimate if one regards Greenberg, Turner, and Zegura's joint effort as a "position paper" that will generate more hypothesis-testing research on their much-debated subject. I think this is necessary, for the material presented here does not convince me that there have indeed been three waves of migration into the Americas.

While Greenberg's analysis of Native American languages may be correct, one has to suspend judgment until his book (Greenberg n.d.) is published and one can examine his evidence. This paper leaves me with the impression that the proof is based on grammar and "etymologies." The former, especially if it consists of unusual grammatical forms rather than a possible borrowed overlay, may be indicative of relationship but does not constitute proof of relationship. The latter, "etymologies," is more problematic. Greenberg needs to show that there are a sufficient number of correspondences (i.e., systematic sound shifts) among the languages to permit the reconstruction of a large number of morphemes. If such morphemes form the basis of his etymologies, then his argument can be accepted. Without demonstration that the etymologies are based on cognates rather than borrowings, his conclusions are unsupported.

Turner's contribution repeats the classification he has proposed elsewhere (Turner 1985) that equates Greater Northwest Coast with Na-Dene. In contrast with his earlier work, his discussion here carefully distinguishes between "Na-Dene-speakers" and "Na-Dene dental group," for the two are *not* the same. The Greater Northwest Coast group, for example, includes Kachemak, Kodiak, and Alaska Peninsula samples that are likely Eskimoan in origin and samples from the lower Columbia River that are non-Na-Dene Indian. Frankly, I wish we had rules akin to the international code of zoological nomenclature that would prevent the assignment of a name that identifies a particular group to something that does not belong to that group. Most would agree that use of a linguistic label indicates linguistic relationship and that for Amerindians north of Mexico linguistic relationship implies probable biological relationship (Spuhler 1972, 1979). Turner's "Na-Dene" in fact includes representatives of what Greenberg calls "Amerind" and "Aleut-Eskimo." How does this, then, constitute evidence for a second wave of migration by the ancestors of the Na-Dene (Greenberg's and Zegura's meaning of the term)? It does not. Nevertheless, Turner would have us believe that there was such a migration and that the teeth of non-Na-Dene-speakers of the Greater Northwest Coast look like those of the Na-Dene because the latter's genes have spread extensively along the Pacific Coast from California to the Gulf of Alaska. If this really occurred, genetic distances should also reveal the claimed extensive and widespread gene flow, yet I found that the Nootka of Vancouver Island, the Na-Dene (represented by Haida, Tlingit, and Northern Athapaskan), and South Alaskan Eskimos (represented by Kodiak Islanders and three Koniag isolates from the mainland) did not cluster together (Szathmary 1979). Turner's equating the label "Na-Dene" with the Greater Northwest Coast group suggests that he is not prepared to question, let alone to reject, the three-migration hypothesis whatever the results of his calculations. Rather, he interprets his analytic results in the light of a preexisting hypothesis that he simply assumes to be true.

Zegura shows restraint in his summation of the genetic evidence concerning the peopling of the Americas. Nevertheless, I

disagree with his conclusion that the Gm allotype distributions (Williams et al. 1985) provide direct support for the tripartite-peopling model. First, I dispute the notion that $Gm^{1,2,21}$ does not occur in Eskimos except as a product of admixture. The allotype is present in 11 of 15 Eskimo populations tested, including groups on St. Lawrence Island (Ferrell et al. 1981) and in North Alaska (Matsumoto et al. 1982), South Alaska (M. S. Schanfield, unpublished data on Eskimos along the Kuskokwim River), Canada, and Greenland (Williams et al. 1985). Furthermore, it is present in polymorphic frequencies in 8 of the 11 groups in which it occurs and is also found in the Chukchi (Sukernik and Osipova 1982). These data alone call into question the assumption that this gene has been absent from unadmixed Eskimos at any time in their history. Secondly, the probability of genetic relationship between any two sets of populations is based not on the presence or absence of any one allele in either but on the overall probability provided by all the alleles at the given locus. It is worth noting that cluster analysis of the genetic distances derived from the same Gm data as used by Williams et al. (1985) shows that Athapaskan-speakers (Haida and Tlingit Gm distributions are unknown) are consistently intermixed with Eskimos and Chukchi (Szathmary 1986). Other Indians are consistently separate. This certainly does not support a tripartite-migration model but lends itself to the notion that Athapaskan-speakers are genetically closer to Eskimos than are other Indians.

Whether one can make a case for more than one time of entry of Asiatic hunting bands into the Americas depends upon the kinds of evidence one musters. I am on record as saying that currently available genetic data are consistent with the view that some groups must have arrived in the New World early enough to have become separated by the glaciers from bands that roamed across Beringia (Szathmary 1984, 1985). This certainly implies more than one time of entry, but it does not commit me to supporting a bipartite, tripartite, or multipartite model of occupation. In my opinion, postulation of the precise number of "waves" is an exercise in hypothesis generation. May there always be creative individuals who propose models, and may there always be scientists whose testing will finally allow us to select the scenario that is most likely.

by KENNETH M. WEISS and ELLEN WOOLFORD

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Greenberg et al. argue from linguistic, dental, and genetic evidence that there exist three distinct groups of American natives and that these are the result of three separate waves of immigration into the New World from Asia. Although it is popular to decry typological sorting of human populations, one or both of these logically independent claims may in some senses be correct. Nevertheless, there are many potential problems with the interpretation of such data that should be noted. Even nonexperts can imagine the difficulties that would be encountered by anthropologists in a distant future trying to reconstruct the immigration waves that produced the *current* linguistic and genetic diversity in the Americas. People of African ancestry speak English in some areas, Spanish in others, and creoles in still other areas, and many Amerindian-speakers in North America and much of Spanish-speaking Latin America are a thorough European-Amerindian and in some places also Black genetic mixture.

In the linguistics section, readers are given very little information beyond a bare presentation of the conclusions, and there is no way to evaluate these results without going to the sources cited. A discussion of the method of linguistic analysis used, paralleling that given in the genetics and dental sections, would have been appropriate, especially as the linguistic evidence is alleged to be the strongest of the three and most read-

ers will not be Amerindian specialists. Was the analysis based on strict comparative methodology, or were other, less widely accepted methods used to establish higher relationships? If the latter, what were these methods? How much actual historical reconstruction was done, and how much of the study was based on surface comparisons of words from the various languages? What sorts of statistical techniques were used? What steps were taken to screen out resemblances resulting from language contact? Although space may have been limited, some of the space devoted to the history of Amerindian language classification and the history of the development of the three-group theory might better have been devoted to a few sample cases showing what kind of evidence under what kind of analysis establishes a link between stocks that other scholars claim to be unrelated. A few examples of the forms hypothesized to be related and their reconstructed parent forms would give the reader an idea of how controversial the evidence is.

The caution urged in this article with respect to the results of the glottochronology dating technique should be taken seriously. The method assumes a constant rate of linguistic change, but the actual rate can be speeded or slowed by language contact. We cannot assume that in these waves of migration people limited themselves to unoccupied areas. There must have been contact of some sort when one wave pushed into areas still occupied by the last wave. (This problem pertains as well to the dental and genetic data.)

Even without the problem of one wave pushing into another, multilingualism is the order of the day in most traditional societies. If one branch of a language group is in contact with an unrelated group, it is likely to change faster than its sister languages and in a different direction. It may thus appear by glottochronology to have diverged from its sisters at an earlier date than is actually correct. Renewed contact between branches of the same group can result in borrowings that will make the branches appear to have diverged more recently than is really the case. Extreme contact situations such as those which result in the formation of a pidgin can throw off the results of glottochronology entirely and may even create a false bridge between two unrelated language stocks. An application of glottochronology to English and the English-based pidgin of Papua New Guinea, Tok Pisin, gives a separation date of something like 2,000 B.P., but Tok Pisin has not existed a tenth of that time. Moreover, the fact that Tok Pisin contains words from Malay and Tolai as well as English might convince a scholar from the future that these stocks were closely connected. Finally, there is one other factor that, if present in a culture, can upset dating estimates. If the culture has a taboo on using words that sound like a dead relative's name, lexical substitution can occur virtually overnight (and substitute words may be borrowed from adjacent languages), and there is no guarantee that words on the basic list used by glottochronology will be especially protected.

Chrétien (1962) and others have pointed out in mathematical terms why glottochronology becomes extremely unreliable after only a few thousand years, even if the rate of language change is constant. Even if each of two daughter languages loses, say, 14% of its vocabulary per 1,000 years, there is no known way to determine whether the same or different words are being lost in each language. As a result, after only 1,000 years, two daughter languages could have anywhere from 72% to 86% common vocabulary. After 2,000 years, the range could be 48% to 74% or even greater if some of the same words that were lost in the last millennium were changed again. Even when the most probable loss is calculated, the probable ranges of common vocabulary after each successive millennium overlap more and more, and after 5,000 years the overlap is so great that the method seems quite untrustworthy. Yet the lan-

guage groups in question appear to have at least that much time depth.

The question of the relative order of migration of the three groups into the Americas from Asia depends to a great extent on the status of Haida in the Na-Dene group. Yet at least two alternative hypotheses might account for the unexpected depth of the division between this language and the other Na-Dene groups. If this division occurred in the Old World, the Na-Dene would represent migrations of two related groups, and the distance between the Haida and other Na-Dene groups could not be used to date the divergence of this group in the New World. This is not an impossible scenario, since if there was pressure on one group to migrate, there could as well have been pressure on two nearby groups to move at about the same time. Alternatively, the Haida language might have encountered an extreme contact situation that caused it to change very rapidly away from the other Na-Dene groups. The fact that the dental traits of the Na-Dene extend farther than the language does could be accounted for if one portion of this wave of migrants were absorbed by groups they encountered while the rest, although not absorbed, had sufficient contact to change their language radically. (It may be relevant that a pidgin, Chinook Jargon, was spoken in the Pacific Northwest before European contact [Thomason 1983].) Under either of these hypotheses, the split between Haida versus Tlingit and Athapaskan-Eyak would not indicate a time depth in the Americas as great as 9,000 B.P.

Genes (markers or those underlying dental traits) may not change as rapidly as language traits, but many of the same contact phenomena would be expected to have a distorting, or even misleading, effect on subsequent population relationships. Tree comparisons are difficult, and much work by Szathmary and Spuhler has clearly shown that the correspondence of language, genetic, and morphological "trees" is anything but perfect, as Greenberg et al. note. The justification for concluding that the language evidence, probably as volatile as any, is more reliable is not clear.

The standard errors on genetic distances and phylogenetic split times are so large relative to the distances themselves that they are not useful in dealing with subspecies. When there is no sharp division (i.e., when there is contact between the diversifying groups), "separation" times lose much of their meaning (Weiss and Maruyama 1976), as indeed they should in that such separation will have been incomplete. What are the standard errors on time estimates derived from language differences? The archeological evidence would suggest a branch depth of about 12,000 to 15,000 years for Na-Dene and Paleo-Indian, with Eskimo-Aleuts arriving about 5,000 or more years ago, though the archeological (Dumond 1979) and cultural (Townsend 1979) evidence suggests a less rigid divisibility of these arctic peoples. With glottochronology incapable of reaching so far back in time, how can language comparisons confidently be claimed to relate to such distant events?

It is also unclear why the authors consider the genetic marker data to be less reliable than the dental data, when presumably the dental traits themselves are chosen because they are genetically determined. Despite what was found by Relethford and Lees (1982), quantitative traits are not always more informative relative to phylogenies than single-locus ones (Rogers and Harpending 1983). If there has been gene flow, as is reflected in the genetic marker pattern, why is this not also reflected in the dental traits? One reason might be selection—but if that has occurred, the rationale is lost for most historical-phylogenetic analysis, since selection can obscure migration and drift patterns. Indeed, O'Rourke, Suarez, and Crouse (1986) and Piazza, Menotti, and Cavalli-Sforza (1981) believe that gene-frequency patterns reflect climatic selection, resulting in latitudinal patterns which may be highly relevant in the American case. It is also probable that these gene-frequency patterns reflect patterns of admixture with Europeans and

large-scale population movements within the Americas long after the original settlement.

Isolation by distance among groups with a long history of habitation of a single local area can produce generally the same kind of diversity as is observed, especially if a certain amount of population movement and expansion or contraction over long time periods occurs. Thus, even if there is a *general* three-way division of arctic peoples, this proves neither that they have a three-part phylogenetic relationship nor that any such relationship as exists is due to separate waves of immigration.

Human variation, both cultural and biological, is substantial everywhere in the world and was so in the "ethnographic present." It may be a mistake to assume that there ever have been monomorphic Amerindian ancestral stocks. It is difficult to distinguish the effects of extrusion from a severe Beringian bottleneck and of a shifting mosaic within North America over 20,000 years. However, even the three-wave theory depends on a shifting mosaic—in Northeast Asia—to provide the different waves to send across Beringia.

How can one tell, then, whether today's diversity developed here or in Asia? More data are needed, especially from the Asian side of the Bering Strait. Perhaps the best would be clear polymorphic-DNA-haplotype data from both sides of the Bering Strait. Groups of closely linked genes could provide the material from which to construct true trees of identify by descent among the identified haplotypes and to determine whether these correspond closely to phylogenies based on other characteristics, such as language or dental traits.

Reply

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Our comments will first be addressed to the specific issues raised in regard to the linguistic, genetic, and dental evidence. After that the broader questions concerning the interrelation of these lines of investigation in their bearing on the peopling of the Americas will be considered.

In the linguistic section, as commentators point out, there is an absence of concrete evidence to support the conclusions advanced. One reason for this is that we expected Greenberg's (n.d.) book to be available well before the appearance of this article. The book is now scheduled for January 1987. Thus Stanford University Press was much slower and CURRENT ANTHROPOLOGY far swifter than anticipated. The other reason is that the linguistic evidence is truly massive and it was impossible to do it justice within a relatively brief article of broad scope. Greenberg's forthcoming book will contain close to 2,000 etymologies and over 100 sections dealing with individual grammatical points. Some of these are intended to establish particular subgroups of Amerind, and others show a distribution over a number of subgroups and are evidence for Amerind as a whole. There is also a chapter on Na-Dene. Borrowing is accepted as an explanation where it is plausible, but it can never be an overall explanation of a mass of resemblances in basic items, lexical and grammatical, with varied and often vast distributions over an extended area. For agreements in morphological irregularities appearing in widely separated regions, as illustrated by Fox's comments, borrowing is simply ruled out.

First-person *n* and second-person *m* are but a small part of the evidence. They were mentioned simply as an illustration of the logical independence of linguistic, genetic, and dental traits and not to prove, by themselves, the existence and limits of the Amerind stock. Nevertheless, even these two items often found together in the same language in numerous cases from

Chile to Canada and embracing all 11 subgroups of Amerind are sufficient to show that there cannot be something like 200 independent stocks in the Americas. There might be a few accidents, but borrowing can be virtually ruled out. There is, to our knowledge, not a single authenticated instance of the borrowing of a first- or second-person pronoun. Thus for borrowing to be an overall explanation an utterly improbable event would have to have been repeated scores and scores of times. This is indeed to strain at a gnat and swallow a camel.

What the *n/m* example shows is that we have the nucleus of one or a very small number of stocks. Further evidence serves to resolve this question and may add further languages, since even first- and second-person pronouns are not eternal. Were one to put *n/m* on a map of the world and then *m/t*, one would find a tremendous clustering of the first set in the New World and of the second in Europe and northern Asia extending to Aleut and Eskimo. Nonrandom phenomena require an explanation, and the genetic is the only reasonable one in this instance.

It was also impossible in an article of this scope to discuss the methodology of linguistic classification. Greenberg (1957: chap. 5 and elsewhere) has already discussed this topic, and in the first chapter of his forthcoming book it is treated in much greater detail. Not all of the issues can be covered here within reasonable compass. It is possible to say, however, as is more than hinted at in Fox's comments, that a vastly oversimplified version of what is called the comparative method has become current in American Indian studies.

The comparative method is associated with the school of Young Grammarians which flourished in the '70s and '80s of the last century in Germany. Indo-European has been the model, and it is not too much to say that a considerable majority of historical linguists have been Indo-Europeanists and that the methods of the Young Grammarians have not been modified in essentials since their time. If one were to name the leading members of this school, even in a short list the names of Leskien, Brugmann, Delbrueck, and Paul would surely figure. Of these only Delbrueck, the collaborator with Brugmann on the celebrated *Grundriss*, wrote a general text which includes the question of linguistic classification. Those who assume that regular sound change, phonetic correspondences, and reconstruction were considered by this school, which first elaborated these methods in their modern form, as relevant to proving relationships, much less as embodying a method of genetic classification, may be shocked by the following statement of Delbrueck (1904:121–22; I translate but add the German originals of certain key terms):

My starting point is that specific result of comparative linguistics which is not in doubt and cannot be in doubt. It was proven [*erwiesen*] by Bopp and others that the so-called Indo-European languages are related. The proof [*Beweis*] was produced by the juxtaposition [*Nebeneinanderstellung*] of words and forms of similar meaning. When one considers that in these languages the formation of the inflectional forms of the verb, nouns, and pronouns agree in essentials and likewise that an extraordinary number of inflected and uninflected words agree in their lexical parts, the assumption of chance agreement must appear absurd.

Delbrueck avoids even the term for "comparison" (*Vergleichung*) as being too elaborate for this stage. Bopp, to whom he refers, is generally agreed to have founded the comparative study of Indo-European in 1816. The topic of his work was the conjugational systems of the verb in Sanskrit, Avestan, Latin, Greek, and Germanic. He did not use regular sound laws, because the concept did not yet exist, nor did he reconstruct. In subsequent works, he gradually added all the languages that were then available and that are now recognized to be Indo-European using the same methods.

The most important subsequent addition was Hittite, which only became known from the cuneiform tablets of Boğazköy in Turkey in the 20th century. No one questions its affiliation,

yet Hrozný, who first convinced the learned world of this, describes the reasons for accepting it as follows (1917:vii): "Everyone who wishes to interpret the Boğazköy texts . . . will like the author come to the same conclusion on the basis of instances like the fact that *wadar* means 'water' . . . , that its genitive is *wedenas*, . . . that the Hittite present is inflected *jami, jasi, jazi, jaweni, jatteni, janzi*." Thus it is the grammatical irregularity of the nominative in *r* as against the oblique cases of *n*, an irregularity also found in other Indo-European languages, that is cited, as well as the close resemblance of the verb inflections to those of Sanskrit and Latin. In the rest of the passage he mentions pronouns and a number of common lexical items.

Hrozný does not present the table of phonetic correspondences that has become *de rigueur* in the pages of the *International Journal of American Linguistics*, nor has anyone since. The reason is simple; the new Hittite data revolutionized our ideas concerning the sound system of Indo-European. The considerable consensus which existed prior to the discovery of Hittite and the other Anatolian languages was shattered, and at present there are very few points on which all Indo-Europeanists agree.

Delbrueck saw this possibility clearly when in another passage of the book cited earlier he said that sound laws were only provisional and could be transformed (*umgestaltet*) by new data. The question is obviously too complex to be treated in detail here, but it is to be hoped that at least the seeds of reasonable doubt have been planted in the minds of those who have accepted present dogma uncritically and that they will read Greenberg's forthcoming methodological chapter with an open mind.

With respect to those comments directed specifically toward the genetics section of our paper, we find little that we disagree with or have not already addressed somewhere in the text. We share Weiss and Woolford's concern with the possible confounding effects of population structure and selection on historical-phylogenetic analysis. Incidentally, one reason we consider the genetic data less reliable than the dental data is methodological. All the dental data were collected by a single individual and therefore subject to no interobserver error effects, whereas the genetic data represent an amalgamation of diverse sources often with unknown reproducibility. We also concur with Weiss and Woolford's call for more genetic data (especially from restriction-enzyme studies of DNA) collected from both sides of the Bering Strait. These nuclear and mitochondrial DNA fragments may provide the crucial genetic test of our three-migration hypothesis, wherein substantial population diversity originated in Asia, against Laughlin's plausible alternative of a single small migration 15,000–16,000 years ago with subsequent diversification taking place in the Americas. It is clear that Szathmari would heartily endorse such a test.

Szathmari, however, does bring up one substantive issue for which we have no immediate answer. She questions our use of the Gm allotype data presented in Williams et al. (1985) as direct support for our tripartite-peopling model. We have accepted those researchers' analysis as valid. Specifically, we tentatively agree with their conclusion that "when the current Gm distributions are analyzed with respect to the three-migration hypothesis, there are three distinct Gm distributions for the postulated migrants: Gm^{1,2,21}, Gm^{1,2,21} for the Paleo-Indians . . . ; Gm^{1,21}, Gm^{1,2,21}, and Gm^{1,11,13} for the second wave of Na-Dene hunters . . . ; and Gm^{1,21} and Gm^{1,11,13} for the Eskimo-Aleut migration 9,000 years ago" (Williams et al. 1985:1). Since we have never discussed Gm^{1,2,21} distribution in detail, Szathmari's comments about polymorphic frequencies, admixture, and the correctness of the assumption that "this gene has been absent from unadmixed Eskimos at any time in their history" pertain to the Williams et al. analysis. There is clearly dis-

agreement between Williams and Szathmary. According to Williams et al. (p. 17), "these allotypic markers cannot support the conclusions of Szathmary and Ossenberg (1978) and Szathmary (1979, 1981) that the Na-Dene are closer to the Eskimo than are other Native Americans. Each category has its own unique distribution." As Szathmary reports here, she has since included the data used by Williams et al. in a cluster analysis of a 16-locus genetic-distance data base and concluded from its results that the Athapaskan-speakers (Na-Dene) are genetically closer to Eskimos than are other Indians. Thus, the qualitative distinctions emphasized by Williams et al. seem less secure when multivariate analyses are carried out with additional loci. Even if the Na-Dene are genetically closer to the Aleut-Eskimos than are the rest of the Amerinds, this by itself does not logically falsify our qualitative trichotomy or our three-wave model (perhaps the Na-Dene and Aleut-Eskimo shared a most recent ancestor in Asia before the bifurcation event). Nevertheless, if the genetic connections between these populations were to prove substantial and not due to gene flow, Laughlin's marvelous last two sentences would take on more than an analogical referent and an American divergence would be considerably more probable.

The diversity of commentator opinion usefully illustrates that there are still opportunities for research into Native American cultural, linguistic, and biological microevolution, variation, and origins. Meanwhile there is a need for a working consensus on the basic elements of this relatively simple and short chapter in late Quaternary human population history. It is worth trying to establish some form of working agreement, since the New World case has considerable potential as a modeling tool for reconstruction of the more complex biological and cultural evolution in the Old World. We will use the commentators' remarks to identify where consensus seems near and where additional work and thought are apparently needed.

Ultimate origin. Laughlin agrees that the homeland of all Native Americans was in Eastern Asia, probably North China, and not Europe. No commentator disagrees on this point.

Timing. Again, Laughlin agrees with the suggested departure date of about 20,000 years ago, and no commentator disagrees.

Number of migrations. All commentators have strong views on the number of migrations, but, with the exception of Campbell, who seems to have trouble recognizing the absurdity of many migrations if New World languages are not classified into a few large groupings, the others allow that only one to three migration waves are needed to explain New World genetic, cultural, linguistic, and other variation. Laughlin's single-migration proposal is an important one because it recognizes the potential richness of the late Pleistocene Bering land bridge river systems, whereas nearly all other Beringian scholars have focused on reconstructing the terrestrial habitat and game possibilities for Paleo-Indian food sources. Laughlin is to be encouraged to start assembling evidence for this revision of his (1963) classic two-wave hypothesis.

Weiss and Woolford thoughtfully explore some of the more theoretical aspects of the three-migration hypothesis. Given that Weiss (with Maruyama 1976) earlier reached negative conclusions about reconstructing Pleistocene and Holocene racial history on the basis of genetic evidence, it is understandable that a residual negative outlook persists. However, it is not directed at the dental evidence, because Weiss and Woolford seemingly understand the diachronic power of paleontology. Unlike genetic markers in living populations, which are limited to synchronic "possible" evolutionary scenarios, archaeologically obtained dental remains are the actual record of human population history and evolution. Thus, we hope for more archaeological and skeletal data as well as the new genetic information they seek.

Divergence before or after leaving Siberia. With the excep-

tion of Laughlin, the commentators seem willing to allow that some of the New World dental and genetic variation first evolved in Siberia. Study of a good East Siberian landform map will show how implausible it would have been for a northward-expanding late-Pleistocene Siberian population not to have broken into several relatively isolated small population systems, each derived from a limited number of founders. As indicated in our paper, at least two East Siberian population systems can be proposed on archaeological grounds—blade-making sea-mammal-hunting and fishing folk of the lower Amur and Hokkaido and the terrestrial- and riverine-resource-based Diuktai people between the Amur and the Lena basin. It seems well established that members of both of these groups reached Alaska. The all-important stratigraphy of the Dry Creek site near Fairbanks, Alaska, demonstrates that both were almost certainly preceded by Paleo-Indians. If all Native American variation arose from a single founding population, then why is the Uto-Aztecan premolar never found in Na-Dene, Northwest Coast, Greater Northwest Coast (or whatever label one chooses to identify the far western prehistoric Canadian and Alaskan people), or Aleut-Eskimo crania? Laughlin is correct that the dental cladogram does not convincingly reveal three migrations. This particular analysis does not show the three New World dental clusters as well as have other analyses that do not include Asian samples (see Turner 1986). For this reason the actual mean-measure-of-divergence values are provided. Study of this matrix will reveal three New World dental clusters.

Correspondences between New World languages, dentition, and genetics. The essence of our paper is that three researchers working more or less independently find three sets of the New World data to have reasonable spatial correspondences and, when held up against the archaeological record of Siberia and the Americas, to suggest three waves of late Pleistocene migrants exploiting three different Beringian niches. The correlation between biology and language has persisted because the New World isolation until European contact prevented the sort of massive disruption which followed contact. That variation in language, teeth, genetics, and archaeology can be interpreted to hypothesize three migrations must be more than coincidence. Models of one, two, or scores of migrations are incapable of accommodating this diversity of independent information. Turner (1983) has evaluated models of one to four migrations, but only the three-wave scenario stands up. For this reason we maintain that it is the best all-round explanatory device for the New World biological and cultural variation under review. Had that variation, particularly the biological, correlated with some environmental element, it is doubtful that we could have called on the peopling of the Americas as an explanatory mechanism; instead, some selective agency would likely be under discussion. Until a different evolutionary scenario better explains our similar linguistic, genetic, and dental classifications, a multiple-, preferably three-, migration hypothesis most adequately accounts for the data presented by the commentators and ourselves.

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