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Is Algonquian Amerind?

*It seemed and still seems to me that
the general cumulative evidence presented
is so strong and that many of the specific
elements compared are so startlingly similar
that no reasonable doubt could be entertained
of the validity of the claim.*

—Edward Sapir (1915)

When Edward Sapir, in 1913, announced his brilliant discovery that Wiyot and Yurok—two seemingly isolated languages on the Northern California coast—were related to the widespread Algonquian family that extended from the Great Plains to the Atlantic seaboard, he initiated a debate that is as fiercely argued today as it was then. The central question was *how* one could prove that a group of languages (or language families) were related, that they shared a common ancestor. In support of his hypothesis Sapir presented a fairly extensive list of grammatical and lexical similarities, including his *pièce de résistance*, virtual identity in the pronominal prefixes used to indicate the first-person, second-person, third-person, and indefinite possessor (i.e. ‘somebody’s, a’).¹ After laying out the evidence for these four prefixes in Algonquian, Wiyot, and Yurok, Sapir concluded: “I fail to see how any ingenuities of mere ‘accident’ could bring about such perfect accord in use and form of possessive pronominal elements” (1913: 622). Sapir’s colleague and friend Alfred Kroeber, who had himself earlier in the year discovered (with Roland Dixon) the Penutian and Hokan families, was equally impressed with Sapir’s evidence, writing him on July 30, 1913: “The pronouns turn the trick, alone,

¹ These are presently reconstructed for Proto-Algonquian as **ne*- ‘my,’ **ke*- ‘your,’ **we*- ‘his,’ **me*- ‘a’ (see Goddard 1975: 251).

but the rest looks good” (quoted in Golla 1984: 112). The material available on Wiyot and Yurok at this time was scanty, and Sapir did not expect that every etymological connection he proposed would stand up to scrutiny; he did, however, firmly believe that he had proved his case: “I am well aware of the probability that a considerable number of my lexical and morphological parallels will, on maturer knowledge, have to be thrown out of court; I cannot hope to have always hit the nail on the head. However, even if we eliminate fifty per cent. of our cognates as errors of judgment (doubtless far too great a sacrifice to caution) we are still confronted with no fewer than one hundred or more reasonably close analogies in stems and morphological elements” (1913: 639).

THE ATTACK ON SAPIR

The task of judging the validity of Sapir’s hypothesis fell to the leading Algonquianist of the day, Truman Michelson, the son of the first American Nobel Laureate, Albert A. Michelson, who received the Nobel prize in physics in 1907. After receiving a Ph.D. in Indo-European philology from Harvard in 1904, Michelson spent the rest of his life studying the Algonquian family. Michelson could hardly avoid expressing an opinion on this quite unexpected, and seemingly improbable, relationship, and indeed he recognized that “the importance of this discovery, if valid, can hardly be overestimated” (1914: 362). Nevertheless, he dismissed Sapir’s putative cognates as “fancied lexicographical similarities”; he criticized Sapir for comparing “different morphological elements”; he asserted that what few resemblances remained were simply “accidental”; and he concluded that “[e]nough has been said to show the utter folly of haphazard comparisons unless we have a thorough knowledge of the morphological structure of the languages concerned” (1914: 362, 365, 366, 367). His conclusion was so harsh and “invidious” (Haas 1958: 161) that the Algonquian-Ritwan relationship (Ritwan = Wiyot + Yurok) came to be considered, by the academic community at large, an unresolved taxonomic puzzle.

Sapir and Kroeber never wavered in their conviction that Wiyot and Yurok were indeed cousins of Algonquian. In an exchange of letters following the appearance of Michelson’s rebuttal (Golla 1984: 151–54), Sapir wrote to Kroeber: “His [Michelson’s] narrowness of outlook . . . is quite apparent . . . I am particularly surprised to see that he makes such an excessive use of what I would consider purely negative evidence.” Kroeber was even less impressed with Michelson’s arguments: “Michelson’s review strikes me as puritanical. I have never had any doubt of the validity of your union of Wiyot and Yurok with Algonkin. . . . I hardly consider it worth while seriously to refute

Michelson. His attitude speaks for itself as hypercritical and negative. . . . I regard the case in point so one-sided as to be already conclusively settled.” As other, similar disputes over genetic affinity arose (e.g. Hokan, Penutian, Na-Dene), there quickly developed two opposing camps. On the one side were scholars such as Kroeber and Sapir who interpreted lexical and grammatical similarities as evidence of genetic relationship. On the other side were men such as Michelson, Pliny Goddard, and Franz Boas who attributed these perceived resemblances to misanalysis, borrowing, and chance. We might call the former Geneticists and the latter Diffusionists. Both camps survive to the present day.

A MODERN PERSPECTIVE

When one reads the Sapir-Michelson confrontation today, one can hardly fail to be struck by two things. First, the case that Sapir presented was simply overwhelming. And second, whatever talents Michelson may have had as an Algonquianist—and these apparently were considerable—he had little understanding of basic taxonomic principles, and the vast majority of his objections to Sapir’s hypothesis were irrelevant. In fact, most of his rebuttal was taken up with listing ways in which Wiyot and Yurok differed from Algonquian, as if this negative evidence could somehow offset the positive evidence that Sapir had offered! Nonetheless, Michelson’s reputation was such that his denial alone was sufficient to prevent the general acceptance of Sapir’s proposal. There can be no doubt that had Michelson given his approval *there would never have been a controversy at all*. But in the face of his vigorous, if ill-conceived, dissent, outsiders were understandably hesitant to question him, loath to overrule an expert on his home ground.

There the question stood until, in the early 1950’s, Joseph Greenberg, fresh from his landmark classification of African languages, reexamined the controversy. He found that the real puzzle was not whether Wiyot and Yurok were related to Algonquian, but why there was any doubt about it: “[E]ven a cursory investigation of the celebrated ‘disputed’ cases, such as Athabaskan-Tlingit-Haida and Algonkin-Wiyot-Yurok, indicates that these relationships are not very distant ones and, indeed, are evident on inspection” (1953: 283). In 1958, with new fieldwork on both Wiyot and Yurok at her disposal, Mary Haas examined the alleged relationship, and pronounced herself “in agreement with Greenberg’s remarks about this relationship.”² Thus, by the end of the

² Haas 1958: 160. This article is often incorrectly interpreted as the conclusive proof that Sapir was right about Algonquian-Ritwan: cf. Campbell and Mithun (1979: 26): “a relationship controversial at the time, but subsequently demonstrated” and Goddard (1975: 249): “At present, however, largely as a result of new data provided by recent fieldwork on Wiyot and Yurok, scholars are in general agreement that the daring hypothesis of Sapir

1950's, with all the experts now on one side of the debate—Sapir's side—the rest of the linguistic community was quite willing to let this pseudo-controversy die the quiet death it had for so long deserved.

But if this particular dispute was resolved, the underlying disagreement between the Geneticists and the Diffusionists remained practically untouched. And, ironically, it was the Diffusionists who came to dominate Amerindian linguistics, Sapir's successes in argumentation notwithstanding. The one scholar who might have counterbalanced this unfortunate swing of the pendulum, Morris Swadesh (a brilliant linguist and student of Sapir), was effectively banished to Mexico for political reasons, where he died in 1967. With the Diffusionists firmly in control of the Amerindian establishment, a series of conferences was held at which the proposed higher-level groupings of Kroeber and Sapir were dismantled, one by one, until the list of independent families in North America approached what it had been at the start of this century. Seen from this perspective (see Campbell and Mithun 1979), Sapir's lone surviving success would be the Algonquian-Ritwan grouping. Almost all of his other proposals were abandoned, and the field settled into a great array of specialist preoccupations.

THE ATTACK ON GREENBERG

The calm was broken in 1987 by the appearance of Greenberg's classification of New World languages, the result of some thirty years of research and the compilation of the most extensive Native American wordlists ever assembled. Greenberg's proposal that all the languages of the Western Hemisphere belonged to one of three phyla (Eskimo-Aleut, Na-Dene, or Amerind) was so at variance with the prevailing Diffusionist climate that it immediately provoked a firestorm of criticism from one Diffusionist after another, each reminiscent of Michelson's attack on Sapir. Even before Greenberg's book was published—and without seeing the evidence it contained—Lyle Campbell called for Greenberg's classification to be “shouted down” (1986: 488). A year later Campbell wrote: “In light of this disregard for the work in the American field, it is indeed surprising that a publisher of the calibre of Stanford Press agreed to publish [*Language in the Americas*]; it is tempting to speculate that this would not have been possible if the book did not bear

is, indeed, correct.” Haas herself, by seconding Greenberg's conclusion on the obvious nature of the relationship, shows that she considered the matter to have been already settled—presumably by Sapir in 1913. Moreover, she explicitly states that “my purpose in preparing the present paper is to give the evidence not given by others in support of the Algonquian-Ritwan affiliation” (p. 160). See also Haas (1966) for further discussion of the Sapir-Michelson controversy.

G[reenberg]'s name. A scholar of lesser renown would not have been permitted to slight the canons of scholarship in this way" (1987; quoted from *Mother Tongue* 5: 22). Terrence Kaufman criticized Greenberg for "comparing words whose morphemic make-up he does not understand," for not "hold[ing] back on the task until accurate and extensive data are available," and for "avowed values [that] are subversive and should be explicitly argued against."³ Wallace Chafe saw Greenberg's "book as a random collection of chance resemblances and resemblances due to diffusion indiscriminately mixed with some that do reflect the common origins of some subsets of these languages" (1987: 653). Victor Golla, after first endorsing the accuracy and usefulness of Greenberg's book,⁴ changed his mind a year later, for reasons unknown. In a thoroughly negative review Golla concluded that "[v]ery little of this [Greenberg's classification] will be taken seriously by most scholars in the field" (1988: 435), primarily because Greenberg's proposed etymologies do not observe regular phonological correspondences. That Greenberg devoted the first chapter of his book to just this question Golla fails to mention. William Bright warned potential readers of the book that "most scholars in native American comparative linguistics regard Greenberg's methodology as unsound" (1988: 440). Finally, Ives Goddard, like Michelson an Algonquianist with a Ph.D. from Harvard and effectively holding Michelson's "chair" at the Smithsonian Institution,⁵ dismissed Greenberg's book as a worthless conglomeration of "[e]rrors in the Algonquian data, . . . incorrect or unsupported meanings, . . . incorrect analyses, . . . chance [resemblances], . . . [and] unacknowledged segmentations. . . . [Greenberg's] technique excludes historical linguistic analysis . . . [and] is so flawed that the equations it generates do not require any historical explanation, and his data are unreliable as a basis for further work" (1987a: 656–57). Worse, Goddard attributed Greenberg's errors not simply to carelessness, haste, or incompetence, but to dishonesty: "Greenberg makes often unacknowledged segmentations that are not based on grammatical analysis but merely serve to make the forms being equated seem more similar than they really are. . . . Such distortions are an integral part of Greenberg's technique" (1987a: 657). Thus, the charges leveled against Green-

³ Kaufman 1990: 16, 63. Inasmuch as Greenberg was almost 72 at the time his book was published—over three decades after he had first announced his classification—one can only wonder just how long Kaufman expected Greenberg to "hold back."

⁴ "I do not mean to challenge its fundamental accuracy. Greenberg has provided us with a useful survey of lexical similarities among the languages of the Americas on a scale far beyond anything previously attempted. His identification of common 'Amerind' elements uniting all American Indian languages with the exclusion of Na-Dene and Eskimo-Aleut will be a lasting contribution to American Indian linguistics" (Golla 1987: 658).

⁵ Michelson worked for the Bureau of American Ethnology in Washington, D.C.

berg's Amerind hypothesis are effectively a resurrection of those directed at Sapir's Algonquian-Ritwan hypothesis: misanalysis, undetected borrowings, and chance resemblances, with a dose of *ad hominem* invective in the bargain.

AN ANALYSIS OF THE ATTACK ON GREENBERG

Whatever its virtues or failings, an assault of this breadth and character carries an air of verisimilitude, and history demands that its particulars be examined. As one of the principal instruments in the assault, Goddard's two-page review of *Language in the Americas* bears closer scrutiny. One of his charges is that because Greenberg's work is based on linguistic data from *contemporary* languages,⁶ it cannot tell us anything about linguistic prehistory; the resemblances Greenberg notes are just as likely to result from accidental convergence as from common origin. Goddard also claims that "[i]n a proper etymology every divergence must be explained by a postulated change consistent with a complete historical hypothesis" (1987a: 657) and, furthermore, "[w]here grammatical elements are etymologized it is necessary to present an hypothesis about the system of which they are a part in its entirety" (1975: 255). This pronouncement (already doubtful, on its face) did not, however, prevent Goddard from "etymologizing" an intercalated **-t-* for Proto-Algonquian-Ritwan with no hypothesis whatsoever about its historical source: "In Algonquian and Wiyot vowel-initial non-dependent nouns insert a *-t-* after the [pronominal] prefixes: Fox *ahkohkwa* 'kettle,' *netahkohkwa* 'my kettle' (with *ne-* plus *-t-*); Wiyot *iʔl* 'intestines,' *dutíʔl* 'my intestines' (with *du-* and *-t-*)" (1975: 252).

Now if one artificially limits one's perspective to the Algonquian-Ritwan family, then the historical source of this mysterious **-t-* is indeed obscure. And it is precisely in such cases that the broader context of the Amerind phylum, with its greater chronological depth, can clarify unresolved issues. In his extensive discussion of the origin and development of the Amerind pronominal system Greenberg adduces evidence from eight of Amerind's eleven basic subgroups for a demonstrative/third-person pronoun whose original form was probably **ti* or **ta* (1987: 44-48, 281-83). From the perspective of his already classic study of the origin of gender markers (1978: 47-82), Greenberg then surveys the development of this demonstrative element throughout the Amerind family. In some branches the original demonstrative meaning is preserved, either exclusively (e.g. Macro-Tucanoan) or in part (e.g. Andean, Macro-Carib, Macro-Panoan). In other branches the demonstrative has developed into a third-person pronoun (e.g. Macro-Panoan, Macro-Ge,

⁶ This is not true. Greenberg also used reconstructed forms, where they exist; the sources of these are listed on p. 181 of his book.

Andean, Penutian) or relative pronoun (e.g. Macro-Panoan). In Macro-Ge and Macro-Panoan it has also developed into a marker of the masculine gender. Finally, in its semantically most eroded form, it occurs in Hokan and Algonquian-Ritwan reduced to a Stage III article, that is, a mere marker of nominality. What is particularly striking is that in Hokan and Macro-Carib, as in Algonquian-Ritwan, this element occurs only before stems beginning with a vowel (e.g., in Carib, Pemon *i-paručí* ‘his, her sister’ vs. *i-t-enna* ‘his, her hand’). Greenberg explains this apparent anomaly as a consequence of an originally ergative pronoun system in Amerind. Proto-Amerind contained two sets of pronouns, an *ergative* set used as the subject of a transitive verb (and in nominal possession, e.g. ‘my foot’), and an *objective* set used as the subject of an intransitive verb or object of a transitive verb (and in nominal predication, e.g. ‘I am a man’). In Proto-Amerind the ergative third-person pronoun was **t*, while the objective third-person pronoun was **i*.⁷ In a number of cases, when the ergative system broke down, what had originally been morphologically conditioned variants of an ergative system became phonologically conditioned. At a later date this irregularity was ironed out by the addition of the “regular” pronoun **i* to all stems, leading to the anomalous distribution of *t* in the Pemon example above. From the deeper perspective of the Amerind phylum, idiosyncratic—and synchronically unmotivated—elements such as the mysterious Algonquian intercalated **-t-* can be explained by the action of well-attested diachronic processes. Such integration of typological pattern with diachronic process is one of Greenberg’s many contributions to general linguistics.

Goddard’s reluctance to consider the broader Amerind context should come as no surprise, since he has already shown a predilection for maintaining the status quo against historical reality, even within the Algonquian family. When Leonard Bloomfield reconstructed Proto-Algonquian in 1925 he posited a proto-segment **θ* to account for an alternation between *t* and *n* in the four languages he considered. In a later work (1946: 87), Bloomfield gave the enigmatic gloss “an unvoiced interdental or lateral?” to describe this segment. But in his survey of comparative Algonquian, Goddard concluded that “[i]t is hard to see what testable consequences the assumption of one or the other phonetic value for **θ* would have . . . and this small point of uncertainty may be allowed to remain” (1979: 73). Other Algonquianists,⁸ however, have argued persuasively that there *are* testable consequences, and that both internal and external evidence points to an original voiceless lateral fricative **ɬ*, not the totally improbable **θ*. This is but one example of the narrow-mindedness

⁷ The extensive distribution of this formative in the Amerind phylum is discussed by Greenberg on pp. 279-81.

⁸ Siebert 1975, Picard 1984, Proulx 1984.

that has characterized Algonquianists from Michelson to Goddard, and that has been duly criticized by Paul Proulx:

Some reluctance to revise PA [Proto-Algonquian] reconstructions on the basis of Algic data is understandable: PA is familiar territory and generally seems securely reconstructed. In contrast, Proto-Algic is very unfamiliar and may seem speculative. But all linguistic reconstructions, including PA, are biased toward the present. It is precisely archaic features in the protolanguage which are the most difficult to reconstruct, and it is here that a deeper time level is invaluable. The reluctance to use Proto-Algic in Algonquian studies must be overcome, for many of the most recalcitrant problems of Algonquian will be understood in no other way. [1984: 205]

But far from using Greenberg's book to inform his own field of study, Goddard's sole purpose, both in his review and in his public lecture (1987a,b), has been to attempt to discredit Greenberg's tripartite classification of the languages of the New World, and in particular to deny that the Algic family (Algonquian + Wiyot + Yurok) has any known relatives. The claim is thus the same as Michelson's, except that Goddard is willing to accept another two languages, Wiyot and Yurok, into the family. Goddard attacks Greenberg's classification by a series of criteria, each of which supposedly "invalidates" a certain number of the 2,000 etymologies Greenberg offered in support of Amerind and its subgroups. He restricts his attention to the 142 lexical etymologies in which Algonquian forms are cited; the *grammatical* support for Amerind—to which Greenberg devotes an entire chapter—is not mentioned. In light of Goddard's claim that "it is virtually impossible to prove a distant genetic relationship on the basis of lexical comparisons alone" (1975: 255), it is puzzling that he should choose to examine the evidence he finds least persuasive, and to totally ignore the grammatical evidence on which he puts so much weight. In any event, of the 142 lexical cognate sets involving Algonquian, Goddard is able to invalidate, by his criteria, all but 35. The significance of the remaining 35 etymologies, which stand up to Goddard's most rigorous methodology (but are not explicitly identified), is not discussed. But it is in fact more illuminating to examine those etymologies that Goddard claims to have invalidated, that we might better understand how his techniques work.

According to Goddard, errors in the Algonquian data invalidate 93 of the etymologies, the largest source of disqualification (34) being cases where Greenberg has cited unrelated Algonquian forms. Goddard gives four examples of etymologies he eliminates by this criterion, and examining one of the four here should prove instructive. In Amerind etymology No. 238 (SMELL₁), Greenberg includes three of Bloomfield's Proto-Central Algonquian reconstructions: **mat*, **mat^si* 'smell'; **mi:s*, **mit* 'excrement'; and **mači* 'bad.' Goddard objects that **mat* 'bad' and **mi:t* 'defecate' are etymologically unrelated, and hence the Amerind etymology is invalid. But if Bloomfield's three roots *are* etymologically distinct, then Greenberg has simply mixed together

two distinct roots, in which case the etymology should be broken up into two separate etymologies, not done away with altogether. (*Croisement de racines* is not exactly an unknown phenomenon in etymological dictionaries!) In the case at hand the etymology is apparently restricted to the three branches of Northern Amerind—Almosan-Keresiouan, Penutian, and Hokan—and thus constitutes one of the pieces of evidence for this higher-level grouping. The original meaning, SMELL BAD/STINK, is preserved either exclusively or in part in all three subgroups of Northern Amerind. In Hokan the meaning is uniformly STINK or SMELL; in Penutian and Almosan-Keresiouan, forms with the meanings DIRTY and BAD are cited alongside others meaning STINK or SMELL. All three branches suggest an original form containing three consonants, *M-T-K, the second of which has been assibilated to *t^s*, *č*, *s*, or *š* in quite a few languages, no doubt under the influence of a following palatal vowel that is preserved in some of the languages. Thus a form such as *MATIK would constitute a reasonable hypothesis for the original phonetic shape, and such a form is virtually identical with the attested Shasta form (*ku-*)*matik'*(-ik) 'it stinks.' The presence of either a glottal stop or glottalized consonant is also characteristic of the root in question, but its original locus is hard to pinpoint. Taken all in all, the thirty forms cited in the etymology are sufficiently similar in sound and meaning that few linguists would be so rash as to reject the entire etymology. One may quibble over certain parts of an etymology, but such loose ends, particularly in a pioneering work like Greenberg's, hardly invalidate the core of the etymology.

Goddard's second criterion invalidates 21 etymologies that involve Blackfoot but no other Algonquian language. By his lights, such roots cannot be reconstructed for Proto-Algonquian and hence are not available for comparison further afield. In fact Blackfoot is, by Goddard's own admission, the most divergent Algonquian language, and the fact that it should have preserved certain roots that have been lost in the rest of the family is therefore not only not surprising, but exactly what one should expect. From *within* Algonquian, of course, it is impossible to tell which of these Blackfoot roots are innovations that serve to define Blackfoot as a distinct genetic group, and which are inheritances from Proto-Algonquian that have been lost elsewhere in the family. That distinction cannot be made without considering Algonquian *in a wider context*, as Greenberg has done, using the method known in biological taxonomy as out-group comparison. But instead of recognizing that Greenberg's methodology of multilateral comparison has revealed certain Blackfoot roots that must also have existed in Proto-Algonquian, even though they have left no trace elsewhere in the family, Goddard uses the isolated nature of the Blackfoot form to dismiss the entire etymology. A more egregious *non sequitur* one can scarcely imagine.

Just how silly Goddard’s “methodology” is can be demonstrated by another example. One of the pieces of evidence that Sapir adduced in 1913 to connect Algonquian with Wiyot and Yurok was the resemblance of Blackfoot (*mo-*)*kíts(-is)* ‘finger’ and Wiyot (*mo-*)*kèc* ‘fingers.’ Sapir was so impressed by this correspondence that he mentioned it in a letter to Kroeber even before his article had appeared, with the comment: “Are these ‘accidents’? Fiddlesticks!” (Golla 1984: 120). To Sapir’s comparison Greenberg added the Yurok form (*cey-*)*ketew* ‘(little) finger,’ as well as Salish forms such as Squamish *čis* ‘hand.’ Goddard rejects the entire etymology, and many other cogent etymologies, simply because the form in question cannot be reconstructed for Proto-Algonquian. For Goddard, such striking lexical resemblances are mere coincidences not requiring historical explanation. But were we to accept his methodology, we could no longer compare even Wiyot *kʔæl-* ‘to ask’ with Kutenai *akʔeʔ* ‘to ask,’ because the former cannot be reconstructed for Proto-Algic. For Goddard, then, Greenberg’s book is simply one remarkable coincidence after another, all without historical import. Also coincidental must be the high degree of correlation between linguistic taxa and biological taxa, including an Amerind group distinct from the rest of the world’s populations.⁹ Perhaps coincidental as well is the fact that a statistical analysis of the distribution of Greenberg’s Amerind etymologies produces a subgrouping and mapping of the eleven Amerind subgroups that are highly plausible on geographical grounds (see Ruhlen 1991).

The example above is symptomatic of the many erroneous methodological pronouncements that Goddard proffers in his review of Greenberg’s book. He also instructs us that “even stricter guidelines are obviously necessary in proposing comparisons between languages whose relationship is in question” (1987a: 657). Again, he claims that “[h]istorical method requires that the facts of each subgroup or family be established separately before being compared with each other. Hence, while Algonquian **neθk-* [‘arm, hand’] and Northern Iroquoian *nētsh* [‘arm’] may properly be compared, if desired, forms descended from these may not” (1987: 657). First, this proclamation is not true; and second, it violates the very point that Goddard is trying to make. Surely he does not believe that each branch of Indo-European was established separately before being compared with other branches. And why is he comparing *Northern* Iroquoian with *Algonquian*? Both of these are intermediate nodes (Algonquian under Algic and Northern Iroquoian under Iroquoian), precisely the sort of elements he claims cannot be compared! Had he claimed that only

⁹ See Cavalli-Sforza et al. 1988. The Diffusionist position has always been associated with the belief, expressed by Campbell (1986: 488), that “there is no deterministic connection between language and gene pools.” Geneticists, such as Trombetti (1905: 55), have long realized that “agreement between language and race is the rule. Disagreement is the exception.”

Algic can be compared with Iroquoian, at least the illustration of his principle would have been aptly selected, even if the principle itself were not. How he *should* be proceeding has been lucidly explained by Proulx: “When both PA [Proto-Algonquian] and Proto-Algic forms are reconstructed from a single pair of cognates (e.g. Menominee and Yurok), the PA reconstruction is *not* logically prior to the Proto-Algic one. The attested forms are constants, the reconstructed ones interdependent variables. . . . [T]he reconstructed forms are not evidence for each other; both depend on the attested ones. The data are scantier in such cases than in most, but this is only a matter of degree” (1984: 167).

Goddard dismisses other etymologies in *Language in the Americas* for a variety of reasons, some as trivial as the misidentification of a language. No doubt he has uncovered some flaws in Greenberg’s Algonquian data, as any specialist would in a work of this breadth, but in no case can these imperfections alone be taken as invalidating an entire etymology. An interesting example is Amerind etymology No. 85 (DIRTY), which Goddard rejects for its “looseness” under the criterion of semantic similarity. The meanings he cites from this etymology—‘excrement’, ‘night,’ and ‘grass’—do seem an unusual combination, and their conjunction provoked a tittering in the audience at his lecture. But an examination of the whole etymology reveals quite a different story. First of all we find, contrary to Goddard’s semantic characterization, that this etymology is not about feces, but about color, specifically the area of the spectrum encompassing black and green. In Almosan the meaning is uniformly BLACK and, as we shall see, the distribution of the various meanings throughout North and South America suggests that this was the original meaning. Keresiouan shows both DARK IN COLOR (Iroquoian) and GREEN (Keresan). In Penutian the meaning has shifted completely to GREEN and its close semantic connections GRASS and BLUE; the original meaning of BLACK/DARK is not attested. In South America, Macro-Tucanoan preserves the original meaning of BLACK in Proto-Ge **tik*, but in Cayapo and Chiquito the meaning has shifted to DIRTY. Finally, in the Equatorial group the meaning is uniformly EXCREMENT. Phonetically, the original form was probably similar to Chiquito *tuki*, though Penutian and Almosan forms raise the possibility that one of the consonants was originally an ejective. Paralleling Penutian’s semantic divergence (BLACK > GREEN) is the presence of a reduplicated stem in the Plateau, California, and Mexican subgroups (e.g. North Sahaptin *tʰəktʰək*, Rumsien *čuktuk*, Zoque *tʰuhtʰuh*).¹⁰

¹⁰ Campbell (1988: 600) also criticizes Greenberg’s etymologies as “quite permissive in semantic latitude. Semantic equations such as the following are not convincing: ‘excrement/night/grass,’ And these are only some of G[reenberg]’s unconvincing semantic equations.”

We can see now how Goddard's analysis proceeds. By failing to mention the fundamental meaning of an etymology, and citing only semantic extensions from this unnamed core, he makes it appear that Greenberg has combined meanings in an arbitrary and capricious fashion. When the whole etymology is examined, however, Goddard's shabby trick is revealed, and the semantic cohesiveness of the entire etymology can hardly be doubted. In resorting to such tactics, Goddard simply reveals the weakness of his own position.

THE VERDICT

In many respects the Diffusionist critique has remained constant from Michelson to Goddard, its chief ingredients being a list of errors (real or imagined), an allusion to accidental resemblances and borrowings, a willful disregard of the most convincing positive points, a dose of taxonomic nonsense, and often an appeal to authority. The current crop of Diffusionists seldom fails to mention "how far outside the mainstream Greenberg's work lies" (Golla 1988: 434) or that "most scholars in native American comparative linguistics regard Greenberg's methodology as unsound" (Bright 1988: 440). Both statements are of course true, but that they *are* true is irrelevant to the truth or falsity of Greenberg's claims. When his work in African classification appeared, almost four decades ago, it was just as far outside the mainstream. And in retrospect it should not be surprising that the foremost Bantuist of the day, Malcolm Guthrie, vehemently rejected Greenberg's proposal that the Bantu family was a relatively minor branch in a larger Niger-Congo family. Though an expert in some family would seem to be in the best position to judge whether or not that family is related to some other family, in practice such experts are often the least receptive to new relationships, as the examples of Michelson, Guthrie, and Goddard demonstrate. Similarly, Indo-Europeanists (with a few notable exceptions) are notorious for their dogmatic denial that Indo-European has any known relatives, a position held despite the overwhelming evidence to the contrary adduced by the Nostratic school and others. At the turn of the century Henry Sweet characterized the narrow-mindedness of Indo-Europeanists in terms that apply equally well to today's Diffusionists:

In philology, as in all branches of knowledge, it is the specialist who most strenuously opposes any attempt to widen the field of his methods. Hence the advocate of affinity between the Aryan [= Indo-European] and the Finnish [= Finno-Ugric] languages need not be alarmed when he hears that the majority of Aryan philologists reject the hypothesis. In many cases this rejection merely means that our specialist has his hands full already, and shrinks from learning a new set of languages. . . . Even when this passively agnostic attitude develops into aggressive antagonism, it is generally little more than the expression of mere prejudice against dethroning

Aryan from its proud isolation and affiliating it to the languages of yellow races; or want of imagination and power of realizing an earlier morphological stage of Aryan; or, lastly, that conservatism and caution which would rather miss a brilliant discovery than run the risk of having mistakes exposed. [1901: *vi*]

The most telling datum favoring the Amerind phylum was discovered independently by several different scholars, including Trombetti, Sapir, Swadesh, and Greenberg.¹¹ They all noticed that first-person *n*- and second-person *m*-seemed to characterize American Indian languages from Canada to the tip of South America. Since these two pronouns are known to be among the most stable items in language (see Dolgopolsky 1964), and are rarely borrowed, their broad distribution throughout the New World, as impossible to overlook as a herd of buffalo, has always constituted an inescapable problem for the Diffusionists. Boas attributed the prevalence of these two pronouns in Amerindian languages to “obscure psychological causes”; today’s Diffusionists call them “Pan-Americanisms.”¹² Both terms are simply euphemisms for the proscribed word “cognate”; for the Diffusionists, cognates exist only within homogeneous low-level groups like Algonquian, Siouan, and Salish. For apparent cognates between distantly related groups, almost any explanation, no matter how implausible, is seemingly to be preferred to the simple and obvious explanation of common origin.¹³ Thus, Bright proposes that “Pan-Americanisms” arose in Asia through borrowing among different Amerind groups *before these distinct groups migrated to the New World*:

I would not be opposed to a hypothesis that the majority of the recognized genetic families of American Indian languages must have had relationships of multilingualism and intense linguistic diffusion during a remote period of time, perhaps in the age when they were crossing the Bering Straits from Siberia to Alaska. We can imagine that the so-called pan-Americanisms in American Indian languages, which have attracted so much attention from “super-groupers” like Greenberg, may have originated in that period. [1984: 25]

Such a scenario would have required a traffic controller at the Bering land bridge, checking the would-be immigrants for the proper “Pan-Americanisms” before admitting them to the New World.

In sum, the evidence that Greenberg adduces for the Amerind phylum is at least as strong as that offered by Sapir for Algonquian-Ritwan, and consid-

¹¹ See Ruhlen 1987 for a discussion of this point.

¹² They really should be called “Pan-Amerindisms” since they usually exclude Na-Dene and Eskimo-Aleut. The fact that the Amerind phylum occupies so much of North and South America has so far permitted the Diffusionists to perpetrate this linguistic sleight of hand unchallenged.

¹³ Cf. Campbell (1988: 597): “those similarities which may *possibly* [my emphasis] have an explanation other than common ancestry must be set aside.”

erably stronger than the evidence Greenberg presented for his African classification, *now universally accepted*. The Amerind phylum is well-defined and amply supported; its validity as a linguistic taxon is firmly established; and in time *Language in the Americas* will be hailed as a monumental achievement. The outrageously vituperative attack on the Amerind phylum by the Diffusionists reflects their blind prejudice, their basic ignorance of the fundamental principles of genetic classification, and perhaps, understandably, an apprehension of redirected research. Appeals to authority and group solidarity cannot save the Diffusionist position, which, after almost a century of dominance in Amerindian comparative linguistics, is finally fading into the night.

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