

On The End of the Ritwan Controversy

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The proposal by Sapir (1913) that Wiyot and Yurok, two languages of northwestern California previously grouped together as “Ritwan” by Dixon and Kroeber (1913), are related to the Algonquian languages occupies a special place in the history of linguistics, for two related reasons: because, unlikely though it seemed, it proved to be true, and because it provides an important example of the issues involved in demonstrating a remote linguistic relationship.

The history of the dispute may be summarized as follows. Sapir (1913) proposed the relationship using the rather limited and poor data available at the time for Wiyot and Yurok, citing as evidence a set of lexical equations (including some grammatical morphemes) without establishing a full set of well-founded sound correspondences.¹ This proposal was attacked by Michelson (1914). Sapir (1915a) replied, Michelson (1915) rejoined, and Sapir (1915b) closed the discussion for the time. Michelson’s critique was to a large extent unsound, based as it was largely on the failure of the Ritwan languages to conform to the Algonquian type. However, weak as the critique may have been, Sapir’s evidence was also weak. Sapir (1923) subsequently added 30 equations of kinship terms.

The result of the debate was inconclusive. Some (Dixon and Kroeber 1919, Trombetti 1921, Rivet et al. 1952, Greenberg 1953, Vinay 1955) accepted the relationship as demonstrated. Most, it appears, did not. Reichard (1925) was not explicit but, as Haas (1958) points out, appears to have taken a negative view of the relationship. Uhlenbeck (1927, 1939) considered the similarities to be the result of diffusion from Algonquian. Bloomfield (1946:85) would not commit himself saying merely “Two languages of California, Wiyot and Yurok, have been suspected of kinship with Algonquian”, nor would Hoijer (1946).

The situation began to change in the late 1950s with fieldwork on Wiyot by Karl Teeter and on Yurok by R. H. Robins and Mary Haas. Stimulated by the new Ritwan material, Haas published her famous paper “Algonkian-Ritwan: the End of a Controversy” (Haas 1958), in which she presented 93 Algonquian-Ritwan equations and detailed sound correspondences. As her title shows, Haas regarded her paper as settling the issue, as have many others. However, based on the new Ritwan material as well as the advances in the study of Algonquian itself, Teeter (1964ab)

¹ According to Trombetti (1921), he had independently recognized this relationship before seeing Sapir’s paper, on the basis of the pronominal possessive prefixes.

and Goddard (1966, 1975) adduced morphological arguments widely considered to have provided the conclusive evidence of the relationship.²

Three subsequent publications are generally considered as merely having added to an already secure conclusion. These are Goddard (1986), which showed that although nearly all of the thirty equations of kinship terms proposed by Sapir (1923) are incorrect, a strong argument can nonetheless be made on the basis of structural similarities of the kinship systems, and Teeter (1974) and Berman (1990), which adduced additional cognate sets.

It is fair to say that the mainstream view is that the Algonquian-Ritwan relationship was not conclusively demonstrated prior to the publication of Haas (1958). Typical is the statement by Elsassner (1978;155) that: “Haas (1958) conclusively demonstrated this relationship in terms acceptable to modern linguists.” Similarly, Pilling (1978;137) wrote: “A later reassertion by Sapir (1929) and Haas’s ultimate proof (1958) of the Algonquian affiliation of Yurok have been accepted by most scholars.” A recent statement of this view, by a scholar intimately familiar with the matter, is that of Teeter (1997:609, fn. 4) that:

... though it is now certain that Algic is a valid entity, Sapir did not establish the matter beyond doubt, as Mary believed.

For some, the solid lexical evidence and extensive sound correspondences adduced by Haas are sufficient. For those who hold the view that such lexical evidence alone is never truly conclusive of genetic affiliation because it can also result from borrowing, although Haas (1958) was a major advance, it was only the publication of Teeter (1964ab) and Goddard (1966, 1975) that concluded the debate.

There is, however, an alternative view, which it is my purpose here to address. This is the claim, by Ruhlen (1991, 1994) and Greenberg (1997) that Sapir’s original paper firmly established the relationship between Ritwan and Algonkian, which failed to be accepted only due to the prejudice of linguists such as Truman Michelson and the influence that Michelson exerted. Indeed, Ruhlen is of the opinion that Sapir’s evidence was later found to be probative (Ruhlen 1991;212):

In point of fact Sapir had himself offered irrefutable evidence in his original 1913 article. Although it is true that this proof was not universally accepted until the 1950’s, the scholarly *acceptance* of proof is an altogether different question from that of *proof* itself, as the history of science has repeatedly shown.

Similarly, Greenberg (1997:669) says:

² Proulx (1991;157-158) appears to be unique among the commentators on this question in denying the particular value of the morphological arguments. He accuses Goddard (1986) of not being sufficiently aware of the possibility of typological similarity and therefore of placing an excessive emphasis on “structural comparison”. However, as shown in detail below, the morphological similarities considered probative by Goddard are specific details, not general similarities in morphological type that might be due to chance.

Since Haas' key article of 1958 a number of misconceptions have arisen, some of which are, in all probability, still current. One of these is that Sapir's (1913) thesis was uncertain until Haas' (1958) proof. I believe that a reasonably careful reading of Haas (1958) will show that she never asserted this. This view is often associated with the notion that it was subsequent data that finally demonstrated what had at first been a brilliant but not fully founded thesis.

Ruhlen makes the even stronger claim that Haas herself viewed the relationship, as presented by Sapir, as obvious. He says (Ruhlen 1994;113-114, fn. 2):

This article is often incorrectly interpreted as the conclusive proof that Sapir was right about Algonquian-Ritwan . . . Haas herself, by seconding Greenberg's conclusion on the obvious nature of the relationship, shows that she considered the matter to have been settled — presumably by Sapir in 1913.

The remark to which Ruhlen refers is the following (Haas 1958; 160, fn. 3):

Since my purpose in preparing the present paper is to give the evidence not given by others in support of the Algonkian-Ritwan affiliation, I am therefore in agreement with Greenberg's remarks about this relationship.

The statement that Haas is in agreement with Greenberg, taken in isolation, is ambiguous, for it is unclear whether Haas means to agree with Greenberg as to the relationship itself or as to its obviousness. However, since she states that she is in agreement with Greenberg because her purpose is to give the evidence not given by others, she must have meant merely that she agreed with Greenberg as to the reality of the relationship. "Since I am arguing that the relationship is real, I agree with Greenberg as to the reality of the relationship" is a logical and coherent statement. "Since I am arguing that the relationship is real, I agree with Greenberg that the relationship is obvious" is not a valid implication.³

Haas' paper provides ample additional evidence that she did not regard the evidence already presented in the literature as sufficient to settle the question (Haas 1958;160):

And any attempt to settle the Algonkian-Ritwan question on the basis of the actual comparative evidence that exists in the literature tends to force one into the completely noncommittal position taken by Hoijer in 1946: 'Sapir . . . set forth the hypothesis that these two [Ritwan and Algonquian] were related. This view was attacked by Michelson.'⁴

³ Note that the phrase "as Mary believed" in the passage quoted above from Teeter (1997) is also ambiguous.

⁴ Haas quotes from Hoijer (1946:13).

Similarly, the language of her reference (Haas 1958:160) to:

... the Algonkian-Ritwan connection, which turns out to be more readily amenable to proof than many of the others he postulated...

implies that she thought that Sapir had not made his case. If she did, we would expect her to have used language like “for which Sapir provided stronger evidence than many others he postulated”. In using the words “turns out”, Haas implies that conditions have changed, that the connection was not clear before, and in using the word “amenable” she implies that the connection could (at the time of writing her paper) be proven, but that it had not yet been.

Haas makes clear that her arguments rely on material that became available after the Sapir-Michelson debate (Haas 1958:162):⁵

In actual fact I have had to rely heavily on unpublished materials for Wiyot and Yurok forms . . . and the Proto Central Algonkian reconstructions which are so necessary for effective comparison have been the result of the labors of several scholars and have been appearing in print at rather widely separated intervals.

She specifically cites the then unpublished work of Robins (1958) on Yurok and of Teeter (1964) on Wiyot, as well as:

1. The publication of Gladys Reichard’s Wiyot material (Reichard 1925);
2. The publication of Leonard Bloomfield’s “On the Sound System of Central Algonquian” (Bloomfield 1925);
3. The publication of Truman Michelson’s “Phonetic Shifts in Algonquian Languages” (Michelson 1935);
4. The publication of Leonard Bloomfield’s “Algonquian” (Bloomfield 1946);

Statements such as the following (Haas 1958:162) make it clear that Haas considered that the Algonkian-Ritwan relationship had not previously been established, and that the evidence available at the time of the Sapir/Michelson debate was not sufficient to establish the relationship.

Ironical though it may be, to Michelson then must go a fair share of the credit for making accessible a good portion of the material needed to establish the relationship he so violently opposed.

As a matter of plain fact, then, the historical sequence of events just described was necessary before the material assembled in the present paper could be brought together and presented with the reasonable hope that the long-standing Algonkian-Ritwan controversy could be brought to an end.

⁵ Haas was hardly alone in this assessment. Proulx (1991:155) makes the same point, stating that the new material “simply made most of his [Sapir’s-WJP] data obsolete”.

The form of Haas' presentation also militates against the view that she thought that Sapir's original evidence was sufficient. If she had, we would expect her to have reviewed the evidence that he presented. Even if she wished, in the same paper, to present her own additional evidence, we would expect her to have distinguished her additions from Sapir's material. This she did not do. Indeed, her evidence turns out to be rather different from Sapir's.

Before presenting her complete list of equations, Haas (1958:161) presented "... a synoptic preview of terms which have cognates ... in all three of the languages", stating:

The above synoptic preview should be sufficient to demonstrate the genetic affinity of these languages for all practical purposes.

Her 'synoptic preview' consisted of twenty equations involving basic vocabulary, in each of which there was a match of at least three consecutive segments. Of Haas' 20 equations, eight are not to be found in Sapir (1913). Of these, five ('my eye', 'his liver', 'grease', 'to suckle', 'to steal') are entirely absent, while for three ('arm', 'his tail', 'tree') Sapir also has an equation with the same gloss, but involving quite different forms. In only twelve of the twenty does Sapir have what we may consider to be the same equation, making allowance for differences in the detail of the cited forms, and even here, in three cases ('louse', 'to drink', 'long'), he had a form in only one of the two Ritwan languages. In short, if we use Haas' own requirement that the equation contain Wiyot, Yurok, and Algonquian members, only nine of her twenty equations are to be found in Sapir. Clearly the evidence that Haas considered probative is quite different from that presented by Sapir. It seems clear that Haas did not consider Sapir to have proven his case. Rather, it was her view that more and better material was needed and that it was her own paper that first made a solid case. Nor was Haas alone in the view that additional data was necessary. Uhlenbeck (1939:43-44) remarked that a larger number of reliable equations would be necessary before it would be possible to establish sound laws.

The ultimate question, of course, is not what Haas thought, but what evidence should and did convince the scientific community. Purely lexical evidence, of the sort provided by both Sapir and Haas, is widely considered to be an inadequate proof of genetic affiliation, since, even when basic vocabulary is involved, lexical correspondances may be the result of borrowing. The most striking evidence for the relationship of Wiyot and Yurok to Algonquian is therefore the system of pronominal prefixes.⁶ Of these, Goddard (1975:250-251) says:

... in each of the three languages there is also a small number of prefixes. These prefixes share so many similarities of form and function, that they alone would be sufficient to demonstrate a genetic relationship between the languages. Each language has a set of precisely four, mutually exclusive pronominal prefixes; they correspond exactly in form and meaning.

A few pages later (1975: 253) he again says:

⁶ The other striking structural piece of evidence is, as Goddard (1986) points out, the very unusual dual kinship terminology, to which Sapir only alluded in a footnote in his 1923 paper.

The resemblances between the pronominal prefix systems of Algonquian, Wiyot, and Yurok would be sufficient to demonstrate the genetic relationship of these languages. These are not vague similarities pulled at random from various parts of the grammar, but represent a single self-contained system which is found in virtually identical form in all three languages.

Greenberg (1997:669) claims that Goddard’s view that the Algonquian-Ritwan relationship had only recently been proved, largely on the basis of new data, is contradicted by his view of the sufficiency of the evidence of the pronominal prefixes, since “. . . the data had already been clearly set forth in Sapir’s 1913 original article.”⁷

On this Greenberg is wrong. It is true that the Wiyot and Yurok possessive prefixes were cited by Sapir (1913: 633), but that is all he did. Sapir was not aware of the detailed resemblances between the pronominal prefix **systems** of the three languages to which Goddard refers. These were only pointed out by Teeter (1964ab) and Goddard (1966, 1975). To be precise:

1. Sapir did not point out the restriction of the indefinite prefix to dependent nouns. (Goddard 1975)
2. Although he cited isolated examples, Sapir was not aware that /t/ is inserted systematically between the prefixes and vowel-initial stems in Wiyot just as in Algonquian. (Teeter 1964b, Goddard 1975)
3. Sapir does not cite the fact that in Algonquian and in Yurok some dependent nouns, including the kinship terms, do not take the indefinite prefix. (Goddard 1975)
4. Sapir does not mention the Wiyot use in all third person possessive forms of a suffix evidently cognate to the Algonquian obviative. (Goddard 1975)
5. Sapir cited only the nominal possessive prefixes. He was evidently unaware of the use of the same prefixes on verbs. He therefore did not point out the fact that the same prefixes appear on both nouns and verbs. (Teeter 1964ab, Goddard 1966, 1975)
6. Sapir does not mention the existence in Ritwan and in Algonquian of verbal paradigms that take the three personal prefixes as well as paradigms that do not. (Goddard 1975)
7. Sapir does not mention that in Algonquian and Wiyot there is a hierarchy whereby, as Goddard (1975:253) describes it: “. . . any form having a second-person participant . . . takes the second-person prefix; any form having a first-person participant but no second person takes the first-person prefix; forms having only third person participants take the third person prefix.” (Teeter 1964ab, Goddard 1966, 1975)
8. Sapir does not mention that in Ritwan and in Algonquian the personal prefixes may be attached to preverbal particles separate from the verb stem. (Teeter 1964b, Goddard 1975)

⁷ Greenberg refers specifically only to the first of these two passages, whose second sentence he quotes, erroneously substituting “are” for Goddard’s “would be”.

9. Sapir does not mention that in Yurok as in Algonquian “. . . there is a pattern of derivation by which verbs of possession are made from third-person prefixed noun stems; when prefixes are used in the verbal paradigm they appear before the derivational third-person prefix.” (Goddard 1975)

Sapir could not assert regarding the pronominal prefixes, as does Goddard (1975:251) that: “. . . they correspond exactly in form and meaning”. He could not make this claim regarding meaning since he was not aware of the detailed parallels in usage just described. And he could not make this claim regarding form since he had not established a full and well founded set of sound correspondences, which was not done until Haas (1958).⁸ There is therefore an enormous difference between the evidence adduced by Sapir and that adduced by Teeter and Goddard. As Sapir made use of them, the pronominal prefixes were no more than four morphemes similar in sound and meaning in the three languages. As evidence of genetic affiliation, they were little stronger than any other four morphemes. As used by Teeter and Goddard, they become part of a complex grammatical system, whose agreement in Wiyot, Yurok and Algonquian is very unlikely to be due either to chance or to diffusion. There is therefore no contradiction in Goddard’s view that these prefixes are sufficient evidence of genetic affiliation in and of themselves but that Sapir’s very different use of them should not be accorded the same weight. The distinction that Goddard made is the distinction widely made by historical linguists between lexical equations that happen to involve grammatical morphemes and true “embedded” morphological correspondence.

We must now momentarily return our attention to Haas’ paper, for Greenberg (1997:669) makes the same claim about Haas that he does about Goddard. He says:

In fact, Haas (1958) notes that the four pronominal possessive prefixes are in themselves sufficient and this evidence, of course, is to be found in Sapir’s original article of 1913.

This would make it appear that Haas held inconsistent views, for although she included some additional forms, unlike Teeter and Goddard, she did not advance the argument beyond that made by Sapir.⁹ She presented (pp. 167-69) equations for the pronominal prefixes but made no observations comparable to those of Teeter and Goddard regarding the grammatical system of which they formed a part. The quandary is resolved by the fact that Haas did not make the statement attributed to her by Greenberg. Nowhere does Haas (1958) suggest that the pronominal prefixes are in themselves sufficient evidence.¹⁰

⁸ Sapir does discuss sound correspondences to some extent at pp. 639-646, but he deals only with isolated points. As he pointed out, at that time even Algonquian comparative phonology was not well understood. Moreover, the Wiyot and Yurok material then at his disposal was inadequate for the task both in quality and in quantity.

⁹ Haas included Wiyot cognates for the first and second person plural prefixes, based on Karl Teeter’s then unpublished work, as well as a Yurok cognate she herself discovered for the second person plural.

¹⁰ Other than Greenberg, the only author to assert that the four prefixes by themselves were sufficient evidence of genetic affiliation is Trombetti (1921).

In sum, the claim that the Algonkian/Ritwan relationship was demonstrated already in 1913 by Sapir is false. Sapir's evidence was weak. Mary Haas' view, like many others, was that the conclusive evidence was provided by her own 1958 paper, which, based on the Ritwan data that had become available subsequent to Sapir's studies, together with the great advances made in the study of Algonquian, contained a larger and more reliable set of lexical equations, on which were based a full set of sound correspondences. Those who do not regard lexical evidence alone as conclusive consider that even Haas' paper did not truly settle the issue, and that conclusive evidence only came in the form of the morphological evidence provided by Teeter (1964ab) and Goddard (1966, 1975). Far from exemplifying the delay of acceptance of valid evidence due to prejudice, the Ritwan controversy exemplifies the development of a strong case over time based on new, more ample and higher quality data and more careful analysis.

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