This paper attempts to analyze and compare the proposals which attempt to link one language or a language family genetically with the Algonquian languages. First, the 'establishment' (in the sense of Hymes 1959) of the Algonquian languages as a linguistic family and the reconstruction of Proto-Algonquian are described (section 1). Then, the historical development of scholarship and debate on the Ritwan languages is taken up (section 2). The evidence for unestablished genetic stocks which group in the Algic languages is examined: Algonkian-Gulf (section 3), Algonquian-Salishan (section 4).

1. Algonquian

Here we deal with the establishment of the Algonquian languages as a genetic grouping (1.1.) and the reconstruction of Proto-Algonquian (1.2.) which proceeded this.

1.1. The Algonquian Languages

The relationship between the Algonquian languages did not need to be demonstrated in the same way as the relationship between Algonquian and other languages needed to be. For the most part their affinity was simply assumed with word lists given for demonstration (cf. Gallatin 1848 for instance). Some of the more 'divergent' (cf. Michelson 1935) Algonquian languages were added later when more words or paradigms became available. In the case of Cheyenne, for example, the language was originally classified as a Siouan language because in a treaty signed in 1825 the names of the chiefs were recorded in Dakota. This, however, was due to the fact that their interpreters were Sioux. Cheyenne became properly classified as Algonquian when 'The numerals of this [Cheyenne] were published...and on these the present writer [Latham] remarked that they were Algonkin' (Latham 1856: 61). Thus, it seems that the classification of languages

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1 Though the relationship was not proven until Michelson's (1935) account of sound shifts in the more divergent western Algonquian dialects. The point here is what was regarded as appropriate evidence for classification by the community of active scholars from a historical perspective.
as Algonquian was essentially uncontroversial as soon as any evidence was provided in the form of word lists or other materials. Often the classification was given with no discussion of supporting linguistic facts (cf. Powell 1891: 48).

Michelson (1912) subdivided the languages into four major 'divisions': Blackfoot, Cheyenne, Arapaho, and Eastern-Central. The latter contains Cree-Montagnais, Menominee, Sauk-Fox-Kickapoo, Shawnee, Ojibwa, Potawatomi, Ottawa, Peoria, Natick and Delaware. His classification is based on 'a study of the present independent and subjunctive modes, together with phonetic and a few other considerations' (Michelson 1912: 238). Michelson compared verb paradigms across the Algonquian languages to see where correspondences arose. He does not explicitly reconstruct the paradigms, however, but one can infer either that Michelson had some type of Algonquian prototype in mind, likely Fox. For instance he states that Cree is characterized by the 'maintenance of clusters sk, sp, st (cp, cp, ct)' (Michelson 1912: 238) thus implying that Cree maintains these from PA. With respect to the languages he considered 'divergent' the Central Algonquian languages were used as the reference point. Sometimes languages are just noted as being similar as in the statement concerning Cheyenne that 'The terminations for WE (excl. and incl.), intransitive, approximate the Ojibwa type' (Michelson 1912: 233). When discussing Arapaho, one of Michelson's more 'divergent' languages, he notes 'some of the more radical phonetic changes' (Micheson 1912: 235). \( tc \rightarrow \theta, p \rightarrow k/tc: w \rightarrow n: m \rightarrow b \). Central Algonquian languages (Fox, Ojibwa and Cree) are used as a reference point for these statements since no protoforms are given. When describing some of the changes in Arapaho, Michelson (1912: 235) states that 'no Algonquian language has deviated further from the normal' (my emphasis).

In an article written in 1913, Sapir tried to link two languages of California; Wiyot and Yurok, with the Algonquian languages. In this article Sapir did provide some reconstructions for

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2 One exception to this is the inclusion of Beothuk for which there was a debate regarding its inclusion in the Algonquian family between Gatschet and Latham (cf. Hewson 1968). As far as I know, however, no such controversy arose with for example the inclusion of Blackfoot within the Algonquian group when a list of probable cognates was provided (Latham 1856).

3 It seems that Michelson assumed that Proto-Algonquian was essentially identical to Fox (the language he had studied the most) similar to the pre-neogrammarian assumption that Proto-Indo-European was similar to Sanskrit (Pentland 201: p.c.).
Proto-Algonquian as in (1), where the reconstructed PA form seems to be given for the sake of showing how close the Yurok form actually is to PA.

(1) Yurok toon-, tsoon- 'four' (perhaps → *toson-, cf. tāmāw- below; this may be dissimilated from *noso- → *nison- or *nison-): Blackfoot nis(o)ó, níso 'four'

Sapir's reconstructions were tentative however. He states that 'knowledge of even Algonkin phonology is yet in its infancy' and 'No very precise data as to phonology will therefore be expected' (Sapir 1913: 639). Sapir's proposal is discussed in more detail in section 4. The 'precise data as to phonology' was developed in subsequent years in Bloomfield's reconstruction of PA, the subject of our next section.

1.2. Reconstructing Proto-Algonquian

Bloomfield (1925) was to provide the first published reconstruction of PA. At this time, however, he referred to this reconstruction as Primitive-Central Algonquian(henceforth PCA). Bloomfield did not commit in 1925 to the idea that he was reconstructing PA since by his own admission the group he was reconstructing was 'doubtful'. He claims that his purpose was to provide an outline which might 'serve as a basis for further discussion' and refute Meillet & Cohen (1954: 9) by attempting 'to dispose of the notion that the usual processes of linguistic change are suspended on the American continent' (Bloomfield 1925: 130, n.1). The Proto-Central-Algonquian label (as opposed to simply 'Proto-Algonquian') was given because he did not know whether Delaware and other eastern languages were daughters of the proto-language he reconstructed from Fox, Menominee, Cree and Ojibwe.

Michelson (1935) demonstrated that his 'divergent' western languages (Blackfoot, Cheyenne and the Arapaho group) were not crucial for the reconstruction of Proto-Algonquian. He did this by giving rules which started with Bloomfield's (1925) reconstructed forms from which Blackfoot, Cheyenne and Arapaho words could be derived. He simultaneously disproved his old classification (Pentland 1979: 13) since this demonstrated that the Central Algonquian languages were not a distinct subgroup within Proto-Algonquian. Michelson's presentation
demonstrated to Bloomfield that his Primitive-Central-Algonquian reconstructions could 'in the main, fit all the languages and can accordingly be viewed as Proto-Algonquian' (1946: 85). The hedge 'in the main' was inserted because Siebert (1941) had demonstrated that Bloomfield's consonant cluster reconstruction *xk has the unpredictable reflexes sk and hk in the Northeastern and New England languages (Micmac, Passamaquoddy-Maecite, Penobscot, Abenaki, Massachusetts, Narragansett, and Mohegan-Pequot, and Delaware). Examples from Siebert demonstrating this are given below in (3) and (4).

(3) PA nemeθkawāwa 'I find him', Foc nemehkawāwa, Cree nimiskawāw, Ojibwe nimikkawā, Penobscot nәәiskławā

(4) PA *anāxkyāni 'mat, rug', Fox anākani, Cree anāskān, Menominee anāhkyan, Ojibwe anakkan, Penobscot ānehkan

What these cognates demonstrate is that there is one consonant cluster which cannot be accounted for solely with the four languages used in Bloomfield (1925) since Fox, Cree, Menomini and Ojibwe have all merged *θk and *xk. Bloomfield's Primitive Central Algonquian reconstruction was given support from the demonstration that the languages he used were conservative enough to reconstruct PA correctly. Notably Bloomfield's (1925) strict application of the comparative method had surprisingly fruitful results given the relatively small number of languages he used and the skepticism regarding the reconstruction of protolanguages in Europe on the American continent (Meillet & Cohen 1956). Bloomfield (1925) algebraically reconstructed the cluster *çk for the correspondence in (5) despite only having one set of cognates where this correspondence showed up (6).

(5) PCA Fox Cree Menomini Ojibwe
   çk ck hk hk ck

(6) PCA *meçkusiwa 'he is red', Fox meckusiwa, Cree mihkusiw, Ojibwe mickusi, Menomini mehkōn (Bloomfield 1925: 152)
Subsequently Bloomfield (1928) published a paper which demonstrated that the reconstruction was in fact confirmed by another dialect of Cree from the Pas where a unique reflex was found, *htk* in the form *mihtkusiw* 'he is red'. Bloomfield's (1946) reconstruction survives essentially intact today with only minor adjustments (Goddard 1979: 173, Pentland 1979: 13). It has been used as a basis for the description of phonemic histories of other Algonquian languages such as Arapaho and Atsina (Goddard 1974, Pentland 1998) and Munsee (Goddard 1982).

2. Algonkian-Ritwan or Agonkian-Wiyot-Yurok

The relationship between the Ritwan languages of California and the Algonquian languages is now generally accepted (Haas 1958a, Goddard 1975). At first, however, it was considered controversial (Michelson 1914, 1915, Sapir 1913, 1915a, 1915b) and hence there now exists an issue in the historiography of American Indian languages as to whether or not Sapir did in fact prove the relationship or whether new evidence later provided by Teeter (1964a) and Robins (1958) was needed to establish it (Ruhlen 1994, Campbel 1997a, 1997b, Greenberg 1997, Poser 2005). This section addresses this issue. First we turn to the classification of the Ritwan languages themselves since, as the title of this section suggests, there is not a consensus concerning whether Ritwan constitutes a genetic subgroup of the larger Algic family itself.

2.1. The Ritwan Languages

Dixon & Kroeber (1903) in an article describing certain broad characteristics of the languages of California first noted structural similarities between Wiyot and Yurok. Later the same authors made a comparison of about two hundred stems in all the languages of California (Dixon & Kroeber 1913). In this article claims of genetic relationships between languages of California are made and Wiyot and Yurok are classified as belonging to the Ritwan family. These languages do not show particularly strong resemblances based on structural similarities; however 'a complete lack of resemblances of both to any other family' in California in their minds 'increases the weight of the similarities between the two' (Dixon & Kroeber 1912: 692).
Sapir (1913) seems to have assumed that Ritwan comprised a subgroup of what he called 'Algonkin'. Evidence for this comes from the fact that he gave reconstructions when he was comparing only the Ritwan forms. Sapir discusses the relationship between these two languages citing Kroeber (1911). After this he gives a brief lexical comparison which includes 41 lexical correspondences. Five reconstructions are given. Sapir's reconstructions seem to imply that PR *n is reconstructed on the basis of Wiyot r and Yurok n. Evidence from Sapir (1913) is given in (7).

(7)

<table>
<thead>
<tr>
<th>Yurok</th>
<th>Wiyot</th>
<th>Proto-Ritwan</th>
</tr>
</thead>
<tbody>
<tr>
<td>-pern</td>
<td>-m-ete-re</td>
<td>*-etene 'nose'</td>
</tr>
<tr>
<td>-ni'-na'-, ne'-</td>
<td>-rit(w)</td>
<td>*nitw- 'two'</td>
</tr>
<tr>
<td>naxkc-</td>
<td>rik(w)</td>
<td>*nikw- 'three'</td>
</tr>
<tr>
<td>ne-,no-</td>
<td>ru-,r-</td>
<td><em>nu-,n-</em> 'my'</td>
</tr>
</tbody>
</table>

An explicit rule is not provided and there is one reconstruction *nil 'I' with reflexes Yurok nek and Wiyot yil which does not follow the r:n pattern. The other cognates which Sapir provides are essentially anecdotal since there are no other correspondences (as represented by more than one cognate set) that I can tell.

Kroeber (1911) originally did not take a position with regard to the hypothesis that these languages were genetically related despite so many similarities in structure (Kroeber 1911: 414-5) since few lexical correspondences could be established. Sapir (1913: 619) utilizes 'more comparable material' and concludes that it seems safe to consider 'Yurok and Wiyot as mutually divergent members of a single stock' (Sapir 1913: 619). This statement is ambiguous because the single stock could be Algonkin or Ritwan. Sapir does not identify any mutual innovations shared by Wiyot and Yurok that could be used to argue for considering them as a subgroup of Algic.

Teeter (1964b) gave evidence that Yurok and Wiyot were genetically close by presenting shared cognates. However, he does not mention any shared innovations. 72 cognates are listed and he does not provie a comprehensive list of cognates for Proto-Agonquian, Yurok and Wiyot.
If it were found that there were generally more cognate forms between Yurok and Wiyot than between one of these languages and Algonquian this might constitute at least very weak evidence for a genetic relationship. However, the list of cognates in Haas (1958a) demonstrates correspondences between Algonquian, and Yurok and Wiyot using 'basic' (Haas 1958a: 161) vocabulary where the Algonquian cognate (in the form of a PCA reconstruction) is always represented whereas Yurok and Wiyot are not always present at the same time, demonstrating that with much of the vocabulary there may perhaps be a closer relationship in terms of cognates between Algonquian and each of the Ritwan languages than the relationship between the Ritwan languages themselves. Examples are given in (8) and (9).

(8) BEAUTIFUL. PCA *melwi- ; Y mamay(ew)- (Haas 1958: 164)

(9) BEAR. PCEA *maθkw-a; W βokw (Haas 1958: 164)

In any case, the only accepted evidence for genetic subgrouping is that of identifying shared innovations between daughter languages (Campbell 2004: 191), that is, 'a departure (innovation) from some trait of the proto-language [which] is shared by a subset of the daughter languages'. The problem is in identifying changes which are so 'quirky' (Hamp 1970) that the chances of them being independent are almost nil. For instance Blevins & Garrett (2007) discuss similar morphophonemic processes which occur in both Wiyot and Yurok. In Yurok $h \rightarrow l$ medially after four preverbs: locative 'o, past-time me/ma and 'eme/'em(a), and conative tem(a). An example is given in (10).

(10) 'o le'm kwelkwk nek ki nepaane'm ko 'o lewoloch-e'm

   'o he'm kwelkwk nek ki nepa-anee'm ko 'o hewoloch-e'm

   LOC say.3.SG CONJ me FUT eat-2.SG/1.SG FUT LOC get.well-2.SG

'It (a plant) said, "You will eat me. You will recover".' (Blevins & Garrett 2007: 76)

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4 That is, if we accept a weaker version of the assumption of glottochronology about the rate of lexical attrition. Instead of accepting the dubious position (cf. Campbell 2004) that there is a constant rate of lexical loss cross-linguistically we are speculating that it is reasonable to assume that generally sister languages will show fewer lexical correspondences the greater the time depth of their protoancestor. Although high lexical correspondences is usually a consequence of geographical closeness rather than genetic.
The same sandhi process occurs in Wiyot as demonstrated in (11).

(11) haʔlabi 'I dance'; pitaba laʔlabi ɬ 'Only he dances.'  

Blevins & Garrett (2007: 90) conclude however that this does not constitute strong evidence for genetic subgrouping and maintain that 'it is a natural development given the phonological profiles of earlier stages of Wiyot and Yurok'. More common changes that result from well-understood phonetic factors (cf. Blevins 2004) do not constitute good evidence for subgrouping either, unless there are many of them.

The literature on the genetic classification of Yurok and Wiyot is not particularly large and the arguments advanced in favor of the subgrouping are not strong. Teeter's (1964b) "preliminary study" has already been discussed. As the title suggests it did not constitute any real evidence for subgrouping Yurok and Wiyot but in principle should be helpful to scholars who might wish to do so. It provides reconstructions of Proto-Ritwan forms which could be helpful for discovering possible shared innovations if there were any. Later Teeter (1975) provides grammatical arguments in favor of the subgrouping of Wiyot and Yurok based on an extemporized (Algic-specific) conception of markedness ('favorite' vs. 'minor'). His argument seems to go as follows. Wiyot and Yurok both show prefix forms in verb paradigms in marked modes, the negative in the case of Wiyot and the subjunctive in the case of Yurok. Since it is likely that the verbal paradigm prefixes were created by analogy with noun inflections for possessives, because the same structure Pn-Noun-Suffix is represented in the Algonquian languages, Wiyot, and Yurok, the origin of this analogy must have been a distinction between what Teeter calls a 'favorite inflection' without the prefix and a 'minor' one with the prefix. According to Teeter (1975: 200) 'If we assume that in the first case the suffixes level in the direction of the most marked, we anticipate exactly the situation in modern Yurok and Wiyot'. For the Algonquian systems the analogical point of contact is that of making 'uniform the expression of person and other categories in the noun and main verb without loss of distinction'.

Teeter's (1975) vision can be represented in (12).
Unfortunately it is impossible to assess Teeter's claim here because he does not explain what he means by 'marked' in this context and why it would have the consequences that it did in the Ritwan languages. The description of syntactic change needs to make reference to reanalysis, extension or borrowing of particular constructions (cf. Harris & Campbell 1995). Vague claims of markedness which are not given clear linguistic correlates as in Teeter's conception or pronominal prefixes in the Ritwan languages are useless because they offer no avenue for this type of syntactic history. The reader is left to speculate on how linguistically contentless 'marked' structures became associated with negation in one language and subjunctive in another.

Berman (1982) proposed that Wiyot and Yurok share two phonological innovations. The first is the loss of distinctive vowel length in the Ritwan languages. Present day Yurok actually has contrastive vowel length but Berman suggested that this was a later innovation. His evidence for this is that Yurok short vowels correspond to Algonquian long vowels (Berman 1982: 416) and that there is some evidence that long vowels have 'a clear secondary origin from earlier short vowels' (Berman 1982: 417). Evidence for the secondary origin comes from the fact that there is a synchronic rule in Yurok which inserts a 'laryngeal increment' (ʔ as h)(cf. Langdon 1979) before a short vowel in initial syllables in polysyllabic words. h is inserted before plain stops or voiceless fricatives as in lohcek 'I take him' and ʔ is inserted before gottalized consonants as in
lo'o? 'I take you (pl.)' (Berman 1981: 258). This laryngeal increment could have been the source of vowel length in Yurok due to compensatory lengthening which is quite common before laryngeals (cf. Kavitskaya 2002). The long o vowels often correspond to Wiyot short vowels as in Yurok ho:leʔm(-) 'to go, to travel, to be around, to fare (plural verb)', Wiyot hol- 'to go, to walk' (Berman 1982: 417).

The other innovation Berman (1982) notes is that Proto-Algonquian-Ritwan *kʔ became Proto-Ritwan *t. The following cognates in (13-15) support this claim. Keep in mind that Berman (1982) also posits that t → ? in Yurok in certain environments after the Proto-Ritwan stage.

(13) GIVE, GIVE FOOD. PAR *ʔkʔ-; PA ahšam- 'to feed'; Wiyot t- to feed, to give food to', Yurok ?- 'to give' (Berman 1982: 417)

(14) SHINE, BE DAYLIGHT. PAR *kikʔ-; PA *kiːšekwi 'day, sky', Wiyot kitakwh- 'sun shines'; Yurok kecoʔ 'light'. Yurok t split to t and ? the conditioning factor for which is unknown. (Berman 1982: 418)

(15) URINATE. PAR kʔek-; PA šekiwa 'he urinates'; Wiyot tikal- 'to urinate'; Yurok ?ahk 'to urinate' (Berman 1982: 418)

Berman concludes that (1982: 418) 'these two sound changes are more likely to have been single innovations in Proto-Ritwan than two sets of parallel independent innovations in Wiyot and Yurok'. Thus, since these changes are not 'quirky' Teeter relies on Occam's razor. Evidently, the consonant kʔ surfaces as kʔ in Yurok and the word for 'hide' (Haas 1958a). The aforementioned shared innovations between the two languages are the only evidence for genetic subgrouping. Most scholars, however, do not accept Ritwan as a subgroup (Goddard 1990).
2.2. Proving the Algic Family

This section discusses the establishment of a genetic relationship between the Californian Algic and Algonquian languages. Sapir (1913) was the first to propose the relationship between Algonquian, Wiyot and Yurok. He was attacked by Michelson (1914) and the idea subsequently remained 'controversial' until more data was provided which proved the relationship 'to the satisfaction of all' (Campbell 1997b: 152). There is a dispute in the historiography of Proto-Algic between those who believe that the evidence Sapir (1913) provided proved the relationship (Ruhlen 1994, Greenberg 1997) and those who think the relationship was proven only when more materials on Yurok (Robins 1958) and Wiyot (Teeter 1964a) became available (Haas 1958a, Goddard 1975, Poser 2005). This section considers the history of argumentation for the Algic family from Sapir (1913) to Goddard (1975) and concludes that the relationship was proven later.

After Sapir compared Yurok and Wiyot (section 2.1.) he went on to show correspondences with the Algonquian languages in order to prove the wider relationship. The Algonquian languages represented in his comparisons are not consistent. This increases the likelihood of chance similarities and thus makes his cognate sets a less conservative conservative argument for genetic relatedness (Campbell 2004: 355). For instance, Wiyot me-lāk 'testicles' is compared with Micmac ālsook 'testicle' and no other forms whereas Wiyot melokaL 'throat' is compared with Natick mu-nnaonk 'throat' and no other forms. More examples of this are given in (16) and (17).

(16) Yurok kuc 'what? where?'; Fox -gu- in wāgunā 'what (inanimate)'? (cf. wānā “‘who?”)
(Sapir 1913: 632)

(17) Wiyot -wa in du-wa 'what?' ci-wa 'what?' wai 'is that so?'; Ojibwe wa 'what?'; Cheyenne -vā, -va in nevā 'who?', henova 'what is it?'
(Sapir 1913: 632)

Sapir divides his comparison into lexical and morphological forms and morphological characteristics. The lexica comparisons include 139 forms. Some derivational morphemes (or
'secondary stems' in Sapir's terms) are considered lexical items because sometimes a primary stem in one Algonquian language corresponds to a secondary stem in another (Sapir 1913: 629). This procedure seems reasonable since different stems would likely go through different paths of grammaticalization in each one of the languages (cf. Gildea 2000).\footnote{It was pointed out by Pentland (p.c.) that Sapir's assumption in this case was wrong because there does not seem to be any derivationa suffix that has become a free-standing stem in one of the Algonquian languages. This is irrelevant because one need only assume that the free stem is the more conservative in each case. No claim need be made about the derivational suffix in Proto-Algonquian.} Thus in Sapir's mind 'Algonkin secondary verb stems... seem to be cognate with Wiyot primary stems' (Sapir 1913: 629) and are hence comparable. An example of this type of correspondence is given in (18).

(18) Wiyot kawi 'to make'; Blackfoot -ka, -xka, -χka 'to make something'; Cree -kke- (Sapir 1913: 630)

Generally the lexical comparisons display fairly strong semantic constraints in the sense of Campbell (2004: 353). That is, remote semantic relationships are not posited. Sapir uses essentially the same methodology when comparing grammatical morphemes. Thus, he uses representatives from Algic sporadically to demostrate relationships. Examples of this are given in (19) and (20).

(19) Yurok matseki 'if'; Cheyenne ma 'if, when' (Sapir 1913: 634)

(20) Yurok nimi-, nimok- negative; Cree nama, nama- 'not' (Sapir 1913: 634)

Sapir lists 42 morphological forms in total. The most convincing correspondence sets for establishing the relationship are Sapir's comparison of the possessive pronominal prefixes (Sapir 1913: 633) because they are reflected in all the languages, and because they have similar grammatical functions (cf. Goddard 1975).
Sapir (1913) provides more evidence in the form of typological similarities between Wiyot, Yurok and the Algonquian languages. These are given in (21-29) from Sapir (1913: 637-8).

(21) 'Possessive pronominal elements are prefixed to noun stems.'

(22) Independent personal pronouns are found which are etymologically closely related to possessive pronominal prefixes.'

(23) 'In Wiyot and Yurok verbal forms pronominal elements are suffixed. In Algonkian pronominal refixes are found only in indicative forms and are identical with possessive elements...The pronominal series for the conjunctive and related modes, possibly also some of the suffixed elements of the indicative, represent, then, the historically oldest Algonkin forms. They are etymologically parallel to the suffixed elements of Wiyot and Yurok.'

(24) 'Preceding the verb stem are often one or more elements of temporal or modal significance.'

(25) 'Several derivative verb suffixes (e.g. causative, reflexive) are found.'

(26) 'Animate and inanimate are carefully distinguished in Algonkian...'

(27) 'Numeral stems are frequently followed by classifying suffixes.'

(28) 'As distinctive of the noun may be noted a general locative suffix and a diminutivizing element which is also suffixed.'

(29) 'Reduplication is not widely used in either Algonkin or Yurok-Wiyot. It is, however, employed to some extent in both chiefly to express iteration.'

None of these typological similarities are convincing for demonstrating genetic relationships. (21) is not typologically rare (Dryer 1993); it is in fact a very strong areal feature
of North America in general (cf. Dryer 2008); (22) is extremely common and is in fact what one would expect to be the canonical process of grammaticalization for possessive pronominal prefixes (cf. Gildea 2000). The *my* in *my dog* is etymologically close to *me* in *he saw me* because they represent vestiges of a case marking system. If English started developing into an agglutinating language all that would be needed is that these morphs become prosodically dependent on their root (cf. Givón 1975, Heine & Reh 1984, Mithun 1984). (23) is irrelevant by itself and incomplete since both Wiyot and Yurok have pronominal prefixes but in Wiyot they occur in negative paradigms (Teeter 1964: 37) and in Yurok they appear in the subordinate (subjunctive) constructions (Robins 1958: 18). (24) is typologically common as well since its true for most European languages including English. (25) is just a characteristic of agglutinating languages generally (cf. Comrie 1986, Baker 1996). (26) is based on evidence which Sapir thought would be discovered later concerning the Ritwan languages.⁶ (27) is typologically well attested across many languages (Aikhenvald 2000: 98). (28) is unconvincing because the languages are already largely suffixal and diminutives certainly are not rare cross-linguistically and especially not in North America (cf. Nichols 1971). Iteration is one of the primary semantic correlates of reduplication, so (29) is an uninteresting typological similarity (Kouwenberg 2001, Bybee *et al.* 1994).

Sapir (1913) proposed 17 sound correspondences represented in (30).

(30)

|   | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 |
|---|---|---|---|---|---|---|---|---|---|----|----|----|----|----|----|----|
| Wiyot- | # | w | p | l | l | t | l | n | r | r | k | tc/ts | tc/ts | tk/k | tk | tkw/ | Vsk, Vskw, |
| Yurok | h | k | | | | | | | | | | | | | | | |
| Algonquian | # | m | m | n-r | t | l | n | n | t | l | s | c/s | tc/ts | sk,c | cw | skw/ | Vk, Vkw, |
| | ø | | | | | | | | | | | | | | | | |
| Cognates | 9 | 7 | 2 | 7 | 6 | 5 | 6 | 5 | 5 | 1 | 12 | 15 | 2 | 5 | 1 | 1 | 7 |

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⁶ Sapir (1913: 638) states that Yurok has a few adjectives with special animate forms, but that 'There must be other evidence of its operation [i.e. gender] that have not yet been disclosed'. He guessed wrong—there is no evidence of gender in Yurok or Wiyot.
Again Sapir does not reconstruct any forms but simply provides examples of sound correspondences. That he applies essentially the same standards with respect to establishing 'sound-relations' as with the comparison of lexical and grammatical material is demonstrated in (31).

(31) Wiyot-Yurok #h : Algonquian #ø
W. hólakw 'deer' : Oj. atíkw- 'caribou'
W. hēikw 'louse' : Cree ikkwa
W. hālu 'seven' : Mic. eluwiginak'
W. hu- 'his' : Cree o
Y. horāu 'arrow' : Cree atus; Ar. hoti
Y. hī-, he- demonstrative element : Fox ī-

It is unclear why Sapir sets up correspondences with only one cognate. In principle every single comparison between languages could be given sound correspondences on this basis and it would not prove anything. One might expect one-cognate sound correspondences to be noted if the cognates differ in only that correspondence. Otherwise the linguist is simply applying Greenbergian mass comparisons where there is a high likelihood of chance similarities (Campbell 1997, 2004) especially when CVC syllables are being compared with a disregard for the rest of the word (cf. Ringe 1999 for a mathematical proof). This is the case for Sapir's cognates for the sound correspondences in column 10 of (30), given in (32).

(32) Yurok r : Algonkian l
Yurok rokw 'wind' : PA *lotin

There is no sound correspondence in Sapir's list (30) where Yurok kw corresponds to PA t: with Sapir's correspondences Vkw corresponds to Wiyot-Yurok Vskw. Nor in retrospect was Sapir (1913) on the right track in this regard because in Haas' (1958a) account Algonquian l corresponds to Yurok l or n as is demonstrated in (33) and (34).
With these considerations in mind it is hard to understand how Greenberg (1997: 670) could come to the conclusion that after he had 'inspected the evidence' it 'showed clearly that Sapir (1913) was right'.\(^7\) Michelson (1914) claimed that many of the proposed typological similarities are more likely due to chance. He criticized Sapir's methodology on the grounds that not all the Algonquian languages are used in each case, thus increasing the power of his tests.\(^8\) Michelson points out a number of typological differences between the Ritwan languages and the Algonquian languages. Since Michelson has no standard by which to judge anecdotal typological evidence himself he should be open to the same criticism which he gave Sapir. In other words, if typological similarities are not good evidence in themselves of genetic relatedness why do typological dissimilarities provide evidence to the contrary? Indeed evidence from contact areas demonstrates that languages can change wholesale their typological characteristics whilst maintaining a core of lexical items which can be identified for the purposes of genetic classification (cf. Aikhenvald 2006a).\(^9\)

\(^7\) That is, of course, unless one is familiar with Greenberg's other work (cf. Campbell 1997a, 1997b 2004).

\(^8\) Notice that there is nothing wrong in principle with leaving certain languages out of cognate sets. Later evidence might prove that these gaps are due to borrowing (Campbell 2004). This methodological point notwithstanding, the less languages that are given in the cognate set the higher the chances of chance similarity.

\(^9\) For instance the Makú languages spoken in the middle Rio Negro in Brazil an be identified as a genetic group because about 300 cognates have been established across the family (Martins & Martins 2006: 253). After the Makú speaking groups of the Íçana-Vaupés linguistic area were overcome nd subordinated to Tucano speaking groups around 1500, the Makú languages spoken in this area (Daw, Yupda, Yuhup) began to change wholesale their typological structure (Aikhenvald 2006b). The other extant Makú language not found in this diffusion area is radically different from the others. Hence, Nadéb has a very complicated verbal structure with up to nine prefix positions (Weir 1994: 293-4) and is one of 'the most ergative languages in the Amazon region' (Martins & Martins 2006: 263), whereas the other Makú languages (Dâw, Yupda and Yuhup) have much less complicated verbal morphoogy and are nominative-accusative systems (Martins & Martins 2006).
The other evidence Michelson uses to attack Sapir is based on not obeying semantic constraints for his grammatical comparisons or even worse simply comparing elements based on an improper parsing of the morphological elements in the forms compared. In the former case Michelson seems to be wrong: there are no very exuberant semantic extensions. One possible example of the latter case is where Sapir compares Ojibwe -m 2.PL from the VAI paradigm which contains gi- -m as a bivalent morpheme encoding person and number as in gidinaabim 'you guys look' with Yurok -m 'thou'. Michelson thus implies that he does not agree one can analyse pieces of inflectional elements from one paradigm in a language from pieces in another.\(^\text{10}\)

Sapir (1915: 193) in a reply actually accepted the latter criticism concerning comparing pieces of inflectional morphology. Although Michelson's negative arguments regarding typological features were essentially moot it ought to be pointed out that Sapir used similar arguments to argue for the genetic relationship. It seems therefore that in the absence of clear criteria for judging the likelihood of typological traits as they appear in different languages the arguments are not very strong either way (cf. Nichols 1992). Later Sapir (1923) added 30 equations of kinship terms to the cognates. He does not discuss how it supports his sound correspondences although he states that some of the cognates 'corroborate a number of phonetic laws suggested in my previous paper' (Sapir 1923: 39). A number of cognates imply new correspondences not represented in (30). For instance the set in (35) seems to imply a Wiyot d: Algonkin d/t correspondence.

(35) Wiyot yi-dux 'my sibling' Penobscot n-dokani:mi:s 'my younger brother, younger sister'; Abenaki ni:-dokan 'my older brother'; Algonkin *-tok-eskw- 'sibling-female, sister'
(Sapir 1923: 47)

Since Sapir's comparison of kinship terminology is concerned 'with a view to their conceptual equivalence' (Sapir 1923: 61) their validity for establishing genetic relatedness is not

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\(^{10}\) Michelson was right in this case. Ojibwe -m- is not a 2nd person marker (it also occurs in 1pl -m-in).
discussed here because it is contingent on the extent to which kinship systems can provide evidence for linguistic genetic relatedness. \(^{11}\) Apparently kinship terminology is considered important in the reconstruction of cultural areas because as Jones (2003: 503) states, in a recent review of the literature on time depth and kinship terminology, 'anthropologists have long noted that kinship is a particularly conservative cultural domain'. Nevertheless, genetic linguistic relationships need to first be proven before any cultural history can be inferred from them.

Haas (1958a) is often cited in statements regarding the proof of the genetic relatedness between Algonquian, Wiyot and Yurok languages (cf. Campbell 1997a: 152 for example). In addition to the debate in the historiography of Algic family concerning whether or not it was Sapir (1913) who first proved the relationship or whether subsequent evidence did so, there is also a debate concerning whether Haas advocated the latter position (Ruhlen 1994, Campbell 1997b, Greenberg 1997, Poser 2005). Haas (1958a) relies completely on lexical items and sound correspondences eschewing functional morphological arguments (as in Goddard 1975) and typological arguments (as in Sapir 1913). Before discussing the lexical items which prove sound correspondences she lists 20 cognates from 'basic' vocabulary which have a minimum of three consecutive phonemes in correspondence in each instance. She states that 'The chances that resemblances of this nature could be accidental are so infinitesimal that the possibility needs to be considered' (Haas 1958a). Of these 11 were actually listed in Sapir (1913). Given the Haas' statement quoted above one could infer that it was the extra vocabulary (arm(finger), my eye, his tail, grease, tree, to drink, to suckle, to steal, long) which provided the definitive evidence (Greenberg 1997). The sound correspondences Haas (1958) gives, however, match the forms in the list without as many gaps as in Sapir's correspondences in (30). Take for instance the cognates for 'bone' from Sapir (36) and from Haas (37) using a PCA reconstruction.

(36) Wiyot *wat-kerāt 'bone', *ra-watkerāt 'my bones'; Yurok *werLker 'bones'; Cree *oskan 'bone'; Ojibwe -kān (< *-kān); Fox ut-o'kAn-emAn 'his bones'; Natick *m-uskan; Blackfoot oχkin (< *askan) (Sapir 1913: 625)

\(^{11}\) As Van Eijk (2006: 410-11) points out 'similar kinship systems may be shared by unrelated languages'. He cites the bifurcate merging system that's found in Omaha and Crow (both Siouan) and Iroquoian (Schusky 1965). Thus outside of the lexical correspondences for kinship terminology it is perhaps unclear how similar kinship systems provide evidence for genetic relatedness.
For Sapir the cognates in (36) supported one sound correspondence; that of Natick \textit{m} to Wiyot and Yurok \textit{w}. Haas (1958a) was able to state that the cognate set in (37) supported four sound correspondences; Algonquian \textit{w} to Wiyot and Yurok \textit{w}; Algonquian \textit{a} to Wiyot and Yurok \textit{a}; Algonquian \textit{θ} to Wiyot \textit{t} and Yurok \textit{l}; Algonquian \textit{k} to Wiyot and Yurok \textit{k} and Algonquian \textit{a} with Wiyot and Yurok \textit{ə}. Thus it is not just additional forms which made which made Haas' (1958a) arguments stronger than Sapir's (1913). It is also that more coherent and comprehensive statements regarding sound correspondences can be made. This is why Haas (1958a) later in the article in apparent contradiction to her previous statement states:

As a matter of plain fact, the, the historical sequence of events just described was necessary before the material assembled in the present paper could be brought together and presented with reasonable hope that the long-standing Algonkian-Ritwan controversy could be brought to an end. (Haas 1958: 162)

Goddard (1975) has a different view of what proved the relationship. For him 'the kinds of similarities which are most valuable for showing genetic relationship are those which involve details of the morphological structures of the languages' (Goddard 1975: 4). For instance Algonquian, Wiyot and Yurok nominal and verbal stems are all built out of roots, medials and final. Hence, we can compare forms morpheme by morpheme as in (38) and (39).

(38) \textit{kaskika:te:sin-} 'break one's leg by falling'

\begin{verbatim}
Cree    kask    -ik:te:    -sin-
      break    leg    fall
\end{verbatim}

\textit{takwaʔon\textordmasculine{o}r} 'break arm by falling'

\begin{verbatim}
Wiyot    takwaʔ -aʔon -or-
      break    arm    by.falling
\end{verbatim}

---

12 Many vowels lengthen word finally in Yurok (Robins 1958: 34). Yurok \textit{newoʔw} or \textit{newoʔw} 'you guys see'.

19
Additionally there are four prefixes in Algonquian, Yurok and Wiyot which show similarities in form and function (40).

(40) The pronominal prefixes in Proto-Algonquian (PA), Wiyot and Yurok

<table>
<thead>
<tr>
<th></th>
<th>PA</th>
<th>Wiyot</th>
<th>Yurok</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Set I</td>
<td>Set I</td>
<td></td>
</tr>
<tr>
<td>First person</td>
<td>*ne-</td>
<td>du(?)-</td>
<td>d-</td>
</tr>
<tr>
<td>Second person</td>
<td>*ke-</td>
<td>kho(?)-</td>
<td>kh-</td>
</tr>
<tr>
<td>Third person</td>
<td>*we-</td>
<td>kho(?)-</td>
<td>w-</td>
</tr>
<tr>
<td>Indefinite</td>
<td>*me-</td>
<td>u(?)-</td>
<td>b-</td>
</tr>
</tbody>
</table>

Nouns are divided into two classes according to whether or not they take these prefixes. In Algonquian and Wiyot vowel initial nondependent nouns insert a -t- after prefixes. For instance, Fox *ne-ahkokwa → netahkokwa 'my kettle' and Wiyot *du-i?l → duti?l 'my intestines' (Goddard 1975: 5). Algonquian and Yurok include a subset of dependent nouns (which include all those that are kinship terms) that do not take the indefinite possessor. In Algonquian obviation occurs with possession with two thirds persons. The same process occurs in Wiyot. All three languages have some verbal paradigms which use prefixes and some which do not. Wiyot has prefixes only in the negative like Arapaho, which has it in the negative and interrogative. In Algonquian and Wiyot any form with a second person participant takes a second person prefix and any form which has a first person participant but no second person takes a first person prefix. Also, adding prefixes to preverbal particles separate from the verb stem occurs in Algonquian, Wiyot and Yurok. Goddard (1975: 13) also emphasizes the fact that etymologies between
Algonquian, Wiyot and Yurok lead 'to productive hypotheses' concerning the history of the languages (cf. Goddard 1975: 11-13 for details). Goddard (1975: 13) concludes that 'there is a superfluity to lexical comparisons in the cases where a relationship can be demonstrated by reference to detailed grammatical and structural parallels'. This position, I think, is debatable, Goddard's strong morphological arguments notwithstanding. One reason is that Kutenai shares striking similarities in its obviation system with the Algonquian languages (Dryer 1992, 2006) however this does not constitute convincing evidence (Campbell 1997a: 118) since there are hardly any or no convincing cognates between Algonquian and Kutenai (Morgan 1980, 1991).

3. Algonkian-Gulf

Both the Gulf family and Algonkian–Gulf are controversial linguistic families (Campbell 1997: 306; 308). The latter is no longer being investigated (as far as I know) despite receiving considerable attention when it was originally proposed (Willey 1958, Gursky 1963, 1968). The genetic unity of the Gulf languages has recently been reopened by Munro (1994), but again is still controversial (Campbell 1997: 308). John R. Swanton (1915, 1919) proposed that Tunica, Chitimacha and Atakapa made a 'Tunican' stock and that Natchez and the Muskogean languages were closely related (Swanton 1924, cf. Haas 1956 as well). Following Swanton's former comparison Swadesh (1946) provided more comprehensive evidence that Chitimacha and Atakapa were related based off sound correspondences in 240 sets of lexical correspondences. Swadesh provided reconstructions but admits they are unlikely because they do not include any other Gulf languages (cf. Haas 1951). Besides this there are various problems with the correspondences due to the orthography used in Atakapa data from Gatschet and Swanton's dictionary of the language. For instance Atakapa n and 'sometimes -η' (Swadesh 1946: 117)

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13 Greenberg (1987) and Munro (1994) both claim that the relationship is not controversial. The latter maintains that only Campbell & Mithun (1979) have raised doubts about its existence. However, the main proponent of the relationship (Mary R. Haas) seems to have abandoned it later (Pentland: personal communication, and cf. Campbell 1997a)

14 Swadesh states (1946: 113) that Tunica is not compared is because he did not have access to enough material. His material from Chitimacha is based off his own field notes and for Atakapa it is based off a dictionary of the language by Albert Gatschet and John R. Swanton.

15 Presumably this was because Swadesh did not yet have access to Haas' (1953) dictionary of Tunica.
corresponds to Chitimacha n and final h. Swadesh (1946: 113) states, however, that 'There are a number of cases of final m having become n or ŋ in Atakapa, but the conditions under which this took place are not entirely clear' due to 'irregular variations in the writing'. The same problem is encountered when reconstructing vowels. For instance, Swadesh (1946: 122) states that 'In speculating about causes, we must remember that e and i show variations in Gatschet's recording, so that any of these examples may be merely a matter of spelling'.

Haas (1951, 1952) put forth the Gulf classification that connected Proto-Muskogean, Natchez, Tunica, Atakapa and Chitimacha with no particular subgrouping as in (41).

(41)

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<table>
<thead>
<tr>
<th>GULF</th>
</tr>
</thead>
<tbody>
<tr>
<td>Atakapa</td>
</tr>
<tr>
<td>Chitimacha</td>
</tr>
<tr>
<td>Muskogean</td>
</tr>
<tr>
<td>Natchez</td>
</tr>
<tr>
<td>Tunica</td>
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<tr>
<td>Creek</td>
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<td>Seminole</td>
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<td>Hitchiti</td>
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<td>Mikasuki</td>
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<td>Alabama</td>
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<td>Koasati</td>
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<tr>
<td>Choctaw</td>
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<tr>
<td>Chickasaw</td>
</tr>
</tbody>
</table>
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The genetic unity of the Muskogean languages is uncontroversial (Haas 1941, 1947, 1949, 1979, Campbell 1997b: 147-9), despite the fact that there are competing theories for subgrouping (Booker 1988, Munro 1993). For the Gulf relationship, just on grounds of providing etymological histories (cf. Campbell & Goddard 1991: 17, Goddard 1975: 11) of the languages in question Haas' (1951, 1952) argumentation is impressive.\(^{16}\) Haas (1951) begins by comparing

\(^{16}\) Consider Campbell & Goddard's (1991: 17) claim:

'When the constraints of accepted historical method have seemed too restrictive, new, less rigorous methodologies have been proposed in order to produce results more easily. A notable example is Carl Voegelin's unfortunate distinction between comparative-method linguistics and phylum linguistics (Voegelin 1965; cf. Voegelin 1942), which at bottom reflected the misconception, widespread in North America, that the comparative method is a method for proving languages to be related by using sound laws (see also, for example, McQuown 1955: 502; Longacre 1967: 199-120). On the contrary, the comparative method is a method used to construct hypotheses about the undocumented history of related languages by systematically comparing their features.'

and Goddard (1975: 11) after comparing the Algonquian, Wiyot and Yurok words for 'bone' and 'liver' states:
Muskogean words with Proto-Muskogean words and providing an etymological of the word for 'water'. Then, the proto-Gulf word is reconstructed using the cognate set in (42).

(42)

<table>
<thead>
<tr>
<th>Proto-Muskogean</th>
<th>Natchez</th>
<th>Tunica</th>
<th>Chitimacha</th>
<th>Atakapa</th>
<th>Proto-Gulf</th>
</tr>
</thead>
<tbody>
<tr>
<td>*akw'i &gt; aku ~ uki</td>
<td>kuN17</td>
<td>wiši</td>
<td>kuʔ</td>
<td>ak</td>
<td>*akw'in</td>
</tr>
</tbody>
</table>

(from Haas 1951: 74)

Haas (1951) then proceeds to provide accounts of the development of the words in non-Muskogean Gulf languages. For instance the shift from Proto-Gulf *akw'in to Natchez kuN by stating that *k becomes delabialized. Evidence for this within Natchez comes from the fact that there is a consonant kN which never occurs before a rounded vowel. Natchez also has two alveolar nasal phonemes, voiced n and voiceless N. Haas uses other cognate sets to provide arguments for the change from Proto-Gulf n to Natchez N. These are presented in (43) and (44).

(43) PG *n > Nt. N when, through vowel loss, N is followed by a consonant, e.g. PG *enuti ~ tooth (PM *-nuti; Tun. -ni, -niri) > Nat. ?eNt

(44) PG *n also > Nat. N when, through vowel loss, N occurs in final position, e.g. PG *enani fish (PM *NaNi/u; Tun. ni'ni: Chit ni- in ni-pi fishscale; At. nti, andi fish, catfish) > Nat. ?eN fish. In view of these rules Nat. kuN water may constitute evidence for a PG reconstruction *akw'ini rather than *akw'in; cf. also the discussion of Chitimacha, 3.4. below.

(from Haas 1951: 75)

'[it] is more than a sound correspondence of the usual sort, discussed above; it throws light on the background of the patterns of consonant clustering in the three languages, a matter which has long been a challenge to Algonquianists.'

The possibility of constructing etymological histories based on a potentially nonexistent genetic grouping (as in Haas 1951, 1952) is not addressed, however. In this case both Campbell & Goddard (1991) and Goddard (1975) may be perhaps overstating their case, because one needs more than one correspondence for a genetic grouping to be accepted.

17 N is a voiceless nasal stop.
Thus, Haas (1951) reconstructs the etymological history of the word for 'water' based off other correspondences which are not listed; 12 in total. The environments under which changes took place are elucidated by these cognate sets. Haas' (1952) later argument concerning the Proto-Gulf word for 'water' is similarly structured, with 3 additional cognates which provide motivation for the sound changes that need to be proposed to account for the form in question.\textsuperscript{18}

In Greenberg (1987)\textsuperscript{19}, 'Gulf' is assumed to be a subgroup of Yuki-Gulf and then at an even deeper level Penutian. However, a high proportion of the cognate sets contain error (Kimball 1992). After reviewing Greenberg's etymologies, for instance, Kimball (1992: 488) states:

'Relative to the Penutian Etymologies, there are 191 Penutian sets that include Muskogean, "Gulf", and Yukian materia, and 39 Yuki-Gulf sets. Of the 191 Penutian sets, 115 required correction based on errors in the Muskogean, "Gulf", and Yukian material. Of these 115 sets, 2 were strengthened, 55 were weakened, 13 were eliminated, and 15 were unaffected. Of the 39 Yuki-Gulf sets, 35 needed correction. Of this 35, 2 were strengthened, 4 were weakened, 21 were eliminated, and 3 were unaffected.'

\textsuperscript{18} Specifically Natchez has three extra cognate sets with Proto-Muskogean (neck, squirrel, below) and Chitimacha and Atakapa have one other set (under/below) (cf.Haas 1952: 239).

\textsuperscript{19} Greenberg's (1987) Amerind proposal and its subclassifications are not taken up here as they have been been criticized extensively by specialists (Campbell 1988, Chafe 1987, Goddard 1987, Golla 1988, Kaufman 1990). The reader ought to keep in mind, however, that this may just reflect my own personal intellectual immaturity. As Ruhlen (1997: 124) puts it with Miltonic fervour:

'Indeed 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.'
Munro (1994) attempts to provide better evidence for Greenberg's (1987) Yuki-Gulf
classification.20 Since the Yukian and Wappo are not in the South East near the Gulf coast like
all of the other Gulf languages the article starts off discussing ethnographic research that might
support the claim that Yuki and Wappo speaking groups migrated to California, their current
location. The article provides 574 cognate sets all of which according to Campbell (1997b: 308)
display 'a lack of phonetic similarity'. Munro (1994) attempts to bolster her claims by appealing
to metathesis, sound symbolism and compounding. There are reasons internal to the languages
compared for using metathesis. Metathesis is widespread in the Muskogean languages. Munro
(1994: 140-1) refers to the following examples.

(45) Creek-Seminole *taph*- 'wide', Alabama *patha*, Koasati *patha*, Chickasaw *patha*, Choctaw
*patha*.

(46) Alabama *hana:biya* 'lizard', Koasati *hayabina*

Metathesis is a very common phenomenon in the Muskogean languages and also in Tunica
(Haas 1953: 185). Munro (1994), however, does not seem to give a principled account of the
environment or the conditions in which metathesis takes place in each case. It is simply evoked
so that cognate sets do not have to be compared segment by segment as advocated by Haas
(1969) (cf. Haas 1963 for an application). Needless to say that evoking metathesis wherever it
seems suitable (which we must assume is the inevitable alternative to providing coherent
environments as in Blevins & Garrett (2004)) increases the likelihood of chance similarity
exponentially (cf. Campbell 1997b).

Sound symbolism is also used to compare some forms where the sound correspondences
do not match. Internal evidence from the Muskogean languages is from Chickasaw.

20 However she states that 'the collection of Gulf cognate sets with Yukian parallels that is appended to this paper
owes little but its inspiration to Greenberg (Kimball 1994: 135).
(47) Chickasaw wahlala 'to be spreading (of a hoopskirt, for example)' and washhala 'to be spreading (of the limbs of a tree, for example)'; siipo'wa 'stretch, do exercises' and shiipo'wa 'be stretchy, elastic' (Munro 1994: 141)

(47) demonstrates sound symbolism between \textit{hh/shsh/p}. Another sound symbolic alternation for the Muskogean languages is an \textit{l/n} alternation as in (48).

(48) Creek \textit{pini:h} - 'wavy (of hair)', \textit{pili:h} - 'curly (of hair)' (Munro 1994: 141)

Thus, the possibility of sound symbolism is used as a way to set up inconsistent correspondences between cognate sets. Again, this practice could in principle be legitimate if the environments under which the correspondences took place were well understood in each case. Evoking sound symbolism increases the likelihood of chance correspondences nevertheless. In addition to these techniques which decrease the conservatism of Munro's (1994) methodology, she does not compare the Gulf languages to Proto-Muskogean but rather to individual Muskogean languages. There is nothing wrong with this practice in principle in terms of developing sound correspondences since these can be set up independently of subgrouping. However, when the power of the comparisons increases in two ways when Muskogean languages are represented sporadically in the data set. First, the likelihood of finding potential cognates (which in this stage of proof are simply similar looking words) increases when reflexes can be selected from a wider stock. If I am comparing languages A, B, C, and D but I can choose any combination of just two of these languages in each case the chances of me finding chance similarities increases. There are nine Muskogean languages represented throughout Munro's cognate set (Creek-Seminole, Hitchiti, Mikasuki, Alabma, Koasati, Apalachee, Choctaw, Chickasaw) and in many cases only one of these languages is selected, in addition to only one of the other Gulf languages of which there are five (Atakapa, Natchez, Tunica, Wappo, Yuki). Examples of this are given in (49-52), taken from (Campbell 1997b: 307).

(49) ALLIGATOR: Natchez \textit{ʔaːtitiː}; Creek \textit{halpata}

(50) BARK: Koasati \textit{kawka} 'to bark(of a fox)'; Natchez 'fox'
(51) BITTER: Atakapa he, Choctaw homi

(52) BLOWGUN DART: Koasati lohpo 'blowgun, dart, thistle', Natchez loho 'blowgun, dart, thistle'

The other sense in which comparing Muskogean and non-Muskogean Gulf languages sporadically increases the power of the comparison is with the sound correspondences. More sound correspondences can be set up by chance if the pool of languages from which the author selects is large but the systematicity in cognate occurrence is low. Thus, Munro (1994) uses the same methodology as Sapir (1913) in this respect. However, Munro's comparisons are probably much less conservative because there are fewer sound correspondences (51) and she evokes metathesis and sound symbolism in her comparisons. Another difference is that in Munro's forms semantic extensions are used more liberally than Sapir (1913). Observe the comparison in (50) for example. Furthermore with respect to Sapir's (1913) comparisons between Algonquian and the Ritwan languages, one could be fairly certain that many of the forms were not borrowings for geographical reasons. The Gulf languages (minus Wappo and Yuki) are all in close geographical proximity to each other. For instance, Campbell (1997b: 307) points out that (52) is most likely a borrowing just based on its semantics.

Munro (1994) also provides some grammatical arguments in favor of the Gulf languages. Muskogean languages, Atakapa, Chitimacha and Yuki all use 'an active system of morphosyntactic oppositions' (i.e. ergative). Ergative systems, although they are rarer than accusative systems, are quite common cross-linguistically, however, (Dixon 1994) and is also commonly diffused across languages (cf. n8 this paper).

Munro (1994: 149) also identifies some semantic similarities given in (53-56).

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21 Her definition of 'an active system of morphosyntactic oppositions' is one 'in which one grammatical form references the subject of an active or transitive verb, while a second references the subject of a non-ctiv verb of the object of a transitive verb' (Munro 1994: 148).
(53) BREAST—'though it often refers to the breast or chest generally, has a specific reference to fowl breasts in the majority of languages'

(54) CUT—'seems to refer specifically to gutting or disembolwing in a majority of languages'

(55) GO OUT (of a fire)—'has a variety of metaphorical extensions: 'close the eyes' and 'die out' appear in two languages each, and further extensions such as 'set (of the sun)'; 'laugh', and 'blink' occur as well'

(56) BEAR—'has been extended to refer to wolves or coyotes. In each of those cases the semantic specifications and extensions occur in Yukian as well as Gulf proper'

(Munro 1994: 149)

Munro’s (1994) semantic comparisons are difficult to assess because it is unclear how well structured radial categories (in the sense of Lakoff 1987) maintain themselves over great time depths and to what extent these systems correspond to cultural facts rather than linguistic ones (cf. Craig 1986 on the relationship between noun classification and culture for example). If these languages were part of the same cultural area at one point (cf. Jones 2003), for instance, these semantic correspondences would be better attributed to diffusion.

Despite these problems the sheer number of cognate sets in Munro (1994) is still impressive (574). Campbell (1997b), nonetheless, considers the cognate sets worthless. He states that 'Munro's data do nothing to bolster the already widely questioned Gulf proposal' (Campbell 1997b: 308). The criticism is perhaps overstated. He claims that 'Other scholars have identified borrowings in a number of these sets' (my emphasis) without citation and without stating who these 'other scholars' are, and provides no evidence which would strengthen his claim that 95 sets are borrowings. The examples he gives are BOX, BUFFALO, CATFISH, CLAM, CYPRESS, DOCTOR, EVERGREEN, FOX, MULBERRY, OYSTER, PEPPER, PINE, ROBIN, TEN, TOWN, WHIPPOORWILL. I fail to see why any of these are obviously borrowings solely based off their semantics, unless Campbell has some special knowledge concerning cultural transmission in the Southeast which he does not cite. (Did the Tunica borrow their term for
'doctor' from Muskogean groups?). Campbell (1997b) also apparently identified 112 sets that involve onomatopeia. The examples he cites are BARK, BEAT, BLACKBIRD, BLOW, BREAST, BREATHE, CHICKEN, CHOKE, COUGH, CRICKET, CROW, DRIP, GOBBLE, LICK, MAKE NOISE (WHOOP), POP (EXPLODE), RATTLE, RATTLE, SNEEZE, SNORE, SPLASH, SUCK, SWELL, WHINE. For most of these (except possibly BREATHE) I am at a loss to say how these are obviously onomatopoetic. Spoken languages are equipped with a limited degree of analogue building potential for creating iconic expressions. Even given an iconic expression the concept is still greatly underdetermined by its form even in modalities with greater analogue building potential than spoken languages, i.e. signed languages (cf. Taub 2001, Tallman 2009a, 2009b). Thus, the fact that all these forms could be sound symbolic is irrelevant—they could still constitute evidence either way. Unless Campbell (1997b) provides statistically significant cross-linguistic evidence which demonstrate that the aforementioned words are predictably onomatopoetic in a wide range of languages it is unclear how these forms obviously fall under 'correspondence due to sound symbolism'.

Campbell (1997b) is the only one to my knowledge who has criticized or even commented on the data provided for the Gulf linguistic stock presented by Munro (1994). Despite the fact that much of Munro's (1994) evidence is not extremely convincing, Campbell's (1997b) dismissal is perhaps a bit overdone. Depending on how we weigh linguistic evidence (amount of cognate sets vs. quality of cognate sets for example), it seems reasonable to take Munro's (1994) evidence for the Gulf relationship to be at least as convincing/suggestive as Sapir's (1913) original Algonquian-Wiyot-Yurok connection.

Mary R. Haas also proposed that the Gulf languages were related to the Algonquian language family. Unlike Sapir (1913), she did not discuss any typological or morphological similarities between the languages. The presentation of the genetic relationship between the Algonquian languages and the Gulf languages is similar to Haas'(1958a) presentation of Algonkian-Yurok-Wiyot relationship. First she lists some forms from more 'basic' vocabulary

22 In fact if we use English as a control language Campbell's (1997b), just the gloss proves him wrong. For instance;

BLOW: Atakapa puns, Chitimacha pu:hte- 'blow through a tube', Creek po:fk- , Natchez puuW- hooʔis, Tunica pusa 'swell, inflate'- Problems: These are onomatopoetic forms, and not all of them are semantically equivalent. (Campbell 1997b: 306)
before showing the more exhaustive list with sound correspondences. She concludes after her initial list that 'This synoptic preview should be sufficient to demonstrate the genetic affinity of these languages for all practical purposes' (Haas 1958b: 161). Like her demonstration of the Algonkian-Ritwan relationship she only gives lexical evidence. There are four important differences between the list Haas proposes for demonstrating the Algonkian-Gulf relationship and that of the Agonkian-Ritwan relationship. First, not as much of the vocabulary is 'basic'; *sharp, swallow, through, turn* for example. Second, some languages have forms missing from the cognate sets. In fact, Proto-Muskogean has only 11 out of the 21 forms given. Thirdly, the morphemes do not to have a minimum of three consecutive correspondences to make the list as in (57).

(57) DIE. PCA *nepe* 'die'; Proto-Muskogean *ili*; Tunica lúpi; Chitimacha nu:p; Atapaka pih (Haas 1958b: 237)

Lastly, the forms presented to demonstrate the Algonkian-Ritwan relationship contained morphological correspondences not found in Haas (1958b). Specifically many of the terms were body parts which had lexical cognates in their possessive prefix and the stem itself. An example of this is given in (58) where *we+stem* is found in each case.

(58) HIS MOUTH. PCA *weto:n*; Wiyot walul-; Yurok welul (Haas 1958a: 161)

In the second part of the paper Haas (1958b) sets up a number of sound correspondences between PCA and Proto-Muskogean and Tunica, PCA and Atapaka, and PCA and Natchez, PCA and Chitimacha. The PCA: Proto-Muskogean correspondences there is an average of three cognates for each sound correspondence, but many correspondences have only one set that exemplifies them. Examples of these are given in (58-67) from Haas (1958b: 242-54).

(58) *p: *-xʷ
DIP. PCA *kap* 'out of liquid'; PM *kaxʷ-

(59) *t: *c
DEER. PCA *atehkw-a 'caribou'; PM *ici/u, icci/u.

(60) *t : *č
ACQUIRE. PCA *tep-; PM ču:pa 'buy'

(61) *m: *n
LAND (GROUND). PCEA *-axkamekw-; PM ihakani(ka)

(62) *l: *l
DIG. PCEA *wa:l-; PM *wil- 'root up'

(63) *l: *n
NECK. PCA *-leskw-a 'neck gland, tonsil'; PM *nukk"i 'neck, throat, larynx'

(64) *θ: *č
TAIL. PCA *-aθany-; PM *-haci

(65) *θ-k: *l
SPLIT. PCEA *pa:θ-k- 'break open'; PM *pəl

(66) *s: *s
CRAWFISH. PCA *aQša:ke:w-a. PM *sakči/u

(67) *w: *p
EAT. *amw-; PM *impa (tr.), *humpa (intr.)

In each case it seems that one-cognate sound correspondences are posited because the rest of the phonological shape of the word demonstrates a better established correspondence. For instance in (65) the sound correspondences PCA *p: PM *p has eight cognates in Haas' list. The PCA Tunica correspondences fair a bit better in having an average of 3.25 cognates per sound
correspondence. There are five examples of one-cognate correspondences. There is one example of one cognate set giving two sound correspondences in Tunica (68).

(68) PCA *
*s: Tunica š & PCA φe: Tunica yi
RACCOON. PCA ehsepan-a, Tunica yiši

The Atakapa correspondences have on average 3.74 cognates each. There are seven sound correspondences which only have one cognate in each case. There are 27 sound correspondences given for the PCA-Natchez relationship; however, 10 of them are only exemplified by one cognate set. Haas only gives 10 sound correspondences for PC and Chitimacha, some of which should be broken up into more since they are correspondences between one segment in PCA and two or more different segments in Chitimacha (69).

(69) PCA *
*n : Chitimacha n, ġ
DIE. PCA *nepe-; Chitimacha nu:p-
FISH. PCA name:- (root); Chitimacha ni:pi 'fish scale'
IN. PCA *enki 'in, at'. Chitimacha -ŋki
NAME. PCA Pre-PCA *i:n- (root); Chitimacha nuy-t- 'call by name'
ONE. PCA *kwet, *kot 'one, other'; Chitimacha ?unk'u

According to Campbell (1997b) several of the cognates are onomatopoetic. Campbell incudes BEAT, BEE, BLOW, CROW, CRY/WEEP, HAWK, RING[HUM, ROAR], SHOOT, TO SOUND, SPIT, SPLIT, SWALLOW, WHISTLE. Again Campbell (1997b) provides no argument as to why these forms must be onomatopoetic and independent iconic innovations in all the languages.

Campbell (1997b) also criticizes some of the cognates for involving liberal semantic extensions. Examples are given in (70) and (71). The former is perhaps not a very serious semantic extension if one takes just the Algonquian and Gulf forms since in many languages 'brain' and 'head' are the same word: Ojibwe for example.
(70) BRAIN (HEAD). PCA *-temp-i; with possible variant *-ntepi; PM *-lupi 'brain'; Natchez ṭeten 'head of hair'  
(Haas 1958b: 243)

(71) DAUGHTER. PCA *-ta:n-a; Wiyot tor 'son, father'. Atapaka tey 'mother, daughter' (E. dialect), 'mother' (W. dialect).  
(Haas 1958b: 244)

Although there are 132 cognate sets some of the sets Haas compares are represented in only one Gulf language as in (72) and (73).

(72) BIG. PCA *meʔθ-, *meʔši; Atapaka mec 'large, tall'

(73) CRAWFISH. PCA *aQša:ke:w-a. PM *sakči/u

Later, Haas (1959) included Tonkawa into the Gulf grouping. Haas compared Tonkawa forms with PCA or PCEA forms, the latter taken from Siebert. 22 sound correspondences are given with an average of 2.64 cognates in each case. Some of the correspondences are highly suspect. For instance the same word 'dig' which would reconstruct to a CVC word apparently demonstrates two correspondences and nothing else. *l:n and w:kw given in (74).

(74) DIG. PCA *wa:l-; Tonkawa kʰan(ʔase)-.

In this case I am competely at a loss at to why Haas included this form since the chances of getting a one word sound correspondence in CVC words when the vowel is ignored is 100%! Generally, however, the one cognate set sound correspondences have more similarities in phonological form.

In further support of the Algonquian-Gulf grouping (in an article which seems vaguely reminiscent of Bloomfield's (1925) 'A Note on Sound Change' in rhetoric if not argumentation) Haas (1963) reported that she found the word háshkuni 'skunk' from a word list compiled by Gatschet. Previously the word for 'skunk' could not be shown to be a cognate for the PA word *šeka:kw-a. A comparison was then possible as in (75).
Haas had also previously pointed out that $kw$- word final clusters in Algonkian correspond to $i/u$ in Proto-Muskogean, and she reports that the correspondence fit quite well with her expectations.

Again no morphological arguments are presented. According to Goddard (1975) these are the most important for establishing genetic relationships. Eventually Haas dropped her own proposal regarding the Algonkian-Gulf relationship (Pentland: personal communication). Unlike the Algonkian-Ritwan proposal none of the etymologies presented in Haas (1958b) have been productive in revealing the phonological history of any of the languages in question. It ought to be pointed out, however, that despite the fact that Haas' Algonquian-Gulf is now generally rejected the evidence she gave is not much weaker than that which Sapir gave for Algonkian-Ritwan. The key difference seems to be in the personal prefixes and that there are clear structural correspondences between the two languages which can be set up as grammatical cognates (cf. Harris & Campbell 1995 and examples (39) and (40) from Goddard (1975)). Given the new cognate set proposed by Munro (1994) and the ludicrously of some of Campbell's (1997b) criticisms of it, the evidence for the Algonquian-Gulf relationship needs to be reassessed.

4. Algonquian-Salishan

Sapir (1929) grouped Salish with Wakashan and Chimakuan in a family he called Mosan. Subsequently Swadesh (1953a, 1953b) attempted to provide lexical evidence for this grouping. Later he abandoned the proposal implicitly in his later classification when he grouped Wakashan with Aleut-Eskimo and some Old World languages (Swadesh 1963). In Sapir's (1929) schema Mosan was a subgroup of a larger family called Algonkin-Wakashan. Haas (1965) attempted to provide evidence that Algonkian was related to the Mosan grouping, however, it is not clear from her cognates represented in the Mosan languages that either Salish, Wakashan or Chimakuan should be considered more closely related to each other than each is to Algonkian.
Hence she suggests that it may be more fruitful to compare Algonquian to each of the Mosan groups individually. Bakker (2006, 2008) has recently provided grammatical and some lexical evidence for connecting the Algonquian with Salish. This proposal is taken up here.

The genetic grouping of the Salish languages has never been questioned. Comparative work with Salish began with Boas and Haeberlin (1927). Boas & Haeberlin presented sound correspondences. No formal reconstructions are provided, but the argumentation presumes the direction of change. Reconstructions from Boas & Haeberlin (1927) for Proto-Salish could thus be assumed (Thompson 1979: 702). Many aspects of not only the comparative phonology but also the comparative grammar have been reconstructed for Proto-Salish (cf. Kroeber 1999). Furthermore, fairly detailed etymological dictionary has been published (Kuipers 2002) and interesting etymological histories can be reconstructed (cf. Thompson 1979). Thus, Bakker (2006) is comparing two fairly well-documented language families with relatively well-understood phonological and in some cases grammatical histories when he attempts to link the Algonquian with Salish. This is in contrast to Sapir's (1913) original attempt to link the Ritwan languages with the Algonquian languages and Haas' (1958) attempt to link the Gulf languages with the Algonquian languages. In the former case better data was provided later before the relationship was accepted by the linguistic community. In the latter case the (non- Muskogean) Gulf languages are still relatively poorly documented and not very well understood (and all extinct). Thus, the lack of solid evidence for a Gulf (or Yuki-Gulf) genetic grouping could still be attributed to the poor understanding of the nonextinct Chitimacha and Atakapa languages (cf. Kimball 1994). The potential Algonquian-Salishan relationship is different in this regard. If convincing linguistic evidence cannot be found we can be sure that there is either no genetic unity between the two groups or that the time depth is so great that even if there was a genetic relationship we would not be able to prove it.

Unfortunately I do not yet have access to Bakker's (2006) original article in the *Papers for the 41st International Conference on Salish and Neighbouring Languages* published in the 18th volume of the *University of British Columbia Working Papers in Linguistics*. Thus, I am

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23 The University of Manitoba does not have the UBC Working Papers. When I attempted to get a copy via document delivery I was told that the particular issue had been checked out. The issue is being brought in from Berkeley 3 weeks after this paper is due.
forced to describe the evidence for the relationship based on a source which argues against the relationship (Van Eijk 2007). Fortunately Bakker (2008) responded to Van Eijk's criticisms so his original arguments are perhaps reconstructable to a certain extent. Van Eijk (2007) argues that any evidence that might suggest a possible relationship between Algonquian and Salish is most likely due to coincidence, to universals or to borrowing. Van Eijk (2007) supports this claim by citing Denny (1989, 1991) who claims that Salish and Algonquian had urheimats that were closer in geographic proximity than has previously been suggested by reconstructions of the Proto-homeworlds of these languages (Siebert's (1967) homeworld for the Proto-Algonquians is in the great lakes area whereas Kinkade (1990) place the Proto-Salish urheimat in southwestern British Columbia). This issue seems to have no bearing on whether or not the Algonquian is related to Salish. Both authors (Bakker 2006, Van Eijk 2007) apparently use it to support their case. For Bakker (2006) Denny's (1989, 1991) claims constitute evidence for a genetic relationship whereas for Van Eijk (2007) they constitute evidence for diffusion.

Van Eijk proceeds to compare the Algonquian and Salishan phonological systems. Bakker (2006: 24-5) cites the fact that Bloomfield's θ may have been a voiceless lateral ɬ, which is attested in Proto-Salish and that Pre-Proto-Algonquian may have had glottalized stops at one point, making it more similar to the Proto-Salish system. These considerations are completely irrelevant. Whatever the phonological inventory of Proto-Algonquian-Salishan might have looked like a very radical shift would have had to have taken place in order to arrive at the Proto-Algonquian and Proto-Salishan systems. Either Proto-Algonquian would have had to innovate long vowels by the loss of consonants or Proto-Salishan would have had to innovate a larger consonant inventory by the loss of vowel segments in Proto-Algonquian-Salish or some combination of this. Considerations such as these make arguments based off the fact that these proto-languages might have slightly more similar phonological inventories based off a few consonants pointless. Van Eijk (2007) cites Hock (1986: 562-3) stating that systematic, recurrent

24 This citation is from Van Eijk (2007) as I do not have access to the UBC Working Papers (cf. n22 this paper)

25 Denny's (1991) claims do not actually contradict Siebert's (1967). It would be more accurate to state that Denny is discussing a pre-homeworld since 'the Proto-Algonquians moved east as one culture, and then their language spread and diversified into the daughter languages in the Northeast' (Denny 1991: 103).

26 This is suggested by Pentland (1987) another source which I have not consulted.
sound correspondences are in fact needed anyway to 'prove a genetic relationship' (Van Eijk 2007: 404). Bakker's (2006) primary arguments were apparently based off morphological similarities between the two languages. Thus, Bakker (2006) follows Goddard (1975) in considering these as the most important for proving genetic relationship. Van Eijk (2007: 407) states that Goddard's (1975) arguments rest on (a) the inescapable overall morphological resemblances between Wiyot, Yurok and Algonquian, and (b) numerous details, particularly in the pronominal prefix system of these three languages. He fails to mention that another important argument for Goddard (1975: 10-11) is that cognate sets in the languages can often elucidate coherent phonological histories instead of just being tabulated similarities between the languages compared. Bakker points out that the overall structure of the examples Goddard presents for morphological correspondences between Wiyot, Yurok and Cree are also exemplified in the Salish languages. These are repeated here in (76-77) from Goddard (1975) and in (78) from Bakker (2006) taken from Van Eijk (2007: 408).

(76) kaskika:te: -sin- 'break one's leg by falling'

Cree kask -ik:te: -sin-
break leg fall
takwaʔsonor 'break arm by falling'
Wiyot takwaʔ -aʔson -or-
break arm by.falling

(77) pakamihkwe:hw- 'hit (someone) in the face'

Cree pakam -(i)hkwe- -hw-
hit face act on animate object
s'o:peʔweyet 'hit (someone) in the face'
Yurok s'o:p- -eʔwey- -et-
hit face transitive marker (Goddard 1975: 4)

27 Hock (1986: 562) does not in fact say this. Rather he states that after 'reducing the likelihood of chance similarities and of similarities due to linguistic contact, we can strengthen our case for genetic relationship by showing that the correspondences encountered in our putative cognates recur in a systematic fashion'. Presumably there is a difference between 'strengthening' a genetic relationship and 'proving' one.
(78) **túpusən**

Lillooet **túp-** -us- -ən

punch in.the.face transitivizer

'to punch someone in the face'

(Van Eijk 2007: 408)

Bakker also mentions some similarities in morphological details. These include the correspondence in meaning and form between the Cree reflexive -iso- and Proto-Salish *-t-sewt-* and the transitivizer -t both in Cree and in Salish. Van Eijk (2007) points out numerous differences, however, that 'strongly argu against any genetic relationship' (Van Eijk 2007: 408). These are given in (79-84).

(79) 'Algonquian has an animate-inanimate gender distinction: Salish lacks this (or any gender distinction, except for a female vs. non-female distinction in Coast Salish)'

(80) 'the distinction between the first person plural inclusive and exclusive, which is fundamentally a feature of Algonquian grammar, is absent from Salish, except for Shuswap...'

(81) 'the difference between controlled and non-controlled events, which is crucial in Salish (as in Lillooet ḥac'x-ən 'to see it' and s-ʔac'x-s 'to watch over it', with 'non-control' -s) is as such absent from Algonquian.'

(82) 'Both families make use of various patterns of reduplication but there are strong differences in forma and function between the individual patterns in both languages.'

(83) Salish and Algonquian have completely different pronominal subject and object marking.

The strength of these dissimilarities in terms of providing evidence against a genetic relationship is contingent on our understanding of how these systems maintain themselves over great time depths. In the case of (79) Van Eijk is actually wrong to conclude that this is a difference. The reason is that according to Thompson (1979) gender is reconstructible into Proto-
Salish. Not only is gender reconstructed into Proto-Salish a near vs. remote contrast is probable. Thompson (1979: 745-6) states:

'Preliminary consideration suggests that Proto-Salish had at least two intersecting oppositions: marked feminine or secondary vs. unmarked non-feminine or primary, and marked absent vs. unmarked present. A possible third opposition, less clear, may have been near vs. remote... it seems impossible to explain the gender distinguishing deictics of the coastal languages, interrelated as they appear to be as innovations.'

The comparison between a near vs remote contrast in Proto-Salish would correspond quite well to the gender distinction in the Algonquian languages. Mühlbauer (2008) has recently discussed the gender distinction in Cree as one involving intentionality. Inanimate nouns are those which have extensionality 'mapped' onto them whereas animate nouns are not specified for the intentionality distinction. This means essentially that speakers do not assign a theory of mind to the concept entailed by inanimate nouns whereas animate nouns are unspecified. Mühlbauer's conception of animacy in Plains Cree can probably apply to Ojibwe according to my own consultants (Roger Roullette: personal communication, Bill Tredway: personal communication), thus perhaps intentionality driven gender is reconstructable into Proto-Algonquian. It would take only a fairly obvious metaphorical extension to arrive at a near vs. remote distinction (or present vs. nonpresent) from the intentionality distinction; REMOTE OBJECTS ARE EXTENTIONAL (cf. Lakoff 1993). Even if there was no reconstructible similarity between the gender systems of Algonquian and Salish, gender structures are not necessarily conservative systems that are retained through great time depths. For instance Old English had a three gender system comprised of masculine, feminine and neuter marked on both animate and inanimate nouns. This was replaced in the modern system by 'logical gender' 'where gender distinctions are in principle based on sex' (Wehna 1978: 399). Furthermore, as pointed out by Bakker (2008: 26) (citing Reichard 1925: 89) Yurok and Wiyot have no gender distinction and the Algic grouping is considered proven. Bakker (2008: 26) also points out that the latter consideration applies to Van Eijk's (2007) criticism in (80) since neither Wiyot nor Yurok have the inclusive exclusive distinction. Van Eijk (2007) bases (81) off a comparison between the

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28 Obviation involves mapping extentionality onto animate nouns, those not specified either way (Mühlbauer 2008).
indicative mode in the Algonquian languages. Van Eijk (2007) compares the structures in (84) and (85).

Lillooet

(84a) ʔac'xən-c-as
    see-1.SG.O/ACC-3.SG.S/NOM
'He sees me'

(84b) ʔác'xən-ø-ɬkan
    see-3.SG.O/ACC-1.SG.S/NOM
'I see him'

Cree

(85a) ni-wāpam-ē-w
    1.SG-see-DIR-3.SG
'I see him'

(85b) ni-wāpam-ikw-w
    1.SG-see-INV-3.SG
'He sees me'

(84) demonstrates that Lillooet operates according to a nominative-accusative system and has V-Obj-Sub order in the verbal piece. (85) demonstrates that Cree operates according to a direct-inverse system where the occurrence of the prefix is conditioned by the person hierarchy. Van Eijk (2007: 409) states 'It is difficult to see how two systems that are so different could have common from a common source'. It is actually not that difficult to see a relationship given a family with a long enough time depth, Indo-European for instance. The Germanic languages use an nominative-accusative system whereas Hindi uses a split ergative system (cf. Dixon 1994, and n8 of this paper for an example from Amazonia). Bakker (2008: 27) points out that the prefixes in Algonquian have been argued to be innovations (cf. Teeter 1974 for instance). One should, therefore, compare the Algonquian conjunct order which has no prefixes. Also, Wiyot and Yurok
appear to operate according to the same system as the Salish languages. Reichard (1925: 73) states 'pronominal suffixes in the active voice, appearing in the following order: object, immediately following the final position stem; indirect object; subject' (cited from Bakker 2008: 27).

Bakker (2006) also mentions the fact that both Algonquian and Salish have person hierarchies. However, both he and Van Eijk (2007) agree that this is most likely attributable to diffusion as it spread from the Pacific coast. Furthermore, Van Eijk (2007) points out that person hierarchies are a common feature of morphologically complex languages.

Bakker (2006) explains a lack of lexical correspondences between Algic and the Salish languages as due to massive borrowing into Salish from non-Salish sources. Furthermore, lexical replacement in the Salishan languages has probably been accelerated from a tabooing custom among certain Salish groups. The result is a 'possible acceleration of "normal" lexical item replacement' (Elmendorf 1970: 74). However, Van Eijk (2007: 412) points out that 'most borrowings and internal shifts have been identified as such'.

In a later article Bakker (2008) gives some lexical comparisons between Proto-Algonquian and Proto-Salishan forms. The first set of lexical comparisons show no sound correspondences which indicates that the resemblances may be due to borrowing. Bakker (2008: 30) states that 'Most of these roots show sequences of two or three consonants with roughly the same articulation places in both reconstruced languages'. This is demonstrated in (86) from Bakker (2008: 29).

(86a) Proto-Salish  m  ø  x  k'/kn
    Proto-Algonquian  e  h  k  w  a
    'louse'

(86b) Proto-Salish  t  a  k
    Proto-Algonquian  t  a  k
'percieve by hearing'

(86a) could be regarded as a fairly weak correspondence because only two of the segments correspond very well; h/x and k'/k. (86b) is a very strong correspondence but suggests borrowing if anything because the assumption that these forms are cognate implies that there have been no sound changes for the relevant segments. Given the phonological inventories this should be abandonned. Other lexical comparisons show correspondences only in the distinctive features of the segments as in (87).

(87)  
<table>
<thead>
<tr>
<th>Proto-Salish</th>
<th>?</th>
<th>a</th>
<th>m</th>
<th>u</th>
<th>t</th>
</tr>
</thead>
<tbody>
<tr>
<td>Proto-Algonquian</td>
<td>a</td>
<td>p</td>
<td>i</td>
<td>w</td>
<td>a</td>
</tr>
</tbody>
</table>

'to sit, to be at home'

It is not difficult to find some segments that match in distinctive features since many include fairly large natural classes of segments. It is unclear to me why Bakker (2008) considers (87) to be a potential cognate. Bakker (2008: 31-2) lists 7 body part terms where only a few segments line up. The cognate set with the most segment per segment correspondence is given in (88).

(88)  
<table>
<thead>
<tr>
<th>Proto-Salish</th>
<th>q'</th>
<th>a</th>
<th>w</th>
<th>a</th>
<th>ɬ</th>
</tr>
</thead>
<tbody>
<tr>
<td>Proto-Algonquian</td>
<td>w</td>
<td>e</td>
<td>ɬ</td>
<td>k</td>
<td>a</td>
</tr>
</tbody>
</table>

'bone'

In addition Bakker (2008) lists 15 lexical correspondences between Proto-Salish and Proto-Algonquian and 4 between Proto-Salish and Proto-Algonquian which demonstrate a sound correspondence in the initial segment, except one which demonstrates a correspondence in the last (Proto-Salish *pan 'white', Proto-Algonquian *aap- 'white'. Given the large consonant inventory of Proto-Salish the number of correspondences cited by Bakker (2008) is rather surprising. However in some cases the comparison involves semantic extension. Examples are given in (88).
(88a) Proto-Salish *pəəl̓x 'pierce, pop out', Proto-Algonquian *patahk- 'enter st. in a hole'

(88b) Proto-Salish *pisaʔ 'small animal (bird, bug), Proto-Algonquian *peleehš- 'bird'

Some forms are exactly the same, which is an unlikely correspondence as it assumes that there were no changes from Proto-Algonquian-Salishan for the relevant segments (89).

(89) Proto-Salish *pan 'time, period', Proto-Algonquian *-pan 'preterite suffix'

Bakker (2008: 35) invokes metathesis which is quite common in the Salishan languages but gives no examples of where this would apply in his correspondences. Van Eijk (2007) thinks its likely that many of Bakker's lexical correspondences are sound symbolic; 'several denote an action with a sever blow ("to peirce, fart,blow, prod," etc.), and thus naturally show p n the forms' (Bakker 2008: 35). Furthermore, presumably following Goddard's (1975) lead in stating that etymologies ought to illuminate problems to do with the phonological history of the languages in question. Bloomfield had reconstruccted in *çk into Proto-Algonquian but the phonetic detail of the symbol was unclear. In Bakker's (2008) view comparison with Proto-Salish helps solve this problem as in (90).

(90) PA *meçkw 'red, blood', Proto-Interior Salish *mit'kayaʔ 'red,blood (but cf. Columbian Salish məłk'áyaʔ)

There is only one form which demonstrates this correspondence, however, and it is unclear how it obviously provides evidence for the phonetic shape of *çk anyway. Thus, Bakker (2008) presents numerous lexical similarities but these are most likely due to borrowing, if even at a great time depth. The one sound correspondence that is provided is interesting but is difficult to assess on its own since it does not even begin to describe the differences in phoneme inventories between the two languages.
References Cited

Aikhenvald, Alexandra Y..

Baker, Mark.

Bakker, Peter.

Berman, Herman.

Bloomfield, Leonard.
1925 On the sound-system of Central Algonquian. Language 1: 130-56.
1928 A Note on Sound-Change. Language 4. 99-100.

Blevins, Juliette.

Blevins, Juliette and Andrew Garrett.

Boas, Franz & Herman Haeberlin.

Booker, Karen M.

Bybee, Joan, Revere Dale Perkins, William Pagliuca.

Campbell, Lyle.

Campbell, Lyle, Ives Goddard.

Comrie, Bernard.

Craig, Colette.

Dixon, Ronald and Arnold Kroeber.

Dixon, R. M. W.

Dryer, Matthew S.

Elmendorf, William W.

Gallatin, Albert.

Gildea, Spike.

Givón, Talmy.

Goddard, Ives.

Greenberg, Joseph H.

Gursky, Karl-Heinz.

Hamp, Eric P.

1995  Historical syntax in cross-linguistic perspective. Cambridge: Cambridge University Press.

Haas, Mary R.


Heine, Bernd, and Mechtild Reh.


Hewson, John.


Hymes, Dell.

Jones, Doug.

Kavitskaya, D.

Kimball, Geoffrey.

Kinkade, M. Dale.

Kouwenberg, Silvia.
2001 Twice as meaningful: Reduplication in pidgin, creoles, and other contact languages. London: Battlebridge.

Kroeber, Alfred L.

Kroeber, Paul D.

Lakoff, George.

Langdon, Margaret.

Latham, Robert G.

Longacre, Robert.

Martins, Silvana and Valteir Martins. 

McQuown, Antoine. 

Meillet, Antoine and Marcel Cohen. 

Michelson, Truman. 

Mithun, Marianne. 

Morgan, Lawrence. 

Mühlbauer, Jeffrey Thomas. 

Munro, Pamela. 

Nichols, Johanna. 

Pentland, David, H. 
Poser, William J.  

Powell, John W.  

Reichard, Gladys.  

Rine, Don.  
1999  How hard i it to match CVC-roots?. Transactions of the Philological Society 97. 213-44.

Robins, R. H.  

Ruhlen, Merritt.  

Sapir, Edward.  

Schusky, Ernest L.  

Siebert, Frank T. Jr.  

Swadesh, Morris.  

Swanton, John R.

Tallman, Adam J. R.
2009a Semantics and Emergent Phonology. ms: University of Manitoba.

Thompson, Laurence C.

Taub, Sarah F.

Teeter, Karl V.

Tomalin, Marcus.

Van Eijk, Jan.

Voegelin, Carl F.
Weir, E. M. H.

Wętka, Jerzy.

Willey, Gordon R.