REPORT 11

SURVEY OF CALIFORNIA AND OTHER INDIAN LANGUAGES

PROCEEDINGS OF THE MEETING OF THE HOKAN-PENUTIAN WORKSHOP

June 17-18, 2000
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PREFACE

The year 2000 was the 30th anniversary of the first Hokan languages conference. That first conference was imagined, planned and run by Prof. Margaret Langdon at the University of California at Berkeley, with the assistance of Prof. Shirley Silver of California State University at Sonoma. Almost every year since then, Hokan workshops and then Hokan-Penutian workshop in the previous few years had been either very small or even cancelled due to the lack of a sufficient number of people submitting paper titles. There was some thought of abandoning the Hokan-Penutian workshops altogether. Margaret felt that it would be a shame for this long tradition to end without a last hurrah, and so I offered to hold a Hokan-Penutian Workshop at Berkeley in conjunction with the “Breath of Life” Language Workshop for California Indians. The Breath of Life Workshop is a biennial gathering of California Indians here at Berkeley, and is designed primarily for people whose languages have no speakers left. We give them tours of the campus archives and show them how to use publications, fieldnotes and recordings of their languages for their own purposes – primarily language learning and teaching. I felt it would be a good thing to show the linguists who spent their careers working on these endangered languages to see the use their work is being put to by the descendents of the very people they worked with years ago. Therefore, the first session of the Hokan-Penutian Workshop consisted of presentations by the participants in the Breath of Life Workshop. The anticipation of this treat may have played a role in bringing a relatively large crowd here in 2000, perhaps along with billing the workshop as “The (Last?) Hokan-Penutian Workshop.” Sixteen papers (not counting the Breath of Life presentations) were given at the workshop, eight of which are published in this volume.

With both the Hokan and Penutian hypotheses in doubt, there is always a question as to which languages should be included at the workshop. Although my sympathies are with the “splitter” camp in linguistics, I’m definitely a social lumper. Therefore, for purposes of the workshop I chose to define “Hokan” and “Penutian” as rubrics rather than language stocks, and advertised the workshop as being “for any language that has ever been hypothesized to be Hokan or Penutian.” We thus have papers ranging from Tsimshianic to Zuni, and—oh, well— we even accepted Juliette Blevin’s excellent paper on Yurok, an Algic language, which has never been hypothesized as either Hokan or Penutian.

At the business meeting held at the end of the Hokan-Penutian workshop, no-one wanted to say that this was the last one. Instead, we voted to continue with the workshops on a biennial basis, to be held here at Berkeley from now on, overlapping with the Breath of Life Workshop as it did in 2000. As I write this preface, the two years have already passed, and we are preparing for the 2002 Breath of Life Workshop, which this year will overlap with – not the Hokan-Penutian Workshop – but the 50th Anniversary Celebration of the Survey of California and Other Indian Languages. The upcoming conference for the Celebration subsumes participants in Hokan-Penutian Workshops. I imagine that our biennial gathering will continue on: whether it will be a Hokan-Penutian workshop in 2004 or something broader than that remains to be seen.

Leanne Hinton
Director of the Survey of California and Other Indian Languages
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NOTES ON SOURCES OF YUROK GLOTTALIZED CONSONANTS

JULIETTE BLEVINS

University of Luton

1. INTRODUCTION. A genetic relationship has been established between Yurok, Wiyot and the Algonquian languages (Sapir 1913, Haas 1958, Teeter 1974, Goddard 1975, Berman 1982a, 1984, 1990). Yurok has ejectives as well as laryngealized fricatives and sonorants. Proto-Algonquian lacks ejectives and laryngealized sonorants, while Wiyot has no ejectives, but does show pre-glottalized fricatives and sonorants which could be considered unit phonemes. In this paper, I build on previous work on the sources of Yurok glottalized consonants, focusing primarily on the origins of ejectives. Yurok ejectives are less common than their non-ejective counterparts, and have a more limited distribution (see Appendix II). As noted by Gensler (1986:71) "The defective and skewed distribution of ejectives in Yurok... suggests that the ejective series is a relative newcomer in the historical phonology...". In this study, I provide support for this view based on internal reconstruction and comparison with Wiyot. In section 2, Berman's (1982a) proposal that glottalized consonants reflect *Ct clusters is strengthened by providing further etyma, and suggesting a regular sound change in the history of Yurok taking *Ct > Ct > C'. In section 3, I identify a morpheme associated with hairiness and bushiness, *-Vp$, with -Vp reflexes in Yurok terms for plants and other growing things. By demonstrating other *t/t correspondences, it is possible to derive all these instances of p' from the same sound change proposed in section 3: *Vp$ > Vp > Vp? > Vp'. In sections 4 and 5 I discuss possible sources of Yurok c' and t' which both have limited distribution. In section 6 I identify ejectives which are unlikely to be inherited, and identify potential loan sources in languages of the area. Yurok data sources are Robins (1958) supplemented by Berman (1982b). Wiyot sources are Teeter (1964) and Teeter and Nichols (1993).

2. *Ct CLUSTERS. The most obvious feature suggesting a relationship between Yurok, Wiyot and the Algonquian languages is the form of the pronominal prefixes (Sapir 1913, Goddard 1975). However, as pointed out by Goddard (1975:250-54), a problem for the comparative method was the fact that Yurok prefixes are glottalized on nouns and verbs, but not in independent pronouns.

(1) Pronominal prefixes

<table>
<thead>
<tr>
<th>PA</th>
<th>YUROK</th>
<th>WIYOT (before alienable nouns, T: 39)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1st</td>
<td>*nc-</td>
<td>?nc- (cf. nek ‘I’)</td>
</tr>
<tr>
<td>2nd</td>
<td>*kc-</td>
<td>k'c- (cf. kci? ‘you sg.’)</td>
</tr>
<tr>
<td>3rd</td>
<td>*we-</td>
<td>?we-, ?u</td>
</tr>
</tbody>
</table>

An ingenious solution to this problem is presented in Berman (1982a:418), where it is suggested that the source for glottalization in the pronominal prefixes of Yurok is the -t- infix found in the related languages: "The glottal element in these prefixes is cognate with the t which is inserted after the pronominal prefixes in Proto-Algonquian and in Wiyot when the following element begins with a vowel." The quality of the prefixal vowel in all languages is predictable, and is treated as epenthetic. Proposed phonological forms of prefixes on vowel-initial stems are shown in (2):

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1 Yurok data is from Robins (1958) unless noted otherwise. Abbreviations used are: B Berman (1982b); PA Proto-Algonquian; PAR Proto-Algonquian-Ritwan; T Teeter (1964); TN Teeter and Nichols (1993, vol. I); WIY Wiyot; YUR Yurok.
(2) Pronominal prefixes before vowel-initial non-dependent stems (assuming [e] is epenthetic)

<table>
<thead>
<tr>
<th></th>
<th>PA</th>
<th>PRE-YUROK TO YUROK</th>
</tr>
</thead>
<tbody>
<tr>
<td>1st</td>
<td>*ni-</td>
<td>*ni- &gt; *niʔ &gt; ?ni-</td>
</tr>
<tr>
<td>2nd</td>
<td>*kt-</td>
<td>*kt- &gt; *k’-</td>
</tr>
<tr>
<td>3rd</td>
<td>*wt-</td>
<td>*wt- &gt; *wʔ &gt; ?w-</td>
</tr>
</tbody>
</table>

This allows us to dispense with the glottal drift towards the beginning of the word suggested by Berman (1982:418), and propose the simple sound change in (3), with subsequent metathesis in the case of pre-glottalized sonorants.

(3) Glottalization in Pre-Yurok

*Ct > (Cʔ) > C’ (C’ a glottalized consonant)

Goddard (1990:109) proposes other etymologies which support the general sound change in (3). These are shown in (4). In (4i) third person *-t- in Proto-Algonquian is cognate with third person stem-final glottalization in Yurok. Third person glottalization in Yurok is regular in third person forms of e-class and type 1 o-class verbs (Robins 1958:33-35) where stem-final /c, k, kʷ, p, t/ become ejectives and stem-final fricatives and sonorants are pre-glottalized. For example, from the stem maʔepet- ‘to tie up’, one finds maʔepetʔ ‘he/she/it ties up’, from the stem koʔmoʔ- ‘to hear’, koʔmoʔʔy ‘he/she/it hears’, and from the stem tenpewet- ‘to rain’, one finds tenpewetʔ ‘it is raining’.

(4) Evidence for *Ct > C’ in Pre-Yurok (Goddard 1990:109)

<table>
<thead>
<tr>
<th>PROTO-ALGONQUIAN</th>
<th>YUROK</th>
<th>WIYOT</th>
</tr>
</thead>
<tbody>
<tr>
<td>i. third person</td>
<td>*-t-</td>
<td>glottalization of final C</td>
</tr>
<tr>
<td>(PA conjunct order)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>ii. ‘spruce root’</td>
<td>*watapya</td>
<td>?wohpey</td>
</tr>
</tbody>
</table>

In (4ii), the *wt cluster resulting from syncope is reflected as ?w in Yurok and as simple t in Wiyot. Apart from the first-person prefix ?n-, there are no word- or stem-initial glottalized nasals in Yurok, suggesting that syncope never occurred in the context [N T]. In the remainder of this section, I present further cognate sets supporting the sound changes in (3).

In (5), the Ct > C’ sound change is suggested by possible correspondences between morpheme-internal Ti in Wiyot with T’ in Yurok, where T is an obstruent.

(5) Yurok T’ < *Tt

a. YUR męk’oh ‘to hit right in the center’; WIY bakt- ‘separate’ [TN:11]
b. YUR pek’ew- ‘to have something stuck in one’s throat’; WIY pik, pikto ‘half, halfway’.
c. YUR ḫup’ʔah ‘backbone’, ḫup’es ‘backbone of fish’; WIY tuptik ‘backbone of salmon’ [TN:313]
d. YUR hok’wah ‘rabbit’; WIY takwet ‘cottontail rabbit’ [TN:308] (cf. WIY hikw ‘buckskin, rabbit skin’ [TN:108])
e. YUR ḥup’o: ‘N., Hupa (place)’; WIY hapulono ‘N Hupa (place)’ [TN:120] (cf. YUR hop’ew ‘Klamath (place)’. hipse ‘ADV, up river, upstream’. hipur ‘ADV northward, toward the (Klamath) river mouth’)

In (5a) and (5a”), regular sound correspondences are found: *m > YUR m, WIY b; and *e > YUR e, WIY a, *p > YUR p, WIY p; *ew YUR ew/o, WIY o. Though the semantic
distances may seem great for these two pairs, it is not uncommon to find single lexemes with this full semantic range. For example, in Nhanda, an Aboriginal language of Western Australia (Blevins, 2001) the stem arda- has the semantic range ‘cut, halve, separate’ with derived forms including arda-thuda ‘short-cut; halfway, in the middle’, and arda-nda- ‘to break (of waves)’. The Wiyot stem bakt- is found in derived forms bakt-en- ‘separate by hand’, bakt-enit ‘open 3-Obj’ (of a door), baktetolit ‘3-Df goes away’, baktetolhabit ‘3-Df throw aside, throws apart’ (the boards of a house) [TN:11]. Yurok mik’othom- ‘to swallow, to gulp down’ may or may not lend unity to the pairs in (5a, 5a’) suggesting an ancient system of initial consonant mutations. In (5b), the YUR form shows evidence of *t > ? initially, with other conditioning factors yet to be discovered.2 The /w/ correspondence in the initial syllable is unexplained, though (5d) suggests a shift of short V > u/hi_p. (. a syllable boundary). In (5c) the YUR form shows diminutive vocalism so vocalic correspondences are regular. The h/t correspondence is irregular, though there could be other instances: compare YUR hikoc ‘across’, WIY takt- ‘across’; YUR holim- ‘to weave’, WIY toliv-akwh-uhy ‘N, weaving’.

For (5d), I suggest a stem *hipElt- with syncope yielding *hipto, and subsequent labialization giving *hi[tuˌo̞jpo], undergoing the sound change in (3).

The sound change in (3) is also supported by at least two correspondences for (derived) morpheme-internal *Rt sequences with Yurok ?R. The cognate set for MAGGOT is from Berman (1990:432), and that for BRANCH is from Proulx (1984:179) with the addition of a possible Wiyot cognate, the stem –bickad, a diminutive form of bitkad.

(6) Yurok ?R < *Rt

a. MAGGOT. PAR *yo:tkw-; PA *a:ξkwe:wa ‘maggot’; Wiyot yutw ‘maggots’; Yurok y′ek’i ‘maggot, worm’.

b. BRANCH. PAR *wehtekwani; PA *wehtekwani ‘branch’, Yurok ?wektun ‘heavy limbs’, ᵃweskwen ‘small branches, twigs’; WIY kosbickadac ‘V, small legs’ (Ikos/ a preverb).

Berman remarks for (6a) that: “Initial *y is rare in Proto-Algonquian. On the simplification of tkw in Wiyot, see Goddard (1975:258).” The PAR reconstruction is based on the PA and Wiyot forms, but the Yurok form is not integrated. I suggest that glottalization in Yurok is a reflex of a derived *yt cluster, with the sequences of changes shown in (7):


The two initial sound changes involve shortening and then loss of the stem vowel. This vowel loss gave rise to the yt cluster which then underwent the sound change in (3). A final change is e-penththesis.3 Proulx (1984:179) has a similar difficulty explaining the seeming loss of *ehr in Yurok in evidence in (6b), and states: “After loss of regular loss of *h in Yurok, *er was dropped by analogy with the general simplification of prefixes in that language...”. However, if as for (6a), we assume a rule of syncope resulting in an initial wt cluster, rule (3) will give rise to the attested initial ?w.

The only Ct clusters in Wiyot are /pt/, /kt/, and /kw/. For this reason, no direct correspondences between Yurok ?R can be found in Wiyot.

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2 Berman (1982a:417) remarks: “PR *t remained as t in Wiyot and split into t and ? in Yurok. The conditioning factor for the sound change *t > ? is unknown. An example of this correspondence is: PAR *tər-, PA *tər- ‘to exist’, Wiyot te- ‘to stay’, Yurok ?- ‘to be, to exist, to be born’. See below for further discussion of this sound change.

3 Berman (personal communication, 2000) suggests that the vowel-loss in (7) is unnecessary, with a change of t > ?, and shift of glottalization to word-initial position. However, as he himself points out, there is good evidence that in general, *kt and *kw sequences are reflected as such in Yurok, with no glottalization, e.g. PAR *tək’er- ‘kettle’, PA *akxekhwana ‘kettle’, Yurok tkek’er?i, tkek’er ‘pot’. The final in Yurok is unexplained.

The only other words with final Ct in Yurok are listed in (17); none have clear cognates in Wiyot.
The restriction of rule (3) to C\textit{t} clusters, combined with the existence of the productive diminutive sound-symbolism system shown in (8), leads us to investigate possible correspondences between *\textit{Ct} and Yurok \textit{Cc}.

(8) Yurok sound-symbolism (Robins 1958; Haas 1970)

<table>
<thead>
<tr>
<th>GRADE</th>
<th>CONSONANTS</th>
<th>VOWELS</th>
<th>MEANING</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>t</td>
<td>1</td>
<td>a, e, o</td>
</tr>
<tr>
<td>2</td>
<td>c</td>
<td>r</td>
<td>ø</td>
</tr>
</tbody>
</table>

Only one clear example has been found. It is given in (9) (see also Proulx 1984:178). The regular reflex of PA *\textit{i}: is Wiyot \textit{i}; Wiyot \textit{u} could reflect another instance of syllable-internal labialization, in this case *\textit{i} > \textit{u} /LAB\_LABX. (where . is a syllable boundary).

(9) Yurok \textit{Cc} / Wiyot \textit{Ct}, *\textit{Ct}

a. YUR \textit{mek} \textit{wu} ‘snail’; W1Y \textit{buk} \textit{t} ‘snail’ (cf. PA *\textit{mi:kehsu}, *\textit{-mi:kw} (deverbal) ‘snail’ [S183])

If rule (3) was a feature of Pre-Yurok, the pattern of sound symbolism in (8) also implies that there will be internal lexical correspondences in Yurok between words with \textit{Ct}’ and those with \textit{Cc}, where \textit{Ct} < *\textit{Ct} in non-diminutive forms. However, this is not the case. As far as I can tell, there are no cases where \textit{Ct} corresponds to grade 1 and the corresponding \textit{Cc} cluster to grade 2. On the other hand, there are a handful of regular correspondences for \textit{Ct}/\textit{Cc}, as shown in (10).

(10) Yurok \textit{Cc} grade correspondences

a. Grade 1
   \textit{lek} \textit{wu} \textit{em-et} ‘to dig’
   Grade 2
   \textit{rek} \textit{wu} \textit{cem-t} ‘V, to hollow out’

b. Grade 1
   \textit{hok} \textit{wu} ‘to butcher, to cut up’
   Grade 2
   \textit{hek} \textit{wu} ‘to chip arrowheads’

c. Grade 1
   \textit{mek} \textit{wu} \textit{ta} ‘N. stump’
   Grade 2
   \textit{mek} \textit{wu} \textit{ak} \textit{wu} ‘N. hip’

There is nothing remarkable about the pairs in (10), except that the \textit{Ct} clusters clearly have not undergone the sound change in (3) (or have been restored through analogy), and there is no diminutive vocalism in (10a, b). More remarkable sets are shown in (11).

(11) Other grade correspondences

a. Grade 1
   \textit{hek} \textit{wu} ‘eating basket’
   Grade 2
   \textit{hek} \textit{wu} \textit{aks} ‘N, small basket used as drinking cup’

b. Grade 1
   \textit{lept} \textit{wu} \textit{enok} ‘N, cloud’ (cf. WIY \textit{laptop} \textit{w} ‘cloud’)
   Grade 2
   \textit{lap} \textit{wu} \textit{swu} ‘to be covered in mist’
   Other
   \textit{lap} \textit{wu} \textit{ru} \textit{wu} ‘V, to be bad weather’ (cf. WIY \textit{laph} \textit{coh} \textit{w} ‘storm cloud’)

c. Grade 1
   \textit{lept} \textit{wu} \textit{y} ‘N, hair’ (cf. WIY \textit{lap} \textit{ti} \textit{w} ‘feathers’)
   Grade 2
   \textit{leps} \textit{wu} ‘N, down’, \textit{leps} \textit{wu} ‘to be hairy’, \textit{lo} \textit{ps} \textit{wu} \textit{moy ‘to be downy; to be hairy’ [B:205]}
   Other
   \textit{lep} \textit{co} \textit{w} ‘N, fur’ [B:205]
In (11a), there appears to be inversion of consonant associations: if [c] occurs in grade 1, then [t] occurs in grade 2. Notice that the grade 2 status of *hak"taks is supported not just by the semantics of this form, but also by the a-vocalism coupled with an instance of the archaic diminutive suffix /-s/ (Berman 1986). Another pair showing this relation is *cokco:p ’N, drum’ and *takta:patew ’V, to clap the hands’.

Berman (1986:419) suggests that grade 1/2 sets like (11b) and (11c) represent an “archaic pattern of diminutive symbolism in which t shifts to s rather than to c.” However, the forms listed under ‘other’ suggest an alternative analysis. Perhaps in these cases, as in (11a), /t/ and /c/ were already in use lexically for grade one (normal/pejorative) meanings. forcing a further alternate, /s/.

Given this possibility, there are two Yurok pairs, shown in (12), where an ejective in one word may be the reflex of *Cl, with a cluster in grade 2, or a related grade.

(12)

a. Grade 1  cek'or ‘periwinkle’
   Grade 2  tak"tam ‘N, dentalium shell’
   Other  seksun ‘N, small shell’ [B:214], seksoh ‘N, shell of any shellfish’

b. Grade 1  kep'olet ‘a hollow, a clearing, a space’ [B:202]
   Grade 2  kepso- ‘to lie in ambush’, kepbenf ‘fawn’

As shown in (10) and (11), Tt clusters do occur in Yurok. T an obstruent. A complete list of these clusters is given in Appendix I. In most cases I suspect that failure to undergo (3) is due to their occurrence (via morpheme concatenation) post-dating the sound change.

3. Reflections of *-ep/. Yurok underlying /p/ is found word-medially and finally, but occurs word-initially in only word, the inflected 2s imperative verb p'ems “cook!” (with doublet pemes). Some Yurok words relating to plant life end with a long vowel followed by /p/. Some examples are given in (13).

(13) Final V:p’ in plant words

a. ka:p’ ‘N, leaf, leaves, greenery, brush, grass [B:201]’


c. nahpayu:p’ ‘N, huckleberry bush’ (cf. nepoyon ‘N, wild parsley’)

d. pa?p’a:p’ ‘N, ladyfern (dyed in alder juice and used in basket making)’ (cf. pa?ah ‘water, juice’)

e. puyu:p’ ‘N, manzanita tree’ (cf. pyah ‘N, manzanita berry’)

f. *wahtu:p’ ‘N, Indian greens’ (cf. *wahtey ‘kelp, seaweed’)

g. ne?mu:p’ ‘N, vetch’

I suggest that these words contain the suffix -ep’. When suffixed to vowel-final stems, the suffixal vowel assimilates fully, giving rise to the long vowels shown in (13). The same Yurok morpheme is evident in the numeral system. The classifier numeral suffix for growing things is -ep’, as illustrated in (14). When suffixed to C-final stems, as in (14a, c, d), the underlying /e/ vowel surfaces. (Assimilation across glottal stop in (14b) is a regular process in Yurok.)

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4 Berman (personal communication, 2000) suggests that both forms in (11a) are from an underlying form /heck^u/, with two diminutives, the first by consonant symbolism (t > c) and the second by vowel symbolism plus suffixation. While this could be the case historically, there is no trace of diminutive meaning for the grade 1 form synchronically, nor any synchronic evidence of /hek^u/.

5 I am grateful to Howard Berman for bringing this form to my attention. It is noted in Robins (1958:46, 237).

6 I am grateful to Howard Berman for pointing out the analysability of (13b).
(14) Yurok number words for growing things

a. koht-ep' NUM. one growing thing [B:202]
b. na?-ap' NUM. two growing things [B:208]
c. nahks-ep' NUM. three growing things [B:208]
d. co?on-ep' NUM. four growing things [B:199]
e. wetowoyp' NUM. ten growing things [B:219]
f. co:p' NUM. 2nd element in compound numerals for growing things [B:199]

The forms in (15) support the hypothesis that -ep' is cognate with Wiyot -apt ‘hairlike object’ [T:52], both reflexes of earlier *-apt, a classifier for plants and other bushy or hairlike objects.

(15) Wiyot -apt ‘hairlike object’ [TN:3]

i. plants

a. donotetk-apt-ih ‘N. wide roundish leaves’ [TN:87]
b. hol-apt-ih ‘N. hand-grass’ [TN:117]
c. wadeik-apt-ih ‘N. leaves’ [TN:387]
d. walokal-apt-ik ‘N. beach pine’ [TN:390]
e. waretk-apt-ih ‘N. thimbleberry leaves’ [TN:393]

ii. other hairlike or bushy objects

f. donotatk-apt-ih ‘N. large bunch of hairy objects’ [TN:87]
g. dot-apt ‘N. a big hair-like object’ [TN:89]
h. haph-apt ‘V. tie in a bunch’ [TN:98]
i. holavol-apt-ih ‘V. what kind of feathers’ [TN:118]
j. kuc-apt ‘N. one hair-like object’ [TN:213]
k. laph-apt-ojad ‘V. bundles, ties in bunches’ [TN:252]
l. parag-apt ‘V. (feathered or hairy) drops down’ [TN:283]
m. rupatar-apt ‘V. hair all hangs down’ [TN:294]
n. ib-apt-ojuhwal ‘N. scythe (‘goes through hairlike objects with’)’ [TN:316]

The Wiyot suffix is exemplified in (15), where an association between this morpheme and plants is evident. Also note its classifier-like use in kucapt ‘one hairlike object’, kuc- ‘be one’. A further semantic parallel is between YUR cyu:p?o?y ‘N. comb; V to comb oneself?’ and Wiyot thaptojuhwal ‘N. scythe (‘goes through hairlike objects with’). Wiyot has undergone *e>a, while Yurok data suggests the development in (16):

(16) *-apt ‘growing things, bushy things, hairy things’

*ep' > *ept > *ep7 > ep'

The changes *ept > *ep7 > ep' are subsumed under (3). The change of *pt > *pt in Pre-Yurok is difficult to motivate. Distributional evidence is suggestive: there are only four lexemes with (final) pt clusters, as listed in (17).

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7 It is unclear why /-apt/ is realized as [apt] in only this form. The reference in the text is to seagulls.
(17) Yurok *pt clusters

a. *hipt ‘N, 4p inalien. ‘tongue’
b. hopít ‘N, deer sinew; sinew, tendon (of humans)’ [B:201] (cf. ho- ‘to make, build, repair, gather, cause’)
c. loílip ‘N, sting of insect, fang of snake’ (cf. le?l-oy- ‘to be burned, to be stung [by a nettle]’)
d. wenípt ‘N, shelled acorn’ (cf. wéna ‘grape’)

Interestingly, there are no clear Wiyot cognates for the forms in (17). However, (17a) does bear a striking resemblance Hokan forms for ‘tongue; cheek’, some of which are given for comparison in (18). Sapir (1917:33) reconstructs Proto Hoko *ipali, and a wealth of comparative evidence for Pomo is found in Oswalt (1977). I suggest that Yurok -hipt is a loan from Hoko which post-dates the change in (16). This morpheme appears to be a component of all the lexemes in (17). Given this, there are no attested exceptions to the sound change *pt > *pt proposed in (16).

(18) A comparison with Hokan TONGUE/CHEEK (v. Sapir 1917; Haas 1964; McLendon 1964)

<table>
<thead>
<tr>
<th>Yurok</th>
<th>h</th>
<th>i</th>
<th>p</th>
<th>Ø</th>
<th>ɨ</th>
</tr>
</thead>
<tbody>
<tr>
<td>Eastern Pomo</td>
<td>b</td>
<td>á:</td>
<td>ɬ</td>
<td></td>
<td>'tongue'</td>
</tr>
<tr>
<td>Southeastern Pomo</td>
<td>p</td>
<td>á</td>
<td>ɬ</td>
<td></td>
<td>'cheek'</td>
</tr>
<tr>
<td>Karok</td>
<td>á</td>
<td>p</td>
<td>Ø</td>
<td>r</td>
<td>i:</td>
</tr>
<tr>
<td>Migueño Salinan</td>
<td>i</td>
<td>p</td>
<td>a</td>
<td>ɬ</td>
<td></td>
</tr>
<tr>
<td>Atugewi</td>
<td>á</td>
<td>p̲n</td>
<td>Ø</td>
<td>l</td>
<td>i</td>
</tr>
<tr>
<td>Chimariko</td>
<td>h</td>
<td>j-</td>
<td>p</td>
<td>e</td>
<td>n</td>
</tr>
</tbody>
</table>

While (18) is suggestive of Yurok borrowing from Hokan, there is also a suggestion that Hokan languages borrowed the Yurok suffix -Vp in plant words. Sapir (1917) presents two cognate sets (119, 124) suggesting a Hokan suffix -ip denoting a tree or bush. In Karok, the ‘tree, bush’ suffix is -ip while in Atugewi it is -op. Silver (1974) extends the range of meanings to plant, tree, bush, and provides larger Northern Hokan data base. However, the absence of vocalic correspondences along with the non-analyzability of many Hokan forms, the language-internal variation within Karok (paha: ‘manzanita green’, xánbi:p ‘black oak’, axvé:p ‘Oregon oak’), and the lack of Hokan cognates for many Karok forms, suggests that Yurok tree, bush and plant names were borrowed into Karok and/or other Hokan languages. And, at the phonological level, the failure of any language but Yurok to show a final ejective /p/ in this suffix is consistent with borrowing first into Karok, which lacks ejectives, and later diffusion to other Hokan languages.

(19) Arguments for Hokan -Vp as loan from Yurok -ep ‘CLASS plant, bush, tree’ via Karok

i. Lack of vocalic correspondences in Hokan languages
ii. Non-analyzability of many Hokan forms
iii. Karok-internal suffix allomorphy
iv. Lack of Hokan cognates for many Karok forms with -Vp
v. Absence of final ejective /p/ in all languages but Yurok

4. SOURCES OF EJECTIVE /p/. The phoneme /c/ is an alveopalatal affricate, sometimes produced as an alveolar affricate. It occurs word-initially, word-medially and word-finally in Yurok, alone and in clusters, as shown in (20). In none of these words does there seem to be a clear association of the phoneme /c/ with diminutive sound-symbolism as sketched in (8): none of these words show the diminutive vowel [a], and five of them have [l] where [r] would be expected. I follow Robins (1958) then in positing /c/ as a Yurok phoneme.
(20) The distribution of /c/

i. initially
cele½i ‘N. kingfisher’
co:leu ‘ADV, below’
ckem ‘noninfl. V, to count’

ii. medially
kuces ‘N. grandmother’
pecelo: ‘N. sugar-pine tree’
peku ‘ADV, up river, upstream’

iii. finally
legeb ‘N. mudhen’
kelac ‘PRO, 2nd sg (obj.)’
pkene ‘N. pitch’

The glottalized counterpart of /c/ is /c/. In contrast to /c/, /c/ is limited in distribution, occurring underlyingly only in the forms listed in (21).  

(21) /c/ in URs

c’ek ‘N. wren’
c’waf ‘N. bird’
c’woloh ‘to wash by squeezing in water’
c’egi ‘N. black oak’
c’wona ‘N. coat’
nowrep ‘c’oy ‘noninfl V, to weed’
c’igol ‘N. saliva, foam’
c’i:yo ‘N. locust’
seyep ‘c’- ‘o-trans V, to sing’
c’ifah ‘N. dog’
waye‘ok(s) ‘N. puppy’
c’urely ‘ADV, how!, extremely’
k’ɔ:r ‘c’ crow’
sra: ‘N. tanned skin, quiver’

Semantically related forms with /c/ correspondences shown in (22) suggest that glottalization may have once played a role in the Yurok sound-symbolism system sketched in (8) (though see section 6 for potential loan sources for some of these).

(22) Evidence for /c/ grade correspondence

a. /t/ grade
tek⁵, tek⁶ eʔs ‘N. largest size owl, black’ [B:217]
/c/ grade
c’ek ‘N. wren’

b. /t/ grade
tewol, tewolew ‘Pacific Ocean’
/c/ grade
c’ewoloh ‘to wash by squeezing in water’

c. /t/ grade
tagas ‘N. rat’ < *tV’s
/c/ grade
c’ifah, c’if ‘N. dog’

d. /t/ grade	
tawoa: ‘N, locust⁹ < *tV:
/c/ grade
c’i:yo ‘N. locust’

e. /t/ grade
tohyet ‘eagle’, tohtel ‘red-tailed hawk’
/c/ grade
c’uc’if ‘N, bird’

As with the grade pairs in (11), in some cases (22c, d) we see an inversion of sound/meaning consonantism. For example, the vocalism and meaning of tagas ‘rat’ suggest that this is a grade 2 diminutive form, with ‘dog’ the grade 1 form, and similarly phonologically for (22d.) The problem then, is that there seems to be a sense in which [c] must be derived, in this case from /t/, but at the same time, certain [c] forms must be basic to give rise to derived diminutives. One working hypothesis is that the diminutive vocalism is a secondary development associated with the inflexion *-eg-, and that the asterisked forms in (22c, d) are the base for the c'-grade and the diminutive. One source of Yurok [c] then would be an earlier diminutive sound-symbolism. One language reported to have a system of C/C' sound symbolism is Wintu (Broadbent and Pitkin 1964:33):

---

8 It should be kept in mind that surface glottalized consonants also arise through regular morphology, e.g. third person marking on -class and type I -class verbs involves glottalization of stem-final C’s, while the imperative singular of certain verbs involves glottalization of the stem-final C plus the suffix -es. In this section I discuss instances of /c/ which do not have a clear source in the inflectional morphology.

9 Perhaps at one time the -form referred to a single locust, and the c'-form to a swarm of locusts.
“Consonant ablaut is characteristic of many word families and frequently involves an alternation between a plain stop-consonant in the basic form and a glottalized, voiced, or aspirated homorganic stop in the “augmented” form (e.g. qan ‘arm pit’, q’an ‘wing’; tipc‘u ‘flip it!’, tipc‘u ‘untie it!’).”

What distinguishes these sets from others with *t/c correspondences is not clear. An alternative hypothesis is that the /c/ grade forms in (22) were borrowed, with back-formation of /t/-grades. This second hypothesis receives some support from the loan search in section 5.

A second source of [c] is assimilation with a preceding ejective, in particular where pc > pc, as shown in (23) (cf. Gensler 1986:51-56). Both of the verbs in (23) appear to be derived from the stem /rep'o]/- ‘V. to clear land’ with syncope of the stem-final vowel, hardening of the sibilant to an affricate, and assimilatory glottalization. Given the meaning of this verb, the /p'/ could well originate from *-epf as discussed in section 2.

(23) Yurok pc > pc
   a. nowrep'c'oy ‘V. to weed’ (cf. rep'o- 2nd o-class V. to clear land’)
   b. seyep'c’ ‘V. to singe’ (cf. rep'o- 2nd o-class V. to clear land’)

5. A SOURCE FOR EJECTIVE t'. Recall from section 2 that third person *t- in Proto-Algonquian is cognate with third person stem-final glottalization in Yurok. Third person glottalization in Yurok is regular in third person forms of e-class and type I o-class verbs where stem-final /c/, /k/, /p/, /t/ become ejectives and stem-final fricatives and sonorants are pre-glottalized. Hence, there are some cases where *t-t > t-t. Morpheme-internal *tt clusters have not been reconstructed for PAR or PA, and syncope in the environment *t-t is so far unattested. For several dozen words then, the source of t' is mysterious. An exhaustive list of t-initial words is given in (24). Of the 13 distinct stems represented, the five in (24i) show initial reduplication, with t’ in both the base and reduplicative prefix; the form in (24ii) has a variant without glottalization; forms in (24iii) have tk’, or one form in t’ with a related form in k’; and word in (24iv) all begin with t’u-.

(24) Yurok word-initial t’
   i. a. t’k’oyah ‘noninf. V. to quiver, to scuffle’
      b. t’la’t’la? ‘to drip’
      c. t’ot’of ‘N. sparks from a fire’
      d. t’ot’of ‘N. mud, swampy ground’. t’ot’ol’-ik ‘noninfl. V. to be muddy’
      e. t’we’tw’wenoh ‘V. to (try to) pry up repeatedly’
   ii. f. t’lewold, tlewold ‘to fall (water)’
   iii. g. tk’eroh(s)- V. to strike with the nail of the index finger, to thump’;
         tk’ert’k’eroh(s) ‘to thump repeatedly’
         t’yoyk’-on ‘V. to be slippery’
   iv. i. t’unim ‘V. to be soft, to be tender, to be rotten’
      j. t’unow- ‘V. to grow (of plants)’
      k. t’weyky’-on ‘V. to be straight’
   v. l. t’p’o(k)(k’- ‘V. to be sensible, to come to one’s senses’
      m. t’ro’t’et ‘N. an edible plant’

While it is tempting to associate glottalization in (24i) with reduplicative sound-symbolism, there are similar reduplications without glottalization, e.g. kokonew ‘redheaded woodpecker’; kyekek’eyur ‘to whistle’, syewsyew ‘to lap (of small waves), to rattle (of strings

10 Other potential grade sets include: k’o:’i ‘bluejay’/ k’o:c’, k’o:’ ‘crow’ (where ? < *t in ‘bluejay’) and tegi’i ‘yellow’/ c’e:gi? ‘N, black oak’.

9
of shells). *tek'èk* ‘to chop, to hammer’, *tkartkar* ‘to stick together, to mend’. So, the glottalization must have some other source. The related lexemes in (25) suggest the sequence of developments in (26) for at least some *tk* clusters.

(25) *tk / t'k' / t* correspondences

   a. *tkeroh*(s-) ‘to thump’ (cf. *tkeroh*(s-) ‘to thump’, ‘to thump repeatedly’

   b. *t'oyk*on- ‘V, to be slippery’; *k'oyk*on- ‘to be slimy’; *todl*al ‘N, mud, swampy ground’; *tkat*. *tko:lon* ‘mud’. (cf. WIY *tukat* ‘is muddy’)

(26) *tk > t'k > t'k' > t'k'

Internal evidence, coupled with a potential Wiyot cognate suggests the sound change *tl > t'l, tl* possibly in progress. There are no word-initial *tl* clusters in Yurok, with the exception of those shown in (27):

(27) *tl > t'l, tl

   *tla?l* ‘to drip’; *tlewولد* *tewولد* ‘to fall (water)’; *tla:moks* ‘to leak’; *tle* ‘N, waterfall’ [B]; WIY *tolt* ‘falls out, drips’

In summary, the correspondences in sections 2-5 together with Berman (1982a, 1984, 1990) and Goddard’s (1975, 1990) earlier etymologies provide strong support for ejectives or glottalized obstruents as innovations in Yurok. Proto-Algonquian and Wiyot reflect the proto-Algonquian-Ritwan situation, where ejectives were absent. This runs counter to the proposals of Proulx (1984, 1985, 1991, 1992, 1994), where glottalized obstruents are posited for the proto-language, being lost in proto-Algonquian, and in some cases realized as aspirates in Wiyot.

6. Loans. In this section I investigate possible loan sources for (i) Yurok words with Ct clusters which seemingly have not undergone rule (3) (see Appendix I); and (ii) Yurok words with glottalized obstruents /p, t, c, k/ which are not the result of inflectional morphology and which do not have one of the potential sources discussed in sections 2-5 (see Appendix II). There is good evidence of bilingualism in Yurok and Karok at Weeps, of Yurok and Tolowa at Requa, and of intermarriage between Yurok and all surrounding tribes throughout the 19th century (Pilling 1978:137).

Words from Appendix I with unaccounted for Ct clusters are listed in (28).

(28) Monomorphemic Ct clusters in Yurok

   a. *hok't* ‘V, to butcher, to cut up’ (cf. *hok'c* ‘to chip arrowheads’) [B:200]
   b. *hok'taks* ‘N, small basket used as drinking cup’ (cf. *hek'c* ‘eating basket’)
   c. *keph* ‘N, horse neck, Washington clam’ (cf. *keph* ‘V, to lie in ambush’)
   d. *lèk'temèt* ‘V, to dig’ (cf. *re'c* ‘V, to hollow out’)
   e. *lepèk'o* ‘N, cloud’ (cf. WIY *lepèk* ‘cloud’)
   g. *neko?* ‘V, to collide, bump into’ [B:209] (cf. *nekcen* ‘V, to meet’)

In section 2 I commented on some of these in the context of the diminutive sound symbolism. Loans can be integrated into the system of sound symbolism (e.g. *p'apas* ‘apple’), so the fact that all words in (28) appear to have multiple grades does not argue for their inherited status. And there are no clear Wiyot cognates for the forms in (28) either, with the exception of (28e), and possibly (28c): WIY *katboh* ‘quahogs’ is similar to *keph* (28c), though the normal YUR/WIY correspondence is m/b, and an irregular metathesis is
also needed. All are potential loans then, and a search has been made for possible source forms in neighboring languages.

Yurok’s immediate neighbors were: Tolowa to the north along the coast; Karok to the northeast and east; Hupa to the southeast; Chilula to the south just inland; and Wiyot to the south along the coast. An initial search involved Tolowa, Hupa and Karok. A second search involved working outwards from these areas to Shasta, Chimariko, and Klamath. None of these searches yielded positive results, with one exception shown in (29). However, given the number of related forms in (11b), and the lack of Hokin cognates for the Chimariko form, it is likely that this word was borrowed from Yurok or Wiyot into Chimariko.

(29) YUR leptenok ‘N. cloud’, lapsway- ‘to be covered in mist’; WIY laptohw ‘cloud’, CHI aaptum ‘fog’ (cf. 11b).

If the clusters in (28) are inherited from *Ct and are exceptions to (3), then their exceptionality appears to be related to the transparent relationship between the two grade forms, with (3) either blocked in *Ct due to sound symbolism, or restored there via analogy.

The majority of other forms in Appendix I with Ct clusters involve morpheme boundaries and are assumed to have evolved after the sound change in (3). In particular, out of the 19 remaining words, 5 involve initial CVC-reduplication, which appears to be a Yurok innovation within Algic. Some of these may be based on borrowed stems. For example (30).

(30) Stem loan from Karok (Hokan), with Yurok CVC-reduplication

YUR tek+tekon- ‘V. to be sticky’; Karok if-tákan ‘to stick, adhere’ (Haas 1980:71).

Sources for Yurok /c/, k', k'w, p', t'/ have been proposed. In Appendix II, all words with initial, medial and final ejectives are listed, where it is clear that the ejective is not a result of inflectional morphology. Forms with potential sources discussed earlier are noted. The residue have no obvious source. Of these, many have Wiyot cognates, as noted, and hence, are unlikely loans.11 What remains are ejectives with potential loan sources. There are no apparent loans from the Athapaskan languages, Tolowa and Hupa. However, some Hokan and Penutian sources are possible, as suggested by the resemblances in (31) and (32).12

(31) Hokan resemblances

Abbreviations and data sources: SH Shasta (Silver 1964); K Karok (Bright 1957); CHI Chimariko (Dixon 1910); EP Eastern Pomo (McCleland 1964)

<table>
<thead>
<tr>
<th>Yurok</th>
<th>Resemblant forms</th>
</tr>
</thead>
<tbody>
<tr>
<td>a. echaf, ech ‘dog’</td>
<td>SH c:wa ‘dog’</td>
</tr>
<tr>
<td></td>
<td>K cist:h ‘dog’</td>
</tr>
<tr>
<td>b. ech'uc ‘bird’</td>
<td>SH c:e:ca:x ‘bird’</td>
</tr>
<tr>
<td></td>
<td>K aevi:v ‘bird’</td>
</tr>
<tr>
<td></td>
<td>*c:iva (McCleland 1964:129)</td>
</tr>
<tr>
<td>c. sra:e ‘N. tanned skin, quiver’</td>
<td>EP f:e ‘rabbit-skin blanket’</td>
</tr>
</tbody>
</table>

11 Though it is always possible that the word was first borrowed into both languages, with an ejective in Yurok and without one in Wiyot. However, where sound correspondences between the two languages appear to be regular, I disregard this possibility for the moment.

12 Yurok alveopalatal affricates /c/, ech are represented as in other languages in (31-32) to assist in comparison. For Chimariko, I do not currently have access to Edward Sapir or John P. Harrington’s field notes, in which ejectives are transcribed; forms included are from Dixon (1910) which does not transcribe ejectives.
(32) Penutian resemblances

Abbreviations and data sources: Kl. Klamath (Barker 1963); Ph Hill Putwin; W Wintu; Phr Hill Patwin and River Putwin (Broadbent and Pitkin, 1964).

Yurok | Resemblant forms
--- | ---
a. c'ek ‘wren’ | Kl. c'ik:as, c'ik:/ - ‘bird’
 | Kl. c'ikdo ‘marsh hawk’
 | Kl. c'iq-t:i ‘tule wren’
 | Kl. c'eqs ‘Brewer’s blackbird’

b. ćewoloh ‘to wash by squeezing in water’
Ph c'ewyño ‘soup paddle’
KL. c'iwlo/ ‘puts liquid on top of’
KL. c'iw'ala ‘puts liquid on the end of’
KL. c'iwlo:la ‘takes liquid off the top of’

c. wayc'ok(ŋ) ‘puppy’ (DIM of *wayc'ok)
KL. wac'ai: ‘dog’ /wac'-a:k/
d. k'oks. k'o: ‘crow’
KL. që: ‘magpie, Pica melanoleus hudsonica’
e. t'eto:iya ‘to quiver, scuffle’
W t'or ‘to rattle’, t'ete:i ‘to shiver’
KL. t'iw ‘to shiver from palsy or old age’
KL. t'iw'tilla ‘shivers, trembles’
f. t'ol ‘N, mud, swampy ground’
KL. t’o- ‘mud’
KL. t'oba:q ‘mud’
KL. t'opa ‘mud, a muddy place’
KL. t'oba: ‘mudhen’
Ph. ñ'opa ‘mud’
g. t'keroh(ŋ) ‘to strike with the nail of the index finger, to thump’
KL. t'aka: ‘to pound’
Ph t'ake ‘to pound’
Ph t'aka ‘to slap’
h. ket'oks. ketoks ‘to be flat’
KL. t'akei ‘palm of hand’
KL. t'aqt'aqant ‘in the flat. where it’s flat, bare’
i. ket'etew ‘palm of hand’
KL. t'akei ‘palm of hand’
KL. t'aga: ‘sole of the foot’
j. kot'à ‘N, bald person’
KL. t’aq: ‘be bare, bald, grassless’
KL. t'aqt'aqli ‘bare, smooth, grassless’
KL. staqtew:lak ‘bald’

Sound symbolism involving word-initial /t'/ with possible reduplication may be a feature borrowed from Penutian (32e-i above).\(^{13}\)

In sum, there is a suggestion here that most if not all cases of word-initial /c'/ and /t'/ in Yurok are the result of borrowing.

\(^{13}\) Callaghan (1964:47, fn.6) lists four roots in Lake Miwok with potentially similar origins: ć'aka-ti ‘to slap’; t'ip-je:tsuka ‘to click’; t'ip-tipi:si ‘to flip (said of a fish)’, t'ip-je:tsi ‘a short piece’ (cf. Bodega Miwok t'ip-kutj ‘to break off’, possibly associated with the sound of breaking something off). And, compare Karok ti:pi:t ‘chain fern, Woodwardia radicans: said to be named from the noise made in a game involving it’.
REFERENCES

BERMAN, HOWARD. 1990. New Algonquian-Ritwan cognate sets. IJAL 56.431-34.
APPENDIX I

Yurok Ti clusters. T an obstruent (stems with Ti are not multiply listed)

Where obvious, morpheme boundaries are marked with ‘-’;
reduplicative boundaries are marked with ‘+’.
asterisked forms look like exceptions to the sound change in (3);
other forms are assumed to have derived clusters which post-date the change in (3).

*hok-k- ‘V, to butcher, to cut up’ (cf. hok-k- ‘to chip arrowheads’) [B:200]
*hok*~toks ‘N, small basket used as drinking cup’ (cf. hek*~c ‘eating basket’)
kep-ten- ‘V, to sing a lullaby’ (cf. kep*~t ‘lullaby’) [B:201] (cf. ten ‘much, many’, wo?kegep ‘cricket’ with
noninflected intensive form kegep)
*kep-toh ‘N, horse neck. Washington clam’
kik*~ten- ‘N, moss. rotten wood’ (cf. kik*~sah ‘N, apron of bark’, ten ‘much, many’)
*leke*~temel(-) ‘V, to dig’ (cf. rek*~cem- ‘V, to hollow out’)
*lepeten-ok ‘N, cloud’ (cf. WIY laptolhr ‘cloud’)
*nek*~ta? ‘N, stump’ (cf. na?ka?skat ‘N, hip’)
nek-ey ‘V, to admire the cleverness of a person’ (cf. nekomy ‘ability’)
*nekto? ‘V, to collide, bump into’ [B:209] (cf. nekcen- ‘V, to meet’)
puk-tek ‘N, deer’ [B:212] (cf. pu:k ‘deer’)
swe-(h)okit-elu~wey ‘V, to be warty’ (compound with hokik- as 2nd member)
si/tey ‘V, to be light brown’ (cf. se-wom ‘brown acorn’)
tek-tekon ‘V, to be sticky’ (cf. Karok jis-takan ‘to stick, adhere’)
tek-tes- ‘V, to be angry’
tek-ets- ‘V, to build, erect’ [B:217]
tek-ey ‘N, stomach’ [B:217]
tek-(toy-) ‘V, to be situated, to grow (plants)’
tek*~temeh(s-) ‘V, to jab repeatedly’ (cf. temeh(s-) ‘to jab, pierce’) [B:218]
tok+hoh ‘V, to be of a certain size, of a round object’ [B:218] Robins (1958:257) identifies tok- as a
dependent prefix.
ton+tok ‘N, mallard’ [B:218] (from English ‘duck’ ?)
tok*~tam ‘N, dentalium shell’ (Robins 1958:259 notes this is a plural bound form.) (cf. 12a)
tok*~tap ‘N, ax’ (diminutive NOM of tekas- ‘to cut, to fell’)
tu:p~tu:p ‘N, swordfern’
wik-tu? ‘ADV, inside’ (cf. wii ‘ADV, there’)

15
APPENDIX II

Yurok ejectives /c/, /k/, /kʷ/, /p/, /t/ which are not the obvious result of inflectional morphology

/c'/
i. initial
  c'ek ‘N. wren’ (20a)
  c’ep’gi? ‘N. black oak’ (footnote 8)
  c’igol ‘N. saliva, foam’ (cf. W1Y bicol ‘N. saliva’)
  c’ijah ‘N. dog’ (20c)
  c’t:yo? ‘N. locust’ (20d)
  c’uc’if ‘N. bird’ (20d)
  c’uwa? ‘N. coat’ (cf. nvegh ‘coon, raccoon’)
  c’woloh ‘to wash by squeezing in water’ (20b)
  c’tund?’ ‘ADV. how!, extremely’ (cf. tu? ‘ADV and, but’)

ii. medial
  c’uc’if ‘N. bird’ (20d)
  novrep’c’oy ‘noninfl. V. to weed’ (21a)
  seyep’c ‘-o-trans V. to singe’ (21b)
  wxe’ok’s ‘N. puppy’ (cf. tekhet- ‘to be a biter [of a dog]’)

iii. final
  k’a:c, k’a:y ‘crow’ (footnote 8)
  sra:c ‘N. tanned skin, quiver’ (cf. srahk’oh ‘loincloth’; srek’ahpi:t ‘breechcloth’. -slek ‘clothes’)
?ek'ero' ‘N. lamp; noninf. V to light’
?uk'iru' ‘N. salmonberry juice’

iii. final
-1k’; ek’ ‘CLASS' suffix, long thin things (worms, ropes, etc.) (also -1k”)
cek’cek’ ‘N. small brown bird with sharp nose’ [Kr] (cf. tok’tok’ ‘N. mallard’)
megi:gonmek’ ‘e-class trans. V, to poke a fire’
na:n:k’ ‘N, ring of black and white dois round the bottom of a basket’
stohselek’ ‘small fir tree’
weno:romeke’ ‘N, point at which another rib is added in a basket frame’

/k”/
. initial (unattested)
. medial
cek’"el’ ‘N, flat place in front of a cliff’ [B:198], see also cik’ar ‘N, chair, stool’
-cnewk”ox ‘inalien. N, son-in-law’
corek’"ik ‘N, hell’
cyu:k”en- ‘e-class intr. V, to sit’ (see also cyu:k”ec- 2nd o-class trans. V, to steer [a boat])
hak’"oh ‘N, rabbit’ (Sc)
kenik”ec- ‘V, to steer (a boat)’ (cf. cyu:k”ec- 2nd o-class trans. V, to steer [a boat])
kemok’"oyS ‘V, to control water, to turn off’ (cf. keromek’in ‘to twist, lock’)
lek’"oh ‘to drag oneself along, to crawl’ [B:204]
mek’"oh ‘N, pus’
nek’"oht- ‘e-class intr. V, to dry surf fish’
no:loyk”el- ‘e-class intr. V, to be feeble, to be weak’
rek’"oy ‘river mouth, Requa’ (cf. ri:k’ew ‘N, shore, sandbar’)
shu:k”oh ‘N, space at the foot of the sweathouse ladder’
srek’"ahpit’ ‘N, breechcloth’ (cf. srek’epiti’ ‘diaper’)
sru:k’"i ‘N, columbine flower’
syuk’"ac- ‘V, to stretch one’s legs’
tkek’"el, tkek’"er ‘N, pot’

. final
-Vk’” classifier suffix (long, thin things)

/p”/
. initial (unattested)
. medial
ckip’ol ‘N, a plant (sp. ?snowdrop)’
ckep’orit- ‘e-class intr. V, to be brushy’ (cf. ka:p’el ‘brushwood’, ka:p’olit- ‘to be brushy’, tkwep’olit- ‘V, there is low scrub’, cewitkep’- ‘to clear an area’ [B:198], slowkep’- ‘to sweep clean’ [B:215])
curp’yo ‘noninflected V, to comb’ (cf. curp’yo ‘N, comb’)
hop’ew ‘N, (site of) Klamath’
hop’iff’ ‘e-class intr. V, to have in itch’
hap’o:n’4p, inalien. noun, nose’ (cf. ?up’o:n 3p pron. prefix form)
 hop’o: ‘N, Hupa’ (cf. hup’o:la’, hup’o:la: ‘N, Hupa Indian’)
 kep’ey ‘V, to be deaf’
 kep’el ‘N, hole in a tree, hole in the ground, house pit’ (cf. kep’oks ‘N, opening’; kep’ol ‘a hollow, a clearing, a space’)
 kep’oh ‘V, to feed an infant soft food’
kle:up’oh ‘N, clover’
lep’ew ‘V, to tidy up’ (cf. legep’ew ‘N, storage place’; rep’of- ‘V, to clear land’)
tmegep’or ‘N, rattlesnake’
-lep’el inalien. N, clack’
mepe’oc ‘N, beard, mustache, whiskers’
nowrep’o: ‘noninf. V, to weed’ (21a)
rap’kah ‘noninf. pl. V, to gang up on’
skep’ol ‘N, wyethia angustifolia’
sop’ep’- ‘to arrange’
wo:p'i, wo:p'lk 'ADV out in the water (of an island)'
?ahkecoup'el 'N, mass of briar bushes' (cf. ?ahkecoup 'N, thorn, prickle')
?ep'e'l 'CONJ if; ADV should'
?up'es 'backbone of fish (taken out and dried)'
?up'ip'h 'N, backbone'

iii. final
coko:p'p 'drum' (cf. coko:p'a?r 'N, drumstick; noninflected V, to drum')
eg-p 'NUM suffix for growing things' (cf. co:p 'NUM, 2nd element in compound NUMs, growing things'; welowoyp 'NUM, ten (growing things)'). Vowel assimilates to preceding vowel in: ka:p'p 'N, leaf, leaves, greencery, brush, grass' [B:201] (cf. ka:p'el 'N, brush, vegetation'); me?y'kelu:p 'N, poison oak'; na?phya:p 'N, huckleberry bush'; pa?apa 'N, ladyfern (dyed in alder juice and used in basket making)'; pya:p 'N, manzanita tree' (cf. pya'h 'N, manzanita berry'); wa?hu:p 'N, Indian greens' (cf. se?repcu:p 'N, shavings', ?ahkecoup 'N, thorn, prickle') (see section 2).
tho:p 'N, slime'
mo?alap 'nasal mucus' (cf. ?skayop')
retip 'noninfl. V, to be concentrated in a given direction, of a wish or a prayer; to be tossed in a given direction in the stick game; to point in a given direction, of a person'
scep 'V, to land, of a boat' [B:214]
stekeop 'noninfl. imp. V, to reach a crest of high water, to reach the highest point of the tide'
two:p 'N, butterfly' [B:220]
-?ep 'N, eyebrow' [B:222]
?skayop 'noninfl. V, to flow slowly, of something thicker than water (e.g. blood, slime)'

l't'
i. initial
t'el'oiyah 'noninfl. V, to quiver, to scuffle'
t'keroh(s) 'V, to strike with the nail of the index finger, to thump'; (23a)
t'levold, levold 'to fall (water)', t'la?la? 'to drip' (25)
t'ot'el 'N, sparks from a fire'
t'olt'el 'N, mud, swampy ground', t'olt'el-t 'noninfl. V, to be muddy' (23b)
t'ro?el 'N, an edible plant'
t'p'el(k) 'V, to be sensible, to come to one's senses'
t'weyken 'V, to be straight'
t'we?innwenoh 'V, to (try to) pry up repeatedly'
t'woodk' 'V, to be slippery' (23b)
t'unmin 'V, to be soft, to be tender, to be rotten'
t'unon 'V, to grow (of plants)'

ii. medial
kat?en'em 'N, basket for valuables'
kep'toh 'noninfl. V, to feed an infant soft food'
ket'etew 'N, palm of hand' [B:202] [4]
kel'oks, ketoks 'noninfl. V, to be flat' (cf. ketoks 'N, a level place', kat'oa 'N, bald person')
ket'ey 'noninfl. V, to park, to moor'
kel'oh 'noninfl. imp. V, there is a lagoon' (cf. ket'ul 'e-class intr. V, to form a pool, to form a lake' tewol, tewolew 'Pacific Ocean'; ket'op 'to be in a pot (food), to be barked (shins)')
kitwa?ot 'ADV, very, excessively'
kotuskin 'CONJ however, whatever'
t'at'a 'V, to pass gas through the vagina' (cf. t'et, -yk'et 'penis', t'atok's 'to spring back, to be supple')
nok'pet?ek's 'N, bug'

iii. final (unattested)
PROBLEMS OF WRITING A HISTORICAL GRAMMAR

CATHERINE A. CALLAGHAN

1. INTRODUCTION. For a number of years, I have been revising my Lake Miwok Grammar (Callaghan 1963), which was my Ph.D. dissertation. Specifically, I have been checking my synchronic claims and incorporating a historical dimension. This dimension was not possible initially, since no body of Proto-Miwok and Proto-Utian was then available. My model is, in part, Joseph Wright’s Grammar of the Gothic Language (Wright 1954), which first impressed me when I was a graduate student. Specifically, classes of nouns and verbs were presented synchronically, although they were labeled historically.

1.1. DEDICATION. The dedication is important. In my case, I would like it to read as follows: “To My Mentor, Mary R. Haas, and my principal consultants, Alma Grace, James Knight, and John Knight. Also, to Murray B. Emeneau and the late Madison S. Beeler, who taught me Indo-European, and to Yakov Malkiel, who taught me to love etymologies.” The dedication should be followed by a list of abbreviations for grammatical terms, such as abl. “ablative case” and smf. “semelfactive.” I have followed this with a list of abbreviations for individual languages, such as Ceb “Chocheño” (East Bay Costanoan), and a list of special expressions.

1.2. INTRODUCTION TO THE GRAMMAR. I have included a statement of the classification of Miwok languages, the aboriginal boundaries of Lake Miwok territory (with a map), a section on aboriginal culture, including oral literature, and finally, a summary of Lake Miwok history since white contact. The last is very important. The public should know in detail the cavalier mistreatment of Native Americans. I conclude with a history of Lake Miwok scholarship and further acknowledgement of gratitude to my consultants.

2. CHAPTER ONE: SYNCHRONIC PHONOLOGY. In this section, I present a traditional statement of Lake Miwok phonemes, junctures, intonation patterns, morphophonemes, and morphophonemic rules. It is close to my dissertation, except that I have checked all examples and extirpated any analysis motivated by “Item and Arrangement.” It is imperative for us to produce user-friendly grammars accessible to all linguists, not only those living now, but also future generations when current theories will be superseded. This does not mean that I see no value to a theoretical presentation, but I believe that it should be relegated to a separate part of the grammar, or otherwise kept apart from the “user-friendly” analysis I am advocating. This procedure is especially important in the case of endangered languages, where the grammar may become the primary source of information (see Langdon 1996).

3. CHAPTER TWO: HISTORICAL PHONOLOGY. The historical sections of my grammar trace Lake Miwok back to Proto-Miwok primarily, with reference to Proto-Utian. Proto-Miwok (and Proto-Utian) had a single series of stops, and Lake Miwok has three or four series, depending upon the status of aspirated stops. In previous papers (Callaghan 1964, 1987), I have discussed how the non-plain stops, /t/, and /ʃ/ came into the language through massive borrowing from neighboring languages. Once in the language, they became “naturalized,” which is to say, incorporated into some native Miwok words.

3.1. LOAN WORDS. In Chapter Two, I enumerate probable and possible loan words, subgrouping them by the presumed language of origin. In most cases, probable loan words incorporate “aberrant” phonemes into words that cannot be reconstructed to deeper levels (Proto-Western Miwok or Proto-Miwok), and they resemble words with the same or similar meanings in neighboring languages. Possible loan words also resemble words in neighboring languages and lack Miwok cognates, but they also lack aberrant phonemes or other means of determining the probable direction of borrowing. Examples now follow:
LAKE MIWOK  |  ENGLISH  |  PATWIN  |  ENGLISH
---|---|---|---
c'udc'udu, c'udc'udu  |  sideways  |  Wph č₂t'určuDU  |  to turn up
č'iwši  |  hail  |  Wph č₂iwsin  |  hail
k'ǔpum  |  finger  |  Wph k'upum  |  finger

The first example illustrates borrowing of a polymorphemic stem. The second underwent a sound change, loss of /n/ in pre-Lake Miwok words of the canon CVCCA, and the third involves borrowing of a common body part term, probably through intermarriage. A striking feature is the preponderance of loan words from Patwin, more than from all the other languages combined.

This phenomenon is consistent with the slow encroachment of culturally dominant Patwin speakers into Lake Miwok territory. Whistler (1977) argues for Patwin intrusion into the lower Sacramento Valley and adjacent foothill on the basis of Patwin plant terms of Miwok provenience. I have found additional possible evidence supporting Whistler’s hypothesis in the form of a possible Miwok substrate for current Patwin place names. An example is Patwin Liwai-to ‘people on Putah Creek at the foothills’ (Barrett 1908:294), which incorporates the Miwok allative case suffix -to. Moreover, liwa means ‘deep (water)’ in Lake Miwok and ‘water’ in Bodega Miwok.

It is imperative that this type of research be undertaken in advance of, or at least concurrently with the reconstruction of Proto-languages. Disaster would ensue if loan words from French were not excluded from comparison of English with other Germanic languages.

3.2. THE DEVELOPMENT OF PROTO-MIWOK PHONEMES AND RULES. In this section, I sort the Lake Miwok and Bodega Miwok reflexes of Proto-Miwok (and Proto-Western Miwok) words by individual sound developments, starting with identifications, i.e. PMi *kɔːl ‘smoke from fire’ > Mib, Mil ká-l ‘smoke from fire’, PMi *ʔɔː-ni ‘to come’ > Mil ʔo⁻ni ‘to come’, and PMi *mi-w ‘person’ > Mil mi-w ‘husband’. Glottalization became associated with small, quick, often semi-accidental action in some Lake Miwok reflexes, hence PMie *cituk-nu- ‘to wink at’, Mil č’it-ak ‘to wink’. The following sets illustrate two developments; PMi *y [i] > Miw u in most environments, and PMi *t(e) > PMiw *č(e) / á, ő /V.

<table>
<thead>
<tr>
<th>ENGLISH</th>
<th>BODEGA MIWOK</th>
<th>LAKE MIWOK</th>
</tr>
</thead>
<tbody>
<tr>
<td>PMi *kata-y</td>
<td>hard, tough</td>
<td>káć-u</td>
</tr>
<tr>
<td>PMi *kyl-a, *ky-la</td>
<td>liver</td>
<td>kůl-a</td>
</tr>
<tr>
<td>PMi *pat-i</td>
<td>wrap around</td>
<td>pác-i ‘rinse’</td>
</tr>
<tr>
<td>PMi *pyne</td>
<td>packed lunch</td>
<td>půne</td>
</tr>
</tbody>
</table>

A detailed discussion of sound developments, combined with lists of reflexes, provides a solid reference base for further analysis. The section ends with a discussion of morphophonemes and morphophonemic rules that can be reconstructed to Proto-Miwok. One of them, //CVV/CV(C)// > /CVV- CV(C)// (Morphophonemic Lengthening), applies with minor variations throughout much of Utian. Recognition of this rule greatly simplifies the Miwok ablaut system. It entails indicating all the phonemic vowels, including /y/. Failure to do so results in an unnecessary proliferation of stem types and epenthesis rules.

Other things being equal, when presented with alternate analyses, both phonological and morphological, I chose the one which best accords with history. This advice came from Mary R. Haas, and after many false starts on my part, I find that it usually leads to the most elegant solution.

4. CHAPTER THREE: INTRODUCTION TO MORPHOLOGY. Basic terms should be defined in this section in terms of the language in question, with a minimum of examples, since longer lists will
follow in the morphology section. In the case of Lake Miwok, morphemes are of two types, roots and affixes. Roots are monomorphemic and affixes are either suffixes or possessive pronominal prefixes. The latter could be analyzed as proclitics, since they sometimes precede clauses. Examples are /Ríti-háju/ ‘his dog’, where /j/ is [y] and tájh kás-ac hél-a ʔúte ʔítíʔóp-ójhintec ‘she had never seen a man do that kind of thing anywhere she had traveled’, where ʔítí-ʔóp-ój-hinte-c (her-travel-where-obj.) means ‘anywhere she had traveled’.

I proceed to define terms relating to nouns, pronouns, adjectives, verbs, particles, and exclamations. I conclude with a short section on submorphemic sequences, such as /...p/, occurring in mólpa ‘cloud’, micpa ‘mist’, and ʔú-pa ‘rain’. Mrs. Grace volunteered that it might pertain to the heavens.

5. Chapter Four: Synchronic Morphology. After much thought, I have decided to present the synchronic morphology first, even though the classification of the Lake Miwok stem types is historical, again like Wright’s classification in Grammar of the Gothic Language. I differ from Wright’s approach, in that he interlarded his historical statements with the synchronic. I have acted under the advice of Margaret Langdon, who believed that alternate approaches would be too confusing. My goal is to make an organized, understandable, jargon-free presentation of the data, so that readers can reorganize it according to contemporary theories or use it for other purposes.

5.1. Nouns and Pronouns. A strictly phonological definition of the word is difficult if not impossible, since unstressed elements tend to cluster phonologically with the preceding word. Possessive pronominal prefixes immediately precede the noun. They distinguish three numbers, with remnants of an inclusive/exclusive contrast in the first person dual. They also distinguish three persons plus an indefinite. The third person is represented by a reflexive prefix contrasting with a non-reflexive prefix. There is no gender distinction, but immediate versus remote reference are marked. Examples follow:

(1) hanaháju ʔúte
   his-own dog sees
   He sees his own dog.

(2) ʔítiháju ʔúte
   his-dog sees
   He sees his (somebody else’s) dog.

I discuss the derivational noun suffixes, including a semi-optional dual and plural, and the distribution of allomorphs for the nine cases. Pronouns lack prefixes and have no vocative case. I conclude the section with numerous elicited paradigms. When possible, my examples come from complete elicited sentences or texts.

5.2. Verbs. Subjective pronominal clitics usually precede the verb. Unlike possessive prefixes, some of these proclitics have weak allomorphs which always cluster with the previous word. Hence háju-n ʔúte? (dog-you sg. see): ‘Did you see the dog?’ Considering possessive elements to be prefixes and subjective elements to be proclitics accords with history as I have reconstructed it. In Proto-Utian, and probably Proto-Miwok, subjective pronominal clitics usually occupied word-second position in a sentence, which means that they preceded the verb if there was a nominal subject. Otherwise, they followed the verb.

In the original version of my grammar, I was forced to set up numerous synchronic classes of verbal stem types. These classes were greatly reduced through reconstruction of the Proto-Miwok ablaut system (Callaghan 1986). This ablaut system is opaque unless one recognizes two morphophonemic rules for Miwok languages, one being the previously mentioned lengthening rule (3.2). The other is V > Ø_V, where V represents any vowel.
I classify Lake Miwok stems and grades historically wherever possible; hence Mil múlak < PMi *mulak ‘to wash the face’ is a Light Stem, but Mil múlut < PMi *mul-tu ‘to eat breakfast’ is a Cluster Stem. Mil pikuc < PU *pek-tu ‘to dream’ is also a Cluster Stem. Mil ?ú-kuc < PU *pe-kut ‘to dream repeatedly’ is a Lengthened Grade of that same cluster stem.

This section is followed by a discussion of verbal suffixes. These suffixes are sometimes strung together in a specific relative order.

6. CHAPTER FIVE: HISTORICAL MORPHOLOGY. Stems can be monosyllabic, disyllabic, or trisyllabic. I list all the Proto-Miwok monosyllabic and trisyllabic stems having Lake or Bodega Miwok reflexes.

6.1. NOMINAL MORPHOLOGY. In this section, I discuss how the system of Proto-Utian possessive pronominal prefixes developed into the Lake Miwok system. I trace Proto-Miwok derivational noun suffixes into Western Miwok, as well as the Proto-Miwok case system. Both the Lake Miwok nominative and possessive cases derived from an old genitive. The Lake Miwok objective, ablative, instrumental, locative, allative and vocative cases can be traced to Proto-Miwok, although the ablative ultimately derived from the locative case. The provenience of the Lake Miwok comitative case is unknown.

6.2. STEM TYPES AND ABLAUT GRADES. The Proto-Miwok ablaut system involves disyllabic stems only. I have discussed this system elsewhere (Callaghan 1986), as well as its development in Lake Miwok (Callaghan 1992). In this section of my grammar, I list and discuss all Proto-Miwok stems and grades with Western Miwok reflexes.

6.3. SIERRA MIWOK STEM TABLES. These tables (Freeland 1951:94-95, Broadbent 1964:38) present the commonest stem alternations in the surface structures of Central Sierra Miwok and Southern Sierra Miwok respectively. Especially in the case of Broadbent (1964), they are based on two contradictory premises: analysis should involve surface structure only, and it is desirable to omit a phonemic vowel if one can predict its occurrence. The result is an unnecessarily complex proliferation of stem types and the conviction that Sierra Miwok exhibits widespread eponymy and elaborate template structure. I have addressed this problem in Callaghan (2000). In my grammar, I will include a concordance of Freeland's system with my own.

6.4. DERIVATIONAL VERB SUFFIXES. Several of these reconstruct to Proto-Miwok and Proto-Utian, including the reflexive, reciprocal, and causative. There were two Proto-Miwok particles marking tense, *ka: ~ *ka ‘past tense’ and *ma ‘perfect tense’. These remained particles in Western Miwok, but were incorporated as suffixes into Eastern Miwok verbs.

6.5. INFICTIONAL VERB SUFFIXES. No person markers can be reconstructed for Proto-Miwok and Proto-Utian verbs except in the imperative (Callaghan 1998). Lake Miwok reflexes of this system have undergone much simplification.

7. CHAPTER SIX: PARTICLES, EXCLAMATIONS, AND SYNTAX. Neither particles nor exclamations can take affixes. Particles sometimes form compounds, but exclamations never do. Exclamations stand apart from sentences, while particles can be subclassified according to their position within a clause. For example, temporal particles usually occur at the beginning or end of clauses, as in hojtot ?itt ka ʔute (ago him 1 see): ‘I saw him quite a while ago.’ Where possible, I trace the provenience of particles and exclamations.

7.1. SYNTAX. There is much overlap between morphology and syntax, since case suffixes can follow noun phrases or clauses as well as nouns, as in jolümi-c ka-ʔínu he ka-ʔap-i-c weliksínen (food-obj. my-mother and my-father-obj. get): ‘Get my mother and my father some food!’ In this sentence, -c ‘objective case’ follows the phrase ka-
únu he ka-áp:i 'my mother and my father'. Sentences tend to be verb-final. Words following the verb acquire emphasis. Nominal subjects usually precede nominal objects. In Proto-Miwok, the verb probably tended to be in final position.

REFERENCES


THE HISTORY OF THE TERM “PENUTIAN”

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In this paper I do not directly address the validity, nature or scope of the Penutian linguistic relationship. Instead, I take the reader on a short onomastic expedition, and ask: Where did the term “Penutian” come from? What has it been used to designate in the past? What does it designate now? And where (if only by implication) might the term go in the future? This may seem like a history of the concept in a nominalist guise, and perhaps it is, but I intend to approach the topic more in the spirit of a librarian or archivist than as a historian of ideas.

1. WHERE DID THE NAME COME FROM? Most linguists know that Dixon and Kroeber invented “Penutian” to label one of the three super-Powellian units of their 1913 reclassification of California Indian languages. ¹ As they put it in 1919:

   It was necessary to find names for the new groups or families. To extend the designation of one member of each group to the entire group would have been as misleading, in the end, as it would have been to name the tongues of Europe “Sanskritian.” Binary designations of the type of Indo-Germanic, Ural-Altaic, and Uto-Aztekian were likely to prove unrepresentative, and certain to be clumsy. There seemed no recourse, accordingly, but to new and therefore arbitrary designations of the types of “Semitic”; and genealogical as well as national appellations being wanting in the native Californian field, and none of a geographical character applicable, the family names proposed were based on forms of the numeral “two” in the families of speech involved (Dixon and Kroeber 1919:54).

   The family that consisted of “the Wintun, Maidu, Yokuts, Miwok, and Costanoan families as previously recognized” was dubbed PEN-UTI-AN by generalizing the forms in the two resemblant sets for “two” in their data:

   #152 Wintuan pale-t, pampa-ta; Maidu pene, pen; Yokuts punoi, ponoi, pongoi, and pungi
   (= PEN)

   #153 Miwok oti-ko, oyo-ke, ossa, otta; Costanoan utsi-n, uti-n, uthi-n, utxi-n, and uti-s (= UTI)

2. PENUTIAN AS A CALIFORNIA FAMILY. Kroeber, the coiner of the term, was reluctant for many years to use “Penutian” to refer to any genetic group beyond the five California families. In this, he diverged from Sapir (as we shall see below) as early as 1915. After the publication of Sapir’s “superstock” classification in 1921, in which “Penutian” was used to label a much larger group, Kroeber’s narrow usage became stubbornly idiosyncratic. Thus, we find him using “Penutian” in the strictly California sense throughout his California Handbook (1925); in Cultural and Natural Areas of Native North America (1939); and in the second edition of his general anthropology textbook (1948). None of these works even refer to Sapir’s or any other linguist’s broader application of “Penutian”.

   This terminological conservatism reflected Kroeber’s deep skepticism about the validity of phylum-level genetic classification. This was expressed most strongly in a paper published in 1940:

   The inspectional method has pushed certain relationships into the realm of very high probability...In this category...are the Hokan and Penutian groupings of Dixon and myself. [But] as soon...as the closely

¹ Although I follow scholarly usage and refer to the coinage as a joint effort by the two co-authors, there is every reason to believe that Kroeber was the primary if not the sole creator of the three new family names.
contiguous California Penutian languages are left behind, and one compares them with, say, Kus in Oregon, the inspectional method begins to leave us in the lurch...A step farther to Chinook...and inspectional resemblances have disappeared altogether ....Essentially what Sapir is doing when he connects HOKan and Siouan, or Chinook and Penutian, is to perceive structural resemblances (Kroeber 1940: 465-6). 2

In passing, Kroeber remarks on the “coined syncopated words” like “Hokaltecian” or “Otomanguean” that were then coming into fashion. He hopes that

the unvary cultural anthropologists will not bandy these shiny new counters about as if they were validated coin of the realm instead of merely provisional tokens whose right to currency remains to be determined by their general acceptance....I have myself had a hand in fathering “Penutian,” which is an equally arbitrary compound. Many of the new compounds, however, represent merely an opinion, and they are being introduced in considerable number, bearing a form similar to that of names of proved families (Kroeber 1940: 470).

If (in Kroeber’s view at least) the strictly California Penutian relationship had “very high probability,” the term “Penutian” so applied was intellectual coinage of proven value. By implication, the use of the term, by Sapir and others, for a “phylum” of heterogeneous languages scattered from Alaska to Honduras (if not further) could only debase it.

In the decade or so following the Second World War, “Penutian” was used in the narrow Kroeberian sense by a number of linguists and anthropologists associated with Kroeber or with Berkeley. Most significantly, the linguists associated with our very own Survey of California (and Other) Indian Languages followed Kroeber’s usage well into the early 1960s, at least in their published work. Thus, Harvey Pitkin and Bill Shipley begin their “Comparative Survey of California Penutian” with the statement:

In the discussion which follows, the term Penutian will be used to designate the languages of California to which Dixon and Kroeber originally applied the name (Pitkin and Shipley 1958:174).

Bill Shipley introduces his Maidu Grammar (1964) by stating that:

Maidu is a Penutian family, clearly (though distantly) related to Yokuts, Wintun, the Miwok languages, and Costanoan (Shipley 1964:2).

In a variation on the theme, Sydney Lamb in 1959 dubbed the California Penutian relationship “Pen-Uti”. (Lamb proposed a rationalized nomenclature for North American genetic relationships in which all family-level units were to be named with a binary term on the “Indo-Germanic” model. Plateau Penutian was “Klamath-Sahaptian”; Oregon Penutian was “Coos-Takelman”. The California languages, by similar logic, constituted the “Pen-Uti” family.)

But Kroeber’s mind was never closed, and in the 1950s, the last decade of his long and productive life, he revisited the question of deep genetic classification. Partly owing to a strong interest in Morris Swadesh’s glottochronological work, Kroeber came at last to accept the possibility that “extra-California” Penutian connections could be proven. And with this came, finally, a shift in his terminology: the California relationship is now “California Penutian”, while “Penutian” is something larger. This shift is seen very clearly in the following passage from one of his last publications on California ethnography, an essay on “Recent Ethnic Spreads” (1959). After referring to his treatment of the “Penutian Family” in his 1925 Handbook, he continues:

We do not know where the California Penutian languages originated. They seem more similar to one another than to any extra-California languages recognized as Penutian, but....it remains possible that

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2 Kroeber expressed many of the same reservations, but without direct reference to Sapir, in his presidential address to the LSA, published in 1941.
California Penutian ...split off from other, non-California Penutian elsewhere than California.... But the determination... cannot even be attempted except within the full context of the entire Penutian group (1959:276-77).

As Penutian specialists, especially Bill Shipley, came to adopt the same attitude, so too did their usage change. After the mid-1960s the California languages were almost universally referred to as “California Penutian,” and “Penutian” ceased to be used by linguists to designate solely the California Penutian group. (The only exception I can think of is Otto von Sadovszky, who hypothesizes an improbable relationship between the Ob-Ugric branch of Uralic and the Penutian languages of Central California, with no claim that any other languages called “Penutian” are involved [Sadovszky 1996].)

3. PENUTIAN BEYOND CALIFORNIA. The first, and most successful extension of “Penutian” to a wider relationship was made by Sapir. This usage shows up in correspondence between him and Kroeber as early as 1915, only two years after Dixon and Kroeber’s first announcement, although it did not reach print until 1920.3

In a letter dated April 21, 1915, Sapir tells Kroeber that he is enclosing “145 resemblances between Coos and Takelma on the one hand and Penutian on the other,” using “Penutian” in the California sense. But then he proposes to Kroeber that “we could collaborate on a paper which would show that Takelma and Coos are members of the Penutian stock,” implying that the California term should be used for an expanded relationship. But in a postscript, Sapir appears to have misgivings about the term:

P.P.S. I don’t like ‘Penutian’. In view of Cost[ano]n ama, Yokuts mai, Maidu mai-, Coos mi, I would suggest “Mai” as stock name. Bother-an! Mai stock would be good enough.

One might suspect a little donnish humor here, were it not that—as we will see below—Sapir continued to use this label for at least several months, apparently in all seriousness.

In his somewhat delayed reply (May 29, 1915), Kroeber appears to buy into a possible extension of the term, thanking Sapir for his “evidence as to new ... Penutian affinities”, but he reverts to the narrower sense when he tells Sapir that other commitments prevent him from collaborating in the near future “on a paper connecting Coos and Takelma with Penutian.”

By the end of the year, Sapir’s comparative work had led him to dramatic and far-reaching conclusions, which he summarized to Kroeber in a bubbling letter dated December 9, 1915. However, this letter shows Sapir still reluctant to extend “Penutian” and searching for an alternative. He begins, equivocally, by telling Kroeber that

You may...quote me as maintaining that your Penutian stock is continued in Oregon by Takelma, Coos, and Lower Umpqua (Siuslaw).

But then makes it clear that “Penutian” should mean (if anything) the California relationship:

I strongly surmise that Coos, Siuslaw, and Alsea form a fairly close unit as contrasted with Takelma and with Penutian proper.... However...we do not even know if Penutian, as first defined, is really a unit. Takelma may turn out to be coordinate with say Yokuts, not Penutian as such.

In proposing even wider connections he allows himself to use the term loosely:

I now believe this enlarged Penutian stock to travel still further north and to include...Chinook!.... And...I think Tsimshian is the most northern outlying member of the stock.

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3 Quotations from Sapir’s and Kroeber’s letters are from Golla (1984).
However, at the end of the letter, in diagramming the relationships that he sees emerging, he uses “Mai” as the overall name for the stock and restricts “Penutian” to its original California scope:

<table>
<thead>
<tr>
<th>MAI</th>
<th>Penutian</th>
<th>Takelma</th>
<th>W. Oregon</th>
<th>Chinook</th>
<th>Tsimshian</th>
</tr>
</thead>
</table>

None of this discussion reached print at the time. The first published allusion to wider “Penutian” relationships appeared in 1918, and came not from Sapir but from Leo Frachtenberg, a linguist working on Oregon languages for the Bureau of American Ethnology. In the second number of *IJAL* Frachtenberg published a paper on “Comparative Studies in Takelman, Kalapuyan and Chinookan Lexicography”, in which he stated that “there exist strong reasons to believe that the Takelman, Kusan, Siuslawan, Yakonan, Kalapuyan, and (perhaps) Chinookan languages spoken in Oregon may be proven to be Penutian sister-tongues” (Frachtenberg 1918:175).

The publication of Frachtenberg’s paper somewhat miffed Sapir—he felt that he was far ahead of Frachtenberg in this area of research—and this, together with the publication of Dixon and Kroeber’s full monograph on their Hokan and Penutian data (1919), stimulated him to prepare a paper of his own, entitled “A Characteristic Penutian Form of Stem” (Sapir 1921a). Sapir sent a manuscript copy of this paper to Kroeber in November 1919, referring to it as his “extended-Penutian” study.” In it Sapir severs “Penutian” from its California usage and applies it unequivocally to a stock that he believes extends from Tsimshian to Yokuts. He writes:

> It is, of course, entirely premature to group the Penutian languages genetically, but the following provisional scheme may be of some slight value...
> A. Californian group
> B. Oregon group
> C. Chinookan
> D. Tsimshian

To Sapir now, the “Penutian languages of California” are coordinate with “the Penutian languages of Oregon” as part of “the Penutian languages as a whole.”

The same proposed relationship, with basically the same nomenclature, formed one of the six “great groups” (or “superstocks”, as we have come to call them) that Sapir proposed at the AAAS meeting in December 1920, and that was published as “A Bird’s-Eye View of American Languages North of Mexico” (1921b). The only difference was that the “California group” and the “Oregon group” were explicitly called “California Penutian” and “Oregon Penutian”.

In 1929, when Sapir summarized his general classification in the *Encyclopedia Britannica*, he added a final West Coast branch to the Penutian superstock, “Plateau Penutian” (comprising Sahaptin, Molala-Cayuse, and Klamath-Mo decent). More important for our nomenclatural survey, the 1929 classification also includes a coordinate “Mexican Penutian” branch, comprising Mixe-Zoque and Huave, and Sapir notes that “both Xinca and Lenca (also Paya and Jicaque?) may be remote southern outliers of the Penutian languages of North America.” In Sapir’s hands “Penutian” has now come to label a relationship of an entirely different order from Dixon and Kroeber’s California grouping of 15 years previously. While the latter is merely a factor in the prehistory of Central California, Sapir’s version of “Penutian” plays a major role on the hemispheric stage:

The Penutian languages, centered in Oregon and California, must early have extended far to the south as they seem to be represented in Mexico and Central America by Mixe-Zoque, Huave, Xinca, and Lenca. These southern offshoots are now cut off from their northern cognate languages by a vast number of intrusive languages, e.g. Hokan and Aztek-Tanoan (Sapir 1929).
Sapir's 1929 classification, minus the Mexican connections, has emerged as the standard version of the stock-level "Penutian" hypothesis in the second half of the 20th century. It was espoused by Hymes in a series of papers from 1954 onwards, and was the working definition of "Penutian" adopted by the Workshop on Comparative Penutian held at Eugene in 1994, coordinated by Scott DeLancey and myself. We reasserted this definition in our special issue of *IJAL* in 1997, and it was adopted by Ives Goddard as the definition of the "proposed Penutian superfamily" in the classification and map accompanying Volume 17 of the *Handbook of North American Indians* (1996).

4. THE MACRO-PENUTIAN PHYLM. By the mid-1930s a number of younger linguists were following in Sapir's classificatory footsteps, proposing new alignments and configurations with the "syncopated" labels that so irritated Kroeber (1940:470). In 1935-36 two Yale linguists, George Trager and Benjamin Lee Whorf, made an ambitious proposal in which Penutian centrally figured. Since this involved linking Penutian with two other widespread and deeply diversified stocks (Uto-Aztecan and Mayan), they dubbed the relationship a "phylum", implying an older and deeper relationship than anything previously called "Penutian", and labeled the proposal "Macro-Penutian." Whorf briefly alluded to the relationship, without naming it, in his paper on "The Comparative Linguistics of Uto-Aztecan" (Whorf 1935:608), but J. Alden Mason, in his summary of the languages of Central America in *The Maya and their Neighbors*, was apparently the first to publish the term:

Resemblances between Penutian, Uto-Aztecan (Utaztecan), Mayan, Mixe-Zoque-Huave (Mizocuavean), and Totonacan have often been pointed out, and their genetic relationship suggested. B. L. Whorf's recent unpublished critical studies have convinced him the first four should be combined in a phylum which he terms "Macro-Penutian"...the inclusion of Totonac is problematical (Mason 1940:58).

Xinca and Lenea are also provisionally added on the accompanying map (Johnson 1940). Another version of Macro-Penutian, reworked by Trager, was published in 1958 in Trager and Harben's short monograph on North American classification:

Macro-Penutian  
1. Penutian  
2. Sahaptian  
3. Azteco-Tanoan  
4. Tunican  
5. Mayoid (Mayan)  
6. Totonac

In this scheme, "Penutian" is as Sapir had defined it, including Tsimshian and "Mexican Penutian" (Mixe-Zoque and Huave), except for Plateau Penutian, which is treated as a separate branch of the phylum and a link between Penutian and Aztec-Tanoan.

The most far-reaching version of Macro-Penutian—or, in one variant of his terminology, the "Penutiology" phylum—was Morris Swadesh's. Based on idiosyncratic lexicostatistical estimates of genetic closeness, he proposed a chain of relationships down the Pacific coast of both American continents, from Tsimshian in the north to Quechua-Aymara in the south, including in all 20 major divisions (Swadesh 1954, 1956).

Swadesh later withdrew this hypothesis in favor of a continuum of North and Middle American languages he called "Macro Mayan" (Swadesh 1967), which included all of Sapir's Penutian (although not as a distinct subgroup) but excluded Quechua, Tarascan, and Zuni (these forming part of a different continuum, "Macro Quechua").

Starting in the mid-1960s—around the time "Penutian" ceased to be used for the California "kernel"—the unmodified term gradually came to be used in place of "Macro-Penutian" for
phylum-level proposals. The first instance of this was in the influential “consensus”
classification that came out of the first “Conference on American Indian Languages” at
Bloomington in 1964 and embodied in the Voegelin’s wall map (Voegelin and Voegelin 1966).
In this version, “Penutian” refers to a group that includes Sapir’s 1929 unit (with Mixe-Zoque
and Huave), plus Mayan and Totonocan, plus Chipaya and Uru in Bolivia.

In 1987 Joseph Greenberg incorporated “Penutian” into his hemisphere-wide classification as
one of the eleven “subgroups” of the “Amerind” macrostock. The eight subdivisions of
Greenberg’s “Penutian” include—besides the usual suspects in California, Oregon, British
Columbia, and Mexico—Zuni (following Stanley Newman’s 1964 proposal) and “Yuki-Gulf”,
the latter consisting of Mary Haas’s “Gulf” relationship (Atakapa, Chitimacha, Muskogean,
Natchez, and Tunica) plus the Yukian languages of California (Greenberg 1987:143-62).

Finally, during the last decade the German scholar Stefan Liedtke has reasserted the
possibility of a genetic link between Quechua-Aymara and Penutian in Sapir’s 1921 sense,
linking also Tarascan, Zuni, Wakashan, and Salishan. He refers to this modest assemblage
simply as “Penutian” (Liedtke 2001).

5. EXTENSIONS TO ETHNOGRAPHY AND ARCHAEOLOGY. From early days, Kroeber was
wont to talk about “Penutian” culture and “Penutian” peoples in an ethnographic sense:

“...the Penutians of California were very plainly the people of the great interior valley. It is chiefly from the
vicinity of San Francisco to Monterey that they impinged on the ocean” (1925:544).

“Central California [was] distinctly a Penutian Province with Hokan fringes” (1925:913).

Kroeber was also the first to project the idea of a Penutian culture and nationality back into
the prehistoric period:

“The speech and perhaps the customs of [the Sacramento-San Joaquin Delta] are likely to be more similar
to Penutian speech and customs of a thousand years ago than the tongues and habits of any other Penutian
area” (1925:349).

“The Penutians....have therefore presumably spread out along this stretch of coast, in which their Costanoan
division was located in historical times and where it may be supposed to have taken shape as a group”
(1925:544-545).

This ethnic reification of “the Penutians” has become quite common in archaeology, and is
now migrating into biological anthropology. My geneticist colleagues at UC Davis regularly
speak about the mitochondrial DNA they have obtained from “Penutians”—meaning anyone
from a Chinook to a Yokuts. In both archaeology and in genetic studies, “Penutians” or
“Penutian speakers” migrate about the map with alarming alacrity. A couple of examples from
among many:

Aggression from Numic speakers, rather than environmental deterioration, may have forced Penutian
withdrawal from the Basin (Oetting 1989).

This model...suggests that the Costanoans and Coast Miwok resulted from the cultural and physical mixing
between early Hakan speakers and incoming Penutian speakers (Breschini 1983).

6. PENUTIAN FURTHER AFIELD. Through textbooks, encyclopedias, and other distillations
of received knowledge, the term “Penutian”, in one or another of its classificatory senses, has
here and there seeped into general American culture. In preparing this paper, I did an Internet
search for the term, looking for instances of the term outside its normal academic contexts. My
results fell into three groups:
(1) “Penutian” as a language or language type:

Reinstatement of the Klamath Tribe has brought about a revitalization of traditional practices, translation of books into the Penutian language, and renewed emphasis on traditional crafts.

“Walla Walla” is a Penutian word meaning “Little River”.

The Bay Miwok spoke a Penutian dialect.

(2) “Penutians” as an ethnic group or subgroup:

These two tribes [of Takelmas] ... belonged to a speech phylum known as Penutian who had inhabited much of the Oregon Territory until approximately 1500 years ago when other tribes moved south into the region.

MEChA (“Movimiento Estudiantil Chicano de Aztlán”—“We are Chicanos and Chicanas of Aztlán reclaiming the land of our birth”) is divided into nine regions. Each region has a communication center... The Centro California center (communication center at CSU-Chico) is “Penutian MEChA Central”.

(3) “Penutian” in geology

Finally, I was startled to find various web references to the “Penutian Stage” of the Eocene Epoch. This turns out to be part of a special North American geological nomenclature, in which the subdivisions—or “stages”—of the Epochs (Miocene, Pliocene, Pleistocene) are given distinctively North American names, rather than the standard names that are derived largely from European locales. It is not clear how widely used this nomenclature is, but Google came up with several citations. Typical is:

Subaerial notching of the shelf edge coincided with the Late Penutian sea level drop.

Given one or two other uses of American Indian names in the nomenclature—the “Commanchean Stage” of the Early Cretaceous—it seems likely that this is a special case of “Penutian” being taken as the name of a group of (prehistoric) people. The model would be the naming of the Silurian Epoch of the Paleozoic after the ancient Silures of Wales, where important strata dating to that epoch were first identified. Apparently the Penutian Stage is defined (for North America) by strata in Central California.4

4 I am grateful to Prof. William C. Miller, Department of Geology, Humboldt State University, for information on the geological use of “Penutian.” He writes:

Names like Penutian have a regional currency, and were coined in most cases when stratigraphic successions were first described and ages were estimated based on fossils for the first time. Each region has its own “time-stratigraphic” (chronostratic) division names. These very often are not the standard names used internationally. Names coined in the places where rocks of a particular age were first recognized are conserved in the international system of naming divisions of the Geologic Time Scale. The European name “Ypresian” (lowest stage- and oldest age-level division of the Eocene Epoch/Series) contains the Penutian and has priority (because rocks of this age were first recognized and named in France). Therefore, in the accepted usage, Penutian would have to be referred to as lower or early Ypresian, if you wanted workers worldwide to know what you’re talking about. You could stick to Penutian if you were talking to geologists from California who specialized in Eocene stratigraphy. I don’t know how widespread the practice is of naming stages/ages for tribes and language groups. Usually, region or place names are used, but not always. The quest is to find a name that hasn’t been used.
7. CONCLUSION. For reasons that are largely accidental, "Penutian" has become one of the most polysemous terms in American Indian historical linguistics, and—more than most labels of tenuous genetic relationships—has made its way into more general usage in allied disciplines and beyond. As students of these languages, we are both blessed and cursed by this terminological chameleon.

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1. ABSTRACT. In this paper I examine certain aspects of relationships which have been proposed to exist between those pairs or more numerous groups of so-called Penutian languages, many of which are spoken in what is now the state of Oregon. Much of what has been previously taken as evidence for a genetic relationship between some of these languages turns out to be diffused material which has passed from one potential branch of Penutian to another and which may mask the existence of actual (or at least potential) cognates which exist between these two branches. Other similarities, which are more typological in nature (involving 'pattern' rather than 'fabric'), are the result of intimate language contact and sometimes language shift, factors which have been brought about by the more complex interlockings of patterns of social relationships within and between small and multilingual speech communities, especially in the period before Euroamerican contact. The fact that this material has been diffused, rather than being inherited from a parent-language, has sometimes gone unnoticed, because what is diffused includes certain kinds of items which in other language families, for instance Indo-European, are taken as primary evidence of an ultimate relationship between pairs or groups of languages.

I suggest that the discipline of historical linguistics as it is generally practiced in more firmly-established language families can be enriched by the findings from attempting to relate groups of Penutian languages to one another, and I point out that a lexicostatistical classification of Indo-European (Dyen, Kruskal and Black 1992), which has used methods similar to those which are available to Penutianists, can serve as a (very partial) working model of what sort of issues the attempts at classifying and affiliating languages which are assumed to be related at a time depth of several millennia may have to contend with.

2. THE AMBIGUOUS ROLE OF TYPOLOGY IN 'PROVING' AFFINITY BETWEEN PENUTIAN LANGUAGES. A considerable amount of work on historical linguistics, including some of the work on comparative Penutian, has been concerned with analyzing shared typological properties or combinations of morphemes in the languages (the sort which I will call 'patterns'), rather than busyng itself about the actual morphs which go to make up these patterns, which comprise what I will call the 'fabric' of the language.

The typological patterns of a language can only give very limited indications of the likely affinities of a language or language family. There is only a finite number of typological patterns which are available to a language, and this number is often very small. In cases of Agent-Verb-Patient order, for instance, there are only six possible orders of elements, even in those sentences which have full nominal agents and full nominal patients. Furthermore, the work on typological linguistics which has been carried out over the past four decades shows

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1 I would like to thank Catherine A. Callaghan, Dave Costa, Kathrine French, Andrew Garrett, Victor Golla, Dell Hymes, Julie Morfee, Lawrence Morgan, Mary Newton, Johanna Nichols, Lynn Nichols, Mike Nichols, Bruce Rigsby, Bill Shipley, Marie-Lucie Tarpent, Donald Whereat and Patricia Whereat for their encouragement and for assistance with materials which have found their way into this paper. None of them is to be held responsible for any use which I may have made of their advice.
that certain groups of typological patterns have a tendency to cluster. If a language is verb-final, for instance, it will almost certainly have postpositions rather than prepositions.

The following paper will concentrate upon discussing certain phenomena found in languages which have been assigned by the slightly more conservative 'lumpers' to the Penutian hypothesis, and which belong to language groups whose members are or were spoken within the bounds of the present state of Oregon. For the record, my list of groups which need to be taken into account in 'Penutian' includes the following: Utian (Miwokan-Costanoan), Yokutsan (which may be specially related with the former grouping as 'Yok-Utian'), Wintuan, Maiduan, Klamath-Modoc, Molala, Cayuse, Sahaptian, Takelma, Kalapuyan, Coosan, Siuslawan, Alsean, Chinookan and Tsimshianic. Most of the languages in most of these families are no longer spoken. This is pretty much the list of branches from which evidence was used by Hymes (1964) when he reconstructed 182 cognate sets beginning in *c- and *s-, which he claimed might be evidence for the genetic unity of Penutian. (If this is actually the case, then by extrapolation we might suggest that the number of cognate sets reconstructible for Penutian on the testamentary principles used for Proto-Indo-European, may be around a thousand, if Proto-Penutian is valid.) At least for the present I omit from consideration the Mixe-Zoquean languages, which Hymes included (Hymes 1964:223), and Tonkawa, which he did not, and I regard Stanley Newman's championing of the Penutian relationships of Zuni (Newman 1964) as an intellectual scherzo, as I have reason to believe he did too.

The breadth of the list above should not be taken as a claim on my part that Penutian as a non-trivial genetic unity is provable or proven. To a large extent I share the radical dissent about Penutian expressed in Shipley (1980), itself the work of someone who has contributed much to the study of Penutian languages. Indeed, I have purposely used the phrase 'the Penutian hypothesis' above. My current position on the validity of Penutian is that there are several groups of genetically-related languages among those listed above, that these constitute macro-groups, and that two or more of these macro-groups may constitute a valid genetic unit, with time depth measurable in terms of several millennia. I would further state that because of the great time depth involved, it will probably never be possible to demonstrate the genetic relationship of these languages—if it exists—in such a way that the evidence for it can be distinguished on qualitative and quantitative grounds from the sets of sporadic, superficial and specious resemblances which can be found whenever any two languages are compared.

In regard to another Pacific Rim linguistic hesion, the Oceanic linguist George Grace (Grace 1990:156) once said that if one tried to reconstruct Proto-Austronesian on the evidence obtainable from three languages, choosing for example a Southern New Caledonian language (such as Tinrin/Grand Couli, which Grace does not mention but upon which he has done extensive work), Yapese of Micronesia, and Atayal of Taiwan, and using only these languages, it might be almost impossible to show that these were related to one another at all. And yet the task of showing some kind of genetic relationship between a set of languages is an essential first step towards reconstructing a proto-language.

I feel that this is the position which Penutianists face in trying to link up groups of languages and proving genetic relationships between them. We have to work with what we have, and we cannot be sure that the languages of which records have come down to us are an especially conservative set, in terms of regularity of sound-correspondences in relation to the as yet hypothetical Proto-Penutian. Indeed we may be dealing within Penutian with a large
proportion of what Grace called 'aberrant' languages, languages which do not provide easy, useful or copious evidence about the proto-language.

To put things in Indo-European terms, we might be hoping to find among the Penutian languages for which we have data a number of Sansricks and Lithuanians, which would make our work easier, but in dealing with Penutian we may have ended up trying to reconstruct the proto-language by using a number of correlates of Albanian, because that is all that have been left to us.

3. TYPOLOGICAL SIMILARITIES BETWEEN COAST OREGON PENUTIAN LANGUAGES.
Typological claims about the genetic unity of pairs or larger groups of languages may seem superficially attractive, but they always need to be assessed against other bodies of evidence, rather than being taken for granted. For instance, if two particular languages, which are spoken in areas which are adjacent to one another, both show a highly-marked order as the normal one, for instance Patient-Agent-Verb, this may be an indication that the languages are genetically related.

Or it may not. It may be the case that such a marked order has been brought about in one of these languages through the influence of speakers of the other language, or it may be that the two languages both originally had a different word order (or else that they used two different word orders) and that their systems converged on this third word-order as a result of independently motivated language-internal changes in each of the two languages. The processes which gave rise to these syntactic changes need not have been shared by the two languages and they need not be traced to a common set of rules and motivations.

A common example of this can be taken from two languages of southern Coastal Oregon with materials on which, and with the descendants of whose speakers, I have had the honor of working. Hanis Coos and Lower Umpqua are both recorded in the linguistic literature as having a basic VOS word order, with an alternative OVS word order. This fact has been known to participants in the Americanist field since 1922, with the publication of Frachtenberg's grammars of these languages (Frachtenberg 1922a, 1922b), but it was brought to wider linguistic attention in Greenberg (1963). Both Hanis and Lower Umpqua belong to families, namely the Coosan and Siulawain families, which are regarded as forming part of 'Coast Oregon Penutian', two-thirds of it, in fact. This is a hesion about which I will have more to say later. (Alsean is the third part of this grouping, and it appears, from the evidence available to me, to have a generalized VSO word order.)

And yet we cannot take this shared word-order as evidence that, even though this typological feature is rare in the world's languages and consequently highly-marked, Siuslawain and Coosan are especially and non-trivially closely related—that is, we cannot be certain that they are each other's closest documented relatives.

There are (at least) two reasons for this. The first is the nature of the evidence itself. Proof of genetic relatability, in this case proof that the Coosan and Siuslawain languages descend from a single language, even if this was spoken several thousand years ago and has left no records (and if it did exist, it hasn't left any), relies upon demonstrating the continuation of transmission of actual elements of linguistic material. We need to see the transmission of free morphemes and the means to bind them together into clauses and sentences. These elements are passed on—often with changes—from one generation of speakers to the next.

What we have here in the case of two languages sharing VOS word order is not genetic material, though. For present purposes it is a typological characteristic, something which
allows us to bracket Hanis (and the related but rather different Miluk) and Lower Umpqua and the very similar Siuslaw together on 'structural grounds,' and to classify them because of these shared structural similarities with some Cariban, Arawakan, Tupian and other, isolated, languages in the Amazon and on the northern reaches of the South American coast, with the long extinct language Hurrian of the ancient Near East, with Ungarinyin and a couple of others in Australia, and with a few more languages in Papua New Guinea, such as Fasu. (A list in Nettle 1999:140 gives the names of most of these languages, including Hanis as the only North American representative of this typological class, and thus omitting Miluk and Siuslaw-Lower Umpqua.) These twenty or so languages are said on typological grounds alone to form a select band of languages, although they are or were spoken halfway round the world. These languages are supposed to be object-initial—a feature which is rare enough in itself—and some of these, such as Hanis, are furthermore supposed to have the verb preceding the subject in main clauses, which is an even rarer typological pattern.

Apart from such purely typological features of major element-order there is nothing else which unites these languages; we have no reason to believe, for example, that they share a common history of development. The feature which brackets these languages together does not include or involve any actual morphemes. It is just an arrangement of constituents, and these constituents differ in actual form, in the morphemes which they contain, from language to language. The same kind of highly-marked constituent order could happen to become supreme in English one day, for all we know.

The second reason why we cannot be sure that Siuslawan and Coosan are specially related is that the capacity to have recourse (as a means of explaining phenomena) to the whole patterning business, which is still so popular with typologists, is built on something of a misunderstanding about what the true linguistic significance of basic word order really is. And certainly it is a misunderstanding in the case of Hanis. In an important cross-linguistic study Marianne Mithun (Mithun 1992) has shown that Hanis Coos does not have exclusively OVS or VOS word order (though Greenberg's claims in Greenberg 1963 about Hanis and Lower Umpqua being postpositional and placing the genitive and the adjective before the head noun are correct enough). True, one can use OVS word order in a Hanis sentence with some assumption that the outcome would have been grammatically acceptable to Jim Buchanan, or Frank Drew, or Lottie Evanoff, or another of the Hanis speaking elders of years gone by whose dictations form the basis of our knowledge of Hanis syntax and pragmatics.

But there are other word orders available. Indeed there are six basic ones, as I mentioned, and Hanis sentences, such as those which have been recorded in our texts from the people mentioned above, can be seen to use any or all of them. None of the six possibilities is ruled out; any or all are possible in a Hanis sentence. The reason for this is that Hanis word order is not syntactically but pragmatically defined, with new information taking priority sentence-initially over old information, and with this pragmatic concern in a Hanis sentence overriding any fundamental syntactically-motivated considerations.

Typological appearances can therefore be deceptive to the genetic linguist. The evidence which is derived from typology can point us in all manner of useful directions in our search for similarities, and the amount of evidence which is provided by typology alone can make it seem as though there is only one valid conclusion to be drawn in a particular situation when we are conducting a historical analysis, but it can never clinch an argument regarding genetic relationship. For example, findings about typological similarities between neighboring languages are invaluable in building up hypotheses about linguistic areas, and in plotting them on maps, but typological change in a language is certainly not unknown, and in any case
areal linguistics is a secondary concern (and the result of secondary developments and convergences) when its findings are placed against those of genetic linguistics. These considerations are not confined to syntax. In Penutian even the 'characteristic form of stem' involving disyllables with identical first and second vowels (Sapir 1921, Tarpent 2000) is only as characteristic as the rules of the historical phonology of the language in question will permit it to be.

Strong arguments about genetic relationships can only ever be made with reference to 'fabric' that is actual morphemes attested in actual languages. Nothing else will suffice; anything else is only conjecture, and any other kind of evidence can be controverted completely if hard morphemic evidence turns up.

4. CHINOOKAN AND ITS MEMBERSHIP IN PENUTIAN: THE TRAP OF TYPOLOGY. If excessive reliance upon the findings of linguistic typology can attempt to compel us to lump things erroneously, it can also cause us to split them erroneously from one another. The problem of the assignment of Chinookan to Penutian is an example of this. The close genetic interaffinities of the Shoomwater and Clatsop varieties of Chinook on the coast, of Kathlamet (and, we assume, the unrecorded Multnomah) further inland, of Clackamas beyond that and of Wasco-Wishram (which is often classified with Clackamas and called Kiksht) are undisputed by all. Chinook, especially the Coastal varieties which were best known to earlier investigators such as Franz Boas, is a head-marking and polysynthetic language; the Penutian languages taken as a potential group tend to be rather aggressively dependent-marking and are somewhat more inclined to subsume fewer morphemes within the space of a word. The Chinookan group therefore stands out anomalously from the rest *prima facie* on typological and patterning grounds rather than on the grounds of not sharing fabric in the form of morphemes which are ultimate cognates with morphemes in other Penutian languages. In fact Edward Sapir and, more recently Dell Hymes, have both made out a strong case for the membership of Chinookan in Penutian and have taken its membership within Penutian as a given fact in their comparative Penutian work (see for instance Sapir 1921, Hymes 1964).

What is more, there is quite a range of typological variety within certain structures which are found in Chinookan, as Michael Silverstein's study of Chinookan tense-aspect systems (Silverstein 1974) shows. In the west, Chinookan TMA systems are structurally simple, with three or so aspects being overtly marked, and they pattern like those of the neighboring Coast Salishan languages. In the east, as one goes further and further downriver, the TMA systems look more like those found in Sahaptian languages in terms of the kinds of distinctions which they make, although not as much in the actual morphemes which they use in order to express these distinctions.

None of this should surprise us if we remember that Chinookan tribes long held sway along the westernmost reaches of the Columbia, and that they enslaved and intermarried with people from a wide variety of tribes (Silverstein 1990); indeed Charles Cultee, Boas' chief consultant (of two surviving speakers then known) for Shoalwater Chinook and the only surviving speaker of Kathlamet Chinook at that time, was one-quarter Kwaltioqua (Washington Athabaskan) by descent (Boas 1901:5). We cannot measure the extent of the linguistic impact of Coast Salish-speaking women or Sahaptin-speaking women upon the Chinookan languages which these transmitted as adult L2 speakers to their children who learned them as their first or coequal language, but we may imagine that it was considerable. The converse, namely the impact of Chinookan upon surrounding languages, is also true,
although here the evidence points more to diffusion through language contact rather than through processes of language shift.

We also find lexical loans from the various forms of Chinook being borrowed into other neighboring languages, although these have sometimes been taken as evidence for relationship between Chinookan and the borrowing language. For instance most of the lexical links which Frachtenberg (1917) saw as obtaining between Kalapuyan and Chinookan are actually loans from Chinook into Kalapuyan languages, and pretty much all of them happen to occur in Chinook Jargon. Berman (1988:3) has pointed out that Chinook-style gender systems, involving a distinction between masculine and feminine, had begun to be replicated in Tualatin (or Atfalati) Kalapuyan, the Northern Kalapuyan dialect which is geographically closest to Chinookan, using Kalapuyan morphemes to discharge new roles on the basis of models supplied by the neighboring Clackamas, but they are found nowhere else in Kalapuyan. Tualatin has also probably given Chinookan its word for the culturally important tuber 'wapato' (whence it entered English), and Tualatin is the Kalapuyan variety which has donated a couple of words to CJ. Even if we exclude Chinook Jargon from the picture as a potential source of influence—for it had no gender system, being a pidgin, and in any case its spread was largely a nineteenth-century and post-European contact phenomenon—it is clear that Chinookan languages played an important role in influencing other languages throughout much of the lower Columbia area and surrounding lands.

The tide of history has turned, and now few varieties of Chinookan have any speakers whatsoever. Language shift has taken place in the past century or so, from Chinookan languages to Chinook Jargon and then English in the west, and to Sahaptin (with on occasion an ancillary use of Chinook Jargon) and then to English in the east. These shifts of language have not been mutually exclusive. On the Warm Springs Reservation in earlier decades of the twentieth century there were people who knew all four languages: Kiksht, Chinook Jargon, Sahaptin and English. Now only Wasco-Wishram among Chinookan varieties still has any speakers, and even then Sahaptin varieties are more widely spoken at Warm Springs than Kiksht is (though neither Kiksht nor Sahaptin is in a particularly secure linguistic position). In fact Morris Swadesh's consultant for Warm Springs Sahaptin, Eva Winishut (Swadesh 1954:132), was actually a Wasco who was married to a Sahaptin, and who used Sahaptin (and English) as her customary language (personal communication from Kathrine French, 1994).

The fact that Wasco-Wishram borrowed several case-suffixes, meaning 'for', 'at', 'with' and 'by means of,' respectively, from Sahaptin (Sapir traced them to the Yakima dialect, but they may have been from Warm Springs Sahaptin), and that it used them productively, as Sapir himself pointed out (Sapir 1911), would have suggested to us, had we not known by other means, that Sahaptin had enjoyed a position of importance at some point, since this kind of borrowing is something which is surprisingly rare in the world's languages. We also have evidence (Rigsby 1965) that Sahaptin was a donor language for a number of loans into Chinook (and often then into Chinook Jargon). Sahaptin and Chinookan even share some free grammatical morphemes, such as kwánisim 'always' (Rigsby 1965), which has also found its way into Chinook Jargon.

What should concern someone who is interested in the historical development of the Penutian languages is not so much that a head-marking language like Wishram has postpositions, but that these postpositions have been taken over, fabric as well as pattern, from another language—and that continued social pressure from this language, namely the Warm Springs variety of Sahaptin, has assisted in the subsequent development of other
postpositions from purely Chinookan materials, whereas we do not find them in more coastal varieties of Chinookan (Sapir 1911:650-654).

5. FINDING MODELS FOR TYPOLOGY AND DIFFUSION IN DIACHRONIC INVESTIGATIONS. If we want to make the best use of the findings of typological syntax, we need to combine it with findings from other parts of the language, and especially with specific morphemes and specific patterns of morphemes. I have spoken before (in my presentation at the conference held at Davis in 1997, which is incorporated into the present paper) about the problems inherent in trying to 'prove' the validity of Penutian as a genetic unity. Two of the most significant problems which we face are its likely time depth, which is unknown but certainly vast, in terms of at least several millennia, and the fact that we have essentially no records of Penutian languages before the beginning of the nineteenth century.

What we are trying to do is to use a form of the Comparative Method—a method which calls for use of linguistic fabric rather than linguistic patterns—in an attempt to navigate an area of historical time-depth which is certainly some millennia beyond that sphere where the Comparative Method still gives plentiful and reliable results. We are further trying to ensure that as a result of using this method, there will be plentiful enough results for us to be certain that whatever results we have at the end of applying the Comparative Method are not simply the outcome of chance superficial resemblances which could be replicated in any pair or set of languages picked at random.

Penutianists are not unique in being in this quandary. This is the same problem which, for instance, has faced people who are trying to reconstruct Afroasiatic, a language family with a time depth of maybe ten millennia. We have records of two—Semitic and Egyptian—of the six families which are subsumed under Afroasiatic which stretch back for five millennia. This is a distance which is certainly long enough in time to show that even then Egyptian and Semitic were very different, even though they were apparently related, and even though there was an ever increasing degree of mutual contact and influence, mostly upon Egyptian from Semitic. We can't 'prove' the unity of Afroasiatic to everyone's satisfaction according to the dictates of the Comparative Method, although all the fabric-based evidence (much of which is presented in Ehret 1995) suggests that it is a valid macrofamily.

A more familiar language family of slightly smaller time depth shows us what can be satisfactorily reconstructed. We can prove with some confidence the existence of Proto-Indo-European, whose ancestor language was spoken maybe 6000 years ago, because we have records of parts of it which stretch back, at the furthest, maybe four millennia (some Hieroglyphic Luwian inscriptions), with plenty of material that is two and a half millennia old—for example material from early Latin, earlier records of Umbrian and South Picene, Attic and Ionic Greek and of course Sanskrit. We can also prove the existence of Proto-Austronesian, which is almost as old as Indo-European, even though we have absolutely no records more than 1600 years old, while most of our relevant data were collected within the last five hundred, or even two hundred, years. And there again most of our data for Penutian languages have been gathered since 1800: the collected pre-nineteenth century Penutian linguistic data would barely fill a school exercise book.

Consequently we welcome any concrete linguistic evidence which will help us to understand or clarify the historical picture of the Penutian languages more easily, even when that evidence points to a period of intensive linguistic contact between two groups, rather than to their non-trivial linguistic interrelationship.
For example, Alsean, Siuslawan and the two Coosan languages, which as previously stated are often classified as Coast Oregon Penutian (henceforth COP), show a large number of similarities, in the shapes of most of their pronominal affixes, in phonological systems (which are largely boiled-down versions of the complex Northwest Coast segmental phonological system), and to some extent in word order (at least we can say that most of them have a predilection for verb-initial sentences). There are some vague similarities of fabric or pattern on occasion between every one of these three families and a non-COP member—for instance some word shapes, though only a few, seem to be shared between Coosan and either Takelma or Kalapuya (though hardly ever both of the latter), while Siuslaw and Molala seem to be more strongly dependent-marking than the other Oregon Penutian languages. Additionally Alsea and Wintu from among the Californian Wintuan languages share a number of similar lexical shapes with similar meanings, most of which are not found elsewhere in COP or among other Wintuan languages (though some of them are: see Golla 1997). For the moment I will rule Wintuan out from the group of Coast Oregon Penutian languages.

But what seems, at least in the eyes of many scholars, to bind these three sets of languages together as belonging to COP is the possession of a large number of words with similar phonological shapes, as was shown in Buckley (1987). These elements of similar appearance are not necessarily cognate sets. Some may be cognates, others may be borrowings from one Coast Oregon Penutian language to another, yet others don't even look very similar (which does not in and of itself mean that they cannot be cognates). Lexical evidence, which is the sort which many subgroupers start with and from which they soon move on with alacrity, is almost the weakest kind of evidence to use when attempting to prove a genetic relationship between languages (though with many languages, as for instance the putatively Penutian Cayuse, we have no other evidence to turn to), but it is still a stronger step towards proof than typological 'evidence' is.

If the Coast Oregon Penutian languages are related, the degree of relationship—and, to take the quickest metric as a preliminary guide, the level of lexical cognacy—is not high. In a classic yet still unpublished paper Kinkade (1978) estimated the number of items shared between Alsea and Hanis Coos on a certain version of the Swadesh 200-word list as numbering 40, and those shared between Siuslaw and Hanis as 32, while some 20 items seemed to be shared between Alsea and Siuslaw. We should remember that the Siuslaw Swadesh list available to us has 41 gaps, and the 200-item one for Hanis has thirty, while a little under 180 Swadesh list forms were available for Alsea, so that these proportions, after scaling down the number of comparanda, are more impressive than they look. 40 resemblances out of a maximum of 172 comparable forms is approximately 24%.

We might wish to compare these figures against those which were provided in a lexicostatistically-based attempt to subclassify Indo-European languages on the basis of reflexes and patterns of cognition in their modern forms. Such an approach has very rarely been employed in the task of classifying Indo-European languages, where material from older language is much more prized. This was conducted 'blind', that is, without reference to earlier stages of Indo-European such as Sanskrit, Latin or Classical Greek, but using modern languages only. The study was carried out by Isidore Dyen, Joseph Kruskal and Paul Black (Dyen, Kruskal and Black 1992). These investigators also used a 200-item Swadesh list, but one which differed from the one used for Penutian languages in about 5% of its entries. Here the percentages for modern Indo-European languages were calculated using linguistic materials which had almost all been collected within the previous century, and many of the
lists used were contemporary ones from native speakers, gathered by linguists who were often themselves native speakers of the languages for which they provided samples.

In short, the general nature of the material which Dyen and his colleagues used was pretty much comparable (at least in its explicit lack of assumptions about possible first orders of subgrouping) with that for the Oregon Penutian languages, although it should be remembered that this kind of examination is limited insofar as it looks at only one kind of linguistic evidence (that is to say, shared lexicon) and excludes shared inflectional morphological features almost completely. But Dyen and his colleagues were working with lexicon in the same way as have other scholars who have commenced preliminary classifications of languages whose structures, though maybe well-attested, were as yet imperfectly explored and understood. In taking this approach Dyen and his colleagues chose to do what Penutians have to do.

The figure of 24% which appears at first blush to separate Alsea and Hanis is the kind of time-depth of cognition within the confines of the lexicostatistical list which apparently also separates modern Catalan from modern German. (This latter figure was presumably arrived at by the calculators after they had subtracted the few percent of diffusional cumulation which was caused by the diffusion of Germanic loans into Western Romance and of Latin loans into German.) If the Alsea-Siuslaw figures are accurate, they exhibit a time-depth of about 11%, which is parallel to that obtaining between Irish Gaelic and Hindi (Dyen, Kruskal and Black 1992:103).

But a large proportion of the close inspectional similarities among the 240 forms which Buckley documents as occurring between at least two of these three languages look more like loanwords, and what directional evidence there is from the phonological forms of these items suggests that they are loans from several directions. In a few cases they appear to be loans from Siuslawan to Alsea, in quite a few other cases they seem to be loans from Alsea to Hanis and especially from Alsea to Siuslawan, and sometimes they look like loans from Hanis to Lower Umpqua and then to its sister-language Siuslaw. We cannot be sure how long these patterns of contact had been going on, but some of the latter, the Coosan to Siuslawan loans, may postdate European-Native contact (after 1826 in this area) or prolonged Native-European contact (post-1851) in the area. An example is the word for the post-contact food item 'bread', which is qalax in Hanis and Miluk Coos and qalxa in Lower Umpqua and Siuslaw. This term is unanalyzable in the Siuslawan languages (and in any case the presence of an /l/ in a Lower Umpqua word is a sign of a loan into Lower Umpqua, since /n/ would have been expected), but in Hanis the sense 'bread' is secondary, the primary meaning being 'white clay', which is the impression that the appearance of bread made upon the Native Americans when they first encountered it (Donald Whereat, personal communication).

On the basis of this linguistic evidence, it generally seems as though the Alseas, who were the first of these groups to be contacted by Whites (in 1789) were usually the transmitters of innovations and that the Siuslawan-speaking tribes were especially prone to be the recipients. The Alseas seem to have had a boarding-house reach which permitted them to pass some ideas and innovations onto speakers of Hanis, and maybe also Miluk, without them being recorded for Siuslaw or Lower Umpqua (for example kwee as the name of a kind of canoe is shared only between Alsea and Hanis: Buckley 1987:60). But there again the absence of these forms in the Siuslawan material may just reflect gaps in our material for the latter group, since Siuslawan is the most thinly-attested of the three groups.
On the other hand, some innovations, such as the largely shared counting system, which involves a number of morphemes which are found in Alsea, Siuslawan and Miluk (and to a slight extent also in Kalapuyan), apparently never reached Hanis-speakers, who used a largely unrelated system. (Nevertheless, the trope of using the expression 'one stick' for 'one hundred', which is found also in Hupa in northern California, an Athabaskan language which does not border upon Coast Oregon Penutian territory, is recorded for both Oregon Athabaskan languages such as Tututni and for the potentially Penutian Hanis and Miluk, though Siuslawan and Alsea both use an expression meaning 'ten tens'; Victor Golla, personal communication). Hanis numerals, which are quinary in construction, comprise a number of forms which are not otherwise found in Coast Oregon Penutian languages.

Most of the elements which might cause people to classify the Coast Oregon Penutian languages together as a genetic grouping after a cursory inspection of materials on these languages are unlikely to be the ones which are the best evidence for actually showing these groups of languages as being (probably) interrelated. These 'rogue elements' which attest to diffusion rather than genetic inheritance include some of the lower numerals, the sharing of certain kin terms (which have apparently diffused between Siuslaw and Alsea, being borrowed by the former as evidence by their possession in Siuslawan of an element which is unanalyzable and otherwise unattested in Siuslawan but which is clearly a prefix of some sort in Alsea), the names for a number of cultural items, flora and fauna and so on, and maybe even the presence of that highly-marked VOS word order to which I referred at the beginning of this paper.

Several kin terms are shared by two or more of the COP languages, but especially by Alsea and Lower Umpqua (Frachtenberg 1922b:461-462). In the latter language they appear with what is spelt in the sources as a prefixed m(i)-, although Lower Umpqua uses suffixes almost exclusively. This prefix is absent in the Alsea forms of these words, although Alsea, like the Siuslawan languages, uses personal pronominal suffixes rather than prefixes, and a prefix of the shape m(i)- would be a verbal prefix marking intransitive and continuative aspect! (See Frachtenberg 1920:286). For example Lower Umpqua has mita 'father', while Alsea has ta'a. This may seem hypocoristic, but the two languages also appear to share forms meaning 'mother', 'elder brother', 'younger brother', 'elder sister', 'younger sister', 'maternal uncle', 'parent-in-law', 'sibling-in-law' and 'son-in-law', while Lower Umpqua and Hanis share similar forms of a term meaning 'two people who are related through someone who is deceased', a commonly-encoded kind of affinal relationship which was accorded a special kin term in other societies in the area (for instance among the Molalas and among the speakers of Grand Ronde Chinook Jargon).

Lower numerals, too, are often shared items in coastal Oregon, as Pierce (1966:385) pointed out in his attempt at ascertaining the nature of the relationship between the Coosan languages, Alsea, Siuslawan and Takelma. These forms look more like loanwords, at least in the case of the forms as they are found in some languages, than a shared set of inherited forms which betoken original genetic unity. To summarize, Hanis, Miluk, Siuslawan, Alsea and Kalapuya (the last is not mentioned by Pierce) and possibly also Takelma share a form with a stem /p:sin ~sin/ for 'three' (the Takelma form is xibini). This is the most widely diffused numeral shape in the area. Siuslawan and Miluk have xáat 'su, and at 'sú respectively for 'two', while in Siuslawan 'four' is xáat 'suun. (Takelma also uses a form of 'two' to express 'four', but in this case the form is simply reduplicated: gamgám 'four'). Hanis kat 'ómis 'five' resembles Siuslawan qátimx 'six'. The forms are similar but the differences do not appear to be the result of sound changes, since such sound-changes are not otherwise
paralleled in these languages. The first ten numerals in Hanis and Miluk are, as Pierce shows, almost entirely different in shape and in morphemic structure, even though the genetic relationship of these two languages is beyond doubt.

The results of language contact have spread not only to fabric but also to pattern. The numerals 'seven' and 'eight' involve the forms for 'three' and 'four' plus additional morphemes in Miluk and the forms for 'two' and 'three' plus additional morphemes in Siuslawan and in Alsea (although in Alsea the morphemes used, apart from 'three', are dissimilar to those used in Siuslawan.) However, in Gibbs (c. 1863), a vocabulary of Lower Umpqua, the forms for 'seven' and 'eight' are <shi-na-háu-it> for 'seven' and <kha-art-tes-hañt> for 'eight'. The form for 'eight' here is ambiguous, as the first part could refer to 'four' or 'two' (the latter being especially likely in the case of a subtractive count), but the first part of 'seven' is clearly a form of 'three' (Siuslawan šiinx), which suggests that Siuslawan originally followed the same pattern for these numerals as Alsea and Miluk did, but that it later changed this, possibly turning an original Alsea-style count into a subtractive count. In the sets of numerals of the first decade in these languages we see ample examples of both diffusion of fabric, and of metatyp of numerical patterns.

Some of the evidence which can best be used to link the Coast Oregon Penutian languages as a genetic grouping is considerably more basic, however, and more compelling. In his discussion of Alsea pronouns Kinkade (1978:8) presents several tables of forms which have similar shapes in Alsea and in neighboring languages, 'Penutian' or 'non-Penutian'. These include the personal affixes, which are used with both nouns and verbs (to indicate possession on the former and to mark agencenhood on the latter). They show remarkable similarities across the various COP languages (and, as Kinkade points out, also beyond there, in Salishan). I present his table below:

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<th>ALSEA</th>
<th>SIUSLAWAN</th>
<th>HANIS</th>
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<td>1S</td>
<td>-an, -in</td>
<td>-n</td>
<td>n-</td>
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<tr>
<td>2S</td>
<td>-ax</td>
<td>-nx</td>
<td>e'</td>
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<td>1incl. du</td>
<td>-ast</td>
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<tr>
<td>1excl. du</td>
<td>-axan</td>
<td>-axun</td>
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<td>3du</td>
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<td>1incl.P</td>
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<td>1excl.P</td>
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<td>2P</td>
<td>-ap</td>
<td>-i</td>
<td>sin-</td>
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<tr>
<td>3P</td>
<td>-dx, -itx</td>
<td>-nx</td>
<td>iš-</td>
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</tbody>
</table>

In all three languages the third person singular form is expressed by zero. Note that the Hanis forms are prefixes, while in the other languages they are suffixes. This typological fact in and of itself is no indication that these forms do not show the ultimate genetic relationship of the three groups of languages. These suffixes cannot usually be further decomposed into smaller elements. Any similarities between the sets of suffixes are either the result of massive diffusion of morphemes from one language family to another, or else they reflect the forms of inherited genetic morphemic material.

Another subset of elements of what may be the morpholexical glue which holds COP together, at least as part of a greater group, is much less dramatic in appearance than the batches of lexical items of cultural, ecological or other significance. I refer here to the
presence of a small number of verbs expressing basic ideas, such as verbs of motion. For example Buckley (1987:61) lists the following:

Alsea qaa- 'enter', Lower Umpqua qaa- 'enter, be inside'
Alsea, Lower Umpqua aq- 'go'
Alsea aya- 'to go, leave', Hanis Coos aya 'lost, gone'

There are several other common verbs which seem to be shared between two or more languages within Coast Oregon Penutian, for instance:

Alsea tsinsu-, Hanis tsim-(siml) 'sleep' (the Hanis form is a plural one; Buckley 1987:64).
Alsea tims- 'agree, close', Lower Umpqua t'oml- 'to close' (Buckley 1987:60).

6. THE PENUTIAN HYPOTHESIS AND THE PARALLEL OF INDO-EUROPEAN AS A 'WORKING MODEL' OF DEEPER GENETIC RELATIONSHIPS. We may profitably compare the findings in the section above with the kinds of data which are generally taken as being diagnostic of relationships between Indo-European languages, and which are usually among the first to be used as a means of showing that a given language is Indo-European. The number of actual forms reconstructible for Proto-Indo-European as traditionally understood runs into the low thousands, and they include morphological elements in addition to plenty of lexical items.

Typology can be illuminating but in Indo-European, as in Penutian, it is difficult to establish firm Indo-European types on typological grounds alone, especially if one examines the evidence of modern languages. Hindi has relative clauses preceding their heads, is SOV, uses post-verbal negation and postpositions, all of these being the opposite of what one finds in English. Genitives can precede their possessed noun, as in Hindi, or follow them, as in French; adjectives can precede their nouns as in English or follow them as in Spanish. Articles can precede the noun as in English, follow them as in Swedish, or be absent, as in Russian. Cases can be marked by suffixes, as in Russian, or largely by morphophonemic changes to initial stem-consonants, as in Celtic languages. Most nouns in Scottish Gaelic have the possessive person-marker following them, whereas it would precede them in English, while in Spanish both models of possession-marking are permissible. In short, there are very few typological properties which one cannot find in at least one modern Indo-European language.

And for those in search of a 'quick and dirty' method of linguistic classification, the forms of the first decade of numerals and of the basic consanguineal kin terms (such as father, mother, brother) have generally been taken as being diagnostic enough, in and of themselves, to be regarded as a sure sign that a language which possesses most or all of such forms is Indo-European. (However, Edward Sapir is supposed to have once said that he would be able to identify a language as being Indo-European even were he to have before him nothing other than three forms of the verb 'to be').

A demonstration of what the cognate residue between two related but distant languages might amount to after the passage of several millennia is provided by a body of evidence from a source from which many Americologists usually rightly turn with horror or derision, namely Greenberg (1987). As part of his counterblast against the claims of the failsafe efficacy of the Comparative Method, Greenberg (1987:20) lists the bulk of the cognate (rather than borrowed) lexical elements shared between French and English. Indeed, it is hard to think of any which he has omitted, although some of his inclusions, such as the pair
normally spelt sœur—sister, are not true cognates in the fullest sense of the word, since their form in English goes back to Old Norse rather than to Old English and is therefore a loan.

Greenberg provides some 84 sets of 'French—English' cognates, and in doing so he seeks to demonstrate that diachronists cannot rely overmuch on the evidence of sequences of regular sound correspondences occurring between a pair of languages which do not seem superficially to be closely related, as a means to alerting them to the possible genetic relationship of these languages. In one sense he is making the same claim about the effects of selecting English and French in an attempt to reconstruct a form of Indo-European as George Grace above made about evidence for reconstructing Proto-Austronesian from individual modern Austronesian languages, and he is suggesting what we might infer from it superficially about possible relationships between some languages. (There are rather more than 84 Germanic stems to be found in French as loans, incidentally, while the number of forms taken from French into English runs into the thousands; the corpus of truly cognate material existing between French and English is the smallest of the tranches of morphemic material which they share.)

But there are other ways of viewing Greenberg's little dataset to diachronic advantage. If, despite our knowledge of the sound-changes and other changes which have occurred within Romance and Germanic, and between Romance (or rather, its ancestor Latin) and Germanic as the result of their splitting off from Indo-European several millennia ago, the concrete fabric evidence for the relationship between modern English and modern French comes down to little more than these 84 cognate sets, then perhaps we should not be surprised if we find that the concrete evidence to link any two groups within the Penutian hypothesis is rather sparse. After all, these attempts to link Penutian languages involve languages which are far less deeply documented and far less well-known than French and English, or French and English's respective ancestors, are. Maybe the scanty French-English dataset, in which over half the forms given also appear as cognates on the Swadesh 100- and 200-word lists (amounting to 22%, give or take 1%), is a simulacrum of what the cognate sets of two languages look like after several millennia, and it can be used in that way as an object lesson. Of course, the vast majority of words which because of their similarity would lead the unwary observer to think that French and English are related are actually similar because they are English loans from French which were taken over within the last millennium. Here, as in other cases, the effects of linguistic diffusion bedizen the unsuspecting.

The fact that we have ample ancient attestation of several Indo-European languages is what makes so much of the difference between the task of reconstructing Indo-European and reconstructing (and indeed proving the validity of) Penutian. If we were to assume that Penutian as it is generally conceived of had existed as a genetic unit, and if we further assumed that it split into various branches at about the same time as the separating out of the major groupings of Indo-European (whatever they really were), then, other things being equal, we might imagine that the modern 'Penutian' languages would appear to be as closely or as distantly related to one another from the point of brute inspection as English and French, or Albanian and Welsh.

Within Indo-European we have the kinds of attestations which will allow us to demonstrate deeper and less easily observable genetic relationships, but we don't have this for Penutian. If Penutian is a genetic unit, then the primary branchings which connect the various families and macro-families, and the original Penutian stemma itself, are lost from our sight because of our inability to reconstruct them, whereas in Indo-European we can see these primary branches much more clearly because of the vastly richer and chronologically
deeper historical record. As Hamp (1979) put it, with Indo-European we are the position to show that Greek mătĭ and Albanian sy, both meaning 'eye', are related, whereas with the task of interrelating most American languages we are still at the pater-fader stage.

Even though the concentration in the examination both of Penutian above and Indo-European is on findings from lexical data, which can be notoriously liable to change and replacement, we can at least claim that Indo-European as reconstructed, and (thanks to Dyen, Kruskal and Black 1992) as its divergences among the modern languages have been plotted, can be used as a kind of 'working model'. Against this we can optimistically project what degrees of difference the processes of filiation and divergence within Penutian would have resulted in, if Penutian were to be of a comparable time-depth to Indo-European.

One of the major differences between our records of Penutian languages and those of Indo-European languages is that in the case of Penutian, most of the material has been gathered within the past 150-200 years, so that what we have are broadly contemporaneous records of a large number of languages. This is not the case with Indo-European, where almost three and a half millennia separate the first attestations of Anatolian and Albanian, and where even conservative branches such as Baltic and Slavic have been attested only for about 700 years in the first case and less than 1200 years in the second. Over a millennium, and often more, separates the data from Latin and those from Old Church Slavonic which are generally used in Indo-European comparative work. Nobody who is engaged in diachronic linguistics ever seems to compare Old Church Slavonic with, say, Old French, which is its approximate contemporary.

The degree of divergence between two modern Indo-European languages goes as far down as a shared cognition rate of 6.8% between (Iranian) Pashto and Albanian (Dyen, Kruskal and Black 1992:117), and we see on the same page that even between Iranian languages, the well-established branch which seems most deeply internally divergent, the rate of cognition dips to 17% between Pashto and Ossetic. (Compare the 24% which separates Catalan and German, mentioned above.)

Of course, none of this means that we must absolutely not be barking up the wrong family-tree in attempting to solder together the disparate strands of the Penutian hypothesis if there should turn out to be, sub specie aeternitatis, no good reason for connecting them genetically.

7. INHERITANCE AND DIFFUSION IN SOME FORMS OF 'PLATEAU PENUTIAN'. Coast Oregon Penutian is not the only Penutian hesion which exhibits features which could be of interest to more general historical linguists. We might also mention some considerations which have to do with the interrelationships of Molala, Cayuse, Klamath and Sahaptian, this last comprising the Sahaptin dialects plus Nez Perce, which, with or without Maiduan in California, have been held to comprise a group called Plateau Penutian. My primary interest here is in the relationship between Molala and Cayuse and with their diffusional connections with other groups, and the question of what among the similarities between Cayuse and Molala is inherited material and what is diffused. This is a topic which has been discussed before, most notably and most copiously in Rigsby (1969).

Berman (1996:16-20) has listed some 25 forms of core vocabulary (including a number of frequently-used verbs) which are shared by Molala and at least one other language in his formulation of Plateau Penutian (a formulation which excludes Maiduan). He has also (Berman 1996:20-22) drawn attention to the presence of a number of lexical items, which he regards as loans into Molala, which occur between Molala (which was spoken in two
discontinuous bands, Northern and Southern Molala) and other languages, especially Plateau Penutian languages such as Klamath to the south and Sahaptian varieties to the northeast. These number 31 items, some of which may actually be old cognates and a few of which may be examples of onomatopoeia.

Some of the similarities between forms in various potential members of Plateau Penutian, especially forms found in both Molala and other languages, can be accounted for by appeal to linguistic diffusion. Sahaptian-speakers, both Umatilla Sahaptins and speakers of Lower Nez Perce, played quite an important role in the later linguistic history of the Cayuses, but there have been Sahaptian-Molala connections too, especially among people who were classified as Southern Molalas (although our linguistic, and to a slightly lesser extent, our ethnographic information on Molala is derived exclusively from Northern Molalas). We note for instance that the parents of Henry Zenk's Chinook Jargon (CJ) consultant Esther Jones LaBonte were a Sahaptin and CJ-speaking mother and a Molala father, Douglas Jones, who also spoke CJ and Klamath in addition to Molala (Zenk and Rigsby 1998:444). We may assume on geographical grounds that Sahaptian languages mostly influenced Northern Molala and that Klamath mostly influenced Southern Molala, but we have no linguistic data from the Southern Molalas. The last speaker of Molala, Fred Yelkes, died in 1958 (Berman 1996:1).

Diffusion has also played a role in the history of Cayuse, as some of Berman's examples show (Berman 1996:24-26). The original Cayuse language was being replaced by a form of Lower Nez Perce among the Cayuses even in the early 1840s (as Hale 1846:214 pointed out), and the last fluent speakers who were capable of constructing sentences are supposed to have died in the 1930s. We have very little material from them, as these last speakers were unwilling to speak their language to investigators. But what little we do have from Nez Perce and Sahaptin-speakers, often themselves of part-Cayuse extraction, who had heard Cayuse spoken and who remembered a few words and phrases, suggests that its segmental phonology was fairly typical for the Plateau linguistic area, in its possession of velar and uvular fricatives and of glottalization. Both of these are features which would not otherwise have been retrievable from the few available naively-recorded White records of the language. (The material on Cayuse that is in public domain is mostly available in Rigsby 1969.)

As an aside, we should note the fact that, whether it be through borrowing, direct or indirect inheritance, or through a combination of these, Molala and Cayuse share 31 out of the 108 items for which I was able to find glosses for both of them on the Swadesh list. This is more than Alsea and Hanis do, and more than Molala and Klamath appear to do either. Pro-rata that is a little more than the proportion which, according to Dyen, Kruskal and Black (1992), the modern Indic languages Marathi and Romani share, and these latter are two languages which nobody would subgroup separately. But I checked the same Swadesh lists for Molala and Santiam Kalapuya as others had used for Alsea, Hanis and the like, and I was unable to find more than 6-7 percent of even broadly inspectional resemblances on the two lists. The only shared salient feature which seems to mark these two languages, Molala and Santiam (as a representative of Kalapuyan), out together against others in the region is their possession of the sound $f$, which, at least in Kalapuya, is a secondary development from $xw$, and its being shared with Molala looks like the result of areal diffusion—from which direction I do not know, although Cayuse also has $f$, while Cayuse and Molala, alone of languages on the Plateau, also have the velar nasal as a probable phoneme (Kinkade et al. 1998:61).

For their part Molala and Cayuse may not constitute a valid subgrouping but they seem to share more resemblant forms (not necessarily cognates!) with one another than with either.
does with Klamath or with the Sahaptian languages or with anything else. However, when I examined the two pages of the Scots Hudson Bay trader Samuel Black's 20-odd page Walla Walla-Nez Perce-Cayuse vocabulary of 1829, the earliest and fullest record of Cayuse, which have been made available publicly (these pages are presented at Goddard 1996:41, Kinkade, Elmendorf, Aoki and Rigsby 1998:62), I found that although I was able to find equivalents or parallels in my (far from complete) Molala lexical materials for some two-thirds of the Cayuse glosses, only about six of the c. 50 Cayuse forms for which I had Molala equivalents looked even like inspitional resemblances between Molala and Cayuse. None of these were words which had not already been noticed in Rigsby (1966). Regretfully, I was unable to add any new Cayuse-Molala parallels to what Rigsby had found.

The kinds of linguistic resemblances which obtain between Molala and Cayuse are similar in a number of respects to those found between any of the three branches of Coast Oregon Penutian. (They number about sixty, of which maybe 60% are either cognates or extremely deep borrowings, while the rest seem to me to be more readily diffused material which has passed between Molala and Cayuse.) Firstly, they are primarily lexical resemblances. This is an important—and, given our knowledge of Cayuse structure, an unsurprising—consideration. We know little of Cayuse grammar, and what little evidence we have of it we are in no good position to interpret. The temptation to read Molala parallels into the two partial paradigms of Cayuse verbs which H. W. Henshaw collected and which Rigsby (1966:371) discusses, is strong (although the similarities with Molala forms are scanty), but it must be resisted. So must the temptation to try to apply some Indo-European-style 'combinatory method' in an attempt to make sense of the Cayuse verbal system, by appealing to assumed corroborative evidence from assumed Penutian congeneres. I for one find the use of the combinatory method far too tentative and the results far too dubious even when it is applied to the task of reading inscriptions in dead Indo-European languages such as Phrygian—let alone if its use were to be transferred into a field such as Penutian.

Furthermore, these similarities of word-shapes are dispersed in an unusual pattern throughout the lexicon. There is a small number of strongly resemblant forms in the basic lexicon, including some verb-stems as well as nouns, but the majority of resemblant forms seem to be found in more peripheral areas of the culturally-shared lexicon. I refer in this case to those parts of the lexicon which relate to everyday phenomena which would have been known to most groups and which are not culturally-diffused traits. Ressemblant forms are also to be found in some parts of the culturally-diffused lexicon, such as a form meaning 'trousers' which in Molala is mä'yug and which Rigsby, citing from Henshaw, gives for Cayuse as më-r-yük (Rigsby 1966:377). There may also be loans into Cayuse from Sahaptian languages (and maybe in the other direction); thus a Cayuse form such as ti-man-uk-tai-yu (Berman 1996:23), which Berman connects with a term meaning 'tattoo-marks', resembles Sahaptin timaš 'book'.

Additionally, the resemblant forms between Molala and other potentially Plateau Penutian languages, including Cayuse, tend to include a rather large number of ecological terms, especially the names of animals. This is also the case with a number of forms of animal names which look similar in Molala and in Sahaptian languages. Molala is an isolate; Sahaptin as a language cluster shows a fairly small degree of internal divergence, and many of these mysterious Molala animal names are partially analyzable in terms at least of possessing a recognizably Sahaptin affix, while others are not. Among these there may be loan material in both directions, and also words loaned to both languages from a third language, although we cannot be certain. Whistler (1977) has demonstrated the importance
of presenting a historical analysis of ecological vocabulary in Penutian languages in California because of the light which it can shed on the patterns of historical migrations.

When this principle is applied to the Molala-Sahaptin/Klamath shared tranche of lexicon, we see that of the 31 forms listed, 16 relate to flora and fauna. Klamath ecological loans into Molala include forms with the following meanings: 'brown bear', 'white-tailed deer', 'dog', 'duck', 'fish-hawk', 'frog' (possibly influenced by onomatopoeia), 'huckleberry', 'mountain lion', 'raccoon', 'robin', 'gray squirrel', 'weasel'. Sahaptin loans into Molala with an ecological meaning are 'cougar', 'ground squirrel' and 'steelhead trout' from Sahaptin, and the possibly onomatopoeic form for 'mappie', taken from Nez Perce. In fact it is possible that the form for 'steelhead trout', Sahaptin šuśainš, Molala susa: 'inc, is actually a loan in the other direction, since it involves a formant -inc which forms animal names in Molala (as Berman 1996: 22 points out), and which is also found in other Molala words which do not appear to be loans, such as pskaync 'beard' (cited in Rigsby 1969). But the list above, with its itemization of frequently-occurring species as loanwords, is surprising, especially if one recalls (following Kinkade et al. 1998: 69) that the Native peoples of the Plateau have no recollection of ever having migrated thither from somewhere else—somewhere with a different ecology.

8. WILLAMETTE PENUTIAN AS A 'DEAD STOCK'. Parallels between the nature of the relationship of the three groups within COP and between various members of Plateau Penutian (but especially between Cayuse and Molala) may also be seen in the case of Takelma and Kalapuyan, a grouping which has occasionally been dubbed 'Willamette Penutian'. The most recent discussion (Tarpent and Kendall 1998) suggests that Willamette Penutian is a spent group. Tarpent and Kendall maintain that there is too little linguistic evidence to link Takelma and Kalapuyan together as being more closely related to one another than they are to other Penutian (or any other) languages. That is to say, there is insufficient morphological and lexical evidence to show that they constitute a non-trivial grouping within Penutian. (It should be pointed out, though, that there is little available on Kalapuyan languages which is explicit and directly user-friendly enough about the morphology of these languages to enable most people to make statements about the structure of Kalapuyan.)

This finding runs counter to the observation in Berman (1988: 3-4), which states that there is a special relationship between Takelma and Kalapuyan, that it is genetic, and that it is obscured by the considerable structural and typological differences between the two families, which affect most parts of grammar including both noun and verb morphology. Berman (1988: 4) points out that morphological similarities between the two languages are few, but that there are some significant cognate sets. One involves the suppletive stem -ham, which means 'father' in both Takelma and Kalapuya, and which according to Berman is used only with 1st and 2nd person possessors in both (although it must be said that the Northern Kalapuyan paradigmatic data given in Hale 1846: 565 shows this stem being used with all persons except 1sg). This looks like a well-preserved archaism and one which suggests common origin.

This whole debate, however, may be simply an argument over a matter of degree of depth in the relationship, and a debate over taxonomic terminology. The relationship of the three branches of Kalapuyan to one another is quite obvious, even to a layperson, upon the presentation of sets of comparanda. The relationship of Takelma (which itself shows a small amount of internal dialectal divergence) to this Kalapuyan family is a much more difficult claim to sustain with evidence. What has not been discussed so far—although it may be a
non-question—is whether Takelma is equidistantly related to all three branches of Kalapuyan, or whether it favors one of them over the other two (or indeed whether one of the Kalapuyan branches is less similar to Takelma than the other two are).

The fact that a special Takelman-Kalapuyan relationship is difficult to prove, however, in and of itself does not invalidate the claim that the two groups of languages are related. And indeed it does not invalidate the claim that Takelma and Kalapuyan enjoy a 'special relationship', namely that they are each other's closest genetic relatives. But it may be the case that although they are each other's closest kin, they are still extremely distantly related, so that the fact of their constituting a single genetic node at some non-trivial point is not very significant. It is also important to remember that it is impossible to prove that any two languages, both of which have been passed on from one generation of speakers to the next via genetic transmission, are not related.

For all that we know, the same may be said of Cayuse and Molala as was said of Takelma and Kalapuyan. They too may once have been the same language, but if so, this period of unity was such a long time ago that next to no evidence of it remains. And in the case of Cayuse and Molala, what evidence does remain may have been obscured by subsequent layers of direct and indirect borrowings from one language to another, from Cayuse to Molala and from Molala to Cayuse, and by borrowings by both languages (singly or jointly) from other sources. Kinkade et al. (1998: 61) cogently suggest that many of the resemblant forms shared between Molala and Cayuse may date from the period when the two languages were spoken contiguously (and, we assume when their territory had not yet been interrupted by the movement of Sahaptian-speakers).

9. A NOTE ON SOCIAL FACTORS IN PRE-INVASION COASTAL OREGON. We have seen that various kinds of linguistic change have taken place in a number of 'Penutian' languages in what is now Oregon, and that these changes have often made it difficult for investigators to classify these languages in a more orderly and fine-grained fashion. Comparisons with what has happened in the course of the histories of Indo-European and its daughter languages are illustrative, but they leave a number of questions unresolved, because the picture in Oregon is one of groups of languages between which it is difficult to decide whether similarities are due to a state of initial cognation which has been followed by extensive time periods of separation, or to the borrowing of fabric to an extent which is rarely if ever found in Indo-European language, or to both. All this leads to a situation not unlike that connecting French and English as mentioned above, where the evidence for genetic relationship is present, but has been obscured by extensive waves of linguistic diffusion which have gone in both directions, but where they have moved especially strongly from French to English.

Since language is (among many other things) a form of social behavior, I feel that an attempt towards an appreciation of the situation calls for the application of a Milrovian style of social network modeling of sociolinguistics, of the kind that has been applied to speech communities in Belfast (Milroy 1987), but with some important modifications. The primary modification is the recognition that the clusters of linguistic variables which are of interest in this case, unlike those in Belfast, actually constitute different languages which may not be immediately comprehensible to some people with less strong links to a particular speech community, so that these languages need to be learned somehow. Furthermore, few or none of the languages which were in contact in Oregon at that time would have been widely spoken in any other area; we are dealing in these regions with a mosaic of small speech communities. In Belfast, by contrast, linguistic contact by Milroy's consultants with other
speakers of what is to members of communities such as Clonard, the Hammer and Ballymacarett clearly the same language as their own (namely 'English') is plentiful in the extreme.

McLendon (1980) has presented a careful sociolinguistically-oriented picture of how a Hokan language, Eastern Pomo of northern California, came to die out, as the multilingual network in which its speakers had participated was weakened more and more by the onrush of Spanish and especially of English. This, and other sociolinguistic models, such as the 'social network' theory proposed by Milroy (1987), and insights from the pioneering work on language shift by Thomason and Kaufman (1988), can be used to shed light on the dynamics which brought about both language shift and language change among speakers of Oregon Penutian languages in the course of their post-contact history.

There are a number of socially-based reasons for the extensive degree of linguistic diffusion among Oregon Penutian languages in the times before extensive White contact, and few of them are exclusive to Oregon. We might suggest at least the following:

a) a tradition of the tacit acceptance of multilingualism as a utilitarian adjunct to everyday living within many communities,
b) the practice of intermarriage at various social levels (between chiefly families, concubinage between chiefs and slaves, and marriages among slaves) between speakers of different languages, with consequent L2 learning on the part of the mobile spouse, which itself was a consequence of

c) the limited choice of acceptable potential spouses within a small community (and within a small speech community),
d) the fact that the subsistence and settlement patterns in this region favored small winter villages in general, rather than the larger towns of the Southeastern US,
e) the presence of alloglot people in many villages (at opposing ends of the social scale, both slaves and shamans were often alloglot immigrants in Oregon tribal villages),
f) the important role of rivers and of villages situated near navigable rivers (including places such as The Dalles) in cross-country traffic, including the maintenance of what were often extensive trade networks for the provision of valued trade goods,
g) the superior technology and greater range of social possibilities of groups with bigger and sturdier canoes, such as the Chinooks.

In addition, the linguistic effects of the reformulation of new multiplex social networks due to intermarriage, and of the custom of addressing individuals by kin terms rather than by their given names, may both explain the wide spread of a number of kin terms with broadly similar shapes and functions across the Coast Oregon Penutian languages.

The population figures provided by Boyd (1990: 136) for Alseans, Siuslawans and Coosans at some unspecified point (but apparently around 1775) are 3060, 2100 and 2250 respectively. These are all a fraction, and a small fraction at that, of the size of populations of speakers of most European languages at that time. It would have been possible for a peasant in, say, Tsarist-dominated parts of Poland, to go throughout life without ever meeting another peasant of their status who spoke a different language, let alone marrying someone whose native language was different from their own. In contrast, and even before the balkanization which led to many speakers of Oregon Penutian languages being exiled to Grand Ronde or Siletz in the 1850s, interaction at many social levels between speakers of different Oregon Penutian languages and indeed different Native languages was a much more everyday occurrence. The very fact that the Cayuses were switching to Nez Perce, as pointed out above, attests to the power of language shift in the era before Native-White contact.
The sociolinguistic history of Native Oregon has yet to be written. If and when it is written, it will show that multilingualism of various kinds, especially in the early post-contact generations, was widespread, as indeed was a history of shifting from one Native language to another (and in these cases, making a further shift to English; Swadesh worked with the last speakers of several languages during his Penutian Vocabulary Survey). The people who worked with Morris Swadesh on the Penutian Vocabulary Survey of 1953 (which was documented in Swadesh 1954) often had life histories and ancestries which indicated this.

For example, we know that Swadesh's Nez Perce consultant Philip Geyer had had some Cayuse-speaking ancestors, and he remembered a little bit of Cayuse which can be found in Rigsby (1969); in the 1930s Swadesh had tried without success to get Cayuse data of his own, and had collected materials in the variety of Nez Perce which the Cayuses were now using. John B. Hudson, the last person who could speak Santiam Kalapuya, actually tells Swadesh on tape that his mother's Yonkalla Kalapuya speech was unintelligible to his Santiam-speaking father, while during the same investigations Swadesh also secured Wasco-Wishram vocabulary from Jasper Tufti, who was apparently of part-Molala origin. The maternal grandmother of Laura Hodgekiss Metcalf, who worked as Swadesh's Miluk consultant in this exercise, was known by an Upper Coquille Athabaskan name which has been spelt Ch'ishggeyuyu, and which has the meaning of 'old woman' (it was apparently given because of her precocious interest in the customs of her people). From an earlier period of investigation, Victoria Howard, who provided Melville Jacobs with some Molala data (Berman 1996: 2), knew some Molala, and spoke English, Chinook Jargon and (most famously) Clackamas fluently, and her husband Eustace Howard knew at least English and Santiam Kalapuya. Much of Leo Frachtenberg's work on Lower Umpqua (Frachtenberg 1922b) was carried out with William Smith, an Alsea who had a working knowledge of Lower Umpqua and some English, in addition to fluent Alsea, and who conversed with his Lower Umpqua wife Louisa (another of Frachtenberg's consultants) in Chinook Jargon.

10. CONCLUSION AND A VIEW TOWARDS THE FUTURE. To sum up: all sorts of linguistic evidence are of interest to the Penutianist who is trying to gain more insight into the history of Penutian languages, although these different types of evidence tell the linguist different things. Typological evidence is interesting and a typological dossier for these languages may be worth assembling as a preliminary step, but even the presence of clusters of marked typological features does not of itself allow us to distinguish any Penutian from non-Penutian languages. The corollary is also true: the absence of these features in a particular language does not mean that the language is not Penutian, simply because it does not obey an assumed 'Penutian type', as the history of Chinookan shows. More important than typological 'pattern' evidence, whether syntactic or phonological or whatever, is the 'fabric' evidence which is provided by the presence of various kinds of morphemes.

Some kinds of linguistic fabric evidence point to the likelihood of a history of common descent—the presence in more than one language family of the forms of a number of high-frequency shared verbs, for instance. But much of the evidence points more readily to a history of shared contact, which was at least in part enforced because of the relative smallness of size of most speech communities in Native Oregon. The opportunities for contact via linguistic diffusion, and (when the social occasion was propitious) for multilingualism, second-language learning and use, and a tendency towards shift from one language to another, were plentiful, and both diffusion and shift appear to have happened to a degree far in excess of what we find in Indo-European societies, which in any case constitute much
larger speech-communities. Consequently, the kinds of material which have been diffused in Oregon Penutian languages are much broader in range and scope than those which are normally found diffused in Indo-European languages, and this naturally has consequences for the diachronic study of such languages. In some cases the linguistic material which has been diffused from various directions in the course of this contact history can swamp by sheer force of quantity that linguistic evidence from the historical period which is actually indicative of shared origins. For all we know this may be the case with the shared material which one finds existing between Molala and Cayuse.

Some of the kinds of 'fabric' which works as being available to the investigator as immediate and instant evidence of genetic relationship in some language families, such as a shared set of basic lower numerals, or kinship terminology, may be effective in Indo-European or in Semitic. However, as markers of genetic affinity they will not work effectively in Oregon Penutian languages, where such concepts are often, or even generally, found to be expressed by the use of locally-diffused material. What works best to provide initial proof of genetic relationships in the Old World need not be assumed always to work as unexceptionably in the New World, as data which have been used to link the Coast Oregon Penutian languages suggest. (However, I would argue that even Indo-European is a language family which still has a number of lessons to teach people who are interested in methods of diachronic investigation, in terms of telling us what kinds of linguistic change are possible.)

The reverse claim may also apply, but historical linguists have been extremely slow to apply those techniques which have been used to prove cognacy among American or Australian or other 'exotic' languages, to the elucidation of first-order relationships among Indo-European or other Continental Eurasian language groups. (Let us remember that there is still not a consensus classification of the branches of Indo-European which most Indo-Europeanists accept.) We can only speculate about what Proto-Indo-European would have looked like if it had been reconstructed using only the methods which have been designed to retrieve earlier forms of American macrofamilies, although the example set by Dyen, Kruskal and Black (1992) demonstrates the kinds of cladistic and other results which one can obtain by comparing one feature of numerous languages, namely Swadesh-list lexicon, and when one is not working to any classificatory preconceptions. If Penutian does exist as the ancestor of its 'classical' components, it may be a few millennia deeper than Indo-European is, but it is useful to see the results of an experiment at cladistic reconstruction which is carried out using equivalents of the kind of data which are all that are available to people working with a system such as Penutian, which contains so many single-language branches.

That there is room for fruitful dialogue between Indo-Europeanists and others on this matter is, however, beyond doubt. We need both to recognize the relative inapplicability for some cases of genetic relationship of some of the tools in the European diachronic toolkit (although most or all should be tried out first), and by contrast, to extrapolate from what we have found effective in 'exotic' cases, in order to increase the number and range of instruments which we may use.

Nevertheless the presence of such diffused material in a language is significant in another way. It does tell us a lot more about the range and depth of contact between groups speaking different languages, and about the possibility of language shift (often from one Penutian language to another, and often as a result of exogamy) at some point in history. The most reliable elements of fabric which will point to clear linguistic affinity in at least Oregon Penutian languages (but they may not be universal cross-linguistically in their diagnostic reliability) are personal affixes, whether they be prefixed or suffixed to the stem, as is
suggested by material in Coast Oregon Penutian languages, and clusters of morphological irregularity or cases of suppletion, as are found shared in Kalapuyan and Takelma. These kinds of evidence generally work very effectively for Indo-European languages as well.

And in any case, it is true that the role of diffusion as a factor which contributes towards the momentum of linguistic change has been seriously underplayed in the historical linguistics of many major families, as have the kinds of items which can be and which are diffused. Simply because a certain kind of item, say a bound inflectional morpheme, is never borrowed among the members of one linguistic family does not mean that borrowing this item from one language into another is a practice which is embargoed globally.

Indeed, if as much attention had been paid to the manifestations and consequences of diffusion, and of uniquely shared innovations, among Penutian languages, as has been given to more purely typologically-based speculations about their interrelationships, we might be rather more advanced in our attempts to reconstruct what Proto-Penutian (had it existed) might have looked like.

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HYMES, DELL. 1964. Evidence for Penutian in lexical sets with initial *c- and *s-.


THE STORY OF JOHNNY BEAR:
A EUROPEAN TALE IN 'IIPAY KUMEYAAY

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INTRODUCTION. The text presented in this paper was collected by me in the course of fieldwork I undertook in the spring of 1963 and again in the spring of 1964 to document the language previously called Diegueño in the literature, since no serious documentation on the language had been done at that time. This work was made possible with support from the Survey of California Indian Languages under the direction of Prof. Mary Haas at the University of California, Berkeley. My fieldwork in San Diego County helped demonstrate the great linguistic diversity of the area, where differences among the various reservations prompted me to revise the classification into three closely related languages: 'iipay, Tiipay, and Kumeyaay (Langdon 1990), all members of a subgroup of the Yuman family also now called Kumeyaay. Its closest relative in the Yuman family is Cocopa.¹

My main consultant was Mr.Ted Couro (1890-1975) of the Mesa Grande reservation in San Diego County, who was then residing in the city of Escondido where I worked with him everyday in my early field work, and whenever time permitted after I moved to San Diego permanently. I worked with him until his death, when he was 85 years old. It is only now, 25 years after his death that I have been able to work with my notes and tapes without crying. Even though Mr. Couro had not spoken 'iipay for many years, he remembered numerous stories which we spent long hours recording and analyzing. Obviously, none of this would have been possible without him and I dedicate this paper to his memory.

He was an extraordinary man: polyglot, fluent speaker of Mesa Grande 'iipay, Spanish, and English, artist, poet, musician, inspiring teacher. He was concerned about the fact that his language was endangered and strongly motivated to have it preserved and made available to all who might be interested, as the last part of this story will attest.

This story is one of a fairly large repertoire of tales of European origin which have become a part of local oral Indian literature. Several stories of this type have been recorded by fieldworkers in the Kumeyaay area. A typical one is the Tar Baby Story which is attested in numerous Indian languages (see for example Hinton 1976 for a version in La Huerta Tiipay).

The Story of Johnny Bear (as Ted aptly called it) is a well-known tale of clearly European origin which has diffused in various forms into a large area of the United States and Mexico (Paredes 1970, Thompson 1939, West 1988). It is known variously as Juan Oso, John of the Bear (Juan del Oso), etc. and has been documented in a large number of Indian languages, as well as French and Spanish (Thompson 1939:334-344). The manner of transmission of the version presented here is clearly through Spanish, which until very recently many Indians in San Diego County spoke fluently. Mr. Couro himself remarked on this by wondering why there were so many Spanish words in the story. More of this below.

The text itself is transcribed in section 7 of this paper both in 'iipay and in English. Mr. Couro’s 'iipay version is given as I transcribed it with him and from his tape-recorded

¹ The Yuman language family consists of the following subgroups and languages: PAI: Upland Yuman (Havasupai, Hualapai, Yavapai), Paipai; RIVER: Mojave, Yuma, Maricopa; DELTA-CALIFORNIA: Cocopa, Kumeyaay ('iipay, Kumeyaay, Tiipay); KILIWA.
performance, in the practical orthography which I designed for a wonderful language class he taught that was sponsored by Palomar College in San Diego County. The text has been divided into appropriate paragraphs, each first given in 'lipay and followed immediately by its English version. In the translation, I attempt to reflect as much as possible the style of the 'lipay version while simultaneously keeping it palatable to an English-speaking audience.

In the following sections, I discuss (1) the outline of the known European versions, (2) the ways in which Mr. Couro’s version varies in content from the European model, (3) the form of the Spanish words and phrases found in the 'lipay text, (4) the Spanish words themselves, (5) the stylistic aspects of the story and the rhetorical devices Mr. Couro used to liven up the tale, and (6) the time depth of the story in Mr. Couro’s family tradition.

1. Outline of European Story Content. A woman is abducted by a bear and later gives birth to a child who is half human, half bear, hence his name. He grows up enormously strong. He is sent to school where he is disruptive and fights with the other children and injures them. He is sent away carrying a magic cane. He meets three strong men who join him.

The four men are hired to do some work and each day one of them is selected to stay in camp and prepare food. Every day, when the food is ready a stranger comes up and steals the food. They follow him in the underworld where several princesses have been kept prisoners by monsters and the hero rescues them. He receives tokens from them. The hero’s companions bring up the princesses and abandon the hero in the underworld. He obtains help from a magician, is carried to the upper world by an eagle and has to give him some of his own flesh before he can be carried up all the way. The hero goes to the court, presents the tokens to the king and marries the prettiest princess.

2. Differences Between the European and Kumeyaay Stories. No reference is made in the Kumeyaay story to the bear/man nature of the hero, only that he is very strong, thus accounting for his name. The hero’s mother is never mentioned, only his father. The three companions he finds have Spanish names describing their individual strengths. The man who steals the food is the Devil and lives in the underworld. There is no mention at all of the king and princesses. The hero kills the Devil. He is abandoned by his companions and is rescued from the underworld by a buzzard who returns him to the upper world, but he can’t find his companions. The story ends abruptly, as Kumeyaay stories often do.

3. Spanish Loans in the Kumeyaay Version. As Mr. Couro mentions in his preamble to the Kumeyaay text, there are many Spanish words in this story. They are listed below in Kumeyaay orthography to indicate the differences between the Spanish words and their Kumeyaay version. As usual for Spanish loans in the language, the stressed vowel of the Spanish word determines the final syllable of the Kumeyaay version by dropping the unstressed part of the last Spanish syllable, to conform to the requirement that basic Kumeyaay words have  

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2 The conventions of the practical orthography that may require explanation are: ' is glotal stop, double vowels are long, tt is an apico-alveolar stop contrasting with dental t, nn is an alveolar nasal contrasting with dental n; ny and ly are palatal n and l respectively, II and I are voiceless laterals contrasting with l and 1, h is velar [x], e is schwa [ə], hw and kw are labialized h and k; r is rather like English r, rr is like Spanish trilled r, ch is an affricate like English ch, sh is like English sh but slightly retroflex, native full vowels are a,a, i, ii, u, uu. O and oo are very rare in native words, but common in Spanish loans; ee, e, d, g, f, g, are found only in foreign words, v occurs only in the unstressed forms of demonstrative suffixes or in foreign words.
stress on the last syllable. The only further syllables that Kumeyaay words can contain are unaccented grammatical suffixes, examples of which are given below. Sometimes, initial Spanish syllables, if consisting only of a vowel, are also omitted. Note that these Spanish words are treated grammatically as if they were Kumeyaay words: they have plural forms that conform to some Kumeyaay patterns, they have Kumeyaay personal prefixes and demonstrative and case suffixes like ordinary Kumeyaay words. They are given below in alphabetical order in the various forms in which they occur in the text and are analyzed morphologically. The number of instances of a particular word or phrase in the story is also given in parentheses. Included in these words are the names of characters in the story. Not surprisingly, the hero’s name is the most often used Spanish name in the story. Note the inclusion of the Spanish article in some nouns.


5. Stylistic Characteristics of the ‘lipay Kumeyaay Text. The text is enlivened by a number of stylistic devices which are characteristic of Kumeyaay story telling. First of all, there is a tremendous amount of direct discourse, where the characters speak essentially in their own voices. This necessitates the profuse use of quote and unquote marks, since the quoted speech is often interrupted by such phrases as ‘he said’, some of which may also be repeated at the end of the quotes as well. Interestingly enough, direct discourse is used when Hwan Osiit is talking to himself (paragraph 13). No instance of indirect discourse is found, although Kumeyaay has syntactic ways of indicating this. The identity of the person speaking is often not overtly indicated and is sometimes ambiguous. I have specified the speaker only when the context did not make clear who was speaking. A common stylistic device which has been reported in a number of American Indian languages is a construction which repeats the last verb of a preceding sentence (‘lipay Kumeyaay is a verb-final language), resulting in such passages as “...he left. When he left, ...” Vivid descriptions are provided by the use of a number of what I have called (Langdon 1977) ‘expressive say constructions’ which consist of an uninflated verb form followed by an inflected form of the verb ‘to say’. They convey the feeling that the action
in question occurs before one’s very eyes. Examples: *laaw wii* ‘turns around quickly’, *pull wii* ‘exactly’, *hall wii* ‘in one leap’, *rur wii* ‘swoops down’, *tulytuly wii* ‘hops along’ and my great favorite *'uy wii* ‘there is nothing there’ which often occurs in the longer and more dramatic phrase *'uy wii neso'on* ‘there’s nothing there, it’s all gone.’

6. **HOW LONG HAS THIS STORY BEEN TOLD IN KUMEYAAY?** I can only assume that this story entered the Kumeyaay repertoire through bilingual speakers and that the original was told in Spanish. This requires a period of contact when Kumeyaay speakers already knew Spanish. It is unlikely that many Europeans or Mexicans learned to speak Kumeyaay sufficiently to tell a complex story such as this one, but Spanish became a lingua franca in much of native California. In the particular case of Mr. Couro’s family, he told me that he heard the story from his wife’s grandmoher. Mr. Couro’s wife, Lillie, was born in 1898. Her grandmother (assuming generations of 20 years) must have been born toward the mid-1800’s and it is possible that the story presented here was already known in her family at that time, which gives it a minimum time depth at the time I recorded it (in 1964) of well over 100 years. To my knowledge, there are no extant native accounts of Indian life in the Kumeyaay area during the Spanish and Mexican periods. Some more recent reminiscences dating back from the period before 1900 can be found in Couro (1975), and Langdon and Gastil (1999-2000). For information about the period 1850-1880 in the San Diego area, see Carrico (1987). For linguistic information on the Mesa Grande dialect of ‘lipay Kumeyaay, see Langdon (1970), Couro and Hutcheson (1973), and Couro, Langdon et al. (1975).

7. **THE TEXT**

7.1 **PREAMBLE BY MR. COURO.** In recording this story, I want to explain in a few words a few things concerning the story. This story has quite a few Spanish words in it. I don’t know the reason why, but that’s the way I learned the story. Maybe it is because of its being a more modern story than what I’d been telling before. We’ll do the best we can to give you more Indian words that I can find without saying too many Spanish in it if I can, but I’m not too sure ‘bout that.

7.2. **THE TEXT PARAGRAPH BY PARAGRAPH.** The text is presented paragraph by paragraph with the ‘lipay version first and the English translation immediately following. Each ‘lipay paragraph is identified at the end by its number, i.e. (1i). The English translation is followed by the notation (1e), etc.

**Hwan Osiit**

Tewaaches *'iikwich 'ehinch hayaay kwapesiiw.* Nyewaayp tewaach hekwany *'ehinkem tenerr,* *'iikwich hekwany 'ehinkem.* Nyaapum *'elymaam puuch hemay 'iikuuch,* *'emaay sepir; skweelem uuchuttem waam tewam.* *'Elymaam wellich apesiiw, kunniimii kwapesiiw; 'elymash chetooch aa-arp chechekwaapem tenam.* (1i)

**Johnny Bear**

There was a man a long time ago. He lived there and had one child, a boy child. In time, that child grew to be big, tall, and strong. His father sent him to school, and so he went there.
The kid was very bad, a truly mean one; he would hit the other children, whip them and knock them down; he kept doing it. (1e)


The teacher didn’t like it. One day he came to tell the father (and said to him): “This child of yours is very bad, you hear? I can’t have that. The rest of the parents don’t want it either. Now I’ve come to talk to you, you hear? You must do something. Some day this boy might kill a child, and that would be tragic. That’s what I came to tell you so you’ll know what to do.” He said that and left; the teacher went back home. (2e)


Then the old man told his son, he called him and told him and he came. “That’s what he said, your teacher. Listen! You are a very bad boy. You’re already grown, you’ve grown up, you’re big and strong. You grab the kids and knock them down; you hit them and throw them around. He doesn’t like that. That’s the way he was talking to me. He says you’d better not go back to school. Now I’ve been thinking. I also don’t want you to act this way. Better you leave and see the world. Go anywhere you please, get away and meet other people, find out how they’re living. Here is a cane I made; I’m giving it to you. Take this, look at it carefully and take it with you; it is inlaid with silver and gold. Take good care of it; it is very valuable.” (3e)

Then the boy said: "Alright" he said "I’ll go." So he left. One day, as he was going along, he met three men coming his way. When they met, then the men stopped and they talked; one man said, he asked the boy: "Where are you heading?" "I don’t know. I’m just walking along. I don’t know where I’m going. And you fellows, where are you going?" the boy asked. When he said that, one of the men spoke: "We are looking for work. Where there’s work to get, we’ll work. That’s what we are looking for. Do you want to go with us? If we find work, then we could all work together." "OK" said the boy, "I don’t know how to work, but anyway I can learn. Alright, let’s go!" (4e)


This boy was a lot bigger than the men. The men that were coming were very big too, but he was a lot bigger. Then he said when they left: "I was going to ask you something but I forgot." "Well, what is it?" "What do the men call you?" he said to one of the men. "My name is Ranka Piin (Arranca Piinos) 'Puller of Pines', they call me that." "What do you do?" "Oh, I’m very strong , you hear? Black oaks, white oaks, pines, any big trees that are there, I take them, pull them out, and throw them down, you hear? That’s why they call me Ranka Pinn." "OK". (5e)


Then he quickly turned to another one and asked: "What is your name?" "They call me Moova Syeeerr (Mova Sierra) 'Mountain Mover'. Big mountains like you see over there, I take them, dig them out, throw them down; I make the land flat." "Gee" he said "and you?" he asked the third man. "They call me Ranka Pyedrr (Arranca Piedras) 'Rock Puller Outer'" "OK, what do you do?" "Me? You see those big rocks high up on the mountain? I pull them out and throw them down the canyon, it doesn’t matter; little ones, it doesn’t matter." Then he said: "Let’s go!" (6e)


They started off—I don’t know how many days they spent—then they met a man coming their way. When they met him, then Hwan Osiit (that’s what they called the big boy), they made him foreman. Then he asked the man they met: “Where were you going?” “Where am I going, I say? I’m going somewhere looking for men; I have some work to do. So I need to get some men together to work.” “We are looking for work. We’re here, you see?” “OK” he said “come on with me!” “What are we going to do?” Then the man said: “See that big mountain over there?” “Yes, I see it.” “I want that levelled off. The big rocks, black oaks and everything that’s there, I want it pulled out and thrown away, tossed down the canyon. Then when we finish, this land will be all level. I want it left that way.” (7e)


“We understand that kind of work,” said Rranke Piin, “OK, let’s go!” They went, they got there, then looked at the big mountain, and then they made camp. When they made camp, then in the morning, Hwan Osiit said: “Now we go to work, hear me? Rranke Piin, you stay here and cook the food! Tomorrow at noon [the food should be ready]. You are the cook now.” “OK” said the man “I’ll do it.” (8e)


Then the men went off to work. At noon sharp the next day, the guys came back. When they got there, there was no food at all, and Rranke Piin didn’t explain anything. What happened? In the morning when he finished cooking, he finished setting the table. He was waiting for the men
to arrive. A man came over riding a black horse, he cantered up and said: "Is dinner ready?"
"Yes, all ready" said Ranka Piin, "I'm waiting for the men." "I want to eat" [said the rider].
"No, you wait. When the men get here, then you can eat." "No, I won't wait for them. I eat
now." He tied his horse and started to eat. Ranka Piin got hold of him by the hair, dragged him
and threw him over there. He dragged him and threw him, then he got up and they started to
fight again. Finally the man—who must have been the Devil—took and whipped Ranka Piin,
hit him with a stick, and knocked him down unconscious. The Devil sat down to eat and ate
everything up. When he finished all the food and got on his horse, Ranka Piin woke up. He
woke up and chased him down the ravine and caught up with him, but the Devil dropped down
into a big hole in the canyon. There was a big hole in which he disappeared as he dropped down.

(9e)

Puwk nyaapum nekemich, 'iikwich kutravaharrchvech 'esuw tenerrh umaaw nesoom.
'Esuw liisth umaaw. Nnetuumiip tenams. Teneway nyaapum maaykally nyakumetnyaally puy
Hwan Osit: “Pily maach mepaamh, meyipa, Moova Syeerr; maach mepaam, mechuhw 'esuw.’
"Hoo" wiich 'iikwichvech. Nyaapum naam trravahaarrh. Ta’urp nyanekewaykem, nyahin
rewiis——'esuw umaaw. Nyii liist tewedha umaaw. Pa’wiich 'iikwichvech puy pa’wiich 'iikwich
puy wenuwuch paak, nyekavaayvuu aannak, aachanp wesaawhem paach menyuupe; menyuupe, mat
wetoo tenam, uchehwiit wesaaw chapch waam. Puu’yuu akewii tenam, puy kuttap wehaph
waam. (10i)

When the workers returned, there was no food at all; it wasn’t ready. They were mad. There
they were and then the next day, in the morning, Hwan Osit said: “Now you, Moova Syeerr, you
will stay behind, you hear, and cook the food.” “OK” the man said. Then the others went to
work. At noon, when they came back, the same thing happened—no food. It wasn’t ready. The
same way, a man came cantering up, tied his horse, got off to eat. Moova Syeerr refused. He
refused and they fought. They fought, but the man took the food away and ate it all. It happened
the same way; he followed but the man dropped down in the hole in the canyon. (10e)

Nyakumetnyaally, nyenuyech puwiches Hwan Ositvech: “Pily maach mepaamh, meyipa?
Moova Pyeerr, mepaamh, mewiih.” Nyaapum puuch 'esuw wechuw newath, tewapem,
'iikwich chepsk wenuwuch, kavaay tullpuu puuch chepsk tullpuu wyis, nyekavaay aannak: “Nyuk
“’esaaaw, meyipa?” “Umaaw, nyii mesaawh umaaw.” “’Esaawh 'iichh, meyihip umaawa?”
“Umaawh, nyii mesaawh umaawh.” Pa’yum puy wenak, 'iikwich wenak wesaaw tewam. Moova
Pyeerr puy ewenuwuch pam. Uuniwch mat wetooch puy tenamvech, tenamvech. 'Iikwich
’echillich puuch weyuuwuch aa-arpech. Aa-arpech aarapem semiiyaay tuuyaq waam.
’Iikwichvech nyenuyech puwch wenuwuch waam. Kuttapvu kullhupem wettuk wehap.
Nyawehepem pam 'uuch Moova Pyeerr puy wenak tewam, wenak rresolyaar tewam. 'Iikwich
nyanekechem, 'esuwvech 'uy wii tuuyuuw. Hwan Osit nnemii. “Menyaawapch 'iikwich
mewiimann mekwapesiiw. 'Enyaach metenyally 'epaamh, ‘esuw 'echuwh, mewuuh.” “Hoo”
wips. (11i)

The next day, again, Hwan Osit said: “Now you will stay, you hear, Moova Pyeerrr, you are
to stay and do it.” “OK” he said. Then, as he was about to finish making the food, the same man
showed up riding his galloping horse, he rushed out in a hurry, tied up his horse and said: “Is
“Yes” said Moova Pyeedrr, “it’s just ready.” “I’m going to eat, you hear?” “No, you won’t eat!” “I said I’m going to eat, didn’t you hear me?” “No, you’re not going to eat!” He sat down, the man sat down and ate. Moova Pyeedrr came running. Right there they started to fight each other. The Devil got hold of him, he whipped him and knocked him down. He whipped him and knocked him down unconscious. The Devil again left running. He jumped down into a hole in the canyon. When he went in there, well, Moova Pyeedrr was sitting there, he sat there panting. The other men came back; when they arrived, the food was all gone. Hwan Osiit got mad: “You all, you men, you are very lazy. Tomorrow, I will stay and cook the food. You’ll see.” “OK” they said.


So the next day, Hwan Osiit stayed. He stayed and the other men went to work. Hwan Osiit made the food and just as he was about to finish the man riding a horse galloped up, stopped his horse short and got off. “Is dinner ready?” “No” said Hwan Osiit, “sit down and wait. I’ll finish right now.” “I’m not going to wait, hear? I said I was going to eat when I came.” “You’re not going to eat at all! I said the food wasn’t ready yet, didn’t you hear me?” But he grabbed everything, got hold of it, grabbed it and Hwan Osiit got hold of him, knocked him down again and again. The man jumped up and they fought each other some more, rolling over and over, and Hwan Osiit finally got hold of his cane, grabbed the man and knocked him down. The man ran and got his horse and away he went. Hwan Osiit chased him but never caught up with him as he ran down the canyon and was completely hidden.


Then Hwan Osiit said: “So that’s the way you guys are. You didn’t tell me. Well, you hear?” he said and left. He was talking to himself. When he got back to camp, the men were already there, they had come back. They were all sitting after working; they were laughing. “When that man comes back, he will give him a good licking, you hear?” They were laughing.
When they came back, dinner was all ready, so they sat down and ate. Then Hwan Osiiit said: “You guys are very bad. Now why didn’t you tell me? For these reasons, I didn’t cook. We all could have stayed here and waited [for that fellow]. Now tomorrow we will catch an animal around there and kill it. Then, we’ll do the hide. We will make a rope and we will follow the man. I will go down in the hole. You guys will do this [with me], you hear?” (14i)


Then the next day, they killed an animal, they got the hide and stripped it off. Then they made a long rope and then they tied Hwan Osiiit to it. Then they let him down. “If I don’t reach the bottom this time, I will jerk on the rope three times, and you’ll take me out. If I get there, I’ll jerk four times, so you’ll know I got there. Then you will wait for me. When I [am ready to] return, I’ll jerk the rope four times, then you take me out.” So Hwan Osiiit went in. He went down and down, but he didn’t reach the bottom. He jerked the rope three times and they pulled him out. They pulled the man out. Then they went and killed another animal and made a longer rope. Then again they let him down, on and on. After a while then he jerked the rope four times. When he did that, one of the men said: “He reached the bottom there, you hear? Now we loosen the rope.” They loosened it and left it. (15e)


It was getting rather late, then one man said: “Well, let’s leave him and go away. How can we keep waiting for him here?” They pulled the rope out, dragged it out. Hwan Osiit stayed there down below in another land. Then he went around looking for the rider, went on and on and on—how many days I don’t know—and finally he met another man riding a horse. He asked him: “One man came this way, a big man, where is he?” With his hand, the other man pointed that way to a house that was there. Then the rider left and called to Hwan Osiit: “Come here! Come with me!” Hwan Osiit followed behind, and when they reached the house, the man said: “Here is where that man lives.” “OK” he said. Then Hwan Osiit reached the door, he knocked at the door and the man came out. When he came out, Hwan Osiit said: “The other day, you were at my place. You were going to whip me, remember?” “Yes” he said “I remember.” “Now I followed you here, I came to fight you, that’s why I came. Now you and I will fight.” (16e)


The Devil man then said: “Oh, I won’t fight you. It’s already too late. Stay here at my house; tomorrow then we will fight each other.” “OK” said Hwan Osiit. So he stayed there. “And in the morning, we’ll do something else.” “OK” said Hwan Osiit. (17e)


Then Hwan Osiit stayed behind and the man gave him a bed. “Lie down right here!” he said. “In the morning we’ll do something else.” Then Hwan Osiit went, he lay in bed and was thinking: “I wonder how I can do it, hit him and finally kill him. I’m not going to give up.” he said. When it was morning, the man showed up. “Come here, let’s go and eat!” Then he ate; when he was through eating, the man went back outside, and Hwan Osiit went around the house looking it over. Everything he had was gold; sparkling, just shining all over, gold, silver, everything, all beautiful. Then also a lot of beautiful flowers. Absolutely everything he had was gold and silver—a very rich man. There he paced around back and forth, looking at everything. (18e)

Then the man called him: "Now come!" There they went into a room. When they went in, he said: "Look over here, there are lots of rifles here. Take what you want; anything you want, take it! Then we will fight against one another." (19e)


Hwan Osiit looked at everything. Finally, he didn’t want it. He said: "I don’t want it at all. I have a cane. That’s good enough. With this I will duel against you.” The man laughed and laughed and then he said: "Oh, man, do listen! If you should happen to lick me, everything that is here, this house of mine, the whole land, you take it all for yourself, you hear? If I lick you, I’ll kill you. I will bury you here and leave you.” “OK” he said. (20e)

Nyaapum waach neteppach 'uuyuki; mat wetoo. Hwan Osiit aa-arpch aarpem, aa-arpch aarapem. 'Iikwichvech mannch wiich 'illykuuyum. Nyaweyaam apekuwu Hwan Osiit puuch waam. Wampch metaawar tewaa. 'Uuch neshaaaw 'ehin tuuyuuw, puy ukuway welyak tuuyaq. (21i)

Then they went outside; they fought. Hwan Osiit knocked him down and whipped him over and over. The man jumped up toward him. When he started to reach for him, he whipped him until he was all mashed. Then Hwan Osiit left. He was walking and was very tired. There was a black oak there and he lay down under it. (21e)

As he was lying there, a buzzard came flying by—a very big bird. He flew with wings flapping and flapping. He flew around banking, looking, waiting. He was thinking that the man lying there was dead and he would eat him. Finally he landed on the ground, he hopped along till he got close. He stopped and looked at Hwan Osiit lying there. Hwan Osiit was watching him and then he said: "What do you want? Come here!" Then the buzzard in one leap jumped to him. When he did that, he said: "What are you doing here? I thought you were dead lying there; I was going to eat you." "No, I'm not dead, man. Do something! Do me a favor, hear?" "OK" said the buzzard. "Now we'll go kill a buck, we'll kill a lot of them and we'll make food. Then you'll carry me on your shoulders and take me away, take me away, you hear? There is a hole over there, I'll take you and show you. When we reach there, you'll fly upward, you'll go, you hear?" "OK" said the buzzard, "but when I fly I will go up in circles and every time I circle you must give me a piece of meat, that way we'll go on until we come out." "Alright" he said. They went that way. (22e)


They were just about to reach the top. The meat was all gone; the buzzard was dropping back. Then again they went and killed more animals. Finally, they went back again, and Hwan Osiit lay on the back of the buzzard; they again went circling, rising, rising until there was just one more circle to go. The meat was all gone. Hwan Osiit reached over, took a knife out and sliced a piece off his rump; he leaned over and gave it to the buzzard who swallowed it. He came out of the hole and landed on the ground. When he sat on the ground, Hwan Osiit declared: "Man, you go back now, hear me?" Then he vomited, the buzzard vomited out the meat and said: "Take this and stick it back again onto your body. I can't eat it. I don't like to swallow it, I'll get sick if I do that." Hwan Osiit got up and laughed, took the meat and stuck it back. Then he shook hands with the buzzard and said: "Now you go back down there and take all the land and everything for yourself. Everything that belonged to the Devil. I don't want it. I belong up here. Four men are waiting for me; now I will go home, then I'll go that way. I'll do it, they will see that I am a man, you will know it. They'll know it." he said. Then the buzzard said: "Alright, mister, you really are a man. Now do it, do it again! As for me, I'll go now, you
hear?” he said and left. He dove down into the hole. Hwan Osiiit picked up his cane and went back. (23e)


Then Hwan Osiiit went back to his camp. There was nothing there. Nobody was there. He was looking for the men, they couldn’t find them. Then he headed for his boss’s house, got there and said: “The men that were around here, where did they go?” Then the boss said: “I wonder where they went. Already there is no more work. It’s all finished now. The men must have gone away. Where they went, I don’t know.” (24e)

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This is the end of the story. I heard this a long time ago when I was little. Now I’ve told you this, you write it, Margaret! Write it! Teach the people so everybody will know the things that we are doing. (25e)

REFERENCES


TWO ZUNI PASSIVES
LYNN NICHOLS
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1. INTRODUCTION.¹ The prevalent view of the passive construction crosslinguistically, largely proceeding from an analysis of English passives, assumes that the passive form of a predicate is derived from an active counterpart and that this derivation is carried out by means of special passivizing morphology that alters the case and argument properties of the active predicate. This view of passive derivation is at odds with the fact that in some languages there are constructions that may legitimately be called passives—in light of (a) the absence of an agent or other external argument, together with (b) the intransitive use of a transitive base that (c) results in the prominence of a theme or patient argument, e.g. as topic—but that contain no identifiable ‘passivizing’ derivational morphology. Such is the case in Zuni, whose eventive passive construction² in (1a) based on the root ‘utte ‘bite’ contains stative -na and eventive -k; the stative passive in (1b) is formed with -na. Neither contains ‘passive’ morphology that might have properties of the kind traditionally presumed.

(1)a. kwa' hom 'utte - na - k - nam - kya
   neg. 1sg. Acc. bite - stat. - event. - neg. - past
   ‘I was not bitten.’

b. ho' 'utte - na - ye
   ‘I have been bitten’

In fact, we might also make the case for English that there is no passivizing morphology, the standard analysis notwithstanding. It has been noted several times (e.g. Bresnan 1982, Beedham 1987, Cowper 1995) that the participial suffix -en appears not only in the eventive passive (2a) and adjectival/stative passive constructions (2b) but also in the perfect (2e), in unaccusative attributive participles (2d), as well as in passive attributive participles, (2e). In other words, the participle suffix is not special to the passive, and it is unlikely that the same participle morphology has the ability to affect case and thematic properties in certain contexts but not in others.

(2) a. The window was broken by the children.
   b. The window is broken.
   c. The children have broken the window.
   d. Broken glass lay everywhere.
   e. The stolen jewels mysteriously reappeared.

¹I am grateful to Leanne Hinton for the opportunity to present this research on Zuni at the 2000 Hokan-Penutian workshop, as a language that, as she put it, has at some point ever been considered Penutian. Fieldwork for this research has been supported by the Philips Fund of the American Philosophical Society, the Whatcom Museum Society, the Bloch Fellowship of the Linguistic Society of America and the Harvard University Clark Fund.
²In order to facilitate crosslinguistic comparison of the passive construction, what is usually called the verbal passive we refer to as the eventive passive, and what is called the adjectival passive we refer to as the stative passive. Use of these terms will make the English constructions more directly comparable crosslinguistically. Also, we believe that the event structure of the passives is the more crucial feature of the constructions than their putative syntactic category.
On the basis of evidence such as that in (1)-(2) we suggest that contrary to the traditional analysis of the passive, there is no ‘passivizing’ morpheme in bona fide passive constructions in many languages, certainly in Zuni but also including English.

Now, if there is no passivizing morphology of the kind originally presumed, then passives cannot be derived in the way they are said to be; in particular, the idea of suppressing certain properties of the active predicate via this morphology to form the passive must be abandoned. This latter suggestion has in fact already been made for semantic reasons. Beedham (1987) has suggested that the supposed derivational relationship between actives and passives should be called into question, claiming that the semantic relationship between the two is inaccurately characterized as a simple voice difference and that the eventive (verbal) passive involves a change of state interpretation that the active does not necessarily have. Instead, he points to the auxiliary rather than the participle as the locus of the passive interpretation in English.

Interestingly, there do seem to be various types of syntactic evidence that suggest that the event structures of passive constructions and their corresponding actives may differ. For example, an adverbial modifier with simultaneous temporal interpretation is possible with the active transitive form that expresses an activity (3a) but not with the corresponding simple passive form (3b) of the same predicate (Nichols 2000). A progressive form of the auxiliary must be used in the passive (3c) to be able to occur with the simultaneous adverbial, even though the active is not in the progressive itself. The oddness of (3b) indicates that the passive does not carry over the activity interpretive component of the transitive and therefore (or because of an additional resultative interpretation as Beedham suggests) the adverbial modifier expressing simultaneity is incompatible with the passive.3

(3)  
   a. Mary blew bubbles as she walked across the room.
   b. * Bubbles were blown by Mary as she walked across the room
   c. Bubbles were being blown by Mary as she walked across the room.

In light of observations such as these, it is clear that the passive construction and our understanding of it needs to be reconsidered. We attempt in this paper to reexamine the nature of the passive construction by putting aside for the moment participial passive constructions like those in English and turning instead to the verb-stem passives of Zuni. The morphological structure of Zuni verb-stem passives allows to clarify the derivational origin of the passive construction as well as the relationship between the eventive (‘verbal’) and stative (‘adjectival’) passive: in addition to evidence for the absence of true passivizing morphology in passive constructions, Zuni provides direct morphological evidence that the eventive passive is not derived from the corresponding active predicate, nor is the stative passive derived from the eventive passive (as has been claimed on the basis of English, cf. Levin & Rappaport 1986, Cinque 1990). A reexamination of these various relationships turns out to tell us much about just what sort of construction the passive really is. In essence, the passive is not a grammatical-function changing derivation but rather is simply the construction of an eventive form of a transitive base minus an external (subject) argument. Zuni, for example, is able achieve this effect with derivational morphology that changes the event structure of a stative intransitive without changing the number of arguments; this morphology is not special to the passive but is found elsewhere in Zuni in other sorts of eventive derivations.

An interesting implication of the notion that there may be no true ‘passivizing’ morphology is that there is then no single passive derivation or construction, and consequently, ‘passive’ does not exist as a formal category crosslinguistically. Instead, the passive is a functional category: languages may share the functional goal to render the logical object of a transitive base the topic of the sentence. The general manner in which this goal is achieved, by not including an external thematic role in the construction, is shared by many languages, but the specific constructional means by which the external thematic role is excluded varies

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3Nichols (2000) discusses two additional sorts of evidence for a resultative interpretation in passives in perception verb complements and small clauses.
crosslinguistically, e.g. from intransitive auxiliary passives (English, Hindi), to cases where reflexive morphology is also used to form the passive (Russian) or a dummy element is inserted in subject position (Navajo, Yoruba), to name only a few such strategies. The explanation of the passive we put forth here based on the analysis of Zuni turns out to have far-reaching consequences, accounting for why the passive is such a heterogeneous category crosslinguistically.

2. **TWO ZUNI PASSIVES.** In this section we give a detailed description of the two Zuni passives illustrated earlier in (1) and discuss several aspects of their structure particularly relevant to the current investigation: properties of the stative morpheme -na and the absence of any sort of subject in the eventive passive.

The two Zuni verb forms with passive interpretation are illustrated again in (4) and (5) below. For convenience we will sometimes refer to these as Passive 1 and Passive 2, respectively.

**Passive 1**

(4)  
ho' 'utte - na - ye
'I am bitten, I have been bitten'

**Passive 2**

(5)  
hom 'utte - na - k - 'appa
lsg.Acc. bite - stat. - eventive - SR (DS)
'I was bitten, i.e., s.t. bit me',
* 'He/it bit me'

Zuni Passive 1 is formed off a transitive lexical base and is characterized semantically by a resultative stative interpretation, ((4) is essentially interpreted as ‘I am in the state of having been bitten’) and takes adverbial modifies such as ‘now, from this point on’ but not ‘yesterday’. The NP argument of the resultative stative passive is a structural subject, and this argument bears nominative case. The resultative passive is characterized morphologically by the presence of the stative suffix -na following the lexical portion of the stem. Where Zuni verbal inflection distinguishes stative vs. non-stative allomorphs, the resultative passive takes the stative allomorphs, e.g. as in the case of present tense inflection and the negation suffix.

Zuni Passive 2 is characterized semantically by an eventive interpretation, and is modifiable by adverbial expression such as ‘in two seconds’ but not ‘since yesterday’. Speakers are very clear on the interpretation of such passives: they are paraphrasable as either eventive passives in English or with an indefinite subject e.g., ‘something bit me’; the latter speakers say they use to emphasize that this is an action passive not a state. The NP argument of the eventive passive is a structural object and bears accusative case. Evidence for the former is discussed below. Zuni lacks lexical case-marking, so that the case borne by the argument in (5) is a structural accusative case. An interesting property of this eventive passive is the inability to occur with an agentive ‘by-phrase’. We will return to this aspect of the construction, a particularly revealing one, in section 3. Finally the Zuni eventive passive stem is morphologically characterized by the presence of what we will call the eventive suffix, -k, following the stative morpheme -na. The eventive passive takes only non-stative allomorphs of verbal inflection.

The box below summarizes the semantic, syntactic and morphological properties of these two passive constructions.
<table>
<thead>
<tr>
<th>Semantics</th>
<th>Passive 1</th>
<th>Passive 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Passivizing</td>
<td>Resultative stative</td>
<td>Eventive passive</td>
</tr>
<tr>
<td>Syntax</td>
<td>NP is subject,</td>
<td>NP is object,</td>
</tr>
<tr>
<td></td>
<td>Nominative case</td>
<td>Accusative case,</td>
</tr>
<tr>
<td></td>
<td>*</td>
<td>No ‘by’-phrase</td>
</tr>
<tr>
<td>Morphology</td>
<td>Stative allomorphs of present &amp; negation</td>
<td>(No present; non-stative neg. Presence of morpheme -k-</td>
</tr>
</tbody>
</table>

Note that the eventive suffix –k only surfaces in certain morphophonological environments, e.g.:

(6) (a) negation: 'utte - na - k - na'm -
(b) switch reference (SS): 'utte - na - k - nan
(c) switch reference (DS): 'utte - na - k - 'appa^4

But not:

(d) past: 'utte - na - 'kya
(e) future: 'utte - na - k'yanna
and other suffixes beginning with {'^', 't', 'j'}

Examples cited here will be in one of the forms in (6a-c) when possible in order to make the presence of the eventive suffix most obvious.
In the following sections we examine each of the two passives in greater detail before moving on to the issues they raise with regard to the nature of passive derivation.

2.1 Passive 1: Resultative Stative. The hallmark of passivizing morphology is said to be the ability to absorb the agent thematic role of a transitive verb as well as the accusative case assigned by it (Jaeggli 1986, Baker, Johnson & Roberts 1989). It can be immediately seen from its usage with intransitive verbs and verbs with inanimate subjects that the Zuni stative suffix -na is not a passivizing morpheme.

-na can be used with active intransitive verbs to form a resultative stative, as in the case in (7b) where -na suffixed to hapo 'gather' produces the stative form hapona 'be gathered'.

(7) a. hon hapo-kya
'Ve gathered'

b. hon hapo - na - 'kya
'Ve were gathered'

Furthermore, stative -na is suffixed to the verb stem when an inanimate NP is subject of a verb of motion like kwato 'enter', 'u: 'go', te'ei 'arrive', (8b).

(8)a. ho' le-m šok'ona kwato - kya
1sg.Nom. wood-sg. hollow enter - past
'I entered the hollow tree'

^4Interestingly, both SR markers are possible with the eventive passive, the reasons for which we will not be able to discuss here.
b. lesnhoł ‘okšik’ a:teyana-n kwato - na - p
   there rabbit track-sg. enter - stative - DS
   ‘There were a rabbit’s tracks going in[to the hollow tree]’
   [Bunzel 1933]

These examples illustrate that -na is simply a stativizing morpheme and does not affect the number of thematic roles or case assignment of a predicate.5

When -na is added to a transitive root, the result is a resultative stative form whose transitive lexical base will determine that it have passive interpretation. The NP argument of this stative passive bears nominative case simply because it is now in essence a stative intransitive verb.

The most significant property of the Passive 1 resultative stative formation with -na, however, is that the Passive 1 -na stem is use by Passive 2 as its derivational base. While previous work on the English passive has suggested that eventive passives are derived from active predicates and stative passives from eventive passives, the Zuni facts indicate that the opposite situation obtains: the eventive passive is derived from the stative.

<table>
<thead>
<tr>
<th>LEXICAL MEANING</th>
<th>FORM STATIVE</th>
<th>ADD EVENTIVE MEANING</th>
</tr>
</thead>
<tbody>
<tr>
<td>Transitive Root</td>
<td>'utte - na'</td>
<td>Eventive passive</td>
</tr>
<tr>
<td>Stative passive</td>
<td></td>
<td>'utte - na - k'</td>
</tr>
</tbody>
</table>

2.2 PASSIVE 2: EVENTIVE PASSIVE. As for Zuni Passive 2 - the eventive passive - the absence of any passive morphology, and in addition, the accusative case & structural object properties of its NP argument, may initially raise doubts as to whether this is indeed a construction on which conclusions about the passive ought to be based. Cook and Frantz (1978) go so far as to say as much, i.e. that the Zuni [eventive] passive6 is in fact not a passive at all. There are two distinct issues here to resolve, whether accusative case can appear in a legitimate passive, and whether Zuni Passive 2 is in fact not a covert transitive. In this section we discuss evidence that addresses both of these concerns.

5On a typological note, it is interesting to contrast stative formation in Zuni with that found in other languages. Zuni stative -na appears possible with nearly any verb type, (e.g. intransitive ‘gather’, transitive ‘bite’ causative ‘feed’, verbs of motion ‘arrive’, ‘go’, ‘enter’), except that a stative formation with -na does not appear to be possible with intransitives whose lexical meaning includes a resulting state. Hence (ib) čapi - na ‘burn’ + ‘stative’, ‘It was burnt’ is not a possible formation.

(i)a. k'yałe - n čapi - kya
    house - sg. burn - past
    ‘The house burned’

b. *čapi - na - 'kya
   burn - stat. - past
   *‘It was burnt’

In contrast, Dubinsky and Simango (1996) report that statives may be formed in Chichewa only from change of state verbs, exactly the set of derivations excluded by Zuni.

(ii) Stative formations in Chichewa
    VERB    STATIVE
    phika  ‘cook’    phik-ika
    swa    ‘break’   w-eka
    luma   ‘bite’    * lum-ika
    ombela ‘shoot’  * ombel-eka

6They identify only a single type of Zuni passive.
2.2.1 ACCUSATIVE CASE IN PASSIVES. The presence of accusative case in the passive turns out to be not at all rare. Accusative is found, for example, in passives in Ukrainian (Sobin 1985), Nepali, Finnish (Goodall 1993), Polish and Welsh (Baker et al. 1989). Goodall (1999) presents some novel empirical evidence from Mandarin Chinese that accusative case may be available in the passive. Chinese passive constructions with bei (so-called ‘retained object’ constructions), (9a), are significantly better than their active counterparts, (9b).

(9) a. Taizi bei ta dale la
table PASS he apply-ASP wax
‘He applied wax to the table.’

b. *Ta dale taizī la
he apply-ASP table wax

It appears that in the active only two of the three arguments of the verb da can be licensed, while in the passive because there is one less argument, both of the arguments are able to be licensed. Goodall concludes that the two constructions have a different number of arguments but make available the same number of cases: nominative & accusative. Examples like (9a) support the idea that there is no a priori reason why accusative case should be impossible in the Passive. Thus the occurrence of accusative case in the Zuni eventive passive has no bearing on the voice structure of this type of clause, and the absence of accusative in passives in English and other languages is likely due to reasons independent of the passive per se (e.g. movement to a nominative position).

In any case, we can confirm the passive nature of the Zuni construction in (5) from another angle, by showing that Zuni Passive 2, the eventive passive is not simply a transitive with an unspecified (indefinite or expletive) subject.

2.2.2 TESTS FOR SUBJECTS. Constructions containing clauses without structural subjects are not unknown crosslinguistically, although admittedly they are not very common. McCloskey (1996) reports a possible instance from Modern Irish. In the case of Zuni, there are at least three tests that suggest the absence of any element in subject position. One test derives from the semantics observed for dummy-subject constructions crosslinguistically and two additional tests are suggested by the peculiarities of Zuni structure.

Some facts about pronominal usage in Zuni are necessary for interpreting the eventive passive structure. Pronouns are obligatory in Zuni, likely related to the lack of person agreement with the verb (verbs show only ±plural number agreement). Thus even if the 1st person argument in (10) is an understood topic, ho’ is required.

(10) ho’ / *Ø
gallup ‘an ’a:n - e
1sg.Nom. G. to go - pres.
‘I’m going to Gallup’

While there is a full paradigm of nominative, accusative and possessive strong and weak pronouns for 1st and 2nd person singular and non-singular referents, there are no 3rd person pronouns.7 Instead, the absence of an overt NP or pronoun in argument position indicates a 3rd person argument whose referent has previously been established. As the contrasting grammaticality of the interpretations in (11a-b) indicate, this Ø usage has definite specific

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7 Newmann (1965: 60) indicates the existence of Zuni 3rd person pronouns, but this analysis is incorrect since the forms given there are not pronouns. The forms given as (’)an are postpositions (with prefixal agreement in the case of plural arguments). The dual ’a:ci is a particle that is not limited to 3rd person and may be found with any of the personal pronouns. The suffix -ya’ accompanying the dual particle in certain cells of the table is actually a focus + specificity marker and is found only in specialized semantic contexts.
reference only. Indefinite reference requires the use of an indefinite pronoun, (12) (čo’o ‘for indefinite specific and ćo’o ‘limat ‘someone it seems’ for indefinite nonspecific).

(11) Gallup' an 'a:n - e
G. to go - pres.

a. ‘He/She’s going to Gallup’
b. * ‘Someone’s going to Gallup’

(12) čo’ol Gallup' an 'a:n - e
someone G. to go - pres.
‘Someone’s going to Gallup’

Turning back to the eventive passive, this construction has no overt indication of a subject argument but is never interpreted with a specific subject, i.e. (13) cannot mean ‘*He/it bit me’.

(13) hom 'awa - na - k ‘appa
1sg.Acc. find - stat. - event. - DS

a. ‘I was found; s.o.(non-specific) found me; they (pl. non-specific) found me’
b. * ‘He/it found me.’

Speakers usually offer for passive sentences like (13) the English equivalents indicated in (13a), though none of the interpretations is necessarily preferred over the other; they report that the key to the interpretation of these sentences is that whoever or whatever is responsible for the action is unknown. Thus a sentence like that in (14) can have one of at least three possible interpretations that are distinguished in English, though these three equivalents merely get at different aspects of a single interpretation in Zuni (Indeed, if the subject of (13) were truly plural, a plural agreement prefix would be required on the verb.)

Therefore, while the absence of any overt evidence for a subject in the eventive passive construction in (13) might suggest to some that this passive may have a null indefinite subject, the properties of pronominal reference in Zuni indicates otherwise: null indefinite subjects are not a feature of Zuni grammar. If there is a structural subject in eventive passives, then it can only be of one sort, a null expletive subject. We return to this possibility in a moment.

First, however, the argument against a null indefinite subject in Zuni eventive passives can be further supported by evidence from subject-oriented reflexives. Zuni makes use of a reflexive possessive pronoun yam (nam for younger speakers) that may only be bound by the subject of its clause, (14). The impossibility of the reflexive possessive pronoun in an eventive passive construction (14b) indicates the absence of an appropriate binder in the subject position of the clause.

(14)a. ho' i Nemmek' an yamį/*k nicikya 'uk-ky a
1sg.Nom. N. to Refl.Poss ring give-past
‘I gave Nemmek myį/*her k ring’

b. hom 'apčį - na - 'kya *yam / hom 'ačiyann - akky a
‘I got cut with my / *someone’s knife’

[Cook & Frantz 1978]

Moreover, the passive argument itself cannot serve as the binder of possessive yam, (15), indicating that the passive argument itself is not a structural subject but rather an object.
(15) * homį yamį nicikya 'uk - na - 'kya
1sg.Acc. Refl.Poss. ring give - stat. (- event.) - past
'I was given my ring'

Even if there is no covert indefinite subject in eventive passives, there is the possibility that a structural subject position may be filled with a null expletive element. Expletive subjects are
found in Zuni, and their presence is signaled by the verbal prefix te-, (16a-b). The te- prefix is
never found, however, with the eventive passive stem in Zuni (cf. (16c)).

(16)a. te - k'yali
expl. - be.hot.
'Its hot out'

b. tel-oše-'a
expl.-hungry-pres.
'There was a famine'

c. kwa' (*tel-)utte-na-k-nam-kya
neg. (expl.)bite-stat-ivevent.-neg. -past
'he was not bitten'

The indefiniteness requirement in expletive constructions is a useful diagnostic for the
presence of a null expletive linked to an argument in the complement part of the clause. If there
is no expletive, there should be no definiteness effect (i.e. requirement of indefiniteness) on the
argument inside the verb phrase. McCloskey (1996) reports that there is no such requirement in
the parallel Irish constructions. The arguments of Zuni eventive passive clauses behave similarly
and may have either definite or indefinite reference.

(17)a. There branched off on the left a/*the dark tunnel

b. kwa' Nemme - ya' utte - na - k nam - kya
neg. N. - Acc.(Foc.) bite - stat. - event. - neg. - past
'Nemme was not bitten'

c. hewe' 'awa - na - ky - 'appa
money find - stat. - eventive - DS
'The money/some money was found'

The natural conclusion to draw from the absence of either te- or a definiteness effect is that
no null expletive is present to induce a pseudo-transitive effect (and accusative case) in this
construction.

While these tests provide us with evidence against certain types of elements occurring in
subject position of the eventive passive, we can go even further and eliminate the possibility that

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8tel- before vowel-initial stems. The syntactic status of te- is not entirely clear: the behavior of te- is consistent
with either agreement or an incorporated nominal.
te- is also used for object expletives, as in the case of (i):

(i) te-[l]-ank'ohak'ekkya hewe' hanlina'kya
'He discovered [it] that his money was stolen'

9The lack of a definiteness effect in the Zuni eventive passive may also be taken as an indication that the accusative
case assigned here is structural case, not the inherent case described by Belletti (1988) that is associated with an
(indefiniteness effect.)
any sort of structural subject position exists at all in eventive passives, whatever may fill it. Causative morphology that is sensitive to structural properties of the base predicate provides us with this type of evidence. One type of causative found crosslinguistically has the property that, whether the causative formation itself is periphrastic or morphological, it does not affect the event structure of the base predicate. This type of causative is sometimes referred to as a syntactic causative (e.g., Hale & Keyser 1993) to contrast it with so-called lexical causatives, such as transitive break from intransitive break, that do change the event structure of the base. Syntactic causatives are identifiable in Zuni by the presence of the verbal suffix -k'ya and may be formed from either a transitive (18a) or an intransitive base (18b).

(18a) hon k'yawe' tutu - k'e - nap - kya
    1pl.Nom. water.pl. drink - caus. - pl.subj. - past
    'We made him drink water.'

    b. ta: s 'imat suski yam tuna: 'i'luwah - k'ya - kkya
       again then I.think coyote self's eyes run - caus. - past
       'So again Coyote made his eyes run around'

[Newman 1965]

Interestingly, a -k'ya syntactic causative may not be formed on the Zuni eventive passive, as (19a) indicates. The reason for this appears to be that while -k'ya is fairly liberal in the type of predicate that it may combine with, one restriction on its distribution is that it must be added to a predicate with its own subject.

(19)  
    a. * hom 'utte - na - kk'ye - kkya
        1sg.Acc. bite - stat. - caus. - past
    Interp 1. (= causative of Passive 2)
    * 'He caused me to be bitten, He caused s.o. to bite me'

    b. ? ho' 'utte - na - kk'ye - kkya
        1sg.Nom. bite - stat. - caus. - past
    Interp 2. (= causative of Passive 1)
    ? 'He caused me to have been bitten'

The ill-formedness of (19a) can be contrasted with (19b), which though decidedly odd, is considered by speakers to be somewhat better that (19a). The interpretation of causative + resultative stative requires a special context for appropriate usage, hence the oddness speakers attribute to it out of context, yet the causative is formable here in a way that it is not with the eventive passive.

While both passives present an intransitive base to the causative predicate, they do differ in the status of the argument of that intransitive. The resultative passive that serves as the base for the causative in (19b) has a nominative argument and therefore a structural subject. The eventive passive has an accusative argument. Since the syntactic causative is so liberal in its distribution with regard to event structure and predicate type, we conclude from the ungrammaticality of (19a) that there is no subject position available in the eventive passive, i.e. any covert element in subject position is ruled out, and the accusative argument itself does not occupy subject position.

The results of this section can be summed up with two important observations. First, both the Zuni stative and eventive passives are passives in the traditional sense since they both

10Interestingly, even though this would appear to be a semantically interpretable combination.
lack an agent or other argument corresponding to the agent or other subject argument in the transitive, with the result that the semantic object (e.g., theme, patient, goal) argument is the topic argument of the construction. What is surprising, however, is that despite their recognizability as canonical passives in these respects, neither of the two Zuni passive stems, illustrated again in (20), actually contains any ‘passivizing’ morphology of the canonical sort.

(20) Passive 1 Passive 2
'utte + na 'utte + na + k
trans.V.root + stative trans.V.root + stative + eventive

Insight into this state of affairs can be gained from a consideration of the relationship of the external thematic role to both event structure and syntactic structure, which we turn to next.

3. PASSIVES WITHOUT PASSIVE MORPHOLOGY: AGENT VS. ‘CAUSING PROCESS’. Cook and Frantz (1978) argue that the Zuni [eventive] passive is not in fact a passive at all, since according to a number of syntactic tests the NP remains structural object. For them, a true passive construction must involve advancement to subject. We do not dispute their syntactic results: the NP in a Zuni eventive passive construction is indeed a structural object. Despite this property, the Zuni eventive passive is much like other passives crosslinguistically in functioning to maintain the object as topic by using an eventive predicate missing an agent argument. (21) contains an example of Zuni passive usage within an extended context (21a-d represent a continuous text) and serves to illustrate this point.

(21) (a) 'a:mu:kwi te:wa:kwe 'anše:kwe tewa-p hom šema - nap - kya
Hoplis Tewas Bear Clan be.next.day-DS 1sg.Acc. call - pl.subj. - past
(b) hom 'illi-n tin-allu
1sg.Acc. have-subord. stay.pl.-go.around
(c) 'itok'ya - na - 'kya
feed - stat.(-event) - past [type of food made of corn paste]
(d) 'a:-mu:kwi 'ewaštok hom seto - pa ...
pl.-Hopi girls 1sg.Acc. carry.on.back-pl subj.
[Bunzel 1933]

(a') The Hopis and Tewa Bear Clan people sent for me the next day.
(b') They took me around,
(c') I was fed hepalkoya,
(d') the Hopi girls took me on their backs...

The first person argument is the object of šema ‘call’ in the transitive clause in (21a). The first person argument continues to be the object of 'illi ‘have’ in the serial verb construction, (21b); here the agent is the same and is merely indicated by pronominal usage (recall that Zuni 3rd person pronouns are Ø). In the next clause containing the passive (21c), the first person object argument continues to be the topic, and the continued agent is further reduced to become completely unspecified by means of the eventive passive construction. When a new agent ‘the Hopi girls’ is introduced in the final clause (21d), the switch is made back to a canonical transitive construction. The role of the eventive passive in this sequence indicates that the Zuni passive fits in quite well with what we know about passive use and discourse structure.

The Zuni eventive passive, or the -k passive, is therefore able to encode passive meaning without passive morphology and does this somehow by constructive an eventive predicate minus an agent argument. The question is, how is something like the Zuni -k passive possible?
The answer lies in the separation the notions ‘causing process’ and ‘agent’. The presence of an agent is commonly taken to both signal as well as impart active or agentive meaning. The form of Zuni passives suggests that the notion of agentivity is a semantically complex construct and includes at least two distinct components: a causing process and an agent. Under this view, in an eventive predicate like ‘break’ in (22), the agent ‘John’ is not the cause or causer of breaking but rather merely an actor in the causing process. A view of agentivity and causation along similar lines is proposed by Levin & Rappaport Hovav (1995:102-103), who distinguish the notion of external cause from a volitional agent.

(22) John broke the vase (i.e. when he dropped it and it hit the floor)

This view of agentivity and the relationship between agentivity and events relies on events being decomposable into their constituent subevents as argued, for example, by Pustejovsky (1991, 1995), as well as on the ability of languages to refer to and/or encode subevents. The event structure for an eventive transitive like break, given in (34) below, contains two subevents, a Process and a Transition. An important aspect of this event structure is that the Process subevent is understood by implication to be the causing process of the breaking event (Pustejovsky 1995). In Zuni, which is morphosyntactically sensitive to the distinction between a causing process and its actor, this causing process may be encoded in structure alone with the morpheme -k.

(23) E
    3
    Process               Transition

In fact, Zuni is quite adamant about the expression of the causing process alone without an agent in -k constructions, so that in the eventive passive an agent must not be specified even as a postpositional adjunct, (24).

(24) hom 'utte - na - k - nan (*waccita 'akkya)
    1sg.Acc. bite - stat. - event. - SS ( dog instr.)
    ‘I was bitten (*by a/the dog)’

Further evidence for the semantic and morphosyntactic separation of agent from causing process comes from the use of the Zuni eventive -k construction for events that could not possibly involve an agent. The sentences (25a-b) occur in close proximity in a text. The sentence (25a) is in the form of the resultative stative, indicated both by interpretation as well as the stative allomorph -ye of the present tense suffix. The second sentence, based on the same lexical root ‘swell’, is in the form of an eventive passive. Here, the interpretation is one of a state resulting from an event of swelling:11 in this context (of a corpse swelling up) there is no question of the possibility of an agent as the causer of or actor in the swelling event.

(25) a. 'el'e šole - na - ye
    corpse swell - stat. - pres.
    ‘The corpse was swollen.’

---

11 The difference in stativity in (25a-b) is also indicated by tense inflection. The tense inflection in (25) illustrates the Zuni ‘narrative’ use of present and past tense, where tense may be marked relative to the particular topic time. A state holding at the topic time will be marked with present tense, while an event occurring prior to the topic time will be marked with past.
b.  šole - na - 'kya
   swell - stat. (- event.) - past
   '[He had been killed wantonly, and so] he[the corpse] swelled up'

[Bunzel 1933]

While the event in (25b) lacks an agent, the context explicitly includes a cause of the swelling, so that the morphosyntactic representation of the event will also reference this causing process. Thus 'swell up' here is expressed as an eventive passive rather than a change-of-state intransitive, which in Zuni is encoded as a pure transition, cf. (26) below.

(26)  k'uho- ti - kya
       break - inch. - past
       'It broke'

Based on these observations of the Zuni eventive passive, we summarize in (27) what might be called a principle of agentivity, which takes into account the semantic complexity of agentivity.

(27)  **Principle of Agentivity**
   a. 'Causing process' and 'agent' are semantically distinct.
   b. An agent is not a cause(r) but merely an actor in the causing process.
   c. 'Causing process' may be encoded in morphosyntactic structure alone, or
      with an actor.

Now, if the process subevent in a complex event structure corresponds semantically to a cause, what does it correspond to syntactically? In other words, what morphosyntactic property is the Zuni eventive suffix -k an instantiation of?

The Zuni data described here seems to suggest that causative derivational morphology may in fact be of more than one type. First, it may, as in the case of canonical causative derivational morphology, introduce both a causing process as well as an agent of that causing process (cf. the Zuni causative morphology in example (18)). In addition, under the proposal made in (27) that 'causing process' and 'agent of causing process' are distinct notions, we might predict that a second type of causative morphology might also exist that encodes causing process alone without the agent of that process. In other words, causative morphology essentially encodes 'causing process', and it is a property of individual lexical items belonging to this derivational category as to whether they do or do not allow the causer to specified in addition (= assigned a thematic role). Thus the semantically complex nature of the notion of causation leads us to expect to find languages like Zuni which possess more than one type of derivational morphology with respect to the specification of the causer. In other words, rather than constituting an arbitrary fact of language, Zuni -k morphology is an expected sort of lexical variation if an agent is discrete from the causing process, i.e., not implicit in the causing process but merely participating in it.

This view of agents and their relationship to events predicts that there should exist paradigms of derivational morphology like that in Zuni illustrated in (28) which hinges precisely on the ±specified causer distinction. The morphemes in this paradigm are semantically similar and differ only in the property that one (-u) is specified for an agent in the causing process and the other (-k) is not. The result in Zuni is a paradigm that encodes several degrees of eventivity and agentivity.
Derivation of Eventive Predicates ± Agent

(a) Simple Stative Stems

<table>
<thead>
<tr>
<th>'uli  ‘be inside’</th>
<th>('uli - k)  ‘be put inside’</th>
<th>('uli - u)  ‘x put y inside’</th>
</tr>
</thead>
<tbody>
<tr>
<td>ho'  'uli - p</td>
<td>hom  'uli - k - appa</td>
<td>hom  Nemme  'ul - u - p</td>
</tr>
<tr>
<td>‘I was inside’</td>
<td>‘I was put inside’</td>
<td>‘Nemme put me inside’</td>
</tr>
</tbody>
</table>

(b) Eventive Intransitives

<table>
<thead>
<tr>
<th>‘ansatto  ‘helped’</th>
<th>‘ansatto - k  ‘be helped’</th>
<th>‘ansatto - u  ‘x help y’</th>
</tr>
</thead>
<tbody>
<tr>
<td>‘imo  ‘be sitting’</td>
<td>‘imo - k  ‘be seated’</td>
<td>‘imo - u  ‘x seat y’</td>
</tr>
<tr>
<td>‘palo  ‘buried’</td>
<td>‘palo - k  ‘be buried’</td>
<td>‘palo - ‘u  ‘x bury y’</td>
</tr>
</tbody>
</table>

(c) Transitives

Other examples:

The base of the derivation is a stative root, such as ‘uli  ‘be inside’. To form an eventive predicate from a stative root in Zuni there are two choices, an eventive without a specified agent formed by suffixing -k to the root (middle column, (28b)), and an eventive with a specified agent formed by suffixing -u or -’u to the root (right column, (28c)). As in the case of the eventive passive, the morpheme -k in the eventive intransitive is associated with the presence of accusative case.

Significant about the paradigm in (28) is the fact that the eventive morpheme -k occurs here in a context other than the passive and with a lexical base whose sole argument is retained in the derived form. This should dispel any lingering doubt that -k itself might be a ‘passivizing’ morpheme with the function of suppressing of the external argument. In essence, the Zuni eventive passive is an eventive intransitive formation parallel to the middle column, (28b), but on a derived stative base.

To briefly summarize: we suggested in the introduction that the absence of true passivizing morphology may be taken as an indication that passives are not necessarily derived by suppressing or reassigning the external argument role of a transitive predicate. A closer look at the structure of Zuni passives reveals that passives are indeed derivable by other methods than the standardly assumed ‘reassignment’ operation.

4. SUMMARY AND IMPLICATIONS FOR THEORIES OF PASSIVE FORMATION. We have suggested here that many of the assumptions concerning the form and derivation of the passive are supported in English only by either ambiguous or outright problematic empirical evidence, including the notion that the passive forms part of a ‘voice alternation’ that consists of deriving a passive construction from a transitive predicate. In addition, the form of Zuni stative and eventive passives provides direct counterevidence to arguments for ‘passivizing’ morphology that suppresses or absorbs case and thematic roles. Instead passives appear to be derived by adding properties to a lexical root containing only object information; in other words, they are derived in the same manner as transitives.

Thus one important claim of this study is there is no ‘passivizing’ morphology in many languages that have recognizable passive constructions, despite the fact that the passive was originally defined essentially on the basis of the presence of such morphology. Instead what we find is that the passive crosslinguistically consists of various different strategies that aim at excluding the external thematic role from the predicate. Zuni, representing one type of passive strategy, makes use of derivational morphology that corresponds to the causing process subevent but is lexically specified as lacking an external thematic role for the causer. English employs another method of avoiding the external thematic role and adds a change of state intransitive auxiliary be instead (and as a result the lexical root takes on participial form). Contrary to the standard approach to the passive, then, in auxiliary passives the essence of the passive is in the
auxiliary, not in the participle. Various alternative forms of the English strategy may be found, for example Hindi uses the auxiliary *jana* 'go' instead of 'be', (29a), and in Korean the change of state auxiliary *eci* 'become' is a bound morpheme that attaches directly to the verb stem, (29b).  

(29) **Hindi**  
\[a. \quad \text{cor pakRa:} \quad (*\text{Ram - se}) \quad \text{ga - ya:} \]
\[\text{thief catch (Ram - by) go - past.M.sg.} \]
\[\text{‘The thief got caught (*by Ram)’} \]

**Korean**  
\[b. \quad \text{totuknom-i cap - eci - ess - ta} \]
\[\text{thief-Nom. catch - become - past - decl.} \]
\[\text{‘The thief got caught’} \]

Russian has a third type of passive, in which reflexive morphology is also used in the passive. Navajo and Yoruba employ a dummy subject in transitives to construct a passive, and Javanese and Indonesian both use subject agreement morphology from a different paradigm and require the agent to be syntactically backgrounded but nevertheless present; these represent yet a fourth and fifth type of passive construction.  
The conclusion to be drawn from such variation is that while there seems to be a notional category of passive serving a particular discourse function, there is no formal category of passive. As a result, what we identify crosslinguistically as the passive are the various structural solutions to the functional problem of object topicality with a transitive base, constrained by independent grammatical principles of particular languages. This phenomenon is not limited to the passive. We find a similar heterogeneity of structures that make up the class of switch-reference obviation systems (cf. the collection of papers in Haiman and Munro 1983) or that constitute an ergative system (Bittner and Hale 1996).  
A contrast between passives and causatives in this light is revealing. Both passives and causatives are usually thought of as argument changing derivations (cf. Baker 1988 chapters 4 and 6) of a related though opposite type: passives remove an argument while causatives add an argument. Yet while the passive consists of a collection of disparate structures crosslinguistically, causative formations falls into essentially two categories, morphological (affixal) vs. syntactic (peripheral), both involving the introduction of a syntactic V head + external argument (Baker 1988, chapter 4). If the passive were truly the simple reductive inverse to the causative that it is said to be, we might not expect to find the wide constructional variation in the passive that we do.  
Finally, the heterogeneous approach to the passive may actually provide us with explanations for phenomena connected to the passive for which otherwise exist either awkward accounts or no explanations. Briefly, we take the example of the variability of whether an agentive phrase is possible with the passive. For the obligatory absence of an agentive phrase Jaeggli (1986) cites Comrie (1977) for Latvian, certain traditional styles of Persian and Classical Arabic and Siewierska (1984) for Latvian, Urdu, Kupia, Classical Arabic, Amharic, Igbo, Tera, Sonrai, Fijian, Atjinjamathanha, Cutenô, Cora, Huichol, Cahuilla, Shoshoni, Pepecano. For other languages, Siewierska indicates that the agentive phrase in the passive is obligatory, e.g. Kota (Dravidian), Palauan and Indonesian (Austronesian).  
Jaeggli (1986: 602) explains this crosslinguistic variation essentially by stipulation: passive morphology in certain languages subcategorizes for an agentive phrase and in other languages does not. Of course, if there is no passivizing morphology as has been claimed in this study, this explanation cannot be right and we are still left with this variability in the expression of the agent  

\[12\] There is a second passive formation in Korean which makes use of derivational morphology that is formally very similar to causativizing morphology, a surprising circumstances that has been remarked on before. An investigation of this state of affairs is beyond the scope of this paper, but the analysis of the Zuni eventive suffix -k in section 3 hints at why this similarity between 'passivizing' and causativizing morphology may not be surprising after all.
to explain. If the passive crosslinguistically is the sort of heterogeneous phenomenon suggested here, there may be as many explanations for this variation as there are passive constructions. The agentive by-phrase may be permissible in English simply because the semantics of the passive change-of-state auxiliary be do not disallow it, while on the other hand the semantics of the passive auxiliary jana ‘go’ in Hindi (29a) will not permit it. Zuni eventive passives disallow agentive phrases (cf. example (24)) because the morpheme -k is lexically specified as -specified causer. Indonesian and Javanese passives obligatorily require an agentive phrase since these constructions are essentially transitives with word order and agreement signaling a shift in topic to the object.

(30) **Javanese**

a. Amrih tuku dolanan
   A. buy toy
   ‘Amrih bought a toy’

b. Dolanan di,-tuku Amrih
   toy 3sg.-buy A.
   ‘Amrih bought the toy’

With the knowledge that the passive crosslinguistically is properly understood as a collection of different structures with a similar functional goal, we now can predict that the explanation for the variable distribution of the agentive phrase will be language specific rather than general.

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TSIMSHIANIC I-INITIAL PLURALS:
RELICS OF AN ANCIENT PENUTIAN PATTERN

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1. INTRODUCTION. Sapir's 'Penutian phylum', characterized by him in few sentences (1929), is often considered to rely on vague typological similarities rather than substantive evidence.\(^1\) Lexical resemblances have been attributed to 'massive borrowing', and the important feature of reduplication to non-conclusive iconicity (Silverstein 1979:664). Such a priori views have prevented serious consideration of the morphological evidence.

In systematic comparisons of Tsimshianic with 'Penutian' languages, taking as their point of departure reconstructed Proto-Tsimshian (Tarpent 1990, 1994, 1996, 1997, in prep.), the number and the type of morphological resemblances, both in structure and in actual morphemes, found throughout the phylum, strongly support the hypothesis of genetic relationship between the constituent families, including Tsimshianic. Particularly important are what appear to be shared retentions or relic forms.

One of these is the plural verbal affix in /I/, which corresponds to a normally segmentable morpheme or formant in several of the languages; in addition, traces of an archaic Proto-Tsimshian pattern discernible in submorphemic alternations between I-initial plural and velar-initial singular verbal stems are also found in several other Penutian languages (Tarpent 1997:78), suggesting that this pattern is of great antiquity. The existence of such an unusual relic feature, added to lexical similarities in the stems showing these alternations, strongly supports the hypothesis of genetic unity of some version of the Penutian group as a whole.

2. THE TSIMSHIANIC LANGUAGES AND THEIR ANCESTOR. The Tsimshianic languages are spoken in Northern British Columbia, close to the tip of the Alaska panhandle. The Maritime branch includes Southern Tsimshian (ST) and Coast Tsimshian (CT), while the Interior branch is comprised of Nisga'a (N) and Gitksan (G). ST and N are the two crucial ones for reconstruction of the proto-language as they are demonstrably the most conservative in phonology and morphology.

ST and CT have five long and five short vowels plus four diphthongs, N and G have five long and (in native words) three short vowels, plus three diphthongs, but only two vowels need to be reconstructed for Proto-Tsimshianic (PTSim): these are noted *e and *a, representing /a/ and /a/ respectively; the PSTsim vowel nucleus could also include an optional element noted as H, probably originally a schwa-glise, whose presence, added to the influence of surrounding consonants or glides, accounts for a great variety of vowel correspondences within the family (Tarpent 1990, 1994 and in prep.). The cover symbols E and A indicate that the basic vowels *e and *a could occur either with or without the presence of H in alternate forms of the same root (e.g. *CEC = *CéC/*CéHC). In most cases this presence is only detectable under stress.

Proto-Tsimshianic morphological structure (Tarpent 1997:70-76) is based in most cases on a *(C)VAC root to which affixes are added for derivation and inflection. In addition, the element H probably represented a morpheme with imperfective meaning, sometimes including repetitive meaning, as suggested from its occurrence in some very old plurals (Tarpent 1994; see example (1) and 3.2.2., 3.2.3. below). There are also reasons to assume that some *CVC roots go back to an earlier, Pre-Proto-Tsimshianic (PPTSim) bipartite stage **CVh-**VC (e.g. [21] below).

Similarly, initial consonant clusters can often be traced back to loss of a vowel, e.g. CCVC <

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\(^1\) I had originally intended to give a general presentation on my growing file of "Penutian morphological elements," i.e. those occurring through most of Sapir's (1921) Penutian. Since the file was much too large and complex to present during the allotted time, I decided to concentrate on only one of the elements. Hence the title "Relics of a plural pattern" added for the conference presentation, and the new title of the final version.
3. **Tsimshianic Plural-Formation (Verbs)**. The distinction between singular and plural is of great importance for Tsimshianic verbs, less so for nouns. Apart from very rare oddities such as²

(1) ST
   ksq: X  ‘(pl Subj) to go out (of a house)’
   ST, N  ksq: xw  ‘(sg Subj) to go out’

PTsim *kséHqw
   *kséqw

where the distinction is evidenced in the vowel (reflecting the former presence or absence of H), Tsimshianic plural verb stems are formed most generally by reduplication, less commonly by prefixation. Numerous irregularities in both types of plural-formation testify to their antiquity, with evidence for at least three distinct historical periods (e.g., for Nisqa’a: ‘Early, Classical and Modern’, Tarpent 1983).³ Although the meaning distinction between the two types is occasionally blurred in the modern languages, there is still a basic difference in semantic and grammatical meaning.

3.1. **Reduplication**. I mention reduplication only as a contrast to prefixation in both morphological and semantic terms. Reduplication is by far the most common method of plural formation in verbs. Full reduplication, typically CVC>CVC on a root CVC (where “”) follows the reduplicated syllable), indicates repeated action by or on the experient of the action. For intransitive verbs, this usually means a single action performed by plural subjects, as in

(2) N  qas/qís
   pl of  qís  ‘to jump’

For transitive verbs, reduplication indicates either repeated action on the same object, as in

(3) ST  q’ats)q’ot
   ‘to cut, cut up, slice, dice, etc.
   (sg/pl Obj)’
   pl of  q’ot
   ‘to cut (sg Obj)’
   (= make a single cut)

or repetition of the same action on several objects, as in (3) and also in

(4) N  lu: =kix/káí
   ‘to stare at
   (several persons
   in turn)’
   pl of  lu: =káí
   ‘to stare at (one person)’
   (lu: = ‘in’, káí ‘to see (s.)’)

(‘=’ indicates the boundary of proclitic and verb)

Contemporary irregular forms such as N q’á: ?aX, plural of q’áq ‘to be open’
(< *q’áHq)q’aq/*q’aq), si: saX, plural of sáq ‘to be sharp’ (< *séHq)sEq/*séHq), and many others, which show a different stress pattern as well as other irregularities, attest to the antiquity of the CVC>CVC formula in Tsimshianic. So do cases of reduplication occurring independently of derivational prefixation, as for instance in N ?apá: paq’askw < * ?a: *páHq)paq-?skw ‘(pl Subj)

²Sources of Tsimshianic data: except where otherwise indicated, N and ST data are from my own fieldwork (see fn. 9), CT data from Dunn (1978) (sometimes rephonemicized), G data from Bruce Rigsby (p.c.) ST, CT â is a high central unrounded vowel or corresponding glide.

³The major features of the description of Nisqa’a plural formations presented in Tarpent (1983) are still correct in their basic outline, and these basic features also apply to Gitksan, Southern Tsimshian and Coast Tsimshian, but many of my speculations about the derivations of some of the more irregular, older or isolated N forms are outdated now in the light of comparative evidence from the other Tsimshianic languages. In particular, it was only in 1990 that I was able to interpret some puzzling vowel alternations by hypothesizing the intervention of the element “H”
to be affected by a strong emotion, e.g. upset, excited’. A more recent, productive version of the formula (as in [4]) allows stems of any length to be pluralized by the reduplicative method.

3.2. PREFIXATION. The other traditional method of pluralizing intransitive stems (including intransitive verbs and adjectives) is prefixation. The plural prefix 조사- (with vowel adjustment) is now unproductive and only a limited number of forms make use of it.4 As with reduplication, irregularities in some of the forms attest to the antiquity of this method.

3.2.1. REGULAR FORMATIONS (MOST RECENT). In the majority of forms, the prefix 조사- is clearly segmentable, and stress falls on the root. Semantically, 조사- prefixation indicates that the same single actions are performed or single states experienced individually but together by several subjects, almost always persons: older consultants translate these forms as ‘(they, etc.) all ...’. At least in N, the prefix is usually accompanied by the very common “medial” suffix noted as -T (phonologically usually /u/ or /o/ [sic], often deleted in absolute final position). Typical examples are:

(5) N 조사-ʔaks-T pl of ʔaks ‘to drink’
(6) N 조사-ts’ē:x-T pl of ts’ē:x ‘(person) to be full (after eating)’
(7) N 조사-Xsit pl of Xsit ‘to vomit’
(8) N 조사-xwatákw-T pl of xwatákw ‘to shoot’
(9) N 조사-láv-T pl of láv ‘(head, container, house) to be large’
(10) CT 조사-pá:s pl of pá:s ‘to be afraid’
(11) CT 조사-wá:y pl of wá:y ‘to paddle’
(12) CT 조사-hě:tiks (pl Subj) ‘to swim’ N hátiks ‘(person, dog, etc.) to swim’

In N the affixal frame 조사-...-T is especially common with words formed with the prefix ʔa- ‘spontaneously, without obvious agency’, as in

(13) N ʔa-ʔa-l(i)-lá:n-T pl of ʔalá:n ‘(boat, person) to be slow-moving, lag behind’
(14) N ʔa-ʔa-láys-T pl of ʔaláys ‘to be “lazy”, reluctant’
(15) N ʔa-li-ski-T pl of ʔaski ‘to be ugly, unusual- or abnormal-looking’

That the plural prefix occurs after the ʔa- prefix, and is directly attached to the root, shows that 조사-prefixation must be older than ʔa-prefixation (cf. a similar case with reduplication above). In a few cases, the plural form has the combination 조사-...-T, and the singular the suffix -tkw, as in

(16) N 조사-ʔa-ptáλ-T pl of ptáλ-tkw ‘to climb’

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4Many other intransitive stems, including descriptive adjectives, use the prefix ʔa-, formerly only a distributive plural used with nouns. No irregularities are associated with ʔa-.
(17) N *li-skwá:y'-T pl of *skwá:y'-tkw ‘to rest’

It is possible that in such cases the -tkw suffix indicates reflexivity, since both stems also occur suffixless with different (though compatible), less personal meanings (*ptá̄l ‘[tide] to rise’, skwá:y’ ‘[action] to come to an end without achieving result’).

In the following cases, the l- prefixed and the -tkw suffixed forms have diverged semantically, but the same original relationship can be recovered: the suffixed forms apply to a group of persons acting as a single entity, but the prefixed form must have applied originally to individuals all doing the same thing:

(18) N *lisán ‘(sg or pl Subj) to travel to another community’ PTsim *-sén

in order to find summer work in canneries’

The PTsim stem *sÉn is found in CT sá:n ‘to put (pl Obj) aboard’ (Boas 1911:382), CT sá:n-tk, ST sá:n-tku (< *sÉn-tkw), N sín-tkw (< *sÉHn-tkw) ‘(pl human Subj) to ride in a boat, be aboard’.

3.2.2. REFORMATIONS BY l- PREFIXATION ON ALREADY PLURAL STEMS. On the other hand, l- plurals include some ‘pleonastic’ or doubly-marked refections on more archaic plural stems (Tarpent 1983), especially those with Velar prefixes, as in

(19) N lu-xwti:tx-T pl of xwtá̄x ‘to be hungry’

(PTsim sg stem *kw-*té̄x ; reduplicated pl stem *kw-*téHx*tx)

(20) CT la-Xkwí:txks pl of Xkwátx ‘(person) to be cold, have a cold’

cf. kwátk ‘(sg Subj) (object) to be cold’

(PTsim sg stem *kwEt-> pl. stem *q-*kwéHt-*q-s, sg. (*q-) *kwéHt-*tkw)

(21) ST la-Xstó:yX pl of XstóX ‘to sleep’

(PTsim sg stem *qs-*tíeq (< **tÉH-*Eq) , pl stem *qs-*téHh-*Eq)

This use seems to indicate that at one time the stems were no longer perceived as unambiguously plural forms and that the prefix was added in order to confirm the plural meaning (cf. a similar justification for N reduplicated forms in 5.1.1. below).

3.2.3. OLDER FORMATIONS STRESSED ON PREFIX. In a few l- initial plural forms, stress is on the prefix, causing reduction and/or adjustment of the stem vowel (this older pattern of stress on the penultimate is also attested in other forms, e.g. the oldest layer of reduplicative patterns, as in the proto-form for the plural in (19) above): the prefix is reconstructed as *lÉH- (*lÉHh- under stress).5

Modern forms: PTsim plural:

(22) ST lítik pl of ták ‘to wake (s.o.) up’ *léHh-*tek

(23) N lískit pl of skát ‘to be born’ *léHh-*sket

---

5One of the clues to the former presence of *H is ST, CT long /i:/ corresponding to N, G short /i/, in stressed positions (Tarpent 1994). Note also that some of these verbs are transitive, unlike the more modern forms. PTsim roots are not inherently transitive or intransitive.
(24) ST  li:mXs  pl of  *máXs  ‘to grow’  *léHh-*maqs

(= N limq:s-T/*máqs ; the unstressed stem vowel is deleted by an independently motivated rule, between a syllabic resonant and an obstruent; *máqs is attested in derivatives)

(25) ST  kw-li:tix  pl of  kw-táx  ‘to be hungry’  *kw-*léHh-*tex

(compare this prefixed plural with the reduplicated N form in [19] above)

(26) CT  qa-li:paXsk  pl of  qa-páXsk  ‘to shake o./self’  *qa-*léHh-*paq-s-kw
(Boas 1911:380)\(^6\)

In addition to the pattern of initial penultimate stress, the antiquity of this pattern is shown by other irregularities such as different affixes, alternate forms of the plural (both seen in alternate N plurals for ‘to shoot’, compare [27] and [8] above), or unusual reduplication, as in

(27) N  lítuxw  (old) pl of  xw-tákxw  ‘to shoot’  *léHh-*tekwx

(28) N  -li:xw  stem of the irregularly reduplicated form

lí:li:xw-T  pl of  yáxw  ‘to hide’  *léHh-*yexwx

(cf. CT  li:-yū:  pl of yū:  ‘to hide’, rephonemized from Boas 1911:380)

3.2.4. SUB-MORPHEMIC ALTERNATION I-/K- (O L D E S T F O R M S ). In what must be the oldest formations, some I-initial plurals correspond to a singular beginning with a consonant; this is especially common with singular stems beginning with a palatal, which are paired with an I-initial plural stem: these are probably relic forms (see more below, 5.1.).

A number of verbs have an I-initial plural stem corresponding to a singular stem beginning most often with a palatal consonant (k- or y-), in which neither consonant seems amenable to morphological segmentation in a synchronic analysis,\(^7\) as in:

(29) ST  li:mk  pl of  kí:mk  ‘to wipe (sthg)’

(30) ST, N  ló:  pl of  kó: [gyó:]  ‘(boat, vehicle) to be motionless:
at anchor, moored, parked; (person)
to behave, act, live (in a certain way)’

(31) CT  lá:  pl of  yá:  ‘to walk, go’

(32) N  lúkx  pl of  yúkx  ‘to move (in the direction indicated by a proclitic)’

---

\(^6\) retranscribed; cf. N qa-páqskw ‘to thrash around, to struggle’ (archaic reduplicated plural qa-pá:paqskw < \(^*\)-páHqpaq-, cf. same stem in ‘to be upset, etc.’ in 3.1.). The morpheme qa- in these forms is a derivative prefix of undetermined meaning at present, not the homophonous plural morpheme of fn. 4.

\(^7\) Two irregular plurals, N li:pákyw-T pl of kipákyw ‘to fly’ (and similar forms in the other languages), and N li:nítimq:s pl of kinítkw ‘to get up’, appear superficially to have prefixes li- and ki- respectively, but the long unstressed vowel of the plural and, for the second word, the morphological irregularities in correspondences with the other languages, make the history of these forms difficult to reconstruct at present. The popular explanation of kipákyw ‘to fly’ as kip-hákyw ‘to eat-odor’ can be ruled out as folk-etymology although similar forms are attested in the other languages.
and there are also l-initial plural forms without (morphologically) singular counterparts, such as

(33) N  lískw  ‘(pl Subj) to hang’  (suppletive plural of yáq ‘to hang’, which also has orig. meaning ‘to sway, to shake’)

In such cases, it is impossible to characterize the initial consonant as a ‘prefix’, but obviously the l-initial formant is associated with plural meaning, just like the lo- prefix of morphologically analyzable forms. Let us just note that initial l- has been associated with the plural throughout the recoverable history of Tsimshianic. In the next two sections, both the prefix and the sub-morphemic formant are compared to similar items in ‘Penutian’ languages.

4. DUAL/PLURAL AFFIXES THROUGHOUT ‘PENUTIAN’. The Tsimshianic prefix lo- < *lEh- can now be considered in a wider perspective, both internally and externally.

4.1. Tsimshianic *lEh ‘PLURAL’ AND *nEh ‘RECIPROCAL’. The old Tsimshianic plural verbal prefix lo- (< *lEh-) is not isolated in the language, but occurs in close parallelism with the reciprocal prefix no- (< *nEh-), which is still productive. In both cases, there is normally a specific prefix/suffix combination which ‘frames’ the stem word to create intransitive forms applying mostly to actions and relationships of persons. Some N examples (which have counterparts in the other languages) are:

(34) N  lo-?áks-T  ‘(pl Subj) to drink’  (pl of intr stem ?áks ‘to drink’)

(35) N  ni-Limó:m-T  ‘to help each other’  (reciprocal built on trans stem Limó:m ‘to help [s.o.]’)

(36) N  ni-wák-T  ‘to be brothers’  (reciprocal built on kin term wák ‘[male’s] brother’)

Such affixal “frames” are frequent in Tsimshianic. They consist of proclitic/suffix combinations in which the proclitic can be quite variable but the suffix is always a medial or medio-passive suffix (-T, *tkw or -s). Proclitics, most of which indicate motion, location or direction, are normally only loosely bound to the stem, but the presence of such a suffix indicates that the proclitic-stem combination is to be treated as a single unit (cf. the status of English out in to run out vs. to outrun). However, prefixes other than the plural and reciprocal are firmly bound to the stem and do not need to be supplemented by a suffix. This discrepancy suggests that the present plural and reciprocal prefixes were originally proclitics, i.e. relatively free morphemes, rather than bound prefixes (Tarpent 1997:80). Note also that *lEh and *nEh have the CVC shape characteristic of Tsimshianic roots, while true affixes have C(V)- or -(V)C only.

4.2. ‘PENUTIAN’ LANGUAGES. Hymes (1957:78-79) called attention to the Penutian-wide distribution of verbal affixes ‘of the general phonemic shape nV, lV’; affixes in /n/ ‘share the sense of plurality in the relations of persons’ i.e. have reciprocal or mutual kin meanings. Like Tsimshianic, most Penutian languages have affixes in /l/ and /n/ with plural/collective or dual/reciprocal/comitative meanings. Depending on the affixal pattern of individual languages, these may be prefixes, or more often, suffixes (Tarpent 1997:80-81). This difference in the place of affixation makes sense if potential cognates of PTsim *lEh and *nEh were originally freer morphemes and later became affixes, in conformity with the affixation pattern prevailing in individual languages. The frequent restriction of occurrence of /l/ and /n/ formants to unproductive subsets of morphemes, especially pronouns, in most of the languages which have them suggests that these are archaic survivals.

4.2.1. FORMS COMPATIBLE WITH PTSIM *nEh ‘DUAL/RECIproCAL’. Although dual and reciprocal do not mean the same grammatically, they have in common the fact that they refer to
two participants. Similarly, comitative can be viewed as yet another aspect of duality. These three meanings are variously distributed between affixes including the formants $n$ and $l$, especially since not all languages have the category of dual, which may have merged with plural.

This does not mean that only these formants have all or any of the meanings in question in a given language. For instance, Bodega Miwok has a comitative $ni$-, but Miwok dual morphemes are unrelated to it (Catherine Callaghan, p.c.). Such facts do not invalidate my point, since (here or elsewhere) an originally dual morpheme may be preserved as comitative, while another morpheme has assumed the dual function. In any case, I am not asking the question “Do all the Penutian dual and plural, etc. morphemes correspond exactly to the PTsim morphemes in form and function?” but “Given the form/meaning/function of the PTsim morphemes, what morphemes in other languages are both formally similar and semantically and functionally compatible?” Although the formants $n$ and $l$ are often associated with other formants or affixes in individual languages, they do occur throughout the Penutian domain.\(^8\)

a) prefixes

Co Han $ne$- in ‘comitative’ affixal frame $ne$-...-(h)e/a ‘with...’
Mk $n\tilde{o}$- in ‘comitative’ affixal frame $n\tilde{o}$-...-i?yu ‘with...’

We can perhaps add also Cay $ni$- in <nipik> ‘they’. This Cayuse form is not the only one given for ‘they’, there is also <cappick> (perhaps [qāpik]). Even though the Cayuse grammatical data available are only fragmentary, the existence of two separate morphemes both translated ‘they’ suggests that one is (or was originally) dual and the other plural. Dual, etc. forms in $n$-, and plural forms in $q$-, attested in other languages (e.g. Mol $qa$- stem of several non-singular pronouns, Win $q$- initial of collective suffixes) make it probable that Cay nipik must have been a dual, at least at some point in its history.

b) suffixes

‘dual’, ‘comitative’ or ‘reciprocal’

- $n$- initial

Siu -naw(a) ‘reciprocal’
Co -ne:i ‘reciprocal’ (also ‘distributive’)

Co -$n'i$ ‘dual’
Mol -$ni$- ‘dual’ formant in pronominal forms; (cf. also -uni ‘3 dual’)
Mi Mib -$ni$ ‘comitative case’ (≠ dual)

\(^8\) Many of these morphemes are mentioned in Tarpen (1997:80-81). Language abbreviations: Al Alsea, Cay Cayuse, Chin Chinook (Kath Kathlamet), Co Coos (Han Hanis, Mk Miluk), Kal Kalapuya (MR Mary's River, S Santiam), Mai Maiduan, Mi Miwok (Mib Bodega Mi, Mins Northern Sierra Mi, Miss Southern Sierra Mi), Mol Molale, NP Nez Perce, Sah Sahaptian (NS North Sahaptin), Siu Siuslaw, Tak Takelma, Win Wintun (Pat Patwin), Yok Yokuts (Chaw Chawchila, Chuk Chukchansi, Gash Gashowu, Wik Wikchamni, Yb Buena Vista, Yd Yawdanchi, Yl Yawelmani). See bibliography for sources.
-n- final

Co  -əni: ‘one another’ (<-ən-i: ?); (also ‘distributive’)
Tak  -an-, -in- ‘reciprocal action’

Sah  -in ‘dual suffix’ (rather ‘comitative’, in nouns)
NS  -i:n ‘dual suffix’ (rather ‘comitative’, in nouns)
     (cf. Tak -i:t below)

‘Mutual kin’:

Co  -ini: used for ‘mutual kinship’, “etymologically related to the
     verbal distributives -ne:i, -əni.” (LF) (cf. above)

‘Plural’ or ‘collective’:

-n- initial

Co  -ne:i ‘distributive’ (also ‘reciprocal’)
Kal  -ni ‘plural’ suffix attached to 3rd person prefixes

-n- final

Co  -n, -n- ‘plural element’ in some nouns
     -əni: ‘distributive’(<-ən-i: ?); (also ‘one another’)
Mol  -yan- ‘plural’ formant in some suffixed pronominal forms
Tak  -han (-an after C’) ‘collective plural’ of some human nouns
Yok, Yd  -in ‘plural’ in pronouns
     -i:n ‘plural’ or ‘collective’ with animate (numerals)
     -hin, -han ‘collective plural’ (inanimate nouns)
     -yan, han ‘plural enclitics’ used in imperatives

Reduplicated -n- forms designating groups of people:

Ch, Kath  -na:na ‘groups of people with a common designation’ (e.g. chiefs)
Mai  -nono ‘a pile, crowd, lot of … [persons]’
Yok  -(i)n’in ‘people, inhabitants, of …’ (> tribal names)

cf. also

Mai  -nini in pene-ny-nini ‘four’ (plyen- ‘two’)

Complex forms including -n- formant:

Ch  -in-iks ‘plural’ of a few nouns (-iks, -uks, -aks also ‘plural’)
Kal  -ni:k in gwinik ‘3 pl pronoun’ (pron. stem gw-) 
Mi, Mins  -a:ni-k ‘plural; many; a group, bunch, etc.’

4.2.2. FORMS COMPATIBLE WITH P'TSIM *IEh ‘PLURAL’

4.2.2.1. PREFIX. Thus far I have not found evidence of /l/- prefixation before verb stems
outside of Tsimshianic (however, I have not been able to consult all possible sources). An
exception might be found in Siuslaw: after explaining that Siuslaw does not mark plurality in the
verb, Frachtenberg added in a footnote: “I have found only one case of such a differentiation. I
was told that the stem *qua-* ‘to enter, to put in’, refers to singular objects, while the stem *Lxa*-
can be used with plural objects only” (1922b:535 fn.1).
Frachtenberg did not think that such meager evidence was enough to establish a
morphological relationship between the two forms, but the stem *Lxa*- would seem to be
plausibly derivable from *l-qaa-* since only *L* occurs before consonants, and plain velar or uvular
stops do not occur after *L*. This one instance is hardly enough to make a case, but might be
significant if more comparative evidence of the same kind comes to light.

4.2.2.2. SUFFIXES. However, there is plentiful evidence for *l* in suffixes, usually with plural
meaning, sometimes also with dual meaning:

a) ‘plural/collective’

<table>
<thead>
<tr>
<th>Language</th>
<th>Suffix</th>
<th>Meaning</th>
</tr>
</thead>
<tbody>
<tr>
<td>Al</td>
<td>-l</td>
<td>‘plural’ in some forms (e.g. yo:l- ~ yu:l- pl of ye:a ‘to talk, speak’)</td>
</tr>
<tr>
<td>Co</td>
<td>-l</td>
<td>‘plural’ of some stems (alternating with -n- for other stems)</td>
</tr>
<tr>
<td>Km</td>
<td>-l’</td>
<td>‘plural’ suffix in some pronouns</td>
</tr>
<tr>
<td>Tak</td>
<td>-l</td>
<td>‘plural’ of some stems (alternating with -n- for other stems)</td>
</tr>
<tr>
<td>Win</td>
<td>-le</td>
<td>‘pronominal plural’</td>
</tr>
<tr>
<td></td>
<td>-VIV(h)a</td>
<td>‘disparity of subject: many separately to …’ (reduplicated verbal form)</td>
</tr>
</tbody>
</table>

cf. also 

Yok

Yl, Chaw, -i...l  ‘times’

Chuk, Gash

b) ‘dual’

<table>
<thead>
<tr>
<th>Language</th>
<th>Suffix</th>
<th>Meaning</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sah NS</td>
<td>-l</td>
<td>‘dual’ meaning in some forms;</td>
</tr>
<tr>
<td>Win</td>
<td>-l/l</td>
<td>‘pronominal dual’ (e.g. ne:l ‘we, dual inclusive’; here ne- ‘1sg’)</td>
</tr>
<tr>
<td></td>
<td>(-)lel</td>
<td>‘dual’ in pa:lel ‘two, pair, both’</td>
</tr>
</tbody>
</table>

cf. also 

Tak

-di:l “dual”, actually ‘comitative’ (‘with X’) in nouns
(possibly < -t-i:l, cf. Sah-i(:)n ; cf. also Tak plural -than < t-han )

4.2.2.3. COMPLEX AFFIXES. There are also -l- components of more limited distribution in
complex plural pronominal affixes, usually contrasting with dual ones which include other
consonants. Depending on the language, these complex affixes may be prefixed or suffixed to
the verb. In Chinook, they are prefixes: compare -lx- ‘1 pl incl’ with -tx- ‘1 du incl’. Similarly
complex forms occur for instance in Coos or Siuslaw, but they must be used with great caution,
since the complexity of the forms and the lack of clear morphological correspondences between
the various languages suggest analogical reformation and/or influence from neighboring
languages such as Salishan. Trying to unravel these problems is beyond the scope of this paper.

However, it is relevant that Klamath and Wintu have what appear to be parallel, alternate 1st
and 2nd person plural forms with suffixes including formants /l/ and /h/ (DeLancey 1987:51):
comparison with Chinook makes is quite likely that these parallel forms were originally plural
and dual respectively (cf. alternate ‘plural’ forms in Cay above), even though they might now be
exclusively plural. In any case, all these provide more support for an original plural morpheme
or formant in /l/ widely distributed in Penutian.
4.2.3. Summary on / and n formants. As in Tsimshianic, there are parallel affixal formants in /l/ and /n/ in many Penutian languages, where they occur most often in suffixes, in keeping with the prevailing affixal pattern in those languages. That their occurrence is often restricted to certain non-productive morphological categories, commonly pronouns, means that these are probably old inherited morphemes. That forms in /n/ especially occur about as often with plural as with dual meanings could indicate loss of the dual category (with dual > plural), since not all the attested languages have a dual. It may also be relevant that /l~n/ interchange frequently occurs because of consonant gradation, which is morphologically productive in some of the languages. Relics of this gradation could explain that in some cases forms in /l/ and /n/ seem to be specialized, for instance between human and inanimate nouns.

5. Plural formant /- replacing initial root-consonant. As in Tsimshianic, there is scattered evidence in other Penutian languages of /- initial CVC stems or roots with plural meanings, sometimes alternating with singular stems beginning with palatal or uvular/glottal consonants. In many cases the consonants are the same as in Tsimshianic, or closely related. Singular and/or plural stems involved in this alternation have similar meanings in Tsimshianic and other languages. Unfortunately, I have only been able to consult a limited amount of material on other languages, so that the fact that some languages are not represented in the examples below does not therefore mean that the feature in question does not occur in them. However, the /-k/- initial alternation which occurs in some Alsea verb forms is independent of the one discussed here.

5.1. Tsimshianic. As mentioned above (3.2.4.), the Tsimshianic languages have a small number of what appear to be plural 'roots' in /- corresponding to singulars in /-y/-, and also but less often /-q/- or /-h/- (i.e. palatal and uvular/glottal stops and glides), here all represented by the cover term /K/. In several cases, only one member of an /l/K stem pair is attested in a given language, but the other one occurs with the same or a very similar meaning elsewhere in the family. There are also several /- initial stems with plural meaning occurring without a corresponding singular, and a few which are not identified as either singular or plural but which could have been plurals originally. In these cases the /l/K alternation cannot be explained simply by deletion of the initial consonant after addition of the prefix /lEh/ (i.e. /lE/AC cannot be from /lEh-*KE/AC), nor is it likely that addition of a monosegmental /- prefix would have caused deletion of the /K/ consonant (i.e. /lE/AC cannot be from /l-KE/AC). More probably, in Ptsim (or even Pptsim) the stems were originally bimorphemic (as were some others, independently attested, e.g. in [21]), thus **/KEh-**VC for the singular, later *KVC, and **/Eh-**VC for the plural, later *IVC. The meaning of the initial elements subsumed under **KEh cannot be determined at present. Alternations among the four /K/-consonants may have been due at least in part to consonant gradation, many traces of which exist in various Penutian languages, even where it is not a productive process (as it is for instance in Sahaptian).

5.1.1. Most Tsimshianic cases have palatal-initial singular stems:

a) /- initial:

<table>
<thead>
<tr>
<th>Ptsim</th>
<th>(37) N</th>
<th>ló:</th>
<th>pl of</th>
<th>kó: [gyó:]</th>
<th>'boat, car) to be moored, parked'</th>
<th>/l/káw</th>
</tr>
</thead>
<tbody>
<tr>
<td>(38) N</td>
<td>lá:L</td>
<td>pl of</td>
<td>ké:L [gyé:L]</td>
<td>'person) to lie down, be in bed'</td>
<td>/l/káHL or /l/káHh-L</td>
<td></td>
</tr>
</tbody>
</table>

(here the long é: of the singular form is due to a general N rule raising long *á: after palatals and sibilants, Tarpent 1983)

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(39) ST lü:nks kü:nks [gü:nks] ‘to be dry’ *l/kéHw-n-ks
(40) ST lámk kámk ‘to be hot, warm’ *l/kém-k
(41) ST li:mk ki:mk ‘to wipe (sg/pl Obj)’ *l/kéHm-k
(42) ST láp káp [gyáp] ‘to dip up (sg/pl Obj)’ *l/kép

In N such plural stems are often reduplicated, as in

(43) N limlámk pl of kámk ‘to be warm, hot’ *l/kém-k
(44) N limlimk kimk ‘to wipe (sg/pl Obj)’ *l/kéHm-k
(45) N lipláp káp [gyáp] ‘to dip up (sg/pl Obj)’ *l/kép
(46) N liplápín kápin [gyábin] ‘(aquatic mammal) to come up for air’ *l/kép-n
(47) N liplápkš kápkš [gyápkš] ‘to be high’ *l/kép-ks
(48) N ?as-lipá:paX ?as-ká:paX ‘to talk incessantly, be a chatterbox’ *?ats- *l/káHp-q

These cases are attributable to a secondary formation emphasizing plurality (cf. reduplication also in N lì:li:xw (28) and addition of lə-...-ə to already plural forms, 3.2.2. above).

b) y- initial

(49) CT lā: pl of yū: ‘to go, walk’ *l/yā(H)h
(50) N lūkw yūkw ‘to move (us. in direction of proclitic)” *l/yēHkw

Some non-plural stems of the shape ST li:C, N lìC can be plausibly related to singular forms in yāC (Tarpent 1983):

(51) N -lil-p stem of ‘to roll (sg/pl)’ *léHl-
cf. yāl- ‘to move in circular motion’ *yēl
(52) ST li:ts-X, N lìts-X ‘to count, read (sg/pl Obj)’ *léHts-q
cf. yāts- ‘to hit, beat (sg Obj)” *yēts

(probably from hitting fingers on a support in order to count, while counting aloud: *-q ‘by vocal means’)

5.1.2. A few other stems have uvular or glottal initials: these are rarer than the palatal initials, but it is possible that this rarity is due to semantic change obscuring the relationship between singular and plural stems which now appear unrelated, as well as to the preservation of only one of the stems in several cases.
a) h- initial

(53a) G **litxw** pl. of **hitxw** (= N **hitkw**) ‘to stand’ *(stem hit/hit-)*

(53b) N **litiks-** (pl only) *(laundry) to be/put in water* *(stem lit-)*

cf. N **hátiks** *(person, dog, etc.)* to swim *(stem hat-)*

The original meaning in (53b) must have been ‘to stand in water’, i.e. ‘bathe’ rather than ‘swim’ (-iks cf. **háks** ‘water’ < *'éks*; loss of initial a is attested in other compounds which include *'éks* (Tarpent 1983).

Another case in which the plural and singular stem have acquired separate meanings is:

(54) N -lánx in **t’im-lanx** ‘neck’ *(t’im- < t’á:-m- ‘place of’)* *(*lé/án-x*

N **hánx** ‘to be thin (not thick)’ *(he/án-x)*

The N form **hánx** has a reduplicated plural **han(h)ánx** which also means ‘temples (of the head)’ (two places where the head narrows in front, and is narrower than, for instance, the prominent cheekbones common in the population of the West Coast). The l- initial form **lánx** in **t’im-lánx** ‘neck’ is most likely an old plural corresponding to **hánx**: the neck is another body part that is definitely thinner than the head above and the shoulders below. That this form is built on a plural stem reflects the symmetricality of the neck, which appears ‘thin’ from all viewpoints in comparison with the rest of the body.

b) q- initial

Examples of q- initial singular stems with l- initial plural stems are also few. An obvious one is:

(55) N **línX** pl of **qínX** *(tree) to fall* *(PTsim form still unclear)*

In the following example, CT has reformed a reduplicative plural on an originally plural stem, as shown by comparison with other CT and Nisq’a forms which preserves the q- initial singular stem:

(56) CT qáp- in **lágapal** *(sg) “(stars) to twinkle”* *(lágapal)* *(le/áp-1l)*

CT qápsl qap’é:ltk *(star) to twinkle* *(qé/áp-(k)-s-?l-T)* *(qé/áp-s-?l)* *(qE/áp-éh-?l-tkw)*

In the following example, the N l- initial form, which is relatable to a q- initial form also attested in ST, has acquired a distinct meaning: from the singular stems

(57a) ST qá:úlká ‘to wind/wrap sthg *(around s.)*’ *(qéHw-l-kw)*

N **qilkw**

one would expect an l- plural such as N **lilkw**, which indeed exists:

(57b) N **lilkw** ‘to lace (shoes, leggings, etc.)’ *(leHw-l-kw)*
(Note that lacing the old type of leggings or boots means wrapping cords around the leg.)

Other non-plural forms in N li*C can be plausibly related to forms in qilC:

(58) N liL-k ‘to (keep) watch on, tend
     (sg or pl Obj)’
    liL-k-s ‘to keep watch, keep an eye
     on things’
N qilLXkw ‘to shout, holler’
       *qêHL-q-tkw

The form qilLXkw analyzes as *qil-q-(t)kw (*-q ‘at/by mouth’, esp. ‘by vocal means’; -tkw adds intransitive meaning). If the original meaning of *liL/*qil was ‘to watch, be watchful’, qil-q could have been ‘to keep watch by vocal means’ = ‘to give warning by shouting’, later simply ‘to shout’.

5.1.3. Some l-initial verbs are plurals without a corresponding singular (isolated or suppletive forms):

(59) N lin ‘(wind, etc.) to cause (trees) to fall’
     (=linX-ʔ,n, see (55); -ʔ,n ‘causative’)

(60) ST lâmtsaX ‘(pl human Subj) to come in, enter’
     (suppletive pl of ts’i:n) (= N lâmtsaX/ts’i:n)
     *lê/âm-ts-q

The association of l-initial forms with the plural suggests that some similar forms now used for both singular and plural could have been exclusively plural at one time, later acquiring a more general meaning, for instance:

(61) ST li:mx; CT li:mi; N, G limx ‘(sg or pl Subj) to sing’
     *lêHm-x

(where the H in the PTsim form supports the original plural hypothesis, see 3. above)

5.1.4. CONCLUSION ON TSIMSHIANIC. Tsimshianic l-initial (submorphemic, not prefixed) forms represent an archaic pattern. PTsim *CVC stems are normally reconstructed as including the initial consonant. Isolating this initial involves splitting the PTsim “root” to recover the PPTsim stage: e.g. N liC ~ KiC < *lêHC ~ KêHC < **lEh-.**EC ~ **KEh-.**EC (see (21) for independent evidence for such bipartite PPTsim forms). It is relevant that none of the K’s in the l-/K- pairs are glottalized, since initial glottalized consonants can all be traced to PPTsim **?EC- which does not fit the alternation pattern (see 2. above).9

5.2. ‘PENUTIAN’ LANGUAGES. The existence of l- and K- initial roots with similar meanings, where l- initial roots are often associated with plural meaning, suggests a similar origin in Tsimshianic and in other Penutian languages which have this pattern. Wintun has some

9 Dunn (1978) gives some CT forms which seem to conflict with this generalization, but his data include so much individual and/or dialectal phonetic variation that it is difficult to establish a reliable base form for comparative work. In fieldwork with a single ST consultant (Mrs. Violet Neassloss, probably the last competent speaker of the language), I have often found it very difficult to determine whether I was consistently hearing a glottalized or non-glottalized consonant, or a uvular or velar. However, it is usually possible to establish reference points from minimal or near minimal pairs. In most cases, glottalization or non-glottalization of initial ST consonants agree with the situation in Nisqa’a, the language with which I am most familiar, having learned it while residing in a Nisqa’a community (New Aiyansh, B.C.), from excellent speakers who have also become very competent writers (especially Mrs. Verna Williams, Mrs. Rosie Robinson and Mrs. Nita Morven). I take this opportunity to thank all these wonderful consultants and very dear friends.
clear internal examples of this alternation but it also exists elsewhere. Non-paired roots and stems with the same or compatible initial consonants and the same or a compatible meaning are even more frequent. Note that as in Tsimshianic, the stops involved are not usually glottalized except in languages like Takelma, where consonant gradation plays a morphological role, and Wintun, which has many traces of now unproductive initial consonant gradation, e.g. c-\textit{ch-}/c\textsuperscript{-}, which must have a secondary origin. Only a few examples are mentioned here, but it is likely that a systematic search throughout the Penutian domain would turn up many more.

5.2.1. SOME WINTU EXAMPLES.

5.2.1.1. \textit{l-} // \textit{y-} \, \textit{lew-} 1 > \textit{lewča} ‘to pick (as berries)’
\textit{‘cf. yew-ča “to pick, pluck, gather (as berries)” (AS; see 5.2.2.1)}

5.2.1.2. \textit{l-} // \textit{q-}

(1) \textit{lOr-} ~ \textit{lo:r-} ‘grind, rub, file’

\textbf{Win} \quad \textit{lo:ra} \quad ‘to file or grind in order to sharpen’
\textit{loroqua:} \quad ‘rub hands together; grind (teeth)’
\textit{loruma:} \quad ‘grind o’s teeth’
\textit{lura:} \quad ‘to grind’

\textbf{Pat} \quad \textit{loru} \quad ‘to grind sharp or smooth’

(2) \textit{qOr-} in

\textbf{Win} \quad \textit{gori} \quad ‘powder from pounded seeds or grasshoppers; seed for flour’
\textit{gura:} \quad ‘to grind’

\textbf{Pat} \quad \textit{koru} \quad ‘to grind’ (there is no \textit{q} in Pat, \textit{k} corresponds instead)

5.2.2. Tsimshianic and other examples (in this context E/A are cover symbols for root-vowel types, not reconstructions unless indicated):

5.2.2.1. \textit{1E/Aw} 1 // \textit{kE/Aw} / \textit{yE/Aw} 1 ‘to pick, gather, assemble, etc.’

5.2.2.1.1. Meaning 1 ‘to pick, gather, etc. (usually food items)’

a) \textit{l-} initial

Root-final \textit{\-w}

\textbf{Win} \quad \textit{lew-} 1 > \textit{lewča} \quad ‘to pick (as berries)’ (cf. above)

Root-final \textit{-b-} or \textit{-p} (\textit{w} \scriptstyle{\sim} \textit{h/p} is a common correspondence in Penutian)

\textbf{Tak} \quad \textit{leb-}, \textit{le:b-} \quad ‘pick and eat (eg seeds)’
\textit{lebe-} \quad ‘“pick up” and eat (seeds)” (non-repetitive vb-stem)
\textit{lē:plup} \quad ‘(non-aor) pick and eat many (seeds)” (redup = repetitive vb-stem)

\textbf{Yok Yv} \quad \textit{lapa:y} \quad ‘gather greens’, \textit{Yt tapay ‘pick, gather” (VG 80:61)’} (The correspondence \textit{l \sim t} is normal between these two Yok dialects.)
b) \(k\)- initial

**Root-final -w**

**without initial augment**

PTsim

\( *kEw > \)

\( *k\dot{e}w > \text{ST } k\acute{a}w \text{ [g\acute{a}:w]}, \text{ N } k\acute{u}: [g\acute{u}:] \) ‘take, pick, get, gather, etc.’

\( *k\dot{e}\text{Hw}-?l > \text{ST } k\acute{u}:l, \text{ N } k\acute{l} \) ‘to take, pick up (sthg)’

\( *k\dot{e}\text{Hw-kw > ST } k\acute{u}:k\dot{u}, \text{ N } k\acute{i}:kw \) ‘to pick (berries), dig for (clams, etc.)’

\( *k\dot{e}\text{Hw-m > N verb-forming prefix } \text{kim-} \) ‘to buy (sthg)’

\( *k\dot{e}\text{Hw-t-ks > ST } k\acute{u}:tks, \text{ N redup kijkitks} \) ‘to buy ... (sg Obj)’

**Co**

\( k.o:w- \) (LF), \( g.uw-, g.u:- \) (MJ) ‘to look, search for s.’

**Kal**

\( \text{geew- in } \text{geewi, geewu} \) ‘pick (berries)’

\( \text{to gather sthg} \)

**with initial augment or prefix**

**Al**

\( -ku:- \) in \( pku:ts- \) ‘to pick, gather (e.g. shellfish, edible insects), catch (food animals)’ (\( p\)- common verbal prefix or augment)

**Si**

\( -ku- \sim -kwa- \) in \( \text{Laku- } \sim \text{L(a)kwa- } \sim \text{Lkw-} \sim \text{Likw- } \) ‘fetch, get, gather, take, etc.’ \((L(V)- \) common verbal prefix or augment)

**Yok**

\( \text{yhiw} \) in \( \text{phihiw } \) ‘catch’ (\( -\text{hi} \)- assibilated from \( k \)-; \( \text{phi-} \) cf. Al \( p\)- above?)

**Root-final \(-b\) or \(-p\)**

**Win**

\( k'ap \) ‘to be caught’ (note C- \sim C- from consonant-gradation as above)

\( k'ampa \) ‘to trap’ (\( -ma \) causative)

\( kapu \) ‘to dig’ (cf. above, ‘dig for clams’, and below, ‘dig for roots’)

**c) y- initial**

**Root-final -w**

**Si**

\( \text{yaw-}, \text{yaw- } \) ‘to pick (berries, etc.), to get (pitch, etc.), to dig [for roots]’ \( > y\acute{u}:\text{yauwt}! \) ‘one who picks, picker’ (agentive suffix \( -t! \) )

**Mai**

\( jawe \) ‘to gather, pick (berries, clover, etc.’)

**Win**

\( yew- \sim yewe\acute{a} \) ‘to pick, pluck, gather (as berries)’ (cf. \( \text{lew- } \) 1)(AS)

**Yok**

\( yuvet \) ‘comprar’

and perhaps also

**Mi**

\( \text{ju:ju- } \) ‘to gather (tr)’ (older redupl. on stem **ju:- ? cf. Yok below)

**Root-final \(-b\) or \(-p\)**

**Km**

\( \{yeb-\} \) ‘dig’, e.g. \( \text{/yebni:ya/ } \) ‘digs for s.o.’

**Win**

\( yube \) ‘to pick water grass’, ‘to gather berries in a basket’
5.2.2.1.2. Meaning 2 'to gather, assemble (people)'

PTsim *kEw ‘to take, pick, gather’ is also found in

(62) CT *kHw-?n-?skw ‘be together (humans); one family, one group of people’;

(la- ‘plural’), cf. above 3.2.1.; *?-n ‘causative’, *?-skw ‘antipassive’; hence literally ‘causing [people] to gather’)

The other descendants of PTsim *kEw (see above 5.2.2.1.1.) do not have a plural counterpart in the modern languages (suppletive stems are used), but such a form could be expected to have an older plural counterpart *lEw. This form, unattested as a bare stem, is preserved in words which were originally noun-incorporating verbs:

(63a) ST *lé(H)w-?l ‘to hold a “feast” (potlatch)’

(63b) N *léHw-?l ‘to hold a “feast” (potlatch)’

Compare *lé(H)w-?l with the singular forms in 5.2.2.1.1. (b) above: ST kü:'l, N kil- ‘to pick, gather s.’ is from *kHw-?l; *kEw ‘people’ occurs as the nominal component of several words interpretable as old noun-incorporating verbs. Hence the original meaning of the forms for ‘feast’ must be ‘gathering/assembling people’. In CT this switch to the specialized, ceremonial meaning explains that the new, morphologically transparent term luki:nsk was coined for a less exceptional gathering of people. This meaning of gathering people as well as food or other resources is also found in other Penutian languages for similar corresponding stems in l- or in k-:

a) l- initial

PTsim *lEw > ST lü:'kit, CT lüülkit, N lil'kit ‘to hold a “feast” (see [63])

Yok Ywl *lOw- in

*lOwOn ‘attend feast’, ‘cf. Chukchansi low-e? ‘to gather’” (VG)

*yOwl ‘to gather’ (= *ya-low, VG 1964:62)

(the last form is one of many examples suggesting archaic prefixation in a distant proto-language, cf. Tarpent 2000)

b) y- initial (p- final root, cf. above)

Win yup- in

yupa ‘(people) to gather’

winyupus ‘get-together, big time’ (win- ‘people’)

c) k- initial

Kal geewu ‘to assemble (intr)’

geewuhwna ‘gathering, assemblage’ (= ge:wufna; -na nominal)

Yok Ywl ke:wi ‘to meet’

Chuk ke:w-e? ‘to meet’ (cf. above low-e? ‘to gather’)

cf. also
Win Pat  \textit{k'awo} ‘to get together, gather’ (C- \textsim C- gradation frequent in Win)

\textbf{5.2.2.2. LE/Aw 2 // yE/Aw 2 / hE/Aw ‘speak, shout, etc.; think, imagine, feel’}

\textbf{a) \textit{l-} initial}

\begin{itemize}
\item \textbf{Kal}  \textit{lāw} ‘call’, \textit{lālāw} ‘shout’ (perhaps borrowed from Tak)
\item \textbf{Tak}  \textit{le(ː)w-} stem of \textit{leu-}/\textit{le:wilau} ‘call by name’
\item \textbf{Win}  \textit{lew-} 2 > \textit{lewega} ‘to tell’
\item \textbf{Mi}  \textit{liwa-} ‘word, language’, \textit{liwːa-} ‘talk’
\item \textbf{Mins}  \textit{liwa-} ‘to talk, to say, to speak (esp Indian); animal to make the characteristic sound; word, language, story, news, question, voice’ (several derivatives)
\end{itemize}

\textbf{b) \textit{y-} initial}

\begin{itemize}
\item \textbf{root-final \textsim w}
\item \textbf{PTsim}  \textit{*yEw} > \textit{*yēw-L-mq} > ST \textit{yā:wLMX}, N \textit{yū:Limq} ‘to advise, lecture (sbdy)’
\item \textbf{Al}  \textit{yEa} ‘to speak’ (could be \textit{[yəa \sim yua]} < \textit{*yEwa} ?), pl \textit{yo:l-}, \textit{yu:l-}
\item \textbf{Sah}  NS \textit{-yawau-} ‘to cry, halloo’
\item \textbf{Kal}  S, MR \textit{yuwi} ‘to make noise’
\item \textbf{S}  \textit{yuwile, yavule} ‘to talk about (sthg)’
\item \textbf{Tak}  \textit{yaway-}/\textit{yiw-}, \textit{yiwin} ‘to talk’;
\item \textit{yiː:w-}/\textit{yiw-} in \textit{yiː:w-an-} ‘play (musical instrument)’ (-\textit{an-} causative)
\item \textit{deː-}\textit{yiː:w-} ‘to sound (intr), give forth a sound’
\item \textbf{Yok}  \textbf{Yn} \textit{yo}:\textit{yo} ‘to call, summon’ (< old redup. form? cf. Mi \textit{ju}:\textit{ju-} above)
\end{itemize}

\textbf{c) \textit{h-} initial}

\begin{itemize}
\item \textbf{PTsim}  \textit{*hEw} \textit{*hēHw} > ST \textit{hāːːu}, CT \textit{hā:w}, N \textit{hi} ‘to say, what is said, how one feels’
\item e.g. N \textit{hi} = \textit{L qː:t-y} ‘I think (lit. “my heart says”)’ (\textit{qː:t} ‘heart’, -\textit{y} ‘1S’); ST \textit{wi-hāːːu-tkũ}, CT \textit{wi-hāːːw-tk}, N \textit{wi-hǐ-tkw} > \textit{wiːitkw} ‘to cry’;
\item \textit{*hēw} > ST \textit{hāːːw}, N \textit{-hũ} ‘what is said, etc.’; ST \textit{sim-hāːːw}, N \textit{simũ} < \textit{sim-hũ} ‘to be true, to be the truth’ (\textit{sim} ‘well, best, etc.’); ST \textit{sim-hāːːw-tkũs}, N \textit{simũtũs} ‘to believe, esp. to be a Christian’
\item \textbf{Co} \textit{haw-} ‘to think, to imagine’
\item \textit{hēwes} ‘a lie’, -\textit{hawas-} in \textit{eʔhawasanáː\.is} ‘you are lying to me’
\item \textbf{Mk} \textit{hEwE:sE:nu} ‘tell a lie’
\item \textbf{Tak} \textit{hēw-} in \textit{hewehehaw} ‘to think, imagine’ (us. with \textit{gel-} ‘breast = here ‘heart’)
\item \textbf{Mai} \textit{hēw} ‘Sure.’
\item \textbf{Mi}  \textbf{Mins} \textit{haw-} in \textit{haw:c-y} ‘to tell’, \textit{hawcny-} \textit{~ hawicny-} ‘to tell, to explain, to guide, to show’
\end{itemize}
5.2.2.3. lE/Am-// hE/Am-

a) /- initial

PTsim *lE/Am : *lē/âm- > ST, N lâm- in lâmtsaX ‘(pl Subj) to enter’
(suffix-tsx < *-ts-q, unglossable at present)

Tak lem- in lem(e)k/- ‘(people) move, go; to take along (pl Obj)’
leme?-x/-lem?-x- ‘(people) go, come together’
e.g. lemê?x ‘they all went’, lemê?k ‘they took them along’
s.-gwi di: lêmk’iauk ‘where have they all gone, any way?’
e.g. in composition with hê:- ‘away, off’ > ‘to destroy, kill (pl Obj)’
as in hê:-i:-lêmek!?in ‘I killed them off’ (i:- ‘hand’)

Kal la:mo, lam:u ‘to go in’
la:mi?:, lam:i ‘to take in’

b) h- initial

- meaning 1 ‘bring together into one bundle or group’

Si ham- in hamx- ‘to tie’ (ham-x-)(= bring together into one bundle)
(ex’s given are of tying hair, hands, as well as sthg on sthg else)

Tak hem(e)-k/- ‘to assemble’ (= ‘get/bring together into one group’)
al-heme-k- ‘to meet (one person)’

Yok Ywl ho:m-in *hO:mOn ‘to welcome, greet’

- meaning 2 ‘bring/take out (sg Obj, or one group, mass etc.)’

Co hem/-hem- ‘bring out, take out (sg Obj)’

Kal S hemyet [hem-y-et?] in deni-hemyet ‘and then they would take them
[in acorns] out’ (in one mass)

Tak hem-g- ‘take out (sg Obj, or in one group/bunch/mass)’
e.g. ba-i-hemegā?in ‘I took (food) out’ (ba-i- ‘out’)

5.2.3. Example unattested in Tsimshianic: lim // yim / *kim ‘flash, blink, shut eyes, etc.’

a) /- initial

Win Pat lim- in
limbâk ‘to flash at intervals; to blink (pl) at intervals’
limbâku ‘to blink (sg)’ (backformed from limbâk ?)
(-bak, -bok suffix or formant > ‘compounds of iterative motion or appearance’)

b) y- initial

Co yim- in yi’mat ‘“is twinkling” (his eyes)’ (probably = ‘is blinking’)
yi’yim ‘eyelash’ ‘compare yim- ‘to twinkle’” (LF)

C) k- initial > assimilation before i

Win c’îm- (glottalization < C-gradation ?) in
 c’îmêča ‘to bat o’s eyelids’
c’îmêčîmêča ‘to blink continuously’
\[\text{'cimi:ra} \text{‘to blink (fast or repeatedly), etc.’} \\
\text{'cime:qa} \text{‘to close o’s eyes, have o’s eyes closed, be asleep (esp. children’)}
\]

Yok Yl \text{*cimik’-wiyi} \text{‘shut the eyes rapidly’} (\text{wiyi} \text{‘to do’})

cf. also without glottalization

Co \text{tsim-} \text{in redup. form tsimint ‘(pl Subj) to sleep (suppletive form’)}
\text{(<tsimintsm-)}

Mi Miss \text{sympy-} \text{‘to close the eyes’}

5.3. INITIAL C-ALTERNATIONS VERSUS ABERRANT CORRESPONDENCES. The examples above show that the pattern of alternating initial consonants in \text{l-} and in \text{K-} occurs not only in Tsimshianic, but also in other languages, although not all show a pairing of forms. While \text{l-} is often associated with the plural, there is at present no way of explaining the difference between the various consonants subsumed under the cover symbol \text{K}, but it is likely that the originally bipartite nature of many Tsimshianic stems at a remote historical level (*CVC < **CVh-**VC) could be postulated also in the other languages.

The alternation pattern which is evident in the above examples can explain some cases of what appear to be phonologically aberrant correspondences. For instance, Broadbent & Pitkin (1964) found a few examples of a correspondence Win -h-/Mi -l-, as in ‘cough’: Win q’uhe, Pat k’ohio, Miss kol’e (p. 37) which they attributed to an ‘aberrant protoconsonant *lh’ (p. 30).

Such a correspondence is indeed very strange, and in this particular case the stem of the Win and Pat forms could be interpreted simply as onomatopoeic and unrelated to the Mi form.

The case of ‘sing’ is more interesting: Pat moho, mohi ‘song’ (cf. also muhu ‘to sing’), Mics myl:i- ‘song’, Miss myl:i-, my:ly- ‘song’ (p. 43; Mi alternations between long and short vowels and between long and short medial consonants are morphologically conditioned). It is relevant that all these forms begin with mV-: an initial sequence mV- is attested in numerous forms in a number of Penutian languages, not only as an apparently submorphemic formant (as in Wintun and Miwok), but also as a normally segmentable prefix (as in Tsimshianic and Alsea) (Tarpent 1997, 2000).

Rather than being due to an aberrant phonological correspondence h/l, the Pat and Mi forms for ‘sing, etc.’ could be interpreted as deriving from ancestral forms including the formant mV-, prefixed to h- and l- initial versions of ancient roots compatible on the one hand with PTsim *hE/Aw ‘speak, etc.’ (see above) in a remote ancestor of Patwin, and on the other hand with a corresponding alternate *E/Aw in a remote ancestor of Miwok (i.e. at a deeper level than currently reconstructed Proto-Miwok). The fact that these languages are now exclusively suffixing does not mean that their ancestors could not have had prefixes, and it is probable that Sapir’s “characteristic Penutian form of stem”, namely CVCV(C), which is particularly noticeable in the languages of California, is a secondary development from either **CVC-**VC or **CV-**CVC (Tarpent 2000). These are only hypotheses at present, but they appear to converge at least in these examples.

6. CONCLUSIONS

6.1. TSIMSHIANIC AND PENUTIAN. The presence of /l/ and /n/ shaped affixes or formants with similar meanings in the morphology of Tsimshianic and of many Penutian languages is suggestive, but could have other explanations since the sounds are very common and these formants are often combined with others. But the occurrence of parallel /l- and K- initial ‘roots’ in which the /l- form is most often associated with the plural (or at least, never exclusively with the singular) is so unusual that it is difficult to attribute it to anything else than shared inheritance. Nor is this grammatical resemblance divorced from lexical and phonological resemblances, since Tsimshianic patterns of alternations in individual /l- and K- initial roots also occur in phonologically similar roots, with the same or compatible meanings, in the other
languages considered. The tradition of comparative linguistics teaches us that it is submerged, unproductive features which provide the strongest evidence for distant genetic relationship. This instance of convergence of grammatical with lexical and phonological evidence is one of strongest, if not the strongest thus far, of the many features that suggest Tsimshianic affiliation and Penutian genetic unity.

6.2. PENUTIAN-WIDE COMPARISON. In most attempts to establish the potential genetic relatedness of the members of the ‘phylum’ postulated by Sapir, emphasis has been on surface similarities rather than on the deeper layers of morphological structure, what Sapir called ‘the most intimate and fundamental features of the morphology’ (1921:59). The depth of historical reconstruction possible within the Tsimshianic family and the very strict constraints imposed by the structure of the proto-language provide morphological and phonological anchor points with which to compare features of other Penutian languages, some of which are otherwise barely noticeable except as curiosities (e.g. the unusual coincidence of the Wintun stems *lew*- and *yew*- both ‘to pick (berries)’), or appear to run counter to normal phonological processes (e.g. the Win/Mi *h/l* ‘aberrant correspondence’). The recognition of the potential kinship between *l*- initial stems and *K*- initial stems, and between stems beginning with any of the four *K* consonants, opens up new avenues for Penutian-wide comparison, since correspondences which may have been unnoticed, or dismissed because of the lack of phonological plausibility might now be fitted into patterns which, although they may be submorphemic in the modern languages, may nevertheless be very regular.

At a mid-point between general typological characterization (Sapir’s phyla) and the emphasis on ‘lexical sets’ appropriate among closely related languages but of limited usefulness for long- range comparison, attention to demonstrably archaic morphological features leads back to individual surface forms with a deeper understanding of their origin and increased confidence in the validity of the comparisons.

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A REPORT ON GEORGE GREKOFF'S COLLECTION OF CHIMARIKO (AND OTHER) MATERIALS

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George Grekoff was a graduate student at UC Berkeley in the '50's and early 60's, who did research primarily on the Chimariko language. He died in 1999, and willed his enormous collection of materials to the Survey of California and Other Indian languages. Chimariko had ceased to be spoken before Grekoff began his research, and his work was based mainly on the field notes of Edward Sapir and J. P. Harrington. Included in his collection are xerox copies of Sapir's, Dixon's, and Harrington's field notes, but the bulk of the materials are his reorganized notes, lexical lists, and analyses—enough to fill two large double-door storage cabinets altogether. We are grateful to Mr. Grekoff's daughter Christine Grekoff of Seattle for facilitating the gift of the collection to the Survey, and especially to his close friend Jerry Hand who stored the collection for several months after Grekoff's death, and painstakingly boxed it and mailed it to Berkeley. The boxes filled a sizeable portion of the Survey archive room for several months until we managed to get it in enough order to put in cabinets.

To quote a 1995 article by Margaret Langdon and William Jacobsen,

Many believe that Chimariko is very important for the understanding of Hokan; in particular, Oswalt presented results of the method he has been developing for measuring similarities between pairs of Hokan languages. Pairings of Chimariko with Yuman languages and with Proto-Western Pomoan are among the most promising. (Langdon and Jacobsen 1995:129)

They also point out

Howard Berman has organized the Chimariko field notes of Sapir for inclusion in a forthcoming volume of the Collected Works of Edward Sapir; the Harrington material of Chimariko remains to be organized and analyzed.” (ibid., p. 129)

Grekoff's materials go a long way toward alleviating this problem. This valuable collection was in the process of creation for decades, virtually without the awareness of the linguistic community. To understand how this happened, we will give a brief summary of Grekoff's life.

1. A SKETCH OF THE LIFE OF GEORGE GREKOFF. George Grekoff was born in Flushing, New York, on Sept 16, 1923, to Russian immigrant parents. Growing up bilingual, he lived in New York, all way through high school and did part of his undergraduate work at Queens' College. He and brother signed up together for the army at the tail end of World War II, and he was shipped over to Germany during occupation, where he met his wife-to-be, Charlotte. He was called back to the Army Reserves during the Korean War, and was stationed in Fort Bragg, North Carolina, where their daughter Christine was born in 1951. A few weeks after her birth, Grekoff was discharged and the family moved to California, and he eventually came to Berkeley to study linguistics. He worked for Mary Haas on a Thai project, and she subsequently set him to work on the Chimariko language. In 1962, before he finished his dissertation, he was hired at the University of Washington in Seattle to teach linguistics and Russian. But as so many people have found when taking a position before finishing the dissertation, he was unable to complete his degree, and after some years was terminated as a result. After that, he worked for American Can Company as a quality control inspector for close to 10 years, until they closed their Seattle plant in the early '70's. Then Charlotte died of pneumonia, and Grekoff went into a depression for several years. Eventually he found a position using one of the other skills he had developed during his varied life—drafting. He worked for a couple of different engineering firms from then until his retirement. One of his closest friends was Jerry Hand, an engineer at Grekoff's last job.
Hand says that Grekoff was one of the most knowledgeable people he ever knew, on an extremely broad range of topics.

Grekoff died on July 25, 1999, due to complications resulting from chemotherapy treatment for cancer.

Both Christine and Hand report that Grekoff always considered that his true life's work was the study of Chimariko. He never lost sight of that, even though he was removed for most of his life from the academic community. He worked continuously on Chimariko in his spare time. I know of only two published articles, one in Bright's *Studies in Californian Linguistics* (Grekoff 1964) and the other in the 1996 proceedings of the Hokan-Penutian Workshop (Grekoff 1996). He also presented a paper at the 1981 Hokan-Penutian workshop at Sonoma State University, but did not publish in the resultant proceedings. We find in his materials notes on laryngealization and glottalization in Chimariko and other languages (mainly Karok, Kiowa, and Wintu), with preparation for a paper on this subject intended for IJAL—but it too was never published. He seems to have suffered from the disease of perfectionism that has often afflicted scholars. Furthermore, he was a shy person, characterized by his daughter as gentle and self-effacing, who, once he left academia, never put himself forward into the notice of the linguistic community. Yet he steadily worked on with his organization and analysis of the Chimariko language materials until his death last July.

2. A LISTING OF THE GREKOFF COLLECTION. Tess Wood was the Survey research assistant in spring 2000, and devoted her quarter-time job almost entirely to the organization and archiving of Grekoff's materials. The archiving is still in process, but the materials can viewed at this point by interested parties who wish to visit the Survey. The following is Ms. Wood's report on the contents of this impressive collection.

2.1. LEXICON. The collection contains a large amount of lexical material, and planning of a dictionary had been begun by Grekoff. Around 35 boxes of file slips largely contain vocabulary (lexical and grammatical elements) and examples, some indexed by English word, others by Chimariko word or affix, a few others by semantic group or grammatical phenomenon. Since the lists are constructed from various sources (the largest number of references appear to be to Harrington's Chimariko notes and texts) most words are referenced to their source.

In addition, there exists:

(a) a paper file of preliminary lists for the lexicon, organized by parts of speech and alphabetized by Chimariko word:

```
Verb list
Modifiers; conjunctions; and miscellaneous
Manner and degree adverbs
Time adverbs
Qualitative adjectives
Locational and directional terms
Pronouns
Numeratives
Modals
Particles
Imitatives
Nouns
Negative verb forms
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(b) a 51-page 'Reconstituted Glossary of Chimariko'—compiled from non-Harrington sources. This seems to be a partial dictionary (only some sections of the alphabet) and/or to contain forms with underspecified consonants.
Grekoff’s copy of Sapir’s field notes, which is primarily a vocabulary list, and samples from glossaries by other fieldworkers.

In addition, there are notes on orthography, alphabetization, indexing, dictionary conventions such as citation forms, how to structure dictionary entries, even sample copies of pages to show layouts, plus two files of notes for the introduction to the lexicon.

2.2. GRAMMAR. Grekoff’s dissertation was to be a grammar of Chimariko. The collection contains many materials for the grammar, including drafts of three chapters:

1. Phonology
2. Morphophonemics
3. Tactics (levels; forms: word, clitic, construction; predication; stems, themes, affixes; suprasegmentals)

There are six ring-binders full of notes for the grammar, roughly organized by part of speech or by grammatical area. The notes show lots of concern with morphophonemic level analysis, especially affixation, vowel alternations, and verb classes:

- Adjectives vs. statives; locatives and postpositions; participles; nominal predication; ‘verb-0’
- Phonetics (contains another draft of chapter 2)
- Phonetics/phonology (distribution, accent data from LM, componental analysis)
- Morphophonemics
- Attributive; sequences of predicates; agreement; subordination, coordination.
- Clause combining, association, apposition, reiteration, phrases, predications

2.3. PHONETICS, PHONOLOGY, MORPHOPHONEMICS. In addition to some basic descriptions (phoneme charts, etc.), the collection includes the following:

- Comparison of phonetics of different speakers’ speech (based on Harrington’s transcriptions); for example, a comparison of affricates transcribed for different consultants.
- Work on laryngealization and glottalization in Chimariko and other languages (mainly Karok, Kiowa, and Wintu). There is a large quantity of material apparently for a paper on this subject intended for IJAL.
- Some historical work (essentially phoneme distribution based on reconstituted lexical items)
- Notes on stress/accents (including, for example, its relation to syncopation)
- Processes of insertion, deletion, elision: their environments, characterization; morphophonemic analysis

2.4. MORPHOLOGY. The collection contains descriptions and notes on verbal morphology, including tense-mood-aspect, theme affixes, and valence changing morphology, most of which is detailed below in the section on verb classes and valence. Grekoff did a large amount of work on the morphology and morphophonemics of affixal negation, as part of a larger interest in negation (see §2.8).

There are also notes on a number of other topics including: nominalization, causatives and essives, adverbatives, benefactives, appellatives (much of this is on English and includes some analysis and diagrams), possession markers, and various other nominal affixes.

There is some work on reduplication, including examples, meanings of reduplicated forms, and the interaction of reduplication with other processes such as deletion and insertion.

2.5. PRONOMINALS: INDEPENDENT AND BOUND. Grekoff covered this topic in considerable detail. There are descriptive notes on, for example, independent and bound pronouns occurring with different verb forms (transitive, intransitive, imperative, adjectival, compared with the verbal stem); possessives, as well as a few comparative notes on independent pronouns.
There are a number of notes for and on a paper on the ‘etymological’ analysis of Chimariko pronouns (presented at the Workshop on the Hokan and Penutian Languages at Sonoma State in 1981).

In addition there are several folders of notes with a more theoretical treatment of pronouns (in the stratificational framework, complete with charts, tables, and “realization graphs”).

2.6. VERB CLASSES AND VALENCE. A significant quantity of material exists on verb classes, transitivity, valence-changing operations, theme types:

- Notes on transitivity; active, passive and stative verbs; double object constructions; impersonals; causatives; complement types; notes on the inflection of verb classes, the basis for their distinction, and their argument types. Both descriptive notes and analysis in the stratificational framework are included. Some topics are covered more descriptively, in others most of the work is theoretical.
- Themes and instruments. These may occur as verbal prefixes in Chimariko (they seem to function a bit like classifiers). There are a number of files containing examples and notes on this phenomenon, e.g. on specific items which occur as prefixes; their meanings in combination with various verbs; their requirements for particular thematic suffixes to be added to the verb with which they occur.
- Complement types: notes on identifying various thematic roles (locatives, goals, benefactives, etc.), complement types occurring with particular groups of verbs or specific verbs, body-parts as complements.

2.7. OTHER TOPICS IN SYNTAX. A number of syntactic topics are detailed in other sections, such as verb valence, complement types, and passivization. In addition, Grekoff’s more descriptive work on syntax (collected examples and description of the phenomena) includes:

- noun phrase structure and constituency
- predicate nominals
- ‘specification construction’ verbs such as ‘want’, ‘like’, and their complements
- relative clauses
- direct and indirect quotation

The more theoretical syntax notes mainly concentrate on phrase and clause structure, complete with many diagrams (using stratificational linguistics).

2.8. NEGATION: AFFIXAL NEGATION, PRIVATIVES, ANTITHESIS. Negation and antithesis are covered in considerable depth. In particular, Grekoff worked in detail on affixal negation (morphology, morphophonemics and semantics) and on surface-marked privatives in Chimariko (primarily semantics). In addition to folders of data, notes, and analysis, drafts of papers on both of these topics exist (one possibly presented at the Berkeley Hokan-Penutian conference).

Grekoff’s work on negation primarily focuses on Chimariko but (as in other areas) includes some notes and comparisons with other languages:

- copied and annotated material from published sources on a wide range of languages
- original notes by Grekoff, largely on English and Russian (and comparisons of these with Chimariko)

2.9. LEXICAL ITEMS, SEMANTIC FIELDS. A series of folders deals with the use and semantics of particular words or groups of words, mainly verbs. Some words are covered in much more detail or have much better general description and details than others. The list of words includes the following:

- put (verbs of displacement); dressing and covering verbs; carry; action with a cover; action with a long, pointed object; do, become; say/do; name; defecate; be thirsty; be; suck; dig; hurt; choke; hurt, be sick etc.; hide; throw; win; become/make; freeze, frozen; verbs of displacement; “act” verbs and agency; know, remember, forget (examples only); fill/full, long, short/low, far; body-part actions.

Folders on “Lexical studies” contain collections of examples and notes for particular words or topics: the only one of significant size is the collection of notes on dentalia and shells. Collections of examples exist for various words and topics but are very limited in size and detail.

2.10. NOTES ON DISCOURSE, NARRATIVE, MYTH ANALYSIS. The collection contains little material on discourse and narrative. There are just two folders containing a few of Grekoff’s notes and observations on narrative style and on the structure of myths and their narration.

2.11. TEXTS. A number of texts are either xeroxed or re-written from other peoples’ field notes. The collection includes a binder of texts from Harrington, typed/written out by Grekoff, as well as loose folders a few smaller texts (source unknown).

In addition, the collection includes a microfilm copy of Dixon’s texts and a set of xeroxes (made from the microfilm) of Harrington’s Chimariko notes/texts.

2.12. ETHNOGRAPHY. The ethnographic material in the collection is largely in the form of xeroxes of published articles on Chimariko and other California groups.

2.13. OTHER CHIMARIKO RESOURCES. Grekoff also indexed or made notes on various sections of Harrington’s work on Chimariko. A number of copies, either xeroxes or re-typed/written, exist of notes and texts from various people who worked on Chimariko, although mainly Harrington and Dixon. One microfilm of Dixon’s work and three of Harrington’s are included.

There is also one reel of tape containing a Chimariko song performed by Grace McKibben (a Wintu singer) and one audio cassette of Chimariko, made by Alice Schlichter (also of Grace McKibben).

2.14. OTHER LANGUAGES. In addition to work on Chimariko, Grekoff did fieldwork on other Native American languages, and the collection contains the following:

- Skagit (Southern Coast Salish): one box of file slips containing notes on morphology, stems and affixes. Five books of field notes etc. from 1964-67, the first containing lexical material, the others a mix of vocabulary and morphology/syntax (e.g. verbs are tested in different sentence frames with different valence patterns)
- Clayoquot (Central Nootka): three books of field notes from 1966-67 containing vocabulary, and sentence frames. Half of a box of file slips (the other half is Kwakiutl) containing vocabulary and some grammatical notes
- SE Pomo: two boxes of file slips containing details of the phonemic system and vocabulary, one indexed by English word, one by Pomo. One book of notes from 1957 (Sulphur Bank), mainly vocabulary but also some sentence frames
- Kwakiutl: one book of field notes from 1968, containing vocabulary. Half of a box of file slips containing vocabulary and some grammatical notes (the rest of box is Clayoquot)
- Sechelt (Salishan): one box of file slips containing vocabulary and examples of roots, affixes, grammatical categories
- Nootka: a brief diary of a trip to Alberni from 1968, containing some vocabulary and notes about consultants

3. CONCLUSION. The notes on the other languages listed just above are especially interesting because they are Grekoff’s own original field notes. However, this valuable collection of original field notes is physically dwarfed by the massive lexical collection and analytical notes on Chimariko. Grekoff’s contribution to the analysis of Chimariko is extremely important, and if utilized by other scholars, can form the basis both for the long-awaited description of the Chimariko language and for future Hokan comparative studies.
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THE PROCEEDINGS OF THE HOKAN-PENUTIAN WORKSHOPS:
A HISTORY AND INDICES

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The First Conference on Hokan Languages was held in San Diego, April 23-25, 1970. Conceived by Margaret Langdon and Shirley Silver, this conference heralded a thirty-year period in which first Hokan, and then Hokan and Penutian, workshops have been held almost every year. That first conference was put into perspective by Bill Bright, who remarked that “15 years ago, no modern grammar or dictionary was available for any Hokan language.” Over the 15 years Bill Bright referred to and the years since 1970, dozens of Hokan and Penutian languages have been richly documented. Besides dissertations, monographs and articles in major journals, and voluminous fieldnotes in archives, the Proceedings of the Hokan/Penutian workshops and conferences have formed a major publication outlet of papers on those language families. The papers from the first conference were published as *Hokan Studies*, edited by Margaret Langdon and Shirley Silver, and published by Mouton. The rest of the proceedings have been in the form of working papers, most of them edited by James Redden and published at Southern Illinois University at Carbondale, occasionally as the University of Oregon Papers in Linguistics, and eventually as Survey Reports at the University of California at Berkeley. Sometimes the conferences have been held as part of a larger program, such as the LSA Institute at UC Santa Cruz, in 1991; as a result several other language families have been represented in these volumes as well as Hokan and Penutian: Mixtec, Athabaskan, Uto-Aztecan, Algonkian languages and others. Twice, conferences that have included the Hokan-Penutian Workshop have been published elsewhere and are therefore not included in this set of indices.

The primary intellectual lineage of the scholars producing the papers that appear in these volumes belongs to UC Berkeley. Indiana University, whose contributions to Americanist scholarship lay chiefly in languages of the Midwest and Southwest, also produced some Hokanists, as did the Summer Institute of Linguistics. But UC Berkeley’s Mary Haas was the advisor of the majority of Hokan and Penutian scholars who attended the workshops, and several other Hokanists and Penutianists either post-dated Haas as students at Berkeley or were her colleagues there. Haas’s student Margaret Langdon, the founder of the workshops, generated another generation of Yuman specialists at UC San Diego. Langdon’s student Pamela Munro went on to produce several more Yumanists out of UCLA. The Berkeley and Indiana traditions are linked a generation back: Carl Voegelin, the prime mover of Americanist linguistics at Indiana University, and Haas at Berkeley were both students of Edward Sapir.

These working papers are now an invaluable resource for the study of Hokan and Penutian languages, although they are not widely disseminated and are hard to find. This paper presents author and language indices and a simple topic index (based on title) of all the volumes, for the sake of improving access to the linguistic treasures held therein.
LIST OF VOLUMES

   (This conference was held at UCSD, in La Jolla, California, April 23-25, 1970, conceived of by Margaret Langdon and Shirley Silver.)

   (Margaret Langdon hosted this workshop at UCSD in conjunction with the research being done at the Yuman Language Archives which she established with the aid of an NSF grant. From now on for most years until his retirement in 1993, James Redden edited all the proceedings of the workshops and published them through Southern Illinois University at Carbondale.)

   (This workshop was again held at the University of California, San Diego, on June 21-23, 1976. From this year on, it was expected that Hokanists and Yumanists would meet every year, and it was decided at this meeting that the workshops would be held alternately at various institutions.)


5. Proceedings of the 1978 Hokan Languages Workshop, held at the University of California, San Diego, 1978. Edited by James E. Redden. Department of Linguistics, Southern Illinois University at Carbondale: Occasional Papers on Linguistics, Number 5. (Since Yuman languages are generally held to be part of the Hokan stock, the tautologous title “Hokan-Yuman” was dropped from this volume on.)


   (This is the first year that the Hokan and Penutian groups met together.)

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(There had not been a sufficient number of papers submitted after the workshops of 1983 and 1984 to warrant publication. Redden's preface states that the linguists had been meet also with anthropologists, archaeologists and geographers working in Hokan and Penutian regions, and that the other specialists preferred to publish in other places.)

(In 1986, the Hokan-Penutian meeting met as a section of the Haas Festival at Santa Cruz. All but one paper from that year were published as part of the Mary Haas festschrift, ed. by Bill Shipley (1988).


(This volume was dedicated to the memory of Madison S. Beeler and James M. Crawford, both of whom died this year. The workshop was held at the University of Arizona, Tucson, AZ, July 4-5, 1989.)


(This was celebrated as the 20th anniversary of the First Hokan Conference. A record 17 papers were published this year, due in part to field methods classes: Diegueño at UCSD and Kashaya at Berkeley.)

(This time the conference was held in conjunction with the LSA Summer Institute at Santa Cruz, and included besides the Hokan-Penutian workshop, the Friends of Uto-Aztecan, the Society for the Study of the Indigenous Languages of the Americas, and the Athapaskan Linguistics Conferences. The people from all those conferences were invited to publish their papers in the proceedings.)


(In 1994, no Hokan workshop was held; instead, the Workshop on Comparative Penutian was held at the University of Oregon, Eugene, June 27-June 8, 1994. The proceedings of that workshop were published in the International Journal of American Linguistics (DeLancey and Golla, 1997)).


(In this volume appear a set of 7 bibliographies of Hokan languages, including the important bibliography of Yuman languages that Margaret Langdon had been keeping updated for 2 decades. This volume also has a set of pronominal reference and case systems in selected Hokan languages.)


(For three years after this, the Hokan-Penutian Workshops were either very small or cancelled due to insufficient interest, and there were not enough papers to warrant publication.)


(This was advertised as possibly being the last Hokan-Penutian Workshop, since the previous three years had shown a decline in interest. Whether it actually was the last will be seen in future years; certainly at the 2000 workshop, everyone wanted more — at the time, it was suggested that we hold it every two years, in conjunction with the “Breath of Life Workshop” for California Indians. In 2000, the reports of participants in the Breath of Life workshop formed the first session of the Hokan-Penutian workshop, and breathed new energy into the gathering. For the year 2002, the conjunction of the two workshops will occur again; but the Hokan-Penutian Workshop will be incorporated into the broader one-time “50th Anniversary Celebration
of the Survey of California and Other Indian Languages," a conference celebrating all California Indian languages and their relatives and neighbors.)

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