REPORT 10

SURVEY OF CALIFORNIA AND OTHER INDIAN LANGUAGES

THE HOKAN, PENUTIAN & J.P. HARRINGTON CONFERENCES
And
THE MARY R. HAAS MEMORIAL

June 28-29, 1996
University of California at Berkeley

Leanne Hinton, Editor
REPORT 10

SURVEY OF CALIFORNIA AND OTHER INDIAN LANGUAGES

THE HOKAN, PENUTIAN & J.P. HARRINGTON CONFERENCES
And
THE MARY R. HAAS MEMORIAL

June 28-29, 1996
University of California at Berkeley

Leanne Hinton, Editor
This volume is dedicated to the memory of

MARY R. HAAS

Professor emeritus of Linguistics

at the University of California at Berkeley
INTRODUCTION

This volume of the Survey Reports is the Proceedings of the Hokan, Penutian and J.P. Harrington Conferences, held at the University of California at Berkeley on June 28-29, 1996. Part I includes five of the papers that were presented at that conference, and also a paper by George V. Grekoff, who was unable to attend the conference but arranged in advance to submit an article for inclusion in the Proceedings. During the conference, a memorial session was also held for Mary R. Haas, who died a month before the conference. Part II of this volume consists of the presentations that were made about her life and research.

We gratefully acknowledge grants from Joseph Cerny, Vice Chancellor for Research and Dean of the Graduate Division, and William Simmons, Dean of Social Sciences, that helped make this conference possible.

Leanne Hinton
Volume and Series Editor
THE HOKAN, PENUTIAN AND J.P. HARRINGTON CONFERENCES

and the

MARY R. HAAS MEMORIAL SESSION

June 28-29, 1996
University of California at Berkeley, Alumni House

PART I: PAPERS FROM THE HOKAN, PENUTIAN AND J.P. HARRINGTON CONFERENCES

Catherine A. Callaghan - Hulpun: my home town language 1

Anthony Grant - Reviving a Penutian Language: Steps and Strategies in the Revival of Miluk Coos 7

Margaret Langdon - Notes on Highland Oaxaca Chontal internal reconstruction 25

George V. Grekoff - Surface-Marked Privatives in the Evaluative Domain of the Chimariko Lexicon 35

Suzanne M. Wash - Hyphenating Harrington-Style 57

Margaret Langdon - J. P. Harrington and Al Hayes 83

PART II: THE MARY R. HAAS MEMORIAL SESSION

Introduction - Leanne Hinton 95

Wally Chafe - Remarks on Mary Haas 97

Murray Emeneau, UCB - Mary Haas and Berkeley Linguistics 99

Margaret Langdon, UCSD - Mary Haas as a teacher 101

William Shipley - Mary Haas as a Historical Linguist 103

James Matisoff - Remembering Mary Haas's Work on Thai 105

Pam Munro - The Contribution of Mary R. Haas to the Study of Southeastern Languages 115

Karl Teeter - The Importance of Mary R. Haas 119

Mary R. Haas 1910-1996 - Awards and Honors 125

Mary R. Haas: Bibliography 129
PART I

The Hokan, Penutian
and J.P. Harrington Conference
Julpun: My Home Town Language

Catherine A. Callaghan

Ohio State University

My home town is Brentwood, California, on Highway 4 between Stockton and Antioch, a small town of some 14,000 residents, which is not to be confused with Brentwood, Los Angeles, the home of prominent movie stars.

The Julpun Indians inhabited what is currently the Oakley area, just north of Brentwood. We know its approximate location from the 1824 map of Mission lands drawn by Father Narciso Durán (Bennyhoff: 1977:166-67). Most of the Julpun went to Mission San Jose early in the nineteenth century, and none of their descendants were interviewed by ethnographers or linguists at a time when they still spoke their language. Determination of the language or languages spoken in the village must derive from indirect evidence. Important clues come from the place name itself and the pagan names of the Julpun Indians, recorded in the San Jose Mission registers.

Three languages were spoken near the Julpun area; Far Northern Valley Yokuts, East Bay Costanoan (Chocheño) and Bay Miwok. We have modern recordings of Chocheño in the form of J. P. Harrington's field notes, taken early in this century. Until now, our corpus of Bay Miwok has consisted of a short list of words and phrases recorded in 1821 by Fray Felipe Arroyo de la Cuesta (Beeler:1955, 1959). Alphonse Pinart and Alfred Kroeber both did field work on Far Northern Valley Yokuts.

Mins hulpun- means 'neighbor', and -n is a suffix common in Chocheño place names, which apparently became generalized throughout the area. The fact that Eastern Miwok -n is an adverbial suffix indicating time or place doubtless facilitated the process. Consequently, Miba *hulpu-n means 'Neighbor Place'.\(^1\) Support for this etymology comes from the Plains Miwok village name hulpu-mni-, where -mni- is another generalized place name suffix, which I believe to be ultimately of Miwok provenience. If I am correct in my interpretation, Julpun is a Bay Miwok place name, suggesting the village was predominately Miwok, either at the time of contact or at some point in the past.

Bennyhoff (1977) began an analysis of the endings on the Indian names to determine language affiliation, and Milliken (1994, 1995) has greatly expanded this undertaking with the aid of a computer. The difficulty with this approach is that it is often hard to determine where a stem leaves off and a suffix begins. In addition, proper name suffixes were apparently borrowed across language boundaries.
I will tackle the problem from a different angle, and, whenever possible, attempt an analysis of the whole name. I wish to thank Randall Milliken for deciphering the Spanish orthography of the Indian names in the Mission registers and re-alphabetizing them for me, Victor Golla for sharing the Pinart vocabularies with me and allowing me to copy the Golla-Whistler Comparative Yokuts slip files, Marc Okrand for permitting me to copy his Mutsun files, and Bill Shipley for allowing me to copy his Rumsen files. I have also referred to Stanley Newman's Yawelmani Yokuts files. This paper is based on an analysis of Julpun female names only, and I should emphasize the fact that my conclusions are tentative.

In recording the pagan names of neophytes, the Mission fathers were aware they were compiling historical records, and I believe they tried to be as accurate as they could. Fortunately, Spanish is a "phonetic" language, and its vowel system is similar to that of East Bay Costanoan and Far Northern Valley Yokuts. In general, neither vowel nor consonant length was recorded. Bay Miwok and Chocheño had a single series of stops, but Far Northern Valley Yokuts also had an aspirated and a glottalised series, and some dialects had a voiced series as well. "tt" in an Indian name might indicate /tʰ/, /t/, or, in the case of Yokuts, /ɛ/ or /tʰ/. "ll" could represent /1/ or /ly/. "ss" usually represented /ʃ/ or /ʒ/, but the latter two phonemes might also be indicated by "z", "c", or "sì".

Of the 84 Julpun female neophytes at Mission San Jose whose names were recorded, 36 are unidentifiable as to language at the present time (?), 26 are probably Miwok (Mi?), 11 are possibly Miwok (Mi?), 6 are probably Costanoan (Co), 3 are possibly Costanoan (Co?), 1 is probably Yokuts (Y), and 1 is possibly Yokuts (Y?). I should add that in 2 cases, a name might be equally well classified as Miwok or Yokuts, and in 1 case, it could be either Miwok or Costanoan.

I consider the stems more significant than the suffixes, which were sometimes borrowed, and conclude that, although the Julpun village was not uniformly monolingual, speakers were predominately Bay Miwok, as both Bennyhoff and Milliken claimed.

The greatest source of error in my methodology stems from the fact that Yokuts names are more frequently opaque and hence harder to identify than Miwok or Costanoan names. Yokuts data are also relatively inaccessible. Consequently, the presence of identifiable Yokuts names is more significant than their absence. This error can be partially rectified by publishing Yokuts-English vocabularies. These should be alphabetized according to the Roman alphabet, with special symbols following the letter they most closely resemble. Any other order greatly impedes research, no matter how scientific it may seem. The practice of publishing word lists in semantic order should be abandoned, even if that was their order in manuscript form. More lists of Yokuts suffixes would also be helpful.
### Sample Julpun Female Names:

<table>
<thead>
<tr>
<th>Name</th>
<th>Lg.</th>
<th>Evidence</th>
<th>Interpretation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Julpun</td>
<td>Mi</td>
<td>mins hulpu- 'neighbor'</td>
<td>Miba *hulpu-n 'Neighbor Place'</td>
</tr>
<tr>
<td></td>
<td></td>
<td>mie -n 'adverbial case'</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>ceb -n 'nominaliser'</td>
<td></td>
</tr>
<tr>
<td>Cononúte (1 A)Y</td>
<td>Y</td>
<td>Yyaw ko·non- 'to alight'</td>
<td>Yfnv *ko·non-ute- 'Jumper'?</td>
</tr>
<tr>
<td></td>
<td></td>
<td>y -ute-, -ate- 'name suffix'</td>
<td></td>
</tr>
<tr>
<td>Yahuilate</td>
<td>?</td>
<td>Pynim *ja·wil 'grass, brush'</td>
<td>Yfnv *ja·wil-ate- 'Brush Person'</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Mil jáwil 'tender, soft'</td>
<td>Miba *jawi·l-ate- 'Soft Person'</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Mip kecmé-t·e- 'Chewing-On-Bone'</td>
<td></td>
</tr>
<tr>
<td>Huyum (35 A)</td>
<td>Co</td>
<td>ceb huj·u 'first'</td>
<td>Ceb *huj·u-m 'First Person'</td>
</tr>
<tr>
<td></td>
<td></td>
<td>ceb -m 'name suffix'</td>
<td></td>
</tr>
<tr>
<td>Oyúmaye (48 A)</td>
<td>Mi</td>
<td>mins hoj·u-c·u- 'to be first'</td>
<td>Miba *hojju? ma·je? 'First Lady'</td>
</tr>
<tr>
<td></td>
<td></td>
<td>mip maajen 'Queen'</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>mie -? 'nominative case'</td>
<td></td>
</tr>
<tr>
<td>Yactsmáye (2 A)</td>
<td>Mi</td>
<td>mip jaky-m·y 'Southerner'</td>
<td>Miba *jaky-č maaje? 'South Lady'</td>
</tr>
<tr>
<td></td>
<td></td>
<td>mip jakwi-t 'south'</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>mins -c 'directional suffix'</td>
<td></td>
</tr>
<tr>
<td>Talatsmay (26 A)</td>
<td>Mi</td>
<td>mip talaa-t 'north'</td>
<td>Miba *tala-č-maj 'North Person'</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Pynim *maj' 'person'</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>Pynim *th'alxa-th 'tongue'</td>
<td></td>
</tr>
<tr>
<td>Quicámaye (33 A)</td>
<td>Mi</td>
<td>mins kik·y- 'water'</td>
<td>Miba *kik·y? maaje? 'Water Lady'</td>
</tr>
<tr>
<td>Oocme (35 A)</td>
<td>Mi</td>
<td>miss hoowok 'beads'</td>
<td>Miba *howok-me- 'Beaded Person'</td>
</tr>
<tr>
<td></td>
<td></td>
<td>mins -me- 'one who is'</td>
<td></td>
</tr>
<tr>
<td>Sepémaye (40 A)</td>
<td>Mi</td>
<td>mins sype- 'digging stick'</td>
<td>Miba *søpe? maaje? 'Digging Stick Lady'</td>
</tr>
<tr>
<td>Holcayaye (2 A)</td>
<td>Mi</td>
<td>mib hólka 'to break wind'</td>
<td>Miba *holka-?aje- 'Little Fart'</td>
</tr>
<tr>
<td></td>
<td></td>
<td>mil -?aje 'one, person'</td>
<td></td>
</tr>
</tbody>
</table>
Another source of error in identifying the language of a speaker from an etymology of his or her name stems from the custom of naming a child after a grandparent or an esteemed relative, who might be of a different tribe. This was a common practice among the Pomo (Sally McLendon, p.c.), and Kroeber (1906:142) speaks of a similar custom among the Southern Yokuts. The Pomo also had commonly used descriptive nicknames in addition to their real names. The nicknames referred to personal characteristics, some of which were embarrassing. The names I have been able to identify often fall into this category, and would presumably represent the language of their owner, but a short, simple name might be in the language of a relative.

Instances of mismatching can be reduced through genealogies, which Randall Milliken is now compiling, under the assumption that Miwok-speaking parents will have a Miwok-speaking child. This assumption will also shed light on opaque names.

In conclusion, analysis of meaningful Indian names, recorded at the missions, yields additional lexical items, morphological elements, and glimpses of the culture. In the case of the Julpun Indians, these insights suggest that maaje- once designated a female tribal official and became generalized, and that the directions might have had ceremonial significance. Of course, they could equally well have designated the village of origin.

Footnotes

1. I use the star to indicate items not directly attested with their meanings.

2. Additional abbreviations now follow: A 'years' (años), Mins 'Northern Sierra Miwok', Mie 'Eastern Miwok', Miba 'Bay Miwok', Mil 'Lake Miwok', Mip 'Plains Miwok', Miss 'Southern Sierra Miwok', Mib 'Bodega Miwok', Ceb 'East Bay Costanoan', Yyaw 'Yawelmani Yokuts', Yfnv 'Far Northern Valley Yokuts', and PYNim 'Proto Nim Yokuts'.

Bibliography


**Reviving a Penutian Language: Steps and Strategies in the Revival of Miluk Coos**

Anthony P. Grant  
University of St Andrews, UK.  
apg@st-andrews.ac.uk

**ABSTRACT**

This paper discusses some of the linguistic and social issues involved in the ongoing revival of Miluk Coos, an extinct Penutian language of coastal Oregon, by two tribes in the area. It examines recent attempts at making Miluk linguistic material available and accessible to interested tribal members, and outlines the stages necessary for implementation before Miluk can be reintroduced as a spoken language.

1. Miluk Coos in its geographical and historical setting.

Miluk Coos is the more southerly of the two Coosan languages, Oregonian languages which have generally been linked to the Penutian hypothesis. Its immediate northern neighbor was the more widely-spoken Hanis Coos.

Before widespread contact with Euroamericans and population displacement took place in the 1850s speakers of Miluk Coos lived south of the speakers of Hanis Coos, in two areas, which were separate but closely-allied political entities. One community lived in on the South Slough of the Coos River, in Coos County, Oregon, and the other, whose members were referred to in Miluk as gwsiyq, or in English, 'the Lower Coquilles', was settled in a village at the mouth of the Coquille River. Two dialects are thus distinguished: South Slough and Lower Coquille. It is likely that Miluk-speakers had lived further inland along the Coquille River but had been displaced by speakers of insurgent (Athapaskan) Upper Coquille, probably in the 18th and early 19th centuries.

When the Coos population was dispersed in the 1850s after the Rogue River Wars and as a result of land deals arising from the treaties enacted between coastal tribes and the United States Government, Miluk-speakers were forcibly settled, firstly at Fort Umpqua, then at Yachats in the so-called Coast reservation, on the northwestern Oregon coast, in what was traditionally Alsea territory. The period of forced settlement lasted from 1859 to 1875 and the Coos were brutally treated. They suffered years of hardship and hunger, no school was provided for the instruction of the children, medical supervision was inadequate, and by 1875 the numbers of the Coos were reduced to less than half what they had been in 1859. The Coast reservation was dissolved in 1875 and its lands thrown open for settlement by whites. Many Miluks returned to the Coos River and nearby territories which they had left a generation previously, but a number settled at Siletz, together with members of most Southwestern Oregon Athapaskan groups (including the Upper Coquilles) and members of other Oregonian tribes.

Miluk Coos is now extinct, and no words of the language are now being passed down to people of Miluk descent. The last person to know any words of the language, Mrs Daisy Wasson Coddington (b. 1874), who had never learned the language
beyond a few words, died in 1963. The last person able to construct new sentences, Mrs Laura (Lolly) Hotchkiss Metcalf, half-sister of Mrs Coddin, lived from 1862 to 1961, and had last spoken Miluk with others in about 1918, according to the Swadesh-Melton tape. The last person who was capable of giving connected texts in the language was Mrs Annie Miner Peterson (1862-1939), of whom more later.

The process leading to the ultimate extinction of Miluk Coos extended over more than a century. It is likely that even in the nineteenth century many speakers of Miluk Coos were shifting to Upper Coquille in the south or Hanis Coos in the north, while many probably also knew the local lingua franca, Chinook Jargon. Speakers of all these languages subsequently adopted English as their main language, and apparently actively discouraged children from learning their ancestral language, or from listening to tribal languages in conversation.

The usual patterns of exponential reduction in language use accompanying language loss - a decline in the use of the language as the speech community shrinks, a greater reliance on the language of the majority community, and a concomitant lessening of active and passive command of structure and lexicon in the dying language - also took place among the last speakers of the Coosan languages, with the added twist that some (but not all) Miluk-speakers, especially those who had grown up with people from South Slough, also knew Hanis and found more reason to speak that language, since by the 1930s there were a handful of people who could speak Miluk but maybe ten who knew Hanis. Consequently Miluk, Hanis and other languages assumed the role of in-group codes, with ever more restricted uses as the community of people who knew these languages grew smaller and smaller.

Miluk Coos never received any official recognition in southwestern Oregon, and was not used as a spoken or written language by evangelists or missionaries in Coos country or at Siletz (and indeed no Oregonian languages, except Chinook Jargon, Sahaptin and Nez Perce, were reduced to writing for evangelistic purposes last century).


There are five known sources of material on Miluk, all collected in western Oregon.

a) A wordlist of 104 items plus some numerals and paradigmatic forms collected in 1884 at Siletz, Oregon, by James Owen Dorsey, from an old man, Coquille Johnson.

b) Some 220 lexical forms and two short phrases collected in 1903 by Harry Hull St Clair II from George Barney during the course of St Clair’s fieldwork on Hanis with Jim Buchanan and Tommy Miller (also known as Tom Hollis).

c) Extensive textual, lexical and grammatical material collected between 1933 and the late 1930s by Melville Jacobs at Charleston, Oregon, from Annie Miner Peterson. Some acetate recordings of songs and tales were also made. Jacobs' data comprises the vast majority of Miluk material.

d) A few dozen words and placenames collected during fieldwork in southwestern Oregon in 1942 by John Peabody Harrington from Lolly Metcalf and her half-sisters Daisy Wasson Coddin and Nellie Wasson Freeman.

e) A 65-minute tape recording including about 300 items, made in 1953 by Morris Swadesh with the assistance of Robert Melton, comprising forms from Lolly
Metcalf, a few of which were corroborated or commented upon by Daisy Codd. This shows signs of English influence in the pronunciation.

It is rumored that Leo Frachtenberg, who worked extensively on Hanis, Lower Umpqua and Alsea, allegedly collected some materials in Miluk in 1909, including a version of the myth 'The Origin of Death', from Jim Buchanan (c. 1849-1932) - who in any case came from the Hanis village of Wualatch, and was a Hanis-speaker - but these have not been traced.

Dialectal differences between South Slough Miluk and Lower Coquille Miluk, as documented in the materials, are not great. The dialects were evidently distinct but completely mutually intelligible. Only the first body of published material, that collected by Dorsey, comes from a Lower Coquille Miluk; the rest is from speakers of South Slough Miluk. However, Jacobs did collect some 350 or so verbal forms on filecards, parts of paradigms, from Mrs Peterson, who supplied pairs of what she described as Lower Coquille and South Slough equivalents for many of these (Lawrence Morgan, personal communication). Among the Coquilles, Mrs Peterson is regarded as an important historical figure and a revered tribal member (although she died before the Coquille Tribe was constituted, and would thus have been a member of the Coos Bay Tribe of Indians, as the Coos were then constituted), and on the whole little attention has been devoted by the Coquilles to privileging specifically Lower Coquille items of Miluk over their South Slough equivalents.

Dorsey's Miluk material was published (Frachtenberg 1914: 141-149) as was much of Jacobs (Jacobs 1939, 1940), which comprises the vast bulk of published Miluk data; the remainder of Jacobs' material, and the work by St Clair, Harrington and Swadesh, remains unpublished.

3. Post-extinction work on Miluk.

Since at least the late 1980s there has been some interest in reviving Miluk Coos as a language for everyday use, and this interest has grown considerably since around 1993. Interest has been expressed both by the Confederated Tribes of Coos, Lower Umpqua and Siuslaw Indians, and by the Coquille Tribe of Oregon. The first-named tribe, which includes a number of people of South Slough Miluk descent among its members, is interested also in the revival of Hanis Coos, and of Siuslaw-Lower Umpqua, since these were also ancestral languages of the tribe (which is itself avowedly a post-contact amalgamation of several mutually friendly coastal Oregonian tribes who intermarried and shared a common culture, but who used different languages). Many of the office-holders in the Confederated Tribes over the past decade or so, including at least one tribal chief, have had South Slough Miluk ancestry. South Slough Miluks mostly joined the Confederated Tribes, and some people of Lower Coquille ancestry, who did not wish to be counted in with the Coquilles, may have done likewise.

For its part the Coquille Tribe has its origin in a breakaway group of disgruntled ex-Coos tribal members, and comprises people who by descent are of mixed Miluk (of both types) and Upper Coquille ancestry. There was a certain degree of intermarriage between members of the two tribes on the Coquille River early last century, although there was never a 'Coquille tribe' as such. Many of the ancestors of members of the Coquille Tribe formerly resided at Siletz. (Interestingly, many people of Upper Coquille descent, for instance the renowned ethnographic consultant Coquille
Thompson and his descendants, and also Archie Johnson, the last speaker of Upper Coquille, always remained members of the Confederated Tribes of Siletz Indians, and did not seek to become part of the new Coquille Tribe.)

Although most Miluks and the speakers of Hanis Coos did not benefit from the treaties contracted with them by representatives of the United States Government in the early 1850s, members of the 'Nasomah' subtribe of Lower Coquille Miluks were mentioned in the treaties and, after almost a century's struggle, they did receive some compensation for their stolen lands; the modern-day Coquille Tribe comprises the descendants of those people who received compensation on behalf of their Nasomah ancestors. The Coquilles had originally been counted in with the other Coos Indians, in addition to being enrolled members of the Confederated Tribes of Siletz Indians by virtue of their partial Upper Coquille ancestry, which accounted for their residence there last century, but they separated from an earlier organization of the Coos tribes in the early 1960s. Both local tribes are Federally recognized tribes (Confederated Tribes 1984, Coquilles 1989).

Political circumstances reflecting continued animosity between the tribes since the breakaway of the Coquilles (for example the Coquille Tribe's claim to entitlement to all land formerly occupied by Coosan-speakers) have so far dictated that the linguistic work on Miluk be done somewhat independently by the two tribes, with each taking technical advice from a different (non-Indian) linguist. Most research and revival work on Miluk has been done by or for the Coquilles. The Coquilles regard Miluk as a key element of their tribal identity, and as a de jure official language, for example in prayers and myth recitations in Midwinter and other ceremonies, and look forward to the day when it can be used as an everyday language among the Coquilles (Troy Anderson, personal communication). Miluk (and also Hanis and Siuslaw) play much smaller public roles among the Confederated Tribes, who have not declared them as official languages, although they cordially support their use and propagation as parts of their own tribal identity.

In practical terms the Coquille Tribe is somewhat less interested in reviving Upper Coquille, which is less well documented than Miluk (mostly Upper Coquille vocabulary has been recorded), although records of closely-related speech-forms exist, since it is part of the Tututni-Tolowa-Chetco complex of Oregon Athapaskan languages, several members of which have been extensively described. Especially interesting in this regard is Tolowa, the ancestral language of the people round Smith River, California, which has been superbly and copiously documented by their tribal linguist Loren Bommelyn, himself a Tolowa, who has studied linguistics at the University of Oregon, and who has spared no effort to become a fluent speaker of Tolowa.

Other linguists in the area have made studies of the languages of their ancestors. In this regard one notes Patricia Whereat, of South Slough Miluk descent and a former councilwoman for the Confederated Tribes of Coos, Lower Umpqua and Siuslaw Indians, who took a Master's degree in linguistics at the University of Oregon, studying the structure of Hanis Coos, and Troy D. Anderson, a graduate in linguistics and anthropology of Stanford University and a Coquille tribal member, who has worked on Miluk Coos, as part of his academic and practical interest in the languages and ethnology of the groups who came to make up the Coquille Tribe.

Anderson, great-great-grandson of Lolly Metcalf, has been one of the prime movers in the revival of Miluk. In addition to compiling a dictionary, he has put Mrs Peterson's Miluk texts onto disk, using a one-to-one symbol transcription adapted to
the requirements of IBM machines, and has examined unpublished Jacobs Miluk materials at the University of Washington. He has also designed an alphabet for Miluk, reminiscent in its construction of the iconically phonetic principles of Korean Han'gul script, which is intended to highlight the unique visual image of the language. It is based around a computer-generated matrix resembling the organs of speech, within which the places and manners of articulation of the various sounds are depicted by stylized symbols. Furthermore Anderson taught language classes in Miluk (which he oriented toward learning short sentences and traditional texts, rather than to the development of fluency in the language) in the winter of 1994 to a largely White audience, and he has encouraged another Coquille tribal member, Mr Shirod Younker, to study the language at Oregon State University. Anderson was vice-president of CEDCO, the Coquille Economic Development Corporation, founded by his father Bruce A. Anderson, and he sees Miluk language revival as an important element in the revival of the Coquilles' economic and cultural fortunes.


Revival of dead or obsolescent languages as media of everyday communication within a speech community has been conducted in many places, with varying measures of success.

Interest among US and Canadian Indians in reviving their moribund or extinct ancestral languages has been growing over the past few decades, and groups with languages which range in descending order of copiousness of attestation from Huron (among the Hurons of Lorette, Quebec) or Miami-Illinois (among Miamis in northern Indiana and separately in northeastern Oklahoma, and also among Tamaroas in southern Illinois: John K. White, personal communication, December 1996) to the scanty records of Adai (by a group of Adai descendants near Natchitoches, Louisiana: Wallace Chafe, personal communication, July 1995) have been involved in various forms of language revival.

Most of these groups have gone back to the published (and in many cases also unpublished) sources on their languages, in order to have as full a picture as possible of the language. A workshop intended to assist Native Californians to make satisfactory use of the archival materials on their ancestral languages was held in June 1996 at the University of California at Berkeley, under the supervision of Professor Leanne Hinton of the Department of Linguistics, and attracted participants from all over the state.

Revival of a language, with the implicit or explicit goal of language revival being to make the dead language a natural medium of everyday use, comes in several stages, from learning a few greetings and culturally-relevant words to using the language fluently and productively.

In regard to what might be called the support of 'language heritage', the acquisition of a few culturally salient words and phrases as a badge of ethnic identity, the former is easily accomplished and catered for (many commercially available instructional materials on Native American languages consist simply of a cassette and a word- or phrase-list keyed to the cassette). This is also the model in use, for example, in books introducing Hawai’ian children of Filipino descent to their ancestral Ilokano or Tagalog languages: a few hundred words (mostly nouns), a cassette, and lots of attractive pictures in a quarto book. And in many cases this may be all that most of the
people want, since learning a second language which lacks native speakers is bound to be a minority pursuit.

There is material of this sort for a Coosan language. A booklet with some Miluk words and phrases has been produced (Grant 1994 a), while a language-learning tape for Hanis (Grant 1994b), consisting of words, phrases and a pronunciation guide, and with an accompanying booklet, is in circulation. It would not be difficult to produce some sound-recorded teaching material for Miluk.

It is also perfectly reasonable to be more ambitious and to set one's sights on the stars, that is, on the complete revival of a language. Many indigenous groups, and not only those in North America, have been attempting, in differing ways, to revive whole languages as functioning entities, as part of the redefinition of their identity, after the thread of continuity with the last speakers of the language has been broken, and no words of the ancestral language are known any longer to contemporary group members.

Some models of language revival include (among many types) the following techniques (instances are taken from languages of the Pacific Rim):

1) ‘Pidginization’ of a language, firstly teaching the vocabulary (and implicitly the phonology) of this language to tribal members, who then use the words in an English language framework. This is what happened in an experiment with moribund Quileute at LaPush, Washington, in the 1970s (Jay Powell, personal communication; see also Powell 1973). Quileute itself is not yet extinct, but many learners preferred to learn ‘Pidgin Quileute’ because of the structural and phonological complexity of the unpidginized form of the language. In fact, Pidgin Quileute is a mixed language, since its structure is English. It was seen as a halfway house, a point in the linguistic continuum between the tribal members' English and their acquisition of full Quileute. Powell has also mentioned (in a personal communication to the author) that the Shoalwater Bay Tribe of Washington is interested in holding language classes to teach Chinook Jargon, a traditional pidginized form of their ancestral Coastal Chinook (Clatsop and Shoalwater) language.

2) Synthesis of existing or surviving materials on a language, which may often be internally inconsistent but which are then used as a basis for developing a specifically new version of the language for greater use, for which in some cases new words are ‘invented’, apparently from nowhere. This has happened with Esselen in central California (David L. Shaul, personal communication, 1992). In this case the materials on the language were scanty; there was no traditionally-oriented text and little text of any sort, while only a few hundred words of lexicon and a certain amount of structure had been recorded. The continuity between this newer form of esselen and the older language is therefore only partial.

3) Synthesis of materials on differing dialects or emergent languages of one group as a basis for a new language. This underlies the development of the so-called Neo-Tasmanian, which draws material from several of the (quite discrete) communalexts formerly spoken in Tasmania (Tasmanian aboriginal language policy is discussed in Crowley 1990). The revival has taken this form largely because next to no morphological information on the Tasmanian languages is available, but fairly extensive (if poorly-recorded) vocabularies of several dialects have been preserved from the nineteenth century (the last speaker of a Tasmanian language, a woman named Truganini, died in 1876). The revived language consists of lexemes from Tasmanian languages used in an essentially English typological framework, with English phonology. (There is some evidence that a pidginized form of one or another
Tasmanian language was used, possibly in conjunction with pidginized English, as a lingua franca among the remnant Tasmanian community on Flinders Island and at Oyster Bay, Tasmania, in the mid-nineteenth century.

4) Use of existing materials on a language as a basis for revival. This strategy has been followed in the revival of Kurna or Gawurna, the original language of the Adelaide Plains, South Australia (Jane Simpson and Rob Amery, personal communication; see also Amery 1993, 1995). These materials are largely lexical and to a lesser extent grammatical or paradigmatic, and are written in an inadequate orthography devised in the 1840s by two German pastors, Teichelmann and Schurmann. Some Kurna linguistic material was recorded phonetically in the twentieth century, but the less accurate materials are preferred by the language revivalists because they are more copious.

The Kurna case is the one which Miluk most closely resembles, but there are differences; notable in this instance is the fact that the little Kurna textual material there is consists largely of a translation of the Ten Commandments, whereas the Miluk text is extensive, original (rather than being translated material), and what is more, it treats Miluk themes. The people reviving Miluk are ultimately aiming for linguistic authenticity: they are trying to recreate Miluk as it was once spoken. The same is true (as far as the sources will permit them) of people reviving Esselen and, to a lesser extent, Tasmanian. However, as yet there seems to be little new textual material being composed in revived Miluk of the sort which is being produced in Kurna.

Of course, the ideology or strategy for language revival adopted by an indigenous group depends upon a number of factors, although a crucial one is the extent to which a particular language has been documented, and the nature of this documentation - lexical, grammatical, textual, or with all three types well-covered. In order to revive the structure of a language one must know what it was like in the first place. Some tribes have sought to revive a language as it was spoken by their immediate ancestors (who in some cases have left records of portions of their language), while others have gone back to published (or to extensive but unpublished) records in order to reconstruct the language as it was used decades or centuries previously; yet others, including those who are reviving the Coosan languages, have pursued a 'pack-rat' approach, integrating all available materials on the language, in the same way that linguists compile synthetic dictionaries of extinct languages drawing on all available resources.

Tribes in North America have now become adept at searching out even the most recondite wordlists and collections of linguistic materials. A new generation of tribal historians, as well-versed as other archivists and possessed of much more self-motivation, has made it its business to know and to collect all the primary sources on the language and ethnology of a particular group. Some languages, such as Adai or the Texan language Coahuilteco, are too scantily attested ever to be available for use as first languages in their respective communities. (Miluk is in a more fortunate position.)

Rob Amery's discussions of work on Kaurna have dealt with a number of theoretical and practical issues that can be largely replicated for Miluk, if this should be what Miluk descendants want. Amery (1995) discusses the situation of Kaurna and neighbouring languages in terms of language reclamation: people of Kaurna descent are being enabled to have access to the material on their ancestral languages, knowledge of these languages is being made widely available to those descendants who want it, and language kits are being designed for use in local schools.
Language reclamation is a necessary first step to language revival, and it is wonderful to see language reclamation of this sort proceeding apace in North America, even among the smallest and most sidelined tribes.

Language reclamation on some scale has been feasible for Miluk ever since the publication of Jacobs' two text collections, in 1939 and 1940, but these collections are still not widely read or used in the community (though most or all Coquilles at least know about them). There are a number of obstacles which preclude the Miluk originals of the texts being widely enjoyed and appreciated - for instance the orthography in which Jacobs wrote Miluk was an adaptation of systems used by American ethnologists earlier this century, and differs in many ways from either the American Phonemic orthography used in linguistic instruction in the US today or from any form, no matter how regularized, of American English orthography. It is characterized by the use of diacritics and other unfamiliar symbols, such as subscript dots, small capitals, schwas, and barred t's, as a means of representing Miluk phonology. Therefore, it does not correspond readily to the way in which most English-speaking people would like to see un-English sounds being represented. Grant (1994b) offered an interim phonemic orthography for Hanis which relied on spelling conventions already established for other Indian languages using Roman letters, with some largely transparent digraphs for unfamiliar sounds; this has been used occasionally by the Confederated Tribes for writing Hanis and Siuslaw, and this could be adapted for Miluk and taught without much trouble.

In an earlier paper, Amery (1993) discusses a number of neologisms in Kaurna which were coined by the pastors from indigenous lexical materials in order to express Euroamerican concepts which White settlers were then introducing into the Adelaide area. He shows that a second wave of neologisms has been created over the past few years by local Aboriginal people (whose preferred self-designation is Nunga) in the course of their use of Kaurna in songwriting classes, as a language in which to create new songs and stories, thus initiating a new period of Kaurna language planning.

There has been some development of neologisms in the Coquille revival of Miluk (for instance, phrases meaning 'please' and 'thank you' have been coined, in order to accommodate Miluk to more Western expectations of the social dynamics of communication), but the process has not been extensive, and the adoption of new vocabulary items from Hanis, Upper Coquille or Chinook Jargon, to fill lexical gaps in Miluk, has not taken place. If Miluk were to be revived for everyday use, a considerable amount of vocabulary extension and expansion would be required, and terminology needed to express the features of a modern society would have to be coined, since much of the recorded Miluk vocabulary consists of ecological terms and bodypart names, rather than names for acculturational items or abstractions. However, at the moment language revival precedes language planning.

The resuscitation of previously extinct languages has taken place, even when several centuries have elapsed between the death of the language and its revival.

Hebrew, which fell out of use as a spoken language in the early centuries of the first millennium of the Christian era, and which is popularly but erroneously believed to have been revived by Eliezer Ben-Yehuda in the late nineteenth century on the basis of the language of the Old Testament, is the most commonly-cited example of a successfully-revived language. There is a degree of myth in this. Ullendorff (1971) has shown that the grammar and lexicon of a limited corpus such as the Hebrew Bible would not provide sufficient material for the construction of a language suitable for all modern needs. Modern Hebrew, or Ivrit, is of course not the direct descendant of
Biblical Hebrew, since more than a millennium elapsed between the death of Hebrew as a spoken language and the emergence of the modern language. It is probable that if Hebrew had not become extinct as a spoken language, its twentieth-century form would look rather different, and would sound very different, from Ivrit.

Ben-Yehuda’s achievement was to codify the language of the copious Hebrew material that had been produced, and on the basis of this codification, to get people speaking Hebrew to one another as an everyday language for everyday purposes. Hebrew was certainly not an unknown language or a closed book to the Jews. Jews who had received a religious education, especially men, were more or less familiar with Hebrew, which they had learned in schools as the centerpiece of their education. Quite apart from the role of Hebrew as a language of religious discourse - prayers, rituals, religious writings - and as a language for writing about ‘loftier’ subjects, some knowledge of Hebrew had been available as a lingua franca to educated Jews of varying linguistic backgrounds throughout Europe and western Asia). Ben-Yehuda could draw on the resources not only of the Bible, but also of some two thousand years’ worth of writing in Hebrew on a variety of topics - poetry, religious matters, translations of works from other languages - in short, on a huge corpus. From the resources of this corpus he was able to coin neologisms, and to take further steps in language planning for Hebrew.

Another instance that is often quoted as a case of successful language revival is that of Cornish, although in fact, what the several hundred Celtic-language enthusiasts in Cornwall speak is in fact Cornic or Revived Cornish, and more than a century elapsed between the death of the last fluent speaker of Cornish and the revival of a form of the language in the early twentieth century. This language draws mostly upon Middle Cornish (pre-1600) lexicon and structure (although the pack-rat approach has been pursued, and data from other periods of Cornish are drawn upon where necessary), with a standardized orthography, and which has an extensive overlay of Welsh and Breton morphological and lexical items in cases where evidence from Cornish sources was lacking. Grammars, dictionaries (for learners as well as dictionaries intended to codify the language) and teaching materials have been produced for Cornic, though as yet no children are acquiring it as a first language (though this may happen in succeeding generations). This language is certainly Celtic, but it is also artificial. Nobody ever spoke Cornish in the manner in which it has been revived.


Could Miluk be revived? The view from the outset looks promising. By comparison with Esselen, Tasmanian (of any sort) and Kaurna, Miluk Coos is in a favorable position for language revival, even though such a revival is built on a closed corpus of data. Materials are moderately extensive (there are some 182 printed pages of text, some of them bilingual in Hanis - a language whose structure is better understood than that of Miluk - and all have been provided with fairly close section-by-section translations into English by Melville Jacobs, the collector), and the available data include approximately 2000 lexical items (though somewhat fewer morphemes), and a considerable amount of grammatical information. The texts themselves are largely mythic or narrative in form and document the history, beliefs and concerns of the Miluk Coos - in short, they are Coos literature. The recording of the material is
semi-phonemic and is probably almost as close to a phonemic recording as can be obtained without putting the material through a process of overt phonemicization: all the necessary phonetic detail is certainly there. We have sound-recorded evidence on intonation and pronunciation from the tales and songs in Miluk which Jacobs recorded from Mrs Peterson. The texts have been published and are available for study.

Furthermore there is interest among Miluk descendants in reviving the language as a spoken medium. This factor is the most important consideration of any, as far as language revival is concerned, since a language revived in vacuo has no chance of survival. And of course no reasonable person would deny that all descendants of Miluk-speakers are fully entitled to have complete access to the riches of their ancestral language in whatever modes and formats avail them best. Unfortunately the number of Miluk descendants interested in continuing with Miluk language classes held by the Coquille Tribe in the 1994 season was not great, at most a few percent of tribal members.

This attendance pattern reflected a syndrome which has been found in many publicly-held Indian language classes elsewhere in the US, where the most assiduous students tended to include a high proportion of non-Indians, with diehard language enthusiasts, connoisseurs of Indian culture (often people who had many friends in the local Indian community), and, more often than not, New Agers seeking their cosmic roots, comprising the backbone of the class, while many people whose ancestors had spoken the language under study were soon deterred by the means of instruction or by what they viewed as the unnecessary complications introduced into the language as it was being taught. The possession of Indian ancestry is in itself no guarantee that one will find the acquisition of an Indian language as a second language a straightforward or painless process.

Given that the intensive study of, and use of, Miluk is and will remain a minority interest among the Coquilles, to make Miluk an everyday spoken language once more, in the way in which Ivrit has become an everyday language, would be to discharge a Herculean task. Many of the features which rest on the continued and regular contact between learner and native speaker/teacher, and which have made the Master/Apprentice scheme, devised by Leanne Hinton, so successful for students of several languages in Native California, or which have brought success to the Language Nest scheme for the perpetuation of Maori in New Zealand, are simply not available to people who wish to revive dead languages.

Problems in making an extinct language available for learning are potentially legion, even in the case of a sensitively-documented language such as Miluk. They exist at many levels and assume many forms, not all of them having to do with the structure of the language. Many problems relate to the level of knowledge of the revived language by individuals and can be remedied, and some relate to the totality of knowledge of a language - what we know about the particular forms and usages of a language - and will remain forever problems.

Non-linguistic problems include a paucity of information on many social aspects of language, for instance the forms and situational applicability of greetings and leave-takings - among the very first things that people learning a language wish to know. In the case of Miluk, much 'social knowledge' about language, the sort of behavior-based knowledge which would have been second nature to users of the language, and which has been all too infrequently described in detail, has to be gleaned from a study of interaction in the texts themselves.
Other problems of language revival affect the form, rather than the function, of language. In this particular instance, they include information gaps in attested paradigms, and lack of certainty as to the grammatical acceptability of sentences devised to demonstrate grammatical principles (unless they are drawn from the texts). There are no longer any native speakers of Miluk who can pass judgment on the acceptability of a sentence.

Indeed, the amount of knowledge about Miluk which has been internalized by people interested in the language is as yet not sufficient to create even a small body of near-native speakers, which is what is needed if the language is to be revived as anything other than a ritual language, the medium of texts recited by people who do not know exactly what the words they are saying mean.

For many people, the complexity of Miluk would be sufficient deterrent to studying it as a second language (for much the same reason that Pidgin Quileute became popular). Miluk differs from English in just about every conceivable way. Its phonology includes numerous sounds produced at several points and manners of articulation which are not used in English, and it includes a number of word-initial consonant clusters which English does not permit. Its structure is extremely complex for people who know only English. It is highly inflected, with a high morphemic density within a word, with nominal and verbal systems whose underlying structures and characteristics differ greatly from that of English, and it exhibits a number of morphophonemic changes which affect the surface realization of the form of an inflected word. It has relatively free word order and ergative syntax. The vocabularies of English and Miluk are also entirely different.

One of the greatest problems in developing teaching materials in Miluk is the lack of a published reference grammar of the language. In contrast to what is available for Siuslaw-Lower Umpqua and Hanis, no grammar of Miluk has been published. Miluk grammar is documented (there is paradigmatic material, sometimes of somewhat questionable value, from J. O. Dorsey and Melville Jacobs) but much of it is only implicitly documented, and it remains to be extracted from the texts by careful comparison and cross-analysis of forms. The main features (and many minor ones) of Miluk have been recorded in the texts but have not been inventorized, and nobody has compiled long lists of verbal inflections in Miluk, illustrating regular and suppletive—stem verbs, lists of the sort which are available for many other languages. Fortunately Troy Anderson's transcriptions of the Peterson texts onto diskette can be used with concordancing packages, which should alleviate this problem to some extent by allowing the user to search for occurrences of particular morphemes.

More importantly, the authenticity as first-language output of much of the material is potentially questionable. Some 95% of the material reflects the Miluk speech of Mrs Peterson, who was a fluent, but technically not a native, speaker of the language. Her first language was Hanis, which was the language of her mother and stepfather (her biological father was Irish and played no part in her upbringing); she had learned Miluk from her mother's mother during her childhood, and had heard Miluk tales, which she had learned line by line, through repetition and by rote, from a number of Miluk-speakers from South Slough and the Lower Coquille River who were living around Yaquina and Yachats in the north of coastal Oregon (many of these tales were also known to Hanis-speakers). It is quite possible that Mrs Peterson was, at one time, as fluent in Miluk as she was in Hanis (and she certainly was at ease telling narratives and relating myths in Miluk), but when Jacobs met her in 1932 there was nobody else in the vicinity with whom she could converse in Miluk, yet there were still
a few speakers of Hanis in the area, so that her daily languages were Hanis, or English, which she acquired during her twenties.

All recorded sentence and text material was taken from Mrs Peterson. Consequently any grammar of Miluk is a grammar of second-language Miluk as recalled by someone who had had no opportunity to use the language for many years. However, it seems from the few clues available to us that her Miluk was fluent and accurate and did not differ in most essentials from that of L1 Miluk speakers. Sources other than Mrs Peterson's materials serve only to corroborate much lexicon and to supply a few new words and placenames.

Some work on producing secondary materials on Miluk, with the intention of documenting or formulating features of the language as it is attested, has already been carried out. Howard Berman wrote a partial, unpublished and undated (but very useful) grammar of the language, based on his close philological examination of the the Peterson texts and other materials, and on his continued work in the Jacobs Collection, University of Washington (Berman n.d., ms). Anderson (1990) is a dictionary of Miluk, written as a Master of Arts thesis, featuring headwords in English, with Miluk equivalents given with a textual citation. Grant (1994a) is a (hastily-produced) wordlist of Miluk items, including forms from minor sources (i.e. non-Peterson), compiled on behalf of the Confederated Tribes of Coos, Lower Umpqua and Siuslaw Indians.

What is to be known about the structure of Miluk will ultimately rest on the Jacobs texts and, to much lesser extent, on the other pieces of evidence. These need to be worked over, inventorized and analysed as far as they will permit, before teaching materials, aiming at the spontaneous generation by language-learners of novel but (presumably) grammatically-acceptable Miluk sentences, can be devised.

Certain items are going to be needed before Miluk can be taught efficiently in formal instruction as a second language. These include:

a) a two-way dictionary of the language, with extensive explanation and exemplification of Miluk forms for the benefit of speakers of English;

b) a solid descriptive grammar, complete with numerous paradigms illustrating the conjugations of verbs, plentiful examples showing how ergativity works in Miluk texts, and so on;

c) a graded stepwise textbook introducing features of Miluk structure one by one, with plenty of (hopefully authentic) examples;

d) a cassette tape or CD-ROM demonstrating the sounds of the language, with sound material electronically retrieved from the recordings of Mrs Peterson made by Jacobs, and with one-to-one equivalents in a usable and simple phonemic orthography (preferably one which can be easily typed).

But before any of these can be produced, there will need to be people available who know the Miluk materials exhaustively and who are completely familiar with the structures and items of the closed corpus.

6. A potential step toward the revival of Miluk: the example of Latin.

It is perfectly possible to teach a dead language to the point where people are familiar with its lexicon and structure, where they can read it fluently, and where necessary can also write or speak it. This is what has happened with Hebrew, and the same experience has befallen anyone who has studied Latin in school.
Latin and the Romance languages parted company as mutually-intelligible varieties of the same language well over a millennium ago. Nevertheless, Latin has been copiously documented from several eras, and is the medium of a colossal literature. It can be taught as a foreign language: there are textbooks, dictionaries, and reference grammars (not to mention the useful but hardly authentic CD-ROMs and cassette tapes). Many people who learn Latin still use the old grammar and translation method that was traditionally used in grammar schools, whereby they learn the language by reading genuine or artificially-created Latin texts, and constructing pre-existing or new Latin sentences on models already taught to them. The materials in use have been produced by people who have learned the language, which was taught to them through formal instruction by others who had learned the language, in an unbroken cultural tradition stretching over centuries, and who had a knowledge of the language as close to native-speaker competence as it was possible to attain.

Latin has been used for a remarkably wide variety of purposes over the past millennium, and has fallen into disuse more recently than one might think. Prefaces to scholarly editions of classical Greek and Latin texts were regularly written in Latin into the early decades of this century. The Vatican uses Latin as its official language and has a terminology bureau whose job is to coin neologisms in the language for use in public proclamations. Nobody speaks Latin as their first language nowadays, but one recalls that in the sixteenth century Michel de Montaigne was brought up with Latin as his first language, on the orders of his father (with the effect that the household servants had to learn it in order to speak with the child), while in Hungary aristocratic young women were brought up to speak Latin as their first language until the nineteenth century.

All this was possible because scholars had written grammars and dictionaries of Latin, and had made it a language which was codified and which could be taught to people as a lingua franca of the educated and as a vehicle of literature. Quite simply, these people had learned Latin as another language which was capable of application to everyday use in many spheres, a language which complemented the functions which their mother tongues played, and they were able to pass this knowledge on.

Of course, Latin became the language of an elite who could afford to be taught in the days before free universal education. Indeed, the learning of Latin became almost synonymous with formal education (for instance, it was Latin grammar which was taught in 'grammar schools'). Acquisition of Latin was often the gateway to privilege, and scholars from as far afield as Ireland, Finland and Croatia were able to communicate in the language and share an almost pan-European literature to which new creative works were being added into the eighteenth century.

Latin's supranational status was a valuable reason for its perpetuation, and the converse of that of Miluk as a badge of Coos or Coquille identity, but one could certainly learn Miluk for a greater appreciation of Coos literature. However, this will not be possible until people learn Miluk as a language rather than as a string of unintelligible sounds which go to make up texts. Of course, Latin was also used in ritual contexts. Many people who used Latin in these contexts did not understand what they were saying: for centuries Catholics recited the Ordinary of the Mass in what is generally termed Ecclesiastical Latin, more often than not without a clue about the meaning of the words they were saying.

The danger of creating a linguistic elite among the Coquilles, of the sort which once existed among those people in Western Europe conversant with Latin - that is, a clique consisting of those who can read Miluk as a ritualistic or 'high' language (even if
they cannot construct elaborate new utterances) - is real, and at the moment any air of mystery about the ability to read Miluk would be exacerbated by the opacity of the spelling system which Jacobs used for recording and publishing his texts, but the effects are unlikely to be harmful or disruptive, since the number of people interested in learning to read and appreciate the Miluk texts in the original is not large.

On the other hand, any revival of the use of Miluk beyond the repetition of purely formulaic and stereotyped utterances will require a body of people who are conversant with the language, who both know it and can teach it. People who are comfortable with written Miluk would potentially be in a good position to help propagate knowledge of the language to others.

7. The revival of Miluk: a way forward?

Miluk already has a role to play in the life of the Coquille Tribe, and can be said to have been reclaimed to some extent. It has been used in the last few years as a medium for storytelling at the Midwinter Ceremony: a narrator learns and recites stories in Miluk, which are then translated passage by passage into English. Some new Miluk texts have been composed; these are mostly short prayers to be said at ceremonies, and were composed by Troy Anderson at the request of the Coquille Tribe.

At the moment the role of Miluk in Coquille life is at best approximately parallel to that of Manx in ceremonies in the Tynwald Parliament of the Isle of Man, or of Latin in the Oxford and Cambridge Degree Congregations: the language is used, and indeed the use of the language has illocutionary force in itself (laws cannot be promulgated in the Tynwald unless the titles of the bills are read in Manx, while Oxford and Cambridge degrees have to be conferred through the medium of Latin), but its use is confined to fixed formulaic utterances, which can be learned by rote. At such a level, and for other emblematic uses (for instance as the source of names for local institutions or places) Miluk could be used indefinitely, as long as there were people interested in learning to pronounce and recite the relevant texts.

As a side-issue, we may note that the discussion of possible gradual stages of implementing the use of Miluk has been largely from a functional perspective, whereby the range of situations where Miluk is available for use is increased cumulatively. One can also envisage a method of gradual formal implementation of the use of the language. It should be mentioned that the possibility of introducing the Miluk language to the Coquille Tribe step by step in the form of a 'Piggin Miluk' within a framework of English morphosyntax (which would gradually be more and more 'Milukized') has been adumbrated by Troy Anderson in e-mail discussions with myself in 1996; however, so far nothing has happened. The process of gradual acquisition of a language as a symbol of tribal identity is one which merits serious consideration, and for which there are precedents: Amery (1995) points out that this the way in which many words of ancestral languages have been incorporated into songs and stories constructed by Kaurnas and other Nunga people.

However, the successful revival of a language as an everyday medium of communication depends upon a community of people who are capable of creating new utterances in the language, and this community depends upon their being people willing to speak Miluk to one another and to raise children speaking Miluk. This is equally true of those languages which still have fluent native speakers who are of an
age to produce children. If an endangered language is not learnt in the home, no amount of formal instruction or encouragement at school will produce fluent speakers, and that endangered language is doomed. Pouring money into bilingual and bicultural programs, in the hope of reviving an endangered or extinct language which is not being learned by children, is a waste of resources unless there is support for language learning from a non-school environment to which a child feels attracted.

Of the extinct Penutian languages of Oregon, Miluk is theoretically in quite a good position for eventual revival as an everyday language. There is rather more material (and a greater proportion of it in print) for Hanis, while there is less evidence (but most of it available in print) for Siuslaw-Lower Umpqua. Materials for Alsea, Takelma, Kalapuya (especially the Central cluster and the Northern dialect called Tualatin) and Molala are even more plentiful than those for Hanis. However, the Coosan languages and Siu lawan are associated firmly with tribes who are actively interested in reviving these languages, at least for purposes of identification; there is less interest among linguists or others in reviving, say, Molala, because a 'Molala' tribe no longer exists, though there are people of Molala descent (for instance at Grand Ronde) who are aware of their Molala ancestry. But there is nobody around to take up the cudgels and to press for the revival of Molala.

Whether any revival of Miluk as a spoken language will ever take place is, however, doubtful. At the present time the language lacks the essential items of codification - a dictionary and a descriptive grammar - from which a textbook for teaching the structure and lexicon of the language could be devised. There are also very few people, if any, who could read, translate and analyse at sight an untranslated Miluk text, or who could construct grammatically-acceptable Miluk sentences of the complexity of the average sentence in Jacobs' collections of Miluk texts. A structural analysis of Miluk precedes assimilation of the fruits of this analysis, which precedes codification, which precedes second language learning, and that precedes language instruction in Miluk.

To try to bring about the revival of Miluk as a community language at the present stage would be to run before one can walk. And we should also note that recent legal setbacks to CEDCO's plans for expanding the Coquille economic base in the Coos Bay area have made cultural revival a low priority for the Coquille Tribe. The Coquille Tribe faces an uncertain economic future, and only when it is once more on an even keel can we expect much interest to be taken in language revival.

The primary sources for Miluk, plus secondary materials such as Berman's grammatical sketch and Anderson's English-Miluk dictionary (plus other unpublished materials of his, such as his Miluk-English vocabulary list and his machine-readable version of the Miluk texts) constitute the data from which teaching materials can be evolved, once an orthography for the language has been agreed upon. Internalization and analytical assimilation of the primary and secondary materials needs to precede attempts to construct new Miluk utterances for language-teaching purposes.

Further descriptive work on Miluk might also be of use to the non-Miluk linguistic community. Documenting Miluk grammar and constructing a well-planned two-way dictionary and a solid textbook for teaching the language would not only assist people of Miluk descent to learn the language, but would also enable the language to be utilized more readily in comparative Penutian work oriented towards proving the hypothesis that the Penutian languages are genetically related to one another. Unpublished material on several Penutian languages, especially those in Oregon, has not so far been explored as rigorously as it might have been. The result
has been that a number of Penutian languages (for instance Kalapuya and Molala), which are primarily documented in extensive unpublished material, have been severely underused in comparative work.

This benign neglect (which will grow more malign as the original field notebooks of workers such as Frachtenberg and Jacobs yellow and crumble into dust, and the sound recordings become more unplayable with the passing of each decade) compares unfavorably with the treatment which has been accorded extinct Indo-European languages (for instance the Tocharian languages of Turkestan, which have been extinct since the Late Middle Ages and which were only rediscovered this century), by comparatists in Indo-European, for which dictionaries, grammars and text collections have long since been produced. For all we know, Molala (for example) may be the Penutian analogue to Hittite or Vedic Sanskrit, and insights into its structure and lexicon may revolutionize our understanding of the workings of this far-flung family. Miluk, too, has yet to be used to the full in comparative Penutian work, because its structure and lexicon, though documented, have largely - until the work of Berman and especially Anderson - remained a closed book.

The prospects for successful revival of Miluk in some form and at some level are good, given the tribal interest and commitment, the relatively large amount and high quality of material, and the moves towards developing this material. However, progress in reviving Miluk will only come when the language has been documented well enough to permit production of grammatically-oriented teaching materials.

The revival of Miluk on the level of Tynwald Manx or Ecclesiastical Latin has already begun, and it is likely that the revival of Miluk will remain at this ritualistic level unless structured pedagogical materials are produced and implemented in some form of tribally-run educational program. Production of usable and student-friendly teaching materials, and of reference works, in a user-friendly orthography, would enable the more dedicated students of Miluk to progress to a command of the language which would permit them to read the texts provided by Mrs Peterson. We must hope that the next decade or so sees the rise of a generation of people interested enough in the language to devote their attentions to learning it, and to passing it onto others.

ACKNOWLEDGMENTS

* I would like to acknowledge the untiring and most generous assistance of Troy Anderson, Lawrence Morgan and Donald Whereat in my studies of the Coosan languages, Jay Powell and Terry Crowley for observations on similar language restoration and reclamation projects, and also the help of Bruce Rigsby, Rob Amery and Jane Simpson for orienting me towards much of the literature on the revival of Kurna, and of Paul Mahoney in supervising the sound recording of the cassette in Grant (1994b).

REFERENCES

1995. 'It's ours to keep and call our own: reclamation of the Nunga languages in the Adelaide region, South Australia.' International Journal of the Sociology of Language 113: 63-82.


Notes on Highland Chontal Internal Reconstruction

Margaret Langdon, UCSD

The title of this paper is deliberately patterned after Mary Haas's "Notes on Karok Internal Reconstruction" (Haas 1980)—one of her last published papers—and I intend it as one more tribute to her.

While Highland Oaxaca Chontal\(^1\) is not—like Karok—an isolate, it is part of a small family of languages that also have been classified as Hokan, and the facts I wish to address here are not present in its sister language, Lowland Oaxaca Chontal. This paper is one more small step in the direction of a better understanding of earlier stages of lexical structure in the Hokan languages. This is part of a strategy of research that Catherine Callaghan has aptly described as "climbing a low mountain."

The main researchers on Oaxaca Chontal are Paul Turner and Viola Waterhouse. They have both worked on Highland, although Waterhouse is best known for her grammar of Lowland. Without their pioneering work, I could of course never have attempted to say anything about the language.

The phonemic inventory of Highland Chontal is given in (1) and that of Proto-Chontal in (2), which represents a consensus of the reconstructions in Turner (1969) and Waterhouse (1969), though they disagree on some details. The Highland phonemic inventory (1), shows in angled brackets < > the orthographic conventions used in the dictionary by Turner and Turner (1971) for those segments where their orthography differs from the phonemic transcription of Turner (1966). All forms are in the orthography of Turner and Turner (1971), since this is the source of all the Highland data used in this paper.

(1) Highland Chontal Phonemes

\[
\begin{array}{cccccc}
p & t,c<\text{ts}> & \zeta<\text{ch}> & k,<\text{c,qu}/_i,\text{e}> \\
f' & c<\text{ts}>' & \zeta'<\text{ch}>' & k'<<\text{c}',\text{q'u}/_i,\text{e}> & \?<\text{ch}> & \text{f}' \\
f & s,<\text{N}<\text{jn}> & \$<\text{x}> & \text{W}<\text{ju}> & \text{h}<\text{j}> & \text{f} \\
b & d & \text{g} \\
m & n & y & \eta,\text{w} & \text{l} \\
i & e & a & o & u
\end{array}
\]

(2) Proto-Chontal Phonemes

\[
\begin{array}{cccccc}
*\text{p} & *\text{t} & *\text{c} & *\text{k} \\
*\text{f}' & *\text{c}' & *\text{k}' & *? & *\text{l}' \\
*\text{f} & *\text{s},*\text{N} & *\text{W} & *\text{h} & *\text{l} \\
*\text{m} & *\text{n} & *\text{y} & *\text{w} & *\text{l} \\
*\text{b} & *\text{d} & *\text{g} \\
*\text{i} & *\text{e} & *\text{a} & *\text{o} & *\text{u}
\end{array}
\]

A few general structural facts about the language should be noted. Word order is basically VSO, but not rigidly so. Verbs take prefixes for tense/person listed in (3) and (5) below. There is also a set of first person plural object prefixes which, if present, do not allow a subject prefix. Singular person object is

---

1 Highland Chontal is a member of a small family of languages spoken in Oaxaca, Mexico, consisting of three languages, Highland Chontal, Lowland Chontal, and Tequixistlan Chontal (de Angulo and Freeland 1926); not much is known about the latter and it will not be discussed in this paper. Oaxaca Chontal is also called Tequixstlanean.
marked by a suffix, and third person object is zero. Plural objects are all suffixes (object affixes are listed in (7) below). Verb stems are formed from roots by suffixing a variety of derivational and aspectual morphemes. The object suffixes are always word-final. Nouns are typically marked by what are called ‘limiters’, article-like prefixes which occur even if possession prefixes are present; limiters are always word-initial.

The facts I wish to address have to do with the shape of verb roots and stems and that of the tense and person prefixes of Highland. Both Turner and Waterhouse agree that Highland is in many ways more conservative than Lowland, due probably to the greater isolation of the former and the considerable influence of Spanish on the latter. Highland has complex sets of tense/person subject prefixes which are absent from Lowland. Both languages, however, mark the object on the verb.

I could not have attempted to say anything about Highland Chontal were it not for the very detailed dictionary of the language by Turner and Turner (1971) from which I obtained the verb forms to be analysed below. In a useful grammatical sketch appended to the dictionary, the authors also provide a concise sketch of the language including a chart each for the allomorphs of the tense/person prefixes for non-past and past subject. The non-past allomorphs are given in (3).

(3) Non-past Subject Allomorphs

<table>
<thead>
<tr>
<th>Person</th>
<th>Singular</th>
<th>Plural</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>I</td>
<td>ga-</td>
<td>do-,du-</td>
</tr>
<tr>
<td>II</td>
<td>gi-</td>
<td>day-</td>
</tr>
<tr>
<td>III</td>
<td>gu-</td>
<td>do-</td>
</tr>
<tr>
<td>IV</td>
<td>ga-</td>
<td>da-</td>
</tr>
</tbody>
</table>

Turner’s analysis is based on the recognition of four arbitrary verb classes (I, II, III, IV) according to the shape of the prefixes shown on the chart. It should be pointed out that Turner (1966) explains that for clarity of exposition he has opted not to attempt a deeper phonological analysis of the language as a whole, but rather to describe the morphophonology of the language using allomorphs of morphemes rather than attempting a single underlying shape. This has actually served him well as the facts are rather complex. In the Turner and Turner dictionary (1971), each verb is identified at to verb type (transitive, intransitive, bitransitive, semitransitive, and impersonal), as well as by the number of the arbitrary verb class to which it belongs.

In a short note in PELL, Richter (1982) has proposed that the four verb classes defined by their prefixes in (3) above and in (5) below be reduced to one simply by assuming that in some cases the vowel alternations within these sets are due to the fact that some of these vowels belong to the stem rather than to the prefix. This proposal was indeed the inspiration for my interest in this question, so I must credit Richter with the idea, even though my analysis differs somewhat from his. I also wish to point out that while Richter was proposing this as a fairly abstract synchronic analysis in the style then current in generative phonology, I make no such claim. Rather, I am attempting to internally reconstruct an earlier stage of the language. In fact, Waterhouse, with whom I have corresponded about these matters, feels very strongly that speakers’ intuitions support the Turner analysis, which I am quite willing to accept.

The nature of the problem is one which was at one time discussed in some detail in the generative literature in connection with Maori languages (Hale 1973), where a classical analysis of data providing the “simplest” solution to a problem in morphological analysis, i.e. assigning certain consonants in inflected forms to the stem, and deleting them when no suffixes were added was demonstrated to actually not match the synchronic state of the language and did not account for all the facts. It is nevertheless worth hypothesizing that unmotivated synchronic facts like arbitrary verb classes have their origin in an
earlier more coherent system.

Following Richter, I assume that Class I stems are actually consonant initial and that all others are vowel initial. This may appear counter-intuitive when one notes that Class I is the only class where some prefixes at least end in consonants, while all others end in vowels. An interesting parallel can be found in another Hokan language, Seri. Both Seri and Highland Chontal avoid word-initial consonant clusters by either inserting a vowel between the first two consonants or by prefixing a vowel to the cluster which allows the offending consonants to belong to separate syllables.

The chart in (4) below restates the facts in (3) in such a way that no arbitrary classes are needed if the phonological rules presented apply and it is assumed that Turner’s classes II, III, and IV have initial vowels i, u, and a respectively.

(4) Morphemes in Non-past Subject Allomorphs

Singular:
First person:  
- g- → ga-/C-initial stems (I)
- g- elsewhere (i-, u-, and a- stems: II, III, IV)
Second person:  
- do-/du-/C-initial stems (I)
- do- + i → day (i- stems: II)
- do- + u → do (u- stems: III)
- do- + a → da (a- stems: IV)
Third person:  
- di-/C-initial stems (I)
- di- + i → di (i- stems: II)
- di- + u → du (u- stems: III)
- di- + a → de (a- stems: IV)

Plural:
First person:  
- 1- → a1-/C-initial stems (I)
- 1- elsewhere (i-, u-, a- stems: II,III,IV)
Second person:  
- dol-/du1-/C-initial stems (I)
- dol-/du1- elsewhere (i-, u-, a- stems: II,III,IV)
Third person:  same as singular except with alternant di- before C-initial stems (I).

Past subject allomorphs are now presented in (5).

(5) Past Subject Allomorphs

<table>
<thead>
<tr>
<th>Person</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>1</th>
<th>2</th>
<th>3</th>
</tr>
</thead>
<tbody>
<tr>
<td>I</td>
<td>ay-</td>
<td>o-/u-</td>
<td>i-</td>
<td>a1-</td>
<td>o1-</td>
<td>i/-i1-</td>
</tr>
<tr>
<td>II</td>
<td>ni-</td>
<td>mi-</td>
<td>i-</td>
<td>li-</td>
<td>oli-</td>
<td>i-</td>
</tr>
<tr>
<td>III</td>
<td>nu-</td>
<td>mu-</td>
<td>u-</td>
<td>lu-</td>
<td>ulu-</td>
<td>u-</td>
</tr>
<tr>
<td>IV</td>
<td>ne-</td>
<td>me-</td>
<td>e-</td>
<td>la-</td>
<td>ula-</td>
<td>e-</td>
</tr>
</tbody>
</table>

2 Not enough information is available in the literature to account for the o/u alternations in some second person forms.
earlier more coherent system.

Following Richter, I assume that Class I stems are actually consonant initial and that all others are vowel initial. This may appear counter-intuitive when one notes that Class I is the only class where some prefixes at least end in consonants, while all others end in vowels. An interesting parallel can be found in another Hokan language, Seri. Both Seri and Highland Chontal avoid word-initial consonant clusters by either inserting a vowel between the first two consonants or by prefixing a vowel to the cluster which allows the offending consonants to belong to separate syllables.

The chart in (4) below restates the facts in (3) in such a way that no arbitrary classes are needed if the phonological rules presented apply and it is assumed that Turner's classes II, III, and IV have initial vowels i, u, and a respectively.

(4) Morphemes in Non-past Subject Allomorphs

Singular:
First person:  g- → ga-/C-initial stems (I)
g- elsewhere (i-, u-, and a- stems: II, III, IV)
Second person:  do-/du- 2 /C-initial stems (I)
do- + i → day (i- stems: II)
do- + u → do (u- stems: III)
do- + a → da (a- stems: IV)
Third person:  di-/C-initial stems (I)
di- + i → di (i- stems: II)
di- + u → du (u- stems: III)
di- + a → de (a- stems: IV)

Plural:
First person:  l- → al-/C-initial stems (I)
l- elsewhere (i-, u-, a- stems: II, III, IV)
Second person:  dol-/dul- /C-initial stems (I)
dol-/dul- elsewhere (i-, u-, a- stems: II, III, IV)
Third person: same as singular except with alternant dii- before C-initial stems (I).

Past subject allomorphs are now presented in (5).

(5) Past Subject Allomorphs

<table>
<thead>
<tr>
<th>Person</th>
<th>Singular</th>
<th>Plural</th>
</tr>
</thead>
<tbody>
<tr>
<td>I</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>II</td>
<td>ay-</td>
<td>o-/u-</td>
</tr>
<tr>
<td>III</td>
<td>ni-</td>
<td>mi-</td>
</tr>
<tr>
<td>IV</td>
<td>nu-</td>
<td>mu-</td>
</tr>
<tr>
<td></td>
<td>ne-</td>
<td>me-</td>
</tr>
</tbody>
</table>

2 Not enough information is available in the literature to account for the o/u alternations in some second person forms.
Morphemes in past subject allomorphs are presented in (6).

(6) Morphemes in Past Subject Allomorphs

First singular: ay- /_C-initial stems (I)
               ni- /elsewhere (i-,u-,a- stems: II,III,IV)
Second singular: o-/u- /_C-initial stems (I)
                 mi- /elsewhere (i-,u-,a- stems: II,III,IV)

All others, like non-past, but without d-. 3

Comparing items (3) and (5), we note that the non-past and past subject markers share a number of properties, most striking being the shared third person forms. Further decomposition in morphemes can certainly be done, but I am more concerned with the demonstration of my proposal than with the simplest formulation. Nor will I try to explain the o/u alternations since I do not know whether these vowels are in free variation or are conditioned by specific environments.

Looking at the rules involving vowels in (4) and (6), it is clear that vowel clusters are not permitted, and this is a general constraint throughout the language. Typically the first vowel is deleted, but coalescence is also attested and is taken care of by the rules in each case.

Class I, as proposed above, consists of consonant-initial stems, Class II defines i-initial stems, Class III u-initial stems, and Class IV a-initial stems.

The parallels between the analyses of (3) and (5) should be quite obvious. In addition, it is tempting to say that first person allomorphs ay- and ni- and second person allomorphs o-/u- and mi- are phonologically related. Note the morpheme alternants of possessive prefixes on nouns (8), where an -n actually appears on one of the first person allomorphs, and both o and m appear on second person prefixes. Their relationship, however, seems to be much more archaic that the others but is certainly intriguing and adds Chontal to the list of languages which have first persons with n and second persons with m.

The object pronominal affixes are listed in (7).

(7) Object pronominal affixes

<table>
<thead>
<tr>
<th>Person</th>
<th>Singular</th>
<th>Plural</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>a₁,li₁,lu₁,la₁</td>
<td>-onah</td>
</tr>
<tr>
<td></td>
<td>(=1pl subj)</td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>-oh</td>
<td>-olwoh</td>
</tr>
<tr>
<td>3</td>
<td>zero</td>
<td>-olah</td>
</tr>
</tbody>
</table>

Note again that the presence of first singular object prefixes does not allow the simultaneous appearance of subject prefixes. All other object markers, being suffixes, cause no conflict.

Some relevant allomorphs of possessive prefixes on nouns are listed in (8).

(8) Some relevant allomorphs of possessive prefixes on nouns:

ay-, ayn- 'my'               o-, om- 'your sg.'

3 Note, however, that there are no o/u alternations in second person plural forms.
Summarizing so far, there is a good case to be made for four types of verb stems, C-initial, i-initial, u-initial, and a-initial. The question to be raised, though, is whether this proposal is nothing more than a restatement of the arbitrary four verb classes or whether more can be said? While my remarks must be considered preliminary, I will pursue the idea that the four classes are motivated by semantic/grammatical factors.

Highland Chontal has the following verb types: transitives which are inflected for subject and object; bitransitives marked for subject and recipient (no more than two arguments can ever be overtly marked on the verb); semitransitives (of which more below) marked only for third person subject and object; impersonals, not discussed in the description. I don't know in which ways impersonals differ from intransitives but like semitransitives, they can take only third person subject markers.

Turner and Turner (1971:325) define semitransitives as expressing “an action of an unidentified subject, which is not translated into English, directed to an object, which is translated into English as the subject.” They illustrate this with the verb ‘to die’, a semitransitive of Class I and its derived transitive ‘to kill’, a transitive also of Class I. Thus, the word dimahmah, composed of the morpheme sequence di-mahmah (third.person.nonpast-die-future-second.person.object) means ‘you will die’ (literally, according to Turner and Turner ‘it will make you die’). However, this is not a causative, and a better literal translation would be ‘it will die you’, whereas the derived transitive containing the transitiveizer/causativizer suffix -ha is illustrated by the word dimahahma, consisting of the morpheme sequence di-ma-ha-hma (third.person.nonpast-die-transitiveizer/causativizer-future) ‘he will kill him’. One might well ask whether the analysis of ‘you will die’ is correct. Another possibility would be to analyse the prefix di- as a dummy with no semantic content (but homophonous with the third person singular di-), present only to fulfill the constraint on verb forms that well-formed verbs must have an overt prefix. Under this analysis ‘to die’ would be a stative verb whose subject is marked by a patient object form. More on this topic below.

Coming back to the 4 classes, a first hypothesis would be to correlate them with the verb types, but that is most certainly not supported by the facts and of course it would have been obvious to the linguists working on the language if the situation were that simple. In reality, most verb types are found in each class, as will be demonstrated below.

Dictionary entries in Turner and Turner (1971) are in the form of whole words: verbs are given in the third person singular of the non-past form with the aspectual suffix -hma, translated ‘he will Verb’; verbs therefore all start with d- as is evident from (3), and so are conveniently grouped together in the dictionary. In fact, all dictionary entries beginning in di-, du-, de- are verbs. Entries also include glosses in Spanish and English, followed by sample sentences illustrating usage, again with both Spanish and English translation and identified by the verb category (transitive, intransitive, etc.), as well as by the verb class (I, II, III, IV) to which the verb belongs. This is particularly useful since an examination of (3) shows that two classes of verbs have the prefix di- for non-past third person. These devices resolve the possible ambiguities, for which I am enormously grateful.

A first significant observation is that membership in each verb class is very skewed: Class I 321, Class II 32, Class III 97, and Class IV 122. So I propose to examine these in turn with examples given in (9) to (12) below.

Let's start with class II with its small numbers.

(9) Class II: -i stems ‘active’

(32 stems: 21 trans, 6 intrans, 1 bitrans, 4 semitrans)

trans:  dus ‘chew it’
jue ‘smoke it’
lotso ‘talk a lot about’

intrans:  mu ‘descend’
          nu ‘run, hurry’
          yu ‘jump, fly’
mels’aygo ‘remember it’

xtu ‘get angry’

bitrans: xawi ‘tickle’ (why bitrans?)

semitrans: (all exceptions) bar may ‘turn out, result’

fu ‘have dust (e.g. in eyes)’

mejgoo ‘forget’

xtej ‘raise blisters on’

Of the total of 32 verbs, judging from the English translations, 27 are active, including the 6 intransitives. It is not clear why xawi ‘tickle’ is characterized as “bitransitive”; the examples listed seem to give both intransitive and transitive usages. Exceptions are the semitransitives which do not fit the ‘active’ characterization. As much as possible I have given what appears to be the basic stem devoid of prefixes and aspect suffixes. I have also stripped from the stem the characteristic stem vowel to reduce the items to their most unanalyzable forms. So the proposal is that Class II verbs begin in the prefix i- and are overwhelmingly ‘active’. This is an interesting result, and may in fact suggest that Highland Chontal may have had at one time a stative/active distinction. In an unpublished paper, Waterhouse (ms n.d.) has proposed that there are two ways of marking subjects in the language: by prefixes as already shown, and by object markers, agreeing with the analysis I proposed above for the verb ‘to die’.

The four semitransitives listed in (9) ‘turn out, result’, ‘have dust in eyes’, ‘forget’, ‘raise blisters on’ don’t seem to fit the ‘active’ characterization of this class. I have no complete explanation for this, but would like to point out that both Class I and Class II have di- as their third person non-past allomorph as shown in (3) above. Since the semitransitives by definition can occur only with third person subject markers, it is not clear to me how they can unambiguously be assigned to Class II rather than to Class I. This must remain an unanswered question until I can ask Turner or Waterhouse about this. Of course, in a work as complex as a dictionary some typographical errors may have crept in although it is somewhat doubtful that such a systematic class would be marked with the same typographical error. My tentative proposal therefore is that Class II verbs are active, and that the semitransitives assigned to it more likely belong to Class I, consonant-initial stems, where they find a more comfortable home (see (12) for examples of Class I verbs).

(10) Class III: u- stems ‘no control’

(96 stems: 64 trans, 14 intrans, 15 semitrans, 3 impers)

trans: c’wi ‘tip it over’
gwehe ‘stir it up (fire)’
huiil ‘aim it at’
lajme ‘wash it (e.g. dishes)’
ljį ‘earn it’

intrans: mmule ‘boil’

nxii ‘rust’
xamu ‘cloud up’
wajm ‘take root’
nts’idiit ‘belch up acid’

semitrans: fdac ‘make it callused’

mfi’ixa ‘swell up’

mlej ‘ache’
yaf ‘exceed, add to’

impers: mlaax ‘break in half’
duna ‘shine’
naskje ‘sprinkled with dew’

Class III transitives describe deliberate actions by the subject on mostly inanimate physical objects (some exceptions), intransitives describe states over which the subject has no control (also some exceptions), semitransitives have an unspecified agent, which may be an internal agency, acting on a non-volitional patient, and impersonals involve a lack of control. A general characterization of stems in Class III could thus be that there is a focus on non-control on the part of the subjects of intransitives, and on the part of the object in transitives (where the objects are either inanimate objects or animates with no control
over the event). It appears that Class III verbs all contain an argument which is a non-volitional, non-control entity, corresponding to the object of a transitive or semitransitive verb and the subject of an intransitive one. Impersonals in a sense have neither agent nor patient, they just happen. The contrast between Class II and Class III is therefore striking, as they represent two poles in the active/stative, volitional/non-volitional, control/non-control dichotomies.

(11) Class IV: a-stems ‘change of state, cause change of state’

(122 stems: 83 trans, 25 intrans, 11 semitrans, 2 bitrans, 1 impers)

trans: bagu ‘raise a flag’
       dej ‘cut with knife’
       gwa ‘skin it’
       xits’e ‘make him sick’

intrans: bo ‘take a bath’
         ganaf ‘rear, jump up’
         jic ‘hicough’
         xayl ‘shift to one side’

semitrans: abal ‘get warm’
           jac ‘get lost’
           xtaf ‘become stiff’

bitrans: bihi ‘give it to’
        gwihi ‘give it (name) to’

impers: mofgo ‘get cold and dried out (food)’

Class IV contains many verbs describing a change of state of the subject of intransitives, the 11 semitransitives, and the one impersonal, and those causing a change of state of the object for the transitives and the two bitransitives. Change of state seems therefore to be a strong component of the semantics of verbs of Class IV.

(12) Some Class I stems (default class)

(321 stems)

trans: bel ‘lick it’
       day ‘carry it on shoulder’
       nantse ‘steal it’
       xpij ‘measure it’

intrans: box ‘put on coat’
         fuits ‘whistle’
         hua ‘go, walk’
         tsel ‘cough’

semitrans: buł ‘sweat’
           c’uaf ‘be folded double’
           dans ‘be squeezed’

bitrans: doq’ui ‘help’
        guyihi ‘accuse him of it’
        nesh ‘speak it to him’

impers: gush ‘get spoiled’
       gwi ‘rain’

Class I (321 stems) seems to be the default class containing verbs of all types. Evidence that could point to the appropriateness of this analysis but is not available to me at this time might come from language acquisition or possibly from borrowed forms. The large number of verbs in this class weighs heavily in favor of its generality and suggests that many vowel-initial stems have been reanalyzed to fit the more general consonant-initial type. Classes II, III, and IV are probably residues of once totally predictable verb types. Examples of Class I verbs are found in (12) above.

Note that I am not claiming that the class membership of a particular verb stem can be predicted uniquely from the semantics of its vocalic prefix. In my experience, this is not an unusual situation, so for example, in Yuman languages, the presence of an m- prefix is strongly indicative of a stative verb, but does not imply that all stative verbs in fact do have m-.
A Note on Lowland Chontal

I mentioned earlier that the prefixes discussed here have been lost in Lowland. The evidence supporting loss rather than innovation in Highland is the fact that Lowland has developed some phonemes absent from Highland, as pointed out by Waterhouse (1976:327), namely *f*, *x*, *x', *p*, *b*, *h*, corresponding to Highland *t, c, c', t, l, l, and n*. These new phonemes typically occur in the environment of high vowels, but cannot fully be predicted since they also occur word-initially in other environments, specifically in third person forms where the Highland cognate has third person *(d)i*-.. This is part of a process by which Lowland has eliminated prefixes but has retained a trace of one in the palatalization of verb forms. Waterhouse analyzes these forms synchronically as allomorphs of the normal stem. The analysis proposed above for Highland therefore points to a period when both languages shared the third person marker *i*-. Examples are: *H* didehma, *L* vëhuy 'he eats it'; *H* ic'aelba, *L* É'alpa 'he tore it'; *H* ik'onk'ebe, *L* L'oëpa 'he removed it'; *H* inaba *L* màpa 'he hit it'.

Implications for Hokan

The hypothesis that Oaxaca Chontal is Hokan has been supported by lexical parallels as early as Kroeber (1915) and added to by Waterhouse (1976). It is strengthened by the fact that the parallels cannot be attributed to areal causes, since Chontal belongs to the Meso-American linguistic area, is completely surrounded by non-Hokan languages, and has obviously been in its present location long enough to share a number of typological features characteristic of Meso-American languages (as defined by Campbell, Kaufman, and Smith-Stark 1986).

Some observations on the preferred form of stem in Chontal may therefore also be appropriate. Several Hokan language families have been proposed to have fairly short roots consisting of such shapes as CVC (Yuman), CV(C) Pomoan, and internally reconstructed bipartite verb stems in Washo. Chontal, I believe, is another candidate for the postulation of very short verb roots, maybe CV(C). In the data used in this paper, assuming that the initial vowels of verb stems are segmentable and represent meaningful classificatory morphemes, a fairly large number of verbs fit into this category. (Examples in 9, 10, 11, 12). Other lexical stems can easily be shown to contain synchronically segmentable stem-formation suffixes and many of the remaining stems might yield to further analysis suggesting an older layer of composition.

Conclusion

While the proposals made in this paper are tentative at best, I hope to have shown that there is at least reasonably good evidence for assuming that Proto-Chontal had short verb roots which old prefixes *i-, a-, u-* classify into grammatical categories which left traces in the recorded languages. Whether these will link Oaxaca Chontal more firmly to the Hokan stock remains of course to be demonstrated.

References


4 Highland examples are given in the orthography of Turner and Turner (1971) and those for Lowland are as given in Waterhouse (1976:327-328).


------. ms.n.d. Person-marking in Oaxaca Chontal.
SURFACE-MARKED PRIVATIVES IN THE EVALUATIVE DOMAIN
OF THE CHIMARIKO LEXICON

George V. Grekoff
Seattle, Washington

The use of a negative element in the derivational expression of attributive-evaluative oppositions such as English wise: unwise has been noted for a number of European languages. Discussions by Zimmer, and more recently by Horn, under the heading of affixal negation, include summaries of the literature and offer fresh insights into the rather remarkable association of negative form and affirmative sense in the antithetic relation. The same association turns up in some of the indigenous languages of the Americas, notably in Chimariko, in Tunica, and in the languages of the Tupí-Guarani family. In all these languages the negative element, in its surface manifestations, is formally identical with the predicative negator, but (in Chimariko and Tunica at least) it has no tactic function, being etymologically embedded in a lexicalization.¹

Languages that make use of negative elements in the creation of antithetic terms differ in the depth and extent of their utilization of the device. Russian, for example, in many of its lexically expressed primary oppositions, derives an attenuated counterpart for each of the two terms of the opposition, and thereby creates a fully realized attenuated grade of the primary opposition. English does so only sparingly, normally deriving an attenuated partner for just one of the terms, so that no more than a partially realized attenuated grade is created. Chimariko derives only one term, but uses it to express the opposition itself rather than its attenuation, in this respect placing the derivational on a par with lexical expression. Thus the values of the "wisdom" parameter for Russian are, in the primary grade, mudryj: glupiyj 'wise: foolish', and in the attenuated grade, neglupiyj 'fairly wise' (< not-foolish) and nemudryj 'rather foolish' (< not-wise), while English has attenuated unwise (< not-wise) standing alone in a cross-grade opposition to wise, without a corresponding *un-foolish to flesh out its grade. The Chimariko values ikišem: ex ikišem² parallel those of the English cross-grade opposition wise: unwise in form, but in semantic function they are analogous to the values of the English primary grade, wise: foolish.

The sense of deficiency typically associated in the European languages with the derivative term (neglupiyj suggesting a deficit of foolishness, and nemudryj and unwise a deficit of wisdom) is also detectable in the American languages. In the case of Chimariko and Tunica, which are no longer spoken, it is clearly not possible to probe the cognitive/semantic implications of this observation with any degree of confidence. But
the data are suggestive and for Chimariko I make the assumption that the sense of deficiency is integral in the underlying semantics of the derived term, and formalize it by associating it with the cogneme of PRIVATION which is manifested in overt morphological form in other areas of the lexicon. Hence the designation "private" for the derived term and, by extension, for its lexically expressed analogs in pairs not formed by derivation. The term which serves as the base for the derivation is then "plenative"; it refers to cognemic ENDOWMENT. In the evaluative domain, these two cognemes are manifested only as the active pairing of opposed terms in the recorded materials.

<table>
<thead>
<tr>
<th>PLENATIVE</th>
<th>PRIVATIVE</th>
</tr>
</thead>
<tbody>
<tr>
<td>BE-GOODₚₜ</td>
<td>eye²w</td>
</tr>
<tr>
<td>BE-GOODₚ₢</td>
<td>e⁰ₐ</td>
</tr>
<tr>
<td>'BE-LONG</td>
<td>iᵢchu</td>
</tr>
<tr>
<td>'BE-LONG</td>
<td>iᵢchu²</td>
</tr>
<tr>
<td>BE-TALL</td>
<td>iᵢchu⁰</td>
</tr>
<tr>
<td>BE-WEALTHY</td>
<td>ahata³</td>
</tr>
<tr>
<td>BE-GENEROUS</td>
<td>ewa⁰</td>
</tr>
<tr>
<td>BE-CLEVER</td>
<td>ikišem</td>
</tr>
<tr>
<td>BE-APT</td>
<td>ošem</td>
</tr>
<tr>
<td>BE-HUNT-SKILLED</td>
<td>akho⁰</td>
</tr>
<tr>
<td>BE-COOKED</td>
<td>ipima</td>
</tr>
<tr>
<td>BE-SAVORY</td>
<td>ik'uy</td>
</tr>
<tr>
<td>BE-SATIATED</td>
<td>imiki</td>
</tr>
<tr>
<td>BE-LACTESENT</td>
<td>ac'am</td>
</tr>
<tr>
<td>BE-HARDWORKING</td>
<td>učhe</td>
</tr>
</tbody>
</table>

**Table 1**

Surface-marked, etymologically transparent privative themes and their antithetic partners.
"Surface marking" is a descriptive label and has no structural significance. It refers to the synchronic transparency of the lexicalized derivative term with its attendant potential for back formation and nonce creations. In Chimariko, the derivative term is necessarily inflected pronominally in its surface forms, and it is the inflectional pattern that constitutes the surface marking of the term. This circumstance is registered in the morphophonemic representation of the theme, and the theme, though not a surface form itself, may therefore also be said to be surface marked.

Tables 1 and 2 list the evaluative antithetic pairs which have turned up in the Chimariko materials with surface marking on the privative term. In Table 1 the privative is quite obviously derived from its partner in the opposition. In Table 2, however, the partners are unrelated in form, and two of the privatives shown there are in fact isolated terms, with no identifiable lexical partner. In both tables the privative themes are lexicalized forms which function tactically as minimal units. A token English "be-phrase" is used to represent

<table>
<thead>
<tr>
<th>PLENATIVE</th>
<th>PRIVATIVE</th>
</tr>
</thead>
<tbody>
<tr>
<td>BE-GOOD</td>
<td>isik</td>
</tr>
<tr>
<td>BE-BIG</td>
<td>tew</td>
</tr>
<tr>
<td>BE-BIG</td>
<td>ichin</td>
</tr>
<tr>
<td>BE-BIG</td>
<td>eχani</td>
</tr>
<tr>
<td>BE-FIERCE</td>
<td>away</td>
</tr>
<tr>
<td>BE-FLEET</td>
<td>lu?re</td>
</tr>
<tr>
<td>BE-ABUNDANT</td>
<td>?amepa</td>
</tr>
</tbody>
</table>

| *** | ***|
| *** | ***|

**TABLE 2**

Surface-marked, etymologically opaque privative themes, paired with antithetic partners, or unpaired.
the lexeme. The token is not intended as a gloss, but it does suggest something of the sense underlying the lexeme. The morphemic representation of the lexeme is written morphophonemic-
cally. A lexeme which is a portmanteau representation of a core sememe modified by one or more categorial sememes has its token marked with subscript tags to reflect those categories which are critical for its definition. The categories reflected in the tables are: p = plural thematic reference; 3 = third person reference exclusively; h = human referent; i = inanimate referent. Superscript numerals preceding the lexemic token serve to distinguish those homophonous tokens for which categorial distinctions are unknown.

All the forms listed in the tables are verbal themes, and all of them, with the possible exception of u̲e̲h̲e̲,⁸ are stative. Not listed are related nominal themes which are lexicalized from the third person surface form of the verbal themes. These (etymologically) derived nominals retain the antithetic character of the original verb themes: hisik 'a good one'; xulik 'a bad one' reflects the opposition isik 'be good': axulik 'be bad' of Table 2. Morphologically the verbal themes fall into two distinct inflectional classes, the prefixed class and the suffixed class, reflecting the positioning of pronominal elements in relation to the theme. Vowel-initial themes and those beginning in pseudovocalic e are inflectable by prefixed pronominal element:⁹

\[
eye^w \text{ '}(\text{sev. pers.) to be good', } \check{h}^w \text{ -eye}^w \text{ 'we be good'}
\]
\[
\check{a}^w \text{ -eye}^w \text{ '}(\text{sev. pers.) to be bad, } \check{h}^w \text{-} \check{a}^w \text{ -eye}^w \text{ 'we be bad'}
\]

Consonant-initial themes are inflectable by pronominal elements suffixed to the thematic core:¹¹

\[
\check{k}^e\text{-ok 'to be sick', } \check{k}^e\text{-} \check{zi}\text{-ok 'I be sick'}
\]
\[
\check{t}^e\text{w 'to be big', } \check{t}^e\text{w-} \check{cu} \text{ 'I be big'}
\]
\[
\check{l}u\text{?re 'to be swift', } \check{l}u\text{?re-} \check{ti} \text{ 'I be swift'}
\]

Partners in an antithetic opposition tend to be of the same broad morphotactic class. Thus both themes in each pair in Table 1 are of the prefixed class. But the themes in Table 2 show some heterogeneity: three pairs have divergent class affiliations, their plenative themes being of the suffixed class and their privative themes of the prefixed class. The partners in these pairs are nevertheless well matched in their categorial implicans and largely so in their semantic ranges: in BE-BIG : BE-LITTLE and in BE-FLEET : BE-SLOWFOOTED both terms are categorically unrestricted, and in BE-ABUNDANT : BE-SCARCE both are limited to third person inanimate and have a distributive reference. None are likely to be perfectly matched, however. The degree to which one term implies the
other probably varies from pair to pair, and some terms are known to participate in more than one opposition. But the tables do not reflect these possibilities. They focus on surface-marked oppositions only and only on the most central semantic parameter if more than one is attested. An instance of multiple contrariety would be the opposition of BE-SAVORY ik'uy to BE-UNSAVORY ax'ik'uy (Table 1) and to BE-BITTER yekhay (not listed).

Because the surface-marked privative themes derive from negated themes, a brief overview of negative formulation will be given at this point, before we discuss the privatives in greater detail.

Negation is expressed in either of two ways in Chimariko: inflectionally by affix within the predicate word, or lexically by postposition. What differentiates the two formulations semantically is, for the moment, unclear, but it may well have to do with focus. The morphemes and morpheme combinations used in negation are listed in Table 3. Prefixed-class themes with vocalic, but not pseudovocalic, initial take the prefixed negative form or forms, usually as a discontinuous combination of elements. Prefixed-class themes with initial pseudovocalic a, and all the suffixed-class (consonant-initial) themes, take the suffixed negative, usually as a combination of contiguous elements. All thematic types can be negated by postposition instead of by affix.

<table>
<thead>
<tr>
<th>MORPHEMES</th>
<th>THEME TYPE</th>
</tr>
</thead>
<tbody>
<tr>
<td>PREFIXED</td>
<td>ax&quot;(..na)</td>
</tr>
<tr>
<td></td>
<td>ek'(..na)</td>
</tr>
<tr>
<td>SUFFIXED</td>
<td>k'ü(na)</td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td>POSTPOSED</td>
<td>k'u(na)</td>
</tr>
</tbody>
</table>

**TABLE 3**

Principal negative morphemes and morpheme combinations.

The most common form of the prefixed negative is the discontinuous complex ax"(..na), which frames the themes. For most
verbs, active as well as stative, the caudal element na is necessarily present (I infer this from the materials; the status of na was not explicitly investigated by any of the field workers), and constitutes the closing element of the negated theme: (active) ama 'eat', əx"-ama-na 'not eat'; (stative) axawin 'be old', əx"-axawin-na 'not be old'. With a few themes, na is occasionally omitted, and with any theme in the passive voice, na is generally omitted. The significance of such omission is not clear; the underlying semantics are probably too subtle to have been registered in the materials. It is also not clear whether the occasional omission of na is an option available to all themes or only to a particular set of themes. In the asseverative mode, though, na is obligatorily excluded; its position in the surface form of the predicate is occupied by the clause-final negative asseverative postfix ətk'i '(not) at all, (not) ever'. The coda-free shape əx" appears as a substructural element in the privatives of Table 1, and somewhat less transparently in those of Table 2.

The superscript " is a morphophonemic operator which induces labioretrusion in a following front vowel: ə, ə are realized as P/ü, q respectively when preceded by ə. The pseudovowel ə is an operator which induces translatory vocalic replication in an intricate choreography that we need not go into here, but that results, generally, in vocalic harmony: əx"-əx"-ixu-na, P/əxuquna 'I not be fat'.

A second discontinuous complex, ek'(..na), appears as an optional variant on many prefixed-class themes which have ū as their initial vowel: um '(several go)', ek'-um-na ~ əx"-um-na '(several) not go'. The significance of this variation is unknown; its highly restricted occurrence suggests a vestigial phenomenon. The shape ek' is discernible in the privative theme BE-LAZY ek'uče in Table 1.

The suffixed negative k'ū(na) is also a complex form, containing the caudal na found in the prefixes. The breve in the first syllable indicates that the vowel is syncopated when an immediately preceding syllable ends in a realizable vowel. Thus in the speech of Sally Noble, as recorded by J.P. Harrington, k'ūna is realized as P/k'una after consonants and P/?na after realizable vowels. In the speech of Polly Dyer, as recorded by R.B. Dixon, the postvocalic form is apparently P/kna (based on an evaluation of his transcriptions of the form). The discrepancy in postvocalic realization indicates that there were two different ways of reducing the articulatory complexity of a glottalized consonant which has come to stand in syllable final, a position which does not tolerate complex articulations. As with the prefixed negative, the final element na may be omitted on occasion, is normally omitted in the passive, and is totally excluded in an asseverative context.

The suffixed negative is positioned immediately after the pronominal element in the case of simple themes such as luʔ 'to drink': luʔ-µi-k'ū-na, P/luʔʔi?na 'he not drink', and phala?
'be strong': phalaʔ-mkha-k'ū-na, P/phalaʔkhak'na 'ye be strong'. But with a discontinuous thematic complex such as po..mū 'to sleep' the negative is positioned after the caudal element of the complex: po-mū-k'ū-na, P/pohmuʔna 'he not sleep'.

To confuse matters a bit, s-initial prefixed-class privatives require the suffixed-class formulation of the negative: axusamu-k'ū-na 'not be blind' (Table 2), ax'ʔ-e-k'ū-na 'not be bad' (Table 1). The suffixed negative also appears to be an option (rarely invoked) available to other prefixed-class themes. I have only one unequivocal example: ipimta-k'ū-na 'not to play' in the vetative predicate P/nipimtaʔna mar'ʔi 'don't you play!' (H/SN-634T). (There may be other, unrecoverable instances in the ambiguous circumstances which I describe under postpositive negation further below.) The significance of a suffixed formulation with a nonprivate prefixed-class theme is not clear; perhaps it adds some degree of emphasis. In its obligatory use with surface-marked privatives the constraint is defined morphotactically, keyed to this thematic subset and incidentally contributing to the definition of the subset.

A striking feature of affixal negation is its suppression of overt third person reference in the lexeme-to-morpheme realization. This does not happen with lexical (postpositive) negation. The suppression is obligatory for prefixed-class themes and optional for suffixed-class themes. Those themes which exhibit third person suppression in the context of affixal negation do show an overt third person element in their affirmative state unless the suppression is inherent in the theme. For example, the prefixed-class stative theme ixu 'to be fat' takes the third person prefix h in the affirmative, but lacks it in the negative:

h-ixu P/hixu 'he be fat'
ax'ʔ-ixu-na P/txuxuna 'he not be fat'

Compare this third person negative form with that of the second person, where suppression does not occur:

m-ax'ʔ-ixu-na P/muxuxuna 'you not be fat'

Similarly, but not obligatorily, the suffixed-class theme k'e 'to die' can suppress the third person suffix mü in the negative:

k'ë-mü P/k'ëh 'he die'
k'ë-k'ū-na P/k'ëʔna 'he not die'

Exercise of the option not to suppress is not easily illustrated for this theme. Although it does appear with an overt
third person in the negative (as P/k'ehk'una), the status of the negative element in the surface form — is it suffix or postposition? — is ambiguous. The theme po..mû 'to sleep', however, clearly differentiates the two formulations, and shows that overt representation of third person need not be suppressed by the negative suffix:

\[
\begin{align*}
\text{po-ů-ů-ů} & \quad \text{P/pohmu} \quad '\text{he sleep}' \\
\text{po-ů-ů-ů-k'ů-na} & \quad \text{P/pohmu?na} \quad '\text{he not sleep} (\text{suffix})' \\
\text{po-ů-ů-ů k'ů-na} & \quad \text{P/pohmu k'una} \quad '\text{he not sleep} (\text{postp.})'
\end{align*}
\]

Morphologically, the negated theme with covert third person reference is a portmanteau representation of a core lexeme (such as BE-FAT) and the appropriate third person lexeme (3. for statives). There are some themes, however, for which overt third person reference is inherently suppressed in any context, affirmative or negative. These include the surface-marked privatives of Tables 1 and 2, and certain suffixed-class themes, for example lu're 'to be swift', četk'a '(several) to perish', s'amepa '(harvestable foods) to be plentiful'. From the synchronic standpoint, the motivation underlying third person suppression seems not to be semantic, nor does it seem to stem from the tactics at any level. Whatever its history, the phenomenon now seems to arise entirely within the lexeme-to-morpheme portion of the realizational system.

Lexical negation is expressed by the postposition k'û(na), which is morphemically complex and has the same form as the suffix except that the vowel of its core morpheme is nonsyncopating. The final element na, here as in the affixes, is excluded in the context of asseverative negation. The postposition follows the predicate word or phrase immediately, but it is probably not phonologically bound to the word that precedes it. It is possible that the spoken language maintained a prosodic distinction between suffix and postposition, which would have been critical in circumstances of segmental ambiguity, and that this distinction was not perceived by any of the field workers. In any case segmental ambiguity exists. It arises when the predicate ends in a consonant: syncope is not induced in the suffix k'û-na postconsonantly, so the segmental realizations of suffix and postposition are identical in that context. Thus, as matters stand at present (in the analysis), either formulation could be invoked in the interpretation of constructions such as P/shaxewink'una tinta 'I am not old' (axawin 'to be old')(D/F-5.44) and P/k'eccokk'unat 'I am not sick' (k'e.ok 'to be sick')(H/SN-654T).

Now I turn to a few remarks on the synchronic structure of the privatives in Tables 1 and 2, and on the etymological derivation that underlies this structure.

In their pronominal inflection, the surface-marked privatives are exactly like negated themes except for the absence of
the caudal negative element na. Table 4 provides an illustration. It shows the pronominal inflection of a typical nonprivative prefixed-class stative theme awi 'be afraid' and its negative form eḫ'-awi-na 'not be afraid', and the pronominal inflection of the surface-marked privative themes eẖaye 'be little' (from Table 2) and eḫ'iẖu? 'be short (in stature)' (from Table 1). For brevity, the paradigms display only the

**NEGLIGENCE**

<table>
<thead>
<tr>
<th>AFFIRMATIVE</th>
<th>NEGATIVE</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>THEME:</strong></td>
<td>eḫ'-awi-na 'not be afraid'</td>
</tr>
<tr>
<td>awi 'be afraid'</td>
<td></td>
</tr>
<tr>
<td>1 P/ḥawāni</td>
<td>P/ḥawaiwāni</td>
</tr>
<tr>
<td>2 mawi</td>
<td>maẖawīnā</td>
</tr>
<tr>
<td>3 hawī</td>
<td>ṭawīnā</td>
</tr>
</tbody>
</table>

**ANTITHESIS**

<table>
<thead>
<tr>
<th>PLENATIVE</th>
<th>PRIVATIVE</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>THEME:</strong></td>
<td>eẖaye 'be little'</td>
</tr>
<tr>
<td>---</td>
<td></td>
</tr>
<tr>
<td>1</td>
<td></td>
</tr>
<tr>
<td></td>
<td>P/ḥaẖaye</td>
</tr>
<tr>
<td></td>
<td>maẖaye</td>
</tr>
<tr>
<td></td>
<td>ḥaye</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>THEME:</th>
<th>iẖu? 'be tall'</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 P/ḥuẖu?</td>
<td>P/ḥuẖuẖu?</td>
</tr>
<tr>
<td>2 miẖu?</td>
<td>mūẖuẖu?</td>
</tr>
<tr>
<td>3 hiẖu?</td>
<td>ṭuẖu?</td>
</tr>
</tbody>
</table>

**TABLE 4**

Boxed paradigms counterpose the negative inflection of a typical nonprivative stative theme and the (affirmative) inflections of two surface-marked privative stative themes.
anumerative (nonplural) inflection. Third person marking is suppressed in both negative and privative; the vocalic replica induced by the pseudovocalic operator $e$ remains unrealized when it comes to stand in word initial in the absence of a personal element.

Scanning down the boxed paradigms, the surface similarities of the negative and privative inflections are readily apparent: a harmonic vocalism in first and second person; a consonant $x$ enveloped by this vocalism; and the lack of an overt third person element. The underlying structural difference, as indicated in the thematic shapes, is that the negated theme is a tactic combination of morphemes, while the privatives are monomorphemic.

The similarity of the two inflections is hardly accidental. We can reconstruct an originally complex form for the privative theme $ax$aye and the other privatives of Table 2. A labioretreactive component must have been present in the etymological form, because the first true vowel in all these themes is either a rounded back vowel $o$, $u$ or the neutral vowel $a$; front vowels are conspicuously absent. This points to the etymological identity of the initial segment with that of the Table 1 themes and with the core element of the negative prefix. We can assume that the residual $aye$ represents the etymological thematic core. But we cannot fully reconstruct the core of a theme which has $o$ or $u$ as its first vowel, because modern $o$ could reflect either $*e$ or $*o$ in the context of labioretaction, and modern $u$ could reflect either $*i$ or $*u$. For example, $exulik$ 'be bad' could derive from $*ex*-ilik$ or from $*ex*-ulik$.

Synchronously $ax$aye is not analyzable, and $ex*ičhu$ is only substructurally analyzable. In substructural analysis, the segments make use of existing morpheme shapes as empty morphs, i.e. without tapping into the semological connection or the morphological valence of any of the shapes. A form such as $ex*ičhu$ utilizes the shapes of the morphemes $ex$ and $ičhu$ without their connections. It constitutes a hypermorpheme, motivated semologically in its own right and recognized by the morphotactics as a minimal tactic unit. Its composition is defined entirely in the realizational system.

We may now attempt to interpret historically the synchronic facts of surface-marked antithesis, with the help of some speculative reasoning. Tables 1 and 2 represent two successive stages late in the life history of the surface-marked privative form. In both stages the privative sense is a given, one that has been introduced into each of the evaluative parameters represented in the tables at some earlier stage, in response to cognitive or semological pressures and developments. Table 1 shows the stage in which the privative sense has appropriated for its morphemic expression the form (but not the sense) of the negated evaluative theme. This process has not disabled the negatability of the (now plenative) original theme, as the discussion under table 5 will demonstrate, but negation is now sparingly used with such themes — a contrary opposition seems
to be preferred in circumstances in which either contradiction or contrareity would adequately render the desired sense. The ambivalence in such a situation emerges clearly when an English prompt is responded to in both ways: Dixon's prompt 'I'm not fat' yielded the unmarked privative theme očunč'al 'be thin, wasted' from one speaker (D/SN-2.21) and the negated theme ax'-ixu-na 'not be fat' from another (D/PD-1.57).

The forms of Table 2 have passed through and beyond the hypermorphemic stage of Table 1 and in the process have lost all substructural identification with the morphs of the originally negated themes. The process may have been as follows. In response to ongoing cognitive or semological pressures, the original lexeme functioning as the plenative term in a Table 2 pair was replaced by an approximate synonym with a morphemic representation unrelated to that of the privative term, or had dropped out of use without replacement. Its disappearance led to a fading of the hypermorphemic configuration of the privative in the realizational system and to its transformation into an unanalyzable, simple morpheme. Nevertheless a subliminal association of the surface manifestations of the theme-initial sequence ax- with those of the productive negative may still be accessible to individual speakers, though I have no evidence of such awareness, for example in the form of back formations based on Table 2 privative themes.

There will be a wrinkle or two in my grammatical and lexical description of Chimariko which will impact my presentation of privative forms. For lexicographic purposes, I have found it useful to resolve any theme-internal morphophonemics that may come into play in the citation form of structurally or substructurally composite themes. This treatment implies something like a performance model of description rather than an analytical model. Consequently the otherwise transparent privatives of Table 1 will, in their morphophonemically resolved citation form, approach the opacity of the privatives of Table 2. To maintain the distinction between the two types in the dictionary entry, a citation form with internally resolved morphophonemics will be followed by the morphophonemic form proper to the analytical model. A typical dictionary entry for a privative theme from each table would therefore be: 33

\[\text{axye} \text{w} (\text{ax-} \text{eye} \text{w}) \text{ s.imm. (IBx) pers. pl. to be bad. Opp. eye} \text{w.}\]

\[\text{axaye} \text{ s.imm. (IBx) to be small, little; not fully grown, hence occasionally transl. as 'young'. This is the categorically unrestricted privative term of the semantic cluster based on the parameter of bigness. See also axovydiye, axocë. Opp. te} \text{w}.\]
Getting back to the paradigms of Table 4: privative themes in general are capable of being negated, though for the surface-marked set in particular, negation is sparingly attested in the materials, and only for a few themes in that set. As mentioned earlier, when negation by affix does occur, it is the suffix that is used, in spite of the prefixed-class affiliation of these themes: (Table 1) ek'úche 'be lazy, shiftless, reluctant' is negated as ek'úche-k'ú-na 'not be lazy' in the text occurrence P/k'úche'nat 'she's not lazy' (H/SN-277T); (Table 2) exusanmu 'be blind' is negated suffixally as exusanmu-k'ú-na 'not be blind' (and also postpositively as exusanmu k'ú-na) in the course of a paradigmatic elicitation (D/SN-2.2). An attempted probe of the possibility of prefixal negation was rejected for the privative themes axome'2 'be tame, gentle' (H/SN-154F verso) and ex'ac'am 'be dry, nonlactescent' (H/SN-226R).

The plenative term of an antithetic pair is also capable of being negated, though again, for plenatives opposing a surface-marked privative, a negative form is rarely attested and only for a few themes: e?a '(sev. inanim.) be good', ik'uy 'be tasty', ac'am 'be lactescent', away 'be cross', and possibly téw 'be big'. Normal negative formulation is used: ex"..na for prefixed-class themes, and the postpositive k'ú-na for the possible suffixed-class instance. On occasion, perhaps when the negative seemed inappropriate for the context the speaker may have had in mind, a reluctance to accept its validity was noted: after producing the privative form P/muxukišemta 'you have no sense' (ex'ikišem 'be stupid'), Sally Noble felt uncomfortable with the suggested form P/muxukišemnat (i.e. with the addition of caudal na) which was offered probabilistically by Harrington, presumably for the same context; the entry is annotated "does not sound normal to informant" (H/SN-268R). This form, taken at face value, would represent the negated plenative theme *ex'ikišem-na (not attested but conforming to pattern) 'not be smart, have no sense'. But SN may have responded to it as an attempt to negate the privative theme ex'ikišem by means of caudal na, which would be incorrect. However, she apparently did not then offer the expectable suffixed formulation using k'ú-na, so there may have been some other reason for the expressed reluctance.

As for the plenative themes of the suffixed class which are listed in Table 2, Harrington attempted to obtain negative forms for téw 'be big' and ?amepa 'be abundant (of harvestable foods)' without success; only the antithetic forms were offered in response. Interestingly, the following negative form turned up in a series of sentences elicited elsewhere: P/ñewu k'ünanta 'it is not big (yet)' (said of the moon) (H/SN-929F) But the instance is inconclusive: P/ñewu in this sentence could be the derived predicate nominal theme ñewu rather than the third person form ñew-ñu of the stative verb. In some contexts it is not possible to determine which is being used.3 For the paired terms of an antithetic opposition, then,
there is, in effect, a parallel pairing of negative terms. However, these terms are only indirectly opposed to each other, in potential "neither nor" expressions for example (there are no instances in the materials). The configuration of an antithetic pair and its negations is illustrated for four pairs of themes in Table 5. The first set of forms is based on the unmarked antithetic pair BE-VISCOS NUMBER1 : BE-FLUID č'ol. The second happens to be the only fully attested surface-marked set, and is based on BE-GOOD p3i eʔa (Table 1). The remaining sets are incompletely attested, and are based on BE-BIG țew (Table 2) and BE-HARDWORKING učhe (Table 1). The negative of țew is enclosed in parentheses in view of the uncertainty pointed out above.

ANTITHETIC OPPOSITION

<table>
<thead>
<tr>
<th>PLENATIVE</th>
<th>PRIVATIVE</th>
<th>PARAMETER</th>
</tr>
</thead>
<tbody>
<tr>
<td>AFFIRM.</td>
<td>t' an</td>
<td>č' ol</td>
</tr>
<tr>
<td>NEG.</td>
<td>t' an.k'ū-na</td>
<td>č'ol.k'ū-na</td>
</tr>
<tr>
<td>AFFIRM.</td>
<td>eʔ a</td>
<td>eχ' eʔ a</td>
</tr>
<tr>
<td>NEG.</td>
<td>eχ' eʔ a-na</td>
<td>eχ' eʔ a-k'ū-na</td>
</tr>
<tr>
<td>AFFIRM.</td>
<td>țew</td>
<td>eχaye</td>
</tr>
<tr>
<td>NEG.</td>
<td>(țew k'ū-na)</td>
<td>---</td>
</tr>
<tr>
<td>AFFIRM.</td>
<td>učhe</td>
<td>ak' učhe</td>
</tr>
<tr>
<td>NEG.</td>
<td>---</td>
<td>ak' učhe-k'ū-na</td>
</tr>
</tbody>
</table>

TABLE 5
Illustrating the intersection of antithetic and negatory oppositions.

A cross relation expressing a degree of semantic equivalence exists between diagonally opposed terms in the configurations depicted in the table, for example between the affirmative plenative t' an and the negative privative č' ol.k'ū-na. Sometimes the equivalence is displayed in a spontaneous close textual sequencing of the two terms, for example (for ak' učhe-
k'ū-na ≈ učhe):

\[ P/k'uche?nat, hisi?ta, hučhet phunsarot. \quad (H/SN-277T) \]
She's-not-lazy, she's-O.K., she's-hardworking that-woman.

Examples from elicited materials are of unmarked antithetics. For č'ol..k'ū-na ≈ t'an,

\[ P/č'olhi?nat, t'anhit. \quad (H/SN-806F) \]
It's-not-thin, it's-thick (of acorn soup).

For čala..k'ū-na ≈ lamre,

\[ P/čala?i?nanta, lamret. \quad (H/SN-607R) \]
It's-not-stiff, it's-soft (pliant; of dressed skin).

Nonsequential elicited forms also attest to cross equivalence: the approximate equivalence of očxuŋč'al 'be thin, wasted' and ex'-ixu-na 'not be fat' mentioned earlier is an instance.

Implicit in the analysis outlined in this paper is an understanding that the structural relation between parametrically congruent terms of an opposition (or of an equation for that matter) is capable of being manifested in the expressive function of language. It is in fact vividly manifested in the recurrent pairing of lexical forms in the narrative flow. But it can also come to the surface in artificial contexts such as linguistic elicitation or word association inquiries. The Chimariko materials provide a number of instances of textual pairing of antithetic terms, marked and unmarked, some prompted by elicitation, others spontaneous. For example, the antithetic pairing of the lexemes BE-GOOD isi? and BE-BAD xuli? is textually realized in the interplay of the third person forms P/hisi? and P/xuli? in the following passage from Sally Noble's simulation of a chief's discourse on the disruptive incursions of the Americans (H/SN-351T):

\[ P/khosiloxan ti?ar hin, \quad \text{Who knows how it will be:} \\
\quad hisi?xan ti?ar, \quad \text{maybe it will be good,}
\quad xuli?xan ti?ar, \quad \text{maybe it will be bad.} \]

The same pair appears again in a passage from a text on childbirth, this time in concert with a second pair, ¹BE-LONG ichu: ¹BE-SHORT ex'-ičhulæla. The focus in this passage is on where to tie and cut the umbilical cord (H/SN-174R):

\[ P/xučhulla xuli? tinta, \quad \text{Short is no good;} \\
\quad hičhu ničya?y hisi?xan. \quad \text{make it long and it'll be good.} \]
The themes phala? 'be strong, sturdy' and (unmarked privative) law..puk 'be weak, frail' are textually paired in a line from the theft-of-fire myth (D/PD-3.11, also in Dixon 350 lines 16-17):

P/phala?čisun, lawmipukni. I am strong, you are weak.

Antithetic pairings were also produced in response to attempts to obtain comparative forms. In response to the prompt, "that one is heavier than this one," the antithetic pair imita 'be heavy': čxal 'be light' was resorted to:

P/p'un himitat, p'un čxalit (H/SN-1014P)
one it's-heavy one it's-light

And the prompt "he does it better than I do" yielded the antithetic pair ošem 'be good at': ax'ošem 'be no good at':

P/pha?mot hošemta, no?ot čhopošemta (H/SN-1014P)
that-one he's-good=at=it, me I'm-no=good=at=it

The response to "one stick is longer than the other" reinforces the pairing of ičhu 'be long' and ax'ičhulala 'be short' noted above:

P/p'un hičhut, p'un xučhullat (H/SN-1000P)
one it's-long one it's-short

And finally, some odds and ends, with a peek under the rug and elsewhere.

There is some uncertainty about which table the privative theme BE-LAZY ek'učhe is most appropriately placed in. Its plenative counterpart BE-HARDWORKING učhe has an active intransitive form in the first person: P/no?ot yučhet (< ?'-učhe) 'I ain't lazy' (Sally Noble's wording; lit. 'I am hardworking') (H/SN-643T). This is the only first person occurrence of učhe in the corpus. If this is indeed an active theme, it would be aberrant in either table. But it may in fact be one of those bivalent themes in which the first person may, for some reason, be expressed either by the active pronominal element or by the stative one. A stative first person form P/*čhučhe (< *čh'-) 'I be hardworking' would have to be attested to assure that a bivalent theme is involved. There are a few documented themes of this type: itahu 'to know'; ino?k 'to recover (from illness, surprise, shock)'; i nahhta 'to be lame; to limp'; isaxni 'to cough; to be hoarse'; imanxy 'to fall off'. In these themes, the bivalence can be manifested only in the first person anumerative, because this is the only person that is diagnostic for the stative/active distinction in prefixed-class
themes. The phenomenon was noted by Dixon (325) in connection with *isarni*; his observation was actually based on a difference between speakers (D/PD–1.53 for the active instance, and D/P–5.51 for the stative) rather than on variants produced by a single speaker. An entry in the Curtin ms. (C/T–189) agrees with the stative form of D/F, while H/SN occurrences are strictly active, in agreement with D/PD. But H/SN data for the other themes do show the fluctuation in the speech of a single speaker. A comparable phenomenon has been noted for other languages. Bright (59) found a similar, exclusively first person fluctuation in Karok to be systemic and used it to define the set of stative verb themes for that language. For Tunica, Haas (1940: 59) noted a fluctuation of trans impersonal and intransitive inflections of themes denoting involuntary action such as breathing or coughing, but this fluctuation is overtly manifested in all persons. Swadesh (326) observed a fluctuation between objective and subjective inflections exclusively in the first person, in a restricted set of "deponent" verbs referring to bodily states and reactions, such as getting tired, feeling pain, tasting, sleeping, shivering. In view of the Karok instance, but in part also arbitrarily, I have place *uče* in Table 1, with the understanding that it may represent a special case.

A stative verb translated as 'to be lonesome', which on the basis of meaning could be expected to be associated with privation, appears in two distinct inflectional renditions. One gives the theme the shape of a surface-marked privative: *ak'untk'ot*, with covert pronominal reference in the third person surface form, P/k'utk'ot- (H/SN–20R). The other treats the theme as an ordinary stative theme: *uk'untk'ot*, with overt third person in the surface form, P/huk'utk'ot- (H/SN–320R). There is no corresponding plenative theme, although an etymologically related stative theme does exist in the form of *utk'očpi* 'be left over, remain, survive', which is negatable by either *ak'*.na or *ak'*.na. The apparent ambivalence in the inflection may reflect uncertainty of recollection, or it may be idiosyncratic, or it may reflect an established fluctuation. If *ak'untk'ot* is a valid shape, it would belong in Table 2 as one of the unopposed surface-marked privatives, alongside *exusamμu* 'be blind'.

A few lexicalized forms displaying an etymologically negative element in their hypomorphemic composition are found outside the set of antithetically paired stative themes of Tables 1 and 2. One of these is used predicatively, but I am not sure whether it is nominal or verbal: *tk'Ipk'ina*, '(ambience) to be still, quiet, hushed'—or perhaps: 'a stillness to be there'. In the etymologically transparent composition of this form the negative suffix *k'ina* is recognizable: this is a variant of the negative suffix *k'una* and it is used productively, though rarely and selectively. The core element *tk'Ip* is not found elsewhere.
A quantifier xuwna 'few; a little bit of' seems to derive etymologically from the the affixal negation of a core element *iwa which is also seen in the quantifier hita 'many'. The adverb xuwe..ko 'slightly, in a subdued manner; slowly', and the verb xuwe..ko 'to keep quiet' may reflect negation of an etymological core of indeterminate shape (*iwa or *uwe). And the adverb xanik'una P/xani'na 'already' derives from the affixal negation of xanik 'soon', with the literal sense of 'not soon' narrowed down to one of 'no longer anticipatable, no longer future' in the lexicalization.

There is also a possibility that the form P/č'imark'una (č'imar 'person, human'), in addition to representing a simple identificational negative č'imar-k'una 'to not be a person' (with or without a pejorative sense), can in some instances represent a lexicalized nominal č'imark'una 'an unhuman', in predicative use 'to be an unhuman', (with obligatory pejorative connotations). (By "unhuman" I mean something like German Unmensch, Russian neljudi, nečelovek.) However, I have not been able to establish that this lexicalization exists.

Lexicalized affixally negated forms with an antithetic rather than negatory sense may be found here and there in a number of lexicons. For example, in Biloxi, the negative affix ku..ni seems to be present in kudini 'to be soiled, blackened, ugly', but the residual (presumably thematic) element di seems to have no occurrence other than in this apparently lexicalized form. In Tunica, however, this type of lexicalization is extensively represented in the lexicon. Three of the four negative postfixes productively used in Tunica appear in lexicalized composite forms. The three postfixes are M/a/ha, M/p'/ha; and M/p'ha; they are conditioned by word class and paradigmatic categories, and their vocalism is subject to morphophonemic processes. There are several lexicalized forms with transparent composition, some of which are:

láp?cho 'bad': lápu 'good'
nís?aha 'anciently': nise 'recently'
?írap?aha 'naked': ?íra 'clothed'
-éti?aha 'enemy': -éti 'friend, kin'

There are also several morphemes in which the etymological core element is unique: štám?ehe 'to be impertinent'; pín?aha 'uncut (of bull)'; wántaha 'formerly'; yórém?aha 'beast, wild animal'; tókaha 'orphaned, poor'; and many more. These forms — the transparent and the opaque — are all distinguished as derived adjectives in the dictionary, though the criteria for distinguishing the transparent forms from simple negation are not spelled out.

In this paper I have not attempted to explain or speculate about the motivation in the cognitive/semological processes...
that might underlie the appropriation of negative forms for the expression of a privative sense, but have taken that phenomenon as a given. The paper deals with the mechanics of structural accommodation in the wake of the privative take-over of negative forms, as strikingly manifested in the Chimariko materials. The presence of a distinct subset of privatives marked only incidentally substructural cues (theme-initial morphophonemic sequences and their phonemic resolutions, which are identical with those of negated prefixed-class themes in their prethematic segments), as opposed to a broader, often more diffuse set of unmarked privatives in antithetic oppositions within the qualitative-evaluative domain of the lexicon, invites comparison with the expression of qualitative-evaluative privation in other languages.

ACKNOWLEDGEMENTS

Early phases of my work on Chimariko were supported by the Department of Linguistics at Berkeley under the sponsorship of Mary R. Haas and Murray B. Emeneau (1956), and by grants for microfilming from the then Survey of California Indian Languages and the Woodrow Wilson Foundation of the University of California (1962). The materials on which my (ongoing) grammatical analysis is based are primarily the extensive field notes of J.P. Harrington (1921), which became available in 1962, and the field notes of R.B. Dixon (1906). The phonological analysis takes account of all existing sources but is based primarily on the Harrington materials. All sources are being fully utilized for lexicographic purposes. The theoretical framework underlying my analysis is a modified version of the stratificational model which was being developed by Sydney M. Lamb in the 60's and 70's.

On a more personal note, I value highly the support, counsel and encouragement offered by Mary Haas in the years that I was active in linguistic studies. Her work, her clarity of perception, analysis, and exposition, have been an inspiration to all of us who were her students.

FOOTNOTES

1. In the Tupí-Guaraní materials consulted (Dietrich 303) it is not clear whether the element in question is used productively in word formation or nonproductively (i.e. found only in lexicalizations). If it is productive, the element would represent a structural unit (morpheme, lexeme) distinct from the predicative negative element, although homophonous with it.

2. Cited forms in this paper are in morphophonemic transcription unless specially marked. Underscoring of forms is interrupted for typographical reasons where subscript elements interfere, but should be understood as being continuous under
the form. Characters with subscript elements, when cited in isolation (as in fn. 13), are likewise to be read as underscored.

3. The combination of occlusive + h is a digraph, not a consonant cluster. I use this notation for typographical convenience only. In theory, a close sequence of occlusive and h should be possible, but it does not turn up in the materials.

4. The pronominal elements used with prefixed-class stative verbs are 看查看", SetName, ighbor in the anumerative, and a distinctive second person ꜠ in the plural. Second person imperative is ꜠ in the anumerative, Checkout in the plural.

11. Typically the suffixed-class stative theme takes the inflection ꜠, ꜠, ꜠ in the anumerative (slash indicates a deletable consonant — see fn. 21), and ꜠, ꜠, ꜠, ꜠ in the plural. However, ꜠ 'be big' takes a u-form of the inflection, a characteristic shared with only one other nonderived stative theme, ꜠ 'be pregnant': ꜠, ꜠, ꜠ in the anumerative, and ꜠, ꜠, ꜠, ꜠ for first and second person in the plural. In its dictionary form this theme is marked with a superscript " as a morphemic tag: ꜠. The tag is not a morphophoneme, and is not used in normal citations of the theme. A number of other statives take the syncopating inflection ꜠, ꜠, ꜠ in the anumerative (the breve marks a syncopating vowel), and ꜠, ꜠, ꜠, ꜠ in the plural.

12. The symbol P/ indicates phonemic transcription. Its domain extends to the end of a citation, including a text segment of more than one line, and also to the entirety of a paradigm in the charts.

13. The phonemic symbol ꜠ is used for the velar fricative as opposed to postvelar ꜠ whenever it can be positively identified. The unmarked symbol ꜠ is intentionally ambiguous; it is used when the phonemic value cannot be reconstituted. Harrington did not distinguish the two positions in 1921, although in subsequent field work he learned to do so.

14. Dixon understood the Mrs. Dyer he worked with (Dixon, 363) to be Polly Dyer (D/PD-1.1; see fn. 23). Polly Dyer was Sally Noble's mother. There is evidence that he may actually have been working with Mary Dyer, who probably was not related to Polly Dyer but was the mother of Sally's half sister Martha. This may explain some of the striking differences between Sally's and "Polly's" speech. Be that as it may, in applying the investigator/native speaker code I use PD for the Mrs. Dyer Dixon worked with.

21. Note that syncope is effected sequentially, in the order of production, in a deletion pass which precedes the realization pass. The morphophoneme ꜠ is deleted postconsonantly and is realized as P/ in postvocalically.

22. The medial consonant in a triple cluster (here the ꜠ of ꜠) is unrealized.
23. The investigator/native speaker codes appearing in this paper are H/SN = Harrington with Sally Noble; D/SN = Dixon with Sally Noble; D/PD = Dixon with Polly Dyer; D/P = Dixon with Friday; C/T = Curtin with Doctor Tom. Numerical and alphabetical symbols which follow these designations refer to location in the set of manuscripts: 634T is page 634 of Texts; F = First Notes, R = Rehearing (of the early notes). For the Dixon materials, 1.1 = notebook 1, page 1.

24. The example is not entirely satisfactory either, because no instance of third person suppression has turned up with this particular theme.

21. When I know that P/k'una is a postposition, I write it as a separate word. Otherwise, as in the examples cited here, I allow it to join the preceding word.

32. Carol Eastman (235) has noted a similar constraint in Haida.

33. a.imm. = stative of immanence; (IBx) = prefixed-class inflectional subclass B (x = with inherently covert third person).

34. The predicate nominal, which I take to be a lexicalized form of the inflected verb (inflected for third person), is in every case a consonant-initial theme, whether it stems etymologically from a consonant-initial theme (as is the case with the predicate nominal tewu 'a big one', < *tew-Mū 'he be big') or from a vowel-initial theme (as in hisi? 'a good one', < *h-isī? 'he be good'). Hence the predicate nominal accepts only suffixal negation and positive negation (the few attested instances are indeterminate as to which formulation is being applied); prefixal negation is excluded. Examples are:

P/hisi?k'unaxananta. (< hisi?-k'ū-na- or hisi? k'u-na-)
'They will not be good (ones).’ (H/SN-351T)

P/hisi?k'u[k'ti]. (< hisi?-k'ū-istik' or hisi? k'u-istik' i)
'They can't be good (ones).’ (H/SN-143T)

41. The selected examples cited here are for the most part in reconstituted or in phonemic transcription as per the sources from which they are drawn, and are not specially marked. But where the cited forms are rendered morphophonemically, they are preceded by the notation M/ to indicate this fact.

42. Literally "not clean" per Dorsey and Swanton (183, under de). For the transcription I follow Einaudi.

43. The Tunica forms are from Haas (1940:120-1, and 1953).
REFERENCES


MANUSCRIPTS

Harrington, John P. Field notes from 1921. Originals are in the BAE, Smithsonian Institution. Microfilm at the Survey of California and Other Indian Languages, U.C. Berkeley.


Curtin, Jeremiah. Powell Schedules and additional notes from 1889. Originals are in the BAE.
Hyphenating Harrington Style

Suzanne Wash

University of California, Santa Barbara

1. Introduction

In his transcription of Barbareño Chumash, Harrington used a hyphen in what appear to be two straightforward contexts: margins and morpheme boundaries. The reason that Harrington would use a hyphen at a margin is obvious. His motivation for using a hyphen at what simply appears to be a morpheme boundary, however, is not as obvious as it may seem at first glance. In Harrington’s transcription, hyphens that occur at morpheme boundaries actually fall into two categories: hyphens that function to clarify what the morphemes are, and hyphens whose actual purpose is to clarify phonological boundaries. There are thus three main contexts of hyphenation in Harrington’s transcription system: margins, morpheme boundaries and phonological boundaries. These contexts may overlap, and indeed the latter two contexts of hyphenation always overlap in Harrington’s transcription of Barbareño, but the function that a hyphen serves is radically different from one context to the next.

2. Hyphens at margins

As with standard usage, Harrington used a hyphen at a margin to indicate that the hyphenated word was to be completed on the following line. Harrington usually divided the word at a syllable boundary, as in (1):²

(1) 59:0355/57  hi-l-∅-iy-e-ɾunixinets  ‘unmarried ones’

DP-ART-NM-PL-N-be.married
As seen in example (1), the word *hilye?uniwetiš* ‘unmarried ones’ occurs twice. It is hyphenated at the margin both times, at different places. In the first occurrence, the word is hyphenated as *hilye?uni-wetiš*, and the hyphen appears after the second syllable in the stem, *uniwetiš* ‘to be married’. In the second occurrence, the word is hyphenated as *hilye?-uniwetiš*, and here the hyphen appears after the negative proclitic, *e*-.. What this shows is that when Harrington used a hyphen at a margin, he did not necessarily hyphenate at a morpheme boundary. This is in keeping with standard usage.

Example (1) also shows hyphenation at a syllable boundary, which is in keeping with standard practice. It is important to note, however, that Harrington would just as well hyphenate a word at an illicit syllable boundary, as seen in (2) (cf. also (29)). In these latter two examples, the hyphen follows the syllable onset, and thus interrupts the contiguity of the syllable.³

(2) 59:0148/198  
*hiho?-p-al-wilipštin-pi*  
*DP-DIS-2-SUB-open.the.eyes-LOC*

Thus in (2), the word *hiho?palxilip-štinpi* ‘where you open your eyes’ is hyphenated as *hiho?palxilip-štinpi*. This word would be syllabified as *hi.ho?pal.ksi.li.piš.tin.pi*, thus the hyphen appears after a syllable onset. In standard practice, words at the end of a line are supposed to be divided at a syllable boundary. For Harrington, however, prosodic factors such as syllabification were irrelevant when he used a hyphen at a margin, because the hyphen served only to indicate that a word continues on the following line. However, as will be seen in section 4, in other contexts there is a pertinent relation between prosody and where Harrington placed a hyphen (e.g. the hyphenated word *pi-kastš寺院* above).

In sum, what examples (1), (2) (and (29)) reveal is that the hyphens that appear at margins were put there simply to show that the word continues on the next line. Such hyphens, in Harrington’s transcription, need not respect morpheme boundaries or phonological boundaries. This context of hyphenation is rarely if at all ambiguous, as would be expected.
3. Hyphens at morpheme boundaries

In the narratives that Harrington wrote down, it is not unusual to find short paradigms or grammatical notes. Often these come at the end of the text. In such instances, Harrington might focus upon one or more of the morphemes that comprise the word or phrase in question. One way he would do this is by using a hyphen to isolate the boundaries of the morphemes. A typical example of this is seen in (3) below:

(3) 59:0666/69   \textit{k-iy-sa?-poš-un-?}  \quad \text{‘we're going to go gather pine nuts’}
1-PL-FUT-pine.nut-VBL-AND

Example (3) shows a short, elicited paradigm, based on the root \textit{poš-} ‘pine nut’, that comes at the end of a text about pine trees. In this context, Harrington’s use of the hyphen is meant to delineate the boundaries of certain morphemes: the first person plural prefix \textit{kiy-} (from \textit{k-} ‘1st person’ and \textit{iy-} ‘plural’), the future tense prefix \textit{sa?-}, the root \textit{poš-} ‘pine nut’ and finally, the verbalizing suffix \textit{-un} (which also contains the andative glottal stop suffix here). Hyphens in this context are meant only to give information about morpheme boundaries; they are not meant to indicate phonological boundaries, such as where syllables begin or end. Thus hyphens in this context can precede vowel initial stems and affixes like the suffix complex, \textit{-un} and the stem \textit{šmax} ‘to throw at’ in (4), both of which begin as ill-formed syllables for lack of an onset.
This kind of hyphenation rarely appears in the narrative proper, but one instance is seen in (4) below, where Harrington and Yee were apparently discussing whether it was better to use the suffix -šiš instead of the suffix -šiš, both of which can be interpreted as either a reciprocal or a reflexive:

(4) 59:0228/232  s-iššmax-šiš  ‘they throw at one another’
    3-PL-throw.at-RCP

\[ \text{iššmax-šiš} \]
\[ \text{they throw at one another} \]

Example (4) also reveals one more general context of hyphenation, as seen in \text{il-xarp’ap} and \text{hilon-ontvangstnito}. This will be taken up in section 4. The hyphen in \text{il-xarp’ap} indicates a post-proclitic pause (to be discussed in 4.1), and the hyphen in \text{hilon-ontvangstnito} marks a syllable boundary (to be discussed in 4.2).

So far I have shown two contexts in which Harrington used a hyphen: at margins, to show that a word continues on the following line, and at morpheme boundaries, to make explicit where certain morphemes begin and end. There is a third context in which Harrington used a hyphen. This context happens to involve morpheme boundaries as well. The purpose of the hyphen in this case, however, is not to delineate morpheme boundaries; rather, it serves to clarify his transcription with respect to phonological boundaries.

4. Hyphens at phonological boundaries

Phonological boundaries are another context in which Harrington used a hyphen. There are two types of phonological boundaries in particular: the syllable boundary and the pause that may occur between a proclitic and the remaining stem. These comprise the third context for hyphenation in Harrington’s transcription of Barbareño.
This third context is somewhat obscure because, in all the cases that I have seen, the phonological boundary happens to coincide with a morpheme boundary. So unless one is familiar with the phonology of the language (in this case, Barbareño) one might think that Harrington decided to put another hyphen at a morpheme boundary for no apparent reason, other than to make the morpheme boundary more explicit at that particular instance. It would then be tempting for one to conclude that Harrington’s use of hyphens was inconsistent and not really meaningful. With such an erroneous assumption as that, one might easily miss out on some important insights into the phonology of the language.

The major difference between the hyphens used at margins and morpheme boundaries and the hyphens used at phonological boundaries is that the latter play an essential role in Harrington’s phonetic transcription. They give explicit information about the pronunciation of a word. They alert the reader that there is a phonological boundary—not just any phonological boundary—but a boundary that cannot necessarily be assumed to exist, given the phonotactic patterning of the language. The hyphen in this context thus serves to make Harrington’s phonetic transcription much more accurate and much less ambiguous than it otherwise would be. I shall illustrate this with examples from the two types of phonological boundaries mentioned above: pauses that may occur between proclitics and the remaining stem, and syllable boundaries.

4.1 Hyphens that mark the presence of a post-proclitic pause

In Barbareño Chumash narratives, Harrington would often put a hyphen after proclitics, especially the proclitics ñi- and hiï(l)-. But one can count just as many instances where he does not hyphenate these clitics. This variation in hyphenation strikes one as arbitrary, until one learns that these clitics may or may not form a phonological word with the constituent that follows them. In listening to Madison Beeber’s tape recordings of Mary Yee, one notices that she sometimes has longer pauses after these clitics. From these variations in Yee’s pronunciation one can make certain assumptions about Harrington’s transcription: the proclitic is written as directly preceding the stem without an intervening hyphen when the proclitic and the following stem do form a coherent, phonological word, with no noticable pause after the proclitic. However, if there is a noticable pause after the proclitic, then Harrington would put a hyphen between the proclitic and the following stem to make explicit the fact that there is a pause.

This variation in pause length is often seen in different tokens of a given word, even within the same narrative. Examples (5) through (8) below show pairs of words that differ only in the presence or absence of a hyphen:
(5) Ṝi-ka-s-ḥis'uy
‘it is a sign [that...]’

a. 59:0140/198 (not hyphenated)

b. 59:0148/198 (hyphenated)

(6) Ṝi-s-wil-waš
‘it was’

a. 59:0531/101 (not hyphenated)

b. 59:0529/101 (hyphenated)
(7)  hi-l-xšo?  'of a sycamore (tree)'
     DP-ART-sycamore

a.  59:0667/71  (hyphenated; second line to the top)
b.  same page  (not hyphenated; second line to the bottom)
(8)  

hi-l-xāp  
DP-ART-rock  

'a rock'

a. 59:0687/76  (hyphenated; second line to the top)
b. same page  (not hyphenated; bottom line)
Examples (5) through (8) illustrated pairs of words that differ only in the presence or absence of a hyphen. Two of these pairs (examples (7) and (8)) are respectively on the same handwritten page of the narrative. When faced with pairs of words like these, one has a choice of two interpretations: either the hyphens were inconsistently used, or there is a meaningful pattern to them. There is also the question of what purpose the hyphens serve. One could choose the interpretation that Harrington simply decided to make a proclitic boundary explicit sometimes, but not at other times. I would argue that hyphens in this context were not meant to simply point out morpheme or proclitic boundaries, however. First, these proclitics occur in great frequency in Barbaresco narratives. As discussed in section 3, when Harrington wanted to make morpheme boundaries explicit, he did so in specific contexts, such as the elicitation shown in (3), or when the choice of a specific morpheme was at issue, as seen in (4) with -šiš vs. -šiš. Also, as is evident in the previous examples, when the dependent proclitic hi- (sometimes elided to form i-) and the article, k-, come together to form hil- (or hi-), Harrington never put a hyphen between them, i.e., one would not expect to see *hi-l- hyphenated as such, within a narrative. Obviously, then, the hyphens in (5) through (8) were not put there to mark off morpheme boundaries.

Harrington is known for going to great lengths to capture variable, phonetic minutiae in the speech of his consultants. It is therefore not surprising that he would make note of Yee’s variation in post-proclitic pausing. It is important to note, however, that the hyphens are not redundant in this context. Since the pausing may or may not occur, one cannot always know when to expect it. Thus without the hyphens, Harrington’s transcription of Yee’s pronunciation would be ambiguous in places: one could not always be certain when Yee’s proclitics do and do not form a phonological word with the following stem. The hyphens thus serve to make Harrington’s phonetic transcription more accurate by indicating a phonological boundary that one could not be sure was there otherwise.

In 4.1 I examined one kind of phonological boundary that Harrington marked with a hyphen: the post-proclitic pause. In 4.2 I discuss one other kind of phonological boundary that Harrington marked with a hyphen: the syllable boundary. In both cases, the hyphen plays a crucial role in Harrington’s phonetic transcription.

### 4.2 Hyphens that mark the presence of a syllable boundary

Syllabification in Barbaresco Chumash is usually straightforward. A syllable must have at least one consonant as an onset. If there is an intervocalic, medial cluster of two consonants, then the first consonant closes the preceding syllable (becoming its coda), and the second consonant begins the following syllable (becoming its onset). Thusly the medial cluster -qw- in ʔqowəl ‘hair; head hair’ syllabifies as ʔqo-wəl, the q serving as a coda, the w serving as an onset. However, there are two conditions for which this generalization does not always work: (1) when a consonant (a stop or sonorant) is followed by a glottal stop, as in -qʔ- or -wʔ-; and, (2) when a consonant is followed by an identical or nearly identical consonant, as in -kk- and -kq-. When either of these conditions occur, the consonants in question usually merge to form one consonant. The following examples illustrate how a -Cʔ-sequence is typically syllabified, when C is a stop or sonorant:
Examples (9) through (13) show that, when a stop or sonorant precedes a glottal stop, one can expect that both consonants will merge into one, glottalized consonant, and be syllabified as such. Thusly mut?ey always syllabifies as mu.ley, never as *mut?ey. One would not expect a stop or sonorant and a following glottal stop to be heterogeneously syllabified, as in *mut?ey. Yet, as it turns out, there are many instances in which the stop or sonorant and a following ? do not merge. When they do not merge, they end up syllabifying to different syllables, and Harrington used a hyphen to mark the ‘unexpected’ syllable boundary between the consonant and the following glottal stop in such instances. Typically, one sees this in reduplicated words, as in example (4) seen earlier and examples (14) through (18) below:

(14)  59:0136/197
p-al-nuh-nu-hik-wun  hi-hoʔ-k-ʔal-ʔalyuʔ
2-sub-R.-com-do-pl.obj  dp-dis-1-r.-brick-em
'[what] are you doing with my bricks?'
(15) 59:0121/192

?i-mes-i-y-?ap-?api-t'owotš  hi-ho?-1-kan-kanyon-
'the cannons were only just throwing out smoke'

(16) 59:0334/137

hi-ho?-1-mol-moloq-iwaš-?  hu-l?-in-inyu-?
'of the ancient Indians'

(17) 59:0639/46

hu-s-ty-kam-kumi  hi-l?-ol-?olomowil-?
RM-3-PL-R.-ARRIVE  DP-ART-R.-automobile-EM
'(when) the automobiles were [first] coming in'
As seen in examples (4) and (14) through (18) above, Harrington often used a hyphen in reduplicated words. Not in all reduplicated words, however. In (18), for example, there is a hyphen between the reduplicant prefix ꞌap- and its base, ꞌapartiš ‘village’, yet there is no hyphen within the reduplication that is written directly above it, ꞌililijik ‘between [the villages]’. Harrington’s use of the hyphen in these examples reflects the expected and unexpected syllabification of consonant clusters. There is no need to put a hyphen in ꞌililijik, because the consonants of the medial cluster, -yl-, syllabify in the expected way: ꞌi.li.jik. This is what one would anticipate given Barbareño syllabification as explained at the beginning of section 4.2. It was also shown that, when a stop or sonorant is followed by a glottal stop, the two consonants usually merge into one, glottalized consonant (cf. (9) through (13)). In the reduplication ꞌap-�apartiš, however, the final coda p in the reduplicant prefix, ꞌap-, does not merge with the glottal stop onset of the base, ꞌapartiš; that is to say, the reduplication syllabifies as ꞌap-�apartiš. Given the examples in (9) through (13), one might expect this reduplication to be syllabified as ꞌap.a.p.a.niš instead.

As it turns out, there is a good reason that productive reduplications like ꞌap-�apartiš do not syllabify like the words seen in (9) through (13). In Barbareño productive reduplication (as discussed in Wash 1995), it is normally the case that the reduplicant and base do not share the same syllabification domain. Hence, the coda (i.e. final) consonant of the reduplicant does not syllabify as (or with) the onset to the base. In other words, the reduplicant and base are separated by a syllable boundary. However, for a significant number of high-frequency words, this boundary has broken down. Often, these words have two variants in reduplication: a productive variant, which has a syllable boundary separating the reduplicant and base, and a lexicalized variant, which has no such boundary. In the latter case, the coda or final consonant of the reduplicant must syllabify homogeneously (and phonetically merge) with the onset of the base if the phonotactics allow it. Most of the remaining examples show both the productive and lexicalized variant of the reduplicated word in question. A few words, however, have retained only the lexicalized variant, such as ꞌakliš ‘word’ in (19) and ꞌenex ‘woman’ in (20) below:
The reduplication of ḫadw "word" is syllabified as ḥadw, not as ṭṭadw, which would be the productive variant. The glottalization that Harrington wrote over the reduplicant's coda, k, shows that it has merged with the glottal stop onset of the base. The syllabification is clear, and thus there is no need for a hyphen.

The reduplicated word for ṭenn "woman" is lexicalized, and thus syllabifies as ṭen.œnex rather than as ṭen.œnex, which would be the expected syllabification if this word were productively reduplicated. Harrington noted this fact, using a hyphen to show where the syllable boundary would be if this were a regular reduplication:

"Absolutely not ṭen-ṭe-ḥēk. Imp. If you did not know the irreg. pl. you wd say ṭen-ṭe-ḥēk" (33:0436).

Other words that derive from the same root for 'female' do show the regular reduplication however, as seen below in (21) for 'old woman' and in (22) for 'adolescent girl':
Given this difference between the reduplicated form for 'woman' and those seen for 'old woman' and 'adolescent girl', if Harrington had left out the hyphen in (21) and (22), i.e., if he had written them as ʾen-ʾenexwāš and ḥol’en-ʾenxwēḵ, then one could not be certain about how these forms should be syllabified or pronounced. Without the hyphen, the consonant and the following glottal stop would be written right next to each other, in this case ṣn?..., which could be interpreted as a glottalized n, i.e. [n]. This would have made his transcription ambiguous. Harrington of course was well aware that the reduplicated form for 'woman' syllabifies differently from the reduplicated forms for 'old woman' and 'adolescent girl'. His use of a hyphen at the correct syllable boundary makes his phonetic transcription clear, and serves to show that there is indeed a syllable boundary between the final consonant of the reduplicant and the initial glottal stop of the base.

The importance of the hyphen in making his transcriptions accurate is even more evident in (23) through (26), which show words that have both a productive and lexicalized variant of reduplication. Again, the main difference between these variants is in how they are syllabified: in the productive variant there is a syllable boundary between the reduplicant prefix and the following base, whereas in the lexicalized variant this boundary no longer exists. The productive variant is given in part (a.) of the examples below, and it is always written with a hyphen; the lexicalized variant is given in part (b.) of the examples, and does not have a hyphen. To further illustrate the difference in pronunciation between the two variants, I show how each reduplication is syllabified in the examples below:
(23) reduplications for itšalayaš 'one's own trail'

a. 59:0651/132 (productive variant; hyphenated)\(^{11}\)

\[\text{hu}-s-\text{i}-\text{tš}-?+\text{alayaš}\] ‘their (own) trails’
RM-3-PL-R.-EP.?+one’s.own.trail-EM

Syllabification: hu.si.itš.?+ a.la.yaš

\[\text{husi} \quad \text{itš} \quad \text{itša} \quad \text{alayaš}\]

b. 59:0531/101 (lexicalized variant; not hyphenated)

\[\text{hi}-s-\text{i}-\text{tš}+?+\text{alayaš}\] ‘their (own) trails’
DP-3-PL-R.-EP.?+one’s.own.trail

Syllabification: hi.si.yi.itš.š a.la.yaš

\[\text{hisiyi} \quad \text{itša} \quad \text{alayaš} \]
(24) reduplications for \( ?elye\text{wun} \) 'swordfish'

a.  59:0207/221 (productive variant; hyphenated)
    \textit{hi-he}\text{-\text{-}l}\text{-}\text{-}\text{-}el\text{-}\text{-}elye\text{-}wun\text{-}\text{-} \\
    \textit{the swordfish (as a group)}
    \textit{DP-PRX-ART-R.-swordfish-EM}

    Syllabification: \textit{hi.he\text{-}le.le.\text{-}ye.wu\text{\text{\u02d9}}}\text{\text{\u02d9}}

b.  59:0071/181 (lexicalized variant; not hyphenated)
    \textit{hi-ho-l}\text{-}\text{-}\text{-}\text{-}el\text{-}\text{-}elye\text{-}wun\text{-}\text{-} \\
    \textit{the swordfish (as a group)}
    \textit{DP-DIS-ART-R.+swordfish-EM}

    Syllabification: \textit{i.ho.le.le.ye.wu\text{\text{\u02d9}}}\text{\text{\u02d9}}
(25) reduplications for *ekwel* ‘to make’

a. 59:0708/248 (productive variant; hyphenated)
\[s-\text{am-ek}^\ast-\text{ekwel}^\ast\]
3-\text{DF-R.-EP.}^\ast-\text{make-NM}

Syllabification: *sam.\text{ek}^\ast\text{ekwel}^\ast*

*b* they are making [a canoe]*

b. 33:0581 (lexicalized variant; not hyphenated)
\[s-\text{ek}^\ast+\text{ekwel}^\ast\]
3-\text{R.}+\text{EP.}^\ast+\text{make-NM}

Syllabification: *se.k\text{ekwel}^\ast*

*b* she is making it

Even high-frequency loan words will show this variation:

(26) reduplications for *\textit{inyu} ‘Indian} < \text{Spanish} \textit{indio}*

a. 59:0590/42 (productive variant; hyphenated)
\[\text{ho}^\ast-\text{lin-\textit{inyu}^\ast}\]
\text{DIS-ART-R.-Indian-EM}

Syllabification: *ho.\text{lin.}\textit{inyu}^\ast*

*b* the Indians

b. 59:0331/138 (lexicalized variant; not hyphenated)
\[\text{ho}^\ast-\text{lin-\textit{inyu}^\ast}\]
\text{DIS-ART-R.+Indian-EM}

Syllabification: *ho.\text{lin.iny}u^\ast*
The hyphen seen in the examples on the preceding pages serves to mark a syllable boundary that one might otherwise not know was there. It thereby makes Harrington's phonetic transcription more accurate. On rare occasions, Harrington left out a hyphen without indicating whether or not the coda consonant was glottalized. In these cases, his transcription is ambiguous with respect to the syllable boundary in question. In example (27) below, there are three reduplications for the word $\rho\alpha l$, 'leg; foot; paw'. The productive reduplication in (a) has a hyphen to mark the syllable boundary between the reduplicant and its base:

(27) reduplications for $\rho\alpha l$ 'leg; foot; paw'

a. 59:0057/178 (productive variant; hyphenated)
   $ke-s-\rho\alpha l-\rho\alpha l'$
   and-3-r.-leg-em

Syllabification: $kes.\rho\alpha l.\rho\alpha l$

The lexicalized reduplication in (b) is written with an 'arc of glottalization' over the reduplicant coda, -l, which means that the coda and the base-initial glottal stop have merged (becoming -l), and thus syllabify as an onset to the base:

b. 59:0053/178 (lexicalized variant; not hyphenated)
   $ke-ho-\rho\alpha l.\rho\alpha l'$
   and-dis-3-r.+leg-em

Syllabification: $ke.ho's.\rho\alpha l.\rho\alpha l$

The reduplication in (c), however, is ambiguous. It does not have a hyphen or seemingly any indication of whether the reduplicant coda is glottalized:

c. 59:0484/86 (not hyphenated, could be either productive or lexicalized)\(^{12}\)
   $ke-ho-s-\rho\alpha l-\rho\alpha l'$
   and-dis-3-r.-leg

Syllabification:

$ke.ho's.\rho\alpha l.\rho\alpha l'$ (if productive)

$ke.ho's.\rho\alpha l.\rho\alpha l$ (if lexicalized)
From the examples of reduplication given in section 4.2, it is clear that the hyphen plays a crucial role in Harrington's phonetic transcription. However, there are instances in which the hyphen is somewhat redundant, as in the reduplications in (28) and (29) below:

(28)  59:0229/232  
    hi-s-iy-îs-?išmax-wun  
    DP-3-PL-R.-EP.?-throw.at-PL.OBJ  
    'stones which they will use] to throw at (the others)'

Syllabification: hi.si.yiš=?iš.max.wun

(29)  59:0264/240  
    hi-l?-iy-ûs-?-ušuyep-š  
    DP-ART-NM-PL-R.-EP.?-change-IP  
    'different (people)'

Syllabification: hi.li.yuš=?u.šu.yepš

In (28) and (29) above, Harrington used a hyphen to make it clear that there is a syllable boundary between the reduplicants îs- and îuš- and their respective glottal stop-initial stems, -?išmax and -?ušuyepš. Since this use of the hyphen parallels its usage in the previous examples to a great extent, one might think that the hyphens in (28) and (29) are just as necessary as those in the previous examples. The reason the hyphens are not as necessary in (28) and (29) is that the syllable boundary is readily predictable, and never ambiguous, for medial clusters that consist of a fricative and a following glottal stop, i.e. -s?-,-z?,-x?- and -h?- for Barbareño. Such medial clusters always syllabify in the following way: the fricative closes the preceding syllable, and the glottal stop begins the following syllable. This is illustrated in the words below:
(30) $u$?is$\text{m}$on $\rightarrow$ $u$?is$\text{m}$on 'to gather'
(31) $u$?$\text{s}$ex $\rightarrow$ $u$?$\text{s}$ex 'to spread'
(32) $e$?e$\text{c}$ $\rightarrow$ $e$?e$\text{c}$ 'to laugh'

Given these facts of Barbareño phonotactics, one can see that the hyphens in (28) and (29) are redundant. What Harrington was probably trying to do, in cases like these, was be as consistent as possible with how he transcribed the syllable boundary in other consonant-glottal stop environments, like those seen throughout the examples in 4.2, in order to make his transcription as clear as possible.

Throughout 4.2 I have shown that Harrington used the hyphen as a transcriptional device for marking syllable boundaries. Reduplications are the most common venue for this type of hyphen because the syllable boundaries in many reduplicated forms must be made explicit and cannot be taken for granted. On rare occasions, however, there will appear a non-reduplicated word containing an unexpected syllable boundary. Likewise in these instances, Harrington used a hyphen to make this boundary explicit in his phonetic transcription. This is seen in (33):\[13

(33) 59:0129/195
hi-p-sa$?$u$?$alakutay-us-wun
DP-Z-FUT-ASSOC-be-kind-APPL-PL-OBJ
'[it was necessary] that you be kind to them [the sorcerers]'

Syllabification: hip.sa.$?$u$?$.a.la.ku.ta.yus.wun

Harrington originally wrote the ...$t$$\text{s}$. as ...$t$. but then apparently decided that the ...$t$$\text{s}$. was not glottalized by the following stem-initial $p$ after all, as would otherwise be expected, hence he erased the glottalization over the ...$t$. and put a hyphen between the ...$t$. and the following glottal stop. The second and last time that this verb was mentioned, Harrington again used a hyphen:
(34) 59:0129/195
hi-siš-ʔalakutay-waš
DP-3-ASSOC-be.kind-PST
'she treated him kindly'

Syllabification: hi.siš.ʔa.la.ku.tay.waš

Tsu 'la mo'koo' hisiš-ʔalakutay-waš.

So far all of the examples of hyphen usage in 4.2 have shown how the hyphen serves to mark a syllable boundary between a consonant and a following glottal stop. While this happens to be the most potentially ambiguous context for a syllable boundary, there is one other context that can be just as problematic. This is exemplified in (35) below:

(35) 59:0089/9
p-sak'k'al'alan-us-wun14
2-R.-holler-APPL-PL.OBJ
'you holler at them'

Syllabification: psak'k'al'ala.nus.wun

'iyemē p'ak'k'al'ala'-

The you holler at them

nušiwin, ʔa'se, ṭa'xin, ʔa

they don't hear you.
In this instance, the hyphen serves to mark a syllable boundary between to identical consonants. In Barbareño, if two identical or similar obstruents are adjacent and syllabified homogeneously, an aspirate form will occur:

\[ k + k \rightarrow k^h, \quad k + \kappa \rightarrow \kappa^h, \quad s + s \rightarrow s^h, \quad s + \ddot{s} \rightarrow \ddot{s}^h, \quad p + p \rightarrow p^h, \quad \text{etc.} \]

This has been discussed by Harrington (various places throughout the grammatical notes in Reel 33), and in Beeler 1970, 1976, 1979; Beeler and Whistler 1980; Klar 1977; Whistler 1980; and (briefly) Wash 1995. Given this characteristic of Barbareño, if Harrington had not put the hyphen in the word in (35), one might misinterpret the sequence \(*\ldots k^h k^h\ldots\) as having the phonetic value \(*[q^h]*\) (Harrington’s \( \kappa = [q] \)), and pronounce the word as \(*[sa.q^h al\ldots]*\) instead of as \([saq^h q^h al\ldots]\). The use of the hyphen in this context, then, makes the transcription less ambiguous and less subject to misinterpretation.

Finally, it should be noted that Harrington sometimes used a period instead of a hyphen to mark what would otherwise be an ambiguous syllable boundary in his transcription:

(36) 60:0622

\[ \text{hirheka-}s^h\text{ut-}s^h\text{utowitš} \]

right-away-3-R-quick

‘he was very quick’

Syllabification: \textit{hi.rhe.ka.} s\textsuperscript{h} u.t\textsuperscript{h} u.to.witš

If Harrington had not used a period or any other device, such as a hyphen, to mark off the syllable boundary between the \(t\) and \(\ddot{s}\), one could then easily misinterpret his transcription and think that the word should be syllabified or pronounced as \textit{hi.rhe.ka.s\textsuperscript{h} u.t\textsuperscript{h} u.to.witš} because, in Harrington’s transcription, the sequence \(..t\ddot{s}\) always has the value \([\ddot{c}^h]\).

5. Conclusion

I have shown that there are three major contexts of use for hyphenation in Harrington’s transcription of Barbareño Chumash: margins, morpheme boundaries, and phonological boundaries. At margins, the hyphen serves to show that a word continues on the following line. In this context, the hyphen may or may not respect morpheme boundaries and/or syllable boundaries. At morpheme boundaries, the hyphen’s purpose is to show clearly what the morphemes are in a given word or phrase. In this context, one usually sees several hyphens in the word or phrase, which is often a grammatical note or elicitation
involving a certain root or stem that Harrington was especially interested in. Finally, at phonological boundaries, the hyphen serves two specific purposes: to indicate that there is a pause between a proclitic and the following stem, and to mark a syllable boundary, especially when such boundaries might otherwise be unexpected or unpredictable given the regular, productive patterns in the language. In every instance, these latter two sub-contexts happen to coincide with a morpheme boundary, so unless one is familiar with the phonology it may appear that Harrington's use of hyphens was not only random, but relatively meaningless as well. I have shown, however, that the hyphen serves an extremely important role in Harrington's phonetic transcription. It alerts the reader to an expected pause or syllable boundary, thereby giving crucial information about how a given word is correctly pronounced. In this final context, the hyphen serves to make Harrington's transcription much more accurate and less ambiguous than it otherwise would be.

Endnotes

1. My work on Barbareño Chumash has been made possible by grant BNS90-11018 from the National Science Foundation (Marianne Mithun, principal investigator). This particular study is an outgrowth of the research I did for my master's thesis, 'Productive reduplication in Barbareño Chumash' (Wash 1995). This study is also a continuation of my research on Harrington's notations and transcription methods (cf. Wash 1993).

The Barbareño data come from microfilms of John P. Harrington's manuscript materials. These microfilms were kindly made available by John Johnson at the Santa Barbara Museum of Natural History.

2. Each example number is followed by a reference number that indicates the microfilm reel, frame, and narrative text to which I have given an interlinear gloss. The citation form for reference numbers is e.g. '59:0355/57', which reads 'microfilm reel 59, frame 708, Barbareño text 57'.

For the hyphenated words in question I have provided an underlying form, interlinear gloss and free translation. The abbreviations in these glosses are as follows:

1  'first person'
2  'second person'
3  'third person'

AND  'andative'
APPL  'applicative'
ART  'article'
ASSOC  'associative'
COM  'comitative'
DIS  'distal'
DP  'dependent marker'
EM  'emphatic'
EP  'epenthetic'
FUT  'future'
IDF  ‘indefinite’
INST  ‘instrumental’
IP  ‘imperfective’
ITR  ‘iterative’
LOC  ‘locative’
N  ‘negative’
NM  ‘nominalizer’
NPST  ‘noun past’
OBJ  ‘object’
PL  ‘plural’
PRX  ‘proximal’
PST  ‘past’
R  ‘reduplicant’
RM  ‘remote’
SUB  ‘subordinator’
VBL  ‘verbalizer’

All Barbareño examples are from Mary Yee, the last known fluent speaker of any Chumash language.

3. In the Barbareño lexicon, syllables must have an onset, as was first noted by Harrington (33:0230). Barbareño syllable structure is discussed in detail in Wash 1995.

4. Incidentally, the syllabification could also be ..pi.štin.pi, but not in careful speech. As Harrington explains (33:0230), "The first consonant of a word-interior two-consonant cluster ends the preceding syllable, the second to the following syllable." I confirmed this in my M. A. thesis (Wash 1995) after listening carefully to Mary Yee’s pronunciation on Madison Beeler’s (1954-1961) recordings.

5. ṭi- means something like ‘as for the one in question; pertaining to what was just mentioned’; hi- is a marker of dependency; i- is an article.

6. Once in a while Harrington wrote these proclitics separately, leaving an obvious space between the proclitic and the following word.

7. This is not to say that one could not make predictions about where this type of hyphen would and would not appear. A good example is seen with the proclitics in the reduplicated words in example (4). One would predict that a hyphen would not occur between hil- and the reduplicated word in hilon-?ontokusnonto? for the following reason: the article, l-, merges phonetically with the glottal stop onset of the reduplicant, thereby becoming glottalized, and is parsed as the onset to the reduplicated form. Since the proclitic merges phonetically with the stem, a post-proclitic pause would be highly unexpected, if not impossible. Thus one would not expect Harrington to write something like *hil-on.. or even hi-lon... (The hyphen that appears after the reduplicant in hilon-?ontokusnonto? serves to mark a syllable boundary between the reduplicant and the base, and is unrelated to the proclitics. This function of hyphenation is further discussed in 4.2).
8. This generalization is more relaxed in fast speech. Further details of Barbareño syllable structure and syllabification are given in Wash 1995. Cf. also footnotes 3 and 4.

9. Interestingly, in Bill Bright's (1952) Barbareño field notes, there is a lexicalized variant of reduplication for the word ‘old woman’ which he recorded from Mary Yee: [ʔɛ·nɛˈnexə·waŋ]. It is quite possible Harrington recorded this variant as well but I just have not come upon it yet.

10. Another difference that is often seen is in the degree of final, emphatic glottalization: the productive variant may show a greater degree of glottalization on the final syllable, but the lexicalized variant may show some or none at all.

11. Example (23) illustrates two trademark features of Barbareño productive reduplication: (1) the coda (final consonant) of the reduplicant is never underlyingly glottalized or aspirated; and, (2) if the base is underlyingly vowel initial, it will get an epenthetic glottal stop onset. For a detailed explanation of why and how this happens, see Wash 1995.

12. Though it is impossible for me to tell at this point whether this is a productive or a lexicalized variant of reduplication, if I had to choose between the two, I would choose the latter. The reduplication in (c) does not appear to have emphatic glottalization, which, as seen in (a) and (b) of example (27), often has the effect of giving emphatic stress and length to the vowel of the final syllable. The lexicalized variant of a reduplicated noun may or may not have emphatic reduplication, but it is extremely unusual for the productive variant not to have emphatic glottalization. For this reason, the reduplication in (c) is probably lexicalized.

Emphatic glottalization is often a secondary consequence of nominal reduplication (the fact that something is reduplicated means that it is, in a sense, given emphasis, hence the emphatic glottalization). In some cases, however, the presence of the emphatic glottal clitic has more to do with the pragmatic use of that reduplication in the context of the narrative, rather than with the fact that the word in question is reduplicated. Example (27b) is a lexicalized variant, and as such would not normally show full emphatic stress and lengthening on the final vowel (both of which are a consequence of the emphatic glottal clitic). However, this particular reduplication appears in a story about a Chumash man who was punished (to death) by having various body parts cut off. Judging from Harrington’s transcription of the story, as Mary Yee tells which body parts were cut off, she uses an emphatic tone of voice for each one.

13. The word in (33) also shows one of the few times that Harrington neglected to hyphenate at a margin.

14. This reduplication is based on the stem sak’alalan ‘to holler, give one holler’. This stem seems to be a lexicalization that involves the classifier prefix ax- ‘of/with the mouth’ and k’al- ‘to loosen up’. Elsewhere in my corpus, another token of the reduplicated form of this word appears as sak’alk’alalan.
References

BEELER, MADISON S. 1954-1961. Tape recordings and unpublished field notes on Barbareño Chumash. [Original recordings are in the language archives at the University of California at Berkeley.]


J. P. Harrington and Al Hayes

Margaret Langdon, UCSD

In May 1977, Mary Haas sent me copies of a number of letters by Harrington expressing his concern about the possibility that Al Hayes, the first fieldworker sponsored by the Survey of California Indian Languages, might be planning to work on Chumash. Mary's letter to me and Harrington's letters to Hayes are reproduced in the following pages. There is also a letter to Prof. Emeneau who was then in charge of the Survey. This is all self-explanatory.

Apparently, Hayes followed one of Harrington's suggestions that he work instead on Diegueño, and spent a field period in San Diego County collecting linguistic material from several speakers. He did not return to the field after that and did not continue in linguistics. He published only one article on Diegueño (Hayes 1954), in which he describes his elicitation tech-
niques and his use of the tape recorder.

He did, however, deposit in the Survey archives some notes and tapes on the language, which Mary Haas allowed me to copy when, in 1963, I decided to record the language. Among the tapes Hayes had collected was a long traditional story told by Rebecca Alto, a very knowl-
dgeable speaker who was a native of the Santa Ysabel Reservation but resided on the Viejas Indian Reservation near Alpine, California, where Hayes met her. He did not attempt any trans-
scription of this tape. Mrs. Alto had died before I worked in Southern California but I was extremely fortunate to be able to transcribe and analyse this story with the assistance of Christina Hutcheson, one of my main Mesa Grande consultants, and two versions of the story are now in print in Langdon 1976 and Hinton and Langdon 1985.

I wish to acknowledge my debt to Harrington for indirectly making it possible for me to work on this text. Harrington's legacy lives on in wondrous ways.

References


Hinton, Leanne and Margaret Langdon (eds). 1985. Diegueño Literature. in Spirit Moun-

Dear Margaret:

You and Shirley may be interested in the enclosed set of letters written by J. P. Harrington. The copies are made by Verifax, an early type of copier. Most are legible except the long letter to Emeneau which I copied off on the typewriter. I found the answers to a couple of illegible items in his letter to Al Hayes of Feb. 10, 1953 so I have made the proper corrections in ink.

I have another Verifax copy of these things but I do not have a copy of my typewritten copy so I would appreciate a Xerox of that.

Hayes was our first field worker after the survey was officially set up and Harrington wanted to run him out of town by any means!!

Love,

Mary
Dear Al Hayes:

I have been here several weeks working with Mrs. Maria de Soto, mostly hearing over again words obtained from her mother. I worked for I do not remember how many weeks with Maria's grandmother when she used to live over on East Ortega St. and took Maria's mother back east for a period of better than six months. Then I worked with her two months here. The granddaughter is not what one would call a good informant, and I have contributed at least half of all the information. They had in the paper absolutely wrong; there are five that speak the language, and not one of them is a real good informant.

So that you won't have to go through all the tortures that I have had to go through on this difficult language, I beg of you to seek fairer fields and let me continue here in peace. There must be at least a hundred languages in California that are crying to be studied and that nobody is working on. Why don't you get one of them, pile up the interesting material, and all glory will be yours? I think of especially the Cehusa language, the San Diego language, the Shasta language, the Achomawi language.

I put Cehusa first because you'll find at Cehusa it is most easy to write up the dialect, get texts, etc.; they still have ceremonies. The Shasta language is a splendid field. Go to Yreka and find some rare informants, especially a man named Welles. The Achomawi language is peculiar in that it has the sound of Semitic b.

I naturally don't want you to work here, if you can possibly help it, but would go to a richer field, and there are many where the ds absolutely no competition. Since your coming here may have entailed bus fares, I will be glad to pay those. I do hope that this letter reaches you and that you'll decide to choose some other field -- or maybe you already have one picked out. It is for your sake as well as mine that I am writing you this truly kind letter, in the hope that you'll find some other field and that I can help you there.

Most sincerely,

[Signature]

Senior Ethnologist,
Bureau of American Ethnology.
Dear Etienneau:

I am writing this to beg you to have Mr. Al Hayes take up his linguistic studies in some other field than this, of which there are many in California in which no linguistic work is in progress, and in which one can still get good informants. I am giving him a hundred dollars to pay for any expense that he may have been put to in coming here. He is a man of my own flesh and blood, born in Maine, and I in Massachusetts, across from the watch factory. He talks excellent German, phonetics, and has a fine training in exotic languages. In spite of all this, owing to my long and uniquely successful work here, I would rather have him go to some other good field, even if I have to help him financially, and ask this for his welfare as well as my own. I was brought here when a small boy, went through the grade schools and the high school here, then went to the University of California and to Stanford University, then spent nearly ten years in Germany, where I took a year of Sanskrit under Winsch and got high marks. I worked with the grandmother of this Maria, who was a really good informant. I took the mother of Maria east, working with her a little more than six months, then worked with her here about two months. In her opinion the present Maria was not worth working with. Yet I have worked with Maria several weeks now, mostly rehearing material that I got from her mother. In this recent work I have contributed half. The paper had it absolutely wrong that Maria is the last speaker. There are five that speak Chumash, of them good informants. I sincerely believe that nothing will make me happy, or will make Mr. Hayes happy after he once gets started, unless he goes to some fairer field in California to make his linguistic studies.

There are many good fields in California. I think at once of San Diego, which is a much larger and more prominent city than this, and where Hayes can get the San Diego language just rattled off, in all its pristine glory. Colusa is a splendid field; the Indians still hold ceremonies there and one can get texts. There is one Ethel Ortega there who talks the language like all get out. Or how about Shasta? The place to get that is at Yreka, California. There is a man named Wells there, who is said to have talent in teaching the language. The Achonawi language on the Pit River is also aching to be worked with. For that one would go to Cassel, California. Please try hard and see what you can do for me, and for the good of Mr. Hayes.

Most sincerely and appreciatively,

Senior Ethnologist,
Smithsonian Institution
Santa Barbara, California,  
Feb. 12, 1953.

Dear Al Hayes:

I am sending you some of the sinews of war to help pay expenses of your renting a house, etc., which you have incurred through no fault of yours. We have of course not yet heard from Emeneau. Am enclosing a copy of my letter to him.

I have thought the matter over carefully, and still believe that it will be better for both of us that you take some other good field, and you can easily find one that is better than this. Even if I have to subsidize you there, I would rather have it that way. That is what I would do if I were you, would go to Colusa, San Diego, Yreka, or some really enticing place.

Most sincerely,

John P. Harrington.
Dear Al Hayes:

I saw the mail man coming and rushed in thinking I was going to get a letter from Emeneau -- and nothing! This beats the band. He evidently hasn't answered us promptly. I want you to go elsewhere for my sake and your sake, too. The informants are too poor and too few to make it easy for you to do here what you propose doing, and I only wish that you could find some better, easier field.

Most sincerely,

John P. H.
Santa Barbara, California,  

Dear Al:

Evidently no letter from Emeneau yesterday. I stayed home all the forenoon expecting you might come around. I sincerely hope that Emeneau will decide to send you to a better, fairer land, one in which you can still get good informants and in which no one is working. I sent you a royal present yesterday, so that you'll be under no loss if you go elsewhere, and would rather finance you materially than have you come here, which would almost surely prove unsatisfactory to both of us in the long run.

Most sincerely,

[Signature]  

John P. Harrington
Santa Barbara, California,
Feb. 16, 1953.

Mr. Al Hayes,
2203 Park Way Drive,
Santa Barbara, California.

Dear Al Hayes:

Your special delivery letter came last night, and I am only too glad to send you a check for $200.00 more. Not only that, but can help you materially. And will. I confidently hope that you can choose some other field, where the informants are good and many, one that has all the advantages of this and more too. We have not yet heard from Dr. Emeneau, but I am sure he will see the matter as I do.

Wishing you the cream of everything, Most sincerely,

J. F. H.
Santa Barbara, California,  
Feb. 17, 1953.

Dear Al:

Your letter is here in which you are returning the hundred dollar check. This is entirely unnecessary. Let us hope that we hear from Emeneau today, but it is very possible we may not. At least I am going over to the street for a while, so if you do get any news you would better call up. Phone here is Santa Barbara 2-8640.

I like to call myself a Santa Barbara boy who has turned linguist. I sure know the country and people around here to a finish. Talk Spanish. I used to doubt whether María was worth working with, but maybe she is. Certainly not without a guiding knowledge of the background.

Most sincerely,

John P. Harrington
Dear Al Hayes:

Your letter of course overjoys me. I hope that you thank Dr. Emeneau personally in my behalf when you see him. The San Diego field is truly a rich and enticing one, and absolutely no one is working in it. You cannot make a mistake in going to Mesa Grande and in starting your work at Mesa Grande, beautiful resort in the mountains back of San Diego. You will find at Mesa Grande dozens of people speaking fluently the San Diego language.

What you say about my not publishing is good advice and well taken. Let’s hope I can do better in the future.

Wishing you the cream of everything and standing ready to help you financially and in other ways, and I mean it,

Most sincerely,

[Signature]
PART II

The Mary R. Haas
Memorial Session
Introduction

The following papers were presented in a special memorial session of the Hokan, Penutian and J. P. Harrington Conference to remember Mary Rosamund Haas, a renowned linguist who, with Murray Emeneau, founded our Department of Linguistics.

This was a remembrance of her intellectual life, by those who knew her in her prime. In keeping with Prof. Haas's own approach to life during her professional years, we felt it appropriate to have this remembrance take place as a session within a conference. This was the afternoon session of the Conference on Hokan and Penutian Languages, a long ongoing annual conference where most of the scholars attending are Mary Haas's own students or the students of her students (or even a generation or two down the line from there).

Mary Haas bequeathed her estate to the three institutions of higher learning where she spent her adult years: first, Earlham College, her undergraduate school; second, Yale University where she received her Ph.D; and third, the University of California at Berkeley, for scholarships for linguistics graduate students. Berkeley has used her inheritance to set up a graduate scholarship gift fund in honor of Professor Haas -- her last gift to the graduate students that she always said were her family.

--Leanne Hinton
Remarks on Mary Haas

Wallace Chafe
University of California at Santa Barbara

Mary Haas was the right person in the right place at the right time. Without her presence in Berkeley in the 1950s and 60s, what we would know now about the languages of California and other parts of North America would be limited to earlier materials which, for all their value, did not come close in comprehensiveness and accuracy to what we now have because of the training and inspiration Mary gave to Haruo Aoki, Richard Applegate, Phil Barker, Jean-Pierre Belan, Bill Bright, Sylvia Broadbent, Cathy Callaghan, Tom Collord, Jim Crawford, Jon Dayley, Brent Galloway, Geoff Gamble, Victor Golla, Bob Hollow, Bill Jacobsen, Kathy Klar, Syd Lamb, Margaret Langdon, Sally McLendon, Wick Miller, Larry Morgan, Julius Moshinski, Bob Oswalt, Cathy O'Connor, Doug Parks, Bruce Pearson, Harvey Pitkin, David Rood, Jesse Sawyer, Alice Schlichter, Alan Shaterian, Bill Shipley, Shirley Silver, Len Talmy, Allan Taylor, Karl Teeter, Kathy Turner, Russ Utlan, Ken Whistler, and Tony Woodbury. (My sincerest apologies to anyone I may have inadvertently omitted.)

Mary's effort began at a time when linguists found their greatest thrills in discovering how different different languages can be. Mary kept that attitude, and was apt to scoff at the very notion of linguistic universals. Perhaps she was a little too unyielding in that respect, but in the present climate it is refreshing to think back on a time and a person for whom linguistic diversity was what really mattered, and who encouraged others to discover as much of that diversity as they could. The subsequent shift to an over-emphasis on how all languages are the same, eclipsing her fascination with diversity, along with a sneering at "mere descriptive" work, gradually put a damper on what she was striving for. But a great deal had been accomplished by that time, and all those grammars, texts, and dictionaries are going to stand forever as monuments to a remarkable person.
Mary Haas and Berkeley Linguistics

M.B. Emeneau
University of California at Berkeley

Mary Rosamund Haas left us on May 17, 1996, after a long illness. At the age of 86 she was one of the last survivors of the generation that produced the flowering of linguistics in this country during and immediately after World War II.

This is not the place or time to expatiate on America's very subsidiary role in the historical, neogrammarian Germany-centered linguistics of the 19th and early 20th centuries; it is sufficient to name Whitney of Yale and Buck of Chicago in this connection. But I cannot omit Berkeley's off-center role as a place where the study of American Indian languages was carried on as a part of anthropological studies and where Alfred Kroeber was a leader in such study.

By the early decades of the 20th century, much of the fire had gone out of the 19th century linguistic efforts in this country. However, though Kroeber's linguistic work in California had almost ceased in favor of more strictly ethnological concerns, a new figure had emerged. At Columbia University, the physicist Franz Boas (Ph.D. Kiel, 1881) had, because of fieldwork as a geographer in the country of the Eskimos and Kwakiutl, become a full-fledged anthropologist. This intense interest in the culture of non-literate ' primitives' led straightway into the recording and analyzing of linguistic texts. This all-embracing anthropology was what he taught at Columbia.

Among his linguistic pupils was Edward Sapir, whose career included finally a professorship at Chicago (1925-1931) and then a Sterling professorship at Yale (1931-1939). Sapir taught and brought on to the doctorate a rather small, but very select, group of pupils. At Mary's death, this group has now completely passed away. My own claim to be the last surviving member of the group of Sapir's pupils rests not on my doctorate in Sanskrit in 1931, but on my having attended some of Sapir's lecture courses in linguistics after 1931.

Here perhaps I may digress a little. In India there is a playful, but still serious, habit of tracing the succession of teachers and pupils in a form called (in Sanskrit) the guru-sisya-paramparā 'the succession of pupils of a teacher.' As a teacher of linguistics as it centers on India I am given a place in such a succession. The habit will bear transplanting into American Indian linguistics: Boas is the original guru in that paramparā (since he had no teacher in linguistics) and his pupil is Sapir, and Mary was Sapir's pupil. Many in Mary's memorial gathering are Mary's sisyas and their sisyas in succession. The line of teachers and pupils centered on Berkeley forms a perfect guru-sisya-paramparā.

Mary's linguistic training started, as did that of so many others in that period, in the study of Indo-European comparative grammar, first at Chicago and then at Yale. When Sapir moved to Yale in 1931, she followed him there, continued linguistic training in a broad way, and in 1932 began her fieldwork on the American Indian languages of the Southeastern USA. This culminated first in her Ph.D. (1935) on the Tunica language of Louisiana and later in her all-embracing recording and reconstructing of the Muskogean language family and other languages of that area, an important part of the legacy she has left behind.

When the linguistics community of this country was coopted into the military effort of World War II, from 1941 on, Mary worked on the language of Thailand, first at the University of Michigan, then in the Army Specialized Training Program at Berkeley. This work on Thai
continued after the war in a lectureship at Berkeley (1945), combined with general linguistics teaching. In the general cut-throat atmosphere in the linguistics world after the war, Berkeley's interest in Thai lost out to that at Michigan. However, Berkeley's great post-war expansion, due in large part to the GI Bill and to California's population explosion, included a great increase in interest in linguistics, and (thanks in part to Kroeber's long-continuing concern) especially in the recording and study of the California Indian languages. A special, non-departmental program in linguistics was established, through which William Bright earned his B.A. in 1949 (Ph.D. 1955). Mary was appointed Assistant Professor of Oriental Languages in 1948.

The general national expansion in linguistics already referred to as 'cut-throat' led in 1953 to Berkeley's establishment of a linguistics department, of which of course Mary was one of the founding members as Associate Professor (Professor in 1957). Her devoted work in the running of the department, the teaching of students, and the training of the long succession of graduate students who won Berkeley degrees continued until her retirement in 1977. She was one of the notable gurus in the succession of linguistics teachers in this country in this century.

There is another feature in Mary's career which needs emphatic mention. The intense national competition which led to the department's establishment in 1953 led at the same time to those concerned (especially Kroeber, Mary Haas, and myself) stressing to the university administration that a survey of California Indian languages should be set up, to be conducted through the department and funded in such a way that trained and interested students should be enabled to make field trips to record these languages, especially those in danger of near extinction, and to analyze them with a view to using them for degree dissertations and eventual publication. The administration agreed, the funding was provided (and has continued ever since), and has yielded in the neighborhood of 40 (or is it many more?) published monographs and many papers and articles, and the survey still continues (now as the Survey of California and Other Indian Languages) to yield most valuable linguistic material. As the department's most notable specialist in the geographically delimited subfield, Mary was of course the ideal person to plan and administer the survey -- si requiris monumentum, circumspice 'if you wish to see her monument, look at UCPL.' Few of us achieve such a monument.

It hardly needs saying that Mary herself did fieldwork in the California languages and used her great knowledge of the North American Indian languages as well as the results of others' work in the Survey to add much that was both original and definitive to our knowledge of these languages and their history. Not being myself a specialist in this field, I can hardly speak profitably of her work in detail. However, I should say that I find most spectacular her demonstration (1958) that two of the California languages, Yurok and Wiyot, were indeed, as Sapir had suggested, closely related to the Algonquian languages of the northeastern and north central parts of the continent. Mary's work will always occupy a central and preeminent place in the linguistics of North America.

Mary's contribution to the university included of course the usual stints of administrative work -- chairman of the department for the statutory period, member and chairman of the committee for the Faculty Research Lecture (her lecture was in 1964-5), co-founder and director of the Language Laboratory. Upon her retirement in 1977 she received the Berkeley Citation, which is the equivalent of an honorary degree (for it is many years since such a degree has been given at Berkeley). In 1984 she was elected a Berkeley Fellow.

The lengthy list of her honors is in your hands.

And so, goodbye, admiringly and affectionately, to Mary, colleague and teacher of us all.
Mary Haas as a teacher

Margaret Langdon, UCSD

This will of necessity be a personal rather than a general statement, for I am sure each one of Mary Haas's students has his or her own special memories of her teaching, but I suspect that much of what I say will resonate with others and I also hope it will generate more thoughts on the subject.

First of all, let me place my experience in time. I was a student at Berkeley officially from 1959 to 1966, which included two semesters of field work and one year at UCSD. Unlike many of Mary's students, I started as an undergraduate major and I believe I was one of the first undergraduates admitted in the then new undergraduate linguistics program for which Bill Shipley was the undergraduate advisor. I took several courses from her: Introduction to American Indian Languages, Field Methods, a seminar on theoretical linguistics and a field workers' seminar. And of course I wrote my dissertation under her direction, although the final version was written in San Diego and comments from her were only in the form of correspondence.

I have had many wonderful teachers at Berkeley. Some were superb lecturers, some were beautifully organized, some were charismatic, some were demanding and critical. In comparison, Mary was not really any of those things. Her style in the classroom was always low key, perhaps a little aloof, the material she presented was to me at least quite overwhelming, especially the Introduction to American Indian Languages where we were to memorize all the North American language names as well as their classification, of which I despaired since I had had no previous exposure to the topic at all. But she also presented data from her own research, in particular Algonkian and Muskogean material; she wrote the relevant forms on the board in her beautiful hand and we copied it frantically. Remember this was before xeroxing, let alone computers. Classic readings such as Boas's Introduction to the Handbook of American Indian Languages were available on reserve at the library, and their content internalized during many long hours of note-taking; there was no textbook. In classroom presentations, wedged inconspicuously among the details of a problem would suddenly be found an observation so profound as to leave me breathless, though it could easily have been missed.

In my experience as an undergraduate, there was rather little personal contact with the faculty. I did my work, turned in the assignments, got a grade. On Mary's part, there was very little hand-holding or praise, which made the very rare low-key praise all the more exhilarating. We were there to learn and we did. The true impact of her teaching did not crystallize for me until some years later, when I would occasionally enjoy what I thought was a deep insight only to remember that I had learned it from her and rediscovered it.

Her overriding concern as a teacher was to impart to her students the fundamental principles and methodology of linguistic analysis and a deep respect for language and languages. This she did mostly by example, demonstrating through her own research results. She shared her love of
language, its diversity, its universality, the complex and structured ways in which it reflects people's thoughts and varied cultures. She insisted on the mastery of the basic skills of analysis and scholarship and she shared with us the excitement of comparative/historical work. Later, she warned against the mistakes of the untrained fieldworker and the blind acceptance of fashionable new claims. But mostly she treated us as adults and as future scholars. Beyond the fundamentals, she never attempted to force her views on us. We were supposed to develop our own conceptions of the field and styles of research. Haas students are not poured out of a mold, they go their own way in full confidence that their training will be relevant.

I came to realize all this long after leaving Berkeley and found myself almost unconsciously trying to incorporate her approach into my own teaching style. As I struggled to report my own research in print, I also came to admire more and more the clarity and simplicity of her writing, some of which should be required reading for all linguists. I also learned to know Mary better and to enjoy her friendship. Particularly wonderful was the Winter quarter of 1977 which she as well as Shirley Silver spent in residence at UCSD. It was then I finally understood that her students were her intellectual family and I believe each and everyone of us has experienced this kinship, and the fierce loyalty we all felt to her. She lives on in her descendants, students, grandstudents, great-grandstudents, and so on. We must make sure they take pride in their heritage.

In conclusion I'd like to share with you specific illustrations of some of the points I made earlier. On going into the field, she warned: "No unanalyzed texts!" On writing a dissertation: "A dissertation is meant to be a piece of original research. It's your dissertation. I'm not going to tell you how to do it." On the question of praise: I wrote a paper once long after leaving Berkeley. Mary read it and sent me a note which said: "I think you're on to something." What more could anyone ask for?
Mary Haas as a Historical Linguist

William Shipley
University of California at Santa Cruz

I would like to talk about Mary's brilliant contributions to historical linguistics. Not only did she do outstanding work in proto-language reconstruction, she also, in her later years, wrote more extensively and coherently on the general subject of the prehistory of Native American languages than any other Americanist.

Mary got her bachelor's degree at Earlham College in her home town of Richmond, Indiana, with a major in music. She has told us that she was browsing at random in the college library one afternoon in spring when she came upon Henry Sweet's seminal work on phonetics as well as a book on comparative phonology, the name of which she could not, later, remember. In her 1984 Hoskins lecture for the American Council of Learned Societies, she recalled that time as a kind of epiphany. She said: "I sat by a window, looking out on the beautiful green of the campus, read these books, and quite unexpectedly found the key to my own career of learning." This event was remarkably heuristic since two main areas of linguistics studies in which she came wonderfully to excel were articulatory phonetics and historical reconstruction. Interestingly enough, even before her conscious awareness of how fundamental her interest in language actually was, she had already studied Latin for seven years, as well as some Greek and Anglo-Saxon.

As we all know, Mary started her graduate career at Chicago, having first gotten a second bachelor's degree there. She soon thereafter followed her great mentor, Edward Sapir, to Yale, where she got her doctorate in 1935, with a dissertation on the Tunica language. A couple of years later, she was provided with funds to work on Natchez in Eastern Oklahoma. As it turned out, as her Natchez informant knew hardly any English, but did know Creek, she had to use a Creek speaker who also spoke fluent English as an intermediary.

She said, in her Haskins lecture: "In the period from 1935 to 1941, I collected a very large amount of material on Creek, some on Koasati, a few vocabularies of Choctaw, Alabama, and Hitchiti (which has since become extinct)."

These, of course, were all Muskogean languages, originally spoken in what is now the Southeastern United States. I'll not go into any of the details about them since Pamela Munro's much more knowledgeable thoughts on Mary's work with Muskogean and its possible extended genetic relationships is also included in this volume.

Another of Mary's dramatic and pivotal contributions to American Indian historical linguistics had to do with the famous Sapir-Michaelson controversy over Sapir's proposal, in 1913, that Yurok and Wiyot are related to Algonkian -- a proposal which Michaelson vehemently rejected. The matter remained unresolved for half a century until, in the late 1950's, Mary brought together a brilliantly analyzed set of nearly a hundred etymologies, providing totally convincing consonantal correspondences as well as insightful proposals on vocalization and morphology, thus clearly establishing a genetic relationship among Yurok, Wiyot and Proto-Algonkian. I would like to point out here that Mary's 1958 paper adumbrated an observation which she made in her distinguished monograph, "The
Prehistory of Languages," published ten years later: "The most challenging way in which new insight into reconstruction can be achieved comes about when one protolanguage is compared with another protolanguage, or, as often happens, when a protolanguage is compared with a single language lacking near relatives. This type of comparison can truly be said to be one of the most important new frontiers of historical and comparative linguistics."

In fact, Yurok and Wiyot are more or less distant from one another as they are from Proto-Algonkian; thus her second alternative applies in this specific case.

In addition to various research papers on comparative issues, Mary published two seminal monographs on the general topic of American Indian genetic relationships. Let me first mention her long article which appeared in volume 10 of *Current Trends in Linguistics* entitled "American Indian Linguistic Prehistory." This work is a very detailed and circumstantial review of all the work which had been done on Native American linguistic relationships up to about 1970, covering more than a century of various attempts, successful and unsuccessful, to achieve some kind of classificatory order among the welter of North American languages.

Mary wrote a more interesting monograph a few years earlier which was published as No. 57 of the *Jama Linguarum* series minor, with the title: "The Prehistory of Languages." This immensely interesting and carefully written work, revised from an earlier essay, resulted from her year as a Fellow in residence at the Center for Advanced Study in the Behavioural Sciences. With its fascinating and detailed essays on various aspects of genetic research combined with presentations of very sophisticated analyses of her own voluminous historical data, this monograph is a beautiful distillation of her vast and varied experiences in historical linguistic research.

At the end of her Haskins lecture, Mary put into her characteristically plain words just what, for her, a life of learning was all about. She said: "It is a life for enthusiasm for learning, for the pursuit of goals which can never be reached, at least not in one lifetime. But the joy, after all, comes in the pursuit."
Remembering Mary Haas' s Work on Thai

James A. Matisoff
University of California, Berkeley

Mary R. Haas belonged to the heroic generation that established the study of Southeast Asian languages and linguistics in the United States. Before World War II the region had been virtually the exclusive domain of scholars from the European countries that had colonized it politically -- Britain, France, and the Netherlands. Hardly a soul in the USA knew anything about the rich profusion of languages and cultures of Indochina, Thailand, Burma, or the Indonesian archipelago. With Japan's incursions into Southeast Asia in the 1940's, some knowledge of the languages of the area came to be viewed as essential to the war effort. The nation's linguists were recruited to study Far Eastern languages, and ordered to produce practical handbooks, teaching grammars and vocabularies, as quickly as possible.

How well they succeeded in this enterprise is now a matter of historical record. The brightest stars in that new constellation of Orientalists included such scholars as William S. CORYN, who was assigned Burmese, and ended up at Yale with the ecumenical title of "Professor of Slavic and Burmese";¹ and Murray E. EMENEAU, the eminent Sanskritist and Dravidianist, who was channeled into the study of Vietnamese, and eventually published the first great grammar of that language to appear in English.² Emeneau, as of this writing still going strong at age 93, was co-founder of the Berkeley Linguistics Department, along with Mary Haas, in 1953.³

To Mary Haas fell the task of describing the national language of the only country of Southeast Asia that had escaped colonization, Thailand. Given the near total dearth of teaching materials on Thai in those days, Haas, like Coryn and Emeneau, had to learn her language from scratch, through direct elicitation from native speakers. This was no big problem for her, since she had merely to apply the classic fieldwork techniques honed to such perfection in her Amerindian work to this new language of utterly different phonological and grammatical structure: from the Southeast United States, where she had worked on Tunica and Natchez, to Southeast Asia -- an effortless intellectual leap.

Mary Haas eventually became one of the leading Thai specialists in the world outside of Thailand, taking her place in a select group that included three other towering scholars of her generation. Needless to say, each of these four possessed unique strengths and pursued complementary interests. The late André-Georges HAUDRICOURT was a quintessential French scholar of the old school, a botanist and

²See Emeneau 1951.
³Emeneau and Haas had previously held positions in Berkeley's Oriental Languages Department (which has recently changed its name to "East Asian Languages").
theoriciant of diachronic linguistics. Not a fieldworker, he was content to sit in his cluttered apartment and make brilliant deductions (often on the basis of crudely recorded old data) about the phonological history of all the language families of Southeast Asia, among them Tai.4 The career of Li Fang Kuei followed a curiously similar trajectory to that of Haas in some respects. Like her, he was a student of Sapir, and was trained in Amerindian linguistics. He applied Western fieldwork techniques to his meticulous recording of the Tai languages and dialects of China, culminating in his reconstruction of Proto-Tai (1977). He succeeded in demonstrating the nature of the relationship between Tai proper and its closest kin, the Kam-Sui languages (1965). In China today he is perhaps most famous for having developed an influential new system of reconstruction for Old Chinese. Along with Y.R. Chao, he must be reckoned one of the greatest Chinese linguists of the 20th century. William J. Gedney is the most Thaiicized scholar in this group. Perfectly fluent in spoken Siamese, he carried out extensive fieldwork on Tai dialects in the 1950's and 1960's in remote corners of Thailand, Laos, Cambodia, and Vietnam, as well as in Hong Kong and Taiwan, discovering such wonders as the Saek language of Nakhon Phanom province, which alone among all known Tai dialects preserves Proto-Tai final *-I.5

Mary Haas made lasting contributions to Thai studies in five areas in which these other scholars barely touched upon:6

(1) Thai language teaching

Building on the pedagogical materials she had assembled during and after the War years (Haas 1942a, 1945a, 1945b, 1954, 1956), Haas taught Thai in the Berkeley Oriental Languages Department from 1947 to 1960. Her book Spoken Thai (1945-48), co-authored with her then husband, Heng R. Subhanka, was the culmination of this early work, and constituted the high-water mark of the Holt “Spoken Language Series”.7 With hindsight it is easy to criticize Spoken Thai on the grounds that the style of its dialogues is sometimes unnaturally formal and polite. Thus the English sentence “Where are you going?” is rendered by khun kamlaŋ câ? pając náj khrâb, with the honorific pronoun khun, the polite masculine final particle khrâb, and the Khmer-derived progressive modal auxiliary kamlaŋ câ? be...ING; though in most contexts of actual usage one is far more likely to hear simply pając náj (literally “Go where?”). Yet it was essential to sensitize the American student to the fact that Thai is a language with highly codified levels of politeness based on such factors as age, status, and gender. It is just as unfair to fault Spoken Thai for over-formality as it would be to attack it for the traditionally practical nature of the content of its lessons (e.g. The Bank, The Post Office, The Doctor). It behooves us rather to appreciate this pioneering book

---

4See, e.g. Haudricourt 1948, 1956. Following accepted usage, I use the spelling “Thai” for Siamese (the national language of Thailand) and “Tai” for the language family to which Siamese belongs.
5A good anthology of Gedney’s most influential articles was published in 1989. See the review by Mattisoff (1993).
6For a complete bibliography of Haas’ publications on Thai see Huffman 1986, pp. 164-5.
7This book was my own introduction to Thai. I remember devouring it from cover to cover during the summer of 1964, before leaving for my first fieldtrip in Thailand.
for its manifold excellences: the clarity and accuracy of its grammatical notes, and the
insight displayed in the organization of its drills and pattern practices.

(2) *Synchonic phonology*

Haas’ analysis of Thai phonology has stood the test of time. Her elegant
phonemic transcription (including her diacritical marks for the tones) was accepted as
standard for decades, and even today has only undergone minor modifications (mostly
for the worse) by one writer or another. A few points are of special interest:

- She uses /y/ as the symbol for the palatal semivowel, thus freeing up /y/ for the high
central vowel [i]. This in turn provides a simple way to transcribe the three
centralizing diphthongs /ia ya uə/.\(^8\)

- She transcribes the low front vowel as /e/, even though it is phonetically closer to [æ].
Since everyone transcribes Thai long vowels by writing the vowel twice, it is
esthetically preferable to have /ee/ rather than the unsightly tetragraph /ææ/.\(^9\)

- Haas writes the single series of Thai postvocalic stops (phonetically unreleased) with
the voiced symbols -b -d -g, even though Thai lacks initial g-, and despite the fact that
a voicing contrast in final stops is almost unheard of in Southeast Asian languages.
This is one point where almost everyone has overruled Haas, and now the transcription
/-p -t -k/ is universal. Yet Haas stuck to her guns here, and I have heard her spiritedly
offer several arguments to buttress her position. For one thing, she maintained that
these final voiced symbols obviate the necessity for using hyphens to show syllable
boundary in binomes like riabrosj ‘well-groomed; polite’ or luuglaj ‘person picked on
by others’: since Thai lacks initial clusters with voiced stops (and lacks initial g-
together), but does have clusters with voiceless unaspirated p- and k-, spellings like
rīaprōj or lūuklāj would be ambiguous (rī-aprōj or rīap-rōj; lūu-klāj or lūuk-lāj).
Furthermore, Haas claimed that when informants are pressed to repeat pronunciations
of words with final stops, when they are irritated beyond endurance they will finally
voice them: "tāak...tāak...tāak -- oh, all right, goddammit, tāa-G !"\(^9\)

- Haas was adamant (rightly I believe) about always transcribing initial glottal stop,
even though it is automatic before an initial vowel that is not preceded by any other
consonant, on the grounds that ‘once a phoneme always a phoneme’. In syllables with
short vowels, in fact, -ʔ does behave exactly like -p -t -k in terms of what tones it may
appear under, so at least in that position it certainly patterns like the oral stops. Unlike
the three oral stops, however, -ʔ cannot occur after a long vowel, so that it is really *sui
generis* in Thai phonology.

- Since all vowels in stressed open syllables are long (e.g. taa), while all short vowels
not followed by an oral consonant are automatically followed by -ʔ (e.g. taʔ), a strictly
taxonomic approach would phonemize the contrast either as ta : taʔ or taa : ta . To

---

\(^8\)This works better than hugging the phonetic ground too closely with, e.g. /iə er uə/.

\(^9\)Lest these arguments seem a bit arcane, it should be pointed out in Haas’ defense
that the devisers of the Tibetan writing system (6th-7th cc. A.D.) also chose voiced
devanāgarī symbols to write their single series of final stops.
Haas's credit, she decided to introduce a bit of redundancy into her transcription, and wrote the contrast as /tæː tæ/. Besides its practical mnemonic value (the learner does not have to remember either the 'vowel lengthening rule' or the 'glottal-stop insertion rule'), this has the advantage of allowing Haas to distinguish a third type of syllable: unstressed, often toneless syllables with short vowels, written with a single vowel but no glottal stop (e.g. /tæ/), as in talâd [tâːlət] < Khmer.  

To my knowledge, Haas was the first to describe the allophonic nasalization that occurs in Siamese syllables with laryngeal initials (ʔ- and b-) and low vowels, especially -a, as in bāa [bâːN] 'five', tɔaw ['tɔːwN] 'take'.

Although Haas never focussed primarily on comparative Tai phonology (in the sense of Haudricourt, Li, or Gedney), even here she made pioneering contributions, as in her comparison of the tones of Standard Thai to those of the dialects of Nakhon Srithammarat, Roi-et, and Chiengmai (Haas 1958).

(3) Thai grammar and sociolinguistics

Haas was among the first to describe the syntax and semantics of numeral classifiers in Southeast Asian languages, both for Thai (Haas 1942b) and for Burmese (Haas 1951a). She was particularly interested in Thai techniques of word-formation, such as reduplication (Haas 1942c), intensification (Haas 1946), and "elaboration". It is to Haas that we owe the felicitous term "elaborate expressions" to characterize these innumerable four-syllable constructions (usually with repeated syllables, ablaut and/or alliteration) that abound in the more elevated styles of Southeast Asian discourse, e.g. nām-hūu-nāam-taa 'tears' (lit. "water-ear-water-eye"), semantically equivalent to the ordinary compound nāam-taa. Haas succinctly discusses these expressions in the memorable "Brief Description of Thai" that serves as a preface to her Thai-English Student's Dictionary (see below). This Preface itself constitutes the best capsule account of Thai morphology ever written.

Haas's anthropological background led her to pay special attention to Thai linguistic phenomena that directly reflect aspects of Thai society and culture. It was in this line of research that she gave relatively free rein to the more humorous, even racy, side of her personality. In "Interlingual word taboos" (1951b), she discusses the titillative malaise felt by Thai-English bilinguals when pronouncing innocent Thai words that fortuitously resemble naughty words in English (e.g. phřag 'chili-pepper'; fāag 'deposit, put down'). In "Thai word games" (1957), she describes how speakers intentionally mutilate the phonological structure of disyllabic collocations for comic effect, often by a kind of spoonerism whereby the initial consonants remain intact while the vowels and tones of the syllables get switched, e.g. běn mǐi 'see a bear' > hǐi měn

---

10 For more on these unstressed syllables in "sesquisyllabic" words, see the section on Lexicography, below.
11 I subsequently dubbed this phenomenon (which is actually fairly widespread in the world's languages) rhinolottophilia (Matisoff 1975).
12 These are also highly characteristic of Chinese, where they are known as chengyu.
'pudendum muliebre odoriferum'. This word-play is actually of great interest, in terms of figuring out how native speakers parse the elements of their syllables. In "Sibling terms as used by marriage partners" (1969), Haas explores the complex realm of Thai terms of address within the family, where couples often start by addressing each other as if they were siblings; then, after having children, they may settle into comfortable teknomyony, addressing each other as 'father' / 'mother'.

(4) Thai writing

Haas's *The Thai System of Writing* (1956) is far and away the best treatment of the subject in English (or any other non-Thai language). Beautifully clear and systematic, but without burdening the learner with historical explanations for the synchronic complexities, this is the indispensable introduction to the Thai writing system.

(5) Thai lexicography

In my opinion, Haas's crowning achievement in Thai studies is her wonderful *Thai-English Student's Dictionary* (1964). After the elegant grammatical sketch in the front matter (pp. xi-xxii; see above), the body of the Dictionary (pp. 1-638) is presented in the Thai alphabetical order. Every entry is painstakingly crafted, with absolute consistency of format. Besides the Thai spelling and the phonemic transcription, each lemma is provided with a form-class designation, and many include information on stylistic level, synonyms, and/or antonyms; all nouns have their appropriate classifier(s) indicated. The glosses are clear and crisp, natural and unstilted, often with three or four English equivalents to delineate the precise range of meaning. The lemmata are richly illustrated by examples and subentries. Even non-initial bound syllables in compounds appear in their proper alphabetical place as head entries, rather than being swept under the rug. The sin of "pernicious interalphabetization" committed in all too many dictionaries of Southeast Asian languages, whereby collocations involving homophonous morphemes are interalphabetized in a single list regardless of their morphemic identity, is rigorously avoided: every collocation appears under its proper head-entry.

An important feature of the *Thai-English Student's Dictionary* is the indication of stress for every entry. Although Thai is basically monomorphosyllabic (i.e. in native vocabulary the syllable and the morpheme are roughly coextensive), the language has innumerable binomial compounds and collocations, as well as many

---

13Haas produced a similar article on Burmese ("Burmese disguised speech" 1969). Her involvement in Burmese was more than casual throughout her career. She was particularly interested in Thai/Burmese contact vocabulary (e.g. 'elephant': Thai châaŋ, Written Burmese châŋ [Mod. Bs. hsö]), Perhaps influenced by R. Shafer's inclusion of the Tai family in Sino-Tibetan, Haas seems actually to have flirted with the notion of some kind of genetic relationship between Thai and Burmese.

14An extended study of pronominal reference in three key Southeast Asian languages was published at about the same time as this article by one of Haas's students, Joseph Cooke (1968).
“sesquisyllabic”\textsuperscript{15} or polysyllabic words of Khmer or Indic origin that contain unstressed vowels, so that the phonological texture of the language is very different from that of uncompromisingly monosyllabic languages like Chinese and Vietnamese. The stress patterns of stretches of speech larger than the monosyllable are independent of the tones of the individual syllables, and Haas insisted on carefully marking both tone and stress for every entry and subentry. This may be illustrated by part of the interminably omate official name for the city of Bangkok (called simply \textit{krunth\text{\textae}eb} in ordinary language):

\begin{center}
\textit{krunth\text{\textae}eb} \cdot \textit{ph\text{\textae}mah\text{\textaa}} \cdot \textit{n\text{\textae}khoon} \cdot \textit{bowcoon} \cdot \textit{r\text{\texta}dtaan\text{\textkoo}} \cdot \textit{s\text{\texti}n} \cdot \textit{mah\text{\texti}n} \cdot \textit{thar\text{\textaa}} \cdot \textit{judthajaa} \cdot \textit{mah\text{\textaa}} \cdot \textit{dil\text{\textog}} \cdot \textit{phi\text{\textph\textob}} \cdot \textit{n\text{\texti}b} \cdot \textit{phar\text{\textad}} \cdot \textit{r\text{\texta}d} \cdot \textit{chath\text{\textaa}} \cdot \textit{nii} \cdot \textit{bur\text{\textii}} \cdot \textit{rom} \cdot \textit{\text{\textudom}} \cdot \textit{s\text{\texta}ntis\text{\texti}g} \cdot \textit{TESD}, p. 15
\end{center}

The 21 stressed syllables of this 42-syllable utterance are indicated by the posted symbol \textbullet\ (substituting for the accent marks in the \textit{TESD}).

***

When I entered the Berkeley linguistics department as a graduate student in the fall of 1962, Haas was Chair, and her influence on the departmental ethos was pervasive. I was somehow imbued with such radical Haasian notions as that to really do right by one’s language of study, one had to produce a grammar, dictionary, and collection of texts for it. Although I never actually took a course from Haas for credit, my whole academic life was crucially influenced by her. It was she who steered me into Southeast Asian linguistics. She was contacted early in 1963 by a young anthropology student at the University of Arizona, an ethnic Jingpho (Kachin) from northern Burma named LaRaw Maran, who told her that he wanted to work on his language with a linguist that summer. Miss Haas knew that I was interested in Japanese and Chinese, and judging that Jingpho was close enough, arranged for me to be the one to work with him. I was eventually offered a Fulbright to do fieldwork on Jingpho in Burma for 1965-66, but all foreigners were kicked out of Burma in a wave of rabid xenophobia by the end of 1964. Again Miss Haas decisively intervened, and suggested that I change my Fulbright destination to Thailand. There was a lovely city in northern Thailand, she told me, called Chiangmai, where I would have access to speakers of many minority languages. Following this excellent advice, and no doubt with the help of a covering letter from Miss Haas, I got my change of venue, went through her \textit{Spoken Thai} again, and took off for Thailand with wife and nine-month-old daughter, with no very definite idea of what language I would be working on. Such was the encouragement given to students of exotic languages in those post-Sputnik days, that I got away with filling in the “Language of Study” box on the revised application form with “Miao, Yao, Lahu, and/or Wa” -- the only minority languages of northern Thailand that I had heard of up to that point.

One of my favorite memories of Mary Haas dates from some years later, right after the Sixth International Conference on Sino-Tibetan Languages and Linguistics,

\textsuperscript{15}These words that are “a syllable and a half” in length, consisting of an unstressed prefixal or “minor” syllable followed by a fully stressed “major” syllable, are especially characteristic of the Mon-Khmer family, but many Tibeto-Burman languages (e.g. Burmese and Jingpho) have a similar profusion of words of this type. The term “sesquisyllable” was introduced in Matisoff 1972.
held at the University of California at San Diego in October 1973. I had purchased a large handsome painted plaster of Paris Buddha statue in Tijuana for something under two dollars. Mary and I were seated next to each other on the plane back to Oakland, the Buddha statue on my lap. The flight turned out to be horrendously turbulent, and free cocktails were distributed to take the passengers' minds off their possibly imminent demise. Mary and I each had several. When at length we landed safely, not a few passengers came up to thank the Buddha for his help and protection. I had never seen Miss Haas as jolly as she was at that moment, demonstrating the proper way to make obeisance.

References


----------------------------- 1942c. "Types of reduplication in Thai" (SiL 1.4:1-6).


1956b. *Brief Description of Thai, with Sample Texts.* Outline for Types of Linguistic Structure, University of California, Berkeley. 27 pp. mimeo.


The Contribution of Mary R. Haas to the Study of Southeastern Languages

Pamela Munro
UCLA

I am honored to have been asked to discuss Mary Haas's contributions to the study of the indigenous languages of the Southeastern United States for this volume, in which we celebrate Mary Haas's life while mourning her passing. Miss Haas's contributions to this area were vast, in terms both of her coverage of many languages from many different genetic groups and of the subject matter of her work, which ranged from lexical studies to text collections to synchronic grammar, from historical phonology to syntax, from anthropological linguistics to classification.

I first met Mary Haas, or at least saw her from a respectful distance, at the first Hakan Conference in 1970 at UCSD, when I was a first-year graduate student and Margaret Langdon's research assistant. Although at the time I had not yet worked on any of the many languages Miss Haas wrote about, I had read some of her work on linguistic prehistory as an undergraduate, so I understood something of her importance, and I realized that it was her influence that had helped to make Margaret so different from all the other professors in our department ~ so concerned at every level with the accurate recording and preservation of real language data, and with the proper analysis and presentation of it, traits that mark both women's work, which they have passed on to generations of their students.

I heard Miss Haas speak several times about Muskogean in the mid-seventies: the two papers I recall best were presented at the Conference on Mechanisms of Syntactic Change organized by Charles Li in Santa Barbara and at Larry Hyman's Stress Fest at USC. Although I had not then studied any Muskogean language myself, I was greatly impressed by these presentations. Here was a female professor presenting fascinating new historical and synchronic research on very complex data, holding the interest of audiences often composed of people thirty years or so younger than her -- what a role model!

The two papers just referred to are excellent examples of how Haas's contributions continued unabated throughout the whole of her long career. She began studying the Muskogean language Creek (or, as she often referred to it, Muskogee) in the 1930s, and it served as the cornerstone for much of her later research. Haas's Stress Fest paper was a detailed description of the complex tonal accent system of Creek. At the meeting, she presented the analysis she had developed after years of working with Creek data: the manuscript of her Creek dictionary reveals the evolution of this analysis and its orthographic representation, with superceded forms carefully corrected and updated. The paper occasioned considerable discussion in the audience, and the final published paper incorporating this is a highly sophisticated product.

The facts of Creek data were also important for the Syntactic Change paper. As one of the few scholars privileged to have had some exposure to all branches of Muskogean, Haas was able to show convincingly how the complex, multi-layered system of verb conjugation in the Koasati and Alabama languages was related to simpler systems in Creek (on the one hand) and Choctaw and Chickasaw (on the other). Once again, she related her own data to the theoretical concerns of interest to others at the conference, arguing that the various Muskogean inflectional paradigms (I can hear her saying "PAR-a-dim" right now) had evolved from an earlier system with unmarked main and inflected auxiliary verbs ~ a system reminiscent of that I had just discovered in my own dissertation research on the Yuman language Mojave, and consequently of great interest to me.
A few years after I heard these two papers, I coincidentally began my own study of Chickasaw (followed later by Choctaw and Creek-Seminole), and then I truly understood the depth and importance of Miss Haas's contribution to Muskogean studies. Really, Mary Haas is Muskogean: any bibliography of works on the language family must rely heavily on her work. Here's an example: I am involved in a joint project to assemble Muskogean cognate sets with representatives from as many languages of the family as possible. While we have found a large number of new sets by now, the first draft of this project mainly consisted of a collection I made of all the different Muskogean sets Haas had discussed in different works. This was a very large number of sets indeed. Remarkably, compared with most other historical phonologists who work on a relatively poorly described family, she considered entirely different cognate sets and reconstructions in almost everything she wrote, so when these are all put together in one place, they almost make up a book by themselves. (This is especially noteworthy when one looks at the length of typical papers by Miss Haas. For one whose writing was almost always exceptionally clear and readable, she had an amazing gift for being brief.)

The most important sets, of course, come from Haas's paper on the classification of the Muskogean languages, and the closely related papers on Proto-Muskogean *kw, the position of Apalachee, and the relationship of Natchez and Muskogean. Although I came to disagree with the classification she argued for (which in fact she later withdrew from slightly), I value the clarity of the argumentation and the data presented in these four papers almost above any of her other work. When I teach historical linguistics, I always use a Muskogean problem that invites students to propose Haas's reconstructions and classification, and they are always struck by its elegance.

As I mentioned, Haas worked extensively on Creek, which I always felt was her favorite Southeastern language, but she also did extensive fieldwork on other languages of the region, especially the Muskogean languages Koasati and Hitchiti and the isolates Natchez and Tunica, the last of which (topic of her own dissertation research) is the subject of her most extensive published descriptions, providing "a grammar, a dictionary, and a body of texts". Her published and unpublished writings do not stop with these languages, however: she also wrote on the Southeastern languages Biloxi, Cherokee, Chitimacha, Choctaw, Ofo, Tonkawa, Yuchi, and the Muskogean-based trade language Mobilian, in most cases citing at least some data from her own fieldwork. Karen Booker's linguistic bibliography of the Aboriginal Southeast lists fifty-six books and papers by Haas, most of them extremely important for any student of the linguistics of the region, and covering all its diverse genetic groups. They deal not only with single languages, but also with much larger comparative issues.

Haas's work on the Southeast provided material for one of her deepest interests, the search for ever larger and deeper genetic groupings of American Indian languages. In this, she clearly followed the lead of her teacher Edward Sapir (though with more specific data and argumentation!), proving to be very influential "surely contributing, for example, to the reshaping of Macro-Algonkian in the revised (and still widely accepted) classification presented by the Voelgels in the 1960s. These days we do not usually think of Algonkian as connected with the Southeast, but Haas persuaded many linguists that Algonkian was related to the Gulf languages, a loose grouping consisting of Atakapa, Chitimacha, Muskogean, Natchez, and Tunica that she supported convincingly in many writings. She also wrote on broad comparative topics in Siouan and Hokan-Coahuiltecan, again citing Southeastern data. In all this work, Haas revealed inclinations that now seem contrary to those recently expressed, for instance, by members of the SSILA committee evaluating the Routledge Atlas; I believe she might have agreed with my own judgment that anti-Greenberg reaction may be taking us too far back in the direction of Powell. But we cannot
know this, and perhaps her own current feelings would be different, if she were here to tell us.

I want to close by telling how warm and generous Miss Haas was, even with younger scholars she did not know at all well, like me. I used to send her copies of most of what I wrote on Muskogean, and she would acknowledge these with very sweet, supportive brief notes, sometimes signed "love, Mary". When I taught a field methods class on Creek-Seminole in the mid-eighties she allowed me to copy the dictionary manuscript she had given me a few years earlier (with its revealing handwritten annotations) for my students, and wrote out a careful description for me of where her fieldwork had been conducted and who the consultants referred to in the dictionary were. (I must say parenthetically that the only thing I really regret about Mary Haas's contribution to Southeastern studies is her decision not to publish this absolutely excellent dictionary, which was ahead of its time in so many ways, such as its innovative use of practical orthography. There will eventually be another dictionary to supplement the existing nineteenth-century missionary dictionary, but it is indeed sad that the first modern Creek dictionary cannot be Haas's.) She also generously gave permission for me to cite other unpublished materials, such as her Natchez texts and the wordlists for Natchez and Chitimacha prepared by her and Morris Swadesh.

Mary R. Haas had a long and wonderfully productive scholarly life, in which she made extremely important contributions to the study of the American Indian languages of the Southeast, which will continue to be influential as long as the names of these languages are known. We are all lucky to have been touched by her work. She will be missed.

Bibliography of Works by Mary R. Haas Referred To Here
(in some cases rather obliquely; many others could have been added)


Haas, Mary R. n.d. English-Natchez Vocabulary [with an English-Chitimacha Vocabulary Taken from the Notes of Morris Swadesh]. Ms.

Haas, Mary R. n.d. [c. 1940]. Creek Vocabulary. Ms.


The Importance of Mary R. Haas

Karl V. Teeter
Professor of Linguistics Emeritus, Harvard University

I left Berkeley in 1959. Now after thirty-seven years, I am here, as are we all, to pay tribute to Mary Haas. She wanted us to say Hawes, but I am a native speaker of English from the Boston area, and my tongue could never quite make this adjustment. Without Mary I have no idea what my career would have been. In the event, I became another one of the unprecedented number of Americanist linguists she trained. I was a fairly early exemplar, beginning in linguistics in 1954, but far from the first: Bill Bright, Bill Shipley, Sydney Lamb, Phil Barker (later Mohammed Ali Rahman Baker) and many others were ahead of me.

When I first encountered Mary in fall of 1954 I was a college dropout returning from military service in the so-called "Korean War", if anybody remembers that little incident in our century of wars. Having been out of college for eight years, in 1954 I became a 25 year old sophomore a month before the first of our four daughters was born. Thanks to my real world experience with the Japanese language, I was able to start off in third year courses and thus majored in Oriental Languages. In my first semester, however, I also took Linguistics 110, Phonetics and Phonemcs, Mary's course. Among my classmates were Harvey Pitkin and Catherine Callaghan. Bill Shipley was Teaching Assistant for the course and we waited eagerly until Friday each week for him to explain just what it was Mary had been trying to say; it was a whole new and complex world to us!

Thanks to Mary, from almost the moment I received my A.B. in 1956 I was able to take advantage of one of the marvelous research opportunities provided by the many and diverse California languages, whose study was organized under Mary’s direction in the Survey of California Languages. In organizing the Survey, Mary made herself personally responsible for preventing these languages from dying without documentation, the fate of so many languages, then and now. In thus becoming the mother of the California languages she also provided, as I
have said, unparalleled research opportunities for young and aspiring linguists. In my case, I was assigned to the last speaker of Wiyot, Della Prince, and studied with her from 1956-59 (she died in 1962). In my day one was given a University car, a notebook, and a Webcor tape recorder the size of a heavy suitcase, and charged to bring back a grammar, texts, and dictionary, whatever these might be (see my contribution to the Haas Festival volume). My Ph.D. thesis was a Wiyot grammar and texts (published in UCPL vol. 37, 1964), and thanks to my early retirement at age 60 in 1989, I am even now completing a comprehensive lexicon of Wiyot, giving me the grammar, texts, and dictionary: a minimal Boasian description, as Mary taught us, in my case completed a mere forty or so years after beginning field work!

Now I make an apparent digression. When I left Berkeley in 1959, I left equipped not only with Mary's indispensable tutelage but also with an introduction to her best pal in linguistics, Frank Siebert, who now lives in Old Town, Maine. In fact, this tribute is in a very real sense a collaboration with Frank, who was kind enough to correct and elaborate on an earlier version of these remarks. Frank Siebert is two years and three months younger than Mary, and only physical and financial problems have kept him from coming to this meeting. He would much rather be here in person, I can assure you.

In the early thirties Frank, who is a medical doctor, commuted from medical school in Philadelphia to Edward Sapir's classes in New Haven and to the seminars of Franz Boas held at Morningside Heights, Columbia, the latter attended by the entire set of linguists in the area: Frank recalls Sapir, Whorf, Hockett, Newman, Swadesh, Roland Kent, Zellig Harris, etc. Frank himself began field work on Penobscot in Indian Island, Maine in 1932, and is still carrying on his research there, although the last speaker of Penobscot is gone. He went with Mary in Oklahoma in November 1940 on an unsuccessful quest for the last speaker of Natchez, Watt Sam, and recalls their finding an old Cherokee woman named Polly Wildcat who knew Cherokee and Creek, but not English (Mary elicited Cherokee material from her by the use of Creek). Mary
had previously travelled to Maine (the first time she saw the Atlantic Ocean) in July 1936 and she helped Frank with the study of Penobscot, utilizing her gift of absolute pitch to transcribe the intonation of the language with musical staff and notes. Mary's closeness to Frank may be judged by the fact that in September, 1933, when he nearly died after urinary tract surgery, she telephoned him at the Maine Medical Center in Portland, which must have been one of her last voluntary acts before she sank into her final illness.

Recently I have been reminiscing about Mary with Frank, who is full of admiration for what he describes as her honesty, integrity, and skill in doing field work. He also points out what I think is central about her, and here ends my digression: Mary, he suggests, trained more American linguists than Boas, more even than Boas and Sapir put together, or anyone else for that matter, he adds to my draft of these remarks. As I put it earlier, she became the mother of the California languages, and her children thrived under her attention even while the speakers were dying out.

A scholar is most directly evaluated on the basis of what he or she has left behind; in Mary's case we are all familiar with the large body of her important written work: her grammar, dictionary, and texts of Tunica, her many papers on Muskogean and other southeastern languages, her Spoken Thai textbook, Thai-English Student's Dictionary, and papers on language teaching, Thai, Burmese, and classificatory problems in native American languages, a written legacy of unique importance. But one of the things I remember about even Sapir is that, as brilliant and multifaceted as his scholarly work was, the students he trained represented a contribution to linguistics beyond the written studies. Now Mary, as Frank Siebert reminded me, and I repeat this here, has trained more American linguists than Sapir, more than Boas, more than anybody! I believe that one begins to fully appreciate her scholarly output only when considering not merely WHAT she left behind, but WHO she has left behind.
In considering this question, I am fortunate to be able to supplement my memory, which is aging along with the rest of me, with two lists I have obtained, thanks to Leanne Hinton and Katherine Turner. On my behalf, Leanne asked the UC computers for a list of Mary's Ph.D. students. The task, apparently, was too much for them, and the best they could do was come up with a list of around sixty individuals who received Ph.D.s since the late sixties. Fortunately, I was personally at Berkeley from 1954-59, so I can beat the computers here at their own game, recalling that I myself received a Ph.D. in 1962, too early for Leanne's list to notice. And I was far from the first. Just to pick at random a few friends and colleagues from my memory, I was preceded by Bill Bright, Syd Lamb, Bill Shipley, Bill Jacobsen, and many others. So it looks as if in actual fact there were certainly more than a hundred Ph.D.s who may be considered Haas' students, the great majority of them Americanists.

The second list I was fortunate to obtain was from Kathy Turner. The individuals listed here partly overlap those on the PhD list, but the list has a different purpose: it includes those who received support for fieldwork from the Survey on California Languages, from 1953-77. My manual count gives me 75 of these, who studied mostly Californian languages, but also others, from Central America to Nez Perce.

Now we all know that California languages are in bad shape, in spite of the excellent work currently being done by such as Leanne Hinton in encouraging natives to study their languages, the "lonely hearts language club.". But even in the extreme case where a language is gone entirely, as with Wiyot, the one with which I am most familiar, Mary's efforts mean that it is unlikely to be gone without a trace. In the case of Wiyot, for example, my work provides a body of data which may be mined in order to develop materials for teaching the language, a central interest of the modern Wiyot people. While it is my work and the efforts of Della Prince which have given us this material, I cannot claim credit for the impetus which sent me to do the work in the first place: that came from Mary.
Her efforts, directly and through those of us she guided into linguistics, more than a hundred of us, as indicated by my lists, more than Boas taught and even more than taught by Boas and Sapir together, have kept knowledge of California languages alive, even in the worst cases where all of the speakers have died.

This is the unique legacy of Mary Rosamond Haas, and I pay tribute to her here for her extraordinary achievement.
Mary Rosamond Haas 1910 - 1996
Awards and Honors

1910  born January 23, 1910, Richmond, Indiana

1923-26  Richmond Senior High School, Richmond, Indiana

1927  First Prize for "Nocturn in A Flat Major", Richmond Art Exhibit, Richmond, Indiana

                      First Prize for essay on Chopin, Richmond Art Exhibit, Richmond, Indiana

                      Honorable mention for poem, "Autumn", Richmond Art Exhibit, Richmond, Indiana

1926-30  Earlham College, Richmond, Indiana

1930  A. B., Department of Comparative Philology, University of Chicago

1935  Ph.D., Yale University. Dissertation: "A Grammar of the Tunica Language", with partial funding from the American Council of Learned Societies

1935-41  Funding for research and field work from:

                      Committee on Research in Native American Indian Languages

                      Phillips Fund, American Philosophical Society

1941-44  Research Fellow, American Council of Learned Societies

1949  Rockefeller Foundation grant for Thai field work

                      Viking Fund Fellowship for Thai field work

                      U. S. Office of Education funding for Thai dictionary project

compiled by Katherine Turner, June 1996
Mary Haas

1956  Vice President of the Linguistic Society of America

1960-61  Walker-Ames Visiting Professor of Linguistics, University of Washington, Seattle

1963  President of the Linguistic Society of America

1964-65  Faculty Research Lecturer, University of California, Berkeley
          Guggenheim Fellow

1967-68  Fellow in Residence, Center for Advanced Study in the Behavioral Sciences, Stanford University, Palo Alto, California
          Senior Fellow, National Endowment for the Humanities

1971  Virginia Gildersleeve Visiting Professor of Linguistics and Anthropology, Barnard College and Columbia University, New York

1972-73  Board of Overseers, Harvard College, member of the Visiting Committee on Linguistics

1974-76  Board of Overseers, Harvard College, Chair of the Visiting Committee on Linguistics

1974  Elected to the American Academy of Arts and Sciences

1975  Doctor of Literature, Northwestern University, Evanston, Illinois
          Edith Kreeger Wolf Distinguished Visiting Professor, Northwestern University, Evanston, Illinois

          Senior Distinguished Lecturer, American Anthropological Association

1977  The Wilbur Lucius Cross Medal, Yale University Graduate School
          The Berkeley Citation, University of California

compiled by Katherine Turner, June 1996
Mary Haas

1978    Elected to the National Academy of Sciences

1980    Doctor of Humane Letters, Earlham College, Richmond, Indiana

         Doctor of Humane Letters, Ohio State University, Columbus, Ohio

1984    Haskin Lecturer, American Council of Learned Societies, New York

         Elected a Berkeley Fellow, University of California

1986    Haas Festival Conference, University of California, Santa Cruz

1996    died May 17, 1996, Berkeley, California
Bibliography

Mary R. Haas

Abbreviations used:

AA American Anthropologist

CTL Current Trends in Linguistics, ed. by T.A. Sebeok

IJAL International Journal of American Linguistics

JAF Journal of American Folklore

JAOS Journal of the American Oriental Society

LCH Language, Culture, and History, ed. by Anwar S. Dil

Lg. Language

UCPL University of California Publications in Linguistics


1935 A grammar of the Tunica language. PhD. dissertation, Yale University. [Published in revised form under the title Tunica 1941.]


1938 Geminate Consonant Clusters in Muskogee. Lg. 14:61-65.


1940 Ablaut and its Function in Muskogee. *Lg.* 16:141-150.

Creek Inter-town Relations. AA 42:479-489.


Noun Incorporation in the Muskogean Languages. *Lg.* 17:311-315.


1942 Comments on the Name *Wichita.* AA 44:164-165.


The Use of Numeral Classifiers in Thai. *Lg.* 18:201-205. [Reprinted in *LCH,* pages 58-64.]


Beginning Thai. (Reproduced from a typewritten copy,) Department of Oriental Languages. Ann Arbor, Michigan: University of Michigan.


*Thai Phrases.* (Reproduced from a typewritten copy: Army Specialized Training Program. University of California, Berkeley.)

*Manual of Thai Conversations.* (Reproduced from typewritten copy: Army Specialized Training Program. University of California, Berkeley.)

*Thai Reader (in phonetic writing).* (Reproduced from a typewritten copy: Army Specialized Training Program. University of California, Berkeley.)


Southeastern Indian Folklore. (A section of "Folklore Research in North America.") *JAF* 60:403-406.

*Phonetic Dictionary of the Thai Language.* Vol. I, Thai-English; Vol. II,
English-Thai. (Reproduced from typewritten copy.) Berkeley, University of California Press.


1948 Classificatory Verbs in Muskogee. IJAL 14:244-246. [Reprinted in LCH, pp. 307-307.]


1950 On the historical development of certain long vowels in Creek. IJAL 16:122-125.


The declining descent rule for rank in Thailand: A correction. AA 53:585-587. [Reprinted in LCH, pp. 48-52.]


Sapir and the training of anthropological linguists. AA 55:447-449.

California Press.


Notes on Some PCA Stems in /k-. *IJAL* 24:241-245.


Thai language. *Encyclopedia Britannica*.


Southeast Asian Languages. *Encyclopedia Britannica.*


Thoughts on L. White's Query. *AA* 67:1556-1559.


1968 The last words of Biloxi. *IJAL* 34:77-84.

Notes on a Chipewyan dialect. *IJAL* 34:165-175.


Haas's reply to Hockett. [Hockett's "Reply to Haas's comments on Bloomfield's "The Menomini Language".]. *AA* 70:570.


Grammar or lexicon? The American Indian side of the question from Duponceau to Powell. *IJAL* 35:239-255. [Reprinted in *LCH*, under the title "The problem of classifying American Indian languages: From Duponceau to Powell", pp. 130-163.]


_The Prehistory of Languages_. Janua Linguarum, series minor, no. 57. The Hague and Paris, Mouton and Co. 120pp., bibliography, index. [Reprinted 1978] [Chapter 5, "Prehistory and Diffusion", pp. 78-97, reprinted in *LCH*, pp. 308-327.]


Author's postscript. LCH, pp. 370-372.

Areal linguistic characteristics of east Asia. *Chinese Language Use* (Contemporary China papers, no. 13), ed. by Beverley Hong, pp. 1-8. Canberra: Contemporary China Center, Australian National University.

1979 Overview. *The Victoria Conference on Northwestern Languages* (British Columbia Provincial Museum Heritage Record No. 4), ed. by Barbara S.


