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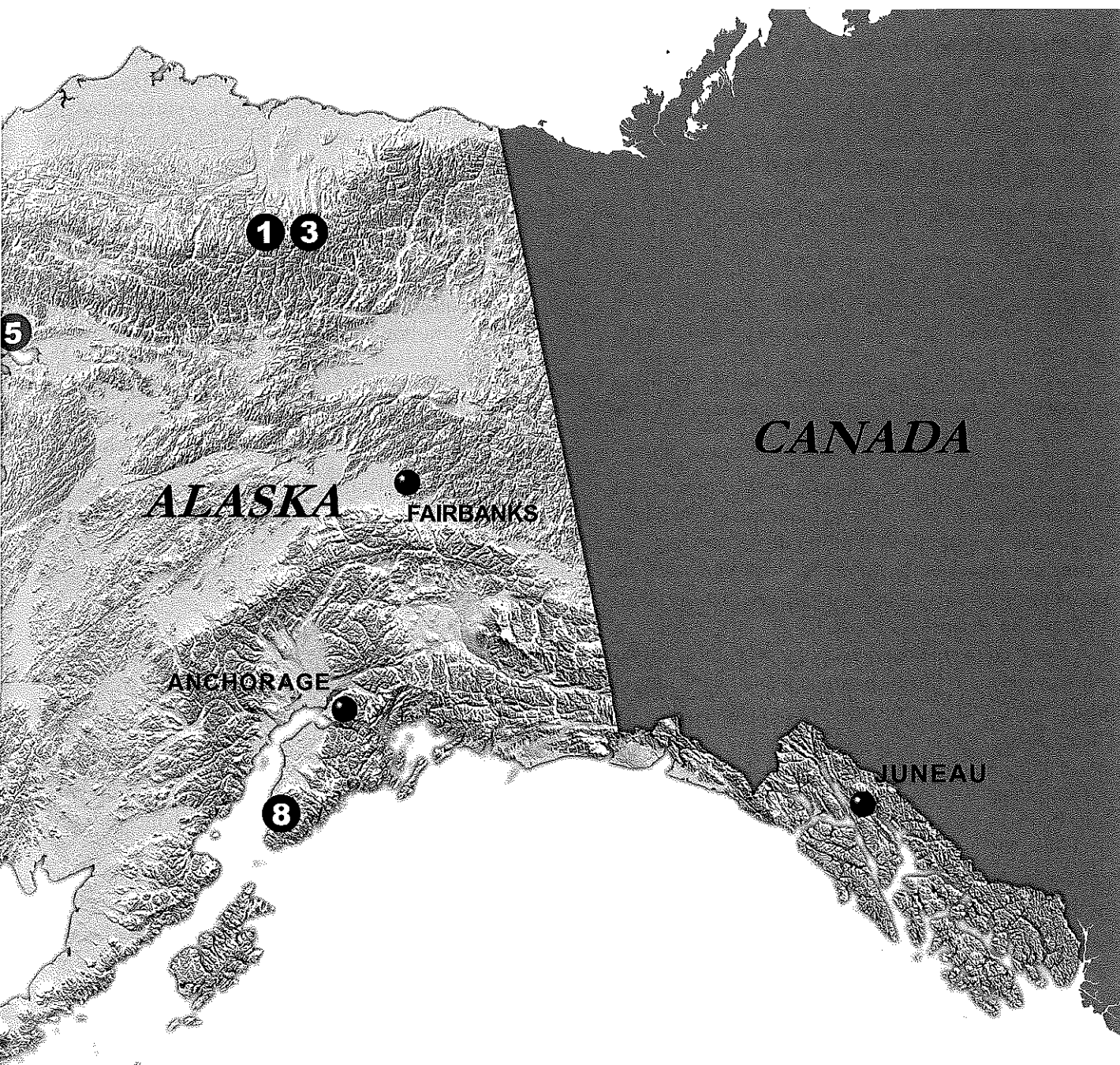
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# INTRODUCTION: MAKING IT: CREATING ARTIFACTS IN THE ANTHROPOLOGICAL SETTING

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*"Yesterday evening and today I have been in a state of excitement, caused by my success in making combs. I have an artistic intoxication, it's a little like writing verse."*

So wrote the father of participant observation, Bronislaw Malinowski, in 1914 (Malinowski 1967:125). Malinowski was in the field at the time, "creating artifacts in the anthropological setting." He purchased the tortoise shell for the combs (at a bargain price, he adds), and drew the designs for them. By his own account he spent many enjoyable hours working on combs, but not without some guilt: "In the morning wasted some time on tortoise shell" and, "I went back home intending to write retrospect[ive] Diary, and to work on tortoise shell only a little while. I started at 9 and was at it until 1..." In successive pages of his famous diary the entry, "tortoise shell" becomes a gloss for time spent designing and making combs. Malinowski's combs were the tall curved tortoise shell variety made famous in O. Henry's "The Gift," and they were destined for his fiancée, Elsie, who waited out his long stint of fieldwork in Australia.

Perhaps Malinowski's comb designs drew upon the native art he saw about him, for at one point he comments—maybe facetiously, maybe not—*"we planned to launch a new Papuan style."* It's worthy of note that his mentor in comb-making, the other half of this "we" was not a native, but a white ex-patriate on the Melanesian island of Samarai, identified in the diary only as "Smith." Why, one wonders, would Malinowski have been spending so much time with a non-Native? Was it the lure of companionship of someone from his own culture, a temptation we anthropologists are urged to forego in the field? Or was it, perhaps, a tradeoff? Had Smith set up a cottage industry and willingly traded companionship for another pair of hands?

Unlike the authors whose papers follow, Malinowski's artistic efforts had no ethnographic purpose, though they clearly might have had. They were simply

diversionary, a welcome respite from the loneliness and frustration of fieldwork. Malinowski obviously enjoyed working with his hands, appeared to be good at it, and waxed far more euphoric about his accomplishments in artifact making than in ethnography.

For many anthropologists in the field making artifacts as Malinowski did continues to be a welcome diversion from the stresses of field research and a milestone of self-accomplishment in a foreign culture. At the same time, it can also be an important form of participant observation, a way of joining in, expressing a willingness to learn, and earning approval. Research methods textbooks and manuals don't mention making artifacts as a participant observation strategy (e.g., Jackson 1987; Russell 2002; Wolcott 1995) perhaps because such activity is assumed too inconsequential, or perhaps because it might blur the separation between observer and observed that such texts are often at pains to emphasize.

Making objects in the context of fieldwork is more apt to come from the toolkit of the aesthetic anthropologist or the art historian studying the art of a particular culture than from that of the ethnographer. At the very least the former have more reason to confess that they have made or attempted to make the objects that are the subjects of their study. Making ethnographic artifacts—"artifaking"—to use a term coined by art historian Bill Holm—has even led former hobbyists, such as Holm, into academe. Their careful attention to process and style has resulted in the identification and articulation of the formal rules underlying art styles (e.g., Bennett 1997; Holm 1965). Whatever else, few would disagree that, having made it, one can explain the process in ways that those without such first hand knowledge cannot.

In the course of his decades long studies of Inuit art Nelson Graburn tried his hand at carving Inuit stone sculpture and he included one of his artifacts in a serious research experiment. In an effort to resolve an ongoing debate about the aesthetic evaluation of Inuit art, Graburn subjected 25 pieces of Inuit sculpture exhibiting a range of styles and workmanship to two different audiences for evaluation. The first comprised Canadian Inuit in several northern communities (Graburn 1977). The second included White collectors, critics, and exhibit selectors, along with intelligent others not so familiar with Inuit art history (Graburn 2001 [1986]). The collection both groups evaluated included two non-Inuit—though not identified as such—sculptures, one of which was a snail carved by Graburn. Commenting on the results of this experiment, Graburn stated that, for neither audience of raters were there any clear-cut “winners” among the sculptures. His snail, a very non-Arctic creature, received a lower rating (C-) from Inuit than from Whites (who gave it a B), and he concluded, as any anthropologist might expect, that White and Inuit evaluations of Inuit sculpture differed markedly.<sup>1</sup>

Making artifacts in the anthropological setting may be uncommon among ethnographers, but it is the very stuff of ethno-archaeology. Doing so allows the archaeologist to put the made object to use to better understand its attributes, its performance, and its contribution to a past way of life. Lisa Frink, Brian Hoffman and Robert Shaw (2003), for example, replicated prehistoric slate-bladed ulus (the curve-bladed women’s knives common to Eskimo/Inuit groups) and gave them to modern Yup’ik Eskimo women for cutting fish to see if the slate ulus performed differently than modern steel-bladed ulus, and, if so, what these differences meant in Yup’ik lives. Ethnoarchaeologists, then, are interested in the information about objects and their uses that can be extrapolated from “artifaking,” whereas ethnologists, making artifacts in the context of fieldwork, are more likely to be creating an occasion for open ended social interaction.

The papers that follow began as a discussion between the authors of this introduction. Blackman argues the merits of “making it” and shares Malinowski’s euphoria. She clearly enjoys humanizing the descriptive sterility that often pervades object studying and believes “making it” offers unique insight into understanding objects. Lee points to the drawbacks of “making it,” espe-

cially the narrowing effects that a focus on the purely technical aspects of an art form can have on its study. “Making it,” in her view, may limit understanding by confining one’s interpretation of ethnographic objects to the very formal rules of their construction and style. Given our divergent views, we thought the topic worthy of a symposium at the Alaska Anthropological Association meetings. Accordingly, Lee recruited several panelists and a discussant to join in the exchange, which took place in March of 2003.

“Making it” raises the issue of what it is about artifacts/art that we should be attending to in seeking to understand them. The contributors brought a range of perspectives, expertise, and Alaskan field experiences to the discussion. Hensel uses his efforts as an occasion for reflecting on the differences between Yup’ik and non-Native learning styles. He thinks of “making it” in respect to the rules of learning the process, the developmental issues of child/adult competence and in the significance of the reproduction of ancient artifacts.

Alix and Brewster, archaeologist and oral historian respectively, document how Alaska Natives choose raw materials from which to “make it.” In doing so, they provide valuable background for future investigators with interests in processual approaches to the study of artifacts. Their study of driftwood and its uses focuses on the raw material for artifact making and the importance of following its natural life cycle from forest to river to sea.

Linn ponders the weighty ethical issues of re-making ethnographic objects in museum collections, when the institutions where they are housed historically oppose such practices. Her case study of a kayak-recovering project in an Alaskan village touches on the question of authenticity and community involvement in the process of “re-making it.”

These five contributed papers and commentary broaden the discussion and expand the applications of making artifacts in the anthropological setting. We hope that they are only the first in an ongoing consideration of this under-investigated topic.

<sup>1</sup>Graburn made the snail out of local stone as part of his ethno-aesthetic research. After a few weeks of showing photographs and actual sculptures to the people of Puvirnituk, he realized that pretty well all the Inuit comments concerned mimesis, the faithfulness to the original subject, e.g. Inuk, animal, bird, etc. So he made the snail so that Inuit would not know how faithfully it reproduced the original, and then showed it to many Inuit for comment. Only then did he get more formal comments: it should be placed more centrally on the base, the lightened (scratched) part of the surface should show better contrast with the polished stripe at the back, or material comments, e.g. the line on the top which looks like a crack should not appear there as white men might think it will break, and so on.

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# FIRST, THE CARIBOU

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**Abstract:** Everyone in Anaktuvuk Pass can tell you how to make caribou skin masks, the signature village craft. Even first and second graders, who decorate their paper plate versions with fur and add the authenticating silver hand tag identifying Alaskan native crafts. Having been awarded a grant to study the caribou skin mask, it seemed an ethnographic imperative that I too learn firsthand how to make one. The story begins with the advice of veteran mask makers: “First, you gotta get a caribou ....”

**Keywords:** Skin sewing, Masks, Anaktuvuk Pass

“First, you gotta get a caribou,” She looked at me and laughed. I’d heard that advice before. It’s often the opener when Anaktuvuk Pass people demonstrate caribou skin mask making at some venue and someone expresses interest in how to make them. “You wanna learn how to make a mask, well first....”

I was sitting in Lela Ahgook’s living room in the summer of 2002, intent on making a caribou skin mask as part of my study of this village’s signature craft (Figure 1).<sup>1</sup> Maybe I could I learn something about masks that had escaped me in all the photographing, interviewing of mask makers and note taking that I had previously done. To be frank, the subject of my research was pretty focused and mundane—a single type of tourist art from a single village. It begged for some new insight or approach; making a mask might just offer that window. If not, it would still be fun, and there’s nothing better than having a good time while doing ethnography.

Lela, my friend and a long time mask maker, was recuperating from recent surgery and eager to make some masks to take to the AFN (Alaska Federation of Natives) convention in the fall (Figure 2). “I gotta scrape some skins before I can make masks,” she added. What she didn’t say but implied was that she wasn’t sure she was up to scraping skins so soon following her surgery. I leaped at the opportunity to offer my labor. “I’ll scrape them for you, put me to work.”

From the outset of this project I had every intention of learning by doing, in part because I’ve always liked

doing things with my hands. I’d eagerly signed on for a class in cedarbark basketry three decades ago when I was conducting my dissertation research with the Haida, and the resultant hat I made, if nothing else, convinced some members of my host culture that I was capable of doing something that demanded a measure of skill, stick-to-itiveness, and achievement of an aesthetic standard. Several years later, under the tutelage of none other than master Haida artist Robert Davidson, I failed exquisitely at carving a wooden soapberry spoon and applying to it what I thought was a conceptually brilliant Haida design of a beaver. When no one was looking, I committed the egregious act of taking a piece of sandpaper to my spoon, but in doing so I developed a new respect for woodcarving and the consummate skill required to create what the mind’s eye sees. In the end I had the dubious honor of donating my pathetic art work to Robert Davidson’s personal collection—as an example of what I’m not sure. Skin working seemed more doable than carving. I’d never carved before, or after, my soapberry spoon challenge, but until recent years I’d always sewed my own clothes, clothing for my daughter, wearable art shirts for my husband, quilts. Why not skin sewing, which was what Anaktuvuk Pass mask making was about?

Lela fetched a luxuriant looking caribou hide from the back room and plopped it down on the floor. She reached in a drawer for her small *ikun* (scraper), while her husband Noah disappeared into the back room and returned with a long handled scraper. After trying both, I settled on the long handled one. The short *ikun* had been made for Lela’s smaller hands. The long handled scraper

<sup>1</sup>The origin and early history of Anaktuvuk Pass mask making is described by Atamian (1966); subsequent developments are described by Blackman (1997). This essay derives from research under a current NSF Arctic Social Sciences grant, “Tourist Art and Traditional Knowledge in Northern Alaska.”

gave more leverage and allowed longer strokes. The job would also go faster with it (Figure 3).

Lela settled into the couch and talked at me as I scraped. “Start at the edges. Always do the edges first so you don’t make holes in it. Don’t scrape where the *kumiiks* are,” she instructed, drawing a line with her ulu where the hatching bot fly larvae had pockmarked the skin. “Too bad one side of it’s bloody; I don’t think we can use that for masks.” This skin was teaching me a lot already. After a day of chasing down interviews I was enjoying the vigorous, rhythmic activity of scraping. The dried bits of connective tissue came off in gratifyingly long ribbons, revealing the soft white hide beneath. Talk naturally turned to masks and the wooden molds on which they’re made—who got so-and-so’s when she died—and scrapers—who made the ones belonging to Lela. It had been a year since Lela and I had seen each other, so I wasn’t surprised when she changed the topic. “Tell me your story,” she demanded in a reversal of our customary roles. I began with the biking trip I’d just completed across northern Spain on the Camino de



Figure 1: Anaktuvuk Pass caribou skin masks



Figure 2: Lela Ahgook and the author. Photo by Judith Gussler

Santiago along the medieval pilgrimage route to that city. She wanted more. “Aren’t you married yet,” she asked. “No,” I answered. “Two marriages and 27 years was enough.”

Noah, Lela’s husband and an indefatigable joker, started in on me. “You’ve got to sing if you’re going to scrape skins. You’ve got to sing, Margaret.” I told him if he’d drum, I’d sing. Fortunately, for all concerned, he didn’t take me up on it. I worked a steady hour that first day on the skin and returned the next afternoon to put in

another hour. As I picked up my shavings and prepared to head home, Lela inspected the skin and said I’d scraped enough for maybe six masks, noting that I would still have to go over all of it again to make it smooth. “Maybe tomorrow,” she invited, “You cut the fur off and go and choose one of my molds to make mask” (Figure 4). They’re made from spruce or cottonwood that comes

from farther south along the John River, or, increasingly, from pieces of 4 X 4’s and other scrap from building activities in Anaktuvuk Pass. Everyone knows that it was Justus Mekiana back in 1956 who invented this process of applying wet caribou skins to wooden face molds to make masks. Molly Ahgook, Justus’ sister, has one of the first molds

Justus made. Nails fill cracks in the forehead, a wad of paper toweling stuffs a hole worn through the upper lip, and a piece of newspaper glued to a cheek smoothes over its worn, rough spots. Molly’s still using the mold. In every mask maker’s box of molds there’s a story of kinship and community—the reciprocity of husband and wife, a trading partnership, inheritance, a favorite uncle’s and a brother’s Christmas gifts. Even the self-sufficiency of widowhood is recorded in crudely carved replacements for worn out molds.



Figure 3: Scraping the skin with long-handled scraper, *ikun*. Photo by Judith Gussler

month after 9/11 and her suitcase of mask making supplies contained several box cutters that she had to explain to airport security. Positioning the tip of her pocketknife in one of the eyesockets, Lela traced the shape of an eye; she repeated the process for the other one. She cut along the lines she had just drawn, then did the same for the mouth. "Do you ever look at the shape of the eyes and mouth on the mold when you cut them on the skin?" I asked. "No, I don't copy it or look at it. I just cut." She flipped the mask over to the inside, and trimmed off a 1/4" band of the short hair from around each eye and the mouth, assuring that no little hairs would show through to the right side. Then she passed the pocketknife to me. I gingerly cut the eyes on my mask, trying desperately to get them the same size and shape. Lela checked my progress. "Big mouth," she exclaimed, trying hard not to laugh. "What are you doing, making a man?"

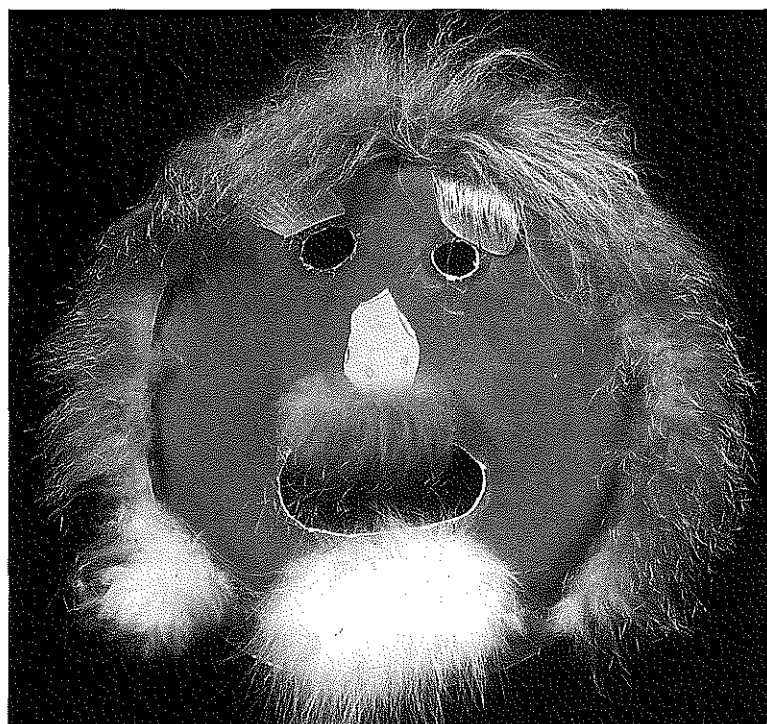


Figure. 6b: Paper plate Anaktuvuk mask made by school child. Photo by James Barker

She began carefully peeling off a thread from a dried stringy slab. "I use real sinew on the face." She threaded her needle. "Real sinew" is a signifier of authenticity and pride in craft. Skins were always sewn with "real sinew" and in this case the opposition is to artificial sinew, dental floss, or worse, glue. Some mask makers even write "real sinew" on their sales tag along with the iden-

tification of the furs that comprise the mask. As Lela's student, I too, would use "real sinew" which is very strong but very short compared to the limitless lengths of artificial sinew one can cut from the bobbin.

Eyelashes are a must for masks of either gender and they require fine, soft fur. Newborn caribou or fetal caribou calf fur was used in the early days, but commercially tanned black calfskin has been the fur of choice for years now. Barbara Lindberg, the owner of Lindy's store in Fairbanks where Anaktuvuk people used to trade masks for groceries and supplies reported that one time a shipment of Anaktuvuk masks arrived at the store sporting false eyelashes. That innovation disappeared quickly; no one wanted Eskimo masks with false eyelashes!<sup>2</sup> Cutting a perfectly even 1/4" wide strip of calfskin with a pocket knife was challenging enough, but sewing these tiny strips on was enough to make me wish humans never had eyelashes in the first place. The fur wouldn't lie in the right direction, the lashes were too far away from the eyelids, and they bunched up and turned in at the corners. I pulled out my stitches and started over.

Eyebrows are cut from the thin-skinned caribou leg pieces. Lela drew a straight line with her pocketknife to mark the bottom of the eyebrow. With her knife she cut along the top of the eyebrow, then folded it on the line to use it as a template for the other eyebrow. "Sew them on any way you want," she instructed. The eyebrows were much more forgiving than the eyelashes; one can take bigger stitches that are easily hidden beneath the fur.

I finished one eyebrow and glanced at my watch. It had taken me nearly three hours to cut the mask off the mold, cut the eye and mouth openings, sew on the eyelashes and one eyebrow. I was exhausted and decided to call it a day.

Word was getting around that I was learning how to make masks. The next morning I was up at the museum when Darryl Hugo, a former village youth tour guide who had graduated to his own tourist escorting business, appeared with a couple of tourists in tow. "Here's someone who might know," he announced. "She's been studying and making masks." I knew the question before they asked it; it was one any ethnographer would want

<sup>2</sup>Many factors drive the market for masks. Consumers want masks that look like "traditional" Eskimos, but masks with animal ears, hooves, or paws (used to simulate hair or beards) are also popular. Male masks outsell female masks, and mask makers themselves emphasize the importance of sewing, rather than gluing, facial features as a mark of quality. Because the masks are made repeatedly on the same molds, there is an inherent conservatism to them.

the answer to as well. So straightforward; yet so complex and difficult to answer. "How long does it take to make one of these masks?" I tallied up the time, not counting the day the masks had dried on their molds, nor securing the caribou: 2 1/2 hours of scraping, nearly 3 hours for soaking and tacking to the mold, 3 hours of sewing so far and my little lady was still bald, minus an eyebrow and a fur ruff. Now I know why informants hate these how long and how many questions. "It seems like forever," I answered, "and I'm still not done."

My time was running out; I was due to leave in a couple of days. Hopefully in another long dedicated afternoon I could finish my mask. I was getting to be a fixture in the Ahgook household. The following afternoon Lela's son Chuck came into the house brandishing the wrenches he was using to dismantle his Argo<sup>3</sup> engine. He spied me sitting on the couch, mask in hand. "You gonna learn how to repair Argos next?" he asked.

I was down to the hair and the ruff. I had a choice for the former—the long thick white throat hair of the bull caribou or a commercially tanned gray mouton sheep hair of similar length. My bandaged left hand already had two puncture wounds from trying to push the needle through tough skin and I knew how thick-skinned fall bull caribou were. I wimped and settled for the mouton. "People are hardly getting the big bulls with that long hair," Lela noted. "You have to get them in late October and younger people don't like to get them because the meat smells bad then. We used to feed it to dogs." I was amazed to discover that it takes only the smallest patch of skin to make the hair on a woman mask. I stitched the square inch patch all the way around, as I was instructed, parted the hair in the middle, tacked it at the sides, and finished it in two tiny braids.

"Whatever you have for ruff is fine with me," I told Lela. She handed me the end of a wolf tail, thick and round with very tough skin. It's the worst piece to use. I wrinkled my nose in disapproval. She laughed and picked up a wolf skin and began cutting a 1/2" thick long piece for the ruff. She started it for me at top of the mask, anchoring it with finer artificial sinew than that which we had used to sew the skins to the molds. Now the sewing felt familiar. I'd sewn ruffs on parkas before, though not on anything as thick as the face of this mask. Only later, when questioned by a reader of this essay, did I realize that the direction in which I pulled the thread as I made overcast stitches (away from myself) was opposite that of my teacher. Attaching the ruff went fairly quickly. A

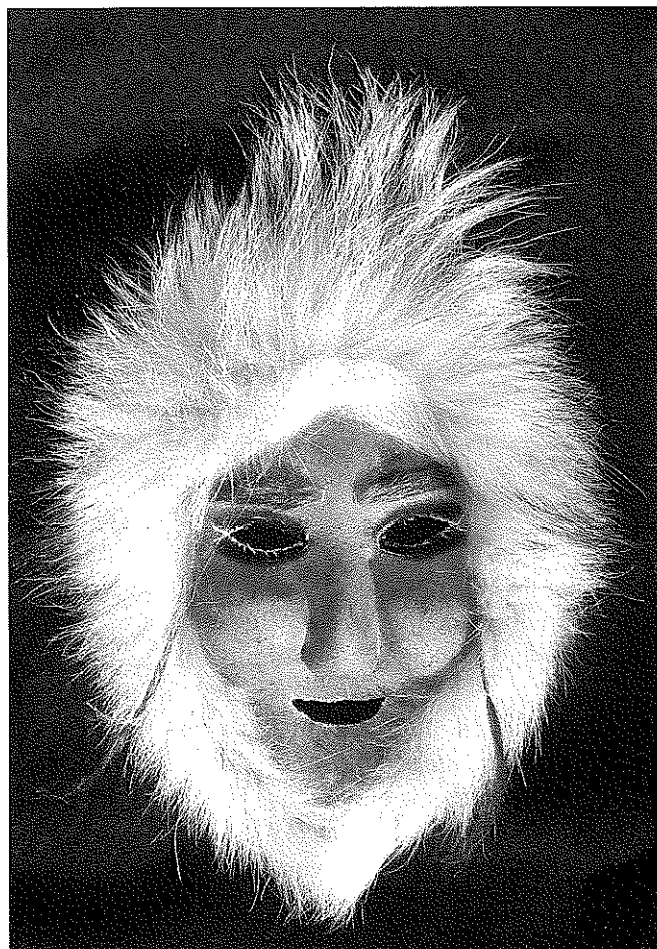


Figure 7: The author's mask

diagonal cut to each end so the pieces met flush below the chin and I was almost done. It seemed fitting that the finishing touch was chewing the ruff skin all around so it would lie flat. Then student and very patient teacher posed, with the finished lady, for photos.

Word spread that I had made a credible mask (Figure 7). Ada Lincoln, a veteran mask maker who debated for three years over the face she would create on one oversize mask, confided to Sarah McConnell, the summer tour company coordinator, "You don't think she's going to start making and selling masks, do you?" Not a chance, Ada. I've gone back to writing about them.

And, since that summer I've been thinking about what it means to make them. In 1964, 36 out of the 110 people in Anaktuvuk Pass made masks. Today there are 16 or 17 or 18 (depending on your criteria for mask maker) out of a population of 310, most of whom were making masks in 1964. Making masks is about money and always has been. Masks pay fuel oil and electric bills; they buy groceries and pay for trips to town. In boom times,

<sup>3</sup>Argo is the brand name of a 6-8 wheeled all terrain vehicle used for summertime travel on the tundra.

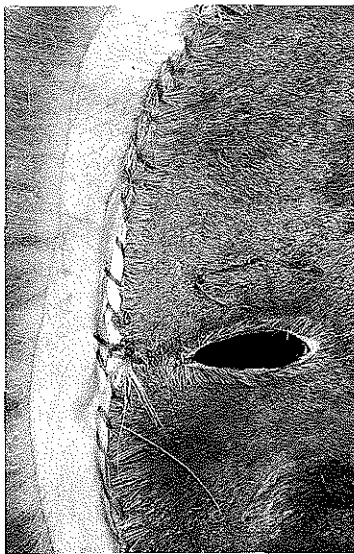


Figure 8a: Interior mask detail. Note shaving around eye. Photo by James Barker

even in remote Anaktuvuk Pass, there are other jobs; but in a downturned economy, like now, mask making provides needed cash and credit.

It is the caribou that will ultimately decide the fate of this craft. Masks aren't the only use for caribou skins today; winter boots, fancy boots, rifle cases, mattresses for camping are others. But it's the caribou as food that is the bellwether for all the uses

of caribou skins. Throughout the years of dramatic changes for the Nunamiut, from settled life to flush toilets to the Internet, the caribou has been a dietary mainstay. Just like Mary Douglas (1975) claimed in her article, "Deciphering a Meal," you need "meat" to make a meal, and "meat" here has always meant caribou. But how far into the future? "This new generation of kids," Lela groused, "thinks caribou is not food."

Regardless of the economic significance of masks, making them is also about remembered activities. Making masks is about using an ulu, that most essential tool, that extension of a woman's very arm. In addition to cutting skins and de-hairing them, making masks is about the skills honed on cutting meat for drying and chopping marrow bones. It's about dozens of movements learned with scrapers, needles, and thimbles practiced over a lifetime.

The familiar rhythms of skin sewing surely carry memories, just as the mask molds remind their owners of the people who made them, the occasions when they were acquired, and the times they've been put to use. I think of Lela's statement, "My favorite time to work on masks is when we go camping" and her remark made a few days later, "If I die while I'm camping, that's OK. Take me while I'm camping!" Skin sewing and camping, two joined pleasures in one woman's life.

And for the student of material culture? Much of the significance is in the details.<sup>4</sup> Like the precision move-

ment of fingers, needle, and thimble that successfully pierces tough caribou skin. Like the use of sinew and artificial sinew and the different feeling of each. Like the economy of raw materials—that little patch of fur that becomes the hair on a woman mask, that little patch whose size you wouldn't know just by inspecting the mask. Or the delicate shaving of hair on the inside of the mask

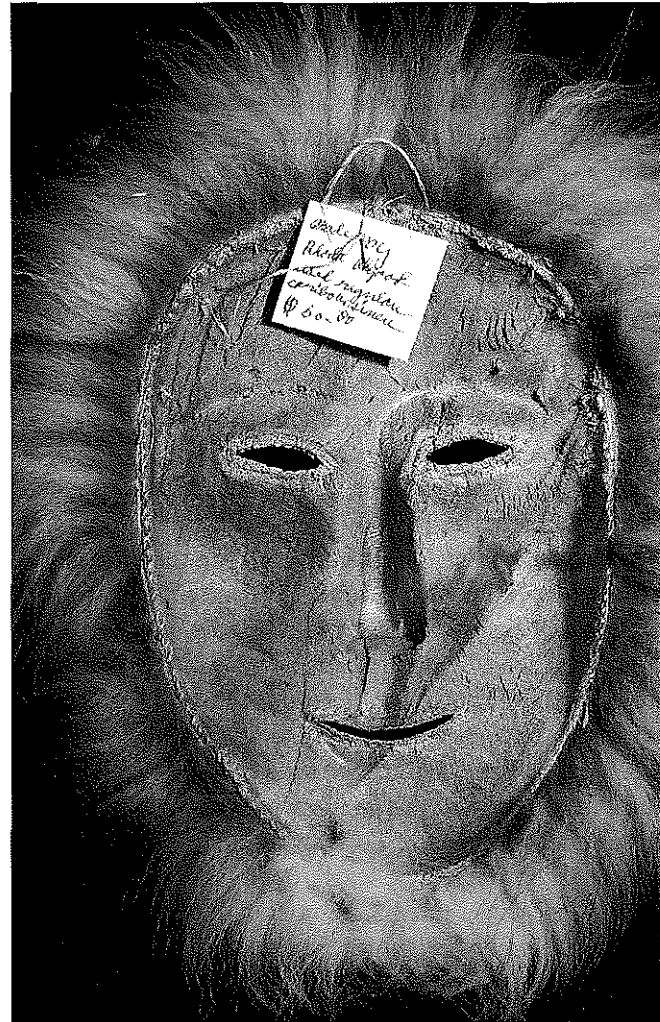


Figure 8b: Inside of mask. Note shaved area around eyes and mouth. Photo by James Barker

around the eyes and mouth that might otherwise go unnoticed (Figures 8a and 8b). Surely, you argue, some of these details would be revealed by simply watching others make masks. True enough.

But there is also the experience. Sewing is both a social activity and a social leveler. The professor morphs into the inept, struggling student, the native artist into the confident and caring teacher. There's something special about the learning environment too:

<sup>4</sup>There are several detailed accounts of Inuit skin working and sewing, most recently Issenman (1997), which contains an extensive bibliography, and Oakes and Riewe (1995).

A living room in Anaktuvuk Pass: Jerry Lee Lewis' biography, "Great Balls of Fire" plays on A&E on the big screen TV. Lela's cheery infant grandson is handed round from lap to lap. Sourdough pancakes and coffee provide a welcome break from work. An afternoon passes in conversation and laughter, in the easy companionship of two women with their sewing.

### **ACKNOWLEDGEMENTS**

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# NOT MAKING IT: FORMALISM, CULTURAL SIGNIFICANCE AND THE STUDY OF NATIVE AMERICAN BASKETRY<sup>1</sup>

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**Abstract:** During twenty-five years of studying of Native American basketry, friends, colleagues, and the lay public have often assumed that my interest stemmed from artistic urge rather than intellectual curiosity. Why should this assumption be so pervasive? The answer may lie in the state of Native American basketry studies, whose progress has been slowed by an emphasis on taxonomy and formalism. Why should this be when the study of other categories of indigenous objects has moved on more rapidly? In this paper I will explore this question, tracing the historical roots of basket studies and their source in the American Arts and Crafts Movement and the feelings of loss and nostalgia that permeated the study of Native American material objects generally at the turn of the 20<sup>th</sup> century. I will then focus on the hobbyist/collector approach that followed. Finally, I will consider more recent approaches to material culture to suggest ways of advancing Native American basketry studies more rapidly.

**Keywords:** Aesthetic theory, American Arts and Crafts Movement, Native American Basketry.

## INTRODUCTION

On a Saturday morning in June long ago, as I slammed shut my car door and joined the crowd of early-bird buyers at a garage sale in Santa Barbara, California, I little dreamed that I was embarking on a journey that would propel me much farther than the length of the driveway of the large suburban house that stood before me. Earlier that week I had bought my first Native American basket – a small, unpretentious example from the Makah Indians of western Washington – which probably explains why my eye was immediately caught that morning by a globular, twined plant fiber basket resting among the other trash and treasures at the sale. It was straw-colored with a dark brown geometric pattern encircling its middle. As I handed over my 50 cents to the about-to-be former owner and sped back down the drive for fear she would change her mind, I felt the rush of excitement that has since become so familiar. Arriving home, I ran in the door to show my husband my new treasure. He took the basket from me, and turned it around slowly. As he followed the chocolate-colored design with his index finger, I watched his eyebrows rise, a sure sign of skepticism. “Hmm,” he said, reading off the design, which turned out to be letters, “‘Hecho en Mejico.’ Some Indian basket!”

I still recall the mixture of incredulity and dismay that I felt before succumbing to hilarity. Like most collectors, I wanted to believe that my “Indian” basket had been made for Native use and had only found its way into non-Native hands by chance. In reality, perhaps 90% of the Native American baskets sold to outsiders over the past century were intended for external consumption, so why was it that I, like all the others, clung so doggedly to this misconception?

I have spent a good part of the intervening quarter century trying to answer this question, and also its corollary, the assumption that I, as a researcher with an interest in Native American baskets, would want to learn to make them. “Look here,” a friend will say excitedly, pointing to the newspaper, “there’s an Indian basket making class this coming Saturday.” Try as I might, I have failed utterly to convince the well-meaning that I’d rather die than take a basket making class, even if I weren’t hampered by having ten left thumbs for fingers. Clearly, “making it” is bundled together with the constellation of acknowledged proclivities associated with Native American basket aficionados and I am deficient in choosing not to learn how.

<sup>1</sup>Prepared for the Alaska Anthropological Association Annual Meeting Panel, “Making It: The Merits and Demerits of Recreating Material Objects in the Anthropological Setting.” Convened by Margaret B. Blackman and Molly Lee, Fairbanks, AK: March 28, 2003.

My refusal to get involved in “making it” stems in large part from a rebellion against the formalist typological approach to Native American basketry research that has been the norm for most of the 20<sup>th</sup> century. There are many valuable insights to be gained from the meticulous attention to detail required by this kind of work (e.g., Cohodas 1976), and it is an essential first step in the more speculative and contextual investigations of today. As an end in itself, however, its uses are important for purposes of identification but somewhat limiting otherwise. For example, this was the approach in which I, as an art history graduate student in the 1980’s, was trained in, and my first publication (Lee 1981), a study of a hitherto unknown Pacific Eskimo (Alutiiq) basket type, certainly fit the mold:

The space occupied by design [on the Alutiiq baskets] is subdivided into primary and secondary design fields, wrote the dutiful graduate student. The primary design field, composed of three of four principal pattern rows, occupies approximately one-half to two-thirds of the upper basket walls. The secondary design field, which may or may not be completely filled in with encircling rows of decoration, is found on the remaining wall space below the primary design field (Lee 1981:67).

I have long since moved on, but others have not, and I continue to wonder why it is that the study of a magnificent art form like Indian basketry should be more about the description of design elements and pattern zones than its changing cultural context, which makes it a rich transcultural object to investigate. This is the question I want to consider here by looking at the meanings non-Natives have ascribed to Indian basketry since it was first collected seriously at the turn of the 20<sup>th</sup> century.

## THE CULTURAL CONTEXT OF NATIVE AMERICAN BASKET COLLECTING

The American middle class welcomed the turn of the 20<sup>th</sup> century with a decade-long fad of Native American basketry collecting. The fad has received some scholarly attention (e.g., Bates and Lee 1991; Herzog 1996; N. Jackson 1984; Washburn 1984), though researchers’ interests have usually been in its history. For that reason, I want to turn to the cultural matrix in which this dramatic phenomenon arose.

For the most part, turn-of-the-century basket devotees shared a common attitude toward Native Americans and their art forms. It echoed the prevailing

outlook of anti-modernism that informed the American Arts and Crafts movement, a social, political and artistic interest group that arose in protest to the growing industrialization that increasingly characterized North America. Typical Arts and Crafts adherents were women of the educated, democratic-minded, upper middle class, who sought to combat their mounting alienation by the promotion of communally-based handicrafts. For them, American Indian cultures served as models of the close relationship between art and life espoused by the Arts and Crafts, as it was known (Boris 1986; Lears 1981). Some, such as Helen Hunt Jackson, author of *Ramona*, surely the best known novel with an Indian basket maker as the main character (H. Jackson 1926), sought refuge in the idealized past by writing about Native American women. Others lived out these ideals by amassing vast collections of baskets. A third, smaller group, consisted of women who actually replicated Indian baskets, out of raffia, to the later consternation of unknowing collectors, museum curators, and eBay dévotées (James 1970:48-83).

For Arts and Crafts adherents, the appeal of baskets rested on a fortuitous blend of practicality, aesthetics and ideological happenstance. They appealed to basket-loving tourists to the southwest or Alaska because they were handmade, but also because they were small, light, easy to pack, and relatively cheap (at least in the early days). As souvenirs, the baskets’ handmade qualities, which contrasted dramatically with the ubiquitous stamped-out pots and pans of the Industrial Age, made them appealing gifts to take back to those at home.

As part of molding the narrative of national angst around the Indian basket and re-contextualizing it as a symbol of anti-modernism, collectors endowed it with an aura of sacredness.

When the art of basketry was at its height, wrote one collector, ... the same [basket design] meant one thing to the Indian on the mountains and another to him who roamed the deserts. Thus[,] a zigzag design may mean waves, [or] a prayer for preservation from shipwreck to one who dwells on the coast and ... a prayer for protection against lightning [to an Indian] inland (Wilkie 1902:3).

In reality, Native Americans regarded their baskets as largely, if not exclusively, utilitarian. Most ethnic groups took the names of basket designs from the natural phenomena they associate with it. Among the Alaskan Tlingit, for instance, common pattern names included “half the head of a salmonberry,” “fern frond” and “shark’s tooth”

(Emmons 1902). While it is true that some Native Californians burned baskets in funeral ceremonies, this was in keeping with the widespread custom of destroying the personal possessions of the deceased and not out of any association of the basket itself with spirituality. Anthropologist Alfred Kroeber, who worked for many years among the California Indians, fought an uphill battle against the presumed sacredness of Indian basket designs. His irritation is clear in the following excerpt:

[Among the Pomo Indians], a typical pattern name ... [an animal part combined with a descriptive term, e.g., "deer-back arrowhead crossing"] is exactly descriptive, he wrote. ... Such a practical purpose, and not any religious or symbolic motive ... seems to at the base of these designs and pattern names. If there is a difference between the Pomo and ourselves, it is that among [them] these conventional figures give no evidence of ... ever having had a symbolic significance (Kroeber 1909:25).

Why did collectors so consistently misinterpret Indian basket designs as sacred symbols? One likely explanation is that it was an outcome of the American middle class's quest for spiritual connection brought on by the waning power of Protestant Christianity. By the late 19<sup>th</sup> century, the rise in industrialization, immigration, and urbanism had dealt a severe blow to the Protestantism that formerly had been the backbone of rural pre-industrial American life (Lears 1981:13). Quite probably, collectors projected the loss onto their Native-made baskets, reading into them the perceived spirituality and closeness to nature that they themselves increasingly lacked.

The search for authenticity in the increasingly inauthentic, mass-produced world that MacCannell describes so well (MacCannell 1999) was a prime impulse of the Indian basket-collecting craze. Spurning the garishly decorated, aniline-dyed tourist baskets, collectors went to unimaginable lengths to ferret out any that remained in Native hands. "There is not a basket [in my collection]," boasted one, "which the Indians supposed, when it was made, would ever be owned by whites" (Brown 1898: 54-56). Especially prized were those baskets showing signs of wear, or those bearing traces of Native food or burns from the hot stones used to heat them for cooking.

During my two visits to the village of Yakutat, wrote one collector, I especially sought for specimens of the basketry of olden time. A vigorous search produced three old baskets, two of which were then in actual use ... and the third had been cast away as worn out... They ... seemed to speak more of the people's life than did the bright and beautiful modern baskets (Meany 1903:213).

If obtaining baskets still in use proved impossible, collectors settled for traditional types replicated for the market, spurning those "faked after meretricious color, designs or shapes" (quoted in Washburn 1984:60). Little did they realize that Native weavers often re-adopted aboriginal forms and organic dyes after years of making more commercial varieties, not out of aesthetic preference, but in response to growing non-Native market demand (Washburn 1984).

## NATIVE AMERICAN BASKETRY STUDIES AND THE TAXONOMIC APPROACH

After World War I when the craze for Indian baskets had dissipated and, before the resurgence of interest in indigenous art forms of the 1960's, basket making, which had virtually disappeared among Native American groups, was kept alive by non-Native American Indian hobbyists/collectors. An outgrowth of the turn-of-the-century, back-to-nature, Boy Scout movement, their main focus was the replication of ethnographic objects (Parezo and Hoerig 1999) though many were also serious collectors (e.g., Chandler and Lanford n.d.). Consequently, the goals of replication and identification were uppermost in their minds. In 1954, Norman Feder founded *The American Indian Hobbyist*, a magazine that published how-to articles on making Indian artifacts. Instructions for replicating Indian baskets occasionally appeared in its pages (Powers 1996).

In 1975, the taxonomic approach to Native American objects was further popularized by the founding of *American Indian Art*<sup>2</sup> magazine, a glossy, dealer-driven publication for which Norman Feder served until his death as principal editorial adviser. Operating largely within the confines of the identify-and-describe approach, *American Indian Art*, now in its 28<sup>th</sup> year, has drawn an extensive and devoted readership among collectors. Recently, the magazine has modified its conservative editorial policy

<sup>2</sup>Interestingly, unlike the collectors from the Arts and Crafts Movement, many Native American basketry devotees from taxonomic/hobbyist period were men. This is probably explained by the close association of the Boy Scout movement with Native American crafts (see Powers 1996, for example). According to Thompson (1985), male takeover of art forms, once they become legitimized is predictable. George Wharton James, who began writing about Native American basketry in the 1920's probably represents the first cross-over.

and has begun to attract a more varied coterie of authors. For scholars, one attraction is the magazine's wide readership; another is that it publishes lavish numbers of color photographs with its articles, a benefit generally unavailable in academic journals.

## CONCLUSIONS

From the fetishistic engagement of the early 1900's forward into the 21<sup>st</sup> century, the study of Native American basketry has been dogged by essentialism. The descriptive approach still characteristic of conservative historians of Native art and many archaeologists attests to its ongoing influence. The rise of Structuralism in the 1980's and the cultural politics and material culture approaches that have replaced it have pointed the way toward more innovative forms of analysis, and within these fields. The research of Pierre Bourdieu (1993), Alfred Gell (1998), Fred Meyers (2003), and Nicholas Thomas (1991) offer tantalizing prospects for breaking its stranglehold (e.g. Bates and Lee 1990). Among indigenous groups in Alaska, California, and the Pacific Northwest, furthermore, Native American basket making itself is undergoing a heartening revival. If my current research on the cultural context of Yup'ik Eskimo grass basketry can count as an example, the study of these living art forms as commodities and commemorators of earlier ways of life is ripe with promise. Yup'ik baskets, for example, are made solely for sale to outsiders, yet my research suggests that through the medium of the beach grass, they commemorate the earlier uses of grass as well as the annual grass harvest women continue to enjoy today. At present, these same baskets are among the most widely sold Alaska Native art form. As a result of this visibility, and because beach grass grows throughout most of rural Alaska, Yup'ik grass baskets are frequently adopted as political symbols in the struggle over Native priority on public lands (Lee 2004).

In the course of this work, I must confess that I have had to learn to "make it." It would have been irresponsible not to. The interpretive analysis of any art form must be grounded by a good, solid description of its variants through time and space. The basketry "traditions" I have studied – from southeastern Alaska north around the Pacific Rim and as far east as Labrador and Greenland – usually include technical and descriptive information. So if I had to, I could probably stumble through the rudiments of coiling or twining a basket. Rather, I am adopting this extreme stance here as a means of pointing out the dampening effect that research based solely on technique and history has had on the study of Native American basketry. I have no wish to buy another "Hecho en

Mejico" basket, though I would never rule out the prospect of studying one. And if I were to do that, I would prefer to look into the many implications of the curiously calligraphic design encircling it than to settle for its mere description.

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# "IS THAT A SLED YOU'RE MAKING?" "AH, NO, A BOOKCASE." LEARNING, HANDWORK AND VISUAL ICONOGRAPHY IN SOME YUP'IK CONTEXTS

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**Abstract:** Doing ethnography requires gaining knowledge of another culture on a variety of levels. This learning is mediated by the culture based learning styles of both ethnographer and host culture. Learning local handwork can help clarify both particular techniques and tasks, and larger issues of how learning is conceptualized. Many opportunities of this sort arose while learning wood and ivory carving, and ulu making in a Yup'ik Eskimo community. My working in local forms also raised issues of appropriation. Simultaneous with my learning of handwork there has also been a slow but steady revival of some traditional Yup'ik forms, including masked dances and festivals, and a perhaps unrelated decline in men's and women's handwork. I conclude that cultural change has left some of my skills as obsolete as those of any colonial wooden bowl maker facing the onrushing tide of pewter and pottery, but that what one learns about learning styles remains useful.

**Keywords:** Ivory Carving, Ulu

Picture me 26 years ago, newly arrived in a small Yup'ik village in Southwestern Alaska. I am literally awash with curiosity. I want to know how everything is done, and why and when. However, it soon becomes obvious that the cultural approach to learning that I grew up with, the question-answer format, does not work all that well. I almost never see Yupiit (the plural) engage each other in this fashion, and when I try, I feel that people are being polite, but long suffering.

But, it's my paradigm, so, the first question I needed to answer was, "how does one find out things?" "How does one learn things in a culturally appropriate fashion?" Over time this led me to ask, "what is learning?" as this also seemed to be conceptualized differently than what I had always taken for granted.

Eventually, I began to develop a mental model for some of these differences. For us Kass'at— (Non-Natives), the focus of instruction is typically on verbal exegesis. Even when there is hands-on learning, it is usually accompanied and/or proceeded by verbal instruction. While we may actually learn most by the activity, we teach a lot by talking, as if that were the most important part. The operative words are "listen to me," or "are you listening to me?" For Yupiit, on the other hand, learning is more often being able to perform the task/activity at hand when faced with the appropriate context.<sup>1</sup> One of

the most common phrases said to children is "*Tang*" (Watch, visually attend). The assumption is that after sufficient watching, that is, when one is ready, one will try the task. After this initial attempt, one will self correct (or be corrected verbally, or by another's example) and try again until mastery is achieved, a process that might be called Watch-Understand-Try-Correct-Re-try (Hensel et al. 1983:Ch. 5, pp. 19-21).

Janet Schantz of Bethel talked about this process of watching, perhaps for years, before attempting a task:

My mom started doing her fish, she started when she was pretty old. . . after her mom died. . . Our grandma had provided all of our smoked fish for us. . . The older people are the ones who are in a position to cut the fish. Somebody else can fish and get them, but the actual cutting and drying and processing is only done by certain people in the family. My mom said that she tried to help my grandma as my grandma grew older, but my grandma always said, "You're gonna butcher the fish, you're gonna to butcher them, you're gonna mess them up". . . And she wouldn't let my mom help. And my mom said when, after her mom died, it was a year or two, it wasn't immediately. . . when she started to make her first flat fish, which is

<sup>1</sup>This discussion applies to non-emergency situations. In emergency or high risk situations, Yupiit are admonished to follow traditional wisdom, and there is a large genre that might be called 'solutions to use in dangerous situations'. So, for example, if you fall through the ice in cold weather and have trouble getting out, wet one mitten and slap it hard onto the sound ice. It will stick (freeze) there and you can use it to pull yourself out. At that point, stuff your clothes with dry grass for insulation, before they freeze around you. And so on. Elders frequently say that even if you were not listening carefully, you will remember this wisdom when you desperately need it.

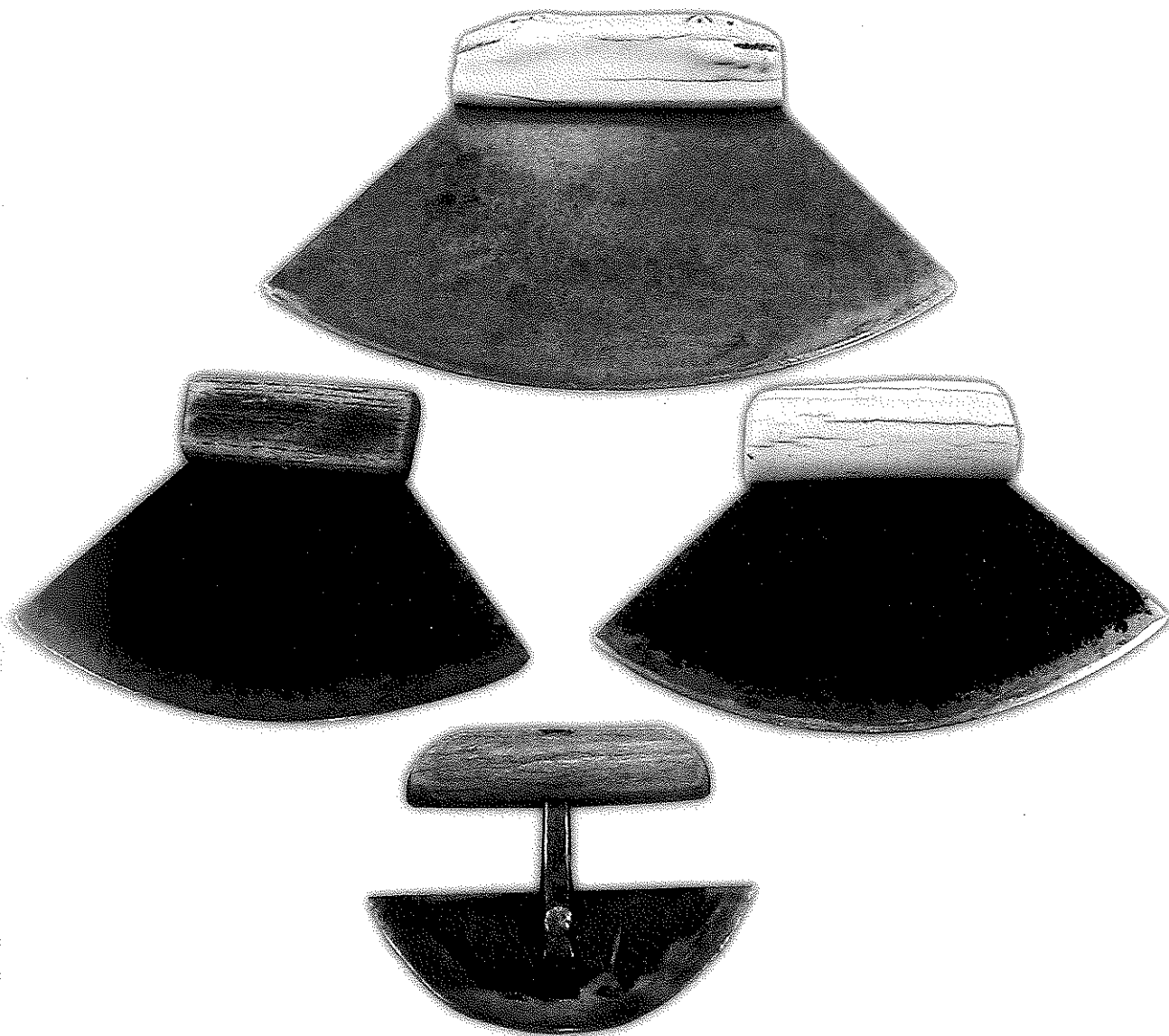


Figure 1: Ulus made by author (Private Collections).

the most elaborate bunch of cutting that you have to do, she stood there at her table, with no help, and made the cuts. And she said it was if my grandma's hand was on her hand, as she made her cuts.

And she said she was just [able to] from watching all those years, and she made a blanket [another name for the 'flat fish' referred to above] for the first time. . . . And she said it wasn't perfect, but it was all there. . . . I'm thirty-eight, and I have friends who have just taken up the knife, and the same thing happened that happened to my mom (Hensel 1986: 61-62).

Much of the proof of learning here can be in the consumption—Is the dried fish good (was it processed in a way that is tasty, not too salty, enough smoke, no fly

eggs or souring)? Which is not to say that people will not eat less than perfect food, only that they recognize and prefer to eat the best.

It is true that Yup'ik often simplify a task, particularly for children, by breaking it down into manageable sub-tasks. A younger person will be allowed/compelled to do increasingly more complex parts of a task until able to complete the task alone (first frying the fry bread, then learning to make a quick dough, then maybe learning to make a yeast dough). Success (consumption of the fry bread) encourages future attempts. Because people reach for the pieces of bread they find most attractive, burned or undercooked breads get left in the bowl until the last, where all of the cook's mistakes are separated out by this consensus of personal choices. This sort of unspoken statement of community standards can also encourage correction in future attempts.

A classroom example of this approach to learning occurred when I was teaching college level math to adults in Nunapitchuk. I expected to lecture on the topic, then have students work some problems. My students kept interrupting me mid-lecture, to ask if they could work some problems. Eventually I adjusted to their patterns, of watching me solve a few problems, then trying themselves. Teaching helped me to understand Yup'ik approaches to learning as well.

Underlying this system is the idea of successive approximations, that attempts will come ever closer to community standards over time. A woman said to me the other day, in reference to a possible substance abuse prevention program in the schools, that the point was to try something, and then you could see what worked and what didn't. Over time, you could get the program to where it needed to be.

A corollary to this system of learning seems to be that most Yupiit feel there is little point in talking about a topic with someone before their interlocutor is at least at the 'Correct—Re-try' stage. It is this belief that was, in part, the basis for my problems trying to learn through a question and answer format (another problem being that repeated direct questions are generally seen as rude or coercive). To engage in a conversation about almost any process/object/activity required knowledge about it on one's part. So, for example, before I went beaver trapping I couldn't seem to get men to talk about it. Most of my questions about techniques were answered with some variation of "the usual way," or "enough", or "it depends." However, after I had helped make a few beaver sets, I was pleased to get someone to talk at length about obscure ruses to trick adult beaver. I actually used information I'd learned in a book as conversational bait, but, together with my hands-on experience, it was seemingly sufficient to show some mastery. This learning to "talk-the-talk" well enough to be a successful interlocutor on a wide variety of topics was a major conversational challenge.

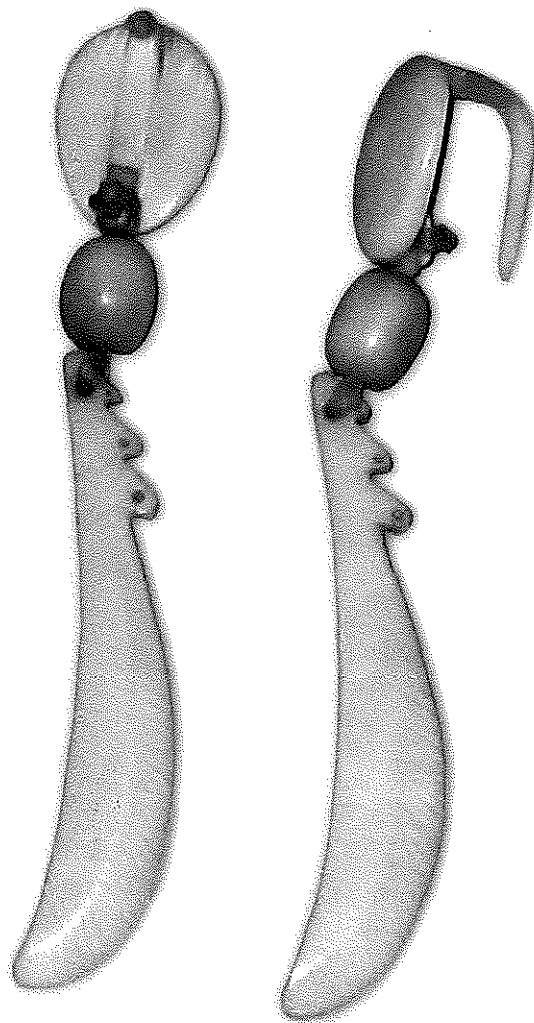


Figure 2: Story knife earrings made by author (Private Collection).

## LEARNING BY DOING

So how does all this relate to material culture? One way to gain a sufficiently detailed understanding of an artistic process (as well as most others) and the local terms in which it is discussed, is to try to learn that process through doing. I am not suggesting that one has to achieve competence, but rather that acquiring at least a beginner's hands-on knowledge will pay big dividends in terms of learning about that process more generally. However, I'm not sure how aware of this I was at the time I started learning various local crafts. If anything, this motivation was only one, among several (poverty, a need for specific tools, a desire for competence) moving me to learn.

Of these, my major motivation to learn some local, gender appropriate craft was that living in a Yup'ik village, I struggled with my incompetence in almost all realms. I used to say that the only skill I brought from my former life that had any local relevance was

that I was competent with a shotgun. Not only was I easily lost when out of sight of the village, but I could in fact be lost within sight of the village. That is, given the complex maze of interconnected shallow waterways surrounding it, it was entirely possible to see the village clearly across miles of marshes, shallow lakes and sloughs and not know the path back to it.

Poverty and our perceived need for ulus also motivated me to make some. My wife Phyllis Morrow was learning to cut fish and needed appropriate tools of her own. And anytime a Yup'ik visitor helped with a cooking or processing task she asked for one to use. One could find ulus for sale, but they tended to be either poorly made, or beautiful and expensive with carved ivory handles. In either case they were unlike the ones I saw people constantly using. Someone showed me a partially roughed out blade and I suddenly understood how I could make them as well. I roughed out blades by grooving both sides of an old cross-cut saw blade with a triangular file, then snapping the steel along those lines to make a rough, trap-

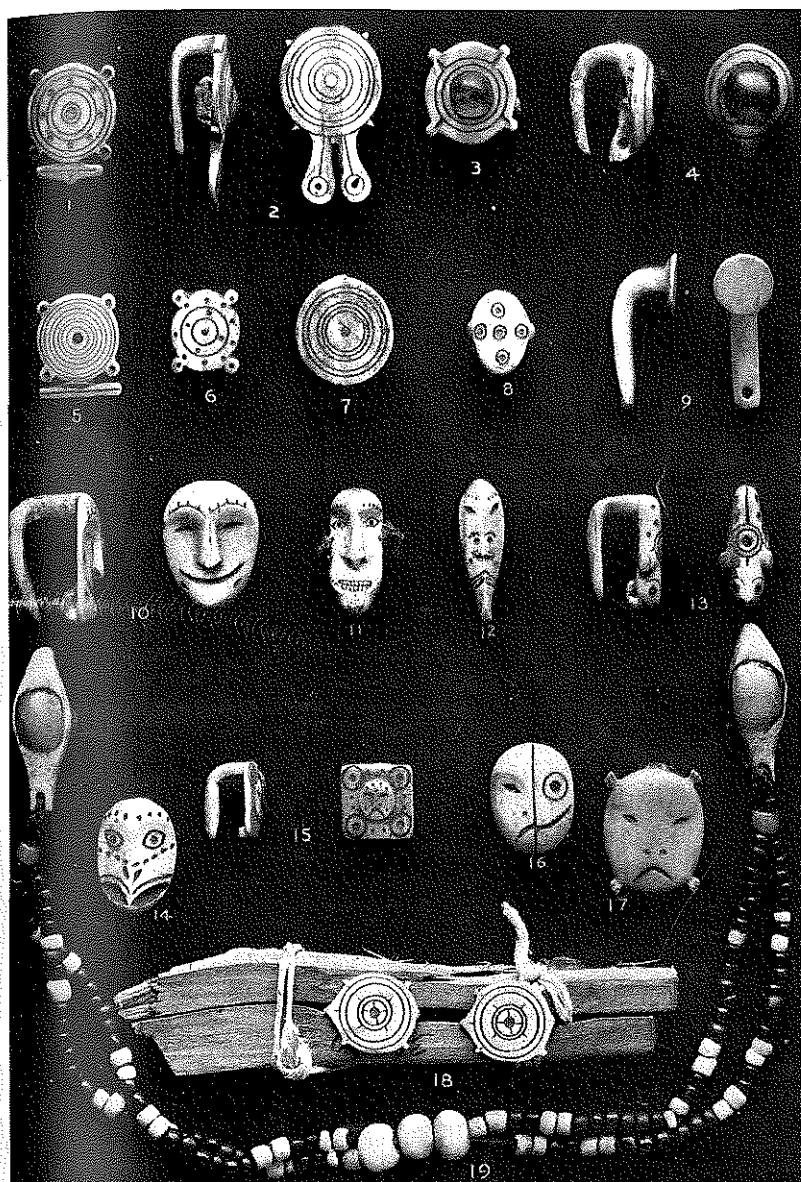


Figure 3. Illustrations from E.W. Nelson (1983: plate XXIV; Fig. 3).

ezoid in exactly the way that Yupiit once worked slate. I then filed the blank to the finished shape and attached a handle. Fifteen minutes with a power grinder would have replaced 10 hours of file work, but I had lots of time. Once I had made some ulus (Figure 1), I realized what appropriate gifts they made for women. One of the issues we grappled with living in a Yup'ik village was how to properly host people, to feed them food that they found really delicious and satisfying, that nourished them. The problem for us was that we needed a stock of subsistence foods to cook from, and it took some time to accumulate these foods, as well as to learn to cook to local tastes. It was the same with gift giving. It took some time to figure out what gifts nourished relationships.

I did my first ivory carving as a substitute art teacher at Bethel Regional High School, and produced a pair of story knife earrings as a present for my wife. They were a bit crude, but well received. I might not have done too much more, except that that pair were misplaced, so I made another (Figure 2). And then I made a replacement ivory earring hook for a broken one. I had reveled in the tremendous diversity of ivory earring hooks shown in E.W. Nelson (1983: plate XXIV; Figure 3) although by this time (the late 1970's) the diversity was much reduced in contemporary carvings. In general, the complex visual iconography that embellished and brought to life thousands of utilitarian objects collected by Nelson was nowhere to be seen. I decided to copy some of those wonderful designs from a hundred years ago (Figure 4). I was aware this was appropriation. I justified this to myself in a number of ways. Yupiit generally were quite interested in, and taken with these earrings, no doubt because of their historical resonances and unusualness. And I wasn't selling them, or producing more than a couple of pairs. At heart, I had a hunger to bring into my life artifacts otherwise only visible in books and museum collections, in exactly the same way I might now brew a Belgian style ale that I can not regularly afford to buy, or bake a French country levain bread not locally available. And in all these cases, while never exactly duplicating the standard, I can strive to come close, hopefully close enough to enjoy the process and the results.

One thing that was slightly odd (or perhaps not) was that I was doing this ivory carving in a tundra village, away from the coast, with no ready source of ivory, and no other ivory carvers, though there were lots of wood workers. I could be the best carver in the village, also, as far as I knew the only one! I acquired my ivory in various ways, including being given a tusk, which had been beach-combed by a non-Native pilot friend and trading for some bits of mammoth ivory, whereas in a more typical coastal ivory carving community, I might have participated in walrus hunts and acquired ivory as part of a crew share (setting legal issues aside).<sup>2</sup>

I continued on an intermittent basis making ivory jewelry and the occasional ulu, learning as I went. I upgraded

<sup>2</sup>According to the Marine Mammal Protection Act, non-Natives may possess beach-combed ivory, but it must be sealed by US Fish and Wildlife Service (USFWS) within 30 days of being found. Ownership of such ivory may not be transferred from one person to another without permission from USFWS. Non-Natives may not hunt sea mammals, receive crew shares of sea mammal parts, etc. There are no restrictions on ownership of fossil ivory.

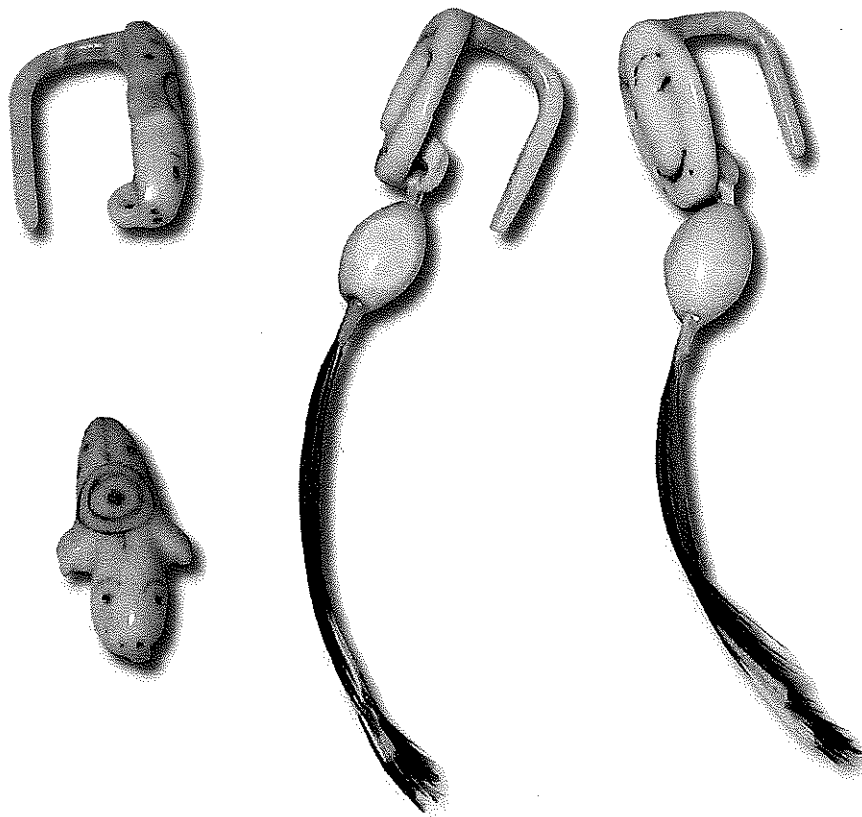


Figure 4: Earrings made by author from E. W. Nelson designs (Private Collection).

my tools over time, getting a set of small files to replace the ignition point file I had used for almost all detail carving. I continued to receive intermittent instruction in two main ways: through suggestions and comments from people who looked at my work, and through conversations with other carvers and examining their work. Once the objects were in use, or being worn, they often drew evaluative comments. I tended to discount polite praise from people, because people are always telling the artist how much they like his/her work, and often they are just being nice. Attempted commissions or compliments given by someone who did not know I was the carver I accepted happily. Friends and acquaintances made suggestions: "sometimes people do it in such-and-such a way, so it doesn't have some problem", which usually I tried, or at least considered carefully. So, for example, I eventually learned to cut out a hidden slot for the blade on ivory ulu handles, rather than cutting a lengthwise saw kerf to take the blade and then plugging the ends of the kerf with additional small pieces of ivory (Figure 5). While more time-consuming, the invisible-slot method reduces both cracking, and the problem of loose end pieces.

Finally, I also talked to carvers when the chance arose, generally at events like Christmas Bazaars. Because of my own carving, I was much more cognizant of details: noticing how the carver used the white outer layer

of the ivory to best advantage; how even and careful the polish was; how well both the lines and pigment were applied on any scrimshaw work, whether a piece was finished in 3D, or only on the upper surfaces; how well the lines flowed, etc. I was also much more able to ask specific questions about materials, techniques and equipment, because of knowledge gained by hands-on experience.

Other Yup'ik carvers were always quite encouraging, exactly like Yupiit were with my subsistence efforts. It might be that they did not see the world as a zero-sum game, so that my carving didn't reduce their perceived market, in the same way that my fishing was not seen as reducing their catch and perhaps even increasing it, given the Yup'ik understanding that fish come because people catch them/need them. Or it may just have been largeness of spirit.

## YUP'IK ICONOGRAPHY GOES PUBLIC

In the early 1980s Phyllis Morrow and I started working as applied anthropologists, developing Yup'ik language curriculum for high school students. We moved from thinking and sometimes writing about culture, to expressly teaching about traditional and contemporary aspects of Yup'ik culture.

It was clear, on a variety of fronts that some more general Yup'ik cultural revival was occurring. For example, people seemed to be more willing to talk about the pre-Christian past, precisely as that past was becoming more distant and less threatening to present Christianity. The first masked dance in many years was held in Bethel in 1982. The Catholic Church was incorporating indigenous symbols in celebrations of Mass (Fienup-Riordan 2000). William Fitzhugh and Susan Kaplan co-curated an exhibit from the E.W. Nelson collection at the Smithsonian, as well as the catalog *Inua: Spirit World of the Bering Sea Eskimo* (1982). Many Yupiit saw that exhibit when it came to Bethel. After E.W. Nelson's book was reprinted in 1983 it again became locally available.

We developed a Yup'ik High School curriculum for the Lower Kuskokwim School District, working closely with a group of Yup'ik high school teachers and aides (Morrow and Hensel 1987). One priority they set was to

include materials about traditional Yup'ik ceremonies. They felt that they, and certainly their students knew very little about this past. We developed a month-long unit to showcase this pre-Christian past and its iconography (including contracting with Elsie Mather for a pamphlet which became the book *Cauyarnariuq* ("Time for Drumming") on traditional Yup'ik ceremonies (Mather 1985). To start off that unit we developed a board game (Yupiit Qaraliit) to teach students how recurring visual themes were combined and recombined to create this densely layered expressive art (Hensel and Morrow 1985). I admit to having had some hope that we would again see local handwork for sale as well as in every day use embellished with these shapes and figures, and that these high school students would grow up with these designs as part of their artistic vocabulary.

Instead, as so often happens, things have gone in another direction. I have not seen any more of that visual iconography in use on things Yupiit hand make for sale or for themselves, but it has become omnipresent on everything from phone books to book bags. If what we were in part doing was trying to re-contextualize this lost patrimony in the sense of bringing it meaningfully back to life, then what has happened is that it has been de-contextualized from its shamanic and ceremonial meanings and reduced to marking primarily Yup'ikness, or even Alaska Native-ness. So, a silkscreen image of a mask that was made to celebrate the Bladder Festival of 1879 in Ikogmut, and that may have celebrated the interaction of one man with one seal, now primarily marks Yup'ikness or Native-ness. So, for example, local Native for-profit and non-profit corporations, the Bethel Council on the Arts and local businesses routinely use this iconography on logos and letterheads, apparently because it is local, and 'cool' in a variety of ways (attractive and artistic, indigenous, etc.) This kind of narrowing and channeling of meaning often happens in cross-cultural interactions, where parts (a cross, a crescent, a red sun, a red star) are both given and taken to represent wholes. And this process is generally circular, as these meanings and symbols are imported and exported simultaneously, with new meanings accruing on both fronts (for further discussion, cf. Hensel 1996:87-96, 179-186; Lee n.d.; Mason 2002).

But what about the ulus and ivory hooks? Ulus are obviously still being made. They continue to be ubiquitous, wherever animals or fish are cut, split or processed. They are also ubiquitous items in the tourist trade, both in locally produced and commercially produced versions.

The trend seems to have been away from heavier blade materials, such as old crosscut saws and flat shovel blades towards lighter ones, such as current carpenter's hand saws (or stainless steel in the commercial versions). But I have seen some beautiful ulus made in the last few years.

Ivory hooks are another matter. They went from being "the" earring of adult women, and some teenagers, to being less common. One thing that has affected the use of ivory hooks, according to some people (including my wife, who kept breaking hers this way) is the introduction in the 80's and 90's of household telephones. If one forgets and holds the receiver, "no hands" in the crook of one's shoulder, it is easy to break a hook. And this problem is probably exacerbated by another change that I believe has occurred, a tendency to wear a greater variety of earrings, many not ivory hooks. Twenty-five years ago it seemed there were many Yup'ik women who wore their same favorite hooks day in and day out. Putting them on was part of getting dressed. They might wear other earrings occasionally, but certain ones such as those of Teddy Moses, a famous Toksook Bay carver, were a staple. Typically, the hook part of such earrings had a larger cross section, which is much stronger, but necessitates stretching out the holes in one's ears. If one wears regular metal wire or stud earrings for a few days, the holes start to close and one has to re-stretch them, using toothpick sections, or tobacco stems, often lubricated with antibiotic cream. A solution is to get smaller diameter hooks made (or have someone thin down the ones you have), but this makes those hooks much more fragile. So wearing a variety of earrings, of which only some are ivory hooks, may mitigate against ivory hooks also. Today, most ivory hook earrings I have seen in use have larger, more durable hooks, supporting the idea that these may likely be the staple earrings of their wearers (or that they have other, similar sized pairs).

## MEN'S CRAFTS

I think there has been a general decline in the number of men making crafts. For example, the sets of arrows or harpoons, spear throwers, etc., ranging from miniatures to lifesize, which used to be so common at craft sales, are now quite uncommon. At the April 2003 Messenger Feast in Kotlik, there were reportedly complaints from the Calista Elders Council that so few of the gifts were hand made (James Barker, personal communications, May, 2003).

<sup>3</sup>This is a perfect illustration of a point made by McLuhan (1964) about obsolete technology becoming art, such as Navajo rugs (suggested by Molly Lee).

Similarly, when I first went to the YK Delta in the late 1970s, it was unusual to be served *akutaq* (Eskimo ice cream) with anything but a locally made wooden ladle. These ladles just seemed 'necessary,' like the saucers put under the cups of older people, so they could pour their tea into the saucer to cool it for drinking. When I was doing fieldwork in three Yup'ik villages in 1996, I noticed that this had changed. I was invariably served *akutaq* with a plastic or stainless steel spoon, but I often noticed wooden ladles, a few new, more old, hanging on peoples' walls.<sup>3</sup> In two cases I knew people well enough to ask about this (Questions and Answers again). The reply I received was that they were no longer used because all of the ladle makers were dead, and the women didn't want them broken by use. They were keepsakes that had somehow made the transition from utilitarian objects to decorative ones. When I pointed out that one of the ladles in question had been made by the woman's husband, who was very much alive, she repeated that they were too precious. At the Messenger Feast mentioned above, the one conspicuously handmade gift exchanged was a wooden ladle that the Stebbins 'King' made for the Kotlik 'Queen.'

When I returned from that fieldwork, I made three ladles to send as thanks to women who had been particularly kind to me. When I saw one of those women at a bilingual conference, I asked her if she was using it. She said, no, it's much too light, it'll get broken. My assurances that it came with a guarantee of replacement should it fail were to no avail (though another woman who was there suggested I make them out of birch, because they would be stronger). On a visit last fall I saw the other two ladles I had made. One was hanging on the wall, never having been used. The other was used to serve me more *akutaq*, and was well colored with blueberry juice over the ocher stain I had applied. Its owner said that the *akutaq* just tastes better served with a wooden spoon.

If I step further back from the issue of men's craft, it would seem that there has been a general shift from local production of many of life's essentials to manufactured production of them. So, for example, 25 years ago the first 'Housing Authority' houses were a new introduction, and most village families lived in houses they built themselves. Similarly, though there were lightweight aluminum skiffs,

the standard boat was the locally made plywood skiff, and indeed, men could often tell what village someone was from by the shape of their boat at a distance. Now the welded aluminum skiff has become the standard larger boat. Similarly, as snow machines replaced dog traction sleds became much less intricate, and, in many communities, both are replaced much of the time by 4-wheelers and trailers. This is true of what was 'women's work' as well, where similar shifts in sewing, baking and other home production can be observed. The clear exception, as Lee (2002) has pointed out, is coiled basket-making (along with doll making by a few women in a few communities). Basket-making is still a flourishing art, even though it probably pays less than minimum wage to the artists. So why has basket making continued while woodworking has largely ceased? Four related reasons come to mind, all economic. First, women in marriages often have limited control of the family finances (Hensel 1996:126-34). Money from basket sales may be more valuable to women because it is less subject to male knowledge or control. Second, even if basket-making pays poorly, it pays something, can be done simultaneously with household tasks such as cooking and child minding, and does not disrupt the household like a woman's wage labor might. Thirdly, it is an 'open entry' occupation. Unlike the few pink collar jobs available, basket-making requires neither the symbolic capital of credentials and certificates, nor powerful family connections. Finally, the demand for baskets may be considerably more elastic than that of carved objects (excluding masks) because they are seen as 'art,' not utilitarian objects. One full time carver might saturate the Delta with ladles. The cost-per-unit difference may figure in here as well. It may be less trouble to sell one \$450 basket than fifteen \$30 ladles. This is an area where further research is warranted.

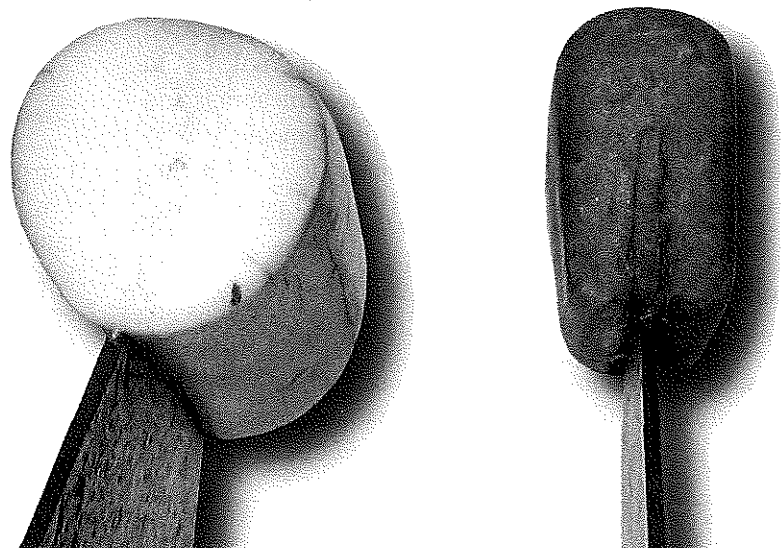


Figure 5: Ulu handles made by author (Private Collections).

One result of this general shift to manufactured objects is that my skills, as in the ladle example above, are old-fashioned, and in a way obsolete. Nowadays for Yupiit, like non-Natives, ladles come from the store. Ladle making has historical interest (like Colonial treen-ware work for non-Natives) but little contemporary relevance. And did I learn to just look first and ask questions later? Not really, or at least not as my first thought. I am reminded of the time our outboard started making strange noises. I went to shore, and pulled out the manual, turned to the trouble-shooting section, and started trying to diagnose the problem. While I was doing this, a Yup'ik friend came by and suggested that we should pull off the cover and take a look. There were the parts causing the problem, laying around inside, and detached from where they should be. But, on the other hand, I cured my competence problem by becoming an academic, where often word knowledge suffices, and if you don't have an answer you can always recommend a book to someone that might be helpful. This was perhaps pre-ordained, as the partially built bookcase being mistaken for a sled in my title indicated. Even living in a village, we had more books than places to store them, and seemingly more need for a bookcase than a sled.

And at this point in my life I find both irony and comfort in the recognition that the constant pace of cultural change has marginalized some of my skills just like those of my Yup'ik age-mates. If a college education is what remains after one has forgotten all of the specifics, perhaps the same is true of an ethnographic education, where understanding *how* to learn remains useful long after the details of *what* was learned are relegated to the midden of history.

## ACKNOWLEDGEMENTS

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# RE-COVERING IT: THE ANAKTUVUK PASS KAYAK PROJECT

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**Abstract:** This paper describes a co-operative project undertaken by the University of Alaska Museum of the North (UAMN) in Fairbanks, the Simon Paneak Memorial Museum in Anaktuvuk Pass, and the community of Anaktuvuk Pass. The aim is to re-cover a Nunamiut kayak (*qayaq*) in the UAMN collection. Here, I describe the history of the kayak and that of the current project, the individuals involved, and the project goals. As a collections manager, I also consider some of the ethical issues of “fixing” ethnographic museum objects.

**Keywords:** Conservation, Material culture, Nunamiut

## THE HISTORY

The UAMN kayak re-covering project took place in the village of Anaktuvuk Pass, which is located at 68° 08' N, 151° 45' W, ca. 400 km northwest of Fairbanks, in the Brooks Range (Orth 1971 [1967]:74). Anaktuvuk Pass is a Nunamiut Eskimo village, with a current population of approximately 300 people. Villagers are descendants of small bands of families who occupied the central Brooks Range for the better part of three centuries. In the late nineteenth and early twentieth centuries, however, a precipitous decline in the caribou population forced most of them to move to the Arctic coast where they became involved in the whaling industry, reindeer herding, and later the fur trade. By the late 1930's many had moved back inland; there three small bands established themselves in the upper Killik, Chandler, and Ulu-Sagavanirktok River drainages (Spearman et al. 1982:2). One of these groups disbanded in 1942, scattering to Wiseman and Fairbanks, and the other two joined at Tulugak Lake in the Anaktuvuk valley in 1949, later settling 26 km south at the newly established village of Anaktuvuk Pass.<sup>1</sup>

The language that many people in Anaktuvuk Pass still speak is a dialect of Iñupiaq, though their cultural traditions diverge from their coastal relatives, partly because of the differences in environment and available resources. In earlier times, land mammals, fish and to a much lesser degree, local vegetation made up the Nunamiut diet, and hunting techniques included the stan-

dard snaring techniques common to many Alaska Native groups, as well as a specialized caribou hunting tradition that included corralling caribou on land and hunting them in lakes from kayaks. The Nunamiut of Anaktuvuk Pass have always relied heavily on caribou, the most abundant animal in this area of the Brooks Range (Amsden 1977; Campbell 1970; Gubser 1965). Caribou have served many purposes to the Nunamiut, from raw materials for clothing and dwelling covers to the main source of food.

The roots of the Nunamiut kayak project can be traced back to 1944, when six Nunamiut families gathered together to conduct the last known kayak-based caribou hunt at Little Chandler Lake northwest of Anaktuvuk Pass in the Brooks Range. The Nunamiut had used firearms for decades by this time, but because of American involvement in World War two they feared that cartridges and reloading supplies might be rationed. As a result, the families were concerned about their ability to sustain themselves over the winter. They concluded that building a small fleet of kayaks to revive a hunting technique that several of the elders in the group had either observed or participated in as youngsters seemed the most viable way to harvest the needed caribou (Spearman n.d.:23-24).

Hunting caribou with the aid of a kayak is certainly not unique to the Nunamiut, and appears to have been widely practiced in the Arctic. Many Canadian Inuit popu-

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<sup>1</sup>For the history of the Nunamiut, cf. Amsden 1977, Campbell 1998, Gubser 1965, Hall, Gerlach and Blackman 1985, Spearman et al. 1982.

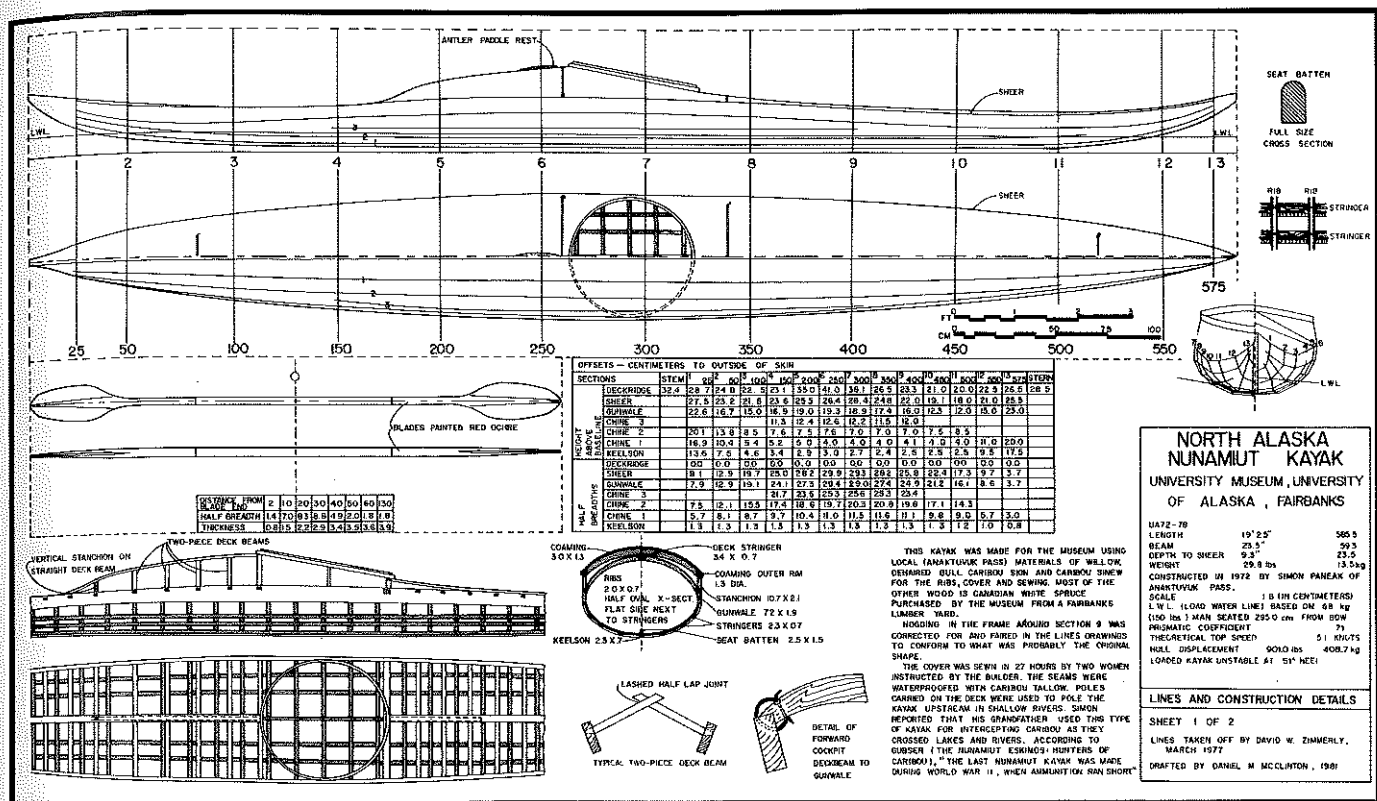


Figure 1: Schematic drawing of Nunamiut kayak. © Canadian Museum of Civilization, David Zimmerly Collection, catalogue no. IV-D-28M.

lations used this same technique at water crossings, utilizing *inuksuk* (cairns, "like a man") and/or human drivers to maneuver caribou herds into the desired position (Adney 1964; Arima 1975; Jenness 1970 [1922]; Turner 2001 [1894]). In Alaska, too, the tradition of hunting caribou in this way was not uncommon and in pre-gun times was widely practiced by the Nunamiut among the many lakes that dot the north face of the central Brooks Range (Spearman n.d.:18).

When spring arrived in 1944, twenty-two Nunamiut from several families set up camp along the southern edge of Ikagiak Creek, which drains into nearby Little Chandler Lake. There the families worked together to construct a small fleet of three kayaks and their associated tools and equipment. In addition to the kayaks, inuksuks were set up in a pattern (a "drive line") (Spearman n.d.:34) to direct the caribou into the lake. As the caribou entered the Chandler valley from the foothills to the north, the hunters allowed the early and lead herds to pass, as is their custom,<sup>2</sup> then all participants took action. Young children were quieted as the teens and the remaining adults took positions along the inuksuk line, while the hunters

took to their boats. The people along the driveline acted as a "human wall," forcing the caribou toward the water. The line slowly moved and pivoted toward the water, forcing the caribou in the same direction. The final phase of driving the herd consisted of individual drivers waving strips of dried grizzly bear intestine above their heads, which made popping and crackling noises. Combining this with the noise and arm waving of the other participants, the herd moved steadily and quickly into the water, where the men in the kayaks waited (Spearman n.d.:43). Over the next hour or so, the kayakers thrust their spears at the swimming animals, following them to the far shore of the lake. Despite repeated questioning of those who participated<sup>3</sup> there is no good estimate of the number of caribou taken during this hunt. "Lots" was the term most frequently used (Spearman n.d.:46-47).

On the second day of the hunt, one young and inexperienced kayaker capsized and nearly drowned, had it not been for Simon Paneak and another hunter's assistance. Only a few caribou were taken that day. On the third day of the hunt, the caribou seemed to have been spooked and never made their way to the lake.<sup>4</sup> Despite

<sup>2</sup>Nunamiut oral traditions emphasize this practice. There are many stories that tell of over-eager hunters who prematurely intercepted the lead herds and neutralized the migration. They tell of an "alarm scent" that caribou have between their hooves that, in the event of being frightened, will mark the ground and warn following animals to find a safer route (Spearman n.d.:38).

<sup>3</sup>Grant Spearman took a group of people back to the site of the 1944 hunt and conducted interviews in both 1985 and 1990 to document this event.

<sup>4</sup>According to tradition, the caribou know if a hunter has been involved in an accident, or killed, and will avoid that area (Spearman n.d.:53).

this disappointing end to the three-day event, the people were elated with their overall success. At the end of the last recorded kayak-based caribou hunt the Nunamiut had ample meat and skins, and, just as important, everyone had saved their precious ammunition (Spearman n.d.:53).

### THE IDEA

In 1971, UAMN, recognizing that many traditional Alaska Native skills were threatened with extinction, commissioned a Nunamiut-style kayak from Simon Paneak of Anaktuvuk Pass.<sup>5</sup> After consultation with Paneak, the museum purchased Canadian white spruce from a local lumber mill and had it delivered to Anaktuvuk Pass in October of that year.<sup>6</sup> It was too cold to begin construction by then, so in the spring, once it was warm enough, Paneak began assembling the wooden frame (University of Alaska Museum of the North 1972). Using local willow to supplement the spruce, Simon constructed the frame by lashing the wooden ribs and stringers together with babiche (rawhide; *quniguq*), using wooden pegs and splints for fine-tuning (Figure 1). The kayak measures 5.85 m and has forty-five ribs (twelve of which Simon chose to make from local willow, positioned visibly inside the cockpit) and seven stringers made from the milled spruce (Figure 2). The craft weighs only 13 kg.<sup>7</sup> The two women who made the caribou skin cover used the hides of approximately four fall bull caribou. It took Susie Paneak, Simon's wife, and Ellen Hugo about twenty-seven hours to sew the cover (Zimmerly 1986:70). The specialized double waterproof seams were coated in caribou tallow to further waterproof them (Figure 3).<sup>8</sup>

On November 13, 1972, after several months of coordination carried out through letters and telephone calls, the finished kayak was crated in Anaktuvuk Pass and delivered to Fairbanks by a Naval Arctic Research Laboratory plane (University of Alaska Museum of the North 1972).



Figure 2: Simon Paneak and kayak frame in 1972. Credit: Lewis Binford.

### THE PROBLEM

Once added to the collection, the Nunamiut kayak almost immediately developed problems. In 1972 the Museum was located in Signers' Hall, an academic building on the University of Alaska Fairbanks campus. Constructed in the mid-1930s, Signers' Hall's original heating system had never been updated. As a result, when the kayak arrived in Fairbanks in November, it was placed in a steam-heated building with low relative humidity.<sup>9</sup> The caribou skins of the kayak cover were "green" (freshly taken and prepared) (University of Alaska Museum of the North 1972) and almost immediately, the tightly sewn cover began to dry out and shrink on the wooden frame.

In an attempt to slow this process, museum personnel removed the kayak to the recently constructed Elmer Rasmuson Library, where it was placed in a room with some environmental controls, known thereafter as the "Kayak Room." The kayak appears to have remained there until 1986, when it traveled to the Alaska State Museum in Juneau for a two-year loan. On returning to Fairbanks, it was installed in the permanent gallery of the newly constructed University of Alaska Museum building on the West Ridge of the campus, where it has been housed until its temporary return to Anaktuvuk Pass for repair in May 2003.<sup>10</sup>

Although the new museum building was equipped with environmental controls, by the time the kayak was moved there, no amount of environmental control could rectify the damage. The skin covering was ripped over the majority of its surface (Figure 4). Several seams had been deliberately cut in an attempt to lessen the tension. Many of the bent spruce ribs had cracked and two of them protruded through the skin covering. Furthermore, the skin covering itself was dry and extremely brittle overall. It was this poor condition of the kayak that led Simon Paneak Memorial Museum (SPMM) curator Grant Spearman to develop the idea of re-covering the boat.

<sup>5</sup>Simon Paneak (born, 1900, died, 1975) was a leader who served as a guide, collaborator and informant to a generation of arctic researchers, including Laurence Irving, Helge Ingstad, Robert Rausch, Jack Campbell, and Edwin Hall. He also constructed numerous items of Nunamiut material culture for the ethnology department at the UAMN. (cf. Irving 1976).

<sup>6</sup>Traditionally local willow and spruce would have been used for construction of the frame.

<sup>7</sup>In overall appearance, the Nunamiut kayak most closely resembles others from Northern Alaska, specifically the Kotzebue Sound/Point Barrow style (Zimmerly 1986:64). The Anaktuvuk Pass style is a bit longer and slightly narrower than the Kotzebue/Barrow boats. Both have a reverse sheer (the frame becomes slightly concave, reversing the typical convex line of the profile) at the cockpit and a ridged deck just to the front of the cockpit. The major difference is that the Nunamiut boat curves upward at both the bow and stern.

<sup>8</sup>Files deposited in the UAM Ethnology Department mention photographs of this process, although at the time of this writing, these images have not been located.

<sup>9</sup>Although the outside relative humidity in Anaktuvuk Pass at this time of the year would have also been very low, a kayak normally would not have been made in the winter and so the initial drying and shrinkage that would have occurred would not have been so dramatic.

<sup>10</sup>The current building that houses the museum is known as the Otto W. Geist building and was completed in 1980.

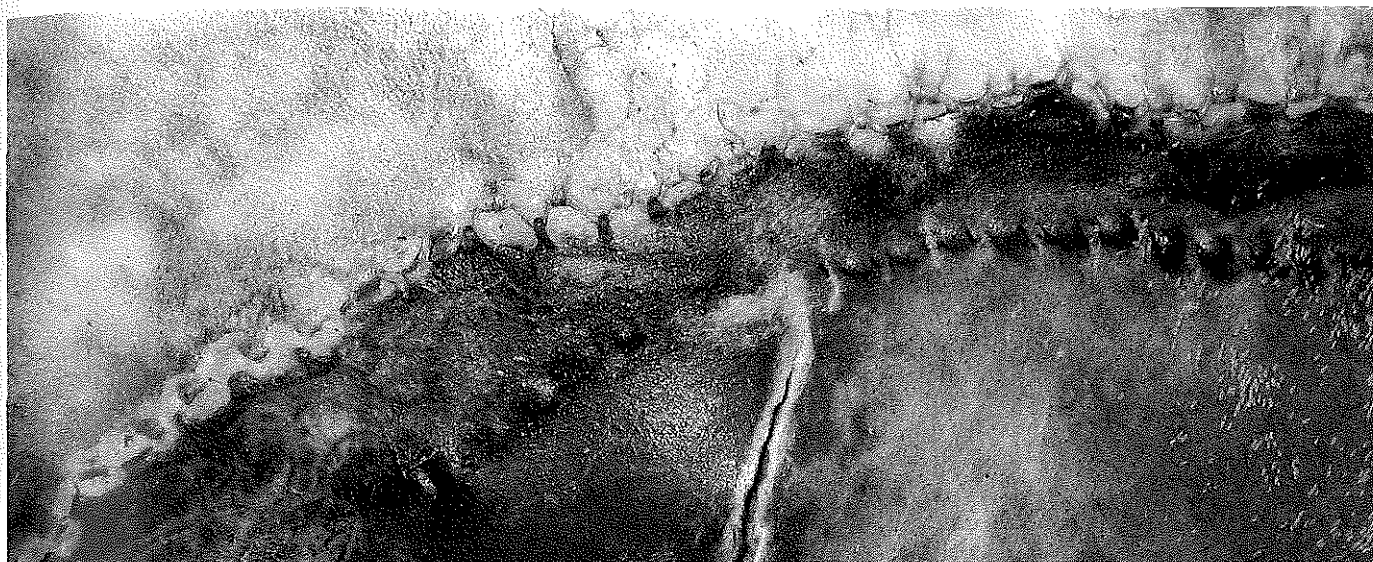


Figure 3: Seam sealed with caribou tallow on original skin covering. Credit: Angela Linn.

Spearman was also concerned that the process of Nunamiut kayak construction be documented for the future.

## THE PROJECT

About 1996, Spearman suggested a co-operative SPMM and UAMN project in which the kayak would be returned to Anaktuvuk Pass temporarily for re-covering. This proposal, sidelined for more than five years, served as the basis for the current project. At the time of the original proposal, Ethnology curator Molly Lee had been newly hired (1995) and project funding sources and Nunamiut personnel had not been identified.



Figure 4: Kayak damage as seen on exhibit at the University of Alaska Museum. Credit: Angela Linn.

During the summer of 2002, Molly Lee invited Roosevelt ("Roosy") Paneak, son of Simon and Susie, to visit UAMN to see the kayak and the associated material (two long poles and a paddle). She informed him of the Spearman proposal; conversations about the old re-covering project were revived, and we began to develop the project anew. Roosevelt Paneak's primary concern

was timing: with each passing year Nunamiut elders with the critical knowledge of kayak-building and skin sewing skills are fewer. In fact, even when Simon Paneak made the kayak he had to instruct the skin sewers in the art of the essential double-waterproof stitching, which they had apparently forgotten. And in the intervening thirty years, there has been no revival of this specialized knowledge. This made the timing all the more critical.

With the interest and motivation of Roosevelt Paneak, and the funding and resource knowledge of Molly Lee, we went about contacting private donors, foundations and local governing bodies. Several months later we secured the necessary funds for the project.<sup>11</sup>

## THE ACTIVITIES

The kayak re-covering project has involved cooperation and collaboration with a variety of complicated activities, among museum departments and in the wider Fairbanks community as well as in Anaktuvuk Pass. First, the kayak was removed from its current location in the permanent exhibit gallery. Exhibits Preparator Steve Bouta constructed a crate to ensure safe passage of the kayak to Anaktuvuk Pass. In Fairbanks, Everts Air Cargo donated the cost of shipping the craft aboard one of its fleet of DC-6 cargo planes as well as airfare to and from Anaktuvuk Pass for project participants.

On its arrival in Anaktuvuk Pass, where photographer James Barker and filmmakers Leonard Kamerling and Takashi Sakurai were already in place, the kayak was greeted with interest and enthusiasm. Once

<sup>11</sup>Financial contributors include: Arctic Slope Regional Corporation, Dinah Larsen, The Evelyn Stefansson Nef Foundation, Everts Air Cargo, the City of Anaktuvuk Pass and Nunamiut Corporation.



Figure 5: Residents of Anaktuvuk Pass examine the kayak at the open house on May 17, 2003. Credit: Angela Linn.

off-loaded it was moved to its temporary home in the Nagsragmiut Tribal Council's snowmachine shop, where it stayed in its crate for twenty-four hours to acclimate.



Figure 6: Roosevelt Paneak watches Esther Hugo scrape a caribou skin on the log while Takashi Sakurai films. Credit: Angela Linn.

Thanks to the generosity of the Nunamiut Corporation and the City of Anaktuvuk Pass, the residents of the village were invited to an open house and kayak-viewing at the snowmachine shop where the activities took place (Figure 5).

Work on the kayak began on Monday, May 19, 2003 with the removal of the old skins and an examination of the wooden frame by project participants. Three skin sewers had been hired to do the work: Ruth Rulland was the lead sewer, along with Molly and Lela Ahgook (see Blackman article this issue). The second day Rhoda Ahgook replaced Ruth, who had an appointment in

Fairbanks. At least two kayak ribs had been identified as cracked due to the stress of the drying skins. Once the skins were off, we realized that in fact, nine ribs were broken and several others showed cracks. These had to be repaired before the new skins were sewn onto the frame.

Five caribou skins were purchased from Ruth Rulland with money donated by Arctic Slope Regional Corporation. The caribou hides had been preserved in Ruth Rulland's freezer, so the women cleaned them after they had been thawed in large plastic garbage cans filled with warm water. They removed the last bits of hair that were still attached, and then scraped them clean, using their ulus (women's knives) and scissors. One particularly dirty skin had a thick membrane that was difficult to remove, and Rhoda Ahgook suggested bringing in a log to place the skin on for cleaning. The log provided a hard surface over which the women could scrape the hide with a kitchen knife held with both hands. This speeded up the process (Figure 6).

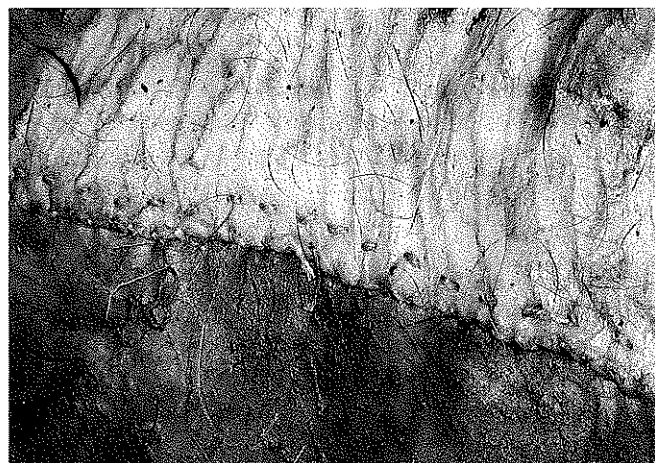


Figure 7: Double-waterproof stitch on new skin covering. Credit: Angela Linn.



Figure 8: Test fit of the new skins. Credit: Angela Linn.

Once cleaned, the women sewed the large skins together using the double-waterproof stitches (Figure 7). The best skins were placed around the cockpit of the boat and during the whole process, the old skin covering was kept within easy reach for referencing. On the third day, the new skin cover was ready to be fit loosely onto the frame to check its size and determine where patching would be necessary (Figure 8).

The seamstresses worked exclusively with caribou sinew (in this case, tendons from along the back of the neck of the caribou), which they prepared during the sewing process. To make this traditional material, they used two methods, either twisting two strands together, or preferably braiding three small strands together. The braiding technique allows for two lengths of tendon to be combined into a single longer piece.<sup>12</sup>

At the same time the women were sewing, the men began repairing the frame. On Monday afternoon Johnny Rulland visited the wooden frame and consulted with

Roosevelt Paneak, who was directing the project, to determine how to fix the nine broken ribs. Wednesday morning, Johnny returned with several wooden ribs that he had made. With the assistance of Jack Ahgook, Jr. and Roosevelt, Johnny removed the broken portions of the ribs, replaced them with new ones shaped from local, fresh willow, and lashed them in place with wax-coated twine (Figure 9). The old wooden pegs were re-used to hold the splinted ribs together and new ones were manufactured to affix the newly constructed ribs to the frame.

With the frame secured, the women began fitting on the new cover. Once the necessary adjustments were made (removing excess skin and patching open areas) it was ready to be sewn onto the frame. First, the frame was laid atop the skins, then the skins were wrapped up over it and the women sewed extra threads to the inside surface to help cinch the skins tight. These threads catch most of the thickness of the skin and help hold it tight to the wooden frame without penetrating the skin's exterior. Next, the women sewed shut the openings along the deck of the boat. Finally, on the fifth day of work, the old ochre-painted willow rim was fitted around the cockpit and secured with a roll of babiche that Simon Paneak had foresightedly stored in the cockpit. The kayak rested in the snowmachine shop to dry, and then was re-placed in the crate.

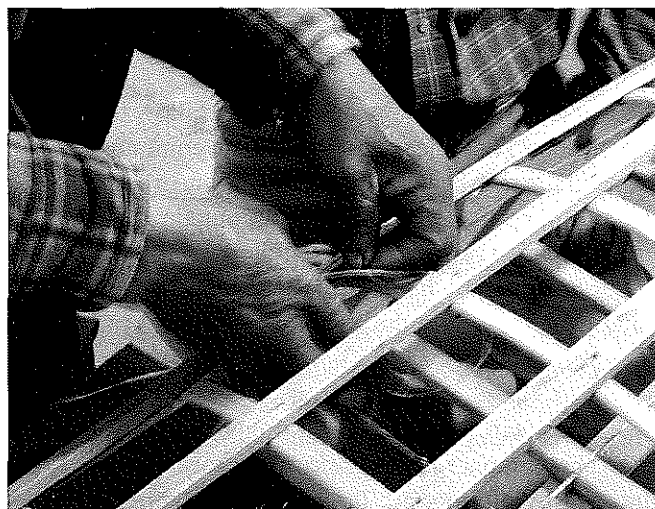


Figure 9: Johnny Rulland wraps the repaired ribs with waxed twine. Credit: Angela Linn.

Over the weekend of June 28-29, 2003, the community of Anaktuvuk Pass came out to view the inaugural floating of the renovated kayak. Before this could happen, however, the seams needed to be sealed. Traditionally, women would have chewed caribou tallow in order to obtain the proper viscosity. The saliva and warmth of

<sup>12</sup>The tendons from the back of the caribou are approximately 40 cm in length. When twisted, this makes for a very short length of thread. By braiding two lengths together, an average thread length of 75 to 80 cm could be obtained, which made the sewing process proceed more smoothly.



Figure 10: Ruth Rulland spreads caribou tallow on the skins to waterproof the covering. © James H. Barker 2003.

the women's mouths helped prepare the tallow for spreading (Roosevelt Paneak 2003, written communication). None of the skin sewers wanted to volunteer for this task. As an alternative, the tallow was put through a meat grinder, which produced small cylinders of fat that were easier to spread by hand and worked into the seams that had opened up when the skin covering dried and shrank (Figure 10).

Roosevelt received a last-minute kayaking lesson from photographer James Barker, the morning of the floating, and by afternoon a large crowd gathered to watch the newly re-covered kayak placed in water for the first time in thirty years.<sup>13</sup> Roosy took the first trip around Eleanor Lake in Anaktuvuk Pass, showing a bit of apprehension, but then settled into the comfort of the boat that

his father built (Figure 11). Members of the community were invited to take a spin around the lake, or for the more timid, just to have their pictures taken while sitting in the boat by the shore. Even the Everts Air pilot who had brought Leonard Kamerling into town that morning took advantage of this unique experience and paddled the kayak on the lake.

Finally, despite the protests of some members of the community,<sup>14</sup> the kayak was re-packed after drying out and eventually brought back to Fairbanks, where it was quickly wrapped in polyethylene sheeting in order to monitor it for potential infestation.<sup>15</sup> After the mandatory waiting period, the kayak was unwrapped and re-hung in the main gallery.

<sup>13</sup>After Simon completed the original kayak in July of 1972, he also floated the boat in Eleanor Lake (Roosevelt Paneak 2004, written communication).

<sup>14</sup>At one point before the kayak was re-packed into the crate, several residents of Anaktuvuk Pass voiced their desire to keep the boat in the village, permanently. As it often happens, once a community is reunited with unique cultural artifacts relating to their past, they want the object to stay in their village so that everyone can enjoy this part of their history. However, Roosevelt Paneak was able to remind everyone how and why the boat exists in the first place. This provided the impetus for the potential manufacturing of a new boat designed for the village, utilizing the knowledge and experience gained on this project, combined with the existing skills of village elders.

<sup>15</sup>The skins that were used to cover the kayak are un-tanned and as a result, are a prime food source for the black carpet beetle. UAMN's Integrated Pest Management (IPM) policy stipulates that all non-tanned skins must be frozen at -40 degrees for a minimum of 48 hours. The kayak, however, is much too large to fit into the fumigation chest freezer, so it was monitored for 45 days, the life-cycle of the beetle.



Figure 11: Roosevelt Paneak rests while paddling around Eleanor Lake. © James H. Barker 2003.

## THE GOALS AND PRODUCTS

Through the activities described above and the work of the participants, UAMN has a newly re-covered kayak to present to the thousands of visitors who come to the museum every year. It is currently exhibited in the main gallery of the museum; however, our future plans for the boat involve something special.

Part of the \$31 million expansion and renovation that is currently underway at the museum is the creation of the *Rose Berry Alaskan Art Gallery*. One section of this gallery is labeled "Art as Process" in which we examine the process an artist undertakes in the creation of a work of art. Typical fine art objects (paintings, sculpture, photography, etc.) will be placed in juxtaposition to ethnographic works. One component of the ethnographic section will look at Yup'ik basketry and the creation of these popular items. The Anaktuvuk Pass kayak will stand

in contrast to showcase the cooperation of men and women in producing a utilitarian object that can also be looked at as a work of art. The photographs of James Barker will illustrate the various stages of construction, from hunting the caribou, the preparing of the skins and sinew, the skills needed to make the wooden frame and, finally, to the fine work of the skin sewers. Portions of the Kamerling/Sakurai film will also be shown in order to help visitors understand the process and significance of this kayak.<sup>16</sup>

Of equal importance is the educational component to this project. We hope to compile materials for the production of an educational unit focusing on the Nunamiut kayak, its traditional manufacture and use, for airing in schools and in both of the participating museums. The University of Alaska Museum of the North and the Simon Paneak Memorial Museum will jointly produce a scholarly publication about the kayak.

<sup>16</sup>It is also the author's hope that the various parts of the boat will be labeled using Nunamiut terminology and systems of measurement for the assorted components.

A very important part of this project is this process of transmitting traditional knowledge to the younger generation. The original plan included the participation of students from the Anaktuvuk Pass School in interviewing skin sewers and wood workers, as well as possible hands-on work on the skins and wood for themselves. The lure of lucrative summer jobs proved too strong a distraction and this part of the project was not realized. There were, however, interested youngsters of varying ages who came to the shop to observe the work on an informal basis (Figure 12). They witnessed the revival of a very old skill and our hope is that their observations will help bring back interest in the construction and use of kayaks and kayak building in the future.<sup>17</sup>



Figure 12: Violet Kakinya and Lillian Weber enjoy themselves while watching the work on the kayak. Credit: Angela Linn.

## THE QUESTION

When the issue of re-covering the Nunamiut kayak first came to my attention, I was dismayed. I immediately questioned the ethics of replacing so significant a part of an ethnographic object with new materials.<sup>18</sup> As the manager of a large and valuable ethnographic collection, my job is to ensure the safety of the 14,000+ objects in my care. In large part, the safety of the collection depends on maintaining temperature, relative humidity, and light exposure, as well as reducing the potential for dam-

age though direct handling of the objects. So the idea of removing a unique and irreplaceable object from the relative safety of our climate-controlled building, and sending it to a rural village to be handled and worked over, was chilling. Doing so would rupture all the standard practices I have been trained to ensure, I protested. How could I be expected to ignore this? Once the project became a reality, I was forced to re-examine both these practices as well as the ethnographic significance of the object itself, weighing them against the costs and benefits of its repair.

Witnessing the process in Anaktuvuk Pass gave me a unique chance to develop a broader, big-picture view of artifact care and conservation. The kayak constructed by Simon Paneak is the *only* Nunamiut-style kayak in existence. It represents a hunting technique that was not commonly used in Alaska except by inland Eskimo people and is therefore of vital importance in the communication of their collective story. Simon Paneak, as mentioned above, was well known as a tradition-bearer and a much-revered member of the Anaktuvuk Pass community. Furthermore, the ethnology and history collection at UAMN includes more than fifty objects made by him, including a full-sized skin house complete with bent willow poles and a rain cover. That he was the maker of the kayak adds to its cultural significance and to the importance of maintaining it for the future. The participation of Simon's wife, Susie, an accomplished and well-known skin sewer, in the original construction complicated the matter even more. If we were to allow the kayak to deteriorate further, we would also be losing her work as well as Ellen Hugo's.<sup>19</sup>

Roosevelt Paneak added to my fears when he told us that it would soon be impossible to find anyone who knew how to do the waterproof stitching, much less anyone who could repair a traditional kayak. If any work was to be done on it, I wanted to be sure that the new cover would be an improvement over the old. However, with the guidance of Roosevelt and Grant Spearman, participants with the appropriate skin sewing and wood working skills were identified, as well as people who had memories of kayaks from their youth.

<sup>17</sup>For example, the Qajaq Club in Greenland. A kayak-making and using club was started by young Greenlandic enthusiasts in the harbor of the capital, Nuuk, after the publication of H.C. Petersen's book *Skin Boats of Greenland* (1986).

<sup>18</sup>In the world of conservation there are two categories of work that are typically done. *Restoration* asks a conservator to not only repair damage to a piece, but to bring it back to a nearly-new condition. In fact, much restoration is done not by conservators, but by "restorers" who make a profession of making things look new again. Much of the value of an ethnographic object lies in the original materials of which it is comprised. A professionally trained conservator may be asked to perform restoration, but for the most part what they do is *stabilization*. This will often include light cleaning, but primarily it is an attempt to halt (and repair) any currently active damage that has occurred and to preserve the piece in its current condition (or that in which it arrived at the museum).

<sup>19</sup>Portions of the original skin covering will be saved for the accession file on this object. Specifically, sections that show the original double waterproof stitch will be kept to document the work of Hugo and Paneak.

The turning point in my ethical dilemma came when I realized that the boat had never actually been used in an ethnographic setting. The 1971 commission was, in fact, a replica of those used before commercially manufactured boats, snowmachines, and firearms were available. So, I concluded, the real value of the object lies in its existence as an illustration of a unique kayak style. Repairing the damage and providing additional documentation, I saw, would only heighten its value to researchers and general visitors alike. In contrast, had this kayak been the last remaining Nunamiut kayak in existence, and had it been used in the 1944 hunt, I would have done everything in my power to prevent any loss of ethnographic material. In effect, I would have refused to support this project.

Just as important, I saw, for the communities and individuals involved, the educational aspect of the proposed project helped to solidify my decision to agree to the replacement. The decreasing number of people knowledgeable enough to make the repairs combined with our intent to involve the young people of Anaktuvuk Pass in the process assured me that this was the right thing to do on several levels. While the repairs were underway, the project created a space for youngsters to interact with older people on a daily basis, with a vibrant exchange of knowledge occurring between them. The young people who came by took an active role in their learning process, by talking to the elders about it. They also had the chance to experience an old object, which helped ignite their curiosity about the past.

The pride and sense of ownership of this artifact continued after the project was completed as the kayak went for its inaugural floating in Eleanor Lake in June 2003. Upon its return to Fairbanks and installation into the art gallery, the people of Anaktuvuk Pass will know that their work will continue to be preserved for generations elsewhere and that they were part of the process that made it happen.

In short, the kayak re-covering project has been an unusual opportunity for the museum to share in the community regaining of heritage, the chance to repair something completely unique, and make it like new again. I have concluded that I am honored to have worked with everyone involved in this process. To learn how something comes from many disparate pieces and combines in

an exciting whirlwind of activity, with a final product of the beautiful kayak is something of which we can all be proud.

## THE FUTURE

Over the last few decades, there have been many projects that have either highlighted the ongoing use of skin boats (Braund 1988) or have helped Native communities re-connect with their nautical pasts by conducting extensive technological studies utilizing oral history and replications of old crafts (Robert Drozda 2004, written communication; Snaith 2001, 2004; Zimmerly 1979, 1986).<sup>20</sup> In 2000, the Alaska Native Heritage Center in Anchorage sponsored a year-long project entitled *Qayaqs & Canoes: Paddling into the Millennium*, where master craftsmen built eight traditional Alaskan watercraft in an attempt to pass along traditional boat-building knowledge and rejuvenate this skill (Steinbright 2001).

Projects such as these are of extreme value when participation is fully supported by the Native communities and the documentation of the event is deposited in the village and conducted by the people. They can bring excitement back into a community or to individuals who had forgotten their nautical traditions.

However, Native involvement with the conservation of museum objects is a topic that needs more investigation. In my research, I have found very few examples of Native American involvement in the conservation treatment or restoration of museum objects (Carrlee 2003; Marian Kaminitz 2004, written communication; Todd 2002).<sup>21</sup> In all cases, the conservation treatment was done by a professionally trained conservator under the advisement of and in consultation with, Native Americans. In contrast, the kayak project, in effect, asked the community members of Anaktuvuk Pass to act as Nunamiut conservators and restorers, performing treatment to an existing museum object so as to halt current degradation of the piece and to prevent future damage.

The collaboration between museums and aboriginal populations appears to be an increasingly important topic worldwide. In Australia, there exists legislation that mandates co-management of sacred sites and the objects associated with those sites (Mulvaney 1999). The Heritage Conservation Act of 1991 "provides a system for the iden-

<sup>20</sup>The referenced projects are in no way a comprehensive list of kayak-related projects that are occurring in Alaska, but merely serve as an example of several such endeavors by Alaskan villages.

<sup>21</sup>Collaborative work has also occurred in two of these examples. Howard Luke carved a new prow piece for the birch bark canoe conservation project at the Sheldon Jackson Museum (Carrlee 2003:2) and Kaminitz (2004, written communication) described several projects undertaken by the National Museum of the American Indian (NMAI) where Native American individuals helped repair items from the museum's collection, in collaboration with conservation department staff.

tification, assessment, recording, conservation, and protection of places and objects of prehistoric, protohistoric, historical, social, aesthetic, or scientific value" (Mulvaney 1999:42).<sup>22</sup> Under this act, the aboriginal population has the responsibility to co-curate these objects, making an investment in the future and preservation of their material culture.

Likewise, in Canada there is a desire to increase the involvement of First Nations people in the operations of museums. According to the Report of the Royal Commission on Aboriginal Peoples (1992), there is hope that there will be shared authority to manage cultural property. The plan is to invite the appropriate First Nations people to assist in defining access to collections, to determine storage conditions and the use of objects, and to recognize traditional authority or individual ownership systems of the originating culture (Assembly of First Nations 1992).

It is our hope, at the University of Alaska Museum of the North, that projects such as the Anaktuvuk Pass kayak project will be a first step to help get Alaska Native communities interested in working in a more collaborative role, to develop exhibitions, collections needs, and conservation techniques.

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Margaret Blackman, Dinah Larsen, The Evelyn Stefansson Nef Foundation, and Linda Yahn.

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<sup>22</sup>See Northern Territory Government 2000 for a review of this act.

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# NOT ALL DRIFTWOOD IS CREATED EQUAL: WOOD USE AND VALUE ALONG THE YUKON AND KUSKOKWIM RIVERS, ALASKA

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**Abstract:** The “Catching the Drift Project” along the Yukon and Kuskokwim rivers in 2002 combined tree-ring and oral history research to explore the natural production of driftwood and its past and present human use. Discussions with Yup’ik and Athabascan elders focused on methods of driftwood procurement, criteria for wood selection, categories of use, and wood working techniques and terminology. Retracing the history of the wood from trees along the river bank to driftwood logs to modified wood or fire wood, allowed us to go beyond the making of objects. By traveling downriver we were able to document people’s perceptions of this important resource: its origin, its cycle, its value. Analysis of our oral history recordings indicates that people along the rivers who have access to standing trees rely on driftwood to a larger extent than expected. Today, driftwood is mainly used as firewood but is also part of some critical subsistence activities, such as fishing. The value of driftwood is influenced by geographic location, local economics, individual and cultural preferences, as well as physical characteristics and post-drift condition of the wood.

**Keywords:** Resource use, Native Alaska, Oral history

“We send the bad wood down river!” a woman ironically told us in Sleetmute, a Native village along the forested middle Kuskokwim River,<sup>1</sup> while we were investigating driftwood use and ecology in interior Alaska. Downriver, in the treeless Kuskokwim delta, Yupiit people do not seem to feel that the wood they get is always bad. They eagerly go after and collect wood the river provides. Traditionally, they “danced to coax the precious driftwood to return when the rivers thawed in the spring” (Fienup-Riordan 1996:153). They viewed wood “as a feeling, knowing being, capable of both gratitude and retaliation” (Ibid.). Important distinctions were and still are made between different types of driftwood depending upon their properties and the use for which they are selected.

To Athabascan people of interior Alaska and Yupiit of southwestern Alaska, wood historically was, and to some extent still is, critical to many daily subsistence activities (Osgood 1940, 1959; Oswalt 1967; Nelson et al. 1982; Nelson 1986). In fact, despite a lack of trees, Yupiit are known as the Eskimo people who utilized wood resources the most (Oswalt 1967). The sources of wood Athabascan and Yupiit have had access to differ in form rather than in species. The former, living along the forested rivers of interior Alaska, were mainly using standing trees and wood from the boreal forest but also ben-

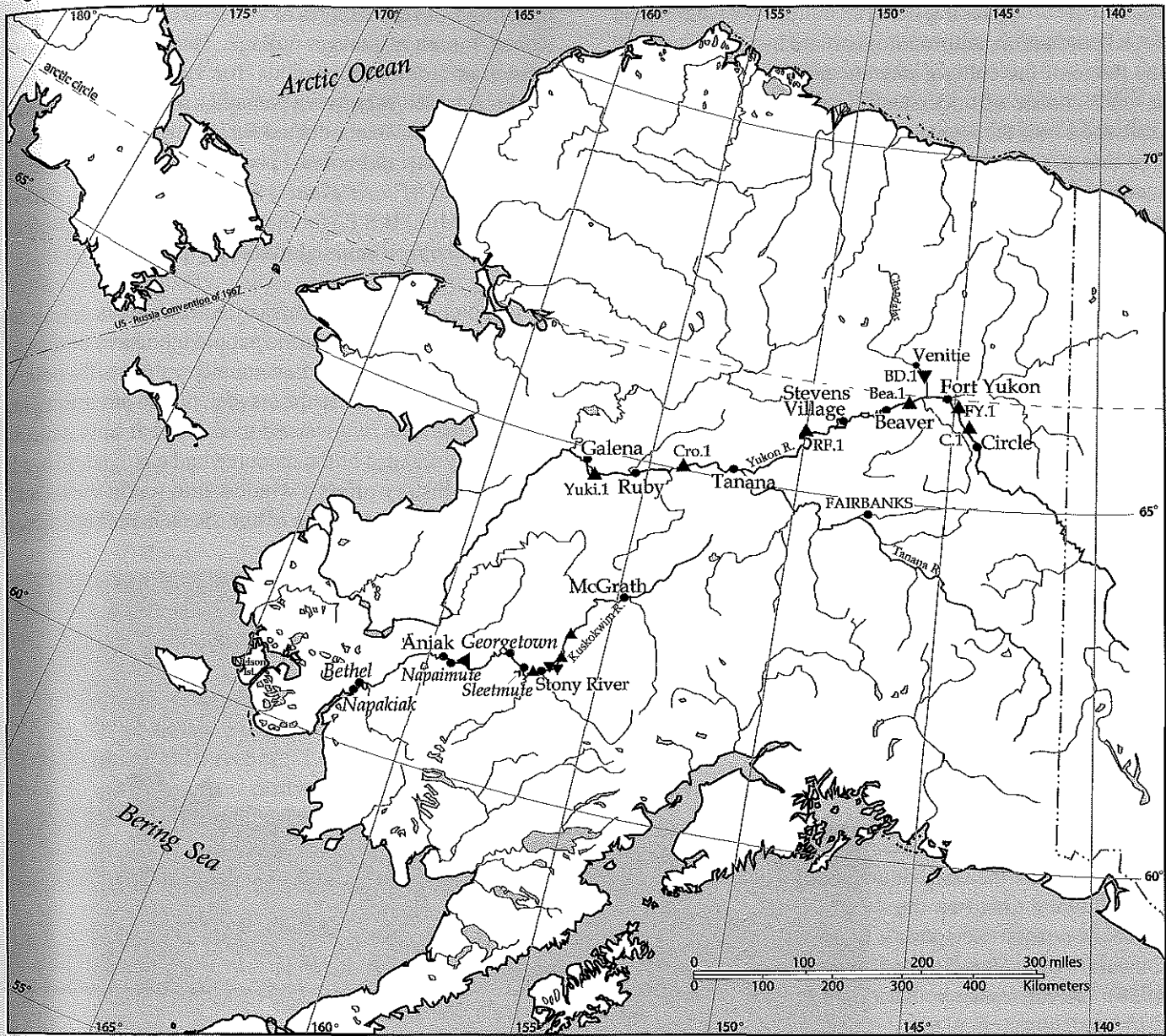
efited from river driftwood. Downriver and on the coast, Yupiit used some tundra shrubs and upriver trees but were dependent upon the arrival of driftwood from the interior boreal forest (Fienup Riordan 1996; Giddings 1941; Oswalt 1967).

In both cases, carvers and users had to look for the right wood and sometimes travel long distances to find the suitable piece (Nelson et al. 1982:204). Still today, Yup’ik carvers from the Kuskokwim delta, like Nick Charles and George Billy, or Athabascan canoe masters like Howard Luke and David Salmon, hunt for the right piece of wood before they start any carving (Billy 2002; Fienup-Riordan 1986:41; Steinbright 2001:4-7,8-17). As Howard Luke from the Tanana River says: “It takes a lot of time. You just got to walk and look, or go on the river and see what you can find” (Steinbright 2001:7).

In this paper, we present the importance of driftwood as a natural resource along the Yukon and Kuskokwim rivers. Our focus, however, is what lies behind the notion of “good or right wood” vs. “bad wood” as conceptualized in Athabascan and Yup’ik communities. The presence of driftwood is not as critical for the inhabitants of the forested upper and middle river as it is on the lower river and coastal regions where no trees

<sup>1</sup>Sleetmute was founded by Ingalik Indians [Deg Hit’an] but today residents of Sleetmute are mainly Yup’ik Eskimo. <http://www.explorenorth.com/library/communities/alaska/bl-Sleetmute.htm>

Figure 1: Map of Alaska, locations of villages and stands of trees sampled



- ▲ Stands of white spruce sampled (*Picea glauca*)
  - ▼ Stands of black spruce sampled (*Picea mariana*)
  - ◄ Driftwood accumulation sampled
- The names of villages in italics are where interviews were conducted.*

grow. However, it appears from talking to people that those who have access to standing trees use driftwood to a larger extent than expected. Recorded discourse on wood and driftwood show that people judge the wood according to what it is used for, their expertise with the raw material and how to work it, as well as where they live on the river. Economic conditions, cultural backgrounds and sometimes social status also influence their perspective.

However, the question is not simply one of people using wood and/or driftwood, but of how they have used it, what choices they made and are still making in selecting the “right” wood. Analyzing resource use requires understanding how the resource is perceived and worked. We went into the boreal forest to the source of driftwood to follow its path downriver, to experience how people view this natural resource, and to collect evidence of its natural “production” and cultural use. We investigated

technical choice in the sense of Lemonnier (1993:26) who states that "... by taking material culture for what it is, a social production, anthropologists and historians expand the range of the cultural phenomena they study as well as their chance of understanding them." In the case of driftwood use, the social production starts with a living tree that is shaped by a complex life cycle of environmental and human forces along its journey to becoming a manufactured wooden object. Understanding the effects of environmental characteristics on the raw material being utilized and how they intermingle with cultural use patterns broadens the explanation underlying how and why objects are made.

## CATCHING THE DRIFT JOURNEY

In the summer 2002, we combined oral history and tree-ring research along the Yukon and Kuskokwim rivers (Figure 1) as part of a larger project designed to explore the natural production and human use of driftwood in Alaska from its source along interior river systems to where it ends up stranded along the coasts (Alix and Brewster 2002).

According to Giddings (1941, 1952), the Yukon and the Kuskokwim river systems contribute the bulk of the driftwood found on the Alaskan coasts (See also Oswalt 1951; Van Stone 1958). River transport of driftwood, its sea circulation and coastal delivery are all dependent upon environmental conditions such as ice breakup, river bank erosion, seasonal floods, currents, winds, tide, coastal orientation and storm activities (for details of these processes cf. Alix n.d.; Dyke et al. 1997; Eggertsson 1994; Euroala 1971; Johanson 1999). However, human activities such as logging and erosion control, also impact this cycle. The "Catching the Drift Project" was designed to document and analyze inter-relationships of the environmental and human processes at play in "driftwood production" (Alix and Brewster 2002; Wein 2001).

In 2002, we spent three weeks on the Yukon River traveling from the villages of Circle to Galena, guided by Robert Joseph of Stevens Village. Then, Nick Kameroff of Aniak/Bethel guided us for two weeks on the Kuskokwim River between Napakiak and Deacons Landing (Figure 1). We conducted fifteen individual oral history interviews and cored 12 stands of white and black spruce trees (*Picea glauca* (Moench.) Voss.<sup>2</sup>; *Picea mariana* (Mill.) B.S.P.) for the tree-ring part of the project that is not being discussed in this paper.

The conduct of oral history interviews is a way to learn and record how riverine people who have access to standing timber view and eventually value driftwood, a highly prized resource in the coastal, tundra environment. At the same time, we wanted to document how access, availability and use of the resource had changed through time. Oral history offers a window into world views and values beyond the lifetime of the speaker that influence



Figure 2: Group discussion, Napakiak, Kuskokwim delta

current practices and beliefs (Finnegan 1992; Morrow and Schneider 1995). In the case of driftwood, people describing the direct uses they and their predecessors have made of the resource reveal decision-making about what quality to look for in wood, when to gather and where to find it, details about how to work with wood, factors affecting drift, and changing technologies as they relate to wood and wood use.

<sup>2</sup>Nomenclature follows Viereck and Little 1986 [1972].

We interviewed elders ranging in age from 60 to their mid-80s who were knowledgeable about traditional subsistence lifestyles. We had a group session with the village council in Stevens Village, informal gatherings of people in Napakiak (Figure 2), and many casual conversations with river residents. We spoke with more men than women because wood working is mostly a man's activity (Osgood 1959: 85; Oswalt 1963:107).

Our oral history recordings show consistencies and differences in quality, value, procurement and selection of wood, between and within Athabascan and Yup'ik groups that can only in part be explained by known principles of wood mechanics and other physical evidence. In getting beyond the physical properties of the wood, we have been able to assess the cultural, social, and personal significance of people's choices and behavior.

### DRIFTWOOD USE IN RIVERINE ALASKA

"We use them for everything. It's just like a free gift to us on the beach"  
(Demoski and Demoski 2002).

Driftwood is seen as a handy and economic resource and is part of several critical subsistence activities, including fishing and traveling. "Like the fish, we depend on driftwood from the river" Joe Matter told us in Napaimute on the Kuskokwim River (Matter and Matter 2002). Flora Demoski echoed: "Driftwood is sort of a very central thing that comes along the river. Because everybody is excited when it starts coming down in the springtime" (Demoski and Demoski 2002). These remarks made us aware of the economic and social importance of driftwood as a resource and an object of celebration in the full seasonal round of the river subsistence lifestyle. For upper and mid-river residents, driftwood is not only an occasional and opportunistic supply of wood. It is often systematically harvested and its use kept for very specific purposes.

Even though driftwood is a valued resource, it also causes frustrations when the river is high and full of floating logs. John Stam, a fisherman on the Yukon River told us: "It is a nuisance that blocks the fishwheel or rips the

net" (Stam 2002). Traditionally, Koyukon Athabascans dealt with this problem by placing a long slim log in front of the fish trap entrance to divert the downstream drift (Huntington 1993:48). For Bill Demoski of Galena, the frustration is different: "There's a time when drift is a nuisance, too. When you're going against it with the boat, you know, the current. You hit a snag or run over it or you got to go all over it..." (Demoski and Demoski 2002).

Along the Yukon and the Kuskokwim rivers and in the Kuskokwim delta, driftwood is used today as firewood and for wood working. It is burnt to heat houses, smoke fish, tan hides, or to heat steam baths on the lower portion of the rivers. As a manufacturing material, driftwood may be used in house construction but most often is part of light structures, racks, smokehouses and fish drying frames. It is also made into sled benders, boat parts, and is carved into smaller objects such as net floats or other tools, weapons and art pieces. While there may be other uses for driftwood, our discussion is limited to the practices we observed and discussed with people, and their subsequent implications. The motivations underlying selection choices in the two main categories of use, heating and wood working, differ, and as such their analyses must be treated separately.

#### *Wood types, condition, quality and selection*

The riparian forest of interior Alaska is composed mainly of pure and mixed stands of white spruce (*Picea glauca*, (Moench.) Voos.), black spruce (*Picea mariana* (Mill.) B.S.P.) and balsam poplar, also commonly called cottonwood (*Populus balsamifera* L.). Stands of white spruce and cottonwood are the most productive (Labau & van Hees 1990; Ott et al. 2001; Viereck and Little 1986 [1972]). As a result, white spruce form the bulk of logs carried downriver to the sea, and cottonwood is relatively abundant in coastal driftwood accumulations (Alix 2004; Giddings 1941; Ott et al. 2001). Nevertheless, we observed large portions of black spruce covered banks eroding in the river (Figure 3).

Together with cottonwood logs, willow trees and shrubs (*Salix spp.*) are the main hardwood (angiosperms) found in river and coastal driftwood of Alaska (Ibid.). Paper birch (*Betula papyrifera* Marsh.) and aspen (*Populus tremuloides* Michx) are more abundant in the



Figure 3: Black spruce on eroding bank, Yukon River

upland forest than on the river bank (Ott et al. 2001; Viereck and Little 1986 [1972]) and little appears in the driftwood accumulations. Birch is also rare or nearly absent because of its low resistance to decay (Alix 2004). The waterproof bark traps moisture, which accelerates rotting of the log (Häggbloom 1982:83).

Driftwood ranges from recently fallen trees with bark and roots to barkless, broken, waterlogged logs. The condition and quality of the wood vary according to when the tree fell, how long it spent in the water, and how often it was stranded and experienced wetting and drying cycles. Some people believe this process makes driftwood a harder wood.<sup>3</sup> As Paul Williams of Beaver explained:

Driftwood, after it stays in the water for quite awhile and the sun, it becomes quite hard. It's good for sled runners and maybe certain kind of wood would make good bow. They're harder than normal because of water and then they get wet and dried up, then get wet and dried up again (Williams 2002).

Carvers in other contexts have also mentioned their impression that driftwood gets harder (Alix 2001a:77). Water transport of timber and water immersion or spraying for conservation and controlled drying purposes have consequences on the physical and chemical structure of the wood (Baylot et al. 1988; Duhamel du Monceau 2000 [1767]; Fengel and Wegener 1988). However, we are not aware of studies investigating the relation between water treatment of logs, density of wood that has been wetted and re-dried, and its hardness. Of course, the quality of a log will also depend upon the species, its buoyancy, its resistance to decay, the conditions under which it grew and the condition of the wood upon entry into the water.

No matter where driftwood is collected (up or downriver), people prefer the fresher or drier logs for wood working and in some places for firewood. They identify "good wood" by looking for logs that "float high;" in other words, ones that are not waterlogged. As one goes downriver these good logs become scarcer. By then, logs have spent a long time in the water and have passed many communities where people may have collected them. This is probably why Sidney Huntington noted:

I guess they have a higher value for driftwood than we have up here, because there's very little timber down there. So whatever drifts down

there, they could really make use of. We don't value every stick. We're looking for the good stuff up here, you know (Huntington 2002).

Contrary to this upriver impression, delta and coastal wood carvers do make choices in terms of wood quality when looking at driftwood that passes through or accumulates in their area. Carver George Billy of Napakiak explains that they look for "the new one, the one that recently fell, the new one is good. It's easier to work with" (Billy 2002). Older driftwood that is waterlogged, severely weathered, and deteriorating may not be chosen even though it is the "right" species or part of the tree (trunk, stump, branch) that is being sought. Elias Venes explained:

Some of it is just beautiful wood, you know. But other stuff is stuff that's been in the water a long time and it's water soaked and heavy. And it will probably never dry out. But, other wood seems to be harder and it doesn't seem to get waterlogged nearly as much as some wood. I think it's the same kind of tree, [but] one tree is hard as a bone and the other is soft and punky or half rotten (Venes 2002).

Carvers also look at the grain and smoothness of the wood when assessing the condition of a log. They prefer straight-grained wood, typically spruce, that does not have cracks or knots, because it is more easily carved, performs the best under a variety of circumstances, and can be used for the widest range of purposes. According to George Billy:

You can recognize good wood by looking and testing it for breakage...You have to take a good look at them. These [here] you don't want to take. These are bad. If they have a crack, it's okay to use them in the stove. That's why they don't use these [for carving] (Billy 2002).

Just because there is driftwood, does not mean the "right" wood is present. For example, George Billy took us to his favorite collecting spot on the Johnson River to show us the types of wood he uses. Even though there was plenty of driftwood, he was unable to locate the "right" wood. Clearly, there is more to selecting wood than whether it is spruce, fresh, barkless, straight, or cracked. Carvers have specific criteria by which they judge wood to be "right." George talked in detail about the three main

<sup>3</sup>In wood engineering, hardness measures the strength offered by the material to the penetration of a hard object such as a metal blade or a pounding device (Charron et al. 2003). While we cannot be exactly sure of what interviewees meant, we suppose they refer to how strongly the wood responds to external forces, such as a tool blade and use wear.



Figure 4: George Billy and Walter Nelson talking about driftwood stumps, Napakiak

types or categories under which he groups different driftwood: soft that will not break; hard that is strong and bends; and hard that breaks easily. In this classification, the wood's physical characteristics and mechanical properties are more important than the species themselves. Wood from different taxa sharing similar properties may bear the same name. George also provided different Yup'ik names for the stump (*mimernaq* and *talliquiriq*<sup>4</sup>), which designate distinct parts of the stump from which different objects are made (Figure 4) [Billy 2002].

Although the bulk of driftwood coming down the rivers and used by people is spruce, other taxa are also utilized if they are in good condition. For example, cottonwood is used for smoking fish and the rare tamarack (*Larix laricina* (Du Roi) K. Koch) was specifically looked for by upper and mid-river residents because its strength, hardness and straight grain made it especially good for sled runners, the basket part of a fishwheel, or for the poles that hold the wheel out from the bank. With experience and a good knowledge of wood, wood taxa can be differentiated within the driftwood even though no bark remains on the trunk. When selecting logs, driftwood users

look at size, weight, shape of stump, type of grain, color once it is cut, and even taste. For instance, cottonwood can be recognized by the shape of its stump, the pattern left by the detached bark on the trunk, and the lightness of the wood. Joe Matter of Napaimute explains how he identifies and collects tamarack:

Tamarack was a preferable wood. If they could find it. You can find it sometimes in the driftwood. Of the 15-20 cords of driftwood I cut and split every year, about one half to one cord of it is tamarack. That's about one or two tamarack logs a year. I can tell when I pick it up and cut it. The tamarack is hard to tell, it looks a lot like spruce. But it's the weight. I can tell when there's a good dry log. It's got a little bit different color, but that's pretty hard to tell when you're cutting the driftwood, especially what some of the logs have been through (Matter and Matter 2002).



Figure 5: Raft of driftwood logs, Tanana, Yukon River

### ***Procurement Techniques: Hunting, Hooking and Rafting***

Most people along the river or in the delta collect their annual supply of driftwood in the spring just after the river ice has broken up and the spring melt causes the water level to rise and the wood to be lifted and floated downriver. This is usually when the river is filled with the largest amount of wood. People literally catch the passing logs by hooking them from a boat, tying them together into a raft, and hauling them to the beach (Figure 5). Sidney Huntington of Galena described how he used to gather driftwood:

<sup>4</sup>Orthography follows Jacobson's dictionary (Jacobson 1984).

You pull it in with a boat right now. I've seen lots of people with ropes hanging them out in the river and stuff like that, pulling in driftwood to use for wood.<sup>5</sup> I pulled in driftwood when I was 12 years old. I put my leg over the canoe, and hooking one leg on to the driftwood, and paddle it to the beach with the canoe, because I learned it from an old man named Charlie Mountain down at Nulato. I watched him, so I had to do the trick too, you know. Go up there about a mile and find a good one. Put my leg on it and paddle it to the beach. Right from the canoe. You don't even have to get out (Huntington 2002; see also Huntington 1993: 67-68).

Catching driftwood is not limited to spring break-up time. This passing resource is taken advantage of whenever spotted and "people watched for good logs" (Huhndorf 2002). These methods of spotting the logs, evaluating their quality from a distance, and catching and hooking them are known from the delta and coastal areas of the Arctic (Alix 2004; Gessain 1984:82; Giddings 1941:46), but as it turns out they also are practiced as far upriver as Beaver on the Yukon and past Sleetmute on the Kuskokwim.

Some people also specifically mentioned getting driftwood after the August rainy season, when the river level once again rises and carries stranded wood off the gravel bars (Matter and Matter 2002). As Peter Zaukar of Sleetmute explained:

...in the fall time after the fish runs it used to be high water. Plenty wood used to drift down. Sometimes it was just like ice in the river when it break up. You can't go across... You'd get more wood for winter then. Whenever they drift down, we try to catch them. It's better than go out there [in the forest] and try knocking them and drag them down. They come down themselves. And all that you do is catch them and tie them up. It's easier to get them off the beach (Zaukar 2002).

In the Kuskokwim delta, George Billy of Napakiak mentioned getting driftwood in the winter as well (Billy 2002). There, contrary to farther upriver, persistent wind and open country prevent the logs from being buried under the snow. This is consistent with descriptions of the seasonal subsistence round in historic Yup'ik coastal/tundra and tundra/riverine villages, where driftwood gather-

ing is mainly a winter/early spring activity (Hensel 1996:42-46).

## DRIFTWOOD AS FIREWOOD: ECONOMIC AND SOCIAL IMPLICATIONS

People's perceptions of the value and economics of using driftwood for firewood affect whether they collect it and the different ways in which they use it. Factors taken into account are: seasonality and transportation methods; timing/availability; price of other fuel sources; preferences; and the amount of heat produced. But other factors are social, such as how one will be viewed within the community for collecting driftwood. Statements like "only the lazy do that [use driftwood]" (Williams 2002) or "I used to get driftwood and bring it to people that are less fortunate than us" (Venes 2002) give the impression that driftwood is the wood for the poor or the "lazy." In other places, however, people who collected driftwood were considered resourceful and wisely making use of a resource that otherwise would be wasted: "Driftwood is just a useful thing that came down the river when you didn't have nothing. And it just comes along and all you have to do is work on it" (Demoski and Demoski 2002).

### *Heating houses*

In our interviews, we heard about three types of wood that are used for firewood: "dry wood;" driftwood; and green wood. Each has its own advantages and disadvantages that are reviewed below. What people call "dry wood" refers to dead standing trees in the forest. On the upper and middle Yukon and Kuskokwim rivers, the preference is to collect this "dry wood" (Honea 2002; Huntington 1993; Venes 2002; cf. Jacobson 1984: 306; Nelson et al. 1982:363; Nelson 1986:33-37), but driftwood is also used. Similar behavior is observed in other river-based cultures, such as the Itelmen of Kamchatka in Russia. Tatiana Petrovna Lukashkina recalled:

When we go fishing while on the river bank we never chop a living tree. We gather dry trees which float from the river. They're called plavnik (driftwood). We gather them then pile them up and they get dry-dry in the sun. If we go into the forest to dig sarana root and see that a dry tree is lying on the ground, we drag it home. We bring it home, saw it up and stack it to dry (Lukashkina 1995).

<sup>5</sup>The word 'wood' is typically used to mean firewood.



Figure 6: Driftwood logs cut into lengths for firewood and piled for drying, Galena, Yukon River

The principal disadvantage of driftwood is that it accumulates sand, silt and gravel while traveling downriver. Cutting it dulls the chain saw and can even damage the chain beyond repair. The advantages are that trunks are already down, often without bark, and only need to be pulled to shore before reducing them into stove lengths. Good driftwood is already dry and suitable for immediate burning. However, like the green wood of live standing trees, some fresher driftwood may need to be split and dried before it can be burned (Figure 6). Waterlogged trunks are too wet and decomposed to be good for firewood, or much of anything else, so they are left behind.

Standing dead trees do not damage tools like driftwood, and do not need drying like live trees and fresh driftwood. This mostly explains why “dry wood” is the preferred firewood even though it has to be felled and transported back to the village or camp (Honea 2002; Venes 2002). Old burn areas are ideal natural “storage” places for this type of firewood. Don Honea of Ruby also recalled how dry wood was “produced” intentionally in the forest when the Native people in the Kokrines area on the Yukon River used “to go out in the spring time and peel a whole bunch of [spruce] trees. They peel them and then in about four years they’re dry” (Honea 2002; see also Huntington 1993:107). This implies long-term planning to ensure the availability of dry firewood without being limited by adequate drying and storage space. This traditional natural resource management practice of creating dry wood and using old burn areas have implications for how nomadic or semi-nomadic hunter-gather groups managed and stored their fuel supplies (Thery-Parisot 2001:20-23).

Apart from the technical advantages of dry wood over driftwood, seasonality is another factor in the decision about whether to collect one or the other. Driftwood is collected in the summer by boat from the river while standing dead wood is cut in the forest in the winter and hauled with a snowmachine or, in earlier times, by dog team (Nelson 1986:34). Today, the calculation is based upon the price of gas for the snowmachine (including the number of back and forth trips required to bring back a sufficient winter supply) versus the cost of gas for the boat, plus the price of a new chain for the chainsaw that will be damaged from the grit in driftwood. The fact that a boat can haul much more wood than a sled is also considered. Bill Demoski of Galena also thinks about the wear and tear on his equipment when deciding how to get his year’s supply of firewood:

You see the reason why I bought a couple of cords of wood is, if I go out there carrying my chainsaw, burn that much gas. Gas is so expensive. And tear up your snowmachine. So I figure for a hundred bucks, I can relax. And just go down and load up my pick-up (Demoski and Demoski 2002).

For the last few years, the rising price of heating oil or other fuel sources has influenced procurement strategies. In 2001, one of us was told that people in Nome resumed collecting driftwood from the beach to heat their homes because the price of heating fuel had abruptly increased (Alix 2001b). A similar situation was related by Elias Venes in Bethel:

In Bethel it’s [the price of fuel oil] very high, especially now, so a lot of people use driftwood for primary heat in their house. They use oil to have a steady source of heat to cook and that kind of stuff. But they use wood to heat their homes. They save hundreds, thousands of dollars that way. I’ve seen ads in the wintertime where you could buy a cord of wood for \$225. But I’ve never ever bought any myself (Venes 2002).

Another factor in whether to collect driftwood or dry wood is the amount of time each activity takes and one’s availability during the collecting season. Don Honea of Ruby explained:

Sometimes you're busy fishing in the summer-time, so you don't have enough time in the fall time to collect all the [drift]wood along the beach you need for the winter. See, in the wintertime, you're not able to get it because it's under the snow. In the wintertime, you had to go into the woods and look for dry trees (Honea 2002).

From our recordings, it seems that using green wood for firewood is a new practice. In interior Alaska communities such as Sleetmute and Ruby, we met some residents who burn green birch. They said it was easy to get; you did not have to wait for it to dry, and a well-packed fire lasted longer than with drier wood. Curiously, only a few people were concerned about the increased creosote build-up and chimney fire danger that comes from burning green wood. The advantage of making the fire last longer seemed to outweigh this problem (Brewster 2002; see also Anderson et al. 1977:239).

Finally, there was variation in what people said about the amount of heat put out from burning driftwood versus burning other dry wood or green wood. Several people mentioned that driftwood does not heat as well as dry wood.

Driftwood doesn't put out nearly the heat that seasoned dry wood, standing dry wood will do. Too much moisture content, I guess. I don't know. But, I think most people would tell you that good standing timber that's been dried will put out much better heat than driftwood that's been hauled in and soaked in water (Venes 2002).

However, others disagreed, saying there was no difference. People burning green birch thought it provided the best heat, while those using dry wood, such as Pete Mellick in Sleetmute, thought it was the hottest and said green wood burns less hot. Besides the type of wood being used, the heat produced also depends upon the kind of stove and how the fire is laid. For instance, in the old barrel-type wood stove that had minimal air intake controls, dry birch got too hot and burned a hole in the stove (Honea 2002). The fast burning dry wood is often combined with green wood to make the fire smolder and last longer (Venes 2002).

Variation in the heat put out by different species of trees may in fact be relatively low (Thery-Parisot 2001). Experiments conducted in controlled conditions show that



Figure 7: Setting green willow smudge in smokehouse, Nick Kameroff fish camp, Kuskokwim River

the amount of moisture contained in the wood seems to be more important than the species itself (Thery-Parisot 2001:151-159). Indeed, no best species came out of our discussions about firewood. People had strong ideas about what ought to be done, and whatever practice they were following, they, of course, thought it was the best and produced the most heat.

The decision to use driftwood, standing dead wood, or green wood is based on a combination of personal finances, preferences, experience, and local social norms. Long-standing habits and ways of doing things are often what people follow, even if the economics do not come out in their favor.

### *Smoking fish*

People have even stronger opinions about the best wood for smoking fish (Figure 7). Choices often are made according to species and dryness of wood. The notion of how the fish tastes and the preferences of individual, family, or even village play an important role in the decision to use one taxon over another. Cottonwood is the most commonly used wood. But even those who use it, either swear by green wood or think dry is the only way to go for the

fish not to develop a bad taste. Some people (like Don Honea of Ruby) who rely upon dry cottonwood do admit using some drifted logs:

We use cottonwood. But, it's drift, too, most of it. If you can get dry standing it's better. But, usually you can't find very much of it in one place. It's better because it gives off a nice white smoke, and it burns much longer without getting real hot. Like if you burn birch it gives off a real black smoke, so your fish you can't hardly eat it. And the same with spruce wood (Honea 2002).

Overall, those who use driftwood for smoking fish are rare. In Stevens Village, one family uses driftwood (any taxa or a combination of many) and feels the fish tastes better than with cottonwood. We do not know if smoking with driftwood was more common historically or if it is just one family's preference. However, Peter Zaukar in Sleetmute did mention that he used to smoke his salmon with driftwood, but changed to cottonwood because "you get more smudge with it and the fire lasts longer" (Zaukar 2002). Paradoxically, in Napakiak, in the Kuskokwim delta where hardly any trees grow, driftwood is only used to start the fire in the smokehouse. Green willow and cottonwood are used for the actual smoking. They are cut within a few miles upriver on an island near Bethel.

### ***Tanning Hides***

Middle and upriver Gwich'in and Koyukon people burnt rotten, dry spruce to smoke and tan moose hides (Joseph 2002; Pitka 2002; Williams 2002). The moose skin was softened in water and moose brains, and smoked over a driftwood fire. The hide was then sewn together and hung over a frame like a makeshift tee-pee. A smoldering fire of dried, rotten red-colored spruce wood collected from the forest floor or driftwood was burned underneath to give the hide a golden color (Huntington 1993:85-86; Joseph 2002; Nelson et al. 1982: 364; Nelson 1986:37; Osgood 1936:67; Pitka 2002; Williams 2002;). Carrie Joseph of Stevens Village described the process she learned when she was a young girl:

In the summertime or early spring, they would make a little round hut, tent-like, out of willow. They put the skin on in there and they cover it with canvas. Then they burn driftwood under it. You're not supposed to burn it. It's just real low. Just smoke. You have to really watch it,

so it don't burn. And then after that they put it in water. Soak it in moose brain overnight. And then they take it out of the water and they have to wring it. Get all that water out. Then they fix a pole. You stand there and scrape it. All day. You can't stop. If you do, it will just dry up. ...then when it's tanned, they sew it together and sew that canvas in the bottom so that moose skin wouldn't touch the ground. ...They don't just have fire on the ground. They have dish-pan or an old pan, or something that have fire in there. Then you have to sit there. Watch it so it don't burn. Grandma and them told us that if there is flames then the skin don't turn out. It turns too dark. They don't like that. They don't like dark moose skin (Joseph 2002).

### ***Steam Bath***

A large proportion of the driftwood burned on the Kuskokwim and lower Yukon rivers today is for the steam bath (*maqi*), which is a central element of Yup'ik identity and social interaction (Hensel 1996:123-126). As Mark Leary of Napaimute on the middle Kuskokwim River said: "Most people use driftwood for steaming" (Brewster 2002). Steam baths, introduced by the Russians when they came into this area<sup>6</sup> (Oswalt 1963:123-124), require extensive amounts of wood to keep the hot fire burning for a long time. When Yupiit have a large enough supply of dry wood piled up they are likely to fire up their steam baths a couple of times a week. According to Nick Kameroff of Bethel, if you steam everyday you burn about five cords of wood in a year (Brewster 2002). Thus, driftwood provides an easy, cheap, and usually reliable source for the steam fire. Because of the prevalence of steam bathing on the Kuskokwim and the lower Yukon rivers, it appears that more driftwood is being utilized compared to the middle and upper Yukon, where people do not steam.

Driftwood burned for steaming is also selected. Annie Nelson of Napakiak had a large pile of weathered logs in front of her house that she described as: "They have been beached for a long time. It's not new. These are mainly for steaming" (Brewster 2002). As mentioned previously, the fresher, newer drift logs were being saved for manufacturing purposes.

## **DRIFTWOOD FOR WOOD WORKING**

The physical characteristics of wood are important in the way people select and work wood and this is illustrated by the terminology used to designate different pieces.

<sup>6</sup>Before the Russian influence, Yup'ik men and boys would take "fire baths" in the *qasgi* or men's house. A fire bath was a "...bath in which fire is the only source of heat, as contrasted with the steam bath, in which water is poured over hot stones to give off heat" (Oswalt 1963:124).

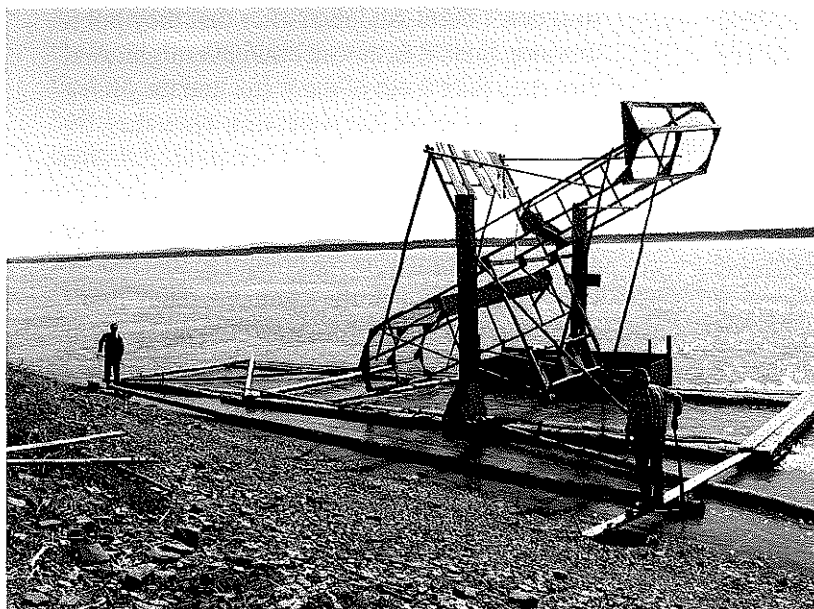


Figure 8: John Stam's fishwheel, Yukon River

From the Greenlandic Inuit to the Yupit of southwest Alaska as well as for the Ingalik Indians of the middle Kuskokwim and lower Yukon rivers, different parts of a log and stump bear distinct names and are kept for specific uses (Billy 2002; Jacobson 1984; Osgood 1959:42; Petersen 1986; Worm and Worm 2002).

Whether the wood used is driftwood or not, our own observation and a brief review of existing literature shows that along interior Alaskan rivers and in the Yukon-Kuskokwim delta, white spruce is the wood most used for carving and building (Nelson et al. 1982; Nelson 1986; Osgood 1936, 1959; Oswalt 1967). Ingalik Indians made no distinction between white and black spruce (Osgood 1959:44) and, while Koyukon and Gwich'in mainly worked with white spruce (Nelson et al. 1982; Nelson 1986; Osgood 1936), they used black spruce mostly "when strong, slender poles were needed" (Nelson et al. 1982:365). In Napakiak, on the lower Kuskokwim, Walter Nelson and George Billy also specified that straight young black spruce driftwood poles were kept for making harpoon shafts as well as "...for all kinds of things including canes and icepicks" (Billy 2002). In fact, Walter Nelson had in his yard a black spruce driftwood pole that he had retrieved earlier and was saving to carve into a harpoon shaft.

### ***Driftwood logs and trunks***

Length, straightness and/or lack of defect in a driftwood trunk is critical to its determination as "good wood." This type of log is and has been used for a variety of purposes, such as the raft of the fishwheel, cabins and caches, fish traps, and sled runner benders.

Fish traps were a traditional Native method for catching large numbers of fish (Nelson 1899; Osgood 1936) before fishwheels were introduced around the turn of the century by non-Natives who had fished with wheels on big rivers in the "lower 48" (Huntington 1993: 49-50). Since their introduction, fishwheels have come to dominate and symbolize subsistence fishing on the middle Yukon and Kuskokwim rivers.<sup>7</sup> Driftwood is critical for the raft of the fishwheel, which is what keeps the structure afloat (Figure 8). As Sidney Huntington of Galena said:

"If it wasn't for the driftwood we'd never have been able to build any fishwheels, because that's what floated the fishwheels. We had to have nice big logs, up to [12 meters] or longer" (Huntington 2002).

These logs must be large, long, straight, and really dry, criteria which driftwood meets especially well. Some logs are very dry from the weathering they experience while being stranded. As mentioned previously, people speculated that being in and out of the water so long made the wood harder but also helped it dry out. Studies of wood immersed in water for long periods of time have shown that it becomes more permeable, which helps with its seasoning (Fengel and Wegener 1988).

Driftlogs are also well suited for fishwheel rafts because of their accessibility. As Paul Williams Sr. of Beaver said, "It is already near the water. Get a nice straight one. Cut all the limbs off of it, and cut it to length and just roll it in" (Williams 2002). It is easier to catch a passing log and float it ashore where you are building a wheel, than cutting down and dragging a large tree through the woods, possibly a long distance from where the fishwheel will be put in the water.

In some cases, driftwood logs were also used in cache and cabin building. Caches were only mentioned in Napakiak where one was left standing in the village. This historical cache, which originally was a house entry, was built entirely of driftwood about sixty years ago (George Billy 2002; Brewster 2004). In the forested areas, some people mentioned building cabins out of driftwood, although it is unclear how common this practice has been. Don Honea of Ruby described his log selection and construction process:

<sup>7</sup>Fishing regulations presently restrict people from using fishwheels on the Kuskokwim River.

I've built two small houses out of driftwood. This has been years ago now. All I did was just go along and picked up the logs along the beach. We try to get straight ones about all in the same size. It usually takes about fifty or sixty. It's so much easier to get it, you know. And all the bark is off of them already, too (Honea 2002).

### *Stumps as a prime material*

The use of tree stump wood was repeatedly mentioned in discussions we had with river residents both in the interior and the delta, and it also appears in several published historical accounts of the regions (Himmelheber 1987; Osgood 1936:70-71,76; 1959:43; Oswalt 1967:147; Zagoskin, and Michael 1967:117, 212-218). According to Hans Himmelheber (1987), who recorded traditional Yup'ik ways of life in the 1930s, the wood of the stump has a higher resin content that prevents it from cracking or breaking under the carver's knife. Oswalt (1967:147) reports that "for certain items, such as net floats and shallow containers for liquid, spruce roots were the most desirable material since they tended not to crack when coming into intermittent contact with liquids."

People we interviewed listed the many items made from stumps and carvers were careful in describing the wood from the stump as unbreakable and strong, but in some cases soft and easy to carve (Abraham 2003; Billy 2002; Walker 2003). The quality of the stump wood seems to be a well-known and generally accepted concept in interior and coastal Alaska.

With their natural curve, stumps are ideal for making ladles, spoons, containers, masks, and certain parts of the canoe or kayak, such as the bow and stern pieces (Figure 9). Long hours were spent looking for a stump



Figure 9: Canoe bow from driftwood stump, Stevens Village

with the correct angle. As Carrie Joseph of Stevens Village emphasized: "That too you have to hunt for it. They look for old stumps. Dry stumps" (Joseph 2002). It is well known that the natural curve of the driftwood stump is used on the coast and on St Lawrence Island for the bow, stern and other elements of the umiak and kayak (Braund 1988; Steinbright 2001; Zimmerly 1979), but it is less documented for boats of the interior.

The use of stumps reported in forested riverine areas (Joseph 2002; Osgood 1936; 1958; Zagoskin and Micheal 1967) probably corresponds to spruce driftlog stumps. No details in the written record explain how Kutchin or Ingalik wood workers extracted the stumps. One can imagine that they were dug up from the forest ground, but it seems reasonable to expect that carvers would look for or come across grounded driftwood logs with the "right" stumps during their travel on the river. Drifted spruce stumps may have been highly prized during traditional times given the quality of the wood, the variety of uses, and the effort that retrieving a stump from the ground would require.

More recently, stumps were also used in house construction for roof bracing. This was observed in Napaimute in an early 1900's frame house, which is one of the oldest homes still standing on the Kuskokwim River (Matter and Matter 2002). Dee Matter, granddaughter of George Hoffman who built the house, explained the stump's function:

My grandfather used drift stumps. See where the stump curves. Where the natural curve is. There were three or four of 'em on each side of the building. There's one left. That one hanging out there, that was the original. There were big logs that were put in that, and they were all pinned in. They were all pinned in with driftwood. They're hard and they don't rot. It was bracing. It held the log up to hold the roof (Matter and Matter 2002).

Dee and her husband, Joe, implied that this was something commonly done in the past. When the Matters had the house renovated, the roofing structure was changed, but they preserved one of these braces and have it hanging as an artifact on the outside of the house (Figure 10).

## CONCLUSIONS

Our research has shown that there is a greater reliance upon driftwood in interior Alaska than anticipated,



Figure 10: Roof bracing made of driftwood stump, Napaimute

and this raises new questions. While we have ascertained that driftwood is used in mid-river areas, we wonder how far back this use goes. Alaskan elders of today have little direct memory of life before the advent of the hand saw, Swede saw, or even the chain saw. Through the few examples given above, we can see the impact of some key technological changes on wood use, such as the powerboat or the chainsaw. Technologically, the transition to the chainsaw in the 1930's and 1940's was a key vector of change in terms of how people have harvested and worked wood in Alaska; it is often mentioned as a reference point when differentiating between how things were done "then" and "now." We know that the introduction of the wood stove on the northwestern Alaskan coast modified people's collecting practices and depleted driftwood accumulations (Stefansson 1978 [1919]:8). We presume the stove must have impacted people's collection of wood in the interior and on the delta as well. At the same time, the extensive wood-harvest for river steamboats in the early to mid-1900's and changes in steambath practices may have influenced the driftwood supply. The question now is to understand how these changes have effected the rivers' ecology, the people's uses of wood and driftwood along the rivers and in the deltas, and ultimately the coastal accumulations of driftwood.

How wood and driftwood are chosen, the ways they are used by different groups of people, and the role that the wood's condition plays in the selection process have become clearer after talking to people both on the delta and along the rivers. The value placed upon driftwood varies depending upon geographic location, local economic conditions, and individual preference, but physical characteristics of the wood and the "story" of the drifted tree also play a role. As one approaches the sparsely wooded lower river, the notion of what is "good" or "bad" wood changes and driftwood acquires more value than that expressed by upriver people.

Understanding the "production" of driftwood and the selection processes are paramount when trying to piece together past and present human use of, and reliance upon, driftwood in interior and coastal arctic Alaska. A systematic record of people's use of and discourse about driftwood and wood working techniques, combined with existing knowledge about the mechanical implications of wood's physical properties, offers critical insight into the manufacture and performance of objects. This approach provides valuable tools for better analyzing wooden objects and structures and reconstructing the past. It takes us beyond the objects themselves to a deeper understanding of the raw material, the lifecycle of its "production" and procurement, and of how people today and long ago related to the natural resources they relied upon.

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# LEARNING, MAKING, TRANSFORMING: CONCLUSIONS ON *MAKING IT*

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In his recent work on visual culture, W. J. T. Mitchell (2002) asserts that culture constructs vision which is in turn influenced by factors such as history, politics, economics, and philosophy. Mitchell (2002:97) elaborates upon this concept by turning the tables on conventional perspectives on objects, claiming:

Works of art, media, figures and metaphors have "lives of their own," and cannot be explained simply as rhetorical, communicative instruments or epistemological windows onto reality.... Vision is never a one-way street, but a multiple intersection teeming with dialectical images.... It makes it clear why the questions to ask about images are not just "what do they mean?" or "what do they do?" but "what is the secret of their vitality?" and "what do they want?"

These wonderful papers, which in their own ways describe efforts on the part of students of culture to participate in the creation of visual culture, offer intriguing insights into the vitality of masks and kayaks, tools and jewelry, baskets and wood. I suspect that as Hensel, Blackman, and Linn, under the guidance of Roosevelt Pankak, learned to make new things in new ways, they gained intriguing insights into what these creations want. As Alix and Brewster floated down the Yukon to collect data, they too, learned of the intentionality of driftwood; indeed, they were told by Nick Charles that wood has feelings, knowledge and emotions. The old ethnologies with drawings and descriptions of usage never would ask such a question.

Those of us in ethnographic art history try to focus on the dynamics of Native-non-Native interchanges from first contact to the present, analyze the endurance of Native culture during the process of colonization and its aftermath, interpret material culture's role in cross-cultural understanding, misunderstanding and mutual ambivalence, and celebrate the mutability and constantly evolving

nature of culture. As Lee points out in her counterpoint to those who "make it," earlier approaches to material culture, inspired by nostalgia and the imminent "disappearance" of both creations and creators, encouraged the development of the "hobbyist" who copied Native art. The "hobbyist's" objective is not so much to discover cultural insights through the act of creation but instead to create an object difficult to distinguish from its Native prototype.

That hobbyist, firmly grounded in essentialism, ignores or resists the historical reality that, from first contact until the present, non-Natives have been integral to Native art history. This collection of papers takes that reality as a given. Acknowledging non-Native involvement in culture history does not diminish the centrality of Native people in the process, but to challenge essentialism and demonstrate how artworks emerged and continue to emerge as negotiations and involvements with, as well as reactions to, the intruders into their territories and expression of changing identity in a world consisting, for better or worse, of Natives and non-Natives.

Here, in an intriguing departure from scholarly convention, anthropologists contribute to art creation from the perspective of student, in an intriguing example of exemplifying how Natives and settlers together contribute to the ongoing history of culture. In their book on colonialism in New Guinea, Chris Gosden and Chantal Knowles (2001:xix) acknowledge the involvement of both local and imperial participants in the colonial encounter:

Chemists make a distinction between a mixture and a reaction. A mixture is a solution in which different chemicals combine, but retain their original form, whereas a reaction creates something new out of its original constituent parts. Colonial New Guinea was a reaction to which all parties contributed, so that there can be no question that all had influence and agency.

Going on to criticize those who insist on an essentialist concept of culture, they assert “anthropologists have tried to undo or ignore the reaction and focus upon one part. New Guineans, creating a partial and static picture in the process” (Gosden and Knowles 2001). What I especially liked about these papers is how none limited its study to the Native creator – although implicit was the primacy of Native inventiveness and tradition – but included him or herself in the process of understanding. Each speaker and his or her teachers had, as Gosden and Knowles (2001) would put it, influence and agency in the creation of new works of art.

Marita Sturken and Lisa Cartwright (2001) pose three more questions for a cultural biography of visual objects: What do images tell us about the cultures in which they are produced? How do viewers look at, utilize, understand and make meaning of images? How do images circulate between and among social arenas, different cultures, and around the world?

Cultural meaning is thus a highly fluid, ever-changing thing, the result of complex interactions among images, producers, cultural products, and readers/viewers/consumers. The meaning of images emerges through these processes of interpretation, engagement, and negotiation. Culture is a process, in a constant state of flux (Sturken and Cartwright 2001:69).

Hensel, Blackman, Paneak and Linn, as viewers, consumers and creators of culture, have contributed to that flux.

Some insights that emerge from the activities of these participant-observers could have come about only as a result of their experiences. From his Yup'ik instructors, Hensel learned truths about learning, and teaching, that have stayed with him forever. Especially interesting is his comment that the skills he learned over the decades have in some cases become almost obsolete, as culture change – the kind of “flux” to which Sturken and Cartwright refer – moved relentlessly ahead. Blackman, the highly successful academic, experienced a “leveling” process during which those who typically would be her consultants became her teachers, she exposing herself as someone not quite so competent. Balancing that was the true connection between two women that resulted from her apprenticeship in mask making. Linn, under the guidance of Paneak, had a different experience, for she herself did not “make it,” but instead helped make it possible for the kayak to be re-covered. She learned something about her own discipline, collections management,

first worrying about the ethics of subjecting an artifact to treatment well outside museum conventions, then reconciling its origin as a museum piece with its ongoing educational value. Even Lee, who resists the very idea of making the baskets she studied, admits reluctantly that learning techniques does enhance her understanding of the subject.

Perhaps the most striking feature of these papers is the experience they describe. There is far more activity in making a ladle than watching someone make one, or reading an ethnographic text describing the procedure. The anthropological process involves not just watching and listening, but feeling and doing. And it is profoundly social. Each of the authors describes how by participating in making something, he and she attained a new and different level of communication. Thus, “making it” becomes a transformational experience, of the raw materials at hand, of the scholar's understanding of material culture, of the relationships that solidify during the creative process. And it is through such transformations that new knowledge can emerge.

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## ARTICLES

# THE SIGNIFICANCE OF DOG TRACTION FOR THE ANALYSIS OF PREHISTORIC ARCTIC SOCIETIES

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**Abstract:** Dog traction was a central element of Eskimo cultures from Greenland to Southwestern Alaska, yet has received relatively little attention from northern archaeologists. This mode of transportation likely emerged in the last 1500 years and was a necessary element in the mobile subsistence strategies and social networks of historic Eskimo societies. An understanding of the effects of adopting dog traction is necessary for analyzing prehistoric societies that preceded and came after its development. The increased mobility conferred by the use of dogs likely had effects on functional and stylistic variability in archaeological assemblages, the costs of resources and means of procurement, settlement strategies, and other aspects of Eskimo culture.

**Keywords:** Mobility strategies, Cultural ecology, Prehistoric change

## INTRODUCTION

The use of dogs as draft animals to haul sleds, or dog traction, is one of the traits universally identified with Eskimo culture. Despite the centrality of this mode of transportation in the societies and economies of Eskimos from Greenland to southwest Alaska, surprisingly little attention has been given to its inception and the roles it played in those systems. This is particularly interesting given that it is widely held that dog traction is a relatively recent development in Eskimo prehistory (Anderson 1988; Bandi 1969; Dumond 1977; Giddings 1952, 1964; Hall 1978; Jenness 1940; Larsen and Rainey 1948; Rainey 1941; VanStone 1955). Almost all researchers who have been concerned with prehistoric Eskimo or Eskimo-like cultures have considered dog traction to a limited extent, and some have briefly considered some of its potential implications (Anderson 1988; Larsen and Rainey 1948; Sheppard 1986). But despite the obvious importance of this technological adaptation, to date only Hall (1978) has made a concerted attempt to focus on the many changes that it may have initiated in prehistoric Eskimo culture and ecology.

One of the principal objectives of modern anthropologists is to understand how cultures function as systems. This requires one to examine how the social and economic components of the cultures connect and interact, and to do so we must understand the mechanisms that allow the flow of information and resources. For human systems we are talking about communication and transportation and, for most of human history, those

mechanisms have been equivalent—the actual movement of people has controlled the flow of information and resources across the landscape, by foot or by other means of travel.

In the prehistoric Arctic there have been two non-pedestrian modes of transportation: water travel and dog traction. The former must be of equal antiquity with the settlement of most of the North American Arctic, which would have been logistically and economically unfeasible without it. Because of that, waterborne travel and subsistence activities can be treated as more of a constant in analyzing cultural systems during the last 5-6000 years of North American Arctic prehistory. The presence of dog traction, on the other hand, cannot be similarly dealt with because it originated at some intermediate point of time and likely involved significant systemic changes. Therefore, we must consider what the implications are for its adoption, especially for the time before dogs were used—when we really do not have an adequate ethnographic model on which to base analyses.

In this paper, I will look at the social and economic implications of dog traction and explore as well the potential ramifications of those changes for archaeological interpretation. In part, one significant goal is to stimulate more interest in this question, particularly since, as will be seen, my analysis raises as many questions as it answers. It is not my intent to “dot every i and cross every t” regarding the issue, but to start addressing the significant

conceptual issues involved. It is also not my intent to discuss all aspects of the domestication, use, and consumption of dogs by Eskimo peoples, most of which have no significant bearing on my analyses. This is not a paper about dogs and dog traction *per se*, but about the systemic implications of the latter.

In the subsequent discussion, I will address the following issues. First, I will examine what are reliable archaeological indicators of dog traction and when this mode of transportation is likely to have arisen. Next, I will look at how dog traction functioned in real societies, using Northwest Alaska as a model. Given that ethnographic framework, I will look at the possible structure of pre-dog traction societies by removing the capabilities provided by dog transport. With that in mind, I will consider how the pre-dog traction system might be materially reflected in the archaeological record. Finally, I will ruminate briefly on how Northwest Alaskan prehistory may reflect the adoption of dog traction and make some general comments about its consideration in relation to broader issues of northern prehistory.

## ORIGINS

The issue of when dog traction first emerged is a thorny one since there are no good indicators of its actual inception. The first use of dogs to pull loads across snow doubtless involved little more than a length of raw hide and whatever else was at hand. This could be as insubstantial as a frozen hide toboggan such as used by the Iglulik Eskimos (Mathiassen 1928:79ff). Thus, early dog traction was probably largely indiscernible if not invisible in the *artifactual* archaeological record. A variety of *biological* indicators might be used to signal the expanded use of dogs in a broad sense. These could include skeletal deformations, increased frequency of bones overall, evidence of selective breeding, and other factors. Some researchers have considered spinal deformation of dog bones found on Banks Island dating over 2000 years ago as relating to dog traction (Morrison, personal communication), however, a case can also be made for such damage to have been produced by use as a pack animal (Arnold, personal communication).

In contrast to the above, dog traction in its fully developed form has several reliable indicators that may be preserved in the archaeological record. These include whips and whip handles, harness parts, trace buckles, and swivels (Giddings 1952; Hall 1978). The simple presence of sled parts is not adequate evidence since these were clearly used prior to the use of dogs, although more than one researcher has proposed the so-called "built-

up" sled as a possible indicator, an idea which I will return to later.

So when do we have evidence for the emergence of developed dog traction? Historical descriptions of dog traction in northeast Asia date to the late 13<sup>th</sup> century (~700 B.P.) when Marco Polo recorded the following:

In order to travel over the frozen surface of the ground, they construct a sort of vehicle, not unlike that made use of by the natives of the steep and almost inaccessible mountains in the vicinity of our own country, and which is termed a *tragula* [emphasis in original] or sledge. It is without wheels, is flat at the bottom, but rises with a semicircular curve in front, by which construction it is fitted for running easily on the ice. For drawing these small carriages they keep in readiness certain animals resembling dogs, and which may be called such, although they approach the size of asses. They are very strong and inured to the draught. Six of them, in couples, are harnessed to each carriage, which contains only the driver who manages the dogs, and one merchant, with his package of goods (Polo 1958:325-326).

Other sources, originally uncovered by Birket-Smith (1929:169), suggest other contemporaneous use of dog traction in Asia. Most specific is the account of Ch'ang Te who observed the Kirghiz use of dogs as draft animals around A.D. 1259 (Bretschneider 1910 [I]:129). Field and Prostov (1940-1941: 388-406) noted the presence of dog harness parts in a site near the mouth of the Polui River that dates between the first and sixth century A.D. The Asian sources imply at a minimum the contemporaneity of dog traction between Asia and North America, if it did not actually originate on the west side of Bering Strait.

Although several authors (Ackerman 1984:110; Ford 1959:156; Rainey 1941:547) have been willing to concede at least the potential for dog traction as early as Birnirk times, others (Giddings 1952:62-63; 1960:124; Hall 1978:212-216; Hickey 1979:427) have only conceded its existence in the last three to five hundred years. On the other hand, in Canada there is long-standing evidence of dog traction accompanying the spread of Thule culture (by whatever means) dating to at least 1000 years ago on Banks Island in the Amundsen Gulf region (Arnold, personal communication) and on Ellesmere Island no later than 900 years ago (McCullough 1989). Thus, unless we believe dog traction emerged in full form immediately after

the expansion to the east, it must have been adopted in Alaska before 1000 B.P.

Returning to the issue of built-up sleds, I believe, as suggested by VanStone (1955:115) and others, that the change in sled construction is indicative of dog traction. It provides an intermediate point between no evidence of dog traction and the presence of all *definitive* elements.

As its name implies, the built-up sled featured a raised cargo area, lifted above the runners by curved wood or antler arched supports which increased clearance by a factor of two or more. Built-up sleds featured much narrower sled shoes and were altogether lighter and more flexible than their counterparts which had a series of cross slats connected directly to wider, heavier runners.

The built-up sled featured several advantages in moving about snow-covered terrain. One improvement would be in traversing snow and snow-covered ice, where a built-up sled would be less likely to accumulate snow between the runners. It would also be less likely to hang up on rough terrain, especially the roots and snags one would find in interior areas. The shorter, narrower sled would also be better fitted for close, windy trails that would be found in forested or brushy conditions. For Northwest Alaska, Burch (1998a:196) notes that the built-up sled or *uniapiaq* (Figure 1) was preferred in interior districts while the long, low *qamun* (Figure 2) was preferred in coastal areas. In general, it is hard to visualize much travel through soft conditions without the assistance of dogs. If we accept built-up sleds as evidence (or at least a necessary precursor) for the beginnings of developed dog traction, then it began as early as 1500 B.P. during Birnirk times. In any event, it is very



Figure 2 - *Qamun* or low sled (from Murdoch 1892: 355 [Figure 357]).

hard to make a case for its adoption any later than 1200 B.P., assuming the eastward Thule expansion with dog traction post-dates somewhat the actual beginnings of that transportation. Interestingly, as far as I can tell, built-up sleds which show up in Alaskan Birnirk assemblages never made an appearance in the eastern Arctic, even in historic times. Thus, there is a situation where people, if they actually migrated to the eastern Arctic as late as 1000 B.P., left behind or discarded a significant component of dog traction technology.

## DOG TRACTION IN HISTORIC ESKIMO SOCIETIES

The following discussion is based mainly on the vast body of data compiled by Burch for Northwest Alaska (especially 1998b, but also 1972, 1975, 1976, 1980, 1981, 1984a, 1984b, 1998a), on my own field research in the region (Sheppard 1983, 1986, 1988; tapes and field notes 1979, 1980, 1982, 1984, 1985 and 1987), and from University of Alaska/Bureau of Indian Affairs ANCSA 14(h)(1) research in Northwest Alaska between 1975 and 1992 (BIA ANCSA Oral History Collection, Bering Straits tapes 1975-1992, NANA tapes 1987). To avoid cumbersome over-citation, the reader may assume the sources of information are citations above unless otherwise noted.

The role of dog traction in historic Eskimo societies before the late 1800s was quite different from how it was used during the gold rush and later periods, when it filled much the same niche as the modern snowmachine. Rather than being a mode of personal transport, it can more correctly be conceived of as an improvement in freight hauling associated with pedestrian movements. In general, it allowed people to move more pounds of food

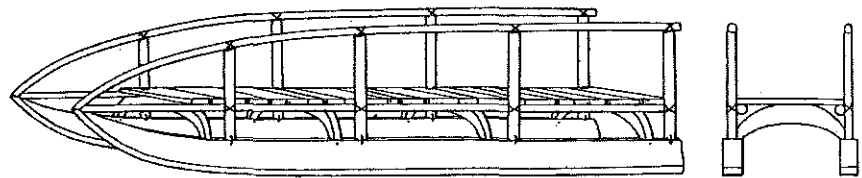


Figure 1 - *Uniapiaq* or built-up sled (from Murdoch 1892: 354 [Figure 356]).

and possessions at a faster pace. Eskimo families throughout the Arctic typically had small dog teams and often augmented canine power with that of women and children. Estimates of the number of dogs per household are quite variable, but I believe about three per family is reasonable. People seldom rode on the sleds. The limitation in the number of dogs was reflective of the economic cost of the teams themselves, which required sustenance. In Siberia, the size of a family's team was directly correlated to its economic well being (Schnirelman 1994:183-185) and it is reasonable to suppose this relationship existed elsewhere. When times were good the number of dogs expanded and when times were bad the numbers decreased, by neglect and human consumption. Although individual families might have few dogs, they did augment their teams with those of other families for specific trips or activities.

It is important to re-emphasize that the husbandry and maintenance of dog teams was not simply a labor

saving development, it did have a significant economic cost. The actual investment in labor and resources could be very difficult to estimate. Historically, the pattern was generally not to feed the dogs in the royal manner in which they were kept in modern times. They were often left to fend for themselves (particularly in the summer) or ate items, bones, etc., that were generally not fit for human consumption. However, working dogs could not function on a maintenance diet and even their consumption of bones took away from potential starvation foods of their human keepers. Before the introduction of the snowmachine, a significant proportion of the subsistence effort was devoted simply to acquiring dog food, and after the adoption of mechanized transport some major species actually dropped out of the subsistence regime in certain locales. In Norton Bay, for example, almost all seal hunting ceased except for bearded seals (*Erignathus barbatus*); much of this decrease was attributed directly to the adoption of snowmachines and decreased need for dog food (Sheppard, tapes and field notes, 1979). Burch has shown that most of the 30 percent decline in daily subsistence consumption between 1966 and 1984 could be accounted for by the subtraction of working dogs from the community (1985:110-112). Clearly, modern people faced much greater dietary demands from large teams used for trapping and other activities, but still one must factor in a significant level of subsistence effort for pre-contact societies. As will be seen below, dog traction facilitated a more diverse diet, but it also required it.

Dogs were important for both summer and winter travel. During the summer, families harnessed the animals to umiaqs and used them to help move the heavily laden craft upstream. They were also used as pack animals. The discussion below will, however, concentrate on winter use of dogs with sleds—partly for the sake of simplicity, and because of the relative invisibility of summer use in the archaeological record.

Winter use of dog teams can be roughly classified into three main areas: household movements, retrieval, and visiting. Families within most Northwest Alaskan societies had two types of household movements, small-scale point to point camp shifts in mid- to late-winter and longer-distance, major household movements from winter areas to break-up-time subsistence sites. The former consisted of the movement of nuclear or small extended families from one resource area to another and did not necessitate moving all possessions that had been hauled to winter sites. In contrast, the latter involved the movement of many people and all their goods, including kayaks and umiaqs, over many miles.

Retrieval took two forms as well. Long distance, seasonal retrieval involved the recovery of cached food acquired earlier in the year. The best example of this is the retrieval of caribou meat which was stored near kill sites while items of higher priority, mainly hides and sinew, were hauled back to settlements on foot. Short-distance retrieval involved the fetching back, usually by women, of harvests by pedestrian hunters.

Visiting varied in scale depending on the abundance of harvests, ranging from the aggregation of members of a single society, to larger inter-societal gatherings at messenger feasts, and occasionally even larger trade fairs involving several societies. Visiting, in effect, amounted to household movements, but typically of intermediate to long distances without the need to haul all of a household's possessions. But sleds could be used to haul food, needed possessions, trade goods, and occasionally people. To give some quantitative perception of the mobility provided by dog traction, the following table (using data from Burch 1998b) presents some estimates of distances between winter and spring settlements (the second column) and maximum distances for intra-societal aggregation (the third column). Looking just at the ability of these societies to aggregate, several have maximal distances of over 30 miles, two or more days by dog team and perhaps double that without the teams. Figure 3 shows the distributions of the societies named below.

Kivalina - Kivalliñigmiut	2-30	20
Lower Noatak - Napaaqtugmiut	20-25	10
Upper Noatak - Nuataagmiut	-	45
Upper Kobuk - Kuvaum Kaġianiġmiut	-	60
Middle Kobuk - Akunigmiut	-	50
Lower Kobuk - Kuuġmiut	-	10
Kotzebue - Qikiqtagrugmiut	30	20
Lower Selawik - Kiitaagmiut	-	18
Upper Selawik - Siilvim Kaġianiġmiut	-	35
Buckland - Kaġiaġmiut	0-35	55
Deering - Pittaġmiut	0-4	40

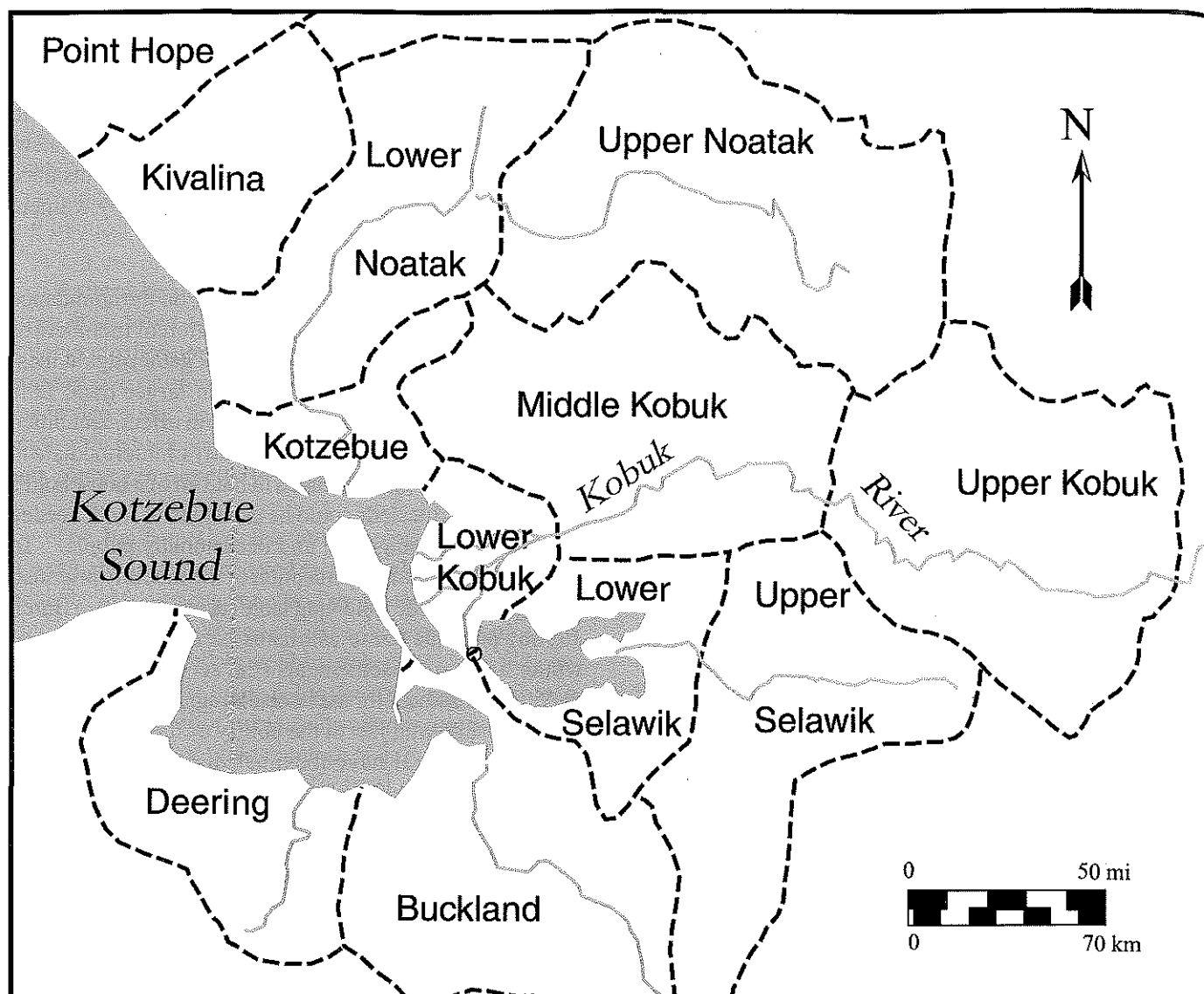


Figure 3 - Northwest Alaskan Eskimo Societies mentioned in text.

### MODELING THE ABSENCE OF DOG TRACTION

If one took away dog traction from an early historic Eskimo society, what would be the result? It should be recognized that there was nothing that was done with dog sleds that could not have been done, at least theoretically, by humans pulling sleds. However, the use of dogs decreased the effort involved in moving people and goods and increased range and speed.

In looking at the probable effects of the absence of dog traction it is most reasonable to propose general trends. Some of the logical implications of a more burdened, pedestrian society would be as follows: (1) smaller overall yearly-ranges; (2) increased coastal orientation away from inland resources; (3) a reduction in the amount of tools and supplies carried away from coastal areas; (4) longer

stays at more widely separated locales; and (5) decreased winter visiting activities, especially between members of different groups.

In terms of more specific impacts, one of the greatest would be on mid-winter caribou hunting carried out from scattered small settlements. By all accounts such an enterprise was problematic at best with dogs and would be very difficult without that increased mobility. Dog traction allowed a greater search range in looking for prey as well as quicker and more efficient retrieval of the cached proceeds of hunts. In the absence of dog traction, caribou exploitation would track much more closely to up-cycles when peaks in the prey population would mean greater likelihood of pedestrian hunters encountering the animals at widely distributed locations. In down cycles, exploitation might be limited to late summer hunts, which were indispensable in providing good hides,

sinew, and other products but less important for the food supplies. The absence of dogs would require pedestrian packing of the summer harvest in a less efficient manner in terms of total time, number of round trips, and potential loss of cached items.

A variety of secondary impacts can be predicted; among these would be a less buffered economy. As Halstead and O'Shea (1989:123) have noted, there are a variety of ways in which hunter-gatherers can buffer themselves against economic uncertainty, including: (1) a more heterogeneous resource base; (2) exploitation of a broader hunting range; and (3) increased exchange. The absence of dog traction limits all of the above because fewer resources could be effectively exploited and people would have less ability to increase their overall hunting range. The absence of improved transportation would also limit the ability of families and larger groups to disperse in hard times. It would also be more difficult to simply pack up and leave an entire territory, which was the last resort for historic Eskimos.

The impact of smaller ranges would be in smaller group sizes. This in turn would likely result in a greater percentage of exogamy. Although historic groups had a decided preference for societal endogamy, in practice a great deal of exogamy occurred. For smaller groups, out-marriage would be an absolute necessity and thus the level of relatedness between adjacent populations would be high.

The combination of small group size and increased kin connections suggests that violence at both local and regional levels would be much more rare. This commonsense statement is supported by observations of modern tribal warfare. Amongst the Yanomamo, stable alliances between villages, hence less violence between them, were predicated on the exchange of women (Chagnon 1992:160-162). Elsewhere, Pospisil noted for the Kapauku of Papua New Guinea an inverse relationship between relatedness and the level of violent conflict. Additionally, there are obviously basic logistical problems for small groups to organize the type of large-scale, long-distance conflict that characterized the immediate pre-contact and early historic period.

The decrease in group size, general mobility, and reduced ability for broader inter-societal interaction would mean that information flow would be slower and more localized. This might lead to more pronounced local differences in dialect, art, clothing and other cultural aspects that are not so closely tied to local economy and material problems, and thus have a greater ability to drift. Tech-

nological innovations developed in one area would also move more slowly across long distances.

It is simplistic to contrast only the total absence of fully developed dog traction with its most sophisticated late prehistoric form. It is very arguable, however, that technological changes following the development of built-up sleds are minor, thus most of the significant structural changes resulting from fully developed dog traction are the result of the new sled form. On the other hand, the use of heavier, lower coastal sleds pulled by dogs could have enhanced coastal adaptations and settlement with little definitive appearance archaeologically. One could visualize the changes as "Dog Traction Lite," with changes restricted mainly to the coastal zone.

## ARCHAEOLOGICAL MANIFESTATIONS

How would the preceding affect what one might find in the archaeological context? From a settlement standpoint there would be a greater concentration of winter sites in coastal locations. In the absence of dog traction, it would be harder for families to move people and supplies overland. Fewer sites would occur away from the coast and those that were found in interior locations would be more restricted to ones that were more readily accessible by waterborne transport. Fewer interior sites would reflect winter occupations because use of the sites, even if accessed initially by water, would require pedestrian movement back to the coast carrying all goods, supplies, boats, and people moved there earlier in the year. Basically, the absence of dog traction would encourage coastal settlement and discourage settlement away from the coast, but it would not absolutely require or eliminate either option.

Coastal and interior sites would likely differ significantly in assemblage size and diversity. It stands to reason that if one must bear the full burden of one's tool kit and use it for a shorter period of time, fewer, more multi-functional tools would be preferable over many tools with very specific uses. One would also be encouraged to manufacture tools on site using locally available materials. Both of those factors would encourage the use of chipped stone over ground slate in interior contexts. The latter would be more time consuming to make and would be less widely distributed. Ground stone tools also tend to have been manufactured for single or limited functions.

Increased local stylistic variability should be reflected in the archaeological context. Such variability would be most apparent in more permanent coastal settlements where there would be a more elaborate and diverse tool kit.

## DOG TRACTION AND NORTHWEST ALASKAN PREHISTORY

I believe there is a strong case to be made that many of the major prehistoric changes in the last 1500 years are the consequence of the adoption of dog traction. In particular, I would contend that reduction in stylistic variability in late prehistoric assemblages owes much to the leveling influence of significantly increased information flow. Prior to about 1000 B.P., Northwest Alaska can be characterized as having regional styles of art and tool forms, including Okvik, Old Bering Sea, Ipiutak, and Birnirk. This stylistic variation was conventionally interpreted in terms of cultural sequences, but there is now good reason to believe all of those archaeological classifications are essentially contemporaneous (Gerlach and Mason 1992). Regional stylistic variation would be encouraged by the absence of dog traction, which would limit long-distance interaction to summer months when people could travel by boats.

The ability to exploit caribou more efficiently in the winter is likely, as Hall (1978) suggested, the factor that promoted the gradual expansion of Eskimos up the Kobuk and Noatak drainages. Winter caribou hunting provides an explanatory basis for settlement shifts observed by Harritt (1994) at Kuzitrin Lake and Hall and Gerlach (1988) at Tukuto Lake. In both cases, earlier occupations of these inland locales was evidently the result of short-term summer use, whereas late prehistoric occupations were characterized by winter use long-term enough to warrant the construction of more substantial and more enduring semisubterranean structures.

One of the most significant general issues facing archaeologists in northern Alaska and indeed throughout the Arctic is the adoption of whaling. Previously, it has been suggested (for example McCartney 1984) that surpluses generated by whaling would have allowed the maintenance of dog teams. I believe it is worthwhile to turn this question around and consider the transportation and

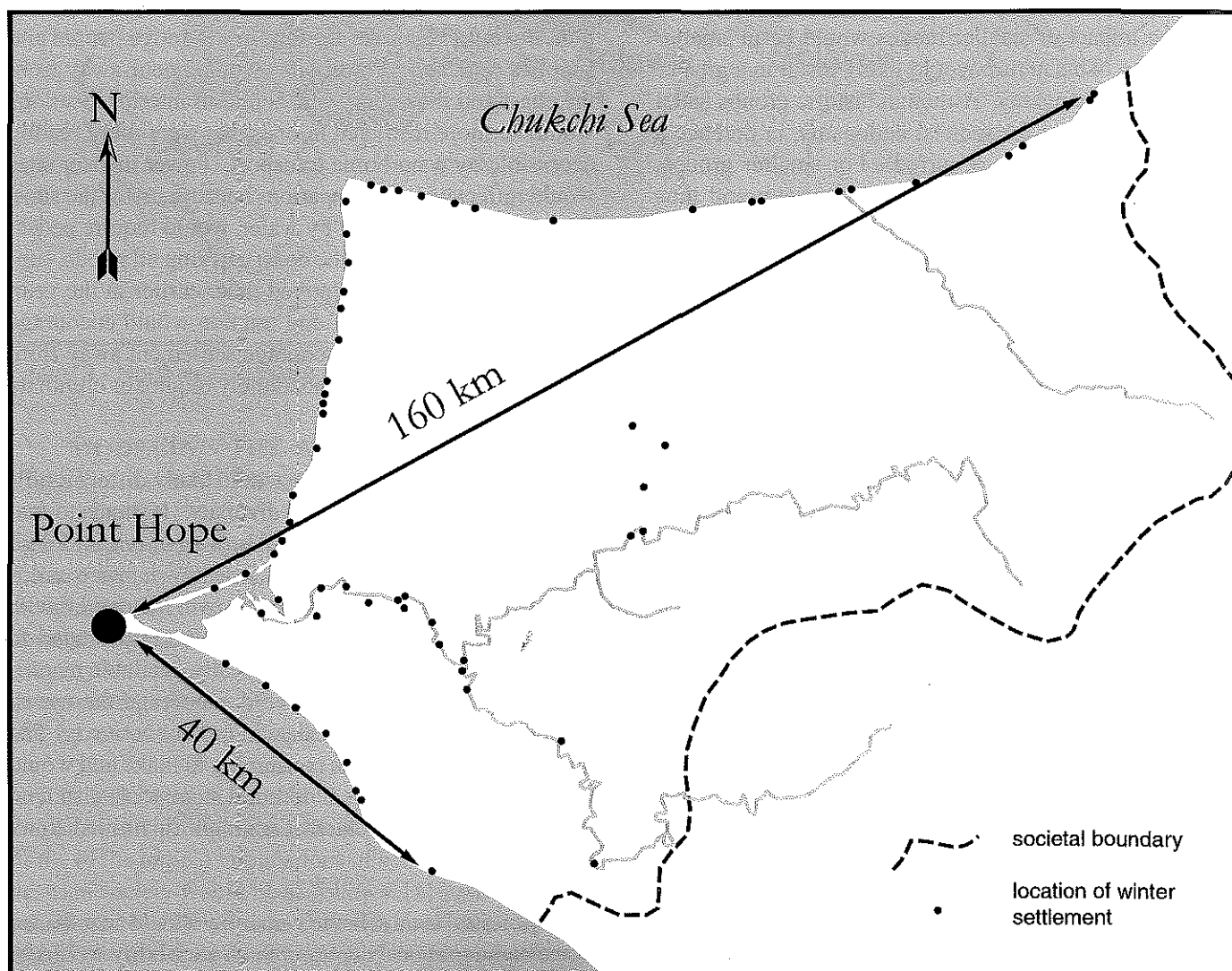


Figure 4 - Maximum aggregation distances for Point Hope whale hunters.

logistical issues related to whaling. At best, even with modern harpoon technology, indigenous whaling can be characterized as highly stochastic. The chances of an individual strike yielding a capture are low, as are the chances of an individual crew making a kill. Thus, the success and effectiveness of whale hunting, particularly with prehistoric technologies, would be contingent on the degree to which multiple crews could be mustered. For example, consider the hypothetical aggregation of six crews. Each crew of eight fit adults would likely represent the manpower contribution of several households composed of four to six people. If households supplied on average two adult hunters then the total manpower of 48 whaling crew members for six boats might involve the total aggregation of at least 24 families, or 96 to 144 people. Even in the best cases, this concentration of people would require the coalescence of families dispersed over literally thousands of square miles. For example, Figure 4 shows the maximum travel distances for Point Hope society members to aggregate for whaling at *Tikiġaq* (Point Hope). While such aggregation is technically feasible by foot travelers, it would have been greatly enhanced by dog traction and may have significantly increased the ability of people to be at the right place at the right time. Naturally, there would be feedback in terms of acquisition of surplus and increased team size; however, I believe there is a strong case for dog traction to be considered integral to the adoption of whaling, not simply a consequence. This is even more clearly the case if one adopts such complex formulations as Sheehan's (1995) model of the interrelatedness of whaling and interior subsistence.

The implications of dog traction for mobility, interaction, group size and other factors also cannot be neglected in analyzing changes in prehistoric social relationships. In a recent paper, Mason (1998) has used the concept of "polities" in analyzing Birnirk, Old Bering Sea, and Ipiutak site distributions and assemblages. Mason's polities are equated with the territorially defined "societies" as developed by Burch (1980, 1984, 1998a, 1998b). While it is laudable to treat the regional prehistory in cultural rather than archaeological terms, it is evident from the preceding discussion that such polities in the historical sense were largely predicated on the integrative ability of dog traction. One wonders whether anything remotely resembling the historical model could have been present without it. I am in greater accord with Burch (1998b:316-317) that a regional system of societies and boundaries likely may have existed as early as Punuk times, and this is more consistent, for example, with the best concrete evidence of warfare, that is armor, which does not occur until that time.

One of the subtler considerations about the effects of dog traction in changing prehistoric societies relates to the recognition that overtly similar archaeological reflections of prehistoric ecologies [faunal remains, technology and the like] can be the result of dramatically different systems. This recognition, or lack thereof, can involve striking differences in interpretation. For example, Giddings and Anderson (1985:318-322) characterize the levels of Choris, Birnirk, and Western Thule caribou exploitation at Cape Krusenstern as essentially the same. Despite the apparent similarities in utilizing this terrestrial resource, the comparable faunal assemblages could have been produced in very different ways. For example, the relatively high level of caribou exploitation by Choris people was likely sustained by local availability of the animals during periods of higher resource levels. Birnirk and Thule occupants of the area could have sustained similar harvest levels either through local hunting or in inland hunts.

Another problem to be considered is how almost exactly the same activity could reflect a system of very different costs and benefits. Harritt (1994:372), for example, notes very similar levels of harvesting of ringed seal (*Phoca hispida*) in Ipiutak and Early Western Thule occupations on Cape Espenberg. The historic pattern for exploiting that resource from Cape Espenberg was to haul tents and other gear far out onto the ice where entire families camped for sustained periods (Burch 1998b:301). Such a pattern was predicated on the use of dog traction for moving camps, hauling harvested seals, and enlarging the breadth of the hunting area. In the absence of dog traction all of those activities would consume more time and energy. Thus, while the archaeological reflection of the activities between the two periods is comparable, one might propose different underlying structural differences in the exploitation of that resource, with and without dog traction, such as size and organization of the hunting groups or perhaps a more protracted period of seal hunting at the expense of other seasonal activities.

In the examples above, the lack of consideration of differences related to dog traction produces a situation where similar archaeological remains can be interpreted as reflecting essentially the same cultural ecology. Looking outside Northwest Alaska, a study by Helmer (1992) illustrates an inverse phenomena, where the same lack of consideration exaggerates differences between two prehistoric traditions. Looking at Devon Island settlement strategies, Helmer concluded that Early Paleo-Eskimo inhabitants did not locate their settlements optimally with respect to resource distributions, while Thule people did

do so. In his analysis, optimal locations were those that minimized the distances to the overall range of resources. But clearly the very definition of optimality would vary for people with markedly different mobility strategies. It makes sense for highly mobile Thule foragers to situate themselves to make most efficient use of several resources from a more fixed residential base. Whereas fixed residential bases make less sense for earlier pedestrian hunters with doubled or tripled travel times and significantly less ability to either haul the proceeds from a hunt long distances or to cache and retrieve them at a later date. The utility of Helmer's analysis would be considerably improved by looking at optimality in terms of both residential versus logistic mobility strategies, in the sense discussed by Binford (1980). It is my contention that many of the other apparent differences between Dorset and Thule are exaggerated by the effects of changes in transportation and derivative effects on settlement patterns, assemblage diversity, and other areas. But that is another paper.

## CONCLUSIONS

Given the critical importance of the dog traction issue, several related questions need to be addressed. First, we need the ability to establish the use of dog traction in the absence of definitive technological indicators. Until that problem is solved, the date for the actual inception of this technology is educated guesswork. A first step might be an examination of skeletal deformation in modern working sled dogs and comparison to prehistoric remains. Second, differences in the technological development of dog traction in the eastern Arctic versus the west need to be addressed. The development proceeded differently in the two areas; the east was more advanced in some respects with the early adoption of swivels and buckles but very conservative in the late use of pegged versus lashed runners. Third, if indeed I am correct in my assertion about the use of dog sleds before 1000 B.P., we need to focus on factors involved in the significant time lag before the Eskimo expansion into interior areas such as the Kobuk and Noatak rivers. Finally, there remains another nagging question. If dog traction was so advantageous to Eskimo societies that they were willing to incur the cost of adopting this technology, why was it not adopted by neighboring subarctic Athapaskan and Algonkian peoples as well?

I have long believed that dog traction is the "polar bear in the living room" for Arctic archaeology. Its largely ignored presence has significant implications for virtually every previous discussion and analysis of Eskimo prehis-

tory. More serious consideration of this issue has, I believe, the potential to overturn some of the main pedestals of northern prehistory. One must underscore the essential need to consider all analyses of prehistoric Arctic cultural systems in light of how dog traction, or its absence, fundamentally effects the functioning of those systems and how they might be reflected in the archaeological record.

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# A HISTORY OF HUMAN LAND USE ON ST. MATTHEW ISLAND, ALASKA

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**Abstract:** St. Matthew Island, an often fog-bound, wind-swept island located in the middle of the Bering Sea, 370 km from the nearest populated Alaskan settlement, remains a largely unknown entity in terms of its history of human land use. Few cultural surveys have been conducted on the island to date. In 2002, the United States Fish & Wildlife Service sponsored an attempt to investigate the history of island land use. This paper reports on the results of this survey and its relationship to previous reconnaissance and testing efforts. Known prehistoric and historic land use activities are summarized and suggestions are made to assist future cultural resource survey efforts.

**Keywords:** Bering Sea, World War II, Thule, Fox trapping

## INTRODUCTION

St. Matthew Island, located in the Bering Sea far from the Alaskan mainland, is uninhabited and seldom visited due to its relatively isolated position and lack of resources desired for development. St. Matthew is one of three islands that make up the Bering Sea Wildlife Refuge, managed by the U.S. Fish and Wildlife Service (USF&WS). The other two islands within this refuge are Hall and Pinnacle Islands. The St. Matthew group of islands is located approximately 300 km west of Nunivak Island, 370 km south of St. Lawrence Island, and 425 km north of the Pribilof Islands.

In order to obtain current data on the condition of the islands' flora and fauna, the USF&WS has been sending a team of biologists to St. Matthew and Hall islands about once every five years. The usual duration of each visit is ten days. In 1997, an archaeologist accompanied this team for the first time to begin to gather information on the history of early human land use on the island. This survey located a single prehistoric house and three historic sites, all located near the western end of the island. In 2002, the USF&WS sent another archaeologist and assistant to St. Matthew to continue collecting data on early island habitation. In order to build upon the results of the 1997 survey, which focused on the western end of the island, the 2002 survey team focused their attention on the Big Lake area of St. Matthew, close to the island's eastern end. This report summarizes the findings of the 2002 ten-day survey.

Prior to conducting the 2002 cultural survey, little information was readily available regarding the results of previous investigations within the Bering Sea National Wildlife Refuge. The results from earlier projects had not been widely disseminated. In attempting to compile information from historic and culturally-based reports, the author found that, invariably, each project failed to take into account the results of earlier projects and research opportunities were spent establishing similar databases rather than building upon earlier conclusions. In this paper, aside from presenting the results of the 2002 survey, I attempt to summarize earlier investigations within the St. Matthew Island group that pertain to earlier human land use activities, in addition to making recommendations that may help to focus future research attempts on the islands.

## PHYSICAL DESCRIPTION

St. Matthew Island (Figure 1), known as Bear Island by whalers (Dall 1870:249), Gore Island (Harper's Weekly 1875:1) by Captain Cook (Maynard 1898:306), and Choris Island by other explorers (von Kotzebue 1821:294), measures approximately 51 km long by 6 km wide and comprises a total of 331 km<sup>2</sup> of land. The island's terrain is generally hilly with prominent peaks and a series of ridges rising over 305 m above sea level. Sea forming cliffs abut the island ridges and provide nesting areas for a wide variety of sea birds. Elevation on the island ranges from sea level to over 457 m at Cape Upright.<sup>1</sup>

<sup>1</sup>An earlier reference (Frink et al. 2001:131) to the tallest peak on St. Matthew Island extending 2373 m above sea level is in error.

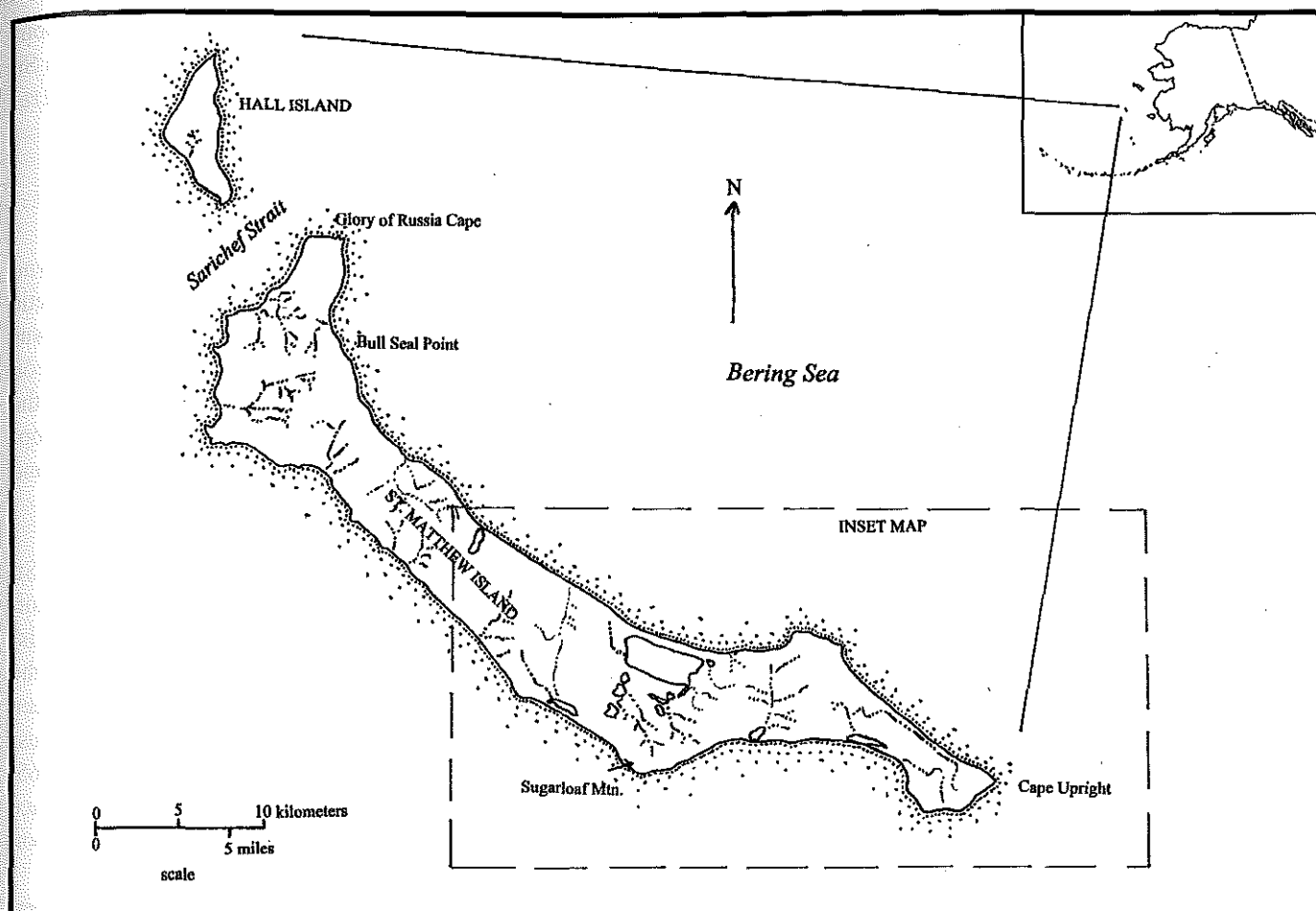


Figure 1: Project Area Map

St. Matthew Island is dissected by numerous valleys with many small streams, ponds and lakes. Vegetation is dominated by low growing tundra. The low growing tundra vegetation in the lowland areas is quite lush, but as elevation increases, the flora becomes sparse and is replaced by rock scree where lichens are the dominant species.

Island flora (Klein 1959; Rausch and Rausch 1968) is similar to that of many Bering Sea islands and contains many indigenous plants known to have been important to Native people on Nunivak Island and the Alaska mainland (see Griffin 2001). St. Matthew Island fauna included many species that were historically actively harvested in Alaska including terrestrial mammals such as polar bear, arctic fox, and an occasional red fox, in addition to marine mammals such as Steller sea lions, walrus, Larga seal, and several species of whale. Polar bear were once year-round residents of St. Matthew but disappeared in the 1890's from over-hunting (Hanna 1920).

Twenty-nine reindeer were introduced on St. Matthew in 1944 (Klein 1968) in order to establish an alternative food source for the military stationed on the island during World War II. The military personnel were re-

moved from St. Matthew in 1945 and the herd was left free to multiply, reaching 1,350 animals in 1957 and 6,000 in 1963. This rapid population increase reduced the available forage on the island, which, coupled with a severe winter, resulted in a crash in population to 42 animals during the winter of 1963-64. The last reindeer alive was seen in the mid-1980's.

In addition to mammals, the Bering Sea National Wildlife Refuge has a population of over one million sea-birds and six breeding land bird populations (DeGange and Sowls 1978:22). Four species of freshwater fish are also known to be available in island lakes and streams. St. Matthew Island offered many species of terrestrial and marine mammals, birds, fish, and indigenous plants that could have been utilized by humans that chose to visit the island. To date, evidence of land use prior to the island's "discovery" by the Russians in 1767 only includes the location of a single house pit on the western end of St. Matthew Island.

St. Matthew and Hall Islands were recognized by the United States government for their abundance of sea bird colonies and were set aside on February 27, 1909

(Executive Order 1037) by President Theodore Roosevelt as a bird refuge known as the Bering Sea Reservation (Hanna 1920:118). The refuge's name was changed to Bering Sea Refuge in 1940. In 1970, the reservation was added to the nation's wilderness system (Public Law 91-504) and is now referred to as the "Bering Sea Wilderness."

## KNOWLEDGE OF ST. MATTHEW

Why wouldn't this island be ideal for human settlement? Its isolated location must be considered one of the chief factors against its popularity, with Nunivak Island being the closest inhabited land. Was St. Matthew's whereabouts generally known by prehistoric peoples? Frink (Frink et al. 2001:136) has reported that on a clear day, residents of Nunivak have seen St. Matthew and that Nuniwarmiut residents have old stories that reportedly document earlier visits to the island by Nunivak hunters. Efforts by the author to record any Nuniwarmiut stories relating to St. Matthew Island have been unsuccessful, to date. None of the Mekoryuk elders interviewed in 2002 can recall ever hearing stories that relate to knowledge of the existence of or past travels to St. Matthew. Contemporary fishermen, however, have reported seeing St. Matthew in the spring while fishing off the island's shores (Howard and Muriel Amos, personal communication August 4, 2002). Nunivak Island is approximately 305 km from St. Matthew Island. Visibility on the ocean is generally reported to be clear for about 37 km (i.e., view to horizon). Reported sightings of land located over eight times the normal range of human eye sight are difficult to account for and may represent mirages rather than actual island sightings.

The Nuniwarmiut, however, are not the only Natives to have reported seeing land much further than is usually believed possible. Veniaminov (1984:134-135) stated that the Aleuts of St. Paul reported seeing Unimak Island in clear weather in the spring time. Contemporary Aleut elders have reported similar spring sightings (Aquilina Bourdukovsky, personal communication July 18, 2002). St. Paul is located approximately 460 km north of Unimak Island. If we can assume that during the spring visibility on the ocean is greatly enhanced and that with proper weather conditions a human can see land located at extreme distances, where would early peoples likely have come to St. Matthew from? What portion of the island would they have chosen to settle? Once there, why didn't they choose to stay? Why haven't more sites dating to the prehistoric period been found on St. Matthew? These are all questions that were attempted to be answered by each of the island's previous cultural resource

surveys but remain unresolved. The following sections summarize the known history of human land use on St. Matthew Island, in addition to the aim, coverage, and results of previous island cultural surveys.

## HISTORIC EXPLORATION

St. Matthew Island was first discovered by Lt. Synd of the Russian Navy during his explorations in the arctic from 1764-1768 (Coxe 1803:264). Captain Cook was next to reach St. Matthew when he noted the island on July 29, 1778 (Maynard 1898:306). Exploration of the island was to follow slowly, with a number of ships stopping off and making observations of the island's flora and fauna over time.

The earliest recorded historic occupation on St. Matthew was by the Russians and their Aleut fur hunters. Five Russians and seven Aleuts from St. Paul Island in the Pribilofs were reported to have passed the winter of 1810-1811 on St. Matthew, where they had been dropped off to collect polar bear furs. Four of the Russians are reported to have died of scurvy (Elliott 1886:465; 1898:191) or starved to death due to the disappearance of the sea mammal populations the hunters depended upon for survival (Dall 1870:248, 326).

Henry Elliott and Lieutenant W. Maynard visited St. Matthew in 1887, while reporting on the Pribilof Island seal rookeries, and cited finding the ruins of the huts which had been occupied by the earlier Russian/Aleut crew. There is some question whether these Russian-Aleut "huts" were built on St. Matthew Island, Hall Island, or both. Modern researchers (Klein 2002; Sowls, personal communication, July 31, 2002) had earlier considered a large house-like depression located near the west end of St. Matthew to have been the site of the earlier Russian occupation. Hanna (1920) suggested that the remains of a Russian hut was identified by their party on Hall Island in 1916. Hanna (1920:121) reported that during the period of Russian occupation, a party of five men from St. Paul Island landed on Hall Island where they built a cabin. This party later had to leave the island due to marauding polar bears. It is likely that Hanna's account reflects that earlier reported by Elliott and Maynard.

## HISTORIC USE OF ST. MATTHEW ISLAND

### Fox Fur Harvesting

The primary attraction of the Arctic to the Russians was the availability of furs for the home market. While sea mammal fur (e.g., otters and seals) remained a pri-

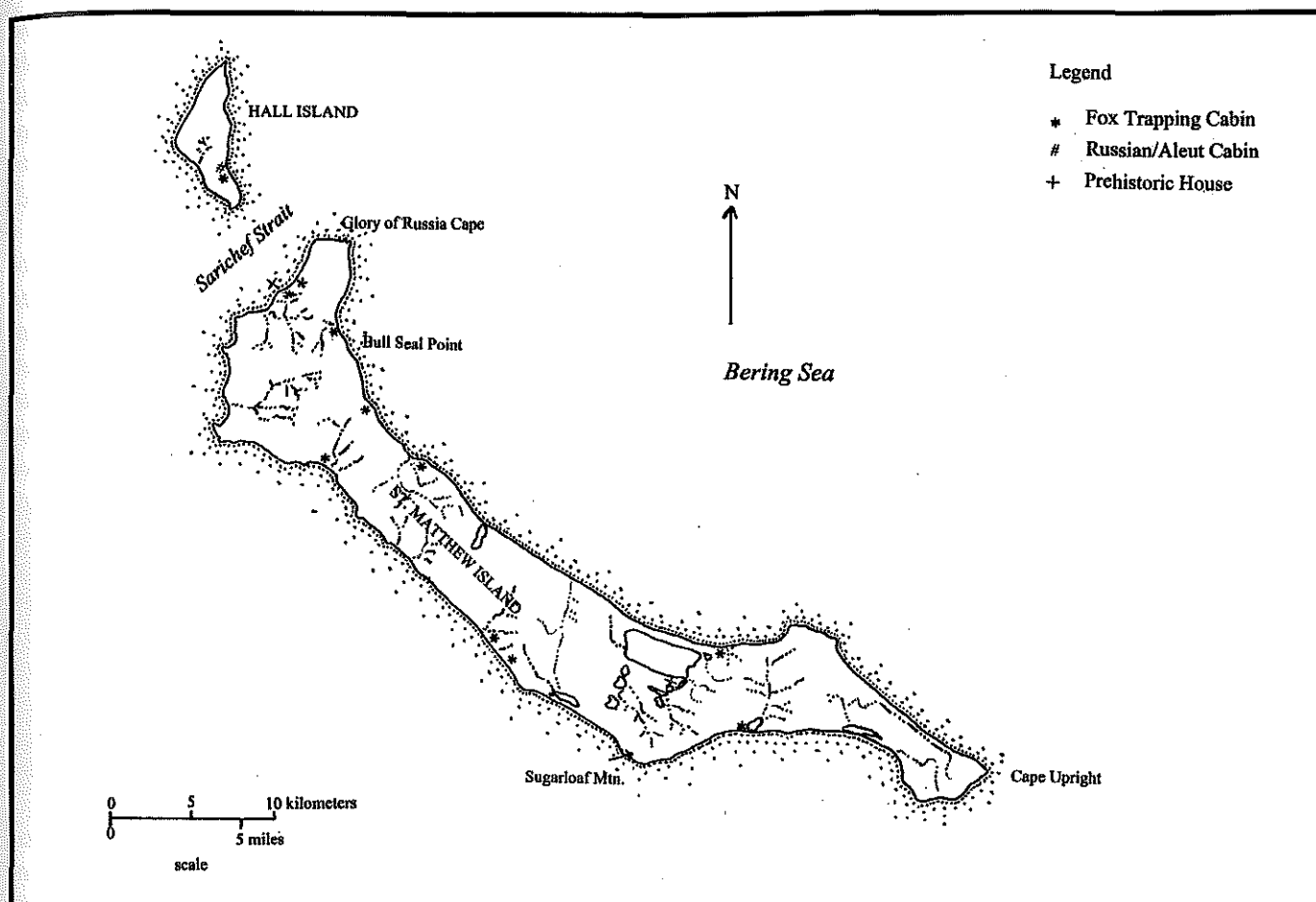


Figure 2: Location of Earlier Reported Cultural Sites

mary focus of most early Russian expeditions, the opportunity to harvest fox fur was also recognized. The Russians were the first people to introduce arctic foxes (*Vulpes lagopus*) to some areas of Alaska for the purposes of fur harvesting (ca. 1750); a practice that was encouraged by the United States after it acquired the Alaska Territory in 1867. Fox farming and trapping spread throughout the Aleutians and continued to grow rapidly in the early 1900s. In 1911, due to the overexploitation of particular fur-bearing species in the Arctic and their rapidly decreasing numbers, an effort was instituted to protect fur seals and sea otters. To assist in their protection, the Aleutian Islands Reservation was created. Fox farming escalated as a result of these restrictions, as an alternative to hunting sea otters (Janson 1985).

The rise in price for fox furs resulted in the consideration to introduce foxes on Alaskan islands in the Bering Sea, including Nunivak (Ivanoff 1933) and St. Matthew (Hanna 1920:121). The interest in St. Matthew appears to have not been developed due to the inability to provide an ample year-round food supply to foxes, which would have resulted in the foxes having to prey on resident bird species the island had been earlier set aside to protect. As clothing styles are known to change, the craze for fox

furs could not last, and during the 1930s a sharp decline in fur prices occurred. Fox farming in the Arctic largely collapsed during the Depression and never recovered.

Given the high price of fox furs in the Arctic from the early 1900s to 1930, it is no surprise that people began to exploit native populations of foxes on the Alaskan mainland and offshore islands. Historic efforts to harvest foxes on St. Matthew are known to have been conducted by residents of Nome. Historic accounts (Beals 1944; Hanna 1920:121) mention the construction and use of numerous trappers' cabins on the island. David Klein (2002), while studying the island's introduced reindeer population, recorded the location of numerous fox trapping related structures on the island (Figure 2). Many of these represented substantial cabins used by trappers while others appeared to consist of temporary, small structures only large enough for a man to sleep in while checking a trap line. The differentiation between structure types was not referenced by Klein at the time of recordation.

### U.S. MILITARY OCCUPATION

Alaska's offshore islands played a major role in U.S. defense during World War II. After the attack on Pearl

Harbor in December of 1941, the Japanese considered the U.S. Navy to be seriously crippled. On June 3, 1942 the Japanese began their offensive on the Aleutian Islands with an attack at Dutch Harbor and the establishment of military bases on Kiska, Agattu, and Attu (U.S. Coast Guard 1946a:65-69). In response, the U.S. military began operations in Alaska to remove the Japanese occupation in the Aleutians, defend the United States from further foreign attacks, and help U.S. Armed Forces in coordinating the War in the Pacific against the Japanese. These efforts involved the island of St. Matthew in two ways, the establishment of a weather station in 1942, and a Loran-A navigation station in 1943.

### Army Intelligence Weather Station

Following the Japanese attacks in the Aleutian Islands, the U.S. military sought quickly to establish a presence on Alaska's offshore islands. With war threatening the country, the military needed to find a way to gain advanced information of any imminent attack. The weather in the Bering Sea was often quite severe with heavy fog and strong gales. The U.S. Army Intelligence decided to send small groups of men to several strategic island locations to establish weather stations that could operate as an early warning system of weather conditions and approaching enemy forces. Such a group was sent to St. Matthew Island on September 15, 1942, under sealed orders (Rhode 1987:84).



Figure 3: Army Weather Station, ca. 1943. (Photo by Ben Schlegel; courtesy of Elaine Rhode)

Ten men were sent to St. Matthew's southern coastline where they established a weather station east of Sugarloaf Mountain. This station consisted of "two Quonset huts, radio transmitters, weather instruments, food for a year, 40 tons of coal and one .30 caliber machine gun" (Rhode 1987:84) (Figure 3). The station's equipment and supplies were off-loaded on the beach near the proposed site location; however, on the night of their arrival, high seas carried much of the coal out to sea before

it could be moved to higher ground (Schlegel 2002). The remaining equipment was moved approximately ½ mile inland with construction of the camp being completed on October 4<sup>th</sup>. The station's radio transmitter was first operational on October 7<sup>th</sup>, where it continued to serve the military as an early warning system until the fall of 1943.

Provisions for the camp were sparse and local subsistence resources were relied upon to supplement the crew's diet. Dolly Varden trout were caught in the local stream and lake and became a regular part of their diet. In addition to local fish, hair/ringed seals (*Phoca hispida*) were taken from the beach and eiders were killed in the island's interior (Rhode 1987:85; Schlegel 2002). Once winter set in, the island remained cut-off from the mainland and daily life at the weather station focused around taking daily weather readings and maintaining life at the station. Weather observers had a wet-bulb cyclometer, maximum/minimum thermometer, and hand-held anemometer (wind velocity) that were all recorded with data relayed every six hours to Anchorage. In addition to a .30 caliber machine gun, the men had 1903 Springfield rifles to use for protection (Rhode 1987:86). By examining historic photographs taken by Sgt. Ben Schlegel during the 1942-1943 camp occupation, a number of machine gun emplacements are known to have been constructed at the site. These include emplacements along the sand dunes to the south of camp, on the tundra directly below (east) of the camp, and on higher ground to the west of camp.

Replacements for the original crew arrived on St. Matthew in June 1943, with this second crew only remaining at the weather station until late fall, when the Army decided to consolidate their quarters with a Coast Guard Navigation station that had been established in June of that year, nine miles to the west. The weather station was totally abandoned by the U.S. military by late 1943, with only sporadic visits to the station by the Coast Guard over the next two years to pick up equipment that had been left behind by the Army.

### U.S. Coast Guard Loran-A Station

The Coast Guard constructed a Loran-A station on St. Matthew Island in June 1943. Loran, derived from Long Range Navigation, is a system whereby a vessel, with special charts, may determine its position in all weather even at a great distance from shore (Willoughby 1980:150). This navigation system was reliable up to 1290 km from the transmitting station during the daytime and 2250 km at night. In order for such a system to work effectively, ground stations needed to be located appro-

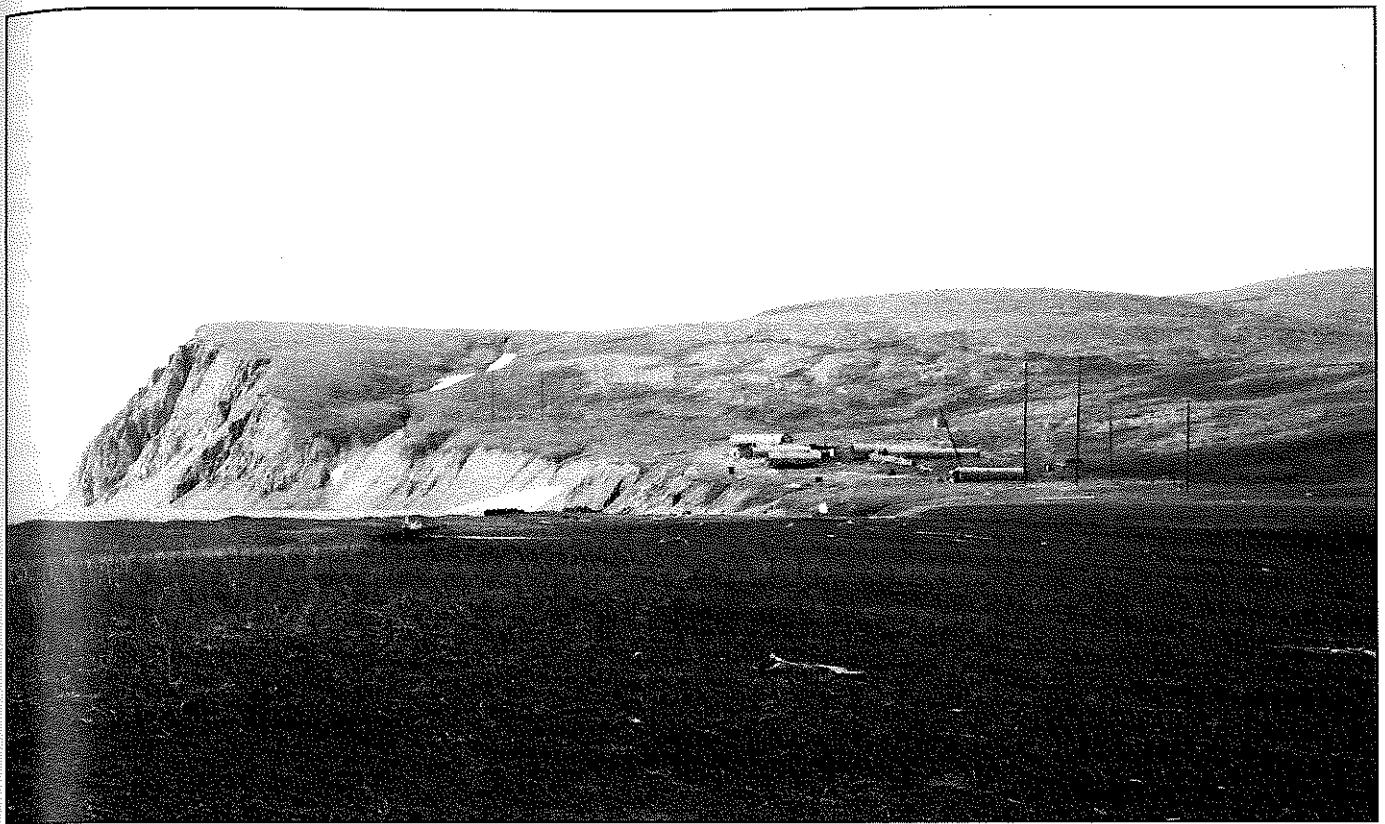


Figure 4: Coast Guard Loran A Station, ca. 1963. (Photo courtesy of Dr. David Klein)

privately to cover the area needing to be served. Two shore stations (a master and slave station) operated as a Loran "pair" which would establish a set of lines over a portion of the earth. Designed during the early days of World War II, the establishment of a Bering Sea chain initiated its use in the Pacific.

The Bering Sea chain consisted of transmitting (slave) stations on St. Matthew and Umnak Islands, a double master station on St. Paul Island, and a monitoring station at Cape Sarichef on Unimak Island (U.S. Coast Guard 1942c:23). The St. Matthew station was designated Site P-3 (Unit #5, code name "Mike") (U.S. Coast Guard 1946b:125; Willoughby 1980:158). Construction materials and crew left Dutch Harbor on June 17, 1943 aboard the cutter *Clover* and proceeded to St. Matthew Island to establish the "slave" station along the island's southwest coast, within 0.4 km of a suitable beach landing. The Coast Guard's Loran-A station on St. Matthew was situated on a 15 m bluff (Figure 4) overlooking the Bering Sea (U.S. Coast Guard 1946c:29).

The St. Matthew Island Loran site was covered with ice and snow when the construction crew first arrived. From the landing area to the construction site, a trail had to be established that would permit vehicles to transport supplies over volcanic sands that were two to three feet

deep. One small tractor had been furnished to the construction crew to facilitate this move; however, the tractor proved too small for the task and was constantly bogged down in the sand (U.S. Coast Guard 1946c:31). A larger tractor had to be shipped in to complete the station's construction. Area tundra was found to be 45 cm to 2.4 m thick and unstable when frozen. In order to set up the six Quonset huts and generators over 600 sacks of cement had to be used in their foundations (U.S. Coast Guard 1946c:31). The St. Matthew station started testing on September 11, 1943, with intermittent transmissions from the completed Bering Sea Loran chain in operation by late September, 10-hour daily service by October 18, 1943, and 24-hour service on July 10, 1944.

The exact date of the St. Matthew Loran-A station's abandonment is unknown but it is thought to have been abandoned after the end of World War II (Rhode 1987:87). Coast Guard correspondence states that it was decommissioned by 1947 (Yhusin 1947). After the conclusion of the war, the Navy Department requested that the three tracts of land used for Loran ground stations in the Bering Sea (i.e., St. Matthew, St. Paul and Umnak islands) be retained since they were still required by the government in view of the proposed permanent Loran station relocations in the Aleutians (Reed-Hill 1947; Richmond 1947). With such a plan in mind, equipment was left on the is-

land for possible later use. Historic photographs reveal that the camp remained largely intact in 1963 with several structures still standing as late as 1977.

## **MANAGEMENT OF ISLAND FLORA AND FAUNA**

Aside from the above mentioned projects, the USF&WS has sponsored or been directly involved in coordinating numerous periodic visits to St Matthew and Hall Islands in order to conduct surveys of the islands' bird and mammal populations and local flora. Robert Rausch and his wife Virginia participated in one of the earlier such studies. In 1954, the Rauschs' spent six weeks on St. Matthew searching for evidence of parasitic disease in the island's vole population, in addition to collecting other data on the island's flora and fauna (Rausch and Rausch 1968). The Rauschs were taken to St. Matthew Island by the US Coast Guard who helped them to construct a cabin near the island's shoreline near Big Lake. This cabin was later used by other island survey crews in 1957 and 1963 (Klein 2002). In 1982 this hut had been partially destroyed by the rising level of the ocean's tide, with no evidence of its existence found in 2002.

## **PREVIOUS ARCHAEOLOGICAL INVESTIGATIONS**

To date, archaeological investigations on St. Matthew Island have been extremely limited. The first informal cultural resource investigation on the island occurred in 1957 when David Klein conducted limited testing of a house-like depression located on the north end of St. Matthew Island. Dr. Klein, then a biologist with the USF&WS, conducted the testing as a favor for archaeologist Frederick Hadleigh-West from the University of Alaska Fairbanks. Prior to Klein's arrival to the island, West had been informed of the existence of a possible house depression located near the island's western end. Klein (2002) excavated a 45 cm wide x 45 cm deep trench across the house feature where he discovered what appeared to be a stone-lined floor upon which he is said to have recovered the remains of a seal oil lamp, numerous other pottery shards and a polar bear tooth. The recovered material was later given to West who deposited the artifacts in the University of Alaska Fairbanks Museum (collection #UA-63-61). While recently viewing the artifacts comprised within this collection, no bear tooth was found but worked whale bone and a possible hammerstone were located. Pottery shards (n=57) exhibited a heavy gravel temper.

In 1963 Francis Faye accompanied Klein to St. Matthew where they discovered the remains of a wooden house on Hall Island believed to be the remains of a Russian dwelling. The structure was thought to have measured 4.6 m x 6.1 m and to have been made from timber brought to the island by boat (i.e., not driftwood) (Klein 2002). This house site is located on top of a dry ridge along a point of land above the only beach access area on the island. In 1982 and 2002, USF&WS biologists are known to have pitched their tents in the general vicinity of this structure.

In addition to the above remains, in 1985 Klein found a peculiar feature near a small lake, behind (south of) Big Lake. Klein originally believed that this feature might be left from a previous island habitation (Klein 1987b). He later tested this area but found no cultural deposits, and now thinks the feature may be of natural origin (Klein 2002). It is unclear what degree of "testing" was conducted at this site, however, and no examples of similar features have been seen on St. Matthew Island to date.

It is interesting to note that Klein, a biologist, is responsible for providing the most detailed notes on man-made features on St. Matthew. Aside from noting the possibility of prehistoric sites on the island, Klein visited the World War II era weather and Loran stations during his cross-island treks, as well as noting the location of numerous fox trapping related structures. Historic photographs provide views of three of the old trapper cabins (cf. Griffin 2002). Klein (2002) noted that only one weather station Quonset hut was still standing at the Army's weather station installation in 1957.

The first professional cultural resource investigation on St. Matthew occurred in 1976 and was led by E. James Dixon (1976). The 1976 survey was conducted in order to identify prehistoric archaeological remains that may date to the period of the Bering Land Bridge. The survey team focused its efforts on the eastern half of the island (Figure 5), while paying close attention to areas likely to reveal evidence of previous human use or occupation. Approximately 59 test probes (size unknown) were excavated with a trowel during their island survey in hopes of identifying evidence of Pleistocene deposits suitable for archaeological preservation. No evidence of prehistoric human occupation of the island was discovered. The crew did comment that pebbles of jasper and agate were commonly seen on the island's northern beach, although no large outcrops of workable stone material were identified.

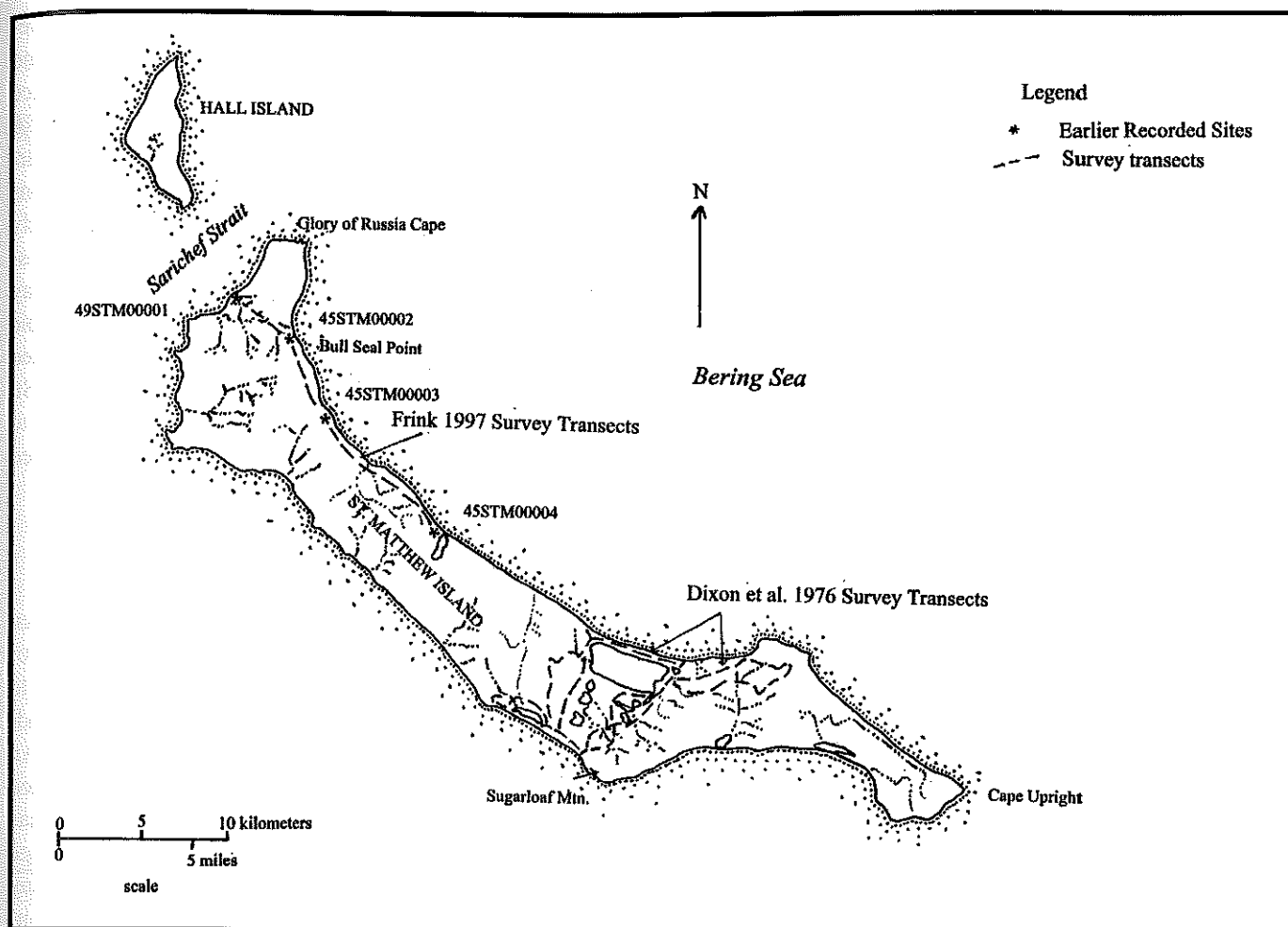


Figure 5: Previous Survey Transects and Recorded Archaeological Sites on St. Matthew Island

Dixon found one possible archaeological site near the southern end of a beach near Sugarloaf Mountain. This site consisted of a log protruding from the bank with a piece of baleen lying on top of its upper surface. The frozen bank made test excavation of the area impossible at the time of discovery. The baleen was later  $^{14}\text{C}$  dated to  $1325 \pm 140$  radiocarbon years.<sup>2</sup>

In spite of the continued involvement of the USF&WS on St. Matthew, it appears that the results from each of their periodic visits to the island were not shared with other resource specialists within their agency or the Alaskan archaeological community. At the time the first archaeologist was sent to accompany USF&WS biologists (i.e., Lisa Frink in 1997), no information on previous island archaeological discoveries were known.

Agency bird biologists regularly visit St. Matthew and Hall Islands once every 5 years in order to obtain population counts for the island bird rookeries. In follow-

ing federal OSHA regulations, by being the single archaeologist Frink was unable to conduct forays on her own on St. Matthew, hence her survey efforts were necessarily linked to accompanying bird biologists on their bird count transects. By nature of their surveys, the bird biologists tended to focus their attention on cliff oriented rookeries. Frink's survey efforts were necessarily restricted to these general areas (Figure 5). Despite these survey restrictions, Frink located four sites including the earlier identified prehistoric semi-subterranean house (tested by Klein in 1957) and three historic sites on the northwestern portion of the island (i.e., she more accurately relocated three earlier reported sites and discovered one new potential site). Frink excavated two 20 cm wide trenches within the earlier tested house depression (Pottery House site - 49XSM00001), with the trenches extending to a depth of 0.45 m below surface where sterile soil and gravel were reported (Frink et al. 2001:133). Total fill excavated was less than  $0.5 \text{ m}^3$ , with all soil examined by hand without the use of screen mesh.

<sup>2</sup>Laboratory Number - GX-5070; material dated - Baleen;  $^{13}\text{C}$  corrected. Uncorrected  $^{14}\text{C}$  age - 1195 BP. Using the reservoir correction calculated by Dumond and Griffin (2002) for 14 matched pairs of marine mammal residue and terrestrial products (580 to 783 year difference with an average of  $735 \pm 20$  yrs, the baleen is likely to date to approximately 500 BP.

Frink's test excavation revealed Thule-like pottery, bone fragments, and two charcoal samples that yielded sufficient material to obtain radiocarbon dates. A total of 40 pottery sherds were recovered from the Pottery House excavations by Frink. These shards were found to represent at least two different vessels. The pottery is described as being heavily tempered, primarily with gravel, with one shard exhibiting evidence of grass temper. The recovered faunal remains included 245 bones, with 49 bones (17 %) having distinguishing characteristics to be positively identified. Of these, 43 were mammal bones representing fox, polar bear, whale, and walrus. Six of the bones were from birds including puffins and cormorant (Partlow 1998). Analysis of the two radiocarbon samples yielded conventional dates of  $350 \pm 60$  and  $430 \pm 50$  BP (Frink et al. 2001).<sup>3</sup>

The remaining three sites located by Frink include: 1) Bull Seal Point (49XSM00002), 2) Cabin site (49XSM00003), and 3) the Firewood site (49XSM00004) (Frink et al. 2001:134). The Bull Seal Point site consists of a single large rectangular depression (5.3 x 3.3 x 0.35m), which compares favorably to the location of a previously identified cabin site plotted by Klein. It is likely that this site represents the location of an earlier fox trapping related cabin. The Cabin site consists of a single feature measuring 4.0 x 3.5 x 0.35 m deep. A pile of fallen wooden beams was seen covering the depression providing evidence of the existence of an earlier semi-subterranean, historic cabin. The location of this site also matches that of a previously reported cabin site (Klein 2002) and is thought to relate to earlier island fox trapping activities. The last site located by Frink is the "somewhat ephemeral Firewood Site" (Frink 2000:6). This site consists of two erect pieces of wood that are thought to represent the foundations for a firewood cache. Possible human-created depressions were noted at the site but the excavation of a 0.2 x 0.2 x 0.3 m test probe revealed no cultural material and these depressions are now thought to be a product of natural erosion and run-off (Frink 2000:6).

In spite of their rediscovery, none of the above sites have been formally recorded. The recording and mapping of each site and their associated features needs to be completed in the future.

## 2002 SURVEY RESULTS

The 2002 cultural resource survey of St. Matthew Island was not project driven. That is, the survey was

not designed to identify potentially significant cultural resources within a distinct area of land slated for some land disturbing activity. As such, the 2002 cultural resource survey was largely opportunistic with potential areas for survey selected by previous knowledge of preferred areas of human occupation and topographic considerations (Figure 6). An effort was made to relocate and formally record all previously mentioned cultural sites on the island. Given that the 2002 survey was limited to a ten-day period (July 21- July 31), with a crew consisting of Debbie Steen (USF&WS) and the author, attention was focused on visiting likely locations within the eastern half of St. Matthew Island. This area was selected as a means to compliment the earlier west end survey effort conducted by Frink in 1997.

## Prehistoric Survey Results

The only possible prehistoric site known to have been identified in the eastern half of the island was the site of baleen found by Dixon in 1976. An effort was made to revisit the site where the baleen had been discovered eroding from the shoreline, west of Sugarloaf Mountain. An intensive survey of the exposed shoreline was conducted with negative results. This portion of the shoreline has been subjected to severe erosion and slumping. Large slump areas were visibly affected by incoming tides. Any potential site areas visible in 1976 have long since vanished.

No attempt was made to examine the large house-like depression seen by Klein in 1985 in the Big Lake area of the island, because information regarding the existence of this feature was not discovered until after our return from the island. In spite of the apparent negative results from reported probing of the feature by Klein (2002), it is recommended that this feature be tested by archaeologists in the future due to its large size and shape and the absence of similarly shaped features being reported on the island.

What was conspicuously absent from our survey results was any evidence of prehistoric occupation. The majority of the island that we surveyed is low-lying and exposed to strong winds, which are quite frequent. Much of the land in the center of our survey area was extremely marshy. Observed island drainages were all small with many dry by the time of our visit. Fish were abundant, however, in most larger streams (i.e., over 61 cm [2 ft] in width) and island lakes. The eastern peninsula of the island is extremely flat and exposed to winds from all

<sup>3</sup>Beta-11765 - wood;  $^{13}\text{C}$  ratio -26.1; 2 sigma calibration AD 1443 - 1647 (CALIB 4.4)

Beta-11767 - wood;  $^{13}\text{C}$  ratio -24.6; 2 sigma calibration AD 1409 - 1525, 1559 - 1630 (CALIB 4.4)

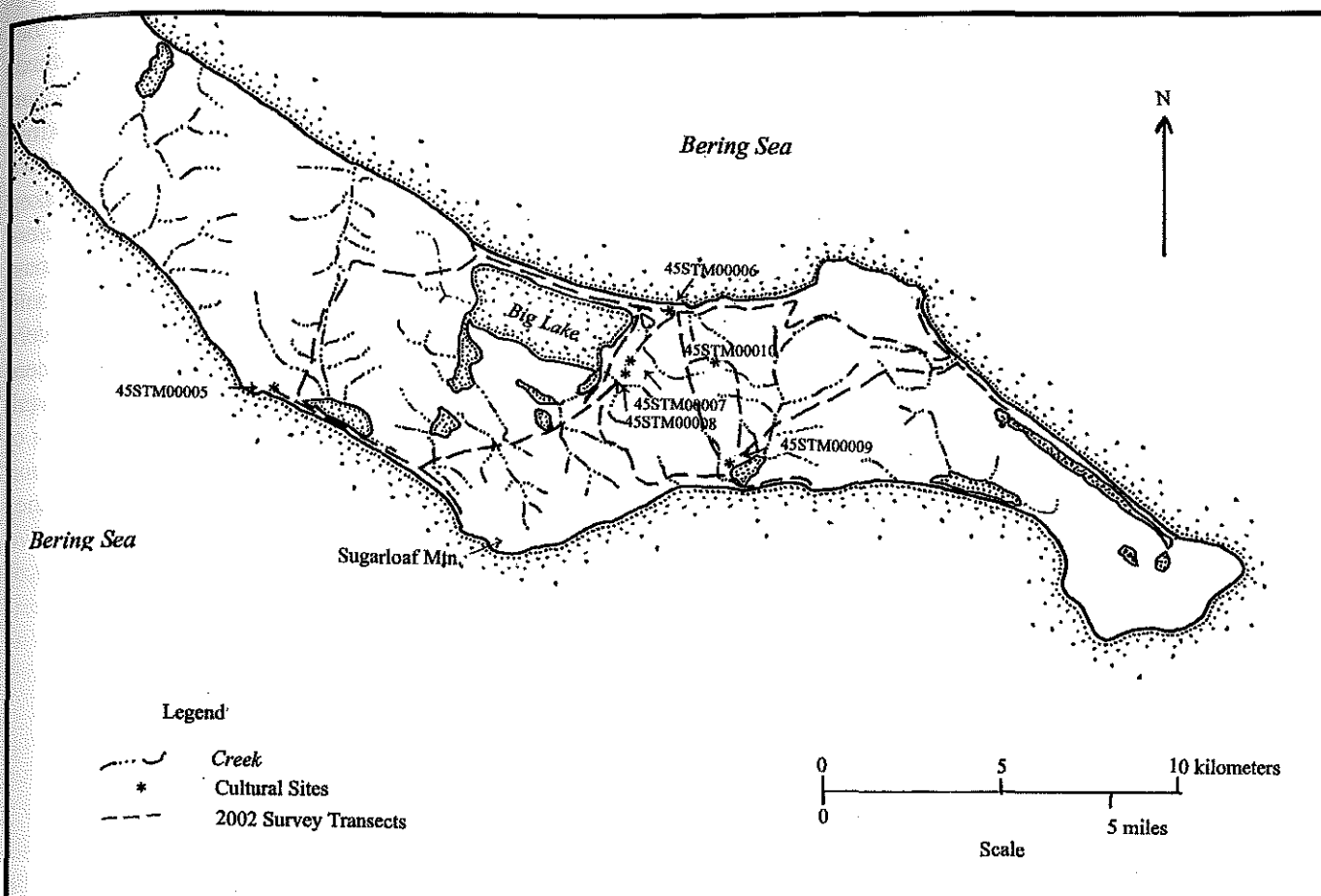


Figure 6: Inset Map - Survey Transects and Recorded Sites from the 2002 Investigation

directions, and is not thought to be ideal for human habitation. The extreme southern tip of the island consists of steep cliffs and bird rookeries. While not surveyed during the 2002 survey, I believe that this area is too inhospitable and open to the elements to warrant a primary focus during future survey efforts. At the completion of our survey, after we had reboarded the Tiglax and were en route back to the Pribilofs, the biologists on Hall Island reported finding what they believe was the remains of an earlier Native habitation. Several buried walrus skulls were noted in one area while an extremely weathered and possibly human-modified tusk was found in another. This constitutes an ideal area for future study.

### Historic Survey Results

The 2002 survey results (Griffin 2002) substantiated the use of the island by fox trappers, the US military, and modern visitors. Six historic era sites were identified and formally recorded. One of these sites is related to earlier fox trapping practices on the island. Five sites relate to the island's military occupation.

Big Lake Fox Trapping Cabin & Cache (49XSM00006) consists of the remains of a large semi-subterranean house and collapsed cache located on the

southern edge of a beach terrace, east of Big Lake. The feature is surrounded by a large earthen mound approximately 12.5 m in diameter x 1 m in height. The structure is made from milled lumber, some of which are 2"x 6" tongue and groove planks. The sod-covered roof has collapsed, obscuring the majority of the feature. Approximately 14 m to the southwest is the remains of a collapsed sod-covered cache. Cache size is difficult to determine due to its collapsed condition and thick covering of sod. Vertical planks can be seen through the collapsed sod in addition to a few large spikes.

Military-related sites include the Loran-A Station (49XSM00005), Army Intelligence Weather Station (49XSM00009), two abandoned caterpillar tractors (49XSM00007 and 49XSM00008), and a possible weather station related structure (49XSM00010). The Loran-A Station consists of the remains of numerous structures from the abandoned World War II Loran-A station. Established in 1943, this station originally is thought to have consisted of six quonset huts which housed 23 men and typified a Loran "slave" station in Alaska. The site itself consisted of prefabricated Quonset huts that were often interconnected to provide combined administration/electronics/quarters buildings (Coneybeer 1998:4).



Figure 7: Loran A Station, 2002.

A complete cultural resource survey of this site was not possible at the time of the 2002 visit due to site size, limited time available for survey before the crew's departure, and the small two-person survey crew. Instead, a sketch map of the site was made and numerous photographs taken to illustrate the current site size and condition (see Griffin 2002). In general, the overall site condition of the Loran-A station is considered poor. It appears that much of the refuse and debris from the Coast Guard installation was left on-site and has deteriorated from the severe weather conditions. Hundreds of 55-gallon metal oil drums have rusted into fragments leaving behind large oil spills that can still irritate your eyes while walking through the site. The remains of the wooden storage sheds have largely blown down and pieces of wood and metal can be seen scattered across the tundra. Rusty motors remain in place and only arctic foxes continue to visit the site (Figure 7).

During the winter months, snow is extremely heavy and prolonged snow blizzards of hurricane strength often last up to ten days. Guide ropes and tunnels had to be constructed linking several of the huts together so servicemen would not become disoriented and lost while traveling between huts. Evidence of such tunnels can still be seen at the site, with the longest tunnel (approx. 50 m long) connecting the central part of the camp with the Loran transmission hut which was located near the eastern end of the site. Figure 8 illustrates the remains of this hut and tunnel system.

When the US Navy constructed the St. Matthew Island Loran Station in 1943 they found there was approximately 45 cm - 2.4 m of tundra over the selected

site. The area had to be "prepared" before construction so that site features would not sink once ground became unfrozen (Willoughby 1980:158). The Army used heavy "Cats" and tractors to remove the top layer of muskeg, "as the muskeg was so spongy that no permanent erection could be anchored to it" (US Coast Guard 1946c, I:128-129). In 2002, both tractors used in site construction were found near the middle of the island, where it appears they became stuck and were subsequently abandoned.

The St. Matthew Island Weather Station consists of the remains of two Army Quonset huts (one for communication and electronic equipment and residence of their operators, and the other as residence and dining hall for the remaining crew [Rhodes 1987:84]), and associated features. This site is known to have been abandoned in 1943, with evidence suggesting that in 1954 both Quonset huts still remained at the site. Only one structure was reported in 1957 (Klein 2002), with the communication hut having been burned to the ground by unknown persons. By 1982, the surviving hut had collapsed due to inclement weather. The 2002 site survey revealed nine features in association with this site complex. These included remnants of the two Quonset huts, an outhouse, storage shed, wash area, work area, a small oval depression of unknown use, and a machine gun embankment. Historic photographs have helped to identify the true shape and composition of several of the identified features (e.g., Quonset huts, a stone encased stove feature, wood-cutting brace).

The last historic site identified consisted of the remains of a square, semi-subterranean pit located on top of a ridge saddle. The above ground portion of the struc-



Figure 8: Loran A Station Quonset Hut and Tunnel, 2002.

ture has been destroyed by wind and wood debris can be found around and below the saddle area. The ship-lap milled planks and lantern found inside the pit are identical to those found at the island's military Weather Station, located to the south. This site is located in relatively close proximity to the US military weather station and is believed to have been built and used by the soldiers that manned this station in 1943. Ben Schlegel (2002), a soldier stationed at the weather station from 1942 to June 1943, knew of no such structure being built during his tenure on the island, so its use must have post-dated his departure. Historic photos taken in the 1950s are believed to represent a view of this structure prior to its destruction (Figure 9).



Figure 9: Army Weather Hut (?) on St. Matthew Island, ca. 1957. (Photo by Dr. David Klein; courtesy of George Allez)

Modern tour boats are known to stop off at St. Matthew Island with passengers rafted ashore for beach combing and bird watching activities. A quick survey on the internet identified three companies currently offering trips to St. Matthew Island (e.g., Clipper, Wild Wings, and WinWin Vacations tour companies). While most of these groups are thought to visit the island in the vicinity of Bull Seal Point in the northwest part of the island, visitors could and probably do visit many other of the island's exposed landing areas. The 2002 discovery of two driftwood lean-tos in the vicinity of Big Lake probably relate to recent island visits.

## SUMMARY AND RECOMMENDATIONS

The majority of cultural features recorded during the 2002 field season had been noted earlier by previous researchers/surveyors but none had been recorded. The 2002 survey represents the first attempt to accurately record, by graphic measurements and photographs, island cultural features. Future survey work on St. Matthew and Hall islands should formally record all previ-

ously noted island features. Copies of all completed site forms need to be deposited at the Alaska Office of History and Archaeology.

So what of the future? How can future surveys build on our current knowledge of island land use while achieving the greatest success in the short time available. My recommendations include:

1. Conduct probing and test excavations on Hall Island where biologists think that they have discovered the site of the 1810 Russian-Aleut hut. This site is reported to have been only used over one winter and would likely reveal a unique set of artifacts that would be useful for comparisons with other Russian era sites. Excavation results should be related to Veltre and McCartney's (1994, 2000, 2002) recent work on the Pribilof Islands. If this structure is found to have a prehistoric component, attempt to relate this area to the Pottery House site tested by Klein and Frink.

2. Conduct more extensive test excavations at the Pottery House site. Determine site depth and obtain more information regarding possible site use. Excavate subsurface probes/test units, both inside and outside the known house feature, using a fine-mesh screen to filter all excavated soil. All recovered artifacts should be recorded within standard stratigraphic or arbitrary levels (e.g., 10 cm).

3. Conduct subsurface probes in and near the possible house depression identified by Klein south of Big Lake.

4. Conduct a pedestrian survey of the western two-thirds of the island. Due to the island's relative inaccessibility and the probable limited time frame for future surveys, the focus of new surveys should be on river drainages and areas having beach access. Work should initially focus on surveying and recording any sites found in areas of reported fox trapping cabins, since these areas probably coincide with the most accessible lands on the island. Surveyors should rely on the use of a Zodiac inflatable boat (if available) to maneuver around the coastline. Cross-island pedestrian surveys, given the rocky terrain of the western portion of the island, will not prove highly productive, given the probable limited time frame of each visit.

5. Complete a more extensive review of federal archival sources to determine additional details of historic land use activities. Seek links to specific fox trapper cabins that might provide additional information regarding the

early use of the island for fox trapping and data concerning specific cabin locales. Contact previous USF&WS personnel in order to assimilate any information noted during previous island visits.

6. Distribute the results of all island investigations through regional conference presentations and published articles so that future researchers will have the benefit of your work.

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## REPORTS

# REDATING THE HOT SPRINGS VILLAGE SITE IN PORT MOLLER, ALASKA

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**Abstract:** In a symposium in honor of Hiroaki and Atsuko Okada held at the Annual Meeting of the Alaska Anthropological Association in 2002, Don Dumond noted that many of the radiocarbon dates from the Hot Springs site appeared to be 1000 years too old when compared with dates run in different seasons of the project. Since that time, an intensive re-dating project was instigated on the Hot Springs materials using charcoal and caribou antler curated in the collection. Nineteen new samples were submitted from contexts throughout the site and the results show that Dumond was indeed correct. The result is that 29 dates from the original excavations at the site must be discarded. A new Hot Springs chronological framework is presented here.

**Keywords:** Hot Springs Site, Radiocarbon dating, Aleutian Archaeology, Alaska Peninsula

## INTRODUCTION

The Hot Springs Village site, on the west side of Port Moller on the Alaska Peninsula, has been well known for many years (Figure 1). First investigated by Dall (1877) and Weyer in the 1920s (1930), it became known for its preservation of organic remains, the size of the village, and the depth of the deposits. In 1960 a group of archaeologists on a joint expedition between Meiji University in Japan and the University of Wisconsin conducted further investigations at the site, some of which were reported in English by Workman a few years later (1966). Between 1972 and 1984, six seasons of excavations were conducted by a group of scholars from Japan under the direction of Hiroaki and Atsuko Okada from Sapporo. The results of those excavations, described in a number of preliminary reports (Kotani 1980; A. Okada 1989; H. Okada 1984; Okada and Okada 1974a, 1974b, 1989; Okada et al. 1976, 1979, 1984, 1986; Okada and Yamaguchi 1975, 1976), have tantalized archaeologists, anthropologists, and others for many years. Whale bone masks, sculptures of humans carved in bone and ivory, thousands of artifacts representing nearly the entire gamut of North Pacific and southern Bering Sea styles, elaborate burial ceremonialism, and one of the largest groups of houses in the region, identified the Hot Springs site as critically important to the heritage of Alaska. These preliminary reports have been used

by many scholars including Dumond (1974, 1987a, 1987b, 1987c, 1992, 1998; Dumond and Bland 1995; Dumond et al. 1975, 1976), Johnson (1988), Maschner (1998, 1999a, 1999b, 2000), McCartney (1969, 1974, 1984, 1988, 1992), Workman (1989; Workman and McCartney 1998), Yesner (1985, 1998) and others (cf. Yamura 1977) in attempts to detail and describe the greater regional prehistory.

Hot Springs Village consists of over 200 house depressions spread along two sides of a hot springs that flows into Port Moller. The site area can be divided into four zones based on the distribution of houses and shell midden deposits. There is a small group of houses on the north shore of the peninsula, a large midden on the southwest shore of the peninsula, a large group of houses to the west of the springs and along the east edge of the springs, and then a line of houses and midden along the east edge of the site on top of the high bluff overlooking Port Moller (Figure 1).

The earliest excavations by Weyer were located on the eastern bluff edge and amongst the northern houses on the west side of the spring. The excavations of the joint Meiji–University of Wisconsin Project were primarily in the midden deposits along the shore in the south-

<sup>1</sup>I expected Hiroaki and Atsuko Okada to be co-authors on this paper. I had discussed the redating of the site and the new chronology with the Okadas during my last visit to Sapporo and Hiroaki Okada was in complete agreement with the new interpretation. But when this paper was written, Hiroaki was too ill to comment on the final draft and Atsuko was uncomfortable having their names attached without Hiroaki having read the paper. She encouraged me to publish this paper without them.

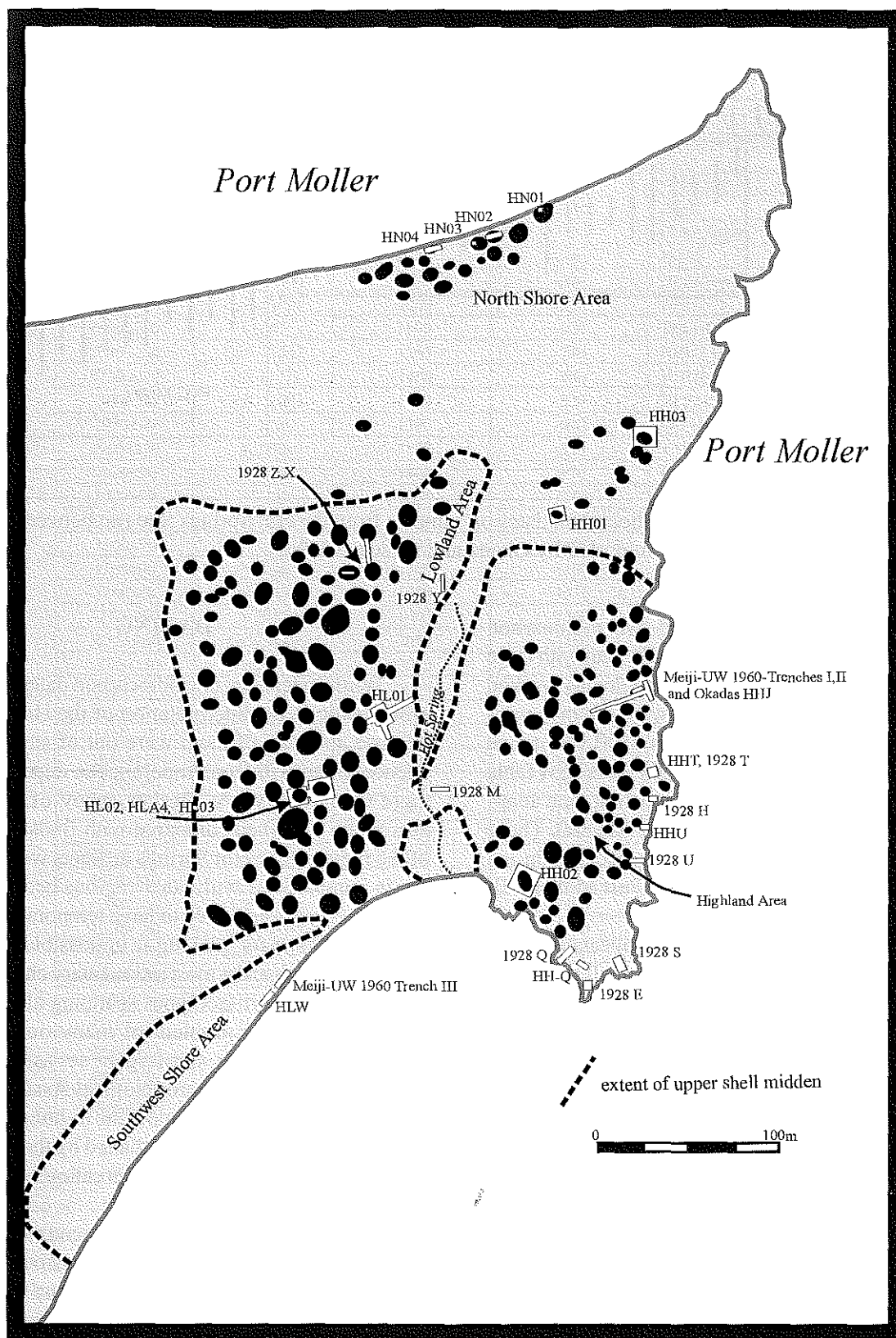


Figure 1: Map of the Hot Springs Site showing the locations of excavation units.

west part of the site. The Okadas excavated a number of large units across the site with the middens and the house floors as the primary research foci. Their most important excavations were along the bluff edge (Units J, T, Q, U, and HHO3), in houses just east of the spring but moving

up toward the bluff (Units HH01 and HH02), and in the large group of houses west of the spring (Units HL01, HL02, and HL03). They undertook smaller excavations along the north coast (Units HN02 and HN04) and along the southwest shore (Unit HLW).

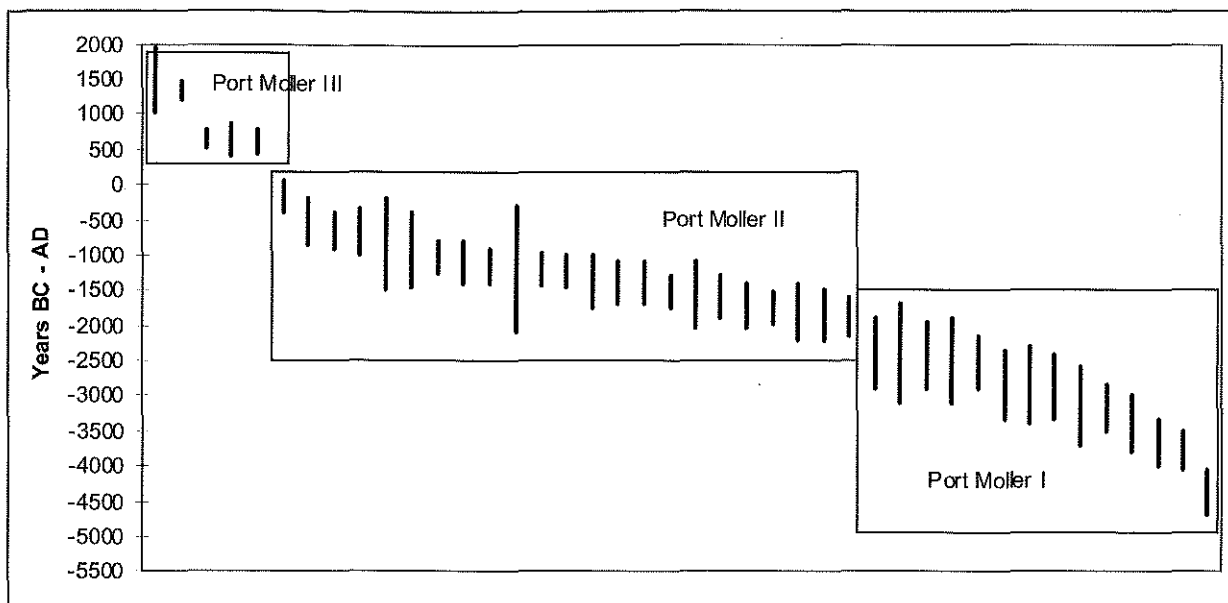


Figure 2: Calibrated ranges of radiocarbon dates on samples from the Hot Springs Site run between 1960 and 1986 and the chronology as reported by Okada and Okada (1989:4).

The 1960 excavation generated two radiocarbon dates, both in the early first millennium BC, but with large errors. During the six field seasons the Okadas worked at Hot Springs, 42 radiocarbon dates were run on a variety of deposits. These dates spanned a 4500 year range from approximately 5000 years ago to 600 years ago (Table 1), with only two small breaks in occupation, one at approximately 500 BC, and another about AD 800. These dates resulted in the development of a occupation sequence broken into three phases termed Port Moller I, II, and III (Figure 2).

Stratigraphically the site is quite complicated but two broad deposits were identified. An 'upper shell layer' was distributed across most of the site (see outline on Figure 1). This shell layer is composed primarily of cockles (*Clinocardium nutteli*) and other species from muddy or sandy intertidal regimes. This deposit appeared to date from the beginning of the first millennium AD to approximately AD 1100. While rather consistent across the site, these upper stratigraphic layers were occasionally disturbed by house construction, burials, and other features.

The lower deposits were dominated by crushed mussel shell (*Mytelus edulus*) and other species more common in a rocky intertidal environment. These deposits were deep and complicated, being truncated and mixed by numerous intersecting house floors, storage pits, burials, and other features. With radiocarbon dates spanning nearly 3000 years, the Okadas paid close attention to the details of the stratigraphic layers in these deposits, often using the characteristics of the deposits to compare excavation units and levels between units (Figure 3).

## DATING

While extensive, the radiocarbon dates often proved problematic to the interpretation of the Hot Springs site because so many of them were out of stratigraphic sequence, often with contemporaneous deposits spanning more than 1300 years. Further, many of the dates appeared too old when compared with finds in other parts of the region. The Hot Springs artifacts were difficult to use in comparisons with other regions because many of the artifact types appeared to have temporal distributions of 2000-3000 years, making it impossible to document anything more than the most rudimentary changes through time, with many artifact types spanning all three phases of occupation. The problem of many out-of-sequence dates was irresolvable and largely set aside until 2002 when, at the Alaska Anthropological Association meeting in Anchorage in a symposium in honor of the long history of Alaska research conducted by the Okadas, Don Dumond made the following observation:

"The field season of 1982 produced an unusual number of early radiocarbon dates from the Hot Springs site. Of 32 determinations from the site obtained over six field seasons, 13 are in excess of 4000 C-14 years. Nine of these were from 1982 excavations, out of a total of 15 determinations received that year from the Gakushuin laboratory. This brings me to one final point, one that seemed to jump out of my manipulation of the dates, and ...also rather aside from what were the very competent ex-

Table 1: Radiocarbon dates on samples from the Hot Springs Site run between 1960 and 1986.

<sup>14</sup> C AGE B.P.	LAB ID	CALIBRATED LOWER	CALIBRATED HIGHER	UNIT
610±90	Tk 125	1220	1460	HH01
540±190	GaK 11041	1000	1950	HHU2 House G
1390±70	Tk 124	530	780	HL01
1410±100	GaK 12089	420	870	HL03 upper
1440±75	Gak 5414	430	770	HH02
2110±100	GaK 12088	-390	70	HL02 upper
2450±130	GaK 11038	-850	-200	HH03
2530±110	GaK 12090	-900	-390	HL03 lower
2580±130	GaK 11035	-1000	-350	HHU2 7c
2680±250	I 1507	-1500	-200	TRENCH 1 1960
2780±100	GaK 11042	-1260	-790	HHU2 House B
2790±180	Gak 11031	-1450	-400	HHU2 5-1
2840±120	GaK 12091	-1400	-800	HL02 lower
2930±90	N 3236	-1390	-900	Q-7 layer 5
2960±320	I 1508	-2100	-300	TRENCH II J-O
3000±90	N 3244	-1440	-970	U-7 layer 7b
3030±90	N 3239	-1460	-1000	Q-7 layer 4-12
3140±140	GaK 9848	-1750	-1000	HN02 Floor 1 lamp
3160±90	N 3240	-1700	-1100	Q7 layer 4-16
3160±90	N 3246	-1700	-1100	U7 layer 8c
3240±80	N 3242	-1740	-1310	T-7 3a
3270±100	N 3243	-1900	-1300	T-7 5a
3280±160	GaK 9849	-2050	-1100	HN02 Floor 1 shell
3380±130	GaK 9847	-2050	-1400	HN04
3430±95	Gak 5415	-1980	-1510	T-4 Lev 6
3450±150	GaK 11040	-2200	-1400	HHU2 House E
3520±95	Gak 5416	-2150	-1600	T-4 lev 7
3540±120	N 3241	-2200	-1500	Q-7 HOUSE B layer 7
3870±140	Gak 11032	-2900	-1900	HHU2 5-4
3890±120	N 3237	-2900	-1950	Q7 layer 4-3
3940±230	GaK 11033	-3100	-1700	HHU2 6b?
4020±130	GaK 9846	-2900	-2150	HN04
4020±180	GaK 11034	-3100	-1900	HHU2 7a
4200±130	GaK 11036	-3350	-2400	HHU2 8b
4210±160	GaK 11045	-3350	-2350	HHU2 HOUSE C 8a
4260±170	GaK 11044	-3400	-2300	HHU2 HOUSE B
4430±130	GaK 11039	-3500	-2850	HH03
4450±180	GaK 11043	-3700	-2600	HHU2
4710±130	N 3238	-3800	-3000	Q7 layer 4-6
4920±130	GaK 11037	-4000	-3350	HHU2 9b
4990±120	Gak 5417	-4050	-3500	J-4 Lev 7+M162
5560± 100	N 3245	-4700	-4050	U-7 Lev 7

cavations by the Okadas and their associates. That is, the set of determinations from 1982 in particular seem older and out of phase with determinations obtained in most of the other years, although for five of the six years all radiocarbon dates were from the same laboratory, Gakushuin laboratory. ...the two year's excavations and dates in Unit T are completely in phase with one another. Comparable to them are the two determinations received from unit U-7, excavated the same year as unit T-7. Those two earlier-obtained U-7 determinations, however, appear out of phase with the bulk of

the series of dates from the expansion of U-7 designated HHU2. As a whole the 1982 dates read just about a thousand years older than the determinations of 1977. Was there a systematic laboratory error in that year?" (Dumond 2002).

This important observation required a rethinking of the archaeological sequence and dating at Hot Springs, a problem the Okadas had been concerned with for many years. In the context of assisting the Okadas in the completion of their final report and in the packing of the Hot Springs materials for their ultimate return to Alaska, a

number of charcoal samples were found in the artifact collection. Further, a large number of caribou (*Rangifer tarandus*) antler artifacts and fragments were identified in the collection. These two sources were perfect for running a new series of dates using the AMS method.

In 2002, ten new samples were submitted to Beta Analytic, eight of which were on the same stratigraphic deposits originally dated by the Okadas. Of the dates in the same stratigraphic levels, one was very close to the original date while the other seven ranged from 610 to 1590 years younger than the original dates, an average distance of 1032 years, just as Dumond had argued.<sup>2</sup>

Table 2: Dates on the same stratigraphic levels showing the difference between the original Gakushuin dates and the newly run dates.

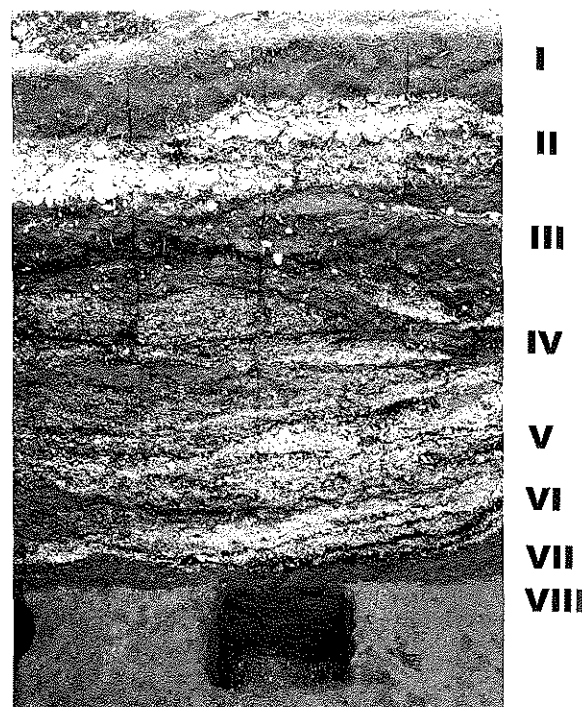
2002 Date	Original Date	Difference
1300±40	1410±100	110
3590±90	4200 ± 130	610
1230±40	2110±100	880
3230±40	4210 ± 160	980
1520±40	2530±110	1010
3410±80	4430±130	1020
2880±60	4020±130	1140
1790±60	3380±130	1590

Mean difference (w/o case 1) =1032 years<sup>3</sup>

What could have happened at the Gakushuin Laboratory? It is unclear and Maschner received no response to a letter sent to the laboratory. There may have been problems with pretreatment of samples, which is especially of concern with midden samples that might be contaminated by sea mammal oils and other sources of marine carbon. But this certainly does not explain all of the error since many of the differences are two to three times the reservoir effect estimated by Dumond and Griffin for the region at 460±41 (2002).

Another problem is contamination from coal and other sources, especially in the 1970-80s when bulk samples of charcoal were analyzed prior to the development of AMS dating. In the samples sent to Beta Analytic in 2002, one sample was determined by that lab to be clearly not charcoal, although it was collected as a charcoal sample during the original excavation. In 2003, another sample from that same unit U, level 4b, was submitted to Beta Analytic and I inspected the sample prior to sending it in and determined it was charcoal. A date of

Figure 3: Stratigraphic profile from Unit U, showing the sequence of the highland areas near the bluff edge. Note the 'upper shell layer' in Level 2.



I: dark brown soil directly below the root mat.

II: consolidated shell layer, the 'upper shell layer' in reports.

III: dark brown soil with cultural materials. Small gravel throughout the upper part possibly representing a disconformity between occupations. Red ochre in the middle of the level.

IV, V, VI: alternating layers of mussel, clam, fish bone, charcoal, and loamy soil. Abundant stone and bone artifacts.

VII: secondary deposits of loamy soil with stone artifacts and debris.

VIII: sterile soils.

>47360 BP shows quite clearly that it was not charcoal. This should come as no surprise since the site is very near a number of large and exposed coal deposits and the Okadas state that coal is found on the beach in front of the site, a situation noted by Weyer 80 years earlier (1930:276).

In 2003 three samples were submitted with the goal of dating the upper deposits to demonstrate that the Okadas were correct in the definition of the 'upper shell layer.' Samples were submitted from the upper deposits of units U and Q, which had a clearly defined upper shell layer, and also from the upper level of HH03, which did not have this deposit. Both samples from U and Q came back

<sup>2</sup>A level by level comparison of the dates for each excavation unit is presented in the Appendix.

<sup>3</sup>The new 2003 dates fall out much the same way, with the new date on the lower part of HL03 1,010 years younger. The only one that is the opposite is a new date on HH03, which is 780 years older than the date received by Gakushuin by the Okadas. It is possible that I still haven't dated the upper part of HH03, but the charcoal sample sent in was labeled from the upper deposits, of course, the coal problem must always be considered as well.

in the late first millennium AD, just as predicted by the Okadas. The sample from HH03 without an upper shell layer is from the first millennium BC, demonstrating that the Okadas were quite correct in that the eastern highland area was the early occupation, but it is overlain by a thick deposit of midden from the later occupations.

Samples from HH02, just to the east of the springs but west of the thick midden deposits of the highland, were also redated in both 2002 and 2003 and found to be in the later occupation. HL02 and HL03 to the west of the springs in the lowland area were redated and found to be in the later occupations of the site as well.

Thus, regardless of cause of the initial date errors, the effect is the same in that 23 of the original dates, all run by the Gakushuin laboratory 1980, 1982 and 1984, must be discarded as in serious error. Further, two earlier dates in Unit Q, and one in Unit U are out of sequence being 1000-2500 years too old for their stratigraphic context and in comparison to other dates in the unit. Since these three dates are in the same stratigraphic context as the >47360 BP date from unit U, I assume that these two were also contaminated in some way, perhaps as a mixture of charcoal and coal. The date of 4990±120 in unit J is considered to date deposits below the cultural layers by Atsuko Okada and thus does not date the occupation of the site. The two dates received by the original Meiji University-University of Wisconsin Project are probably fine, but their calibrated ranges span between 1300 and 1700 years, making them unsuitable for further analysis. Therefore, 29 of the original dates are considered in error, not usable, or not cultural, leaving 15 of the original dates for chronological analysis. In summary:

- GaK 9846-9851 (1980), GaK 11031-11045 (1982), GaK 12088-12091 (1984) – **All in error.**
- GaK 5414-5417: **Good** but 4990±120; (Gak-5417) **Not cultural** (A. Okada, personal communication).
- N 3236 - 3246 probably good but N-3237, N-3238, N-3245 **Out of sequence.**
- Tk 124-125: **Probably good.**
- "I" dates from 1960: **Probably good but 1300-1700 year calibrated range makes them unusable.**

In total, 19 new samples were submitted to Beta Analytic for standard radiocarbon analysis, AMS dating on charcoal, or AMS on caribou antler. Of these, one was returned as not charcoal and another returned a date of >47360 years, which means it was not charcoal either. The remaining 17 samples, when combined with the 15 usable samples from the original project, allow us to revise the Hot Springs chronology as follows (all calibrated dates).

In their initial formulation of the chronology, Okada and Okada (1989) used the designations Port Moller I, II, and III. Now that the chronology is substantially altered because of the new dates, and because there have recently been projects in the Port Moller – Nelson Lagoon area that found sites dating to the periods when Hot Springs is not occupied, it seems more efficient to use the term Hot Springs in the chronology to avoid confusion with the broader regional prehistory (Figure 4). A complete list of all of the dates that are now considered to be usable in constructing a chronology of the Hot springs site are shown in Table 3.

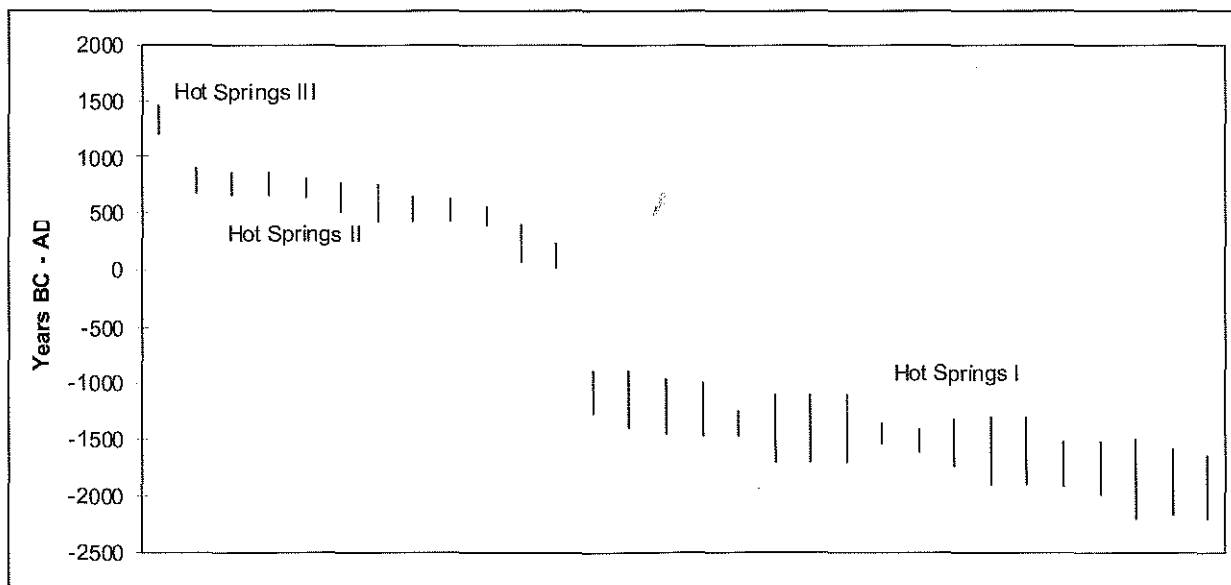


Figure 4: Calibrated ranges of all usable original dates and all new dates.

## SPATIAL AND CHRONOLOGICAL CONTEXTS

This sequence can be investigated spatially across the site as well. Hot Springs 1 is only found along the bluff edge on the eastern margin of the site. This is the area of greatest stratigraphic depth and represents an intensive occupation. Why the site was abandoned around 1000 BC is open to investigation and the subject of a future paper, but possibilities include climate change as described by the Okadas (1989), the eruption of Mt. Dana just to the south reportedly between 1000 and 100 BC, changing sea levels, or any number of complex factors. Regardless, when the site is again reoccupied around AD 100, the residents first occupy the northern shore in a few houses and the southwestern shore where they created a stratified midden deposit. The primary occupation of the site does not appear to have occurred until after AD 500, when the majority of the surface houses were constructed. This occupation terminated around AD 850. There is a single date with a range of approximately AD 1300 to 1400. Based on house construction and other features, I believe this to be accurate, and was probably representative of just a few houses (I will return to this below).

Regionally, the new dates and chronology from the Hot Springs site reconciles many of the problems that have plagued archaeologists in the region when attempting to construct a regional sequence. The deposits that date prior to 1600 BC are contemporaneous with the later part of the Moffet Phase<sup>4</sup> on the lower Alaska Peninsula. This is a poorly defined phase of rather generic looking artifacts. But the dating of the occupation at Hot Springs is accurate because, except for an early date in Unit U that is slightly out of sequence (probably is so because of house construction disturbance), the other three early dates are at the bottom of units T, Q, and HH03 are in perfect context. A series of calibrated dates between 1600 and 1300 BC are on deposits that have many of the same features and artifacts as the Russell Creek phase further down the peninsula and are certainly related. These artifacts and features are not found earlier than 1600 BC, nor later than 1300 BC in either area (Maschner and Jordan 2002). Following the Russell Creek Phase on the lower peninsula is the Kinzarof Phase dating between 1300 and 400 BC. The dates from later part of Early Hot Springs coincide with the early Kinzarof Phase. Again, this phase is characterized by rather ge-

neric artifacts but important similarities between the two areas are evident in artifact form and distribution. Thus, based on stone end blades, hearths, and the bone technology, the Hot Springs 1 could probably be divided into three occupations along the lines summarized in Table 4.

The major abandonment of Hot Springs is contemporaneous with the occupation of the Adamagan site on the lower Peninsula in Morzhovoi Bay, which represents the later part of the Kinzarof Phase and the entire Adamagan Phase. Hot Springs 2 begins around AD 100, coeval with the Ram's Creek Phase further southwest. This part of the occupation, which might be called Hot Springs 2a, is of limited spatial distribution at the site. Hot Springs 2b, the largest occupation of the site, is equal to the Frosty Creek Phase on the lower Peninsula. The artifacts found throughout Hot Springs 2 are quite similar to those found elsewhere in the region in the same time period.

Between AD 1100 and 1250, and then again after AD 1475, the use of the large, nucleus-satellite houses on the Alaska Peninsula is common (Hoffman 2002; Maschner 1999a; Maschner 2004; Maschner and Hoffman 2003 [on the lower Peninsula the Cape Glazenap and Morzhovoi Phases respectively]). This house form is large, 8-20 meters in length, has internal storage facilities, side rooms, and is found in groups from 5-7 houses early, and 10-30 houses after AD 1475. These are found throughout the lower Peninsula area, Sanak Island, the Shumagin Islands, on the rivers of Nelson Lagoon just to the west of Hot Springs, and at Bear Lake just to the northeast of Hot Springs. The fact that the Hot Springs site is completely surrounded by sites with this house form, but that none of these houses are found at Hot Springs, indicates that the site was indeed abandoned during these periods.

But between these two phases on the western peninsula is the Izembek Phase. Dating between AD 1250 and 1475, this period saw a regional population decline, a return to small villages of 1-4 houses, a return to smaller houses of 6-8 meters in diameter, and the use of external storage pits. The single late date representing Hot Springs 3 falls into this time range. The excavated house, HH01, looks very much like houses I have tested during this phase with occasional internal storage pits, no side rooms, and a circular outline about 8m in diameter. While the type artifact of the later phases is the Izembek Point, which was

<sup>4</sup>Maschner and Jordan have recently reworked the chronological sequence, including the names of several of the phases, to make them more consistent with McCartney's early researches (1974), and new data from the region. The complete sequence is described in Maschner (2004) and will be the subject of a forthcoming report by Maschner and Jordan

Table 3: All new dates (Beta Analytic series) and old dates that are considered reliable.

<u><sup>14</sup>C AGE B.P.</u>	<u>LAB ID</u>	<u>CALIBRATED LOWER</u>	<u>CALIBRATED HIGHER</u>	<u>UNIT</u>
610±90	Tk-125	1460	1220	HH01
1230±40	BETA 167536	900	680	HL02 upper
1270±40	BETA 184116	670	870	HHQ 2-1
1270±40	BETA 184114	670	870	HH02 upper
1300±40	BETA 167528	810	650	HL03 upper
1390±70	Tk-124	780	530	HL01
1440±75	Gak-5414	770	430	HH02
1500±50	BETA 184117	430	650	HHU2-2a
1520±40	BETA 167532	640	430	HL03 lower
1590±40	BETA 184113	400	560	HL01 upper
1790±60	BETA 167529	400	80	HN04 upper
1890±40	BETA 167540	240	20	HLW4
2880±60	BETA 167533	-1260	-890	HN04 3-4
2930±90	N-3236	-1390	-900	Q-7 layer 2-5
3000±90	N-3244	-1440	-970	U-7 layer 7b
3030±90	N-3239	-1460	-1000	Q-7 layer 4-12
3100±40	BETA 167534	-1450	-1250	HHQ7 4-5
3150±90	BETA 167537	-1700	-1100	U7 8C
3160±90	N-3240	-1700	-1100	Q7 layer 4-16
3160±90	N-3246	-1700	-1100	U7 layer 8c
3200±60	BETA 184115	-1540	-1360	HH03 above floor
3230±40	BETA 167531	-1610	-1410	HHU2 House C
3240±80	N-3242	-1740	-1310	T-7 3a
3270±100	N-3243	-1900	-1300	T-7 5a
3320±110	BETA 167535	-1900	-1300	HHQ7 6-1
3410±80	BETA 167530	-1920	-1510	HH03 unit D Floor
3430±95	Gak-5415	-1980	-1510	T-4 Lev 6
3540±120	N-3241	-2200	-1500	Q-7 HOUSE B layer 7
3520±95	Gak-5416	-2150	-1600	T-4 lev 7
3590±90	BETA 167538	-2200	-1650	U7 8B

Table 4: A revised chronology of the Hot Springs site based on radiocarbon determinations, stratigraphy, artifacts, houses floors, and comparisons with the greater regional prehistory.

- **Hot Springs 1: 2000 - 1000 BC.**
  - Hot Springs 1a: 200-1600 BC
  - Hot Springs 1b: 1600-1300 BC
  - Hot Springs 1c: 1300-1000 BC
- **Hot Springs 2: AD 100 - 800.**
  - Hot Springs 2a: AD 100-300
  - Hot Springs 2b: AD 500-850
- **Hot Springs 3: AD 1300-1400.**

not found in the Hot Springs excavations, I argue that a limited reuse of the Hot Springs Village occurred during the years of the Izembek Phase on the lower Alaska Peninsula.

Based on comparisons with the rest of the western Alaska Peninsula, and a detailed investigation of the artifact assemblage, a new Hot Springs chronological framework is constructed (Table 4).

## CONCLUSIONS

It is unclear as to why so many of the radiocarbon dates from the Gakushuin Lab were so far in error.<sup>5</sup> While this may never be resolved, the advent of AMS dating has allowed us to reconcile these problems and make corrections in the chronology. As a warning to many of us working on the region, the use of natural coal by the inhabitants must now be considered an important problem and one that could affect radiocarbon dates in other areas as well.

In an unpublished manuscript on the Aleutian region, William and Karen Workman (n.d.) argued that the Hot Springs Village site is one of the most important sites in the western arctic. With the new dating of this site and a reworking of the chronological sequence, this statement is even more accurate than it was when first conceived. In a series of forthcoming papers and a monograph on the site, I will show that the temporal distributions of many stone tools, harpoons, anthropomorphic figurines, zoomorphic representations, and household features have broad implications for the prehistory of the entire Bering Sea region.

## ACKNOWLEDGEMENTS

The author would like to thank Don Dumond and William Workman for helpful comments in reconciling the radiocarbon sequence at Hot Springs. Two anonymous reviewers helped make the text clear and grammatically consistent. ISU graduate students Garrett Knudsen and Andrew Williamson assisted in organizing the stratigraphic data and the artifact collections. Williamson was also instrumental in teasing out the intricacies of the Hot Springs stratigraphy. I would especially like to thank Hiroaki and Atsuko Okada for allowing me the pleasure of assisting in the completion of the Hot Springs project.

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<sup>5</sup>It should also be noted that the two earliest dates in the Yukon Delta region (Okada et al. 1982:9) were also run by the Gakushuin lab and may be too old as well.

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## APPENDIX

Now I should discuss each excavation in some detail. Unit T is the simplest as none of the dates in question come from this excavation. It has fairly straightforward stratigraphy and the dates fall in sequence (Table 5).

Table 5: Date Sequence in Unit T.

	T-4 (1974)	T-7 (1977)	FINAL UNIT T
Level 2			
Level 3		3240 ± 80	3240 ± 80
Level 4			
Level 5		3270 ± 100	3270 ± 100
Level 6	3430 ± 95		3430 ± 95
Level 7	3520 ± 95		3520 ± 95

Unit Q is more complicated because two of the dates must be discarded as in error, probably contaminated by coal. Those marked by an \* are considered too old and out of sequence (Table 6).

Table 6: Date Sequence in Unit Q.

	Q7 and HHQ (N dates 1977)	Q7 and HHQ (Beta 2002-2003)	FINAL UNIT Q
Q-7 2-1		1270 ± 40	1270 ± 40
Q-7 2-5	2930±90		2930±90
Q7 layer 4-3	3890±120*		
Q7 4-5		3100±40	3100±40
Q7 layer 4-6	4710±130*		
Q-7 4-12	3030±90		3030±90
Q7 4-16	3160±90		3160±90
Q7 6-1		3320±110	3320±110
Q-7 HOUSE B 7	3540±120		3540±120

Unit U, one of the most important in the Okadas' excavations, has many Gakushuin dates. Those marked by an \* are considered too old and out of sequence. Here, a single date from the 1977 run is discarded as being too old and perhaps contaminated. The entire run of 1982 dates must be discarded in bulk. These are the dates that formed the basis of Dumond's quote presented above. The single 2003 date of >47360 is also discarded for the reasons described above (Table 7).

Table 7: Date Sequence in Unit U.

	U-7 (1977)	HHU2 (GAK 1982)	U-7 and HHU2 (2002-2003)	FINAL UNIT U
Level 2-a			1500 ± 50	1500 ± 50
Level 4-b			>47360*	
Level 5-1		2790 ± 180*		
HOUSE E		3450±150*		
Level 5-4		3870 ± 140*		
Level 6-3		3940 ± 230*		
Level 7a	5560±100*	4020 ± 180*		
House G		540 ± 190*		
Level 7b	3000 ± 90			3000 ± 90
Level 7c		2580 ± 130*		
Burials 2&3		4450 ± 180*		
House B		4260 ± 170,* 2780 ± 100*		
Level 8b		4200 ± 130*	3590±90	3590±90
Level 8c	3160 ± 90		3150±90	3150±90
				3160 ± 90
House C		4210 ± 160*	3230±40	3230±40
Level 9b		4920 ± 130*		

The highland excavations reveal three occupations. HH01 is the only dated structure in the 2nd millennium AD, and, based on similarities with the Izembek Phase (AD 1250-1475 in Maschner 2004), this date is probably correct. HH02 has two dates firmly in the 1st millennium AD, and since there was very little stratigraphy or deposition here, it is unlikely that there are earlier deposits at this location (or they were destroyed by the later construction). HH03 is more problematic. The 'under house' date came out 1000 years too old, but the 'on floor' date turns out to be 800 years too young, even though I expected it to be 1000 years too old, but firmly in the 1st millennium AD. Is it possible that both of the dates from Beta Analytic in 2002-2003 are from an older occupation and there is still a poorly dated later occupation at this same house? Of course. But based on the stratigraphic profiles and artifacts, this does not appear to be the case (Table 8). Those marked by an \* are considered too old or out of sequence.

Table 8: Date Sequence in Units HH01, HH02, and HH03.

	<b>HH01 (Tk Date)</b> 610±90	<b>HH01 (Beta 2002-2003)</b> None	<b>HH01 Final</b> 610±90
	<b>HH02 (Gak 5414)</b> 1440±75	<b>HH02 (Beta 2002-2003)</b> 1270 ± 40	<b>HH02 Final</b> 1270 ± 40 1440±75
on floor	<b>HH03 (GAK 80)</b> 2450±130*	<b>HH03 (Beta 2002-2003)</b> 3180 ± 60	<b>HH03 Final</b> 3180 ± 60
under house	4430±130*	3410±80	3410±80

The lowland house excavations are much simpler and less complicated stratigraphically, but all of the Gakushuin dates from 1984 had to be discarded (Table 9). Those marked by an \* are considered too old and out of sequence.

Table 9: Date Sequence in Units HL01, HL02, and HL03.

	<b>HL01 (Tk date)</b> 1390±70	<b>HL01 (Beta 2002-2003)</b> 1590 ± 40	<b>HL01 Final</b> 1590 ± 40, 1390±70
upper	<b>HL02 (GAK 84)</b> 2110±100*	<b>HL02 (Beta 2002-2003)</b> 1230±40	<b>HL02 Final</b> 1230±40
lower	2840±120*	None	
upper	<b>HL03 (GAK 84)</b> 1410±100*	<b>HL03 (Beta 2002-2003)</b> 1300±40	<b>HL03 Final</b> 1300±40
lower	2530±110*	1520±40	1520±40

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Five other excavation units bear mention in this discussion. The 2002 date on unit HLW4 is the only date from this excavation and dates at least a part of the occupation from the southwest shore of the site. HN04 was redated to over 1200 years younger than the dates from Gakushuin 1982. Since HN04 and HN02 have basically the same stratigraphy, they can probably be considered to have similar dates. Trenches I and II from the 1960 excavations are in the bluff edge area of site and probably had a similar stratigraphy to excavation units T, Q, and U. These dates fall at the end of the expected time frame, but their large standard deviations place them anywhere in a 1300-1700 year range. They should probably be ignored in favor of the better dates from the same area. According to the Okadas, there were some problems with the Unit J excavation, and the single date from that unit was earlier than the occupation. No further dates were run on unit J (Table 10). Those marked by an \* are considered too old, out of sequence, or have extremely large errors.

TABLE 10: Date Sequence in Units HLW4, HN, and the 1960 excavations.

	<b>HLW4</b>	<b>HLW4 (Beta 2002)</b>
	None	1890±40
	<b>HN02 (GAK 82)</b>	<b>HN02 (Beta 2002)</b>
	3140±140*	None
	3280±160*	None
	<b>HN04 (GAK 82)+C124</b>	<b>HN04 (Beta 2002)</b>
Upper	3380±130*	1790±60
Lower	4020±130*	2880±60
	<b>TRENCH I and II 1960</b>	<b>Beta 2002-2003</b>
TRENCH I 1960	2680±250*	None
TRENCH II J-O	2960±320*	None
	<b>J-4</b>	<b>Beta 2002-2003</b>
J-4 Lev 7+M162	4990±120*	None

# RADIOCARBON DATES FROM THE EARLY HOLOCENE COMPONENT OF A STRATIFIED SITE (SEL-009) AT AURORA LAGOON, KENAI PENINSULA, ALASKA

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**Abstract:** Tests at Aurora Lagoon on the Kenai Peninsula in 2000 and 2001 produced six new radiocarbon dates, five of which support the presence of human activity within the Cook Inlet basin during the Early Holocene. Calibrated dates ranging from 6220-5470 BC almost double the known span of human occupation in lower Cook Inlet. Stratification of the lower component of the Faulkner Site suggests a minimum of two occupations reflecting seasonal use of a maritime setting by a currently unidentified group of hunters. A less than diagnostic lithic assemblage obscures cultural identification and the absence of organic preservation precludes a definitive statement on subsistence.

**Keywords:** Southcentral Alaska, Kachemak Bay, early peopling

## INTRODUCTION

Cutting deeply into the southern end of the Kenai Peninsula, 64 km long, 38 km wide Kachemak Bay is the first major embayment on the eastern shore of Cook Inlet accessible to maritime travelers. Glaciers from the local Kenai Mountains and the distant Alaska and Aleutian Ranges to the north and west shaped the diverse landforms of Kachemak Bay (Reger and Pinney 1996:21). Deglaciation began prior to 15,000  $^{14}\text{C}$  yr BP (Reger and Pinney 1996:27). Glacial ice had retreated more than 10 km inland from the present shoreline of Kachemak Bay by 10,240  $^{14}\text{C}$  yr BP (Wiles and Calkin 1994:282). The pioneer herbaceous tundra was replaced by shrub willow, dwarf birch, and some deciduous species (*Populus*) by 9,500 years ago. The Early Holocene occupants existed on a landscape which lacked the extensive evergreen forests so characteristic of the contemporary environment, for it wasn't until about 3,500 to 4,000 years ago that white spruce invaded Kachemak Bay from the north and 1,650 years ago that Sitka spruce invaded the south shore from Prince William Sound (Ager 2000:147-149). Although no archaeological evidence supports this theory, a likely scenario would suggest that Early Holocene peoples were harvesting resources from a developing or fully developed marine food web such as Kachemak Bay supports today.

## PRESENT ENVIRONMENT

A variety of ecosystems characterizes the south and north shores of Kachemak Bay (Workman 1998:147-149).

The Kenai Mountains, which define the south shore, contain barren glaciated mountain peaks, alpine tundra, and forested mountains fronted by a convoluted coast line. The majority of archaeological sites in Kachemak Bay exist along the coastal fringe of the south shore. In contrast, the north shore is characterized by a relatively straight shoreline fronted by extensive mud flats and backed by bench lands and rolling hills.

Two low tides and two high tides occur every 24 hours and 50 minutes. At their extreme, tides range from about 2 m below sea level to 7 m above sea level, exposing almost nine vertical meters of intertidal habitat (Pioneer Publishing 2004:96-107).

## THE FAULKNER SITE (SEL-009)

Aurora Lagoon is situated deep within Kachemak Bay along the mountainous south shore (Figure 1). The shallow lagoon is backed by spruce and birch-covered slopes and fronted by low-lying Aurora Spit. The sand spit, which trends north to south, is about 1.2 km long and .2 km wide. The northern half is owned by the State of Alaska, the southern half, including the site, by the Faulkner families of Homer and Anchorage. Identified as SEL-009 on the Alaska Heritage Resource Survey, the site is located on the northernmost forested rock knob in a cluster of five knobs of varying sizes (Zollars and Klein 2002). Prior to the deposition of quaternary sediments, the knobs were islands, the lagoon non-existent (de Laguna 1975:24).

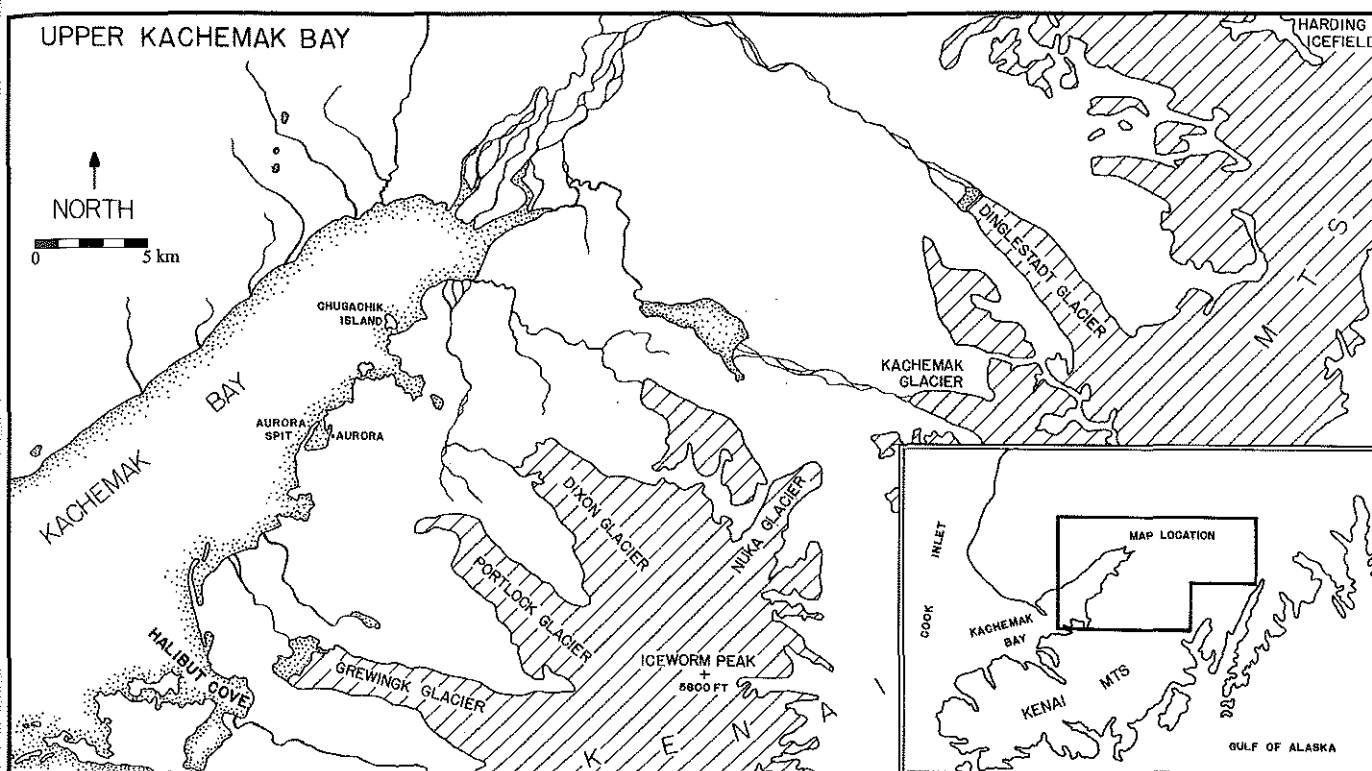


Figure 1. Map locating Aurora Spit in upper Kachemak Bay and the bay on the lower Kenai Peninsula.

The saltwater of Kachemak Bay enters Aurora Lagoon through a narrow, rock-choked channel on the north. The lagoon opening may change through time. William Healy Dall, "dean" of Alaskan scientists, illustrated Aurora Lagoon as opening on the south when he mapped Kachemak Bay in the late 19<sup>th</sup> Century (Dall 1895: Plate L1). The late Sewell Faulkner, who purchased property on Aurora Spit in 1978, told the authors that previously, a channel had opened at the south end of the lagoon. He also stated that the lagoon is filling slowly; more sediments are being deposited by the few freshwater creeklets than are being flushed out. Faulkner suggested that this began after the land subsided following the Good Friday Earthquake of 1964.

## PREVIOUS INVESTIGATIONS

Aurora Spit has been visited by numerous archaeologists. Frederica de Laguna, who conducted the pioneer field work throughout Kachemak Bay in the early 1930s, tested a 1.8–2.1 meter deep midden on the northernmost rock knob in 1931 (de Laguna 1975:24). De Laguna defined the artifact assemblage and fauna as the Kachemak Culture, later re-named the Kachemak tradition. At present, that Kachemak tradition site is recognized as the upper component of the stratified Faulkner Site. De Laguna did not locate the Early Holocene component; possibly, it was vegetated in the 1930s.

The coastline of Kachemak Bay subsided 0.5 to 2 m following the 1964 earthquake (Waller 1968:D1). To assess damage to archaeological sites, the Homer Society of Natural History organized a survey in 1973. Found eroded onto the beach at SEL-009 was a flake core with a prepared platform (Reger 1974:15). A decade later, Neil Campbell, James Stone, and Peter Zollars surveyed upper Kachemak Bay for the State of Alaska. Like de Laguna, they saw no indication of the Early Holocene component at SEL-009 (Zollars 1983:19).

In 1989, Janet Klein assessed a mainland site within Aurora Lagoon. During the next three years, over 200 artifacts were collected from the beach, identified as late Ocean Bay, and housed at the Pratt Museum (Klein 1996:53–54). In 1992, Professor William Workman, University of Alaska Anchorage, with a small field crew excavated the stratified Sylva Site (SEL-245). The assemblage and an uncalibrated radiocarbon date of  $4,440 \pm 90$  years BP confirmed a late Ocean Bay occupation which reflected an incursion of people probably from the Alaska Peninsula (Workman et al. 1993:12). An older undated occupation, which lacked diagnostic artifacts and carbon, was identified beneath the Ocean Bay occupation (1993:4). How it relates, if at all, to the Early Holocene component at SEL-009 is unknown.

## Field Methodology and Results

During site surveys of Aurora Spit in 1996 and 1998, we noted carbon flakes *in situ* below the Kachemak tradition component at SEL-009. Although not impacted by even the highest tides, the site was actively eroding due to wind, rain, and gravity. With permission from the land-owners, we tested the exposed face August 4-7, 2000 and June 10-23, 2001 to salvage information and artifacts, particularly from the lower unidentified component. Testing occurred on the north-facing side near the western end of the rock knob. We trimmed the overhanging vegetation, established a temporary datum, then excavated the shell midden. A sample of charred material provided a measured radiocarbon age of  $1770 \pm 70$  BP or cal AD 110-430, solidly anchoring the midden within the late

Kachemak tradition (Beta-152921). Numerous post holes extended into the Early Holocene component. Below the midden, we cleaned and straightened the vertical wall to bedrock at 120 cm below the surface. A metric tape stretched 160 cm parallel to the erosion face and across bedrock, provided the control necessary for stratigraphically plotting artifacts and samples. Excavation proceeded southward (forward) until the Early Holocene component ended. Approximately 8-10 m<sup>3</sup> of sediments were excavated during the two seasons. At the close of the excavation, back dirt was shoveled onto the bedrock shelf.

### One Feature, Two Occupations

One feature and two occupations were identified in the Early Holocene component. The feature, a 30 cm wide by 20 cm deep hearth, was excavated at about 90-110 cm below the surface. Considerable spoil ash was displaced in several thin well-defined layers, indicating repeated use. All <sup>14</sup>C dates were derived from unspiciated charred wood collected from the hearth and associated spoil ash. Although the soil matrix remained relatively consistent throughout this component, the texture, color, artifact distribution and carbon content changed with depth, suggesting a younger and older occupation (Figure 2).

### SAMPLES, DATES, AND LITHICS

Six carbon samples were collected, one from the Kachemak tradition component (discussed in text) and five from the older occupation of the Early Holocene component. Dates reveal that the Early Holocene people either occupied the site for hundreds of years or returned, repeatedly, over the centuries (Table 1). As of this writing, SEL-009 is the oldest dated site in mainland Southcentral Alaska.

Other samples collected included one from the culturally stained, oily soil representing the bottom of the Kachemak tradition, two soil samples from the Early Holocene component, glacial till from the colluvium, and a sample of peat from a lens found about 100 cm below the surface. Sandwiched between the older occupation and the colluvium, the peat was exposed only on the eastern end of the excavation. The possibility exists that it and the Early Holocene

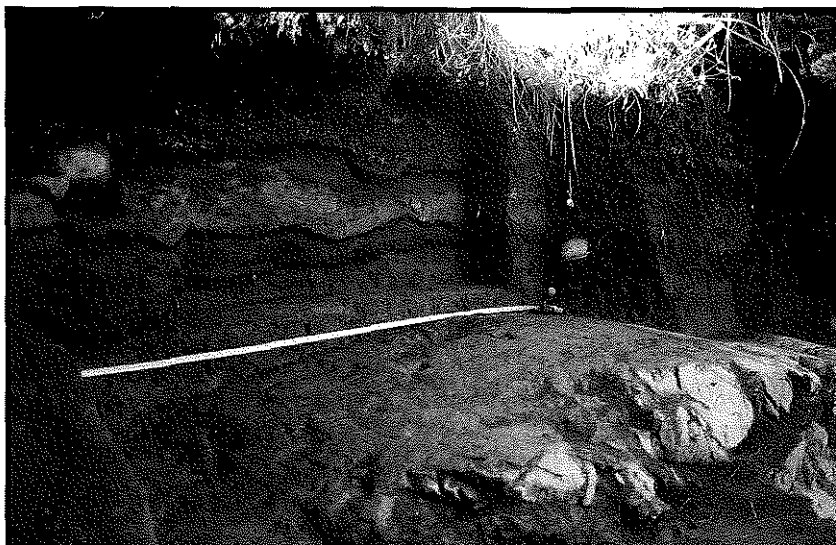


Figure 2. Stratigraphic Profile: A 1.38 m wide section showing the Kachemak tradition and Early Holocene components. August 2000. © Janet R. Klein.

Vegetation.

Kachemak Tradition Component: 60-70 cm thick; shell midden; post hole with two rocks, left, and hearth, right, cut into the Early Holocene component; Munsell Color Chart 10 YR-3/4, deep brown.

Early Holocene Component: younger occupation – 15-25 cm thick; silty soil; carbon smears; no fauna; Munsell Color Chart 10 YR-8/2, off-white.

Older occupation – 10-15 cm thick; silty soil; carbon ranging from flecks to 1 cm wide chunks, largest in hearth; unspiciated calcined bone fragment in hearth; Munsell Color Chart 2.5 YR/4, dark reddish brown.

Colluvium – 12-15 cm thick; sterile, gritty, granular, glacially deposited till; Munsell Color Chart 10 YR-4/6, dark yellowish brown.

Table 1. Radiocarbon dates from the older occupation of the Early Holocene component of the Faulkner Site.

Laboratory Number	<sup>14</sup> C yrs BP	<sup>13</sup> / <sub>12</sub> C Ratio	Sigma	Calibrated Years
Beta-152922	7160 ± 100	-25.9 ‰	2	6220 – 5810 BC
Beta-158403	7070 ± 90	-25.3 ‰	2	6080 – 5740 BC
Beta-152923	6840 ± 90	-25.6 ‰	2	5880 – 5610 BC
Beta-152924	6810 ± 70	-26.2 ‰	2	5790 – 5610 BC
Beta-158402	6690 ± 90	-26.0 ‰	2	5720 – 5470 BC

component continue eastward. If so, scattered spruce, alder, and other dense vegetation would need to be removed to access them.

One hundred and seventy-four lithic artifacts were excavated from the Early Holocene Component; 104 from the younger occupation, 70 from the older. Of those, 160 were flakes with seven exhibiting signs of retouch (Figure 3).

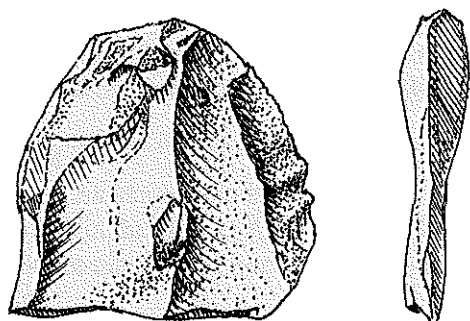


Figure 3. Flake, tuffaceous material, younger occupation, Early Holocene component. Actual size.

The minimal tool assemblage included a biface, three biface fragments, two cores of which one had a prepared platform, and three core flakes (Figures 4, 5, 6). Almost 54% of the lithics were chert, 35% indurated tuff, tuffaceous sedimentary rock or a variant thereof, and 11% recognizable local or foreign material. Tuffs, tuffaceous sedimentary rock, and variants are found in the Peninsula Terrane comprising the tip of the Kenai Peninsula from Seldovia Bay southwestward to Koyuktoik Bay (Bradley et al. 1999). Patrick Saltonstall, archaeologist at

the Alutiiq Museum and Archaeological Repository, Kodiak, states that tuff is known from Kodiak Island sites although the material remains unsourced. Whether lithics associated with the Early Holocene component at Aurora Spit are local or foreign is undetermined.



Figure 4. Core, tuffaceous material, younger occupation, Early Holocene component. Actual size.

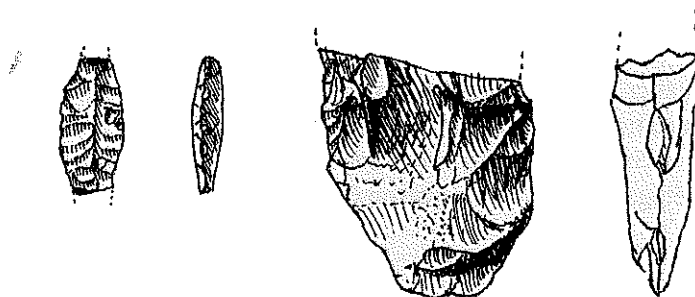


Figure 5. (above left). Bipoint, medial fragment, chert, younger occupation, Early Holocene component. Actual size. Figure 6. (above right). Biface, broken, chert, older occupation, hearth associated. Actual size.

## CONCLUSIONS

Radiocarbon dates from the Early Holocene component reveal that people utilized the site repeatedly between 6220-5470 cal BC. Although adept at flaking bifaces and cores from a variety of lithic materials, the lack of a diagnostic assemblage obscures cultural identification. It also prevents the authors from suggesting whether the people preferred maritime or terrestrial resources.

After receiving the radiocarbon dates firmly establishing a human presence in Kachemak Bay over eight millennia ago, the authors began reassessing sites and artifact collections in Halibut Cove, about seven miles south of Aurora Lagoon. Two collections contain lithic materials similar to those from the Faulkner Site, strongly suggesting the presence of an Early Holocene occupation there. Thus, the search for early sites in Kachemak Bay continues.

## ACKNOWLEDGEMENTS

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# A PUNUK WHALE BONE GRAVE FROM SIVUQAQ, ST. LAWRENCE ISLAND: EVIDENCE OF HIGH SOCIAL STANDING, AD 775-1020.

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**Abstract:** Cultural resource monitoring from 1991 to 1994 revealed 114 graves within the Gambell beach ridge plain; only one grave contained a sizable inventory of associated objects, classifiable within the Punuk phase. A single  $^{14}\text{C}$  age on the driftwood grave cover placed the interment between AD 775 and 1020. The associated grave goods are anomalously elaborate for the Sivuqaq region and somewhat resemble the high status graves reported from Cape Dezhneva, Chukotka.

**Keywords:** Bering Strait archaeology, Mortuary Practices, Cultural Change

## PROJECT HISTORY

Between 16 July and 15 August 1991, David P. Staley—then of the University of Alaska Anchorage's Arctic Environmental Information and Data Center (AEIDC, re-named as Environmental and Natural Resources Institute (ENRI))—conducted archaeological monitoring and data recovery associated with water and sewer improvements undertaken by the City of Gambell, Alaska. In 1991, twelve burials were disturbed by construction activities; six were directly observed and documented during monitoring (Staley 1991). In 1993 and 1994, Staley (1993a, 1994a) documented 102 additional burials during the course of monitoring.

## THE ARCHAEOLOGICAL CONTEXT

On 23 July 1991 blading operations uncovered a large fragment of whalebone in the vicinity of a proposed sewer line between a manhole and House B46 in Gambell, Alaska (Figure 1). A lull in ground-disturbing activities on the next day allowed an opportunity for careful excavation of the area. Grave #7 (for the year 1991; hence, Grave 91-7) was found southwest of the whalebone fragment at a depth of 60 cm.

Excavation revealed a bone paste and crumb outline of an individual, a portion of the skull, two complete molars, fragments of left and right femurs, fragments of the right ulna and radius and two bones from the right hand. The body was extended, face-up, oriented north,

with arms to the sides (Figure 2). Extreme wear on the molars (worn flat nearly to the gum line) indicates that the individual was probably an older adult. This person was between 165 and 180 cm tall. Although sex could not be determined from the skeletal remains, the associated artifacts suggest a male. The individual was interred with a variety of grave goods placed around the body (Figures 2, 3); including 89 items of finished stone and ivory artifacts and unfinished materials. Artifacts are listed and briefly described in Table 1. The vast majority of this assemblage consists of chipped stone debitage and raw slate with much of the remainder including weapon blanks, broken weaponry, and utilitarian tools. The most significant artifacts include five fragmentary harpoon heads, classifiable as III-a-x and V-a x types (Figure 3), with decorative motifs associated with the Punuk phase (cf. Collins 1937). Many of the grave goods were found in the vicinity of the individual's right hand (Figure 2). Although no artifacts were noted near the left hand, it appears that an ash pile was placed there.

A thin and slightly concave layer of decayed wood was found just above the skeletal remains. This layer covered a rectangular area 2.5 by 0.75 m and represents the remains of two driftwood logs used as the grave cover. Like the body, these logs were also oriented North/South. The remainder of the whalebone initially observed on the surface paralleled the decayed logs on the east. The proximal end of this whale rib rested at approximately 60 cm below the ground surface and tilted upward to the north.

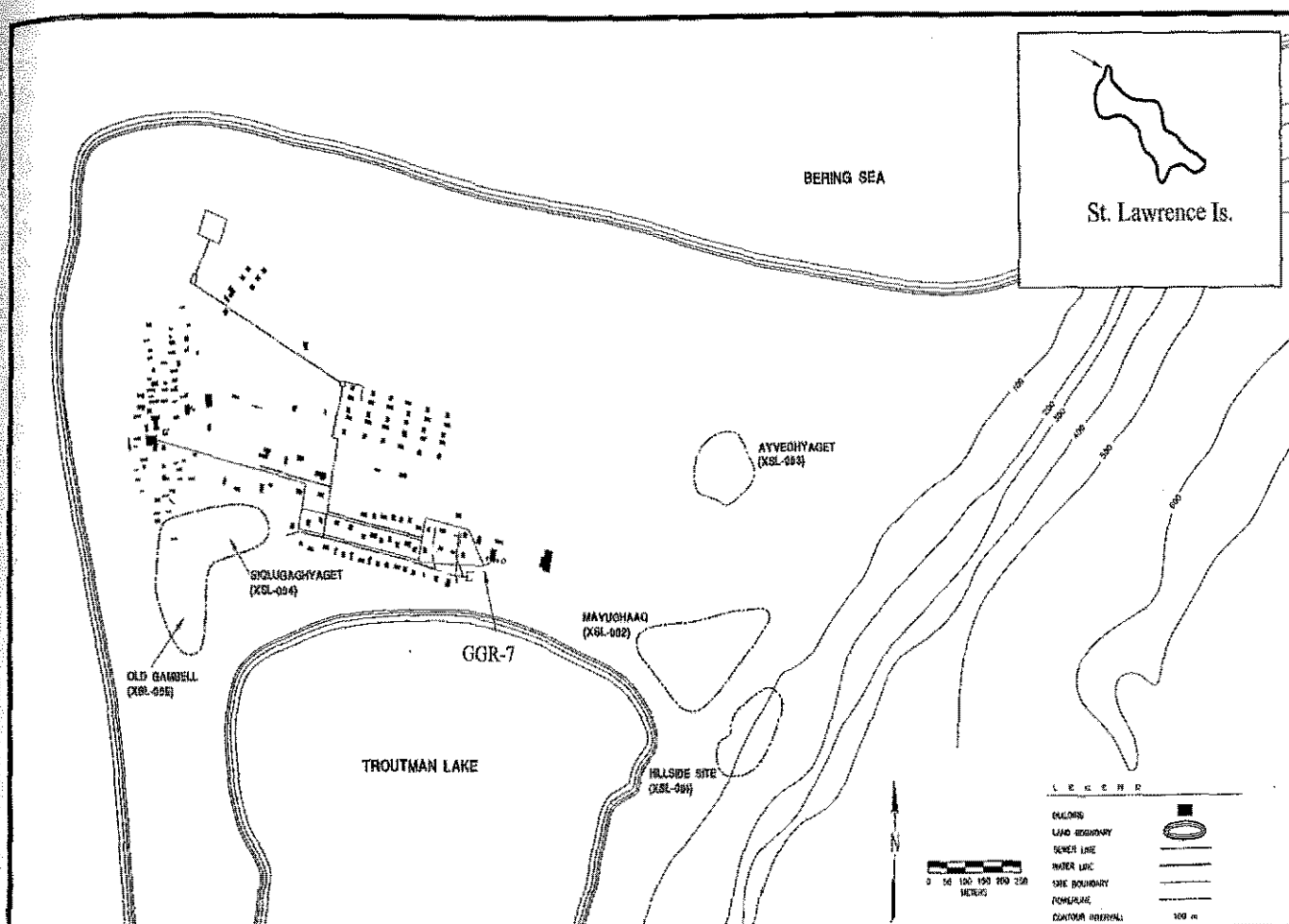


Figure 1. Map of Gambell indicating area of Grave 91-7 (inset of St. Lawrence Island).

This bone was probably a grave marker. A radiocarbon sample from the wood grave cover yielded a  $^{14}\text{C}$  age of  $1140 \pm 60$  BP (Beta-46510), calibrated, within  $2\sigma$ , to the calendar intercept ranges of AD 727-738 and AD 774-1018 (using the Oxcal probability option for the University of Washington data).

## COMPARISONS AND SIGNIFICANCE OF THE PUNUK WHALE BONE GRAVE

The significance of this find was unappreciated at the time of its discovery, prior to the publication, dissemination and, more importantly, translation, of the results of the Swiss research of the late 1960's-1970's (Bandi 1984, 1995; Bandi and Blumer n.d.; Bandi and Blumer 2002; Bandi and Bürgi 1972; Blumer 2002). From the perspective of 2004, however, the find is remarkable because so few of the graves in the Mayughaaq cemeteries, southeast of Gambell, contained *any* burial goods or artifacts that could be defined to a particular style or archaeological culture. Of the >360 burials within 25 km of the Gambell or Sivuqaq (named for the prominent bedrock bluff) vicinity (Kitngipalak to Dovlayaq), few graves

contained any significant amount of grave goods, although about half had at least one artifact. Multiple burials at Kitngipalak were more frequently provided with offerings (Bandi and Blumer 2002:35). The Swiss investigations encountered whalebone or driftwood structures in half of the burials ( $n=49$ ) (as calculated from Bandi 1984). Graves were frequently accompanied by either unburnt ( $n=19$ ) (Bandi 1984:63) or burnt offerings of animals: e.g., the ash concentrations observed in or adjacent another 18 graves (G XV, G 3, 4, 10, 13, 15, 16, 19, 21, 22, 26, 30/1, 38/1, 39, 40, 42/4, 42/18 and 42/16). Staley (1994b) calculated that 13% of the AEIDC/Mariah sample of graves ( $n=114$ ) contained one or more grave goods, 6% were associated with just a container ("casket") or a marker, and only 5% had both grave goods and a container.

Because so few of the Sivuqaq region graves contained any artifacts or offerings, ethnic and temporal inferences remain tentative (Bandi 1984; Bandi and Blumer 2002:38; Staley 1991, 1993a, 1994a). The definition for graves as Punuk (Early to Punuk-Thule) is often based only on spatial associations i.e. proximity to the Mayughaaq

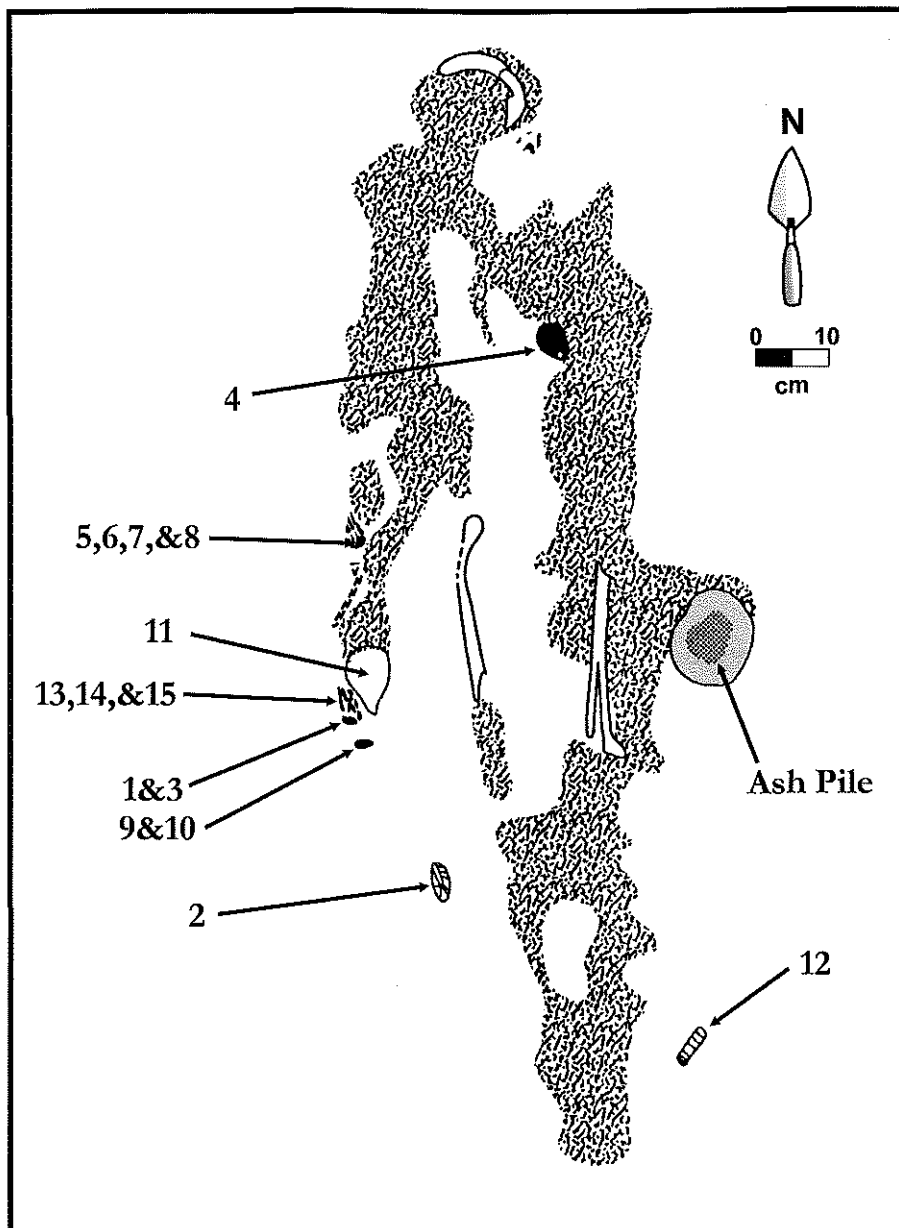


Figure 2. Plan view of Grave 91-7, offerings numbered (cf. Table II).

site. Punuk burials were more frequent than any other time period represented by burials with diagnostic artifacts, a fairly weak conclusion in view of the low number ( $n=16$ , out of 363 total, or 4.4%) of graves with diagnostic artifacts. Poor preservation accounts for some of the differences—many burials preserved only as “bone paste” and no other organic fragments (Staley 1991, 1993a, 1994a).

One purported, but undated, Punuk burial contained so many weapons points that Bandi and Blumer (2002:33) consider the internee a casualty of war. In addition to Grave 91-7, at least two other Gambell graves contained a bounty of decorated Old Bering Sea (OBS) III objects and iron (Bandi and Blumer n.d.; Collins 1937:64-65). However, one grave was discovered by non-archaeologists and its documentation is limited.

Can archaeologists conclude that wealth and status were differentiated at Sivuaq? Clearly, a few individuals were more honored than others, if not on the scale in Chukotka. If burial offerings translate into the residue of sacrifices or communal feasting, then Sivuaq peoples may have had abundant surplus goods. Instead of burying high status artifacts, Sivuaq residents may have believed that status was measured in sacrificed food—possibly according feasting a higher honor than among other Bering Strait peoples. Burial practices apparently differed considerably across Anadyr Strait—or the nature of society differed even more dramatically, in view of the re-used whalebone and driftwood precincts (family crypts) and very richly appointed burials at Ekven and Uelen (Bronstein 1993; Bronstein and Dneprovsky 2002; Bronstein and Plumet 1995; Leskov and Müller Beck 1993; Levin 1964; Mason 1998). Analyzing the published Ekven data, Mason (1998:257ff, Table 3) concluded that three OBS graves, ca. 1%, contained a disproportionate amount, roughly 66%, of the total harpoon heads ( $n=39$ ). Winged objects were also comparatively rare—occurring in only 10 graves out of >300. Thus, the status differentials at Ekven were as pronounced as on Sivuaq.

Mayughaaq cemetery precincts have produced 18 graves dated within AD 700 and 1200 (Bandi 1984; Staley 1991, 1993a, 1994a, this paper, Table II). Six interments predominantly older than AD 1020, based on two sigma values, while eight are predominantly younger than AD 970, some as young as AD 1300. Four graves equally likely dated between AD 850 and AD 1200. Nearly all of the dated cemetery graves contained one or more whalebone (16 of 18, 88%) and half (50%) had used wood for supports or a grave box. Only four cemetery graves (22%) had artifacts with diagnostic designs, three with Punuk or Thule attributes, one with Old Bering Sea motifs. Otherwise grave goods were comparatively rare; six had none and five only had a single object or two.

Table I. Associated artifacts from Gambell Grave 91-7; numbered to locations in Figure 2.

Artifact	Description
<b>Chipped Stone</b>	
1. Debitage	28 flakes of five different cryptocrystalline material types, all <3 cm
2. Flake Knife	5.7 x 2.4 x 1.1 cm jasper-like material, two lateral edges and terminus showing edge damage.
3. Biface fragment	Crudely worked, steep edge, 3.7 x 2.8 x 1.2 cm, Gray green argillite.
<b>Ground Stone</b>	
4. Blade	Possible pendant, beveled along single edge, three drill holes in triangular configuration.
5. Unworked "raw" Slate	43 pieces, ten >3 cm largest dimension.
6. Slate blade	5.0 x 4.2 x 0.2 cm, single edge beveled.
7. Slate End blade	8.0 x 4.9 x 0.6 cm, beveled both faces.
8. Slate End blade blank	3.1 x 2.5 x 0.3 cm, roughly chipped and ground
9. Pumice abrader	3.1 x 2.5 x 0.3 cm, roughly chipped and ground.
10. Palette	6 x 5 x 1 cm, rectangular, one face smoothly ground concave.
11. Vesicular Basalt Sinkers	13 x 11 x 6 cm, hole drilled though end.
<b>Ivory</b>	
12. Rod	5.5 x 1.0 cm End faceted.
13. Harpoon Head fragments	5, all approximately 8 cm, all have raised bosses as design elements, open with open socket, end blade slot parallel with a round line, single lateral spur and two rectangular lashing slots with Punuk design elements such as a raised boss with ellipse or "eye" similar to Collins (1937) type III-a-x, four are Collins type V x: All are closed socket, symmetrical single spur, round line holes, three with parallel slots, one with a perpendicular slot, Punuk design elements including one with a raised boss with ellipse or eye design near line hole
14. Arrowhead fragments	2 bases, roughened shaft, both approximately 4 x 1 cm.
15. Unidentified	Flat rectangular piece, 10.5 x 2.0 x 0.4 cm. A series of 12 drill holes along edge, similar to a "toy" piece photographed by Collins (1937:Pl. 59:11, pp. 413-414).

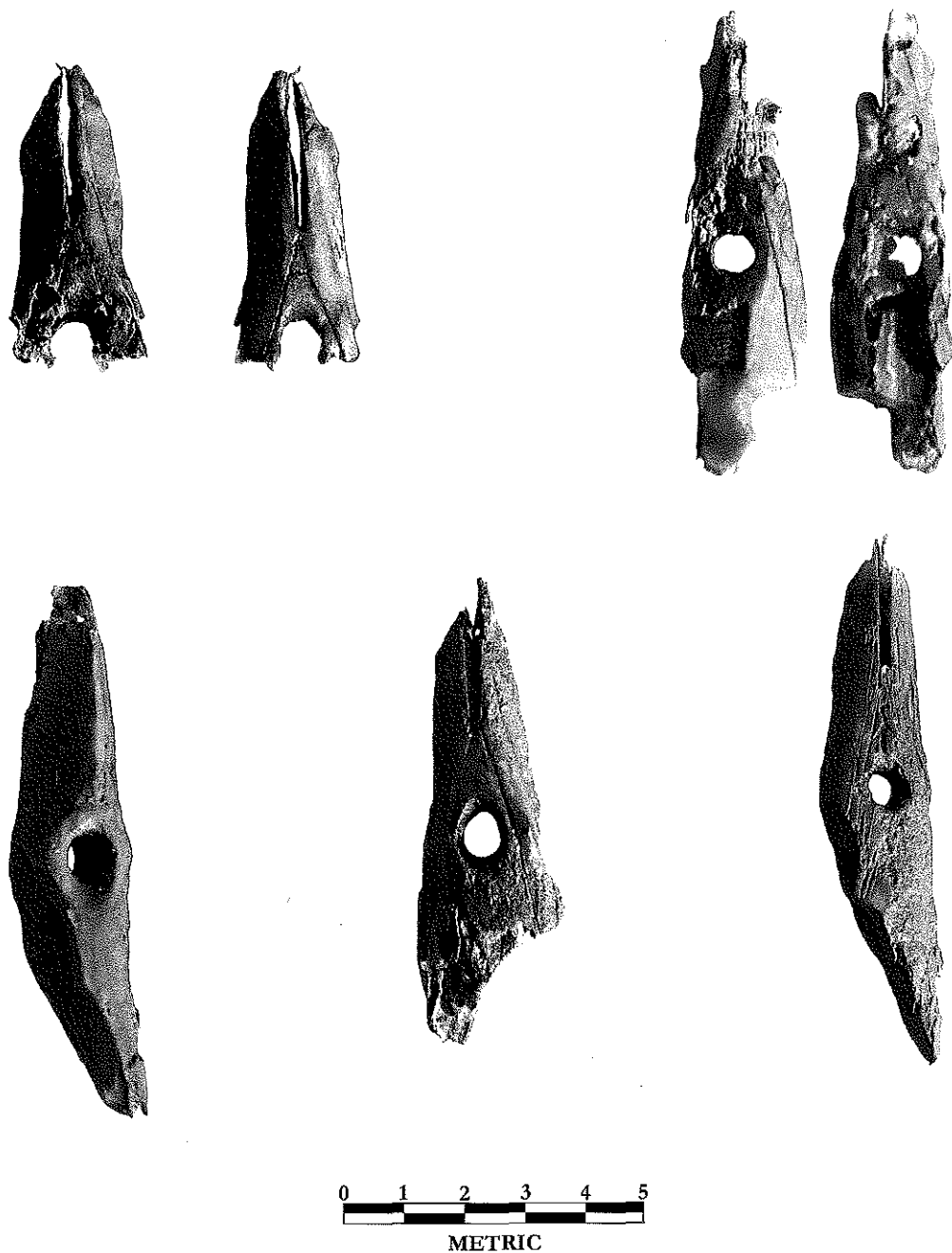


Figure 3a. Associated diagnostic artifacts from Grave 91-7. Punuk Harpoon Heads, Collins' Type III-a-x ("Sicco"), location #13 in Figure 2.

The ethnic complexities or, alternatively, the rapidity of change in the Sivuqaq region are evident in three possibly contemporaneous graves (Table II). Its contemporary (AD 715-750, 765-1025), G 42/16 had Thule motifs: a Y-shaped line that frames and issues from the line hole (Bandi 1984:50, Pl. 28:3,4), in contrast to the Punuk motifs within Gr 91-7. Possibly within years or decades, or even centuries later (AD 970-1340), OBS objects were deposited in the twin grave G 24 (Bandi 1984:43, Pl.68). Several questions remain: Where the three internees contemporaries or descendants, were the artifacts curated

heirlooms, or is OBS anomalously young at the Mayughaaq cemetery?

Two other contemporaneous graves also contained a fair amount of offerings, although not as extensive as Grave 91-7. Its contemporary, the adult male in G 42/16, had two harpoon heads and a single cobble scraper (Bandi 1984:50; Pl. 28). A young woman in G38/1 (dating as early as AD 876 to 1189) warranted five Punuk harpoon heads and an engraving tool (Bandi 1984:46, Pl. 22). The placement of harpoon heads within a female burial may

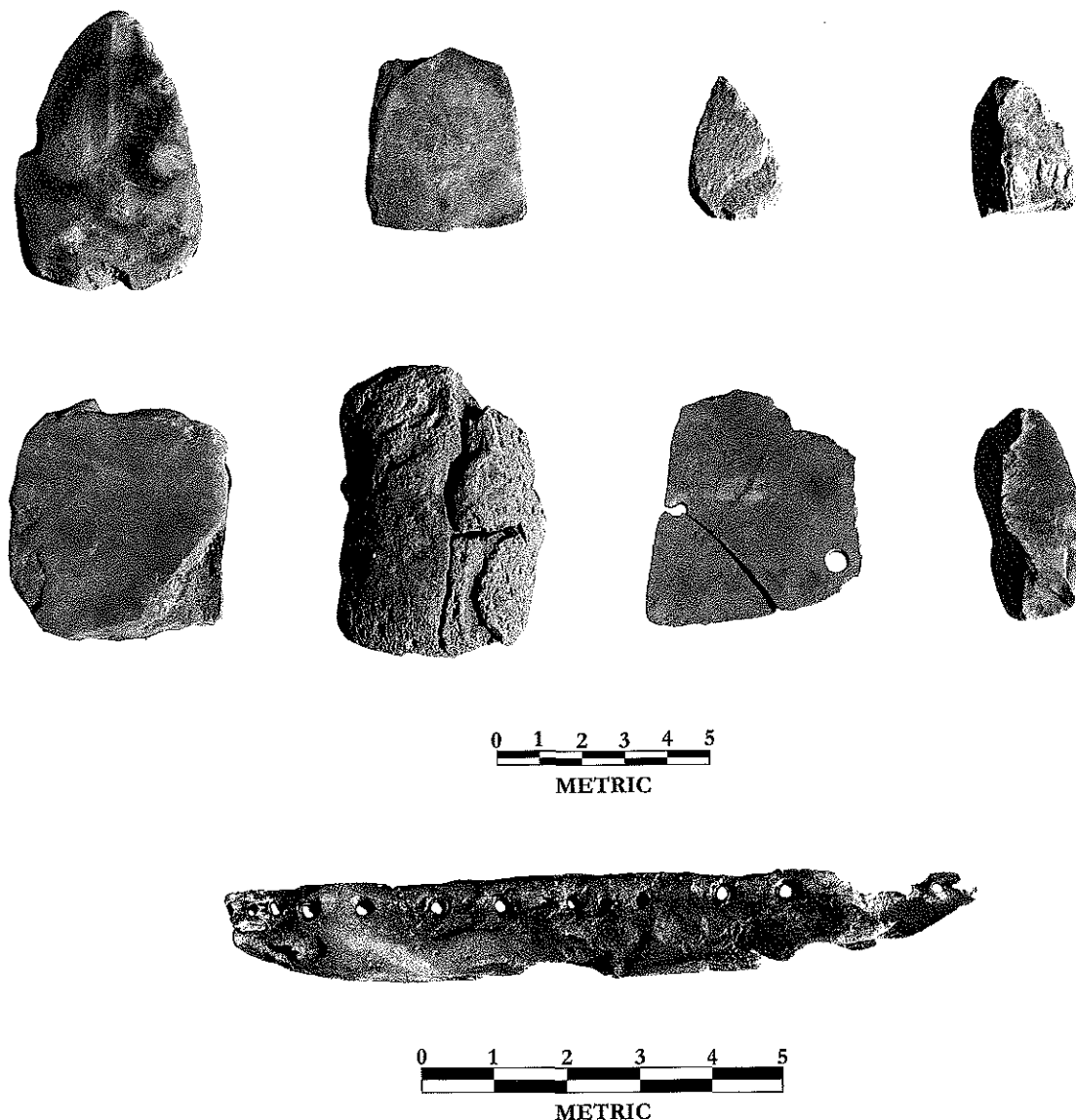


Figure 3b. Associated diagnostic artifacts from Grave 91-7, as located in Figure 2. Upper row: Slate blade (#7); Slate blade fragment; Worked triangular flake; Biface fragment (#3). Middle row: Pallette (#10); Pumice abrader (#9); Beveled blade, possible pendant (#4); Slate flake. Bottom row: unidentified ivory piece (#15)

reflect participation in hunting, a circumstance worthy of future research. Based on the extant data, few people at Mayughaaq during the late 1st millennium A.D. warranted any grave goods—roughly one in five, a finding that may indicate a basic difference in status between its prehistoric residents. Quite significantly, though, whalebones were a frequent accompaniment or marker. The OBS grave, stratigraphically earlier than early Punuk House 3 (Collins 1937:64) that dated between A.D. 645-945 (Blumer 2002:73), was more elaborate with a panoply of artifacts, including two needle cases, harpoon heads, socket pieces and possibly a winged object—as well as matted skins over the body (Collins 1937:64-65).

Although Gambell Grave 91-7 was well provided with grave goods compared to others at Sivuqaq, the grave goods remain modest compared to those in some OBS graves and to a few of those in Siberia. Ironically, the modern economic balance is reversed: Modern Gambell residents are much better provisioned (“wealthy”) than their Siberian cousins. The Siberians have roughly equal and modest tools with one or two families having slightly more or better tools, etc.—although consumer goods are doubtless entering Provideniya in higher amounts since 1991. At the other end of the world system, most of the Americans at Gambell have four-wheelers, high-powered guns, electronic appliances, televisions, and the occasional computer.

Table II. Graves in the Mayughaaq area dated between AD 700 and 1200.

[Sources: **G** series: Bandi 1984; **Gr** 91, 93, 94 series: Staley 1991, 1993a, 1994a). <sup>14</sup>C ages are on wood, unless otherwise specified by *italics*, which indicate that whale bone samples were used. Calibrated ages from Blumer 2002 and Staley 1991, 1994a). Whale bone ages were corrected following the 720 yr marine carbon value established by Dumond and Griffin (2002).].

	<u><b>AD 700-1000</b></u>			<u><b>AD 950-1200</b></u>	
<b>G 2</b> Male, mature	1040±90 BP (B-2431) <b>AD 810-845,</b> <b>855-1225</b>	Slate Blades (n=6) for Harpoon Heads Whale Bone Wood supports	<b>G VIII</b> No bones	840±70 BP (B-890) <b>AD 1035-1045,</b> <b>1150-1350</b>	Whale jaws, scapula, wood, no other artifacts
<b>G 42/1</b> Female Adult	1270±70 BP (B-2852) <b>AD 645-900,</b> <b>920-945</b>	Wood, Whale Mandible Superstructure Fairly sizable inventory, sled runner/mattock, slate points	<b>G X</b> Male 14-17 yr old	780±50 BP (B-894) <b>AD 1160-1175,</b> <b>1180-1305,</b> <b>1365-1390</b>	1 weathered slate-like tool
<b>G 42/12</b> Child—teeth only	1070±70 BP (B-3216) <b>AD 775-1065,</b> <b>1085-1125,</b> <b>1135-1160</b>	Worked wood and ivory	<b>G 14</b> Female, Older adult	950±90 BP (B-2860) <b>AD 975-1285</b>	Whale mandibles; 1 retouched flake
<b>G 42/15</b> Female adult	1160±80 BP (B-3219) <b>AD 685-1020</b>	Whale rib, wood cover Lithic flakes Wood, ashes	<b>G 16</b> Adult, Child	940±60 BP (B-2862) <b>AD 1045-1385</b>	Whale mandible, walrus bone, Wood, Ash offering, No artifacts
<b>G 42/16</b> Male adult	1130±70 BP (B-3210) <b>AD 715-750,</b> <b>765-1025</b>	Wood planks, whale rib 2 <b>Punuk/Thule</b> harpoon heads, cobble scraper	<b>G 24</b> Male – young adult; Female adult	850±70 BP (B-2434) <b>AD 970-1345</b>	Whale mandible, wood Extensive grave goods <b>OBS</b> winged object, scraper, slate spearhead, disc with <b>OBS</b> .
<b>G 40</b> Male adult	<i>1820±80 BP</i> <i>1100±80BP</i> (B-2869) <b>AD 717-748</b> <b>766-1045</b> <b>1088-1122</b> <b>1138-1156</b>	Whale mandible, rib, wood superstructure; 1 Slate knife; ash concentration	<b>G 38/1</b> Female Yg adult	<i>1550±60 BP</i> <i>830±60 BP</i> (B-2876) <b>AD 876-1189</b>	Whale Mandible, Bone Needles, Skin, Five <b>Punuk</b> harpoon heads, Engraver

	<b><u>AD 700-1000</u></b>			<b><u>AD 950-1200</u></b>	
<b>Gr 91-7</b> Male, Young adult	1140±60 BP (Beta-46510) <b>AD 727-738;</b> <b>AD 774-1018</b>	Whale mandibles; 5 <b>Punuk</b> harpoon heads, slate blades, ash concentrations, pumice abrader, flake knife, biface fragment, ivory arrowhead shafts	<b>G 58</b> Male adult mature	980±60 BP (B-2850) <b>AD 985-1220</b>	Whale mandibles, walrus rib, animal bones, rod shaped wood.
<b>Gr 93-4</b> Old adult	1310±90 BP (Beta-57399) <b>AD 540-900</b> <b>920-950</b>	Driftwood marker No associated. Funerary objects: one ivory flake	<b>G59</b> Male adult	940±70 BP (B-2856) <b>AD 1000-1265;</b> and 970±50 BP (B-2855); <b>AD 1000-1010;</b> <b>1015-1215</b>	Whale mandible, ribs, wood, walrus ribs. No burial goods.
			<b>Gr 94-67</b> Young Adult	910±60 BP (Beta-66843) <b>AD 1010-1250</b>	No super-structure or box. Two slate blades (4-5 cm width) “placed” over the body
			<b>Gr 94-78</b> Adult, elder	1280±60 BP (Beta-66845) <b>AD 1020-1260</b>	Organic sediments indicate offerings, no other goods

## CONCLUSIONS

The results of the 1991 monitoring operation indicate the potential for a single discovery to amplify and test archaeological preconceptions. One expectation would be that the social stratification and hierarchy arose during the development of organized whaling during Punuk, as Collins (1937) might have argued—if he had recovered a significant number of burials. The contribution and intensity of whaling during OBS and Punuk on St. Lawrence Island cannot yet be determined in the absence of data any more sophisticated than the qualitative statements on the archaeofauna by Collins (1937:247ff). However, very successful and large-scale whaling had developed by the latest centuries BC on the Chukchi Peninsula (Dinesman et al. 1999; Mason 1998).

The early 1990's cemetery data indicated to Staley (1994b) that Punuk was stratified but not as strongly as during OBS with comparatively less wealth in the hands

of fewer individuals during the Punuk culture. Two other graves may support this supposition: A richly endowed OBS grave (Bandi and Blumer (n.d.), as well as the mummified and well-provided for OBS internee uncovered by Collins (1937:64-65). However, the proportion of OBS graves on St. Lawrence Island with significant grave goods is still quite low in contrast to the situation on the Chukchi Peninsula (Mason 1998). Excluding taphonomic biases, several factors might explain this situation: (a) Sivuqaq had fewer high status shamans, warrior or whaling captains; (b) St. Lawrence mortuary practices favored status display by feasting rather than by depositing prized possessions; (c) descendants removed and recycled high status objects after deposition; (d) population increase or other social changes decreased surplus available for disposal in burials during Punuk as compared to OBS. Poor and middle economic classes were roughly equal in size during the OBS with wealth then being more broad

based. During Punuk, "the population of the lowest ranking swells, the middle ranking contracts, and the highest ranking remains constant" (Staley 1994b).

In addition, the second author's belated discovery of the 1991 report in the files of the Office of History and Archaeology highlights the untapped potential of the gray literature and its contributions to the greater labor of synthesis. The successes of the original monitoring should encourage the community to participate in archaeological enquiry (cf. Staley 1993b).

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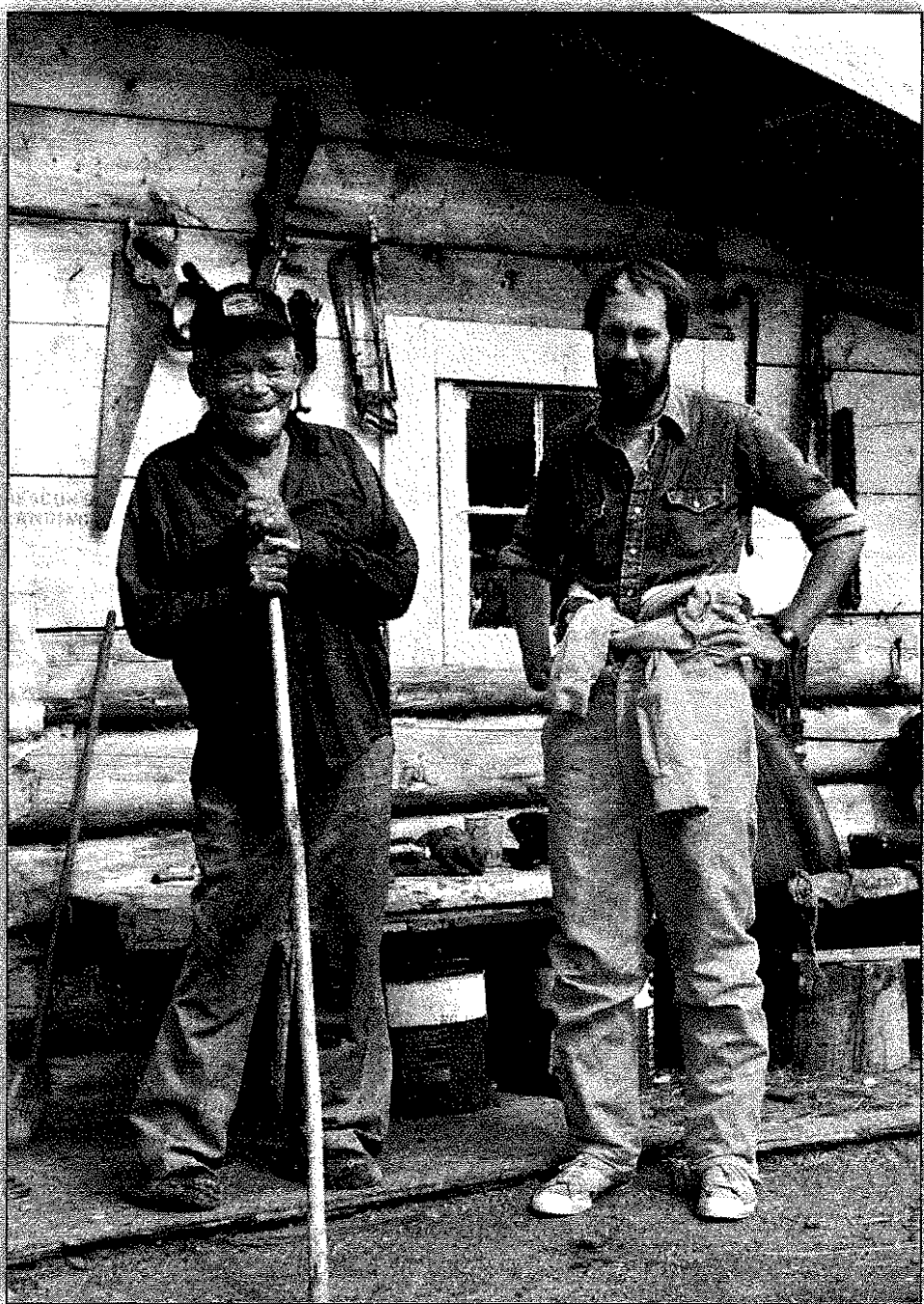
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Deacon Deaphon and Jim Sykes at Deacon's Landing, Alaska. Summer 1981. Elizabeth Andrews, photographer. Alice J. Lynch Collection, Accession # 98-18-3, Archives, Alaska and Polar Regions Collections, Rasmuson Library, University of Alaska Fairbanks.

## OBSERVATIONS ON RESEARCHING AND MANAGING ALASKA NATIVE ORAL HISTORY: A CASE STUDY.

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**Abstract:** Cultural, legal, practical, and/or ethical considerations challenge the management of virtually every oral history collection—regardless of geographical, topical, or ethnic affiliations. The oral histories of Alaska Natives are no exception, and as public interest in such materials increases so will associated access and use requests. Every individual and organization that performs oral history research (or manages the resulting records) should be educated about this subject, yet it has received little attention in the anthropological literature on Alaska. This essay examines the problem in the specific context of oral history records compiled during research based on historical place and cemetery site applications filed pursuant to Section 14(h)(1) of the Alaska Native Claims Settlement Act.

**Keywords:** Land Claims, Federal Collections, Public Domain Issues

### INTRODUCTION

Alaska differs from all other states in our nation in several ways but particularly with respect to its Native peoples, who are broadly separated into three groups: Eskimos, Aleuts, and Indians. The Federal government has further sub-divided Alaska Natives into 229 separate “tribes.” Most people are unaware that many Alaska Natives followed a seasonal subsistence round as recently as 1940. This means that individual families often did not have a “permanent” residence and only combined with other families into groups during a small portion of the year. Throughout most of the year, families moved independently between seasonal camps according to available subsistence resources—procured through hunting, fishing, gathering, and trapping. In this “subsistence” lifestyle, they lived not in modern framed houses but in semi-subterranean sod dwellings, cabins, tents, or other temporary shelters. Travel from point to point was not by planes, trains, or automobiles but by foot, dog team, or non-motorized boats. Partly because Native groups were so recently (and widely) dispersed across the remote and immense Alaskan landscape, in some areas Western educational and religious systems did not arrive until after 1920. Perhaps more telling yet, televisions did not appear in many Native villages until after 1980.

In Alaska, as elsewhere, the impact of non-Native influences on the transfer of information and knowledge has been severe for indigenous peoples who for centuries passed on knowledge by word of mouth. In contrast to Western civilization, the ability to share knowledge and information via written words is a recent introduction to Alaska Native cultures. In our Western literate society we could cease writing altogether, right now, and still be able to access major elements of our history and culture through previous literary works. However, Native cultures in transition from exclusively oral traditions to literacy risk losing that historical thread, because *their* books and archives live in their elders’ heads. When *every* elder has potential for significant contributions of historical facts and cultural practices, *any* elder’s death could constitute an irretrievable loss of cultural, historical and idiosyncratic knowledge. This is one obvious reason why such knowledge should be documented and preserved.

These comments provide necessary context to the unusual problems the Bureau of Indian Affairs (BIA) must address relative to the oral history collection this essay concerns. In the pages that follow, I describe how, why and by whom this collection was assembled, and evaluate key aspects of the associated research. I also

<sup>1</sup>The information and views presented are the personal opinions of the author.

elucidate some of the most significant factors that have complicated the efficient management of this collection, and others that may threaten its future integrity and usability. It is hoped that this critical, cautionary treatise allows others to learn from mistakes made and problems encountered by the BIA during its extended oral history research effort among and concerning Alaska Native peoples.

## BACKGROUND<sup>2</sup>

The BIA administers a nationally unique research program in Alaska to satisfy its mandate under Section 14(h)(1) of the 1971 Alaska Native Claims Settlement Act (or "ANCSA" [Public Law 92-203; 43 USC 1601 et seq.]). In addition to a cash settlement, the ANCSA legislation granted Alaska Natives fee simple title to 40 million acres of land and extinguished aboriginal title to any additional lands (Arnold 1978:146). One small part of this legislation, Section 14(h)(1), allowed Alaska Natives to receive a portion of their acreage entitlements in the form of historical places and cemetery sites. Whereas "cemetery sites" is a self-explanatory term, "historical places" include a wide range of cultural sites, such as abandoned Native villages and camps, rock art sites, and sites with legendary significance.

Eleven of the thirteen Alaska Native regional corporations created by ANCSA elected to take land entitlements provided under Section 14(h)(1).<sup>3</sup> The implementing regulations (i.e., 43 Code of Federal Regulations [CFR] 2653) made the BIA responsible for conducting 14(h)(1) site investigations, leading the agency to create its Anchorage-based "ANCSA Office" in 1978. Reports generated by these investigations are used by the BIA to determine if the selected sites are eligible for conveyance to the Native corporations as historical places or cemetery sites.

From its inception through April 1983, responsibility for implementing the 14(h)(1) program was shared between two organizations: the BIA ANCSA Office and a now defunct division of the National Park Service (NPS)—the Anthropology and Historic Preservation branch of the Cooperative Park Studies Unit (AHP-CPSU [popularly known by the shorter acronym CPSU])—then based at the University of Alaska Fairbanks (UAF).

During this period, BIA ANCSA developed the annual 14(h)(1) fieldwork plans, provided housing and meals, ran the field camps, and controlled the logistics of site investigations. The on-site responsibilities of BIA ANCSA personnel were essentially limited to the surveying of site boundaries. In contrast, CPSU personnel served as technical consultants to the BIA. Its researchers (most of whom were anthropologists, archeologists, or historians) verified the physical existence of sites, mapped and recorded all surface cultural remains, compiled data addressing site significance, and submitted individual site reports to the BIA for its use in determining site eligibility under the criteria of ANCSA Section 14(h)(1).<sup>4</sup> CPSU researchers also had primary responsibility for conducting oral history research, but their reliance on BIA for any necessary logistical support while in the field (among other reasons) often resulted in interviews being performed by joint CPSU-BIA teams. In any case, the division of research responsibilities just described is more clear-cut on paper than it was in reality. Which organization or individual had what authority—in the field—was a common subject of dispute. Thus, the role any given person actually filled tended to be less dependent on employment affiliation than on crew dynamics, individual experience or personality characteristics, and/or field "leadership" factors.

By 1980, the NPS was considering transferring the role then being filled by CPSU to another Federal entity (i.e., the U.S. Department of the Interior [DOI] "Heritage Conservation and Recreation Service"). Precisely why this action was being entertained is somewhat mysterious; but there is evidence that some NPS managers questioned the value of CPSU to the agency, and it had also become obvious to NPS that implementation of the ANCSA 14(h)(1) program was a very complicated undertaking. Another factor that must have been involved is that—unlike the BIA ANCSA Office—the original purpose for creation of the Anthropology and Historic Preservation branch of the (UAF) CPSU was *not* to perform ANCSA Sec. 14(h)(1) work (see Libbey 1984; Williss 1985 [Chapter 5]). In any event, the NPS ultimately disbanded AHP-CPSU and transferred its 14(h)(1) program responsibilities to the BIA in April 1983. This ongoing program has been administered solely by the BIA since that date.

<sup>2</sup>Some information in this section is drawn from Pratt (1992), or Pratt and Slaughter (1989).

<sup>3</sup>Arctic Slope Regional Corporation voluntarily opted out of this process. In contrast, the "Thirteenth Corporation" (comprised of Alaska Natives living outside the state) was not eligible to make ANCSA 14(h)(1) selections.

<sup>4</sup>Disagreements about what constituted an eligible ANCSA 14(h)(1) site, and how much weight BIA should give to CPSU recommendations on that subject, were frequent. One reason for such disagreements was that BIA felt CPSU operated from an academic perspective that every 14(h)(1) site that was positively located should be certified eligible by the BIA; as such, CPSU researchers' recommendations about site significance often were not taken seriously. On the other hand, CPSU considered BIA staff incompetent to perform the type of research and evaluations necessary to satisfy its mandate under ANCSA Sec. 14(h)(1).

As discussed below, deficiencies in the regulations that drive the ANCSA 14(h)(1) program have complicated its implementation. But most of the problems the BIA has encountered during its management of the program are linked to a far more subtle factor: that is, the anomalous nature of this program as compared to all others administered by the BIA, in Alaska and nationwide. The ANCSA 14(h)(1) program is the only one in the entire BIA universe whose beneficiaries are *not* Native tribes or individuals. Instead, its beneficiaries are Native regional corporations...an entity so unique that it seldom registers a blip on the BIA radar screen. The regional corporations are further obscured from visibility by virtue of the fact that their *shareholders* also happen to be the principal constituents of the BIA: i.e., individual Alaska Natives and/or members of Native tribes. As if the vague identities of the 14(h)(1) program's primary beneficiaries were not enough, the collection of records generated by its implementation is unlike any other held and managed by the BIA (again, both in Alaska and nationwide). Thus, in the long history of BIA operations across the United States no obvious precedents existed that could have guided the administration of this program. Consequently, the whole enterprise has been a huge and difficult learning process, and many missteps have occurred along the way. Readers should bear this point in mind when considering the observations presented in this essay regarding problems with field research and records management work on the ANCSA 14(h)(1) program.

### THE ANCSA 14(h)(1) COLLECTION

More than 2200 14(h)(1) site investigations have been completed to date, an effort that has produced an extensive archeological data collection. In addition, the 14(h)(1) collection includes an estimated 40,000-50,000 photographs, and a large variety of associated records. However, arguably the most valuable (and problematic) data are found in the 14(h)(1) oral history collection. It contains nearly 2000 taped interviews with over 1000 different individuals, 99% of whom were Native elders.<sup>5</sup> If these tapes were completely transcribed, the vast body of Alaska Native knowledge they contain would fill an estimated 50,000 pages. Additional oral history information is contained in notes on about 600 interviews that were not recorded, and annotated on topographical field maps that were used during many interview sessions.

The primary purpose of these interviews was to collect site-specific information, such as the Native site name, how and when the site was used, and by whom? But they yielded information on a wide range of related subjects including religious and ceremonial life, health and disease, culture change, folklore, technology, subsistence resources and activities, site types, land use patterns, kinship, social organization, language, values, and ethnogeography. Many of these subjects are poorly represented in the literature, if they are represented at all (Pratt and Slaughter 1989). Unfortunately, the collection is difficult to use because it lacks a comprehensive index and many of the oral history tapes have not yet been fully translated, transcribed or indexed.

Since most of the elders interviewed are no longer living these recordings often represent the only "permanent" records of their knowledge and life experiences. Thus, they provide the only means by which past elders can instruct future generations about their Native heritage (e.g., see Drozda 1995:117-119). It is important to note that many elders who shared their knowledge with ANCSA researchers described "traditional" Native life and customs not only through the stories of their ancestors, but also through first-person experiences. For example, elders gave detailed accounts of traditional ceremonies in which they had participated that now have been obsolete for half a century or more. Additionally, because many interviews were conducted in the Native language of the participating elder, these recordings can also be used to fight the continuing loss of Native American languages and dialects.<sup>6</sup>

The 14(h)(1) collection (particularly the oral history component) is rich in data about customary and traditional land use, occupancy, and subsistence practices. These data are fundamental to establishing and/or validating Native claims concerning subsistence harvesting sites and activities; and, used properly, they could facilitate sincere efforts to expand Native involvement in the management of natural and subsistence resources (cf. Nadasdy 1999). Less obviously—and consistent with increased interest in biodiversity and climate change—such data can also be profitably used to map historical trends in the population dynamics of marine mammals, caribou, and numerous species of birds and fish in support of national and international research on the health of arctic ecosystems (e.g., see ARCUS 1997:49-50;

<sup>5</sup>A minimum of 200 additional taped interviews related to ANCSA 14(h)(1) claims were produced between ca. 1974-1993 by Alaska Native regional corporations, and are not part of the BIA ANCSA collection. To date, efforts to obtain more detailed information about these recordings have been largely unsuccessful. It appears that the subject corporations have retained most (if not all) of the original tapes; but—despite offers of assistance toward this end—the majority evidently have not been copied, inventoried, or processed to any significant degree.

<sup>6</sup>The recent work by Amos and Amos (2003) is an excellent case in point, in that the authors extracted much of the older vocabulary—and many of the place names—presented in the dictionary from ANCSA oral history tapes.

CAFF 2001:100; 2004). In the process of searching through the collection to extract information of this nature, serious and objective researchers could not help but learn much about Alaska Native relationships to the environment and associated spirituality. Significantly, within the United States, first-person "traditional ecological knowledge" of the sort documented in ANCSA oral history records is comparatively rare outside of Alaska; but it is not unique within the larger circumpolar region (e.g., see Crate 2002; Nagy 1994).

Clearly, this collection has enormous potential for contributions to Native cultural heritage programs across the State of Alaska. It also contains a wealth of information about the history and establishment of various religious missions, schools, riverboat operations, trading enterprises, the reindeer herding industry, and other by-products of Euro-American contact with Alaska Natives. Thus, the collection is not only a record of Alaska Native cultural history but of the history of Alaska and the United States as a whole. Unless and until it is fully inventoried and indexed, however, the data it contains will remain largely unusable to all but the most diligent researchers; and even they will not be able to navigate through the collection without direct assistance from ANCSA program staff.

### **ORAL HISTORY RESEARCH IN THE CONTEXT OF FIELD LOGISTICS**

As suggested by Drozda (1995:110-115), the unique nature of the ANCSA 14(h)(1) program has contributed significantly to the difficulties encountered in its implementation. For instance, the high cost of conducting work in remote areas of Alaska had major impacts on the quality of oral history research performed by any given ANCSA field crew. To clarify this point, the following discussion intersperses details about one field project supervised by the author with information of more general relevance to ANCSA fieldwork as a whole.

In the summer of 1985 I directed a field crew conducting 14(h)(1) site investigations in the lower Yukon River region of southwest Alaska. Based at the village of St. Marys, this crew was in the field for a continuous period of 105 days. However, when Sundays (on which work was not authorized) and "weather days" (when high winds, fog, etc., precluded work away from camp) are eliminated, the crew actually had about 80 days in which to complete its assigned 85 site surveys. The actual locations of the targeted sites were not positively known and often could only be confirmed with the aid of Native elders; and the sites were scattered over an area of about

7,900 square miles (i.e., an area roughly the size of New Jersey). The lack of any road system in the region meant that time-efficient access to every one of these sites required a contract helicopter.

The project area contained 12 Native villages, and oral history research needed to be conducted in each of them. A total of 100 oral history tapes were produced by this crew, 27 of which were recorded "on-site" (i.e., at abandoned villages, camps or cemeteries). Production of these 100 tapes involved the participation of 69 different elders, 25 interpreters, and 11 interviewers. Records indicate Native elders and interpreters were paid for a combined total of 529 hours of work, amounting to about 5 hours per day of Native participation in the fieldwork. One might think that having 94 individual Natives participate in this effort was a positive thing, suggestive of broad involvement and a healthy diversity of viewpoints. But, in fact, the large number of participants hints at a lack of efficiency in the oral history process. That is, the number of elders involved versus the number of tapes produced indicates most elders were interviewed just once, precluding the opportunity for real rapport to be established between the elders and interviewers. Similarly, the large number of interpreters used assured uneven quality in the translations obtained.

In most years, oral history research on this program was hindered by the fact that employees were not dispatched to Native villages prior to the field season to identify the most knowledgeable elders for specific sites and project areas. Because the field season (i.e., May through September) is also the busiest season for subsistence activities, this meant ANCSA researchers often arrived in villages to conduct oral history work only to find many Native elders, as well as potential interpreters, away at fish camps or otherwise unavailable. Sometimes elders were found who only had limited time to talk, leading to rushed discussions among distracted participants who were literally "watching the clock." Additionally, the large number of site investigations planned for each field operation restricted the amount of time crews could devote to oral history work. The high cost of logistics was a further constraint.

Some of the major expenses involved in conducting field projects in the Alaskan bush may not be apparent to most readers: that is, costs additional to supplying and maintaining field equipment and covering wages, room and board, and airfare to and from the field for each employee. I will again use the 1985 lower Yukon River project as an example, focusing solely on the project's helicopter contract. The contract terms obligated the BIA

to pay all of the following costs: (1) a daily "availability rate" for each day the helicopter was mechanically able to fly, during normal work hours [and including Sundays]; (2) a fixed rate per flight hour [of which over 400 were flown on this contract]; (3) all required fuel; and (4) lodging and subsistence costs for the helicopter pilot and a part-time mechanic. Existing records indicate this particular helicopter contract cost the BIA about \$175,000.00. Bear in mind that this expense applied to a single ANCSA 14(h)(1) field project, and to only one component of that operation. Throughout the 1980s four ANCSA field projects were normally undertaken annually: each spanned three to four months and was staffed with a crew of 7-10 people, and most required contract helicopters. Since many of the related expenses were additional to normal BIA personnel and operating costs, it is no surprise that project managers tended to "pinch pennies" when they could; and costs tied to oral history work were not immune. Consequently, it was not at all uncommon for ANCSA researchers to be restricted from scheduling on-site interviews, and/or for the oral history research component of some projects to be severely curtailed.

Constraints on oral history work sometimes reached absurd levels due to conflicting opinions about its importance relative to other field tasks, and/or serious misconceptions about the amount of time required to adequately perform that work. For example, in July 1981 a BIA camp manager unexpectedly granted the author and three other CPSU researchers *one day* to gather oral history information about *twenty-one sites* that had been located and surveyed by that point in the field season. Although aware that the stated objective could not possibly be achieved, we jumped at the chance to engage in oral history work and did what we could. Another point must be made about this event. In the context of that specific field project, the "gift" of an entire workday for the sole purpose of conducting oral history research was not so much a nod to its importance in documenting site histories as it was a conciliatory gesture to CPSU staff on the part of the BIA camp manager. That is, to that stage of the project the CPSU crew had been allowed almost no opportunity for input into field planning and related logistical decisions. The camp manager's decision that day marked an important watershed: i.e., more than one month into the field season, he had finally recognized that CPSU staff participation in key operational matters was vital to the success of the field project.

The experience just described was not necessarily typical of the BIA-CPSU era. Some of those two-party field crews worked fairly smoothly and cooperatively from start to finish, whereas others were characterized by discord throughout their given field projects. For successful work relationships to develop, BIA and CPSU field personnel had to be committed (first and foremost) to the success of the projects they worked on. But also, and nearly as important, employees of each group had to overcome negative indoctrination they received about the other. Thus, in 1980 the author and all other newly-hired "BIA Field Investigators" were warned by BIA management that CPSU staff (sometimes derisively referred to as "academics") would try to assert control over most aspects of the fieldwork, including oral history research. We were firmly advised that BIA was the lead agency on the 14(h)(1) program and, accordingly, were instructed to resist any CPSU efforts to direct or restrict our involvement in the work. Hired as a "Research Associate" by CPSU the following year (1981), I soon realized that CPSU generally considered its BIA counterparts to be paranoid incompetents, whose participation in the 14(h)(1) work made an already difficult job nearly intolerable. There were elements of truth on both sides of this indoctrination; fortunately, however, most BIA and CPSU field personnel succeeded in rising above the polarizing distrust and resentment that seemed to exist between their respective home offices. As a result, although tension and issues of "turf" among crews were never far from the surface, the day-to-day operations of most field projects tended to run fairly smoothly.

### ISSUES IN THE MANAGEMENT OF ANCSA 14(h)(1) ORAL HISTORY RECORDS

The question of who properly controls access to and use of the ANCSA 14(h)(1) data—especially that derived from oral history research—has a contentious history, one result being that these data have been relatively inaccessible to researchers and the general public. This situation developed through a combination of factors.

For many years the BIA relied almost exclusively on ANCSA regulations to craft 14(h)(1) program policies and, unfortunately, those regulations do not address data management (see 43 CFR 2653). The ANCSA legislation, by itself, does not require 14(h)(1) data to be used beyond report preparation. It also does not require completed site reports to be published or widely disseminated,<sup>7</sup>

<sup>7</sup>Copies of all final reports are maintained by the BIA ANCSA Office and the Alaska State Office of the Bureau of Land Management (BLM) in Anchorage. Also, one report copy is submitted to the applicant regional Native corporation, and one is sent to the Alaska office of the Federal agency having jurisdiction over the lands on which the site is located. Besides the BLM and NPS, Alaska-based agencies that receive 14(h)(1) reports include the Fish and Wildlife Service (Anchorage) and the Forest Service (Juneau). The Alaska State Historic Preservation Officer (SHPO [Anchorage]) receives abbreviated, computerized site records. Otherwise, these reports typically are not distributed further (Pratt 1992:75).

nor does it stipulate actions to provide for long-term data preservation. Not until late 1990 did the BIA acknowledge the need to apply Federal laws concerning the management of archeological and ethnographic collections to the 14(h)(1) program. Other factors were involved, but the agency's failure to address such laws and regulations earlier was due mainly to its (and other DOI parties') original, narrow perception of the program as simply one part of a massive land transfer process—a "conveyance action" presumed to be disconnected from historic preservation, or cultural heritage/resources management. By logical extension, this led the BIA to place control of the program in the hands of "Realty Specialists" (as opposed to individuals trained in the social sciences); this situation prevailed from 1978 to 1996.<sup>8</sup>

Additionally, the BIA has major trust responsibilities to its Native clients. More specifically, the agency is responsible for managing "Indian trust assets": i.e., particular lands, natural resources, monies or other assets held in trust at a particular time by the Federal government for Native American tribes or individuals. These responsibilities apparently do not extend to Alaska Native regional corporations, the direct beneficiaries of ANCSA Sec. 14(h)(1), because ANCSA 14(h)(1) records do not involve "trust" lands or other "trust assets." Nevertheless, it is likely that 'trust' considerations factored into the development of a BIA policy whereby the release of 14(h)(1) data to a requesting party was forbidden without the written consent of the applicant Native corporations—most of which consistently denied data access and use requests. Until January 1989, BIA ANCSA management also stubbornly used this policy to withhold 14(h)(1) site data from the Alaska SHPO. Although Native regional corporations endorsed it, the fact that this stance was contrary to Federal and State historic preservation/cultural resources management laws helped fuel an increasingly negative opinion of the BIA (and the ANCSA 14(h)(1) program) in the Alaskan anthropological community. Despite repeated objections from both within and outside the agency, this highly restrictive approach for handling data access and use matters was in effect at the BIA from about 1978 to 1990. Its most significant legacy is that it fostered a belief among most regional corporations that they own and control all of the data generated by the 14(h)(1) program. The BIA finally obtained a legal opinion on this matter from the

DOI Solicitor's Office in September 1990; it concluded 14(h)(1) data should be treated no differently from data collected by other Federal agencies working on public lands with public funds. On paper, this opinion significantly increased rights to access and use of the data, but it also produced a rash of objections from the Native regional corporations (see Pratt 1992:76-77).<sup>9</sup>

The question of ownership and control of these data (particularly oral history data) is further clouded by the fact that other Native entities besides the regional corporations have voiced opinions on the issue...mostly based on nascent perspectives on the concepts of cultural or intellectual property rights.<sup>10</sup> Many of the participants in this debate are unfamiliar with, or unwilling to acknowledge, the principal parts of its foundation: i.e., the specific purpose and requirements of ANCSA Sec. 14(h)(1); the process by which these data were collected; and the framework of Federal laws within which these data must be managed. Not surprisingly, these various Native parties do not necessarily agree with one another on how requests for access to and use of this information should be handled (even assuming the related decisions could be entirely under Native control). Villagers may disagree with positions taken by their regional corporations, or tribal officials; and some regions are beset by political strife between the regional corporation, local tribes, and other Native organizations. Even individual members of the same family sometimes disagree on this issue. Factionalism of this sort is a practical reality—one that the passage of ANCSA surely exacerbated (e.g., see Dauenhauer and Dauenhauer 1994:98-103)—and it can create impediments to public use and dissemination of information about Alaska Native history and traditions. At the most basic (philosophical) level, it also begs the question of which Native entity is the right one to consult with on any given issue. But, more cynically, if knowledge truly is power then so is the control of knowledge; and some parties are clearly less concerned about the preservation and future usability of this collection than they are about controlling the knowledge it contains. This is especially ironic given that most of the parties that have exhibited such attitudes are ignorant of the collection's content.

In any event, a number of difficult issues must be resolved before the BIA can develop a comprehensive

<sup>8</sup>Historically, BIA decisions about how the program should be organized, conducted and managed rested largely on its interpretations of the language contained in the implementing regulations (i.e., 43 CFR 2653), much of which is vague and open to debate. This problem was compounded by the absence of references in those regulations to other relevant Federal laws—most notably the National Historic Preservation Act of 1966 (16 USC 470 et seq.), on which the ANCSA 14(h)(1) site eligibility criteria are based.

<sup>9</sup>It should be explicitly stated that site location information is, and consistently has been, kept confidential.

<sup>10</sup>The subject of cultural and intellectual property rights is beyond the scope of this essay. See Brown (1998) for a thorough review of these concepts and their associated effects on the treatment of indigenous knowledge in the public domain.

policy for access to and use of the 14(h)(1) oral history materials. For instance, until all of the interview tapes have been topically indexed there is no reasonable way to determine how many of them contain information of a "sensitive" nature that must be restricted, for either legal or ethical reasons. It could be a major mistake if the BIA simply made these tapes available to anyone who requested access, because we really do not know their contents. Even when the content of a given tape is known, however, the process of deciding whether any of the information it contains is 'sensitive' is typically—and unavoidably—highly subjective. The end result is that requests for access to and use of these materials must be evaluated and decided by the BIA on a case-by-case basis.

Efficient processing (e.g., including transcriptions, translations, indexes) of the collection is further complicated by the fact that its creation involved more than 150 separate interviewers, the vast majority of whom (whether affiliated with BIA or CPSU) were sent to the field with no prior experience in oral history research—and without written guidelines (e.g., see Tobias 2000) or explicit methodologies on which their work could be anchored.<sup>11</sup> Some interviewers received virtually no training or instruction on how to do ethnographic research; others (including the author [in 1980]) were subjected to a short "training course" by an Alaska State Trooper in what can most accurately be characterized as "interrogation techniques;" and still others were evidently deemed qualified for this work simply by virtue of holding academic degrees. Insufficient training or experience in oral history research was manifested by poorly organized interviews, the common use of low-grade tapes or audio equipment, and inattention to technical details (e.g., recording levels) during interview sessions. Thus, although many excellent recordings were ultimately produced, a lack of consistency in the conduct and quality of these interviews is pervasive (cf. Drozda 1995:110-117).

Some ANCSA researchers undertook oral history research with trepidation...cognizant of their lack of expertise with the process, their lack of rapport with local villagers, and the consequent potential for failure. But others approached this enterprise carelessly, as if inter-

viewing Native elders was a fun and simple task...as opposed to "work." (After all, how hard is it to pop a tape into a recorder, turn it on, then sit back and listen to an elder talk about the good old days?) The corollary to this latter mindset was that anyone could do oral history research; consequently, virtually everyone participated in this activity—and with predictably mixed results. Thus, many interviews are disorganized, confusing, and sorely lacking in basic contextual information.<sup>12</sup>

Yet another problem associated with early ANCSA recordings is that some CPSU researchers—at the direction of CPSU management—made canned statements at the start of interviews asserting that information provided by the interviewees would not be published, and their names would be kept private.<sup>13</sup> Presumably, a "policy" of this sort would have been committed to writing; but no such document has been located to date, so the actual genesis and purpose of these statements is not clear. According to some former CPSU employees, however, they may have been meant to protect elders from potential legal proceedings arising from particularly controversial ANCSA 14(h)(1) claims. Even if that is true, the existence of such statements on ANCSA oral history tapes is troubling on several levels.

First, efforts to "protect" elders' identities are contrary to Alaska Native cultural traditions concerning the transmission of knowledge. Native elders typically talked only about what they knew from personal experience, and they took great pride in ensuring the accuracy of their accounts. In this sense, failing to identify by name the elders who provided specific oral history accounts may be interpreted as acts of disrespect: i.e., equated with ignoring or devaluing the elders' recognized status and authority as tradition bearers and local cultural historians. (On an operational level, this practice also reinforced BIA management's basic distrust of CPSU. That is, the failure of CPSU researchers to explicitly identify the sources of oral history accounts they used to argue the merits of 14(h)(1) sites sometimes caused BIA to question the veracity of that information.)

Second, elders' concerns about passing on and preserving their knowledge have been magnified by the pro-

<sup>11</sup>Interestingly, an 18-page long, procedural manuscript drafted in 1978 or 1979 by the first Director of the BIA ANCSA Office (Naughton n.d.) included 8 pages of thoughtful guidance on conducting oral history interviews; but it apparently was never incorporated into a formal training manual. It should also be noted that, up to about 1985, many ANCSA researchers entered the field with little or no knowledge of the Native cultures and histories of the regions in which they would be working (cf. Drozda 1995). In 1987 the BIA produced an ANCSA 14(h)(1) field manual that addressed many of these shortcomings.

<sup>12</sup>In fairness, however, it should be noted that even experienced, well-prepared and respectful interviewers could not guarantee good results. Put another way, "all the experience and training in the world cannot prevent a bad interview" (Drozda 1995:113).

<sup>13</sup>Not all CPSU researchers recorded such statements; some were never told to do so and others simply ignored their supervisors' instructions. Similarly, some CPSU managers may not have been concerned about matters of this sort. In any case, because it was specifically tied to CPSU, this particular problem is temporally restricted to ANCSA 14(h)(1) oral history recordings produced prior to 1983.

cess of culture change, which has greatly affected information exchange within Alaska Native communities and groups. The standard venue for such exchange was traditional teaching and storytelling, which was the elders' domain. Natives who grew up in times characterized by close contact between elders and younger generations know how important such contact is to retaining their cultures, languages and beliefs. Many elders who lamented that young people were growing increasingly ignorant of their Native heritage shared their cultural and historical knowledge with ANCSA researchers in the ardent hope that doing so would preserve their teachings for younger, and future, generations.<sup>14</sup> To treat the information these elders (openly, willingly, and purposefully) provided as "confidential" would essentially lock it up, thereby preventing their wishes from being realized.

Third, CPSU researchers almost certainly had no legal authority to impose blanket restrictions on the information collected during oral history interviews; and it is equally doubtful that CPSU ever sought legal advice on the matter. Procedural inconsistencies within CPSU, both in field and office settings, testify to the evident lack of critical attention given to this subject. For instance, a number of the organization's "Occasional Paper" publications (see Libbey 1984) drew heavily on ANCSA 14(h)(1) oral history information (e.g., Andrews 1977; Koutsky 1981; Lynch 1982; Sackett 1979). Ironically, these publications were being produced coincident with CPSU field researchers asserting, on tape, that oral history information provided by Native interviewees would not be published in any form.

In fact, since it was gathered as part of a publicly-funded project (mandated by Federal law) and produced by Federal employees<sup>15</sup> within the scope of their employment the information is in the public domain; and, evidently, none of it can be restricted except in ways explicitly specified in relevant Federal laws (e.g., Archaeological Resources Protection Act [16 USC 470aa-mm], Privacy Act [5 USC 522a], Freedom of Information Act [5 USC 552]). It should also be noted that works produced by the Federal government cannot be copyrighted.

The preceding remarks logically lead to the topics of release forms and informed consent, neither of which can be discussed in isolation from cross-cultural communication problems encountered between ANCSA researchers and many Native elders (see Drozda 1995). Language barriers posed serious problems to effective communication. Many ANCSA 14(h)(1) oral history interviews were conducted in two languages, English and an Alaska Native language. Since the overwhelming majority of ANCSA researchers spoke only English, or did not speak the given Alaska Native language, interpreters were required for most interviews—and even the best of them were unable to accurately convey certain words and concepts between the two languages (cf. Morrow and Hensel 1992; Pratt 1993). Further, as a result of generational change in Native languages, interpreters often could not understand (much less translate) some words and phrases used by their elders (cf. Dauenhauer and Dauenhauer 1987:8). This was especially true when the interpreter and the elder were separated in age by several decades and/or came from different villages: and neither scenario was uncommon. Other factors also complicated this process.

For example, in 1982 I was part of an ANCSA field crew conducting site investigations in the lower Yukon and Kuskokwim river regions of southwest Alaska. In the Yukon River village of Pilot Station I was unable to find anyone to interpret for me in oral history interviews. One man, perhaps 30 years old, declined the job claiming that—at that time—his generation had little interaction with their elders (a fact I later learned, from elders, was a source of tension in the village). His unwillingness to act as my interpreter seemed to be based on shame for his lack of relations with the very elders I hoped to meet; of course, political issues or cultural mores (e.g., see Drozda 1995:113-114 [#2]) unknown to me might also have been at play. I ultimately hired a 64-year old man from the nearby village of Marshall for this job. An elder in his own right, this man had essentially grown up with Pilot Station elders and shared similar life experiences; thus, he spoke their language in more ways than one. As luck would have it, I could not have asked for a more capable interpreter.

<sup>14</sup>The BIA has tried to increase the possibility that this goal will be achieved by: (a) in 1995, funding a \$115,000.00 contract to produce multiple copies of every tape in the collection; (b) providing copies of all ANCSA 14(h)(1) oral history tapes, region-by-region, to the relevant Native regional corporations; and (c) placing a duplicate set of the entire tape collection in the UAF Archives.

<sup>15</sup>AHP-CPSU employees were considered UAF staff; however, the entirety of the AHP-CPSU operation was paid for with public funds under a Federal contract...the principal purpose of which was to satisfy the NPS' responsibilities per Sec. 14(h)(1) of ANCSA. The work of AHP-CPSU was, therefore, a Federal "work for hire" and the information generated thereby is in the public domain. Clear support for this position is found in the General Provisions (Data [c (*Rights in data*)]) of the NPS-UAF contract (No.CX-9000-9-0060 [April 1979]), which states that data produced under the contract become the property of the Government.

Conversely, there were also occasions when interpreters were readily available, and badly needed, but their participation was considered unnecessary by the interviewees. That is, in interview settings certain elders (regardless of their fluency with the language) insisted on speaking English.<sup>16</sup> Interviewers faced with this problem had little choice but to defer to the elders' wishes, although doing so greatly impeded some interviews. Due to language deficiencies, elders determined to speak English sometimes could not understand the questions being asked and/or were unable to construct clear and appropriate answers. Conversely, ANCSA researchers did not always recognize how important the language factor was to making elders comfortable in interview settings and to producing worthwhile end products; thus, many interviews began in confusion and never got on track. The resulting recordings can be extremely convoluted and difficult to interpret; in fact, some require translations from "village English" to standard English.

In my experience, even elders who possessed a good command of English invariably expressed themselves more eloquently and precisely in their native tongues. A knowledgeable elder who was able to converse in his/her native tongue without undue interruptions, an experienced interpreter who understood what types of information ANCSA researchers needed, and a trusting relationship between all participants provided the best foundation for rich and coherent interviews (cf. Drozda 1995:114-115). This ideal was hard to achieve, however, given the time constraints imposed by the large volume of work individual ANCSA field crews were expected to complete.

Release forms were typically presented to interviewees as permissions to use their words in reports and, secondarily, to preserve and safeguard the words of elders by providing copies of the resulting tapes or transcripts to designated entities or repositories (e.g., local schools, UAF Archives) (Drozda n.d.). Thus, although signed release forms are on file for most interviewees, they are very general in nature and may be more accurately characterized as "partial" releases. But assessing whether or not a proper release was obtained from an interviewee must take into account several other important points—one of which is whether the conditions and agreements specified in the release form are legally enforceable. Additionally, ANCSA researchers did not interview or tape record any person without his or her permission. Each person was given the option of saying "no" to the request for an interview, as well as to the

request that the interview be recorded; and some people *did* decline to participate in this process. Because participation was strictly voluntary, the simple act of agreeing to be interviewed—and to have the event tape recorded—*may* constitute a release. The same goes for accepting payment for services rendered to the government, and interviewees were customarily paid for their services. Thus, "releases" for ANCSA 14(h)(1) oral history information may derive from one or more of several distinct but closely connected factors: signed release forms, cash payments, and voluntary contributions of information by individuals who openly consented to having their words tape recorded.

And what are the practical and legal parameters of "informed consent"? Regardless of how hard I tried to explain things, I know that some elders who signed, or made their marks on, release forms in my presence did not understand their purpose. (This is also true for some interpreters who helped "explain" the release forms, and even personally signed them as witnesses.) In fact, in their eagerness to participate in the oral history documentation process some elders with whom I worked essentially "tuned out" during discussions of the release forms: they just wanted to get started! Another consideration (brought to the author's attention by Drozda [n.d.]) is that the insistence on securing permission, in written English, to use or repeat the words of individuals who were illiterate, or did not speak the language, was almost certainly culturally inappropriate. At the very least, release forms' origin in Western academic or legal rituals rendered them foreign to Alaska Native elders. Given such problems, one could argue that elders' signatures on interview release forms are of dubious value. My point is that informed consent may sometimes be an unattainable and/or unrealistic concept (particularly in cross-cultural settings)—even if it is pursued with integrity, the best of intentions, and sincere respect for the interviewees (cf. Brown 1998:199-201).

Finally, although 'informed consent' is a laudable objective (and one that researchers should always strive to attain), the concept embeds the risky assumption that the involved parties recognize and mutually understand the full range of potential uses to which the oral history in question may apply...both at the time of the interview and in the future.<sup>17</sup> This is especially problematic with regard to information gathered to resolve Native land claims filed pursuant to ANCSA Sec. 14(h)(1), because every final decision rendered on such claims is subject to

<sup>16</sup>This was usually manifested as a matter of courtesy (e.g., "We should speak English because I know you can't understand me when I speak Cup'ig").

<sup>17</sup>For example, in 1980 who could reasonably have envisioned creation of the Internet/World Wide Web and the many attendant problems with regard to information use and dissemination (cf. Brown 1998)?

legal appeal. This alone suggests that the BIA could not arbitrarily restrict access to related oral history materials. In fact, legal actions tied to the 1989 Exxon Valdez Oil Spill required the BIA to share its oral history and archeological files with Exxon attorneys, despite strenuous objections from the relevant Native regional corporation.

## PRESERVATION CONCERNS

For the time being, the ANCSA 14(h)(1) collection is secure and intact, and work devoted to improving its organization, accessibility and usability is ongoing. But, though its status is not likely to change in the immediate future, there is legitimate cause for concern about the ultimate disposition and preservation of this valuable collection. Just how grave this concern should be depends largely on whether the BIA labels the collection as museum property, official records, or fiduciary trust records. The specific management responsibilities vary between these three record types, as does the availability of the subject records for access and use. The fact that a decision about what to call the 14(h)(1) collection is even necessary at this late date is a testament to its unique composition. That is, this collection differs from all other BIA record sets to such an extent that it does not have a clear place in the agency's present records management scheme.

One point all interested parties would certainly agree on is that records in the ANCSA 14(h)(1) collection have sufficient historical or other value to warrant continued preservation by the Federal government beyond the time they are needed for administrative, legal, fiscal or other official purposes. In other words, they must be managed as "permanent" not "temporary" (i.e., disposable) Federal records. There would probably also be unanimous consent among these parties that the collection, in its entirety, should be preserved and housed in Alaska—its place of origin and home of the people to whom the information it contains is most relevant. However, this objective evidently cannot be achieved unless the 14(h)(1) records are officially designated a "museum property collection" (as defined by the DOI in Departmental Manual Part 411, Chapters 1-3 [411 DM 1-3]). This action would give the BIA the necessary (and currently non-existent) legal authority to negotiate an agreement whereby the 14(h)(1) collection could be archived at a suitable Alaska repository.

The ANCSA Office completed the necessary documentation to have the collection designated 'museum property' and—with the Alaska Regional Director's concurrence—submitted it for approval to the BIA Central Office (in Washington, D.C.) in November 1998. Six years have now passed without a formal response from the Central Office to the ANCSA museum property documents. The lack of response is particularly frustrating given that—since submission of the documentation—two different teams of museum property experts have traveled to Anchorage from Washington, D.C. specifically to perform on-site assessments of the collection, and both unanimously concluded that it clearly satisfied the definition of museum property.<sup>18</sup>

So, why has the BIA failed to take positive action on this matter? There is no clear answer to this question, but several factors are apparent. The first is that oversight of the Bureau's museum property program (from its inception in September 1991 through February 2004) was assigned to the BIA Division of Property Management. This is significant because the things staff of that division have traditionally been responsible for managing include buildings (and other real property), furnishings, office equipment, and the like—not cultural and historical records/items, which require a very different management mindset. Another factor is that once an agency officially acknowledges that it has a museum property collection it is also accepting responsibility for organizing, housing, and preserving that collection for access and use over the long term. The involved property managers at the BIA Central Office have been averse to committing the agency to such obligations; in the present case, this has been manifested in arbitrary refusals to entertain any argument that the ANCSA 14(h)(1) collection is museum property. A final "property" issue that has periodically surfaced, in a negative way, relative to this collection is the fact that it consumes a comparatively large amount of floor-space...the annual cost of which is not inexpensive. Thus, some BIA managers seem to think of the 14(h)(1) collection as just a bunch of file cabinets eating up space that should really be accommodating workers.

The present situation can be further clarified by noting that BIA document collections are normally categorized as "official records." When no longer needed to conduct current business, records in this category are delivered to National Archives and Records Administration (NARA) facilities (i.e., Federal Records Centers)

<sup>18</sup>The first assessment was performed in July 2000 by a four-person team: i.e., the BIA Curator of Museum Property; the DOI Museum Property Program Manager; the Chief, DOI Museum Services; and the Assistant Chief, DOI Museum Services. The second assessment occurred in March 2002 and was done by a two-person team: i.e., the Chief Curator and NAGPRA Coordinator, BIA Museum Property Program; and the Staff Curator, BIA Museum Property Program.

for organization, housing, and preservation. Records that go to a Federal Records Center are not immediately available for research; in fact, that situation only changes when (and if) title to the records is officially transferred to NARA. Significantly, NARA does not accept museum property collections. Since a NARA facility is present in Anchorage, however, it might seem that an easy way to eliminate existing preservation concerns related to the 14(h)(1) collection would be to designate it as “official records”—and abandon the museum property tack altogether. But doing so might threaten the collection’s integrity in at least two ways: (a) NARA generally does not accept archeological materials (i.e., artifacts); and (b) previous contacts with the Alaska Branch of NARA indicate the 14(h)(1) oral history tapes could potentially be shipped out of Alaska, for archiving at a NARA facility in Maryland.

Assuming BIA facilities satisfied NARA archival standards, it might be possible for the agency to label the 14(h)(1) collection official records and then negotiate an agreement with NARA whereby BIA facilities were designated an affiliate archive. This would allow the collection (in its entirety) to be permanently retained for ongoing business of the BIA; however, there is not presently any BIA building that could qualify as an affiliate archive.

A reasonable argument can probably be made for managing the 14(h)(1) collection as either museum property or as official records. In either case, the basic management requirements are the same: organize, house, and preserve the collections for access and use (Wilson n.d.). The best (and only reasonable) way for the government to facilitate future access and use of these records is to make certain that the individuals most knowledgeable about the ANCSA 14(h)(1) program are provided the requisite time and support to develop “finding guides” that cross-reference and link the collection’s multiple components *prior to archiving*. Valuable information (both physical and contextual) will ultimately be lost if this basic objective is not met.

In contrast to those tied to museum property and official records, the management requirements attached to “fiduciary trust records” differ in important respects, the most notable being that these records are not accessible for public use or research. The materials that make up the ANCSA 14(h)(1) collection do not appear to meet the existing definition of ‘trust records’: i.e., Federal records that include information that influences, affects, governs or controls Indian trust assets. But some expansion of this definition may occur as an indirect response

to ongoing litigation involving BIA mismanagement (outside Alaska) of Indian trust records and assets. This litigation has understandably made the BIA extremely cautious about how it defines the records now under its control; thus, if there is any doubt as to whether a certain type of document, etc., is a trust record the BIA will err on the side of caution and treat it as such. For example, because the records it contains are so important for documenting the cultural history and heritage of Alaska Natives the ANCSA 14(h)(1) collection could conceivably be considered a ‘trust asset’—thereby setting the stage for a trust records designation. This would almost certainly torpedo efforts to keep these records in Alaska, because the intended repository for all BIA trust records is the “American Indian Records Repository” in Lenexa, Kansas (established in 2004).

### CLOSING REMARKS

The ANCSA 14(h)(1) oral history collection is relevant to a broad range of projects and issues of concern to Federal, State, and Native organizations, and the general public. But its accessibility, usability, and preservation are most critical to Alaska Natives, the majority of whom remain unaware of its existence. It is also noteworthy that complex and highly-charged political issues such as “subsistence” and “tribal sovereignty” have, over the years, created a “Native-vs-non-Native” dichotomy in the minds of many Alaska residents. Widespread ignorance of Alaska Native histories and cultures is arguably the root cause of this problem, just as education is its solution. And who better to educate the masses on these subjects than the hundreds of elders whose collective knowledge and experience is captured in the 14(h)(1) oral history collection (cf. Burch 1991:13-14)? These elders’ sincere desire to continue to teach even after death was a major reason why they collaborated with ANCSA researchers and agreed to be recorded on tape. Simply having the words recorded on tape, however, is not enough to preserve this knowledge.

The tapes are secure, but they remain severely under-processed; and the longer this situation persists the more difficult it will be to accomplish the associated tasks. Many people assume that lack of funding is the chief impediment to fully processing this collection, but the real problem is a lack of qualified people to perform the required work. For instance, no amount of available funding can generate a transcript of an interview conducted primarily in a Native language if a competent interpreter/translator cannot be found who is willing to do the work. Locating such people is more difficult than one might think and keeping them on task can be harder still, particularly

once it is clear just how tedious and demanding a job they have taken on.

For example, from 1986-1991 the BIA spent roughly \$250,000.00 under contract with the UAF Alaska Native Language Center (ANLC) to produce translations and transcriptions of selected tapes. Although ANLC employed Alaska Natives with professional training in the orthographies of the relevant languages and dialects, this contract work was highly problematic and most of the end products can only be characterized as "preliminary" or "works in progress." The results have been even less positive when BIA has provided funds directly to Native tribes and organizations to process oral history tapes. The primary cause for the poor return on such contracts has been the difficulty of finding and retaining committed, qualified workers.

The BIA now realizes that detailed topical indexes of these tapes constitute the minimum level of processing required to effectively manage this part of the oral history collection; and indexes are a more realistic objective than full translations and transcriptions. But this processing goal also will not be easy to meet. Recent experiments indicate an experienced researcher requires an average of eight hours to topically index one 60-minute oral history tape of good audio quality. This statement must be qualified. An "experienced researcher" means someone knowledgeable of the region, local geography, and cultural group(s) represented on the tape, and who is also familiar with the ANCSA 14(h)(1) program. An interview tape "of good audio quality" means one that was recorded at the proper speed with good equipment, does not contain excessive background interference (e.g., blaring CB radios, crying infants, wind noise), and whose participants spoke clearly and above a whisper. These ideal conditions could hypothetically lead to production of a topical index of one interview tape for a cost of around \$200 (based on eight hours at a rate of \$25.00 each [which is probably an unreasonably low rate]). In this best case scenario—which ignores known language problems and also assumes the necessary workers are available—the entire 14(h)(1) oral history collection could conceivably be indexed for roughly \$400,000.00. But the real cost would certainly be at least two to three times as high...because the majority of tapes do not satisfy the 'good audio quality' criteria and there are a limited number of appropriately 'experienced researchers' capable of doing the indexing work.

Along with ensuring the tapes are completely indexed, the BIA must give careful attention to inventorying and indexing the hundreds of note files on non-taped inter-

views, and systematically working through the annotated field maps to identify and organize the oral history information they contain. The successful performance of these tasks will be similarly problematic, time-consuming, and expensive.

Given the scope and complexity of the associated challenges, the only way the ANCSA 14(h)(1) oral history collection can be adequately processed is through a sustained partnership involving the BIA, Alaska Natives, and other interested parties. The feasibility of such a collaborative effort would ultimately depend on the participants' abilities to get beyond any suspicions they may initially have toward one another (e.g., see Nadasdy 1999:2-3) and keep their respective energy focused on the preservation needs of the 14(h)(1) oral history collection. If this level of shared commitment was achieved there is little doubt that the necessary funding to complete the work could be secured, and the enormous potential of this collection could finally be realized.

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## BOOK REVIEWS

### *SHEM PETE'S ALASKA: THE TERRITORY OF THE UPPER COOK INLET DENA'INA*, 2ND ED.

BY JAMES KARI AND JAMES A. FALL. (UNIVERSITY OF ALASKA PRESS, FAIRBANKS, 2003). CLOTH, ISBN 1-889963-56-9, \$65.00; PAPER, ISBN 1-889963-57-7, \$29.95. 392 PP. MAPS, TABLES, FIGURES, INDEXES, BIBLIO.

Reviewed by Don E. Dumond

University of Oregon

Referred to by the authors as an "annotated and mapped ethnogeography of traditional Dena'ina place names" within the upper Cook Inlet dialect area of the language, this is a new edition of the early favorite published by the Alaska Native Language Center in 1987. According to its preface, this much revised and expanded version includes an additional 253 named places (for a total of nearly 1,000) and revises more than 75 of those listed previously; it adds 14 new stories or expository articles; and it is equipped with handsome shaded relief maps, the work of Matt Ganley.

Going beyond these simple statements of the authors, it is abundantly clear that the book has been expanded in every way. There are now 66 numbered maps, compared with 28 previously; there are new photographs, the total now numbering more than 225, and pictures used previously are much improved in quality of reproduction. There is a section of color plates that provide much improved notions of the nature of landscapes. Most useful, where the original edition was not indexed there are now three separate indexes: by geographic name, by personal name, and by subject. As before, the present edition includes a word profile of Shem Pete (who died in 1989) with additions to bring it to present date. And it also includes profiles of additional source-individuals drawn on especially for this edition, although not entirely invisible in the previous one. The most important of these are Shem Pete's son, Billy Shem Pete, and Billy's second cousin and clansman Sava Stephan Sr. (their maternal grandmothers were sisters). These two provide numerous additions and emendations to the corpus of places and the stories about them.

Basic presentation is again carried by 16 substantive chapters, each listing named locations in a separate geographic subdivision of the area — a sub-area such as a fairly contained drainage basin or the shores of a large water body. Places are listed in the former fashion, numbered by chapter and by individual place. But within this

overall plan, the format in the second edition is changed: whereas stories and articles relevant to each chapter were earlier placed together at the chapter's end, following the enumeration of places, they are now larded through the place listings themselves. And whereas named places in the earlier edition were given in order strictly numerical (e.g., from 1.1 through 1.117), the numbers now appear in more scattered fashion, as the listing follows a geographic rather than purely numerical progression.

The implication should be clear: this is a handsome and promisingly useful book. To be sure, a few production boggles can be found if one looks carefully — numbering of maps and figures not always in agreement with text citations, cited reference omitted from the bibliography — but there are so many citations of so many maps, figures, and references that the reader must be impressed that mis-citations are so few. The leavening of articles and personal stories lifts chapters above their enumeration of places. Thus, the descriptive entries of places combine with those stories and articles, recycled and new, to produce much more than an annotated geography. The book is an encapsulated ethnography of the Dena'ina people emphasizing both subjective and objectified views of their territorial world.

Among the new expository sections in the text are a segment on Dena'ina log construction (republished from a work by D. C. Beard), a section on Dena'ina watercraft, by author Kari, and a somewhat expanded explication of the Dena'ina sound system and practical alphabet, together with a parallel discussion of similar aspects of the Ahtna language as well as a few notes on at least the alphabet of the Upper Kuskokwim Athabascans. The significance of a substantial part of the expansion is clear in that a large number of the new articles relate to Dena'ina history and hypotheses bearing on it. More overtly historical additions include an article on the pre-history of the upper Cook Inlet region by Douglas Reger, a brief report of archaeological investigations at Hewitt

Lake in the upper Yentna River drainage by R. Greg Dixon, and an analysis of Dena'ina and Upper Kuskokwim relations by James Kari.

These come together around the authors' proposals that prehistoric Dena'ina-speaking people maintained close relations with their Atna-speaking Athabascan neighbors, now to the east of them; that nevertheless the same Dena'ina, or at least those of the ancestral Upper Inlet dialect group, had at one time occupied an area nestled among Upper Kuskokwim bands west of the Alaska Range; that the ancestral Upper Inlet group moved to the east of the Range (which would have involved the Hewitt Lake area) and onto the Inlet from the west, being the first of the Dena'ina to arrive on salt water. Kari proposes that this arrival was as much as a couple of thousand years ago, by which time they had established close contact with the Atna. Reger, in his article, suggests on archaeological grounds that the Upper Inlet group of Athabascans arrived in the area possibly as early as 1,500 years ago, apparently following abandonment of the area by coastal people with closer relations toward the Kodiak Island group — people of so-called Kachemak cultural tradition. Tantalizingly, from what must be Kari's point of view, Dixon in his description of work at Hewitt Lake reports a radiocarbon determination dating some occupation at about 4,400 years ago. But Dixon provides no immediate support for ancient Athabascan residents in that region not far southeast of the Alaska Range, for he is inclined to relate the Hewitt Lake finds, which include some objects of polished slate, again to people of Kodiak of that date.

These discussions together provide a nod to an especially uncertain area in Alaska prehistory — the geographic position of ancestral Athabascans. Whereas there seems to be a certain amount of agreement among prehistorians on the presence of ancestral Northern Athabascan and linguistically related Eyak people somewhere in south mainland Alaska by 2,000 or 2,500 years ago, the location of their predecessors in the millennia before that is both unclear and disputed. At one extreme, it has been argued that ancestral Athabascans, or Na-Dene, have been present in interior Alaska for more than 10,000 years. On the other, lie suggestions that much of that interior was unoccupied between around 5,000 and 2,000 years ago, and that ancestral Athabascans appeared from somewhere to the south. Difficulties in relating the relatively few well-dated interior archaeological finds of this period to historically known Athabascans, include both the extreme variation in subsistence adaptations manifest by the different Northern Athabascan groups of history (coastal beluga hunters, riverine fishers, interior moose

and caribou hunters) and the very attenuated inventories of artifacts revealed in so many apparent Athabascan sites.

But these questions reach far afield from the subject that is tapped in the book under review. The subject here is the geographic outlook of the most extensively settled coastal dialect group (Upper Inlet) of the single Athabascan language group (Dena'ina) that was thoroughly adapted to life on the salt-water coast at the time Europeans first arrived in southern Alaska. To this subject *Shem Pete's Alaska* makes again a signal contribution.

# ***NORTHERN ATHABASCAN SURVIVAL, WOMEN, COMMUNITY AND THE FUTURE.***

BY PHYLLIS FAST. (LINCOLN: UNIVERSITY OF NEBRASKA PRESS, 2002). HARDCOVER, ISBN 0803220170, \$55.00. 288PP. WINNER OF OUTSTANDING NATIVE AMERICAN BOOK AWARD FOR 2000 FROM THE UNIVERSITY OF NEBRASKA PRESS.

## **Reviewed by Steve J. Langdon**

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As anthropologists strive to develop new forms of analysis and presentation of ethnographic materials, the balancing of data, analysis, reflexivity and relevance poses an enormous challenge. In *Northern Athabascan Survival, Women, Community and the Future*, Phyllis Ann Fast has met these challenges in a path breaking and illuminating volume. The book is a thematic, powerful account of the lives of the contemporary Gwich'in of northeastern Alaska that flows from three years of fieldwork by Fast in the region. Fast, of mixed northern Athabascan (Koyukon) heritage raised in an urban Alaskan environment, set out to understand the political and social dimensions of Gwich'in politics in their villages but moved beyond this goal to a fuller engagement with the nature and process of existence in the communities.

The three themes that Fast explores in detail are the nature of the economic system in the Gwich'in communities, the nature of leadership and action, and the position and role of women in the society. As foundation for her discussion, Fast establishes core Gwich'in values that are demonstrated in oral traditions, myths and legends whose behavioral impacts she discerns in the lives of those with whom she lived. Particularly influential are the promotion of self-reliance and the respect for autonomy that are merged with an expectation that nonvoiced, but shared understandings about and commitment to respectful interpersonal engagement will promote social harmony. She establishes the key features of kinship, warfare, trading and cosmological connectedness that were the center of Gwich'in existence. She then tracks the progressive transformation of Gwich'in life over the past 150 years as new material, demographic, social, economic and religious elements affected Gwich'in existence. Sustained down to the present and especially evident in the last 50 years are behaviors designed to maintain as much control as possible over resources and activities in Gwich'in country despite critical alterations to practices in all these domains.

Fast's thematic exploration of the "economic" domain is constructed around the concept of addictiveness. She treats the entire cash apparatus of the modern economy as promoting forms of material addictive behavior to things but saves the core of her analysis for the intersection of the classic addictions (alcohol, drugs, gambling) with the bureaucratic institutions that have sprouted up to "cure" them. Her critique notes that it is mostly non-Gwich'in that benefit from both expenditures on the addictions themselves as well as on the provision of services to the addicted. This insures that the definitions of such behavior and the solutions for it are externally controlled. Fast explores the incompatibility between the internal, Gwich'in grounded system of open-ended, ambiguous reciprocity in which need is addressed indirectly and nonjudgmentally and the external, market grounded system of closed, precise calibration of all such exchanges. She sees the latter as eroding the trust and mutual support of the former through the penetration of personal addictions that consume resources and minimize extended relationships.

Leadership and action are a second thematic development of the book. Here Fast most powerfully grounds her discussion in the Gwich'in language and its development of intersecting notions of metaphysical power, personal power (mental and interactional) and demonstrated power (through accomplishment). She traces traditional male leadership modes in terms of antecedent patterns of trader, warrior, and spiritual leader. While these models for leader behavior are seen by Fast as limiting and difficult to attain, she also articulates the contradiction between the core value of nobody being in charge with the expectation that behavior will flow according to the previously noted notion of being of the same mind. Fast demonstrates the problematic inherent in this design for living and advances the concept of consensus to account for how the Gwich'in accomplish familial ends through forms of implicit and explicit violence to expel non-Gwich'in threats and violators from their communities. She documents the role of administrators in institutions

such as the school, city, and police accommodating and deferring to expressed and acted wishes of the Gwich'in violated. The consensus is then that those violated have the right to redress directly through their actions. She sees this as emergent behavior from a traditional basis that has become more evident and acted upon due to the increasing stress in the communities as greater frequency of contact with nonlocal persons occurs. An especially sensitive node in this pattern is the school that drops unsophisticated and uncaring outsiders into the community on a continuous basis.

Gender is the final thematic development of the book. Here Fast demonstrates sensitivity for nuance and specificity in her relationships with and observations of various Gwich'in women with whom she interacts. Her reflexivity about the learning obtained and the contexts of these encounters are important demonstrators of Fast's connectedness with the intimate decisions and events occurring around her. Her main contention is that women, since the basis and form of their leadership is traditionally underdetermined, are now molding and shaping the manner in which leadership takes place and moving into fields of influence outside the bounds of traditional behavior. She sees them as central in addressing the welfare of the community as nodes of redistribution to those with needs. Women are also developing strategies to protect themselves and children from the dangers presented by Gwich'in men who are recognized as having difficulties managing the addictive forces they encounter. Fast here explores various theories and cases of colonial impacts on indigenous women concluding that what she observed among Gwich'in women were periodic waves of anger, depression and assertiveness that could occur in any order and never reached levels of resolution. She found that the search for the numinous (Christian, tradition or both) was often the route to addressing a variety of problems.

An important element in Fast's critique of the addictive society that envelopes the Gwich'in is her deconstruction of statistics developed by state institutions that demonstrate high rates of destructive violence among Gwich'in villagers. She exposes the hidden assumptions about what constitutes violence and demonstrates that census data, when these assumptions are reasonably altered, indicate that Gwich'in rates actually fell from 1970 to 1990. She sees the development of such stigmatizing data as an artifact of segments of the health industry addicted to the maintenance of addiction in the client population. She concludes that the Gwich'in neither define nor act in the world on the basis of the definitions and assumptions of the statistics.

In the final two chapters, Fast looks to the future and recapitulates the central claims of her book highlighting the relevance of certain key findings for the future. Here the relevance of her findings to specific areas of action is presented. While she clearly intends this section for a Gwich'in readership, it is unlikely that any Gwich'in will be drawn to the discussion. Nevertheless she asserts her belief that they will find Athabascan solutions to their problems. Unfortunately Fast does not address how the external institutional structures might be reconstructed to better meet the needs of the Gwich'in. She starkly asserts that the Gwich'in villages are threatened by a myriad of forces for destruction but concludes that through the search for the identification of the problems, the Gwich'in will persevere. They will do so from a self-definition that as a people, "We suffer, we endure, but we continue on" (Fast: 255)

Through the preceding review I have attempted to indicate the value and power of Fast's moderately experimental ethnographic construction and its worthiness for the American Ethnological Society's Junior Scholar Book Award. The tapestry that is woven does not claim to be exhaustively coherent, which indeed if it were so claimed would not be honest, for ultimately *Northern Athabascan Survival* is a statement of personal engagement not distanced measurement

# ***THROUGH SPANISH EYES: SPANISH VOYAGES TO ALASKA, 1774-1792.***

BY WALLACE M. OLSON. (HERITAGE RESEARCH, LIMITED EDITION, 2002). AUKE BAY, ALASKA. SOFT COVER, ISBN 0-9659009-1-6, \$60.00, PLUS POSTAGE. 576PP. 8 CHARTS, ANNOTATIONS, BIBLIO., TOPICAL INDEX.

Reviewed by J. David McMahan

Alaska Office of History and Archaeology

*Through Spanish Eyes: Spanish Voyages to Alaska, 1774-1792* (576 pages) is a long overdue translation and synthesis of the accounts of Spanish exploration in Alaska. Thanks to Dr. Olson's commendable work, those of us involved with the anthropology and history of Alaska must no longer resort to poorly reproduced manuscripts or sketchy translations to research this little understood chapter of our state's history.

Spanish interest in the north Pacific Coast extends at least as far back as the early sixteenth century. Following the conquest of Mexico, Hernan Cortes outfitted several ill-fated expeditions to search northward along the Pacific coast for riches, a coastal route to the Spice Islands (Moluccas Islands, Indonesia), and the ever elusive Northwest Passage (Strait of Anián) through North America. As early as 1524, Cortes was preparing three ships for Pacific exploration [4th Letter of Relacion, 1524, Pagden 2001:320-321], and in 1525 the ships were at the port of Zacatula (250 miles north of Acapulco) "prepared to embark on a voyage of exploration along those [Pacific] coasts [5th Letter of Relacion, 1525, Pagden 2001:320-321]." Shortly before their belated departure in 1527, and much to the consternation of Cortes, King Charles redirected the expedition (led by Alvaro Saavedra Ceron) to the Moluccas in search of survivors from the Magellan voyage. Two of the ships were reported lost at sea north of the Marshall Islands. Subsequent Pacific voyages by Spain during the 16th and 17th centuries were dedicated largely to the Manila-Galleon trade between Acapulco, Peru, and the Philippines. Sporadic expeditions, however, continued to ply the waters northward to Baja and California. In 1769, California became a colonial province of the Spanish Empire, although Spain claimed a divine right to all lands along the Pacific Coast of North America under the 1494 Treaty of Tordesillas. Despite her claim, Spain was aware of British, American, and Russian trade along the North Pacific Coast during the late 18th century. It was in the context of these emerging empires competing for lands along the north Pacific coast that Spain sent late 18th century expeditions northward. Following the 1789

Nootka incident with the British, which nearly brought the nations to war, Spain eventually limited her control to lands below 42 degrees north.

*Through Spanish Eyes* provides details of the eight documented Spanish voyages that extended into Alaska during the late 18th century. These include voyages in the years 1774 (Juan Perez), 1775 (Bruno de Hezeta, Juan Francisco Bodega y Quadra), 1778 (Esteban Martinez, Gonzalo Lopez de Haro), 1779 (Ignacio Arteaga, Juan Francisco Bodega y Quadra), 1790 (Salvador Fidalgo), 1791 (Alejandro Malaspina, Jose Bustamente y Guerra), and 1792 (Jacinto Caamano). By 1790-1791, voyages had extended up the coast as far as Kodiak Island, English Bay, and Prince William Sound. Don Antonio Maria Bucareli y Ursua, Viceroy of New Spain 1771-1779, was directly responsible for the first three voyages, and for humanitarian policies towards Natives that influenced subsequent voyages. Some of the voyages are documented through journal entries by more than one person. *Through Spanish Eyes* includes translations of a number of parallel journal entries. These complimentary selections, along with commentary by the author, provide for a more complete picture. In addition to translations, the book sets the stage with background information on the archaeology, linguistics, and anthropological understanding of the Northwest. It concludes with a chapter on Spanish withdrawal from the region, an extensive bibliography, and 10 appendices. The latter include sections on Spanish measurements, Spanish naval ranks, biographies, Northwest Coast artifacts in the Museo de America, an illustration of a Spanish Pedrero (swivel gun), Spanish place names from the 1779 and 1788 expeditions, vocabularies collected on the 1788 expedition, ships engaged in the fur trade from 1785-1791, and one page from Tadeo Haenke's transcription of a Tlingit Song of Peace at Port Mulgrave in 1791. In addition to the book, the publisher sells a complimentary chart (24 x 18.5 inches) of the Bucarelli Bay area reproduced from the atlas which accompanies the journal of La Perouse (1799).

Professor Olson, has prepared a well-organized, detailed monograph that will be a welcome addition to the bookshelves of anthropologists and historians interested in Spain's voyages along the northern Pacific coast.

*Through Spanish Eyes* (\$60) and the complimentary chart (\$7.50) may be ordered from Heritage Research, Box 210961, Auke Bay, Alaska 99821 or by contacting the author directly (phone/fax 907-789-3311; e-mail [wmolson@ptialaska.net](mailto:wmolson@ptialaska.net)). Shipping costs for the book are \$2.68 for media rate. Priority mail rates vary according to the distance from Juneau.

# ***FAITH, FOOD & FAMILY IN A YUPIK WHALING COMMUNITY.***

BY CAROL ZANE JOLLES WITH ELINOR MIKAGHAQ OOZEVA. (UNIVERSITY OF WASHINGTON PRESS, SEATTLE, 2002). A McLELLAN BOOK. 364 PP. \$50 (HARDCOVER), \$24.95 (PAPERBACK). BLACK AND WHITE PHOTOGRAPHS, FIGURES, FOOTNOTES, APPENDIX, GLOSSARY, BIBLIOGRAPHY, INDEX.

## **Reviewed by Katherine Reedy-Maschner**

Department of Social Anthropology, University of Cambridge, UK. Department of Anthropology, Idaho State University, Pocatello

St. Lawrence Island was a fistful of deep-sea earth squeezed dry in the hand of the Creator and placed in the Bering Sea, according to local belief. Jolles traces the spiritual world of the contemporary marine mammal hunters who inhabit the island, particularly in the village of Gambell, one of only two inhabited villages on the island. Yupik people are considered in relation to clan organization, names and souls, marital unions, birth and death, hunting and religious ceremonies, and the commitment to wild subsistence foods. *Faith, Food and Family* is an engaging read, from Jolles' Easter arrival in 1987 through more than a decade of mutual respect in sharing life's moments, good food and stories. Jolles' commitment and concern for the people of St. Lawrence Island is unquestionable. The trials of everyday living—such as getting water (before indoor plumbing became the norm in 1997) and following ever-changing 'roads' through mud, ice and snowdrifts—are vividly described for the Yupiit, but also for Jolles herself, who sometimes fumbles but learns with patience and diligence.

The 1878 epidemic and famine resulting from introduced diseases from foreign whalers was a defining crisis in their history that ultimately characterizes today's island life, religious dedication, and family structure. Jolles reprints Nelson's description of his 1881 encounter on the island with his chilling Pompeii-esque scenes of bodies frozen in death. The proselytizing that followed this time, which was clearly opportunistic, disregarded the social system, and missionaries successfully convinced the Yupiit of a superior Christian god. Jolles outlines the conversion processes to Christianity and aspects of the traditional religious system as they have shaped one another. She highlights the practicalities of conversion, for example, in how people accepted Christianity because their prayers for food during a famine were answered (p. 177), as well as the spiritual transformations of shamans and others. The introduction of Christianity—Presbyterian and later Seventh Day Adventist—brought both confusion and resolve in matters of life and death.

Jolles explains the *ramke* (clan) system not found in other Eskimo/Inuit societies and how courtship and marriage have changed within this system due to population loss, religion, television and other outside influences. She also takes us through the naming process, describing how names are alive and carried through new generations.

Strong gender roles limited Jolles access to men's activities, so the focus is on the lives of women; however, the whale hunt, polar bear hunts, and the walrus hunt clearly carry great social and nutritional importance to the *ramket* (pl.), the individual hunters and the community as a whole. She writes, "...although whaling is superimposed on all things, sacred and secular, walrus and seals feed people everyday" (p. 279), indicating the power behind whaling. Taking a polar bear or a whale accords great status to the hunter and the heights of the graveyard are reserved only for these hunters (p. 116, 221). It is also significant that boat captains were early converts to Christianity, seeking power and prestige (p. 85, 218).

*Faith, Food and Family* is part of a long tradition of modern Alaskan ethnographies following such figures as Chance and Fienup-Riordan, however it falls short in placing St. Lawrence Islanders within problems faced by many people of the arctic. Some of these limitations are due to agreed-upon topics as being out of bounds. Social problems are alluded to, but not fully considered within the religious framework that she has laid out. Though Jolles is clear about the goals of the book, I still expected environmental concerns surrounding pollution and climate change as well as oil and gas exploration and how Gambell and Savoonga villagers engage with corporations and governments to play a larger role than in the story Jolles tells. Though Jolles has written about the market economy in a 1997 *Arctic Anthropology* article, the uninitiated reader of her book is left wondering just how they earn a living on the island, given the undeniable need for cash. There is a nod to a transition from autonomy to dependency on a market economy (p. 11), but there is no dis-

cussion of wage work. She mentions baleen as a local currency (p.73), but does not fully explain its worth. The communities pay close attention to boys as “our food for tomorrow, you know, our hunters for tomorrow” (p. 163), so I would think that activities of the Eskimo Walrus Commission and the Eskimo Whaling Commission would be forefront in people’s minds.

Archaeologists might cringe at a favorite pastime of islanders’ digging for “ancestral treasure” described in Chapter 3. Jolles even lists the prices some pieces fetch in distant galleries. That description could have been situated in context a bit better. I would hope that some members of the Sivuqaq community see these activities as destroying their heritage, since the work of early archaeologists in which skeletal remains were removed from the island was described as “disrespect for their ancestors” (p. 58 n.9).

I would have liked to see some comparative reference to other ethnographies of whaling communities, as well as some seminal studies on religious conversion as some statements and conclusions leave the reader hanging. I also wonder what the Elder Advisor’s role was in preparing the manuscript because other than her direct quotes, the Elder’s contributions are unclear. Despite these criticisms, I enjoyed this book and found it to be an important contribution to arctic social sciences. Ultimately, it is wonderful to read about the lives of people in such a marginal place in an accessible, eloquent way.

# **ARCHAEOLOGY ON THE ALASKA PENINSULA: THE LEADER CREEK SITE AND ITS CONTEXT.**

By Don E. Dumond. *University of Oregon Anthropological Papers No. 60.* Eugene. 2003. 174 pages. \$14.00.

Reviewed by R. K. Harritt

Cultural Heritage Studies, Environment and Natural Resources Institute (ENRI), University of Alaska Anchorage.

This volume is the latest installment (at the time this review was prepared) in a series of reports generated by Dumond based on approximately 45 years of research on the northern Alaska Peninsula. The UOAP series was inaugurated 33 years ago with two volumes released in 1971; the second issue of that year, No. 2, is one of the first publications on the archaeology of the 'Katmai region.' Through the years, a hallmark of the UOAP series has been extensive presentations of research results, including considerable amounts of data accompanied by syntheses of new information with previous knowledge. Overall, a dozen of the reports in the series have been important, solid contributions to the prehistory of Alaska. The 2003 Leader Creek site report continues this tradition; it also contains an unusually large amount of history, that covers archaeological research in the area and late precontact through contact period history. The organization of the volume is unusual with regard to the series of plates that appear near the front, on pages 8-15, containing artifact sets with attributes that define the different cultural traditions of the Naknek drainage over the past 4,500 years. Students of the region's prehistory have seen these images before, most importantly in Dumond's 1971 monograph on his Naknek Region cultural sequence.

For those unfamiliar with northern Alaska Peninsula ethnohistory, it is important to note that the contact period history for the western side of the Aleutian Range is quite meager in contrast with that of areas such as Kodiak Island and the Aleutian archipelago. Among the reasons for this circumstance are the relative amount of attention given by early Russian colonists, whose interests were focused on acquisition of resources such as sea otter furs. In addition, the upper drainage was depopulated as a result of the 1912 eruption of the Mount Katmai volcano (but, compare Davis 1954:3 and Alaska Packers Association 1919:5-6). The very deep deposit of tephra that covered the area at that time also obscured remains of occupations that historic sources indicate were located on the Ukak River, and without a doubt other types of seasonal use locations have been deeply buried

as well. While this situation presents substantial challenges in reconstructing the behavioral characteristics of the contact period inhabitants of the upper drainage and nuances of their material culture, it is nevertheless excellent fodder for developing and refining archaeological methodology.

In 'Leader Creek,' the history of research is presented first by way of the volume introduction (pages vii-ix) then in earnest in the chapter 1 'Introduction and Background' (pages 2-30). This section is a history of the various projects that have been carried out over the past 50 years, and the issues of prehistory that have been addressed over the years are also described. Reviews of this type are rare in the literature on Alaska, but some parallels can be seen with Giddings' (1967) *Ancient Men of the Arctic*, which chronicles the main portion of his archaeological efforts in northwest Alaska. Dumond's review of Naknek research is considerably more limited in geographic scope, but it has a clear focus on the progress of the archaeology. The research history is therefore a generous gift to those who labor to learn the prehistory of the area, and to those who plan research for the future.

Among subjects addressed in chapter 1 is a brief comment concerning the limited physical anthropology conducted so far (page 19). Significantly, no mention is made of problems with the samples and Dumond's present review continues to gloss over those issues. It would be appropriate to acknowledge the deficiencies of the samples as they represent Naknek drainage inhabitants related to a specific phase of the prehistoric sequence; in prior treatments, the remains ascribed to the three prehistoric phases of the area (Camp, Bluffs, and Pavik) are lumped together as one population in some analyses (cf. Harritt 1997:51). This issue is especially germane to the discussions of ethnicity and social groupings in the area.

The contact period history (pages 20-25) is a welcome synthesis of what must be considered to be the

bulk of existing historic documentation for the area. Although this hunt has involved several researchers over the past five decades none have been in a better position than Dumond, who has reviewed all and sundry firsthand, to evaluate the import of the information. In this regard, the Leader Creek report contains what must be regarded as the definitive contact period history for the area. An obvious question related to progress in this area is 'why has it taken several decades to develop a contact period history for the Naknek drainage?' Undoubtedly an underlying reason is that the questions asked of archaeological data and history, which are posed with the initiation of the research and at succeeding points along the way, guide the development of knowledge. The lateness of the development of a definitive contact period history is an exquisite example of this process.

Early Naknek drainage research was concentrated on establishing the cultural sequence and the nature of the prehistoric human use of the area, a process which eventually subsumed Davis' (1954) initial work and resulted in Dumond's (1971, 1981) syntheses; subsequently, during the past two decades substantial new data were developed through investigations of prehistoric human social behavior organization and relationships. The new data also corroborated the established cultural sequence. The use of historic analogues was an integral component of my dissertation (cf. Harritt 1988, 2000), based on historic analogues that I used as a method of interpreting archaeological data. It has been stated as a set of postulates focused on whether a late prehistoric society inhabited the upper Naknek drainage during the Bluffs phase, AD 1450-1800, immediately prior to the persistent Russian presence to the east (Harritt 1988:99-106). This approach to late prehistoric archaeology on the northern Alaska Peninsula and elsewhere has been re-evaluated more than once (e.g., Harritt 1997, 2000, 2003), and in 'Leader Creek' (pages 89-90) Dumond again assesses the evidence for social boundaries in the Naknek drainage. It remains a useful tool.

It is important to point out that although the use of analogues has been directed toward proving the existence of such a societal entity in the area, it is not the sole objective of the exercise. In this regard, it is reasonable to assume that humans distributed across a landscape will organize themselves socially into definable groups. I believe that few anthropologists would reject an assertion that this is a propensity in human behavior. Substantial gains in understanding prehistoric human social behaviors can be made by developing ways to identify the archaeological signatures of prehistoric societies. Through this approach, the suites of artifact forms and attributes

that define the Bluffs and Pavik phases of the Naknek drainage can be analyzed and compared with those which can be used as examples of the ethnic essence of the late precontact and contact period Koniag of Kodiak Island. Did the late precontact Alutiiq-speaking inhabitants of both areas, who undoubtedly were organized as separate societies, share the same ethnohistory over the course of several centuries (ca. AD 1500-1800)? The answers to this line of inquiry are at the core of Dumond's 1998-1999 investigation of the form and attributes of prehistoric houses at Leader Creek specifically, and in the area in general (page 36). 'Leader Creek' builds on prior arguments by Dumond and others that Bluffs phase houses of the Naknek drainage were consistent with multi-room houses reported on Kodiak Island (e.g., Dumond 1994). Because the matter of house forms as a reflection of ethnic identity has figured prominently in recent literature of the area, the remainder of this review will concentrate on that subject, and leave consideration of other Bluffs phase attributes and data for another time.

The preamble to the latest Leader Creek investigation flows without break or pause in Chapter 2 (pages 31-42) from discussions, first of 'House Forms in the Brooks River Bluffs Phase,' then 'Distribution of Ethnic Groups,' then 'The Leader Creek Site,' to 'Strategy' to 'Fieldwork Summary, 1998-1999.' Chapter 3, 'Excavations,' presents results in the concise and clear fashion one expects from the author. House A and House B excavations are both illustrated in plan views and profiles and described. One quickly realizes that the approach used in the house excavations are strategically placed small excavation blocks, trenches and units, rather than complete block excavations of each. One cannot fault the investigator for not excavating the entire areas of each house, insofar as the sizes of the areas are daunting. By my rough estimate, the combined excavations carried out in House B, are distributed over an area of slightly under 53 square meters, of which more than 40% was excavated over a relatively brief period.

In the end, the investigation was successful in most cases in establishing clear relations between the side rooms and main rooms of both House A and House B. A possible exception is the treatment of excavation unit 'C-1,' illustrated in the report in Figures 3.2, 3.4, and 5.1, and in the discussions on pages 43-56. In Dumond's final evaluation of the clay-lined pit features uncovered in 'C-1,' he proposes (page 77) that it and the similar feature in 'C-2' represent small separated structures covering sets of clay-lined pits that were not connected to either of the houses. The C-1 clay-lined pits lay above what is presumed to be the interior end of the entry tunnel to House A (pages 48-

49), even though the excavation did not accomplish complete excavation of the remaining segment of the tunnel. The unexcavated strips between the main room of House A and the C-1 excavation present the reader with a modicum of uncertainty about whether unquestionable direct evidence of their physical relationships has been obtained. The same is true for C-1 and the main room of House B (Figure 5.1). And, while we know that the C-1 pits are younger than House A, we apparently don't have its documented stratigraphic relationship to House B. The relative chronological positions of House A and House B are indicated by way of a suite of radiocarbon dates, however (pages 63-67). Therein, Dumond nevertheless suggests that House B was occupied only approximately 100 years later than House A. One cannot help but wonder if this assessment of relative ages is overly precise, given the number of variables associated with any radiocarbon date. In this respect, the spatial organizations of the side rooms of the two houses illustrated in Figure 6.1 are laid out in a manner that indicates mutual accommodation of the side room locations for each house, thereby suggesting contemporaneity of the two dwellings. It is noted that my speculation here assumes that the projected configurations are related to some unknown degree to the buried physical remains.

Along slightly different lines, I encountered an incongruence concerning the relationships between the unexcavated House A surface depression illustrated by Dumond in his Figure 3.1, and the projected forms and dimensions of House A elements illustrated in Dumond's Figure 3.2. It is not clear what the 'projected' outlines of the features are based upon, and no explanation is provided concerning how the projected dimensions and forms presented were developed. As an exercise to determine which elements of House A, visible on the surface, were actually exposed in the excavations, I modified Dumond's Figure 3.2 by eliminating the 'Projected' dashed lines from the plan map. There appears to be good reason for uncertainty about what the projected outlines and dimensions represent in some cases, both with respect to the surface depression and to the portions of some of the house elements that were excavated, specifically, side rooms A 4, A 5, and as noted, the entry tunnel below C 1. I am therefore less than completely satisfied that the configurations and dimensions of multi-room houses at Leader Creek have been established. But it is nevertheless now convincing that multi-room houses are one important aspect of the Bluffs phase in the Naknek drainage. But other configurations also occurred, as Dumond qualifies his argument for the pervasiveness of the multi-room house in the Bluffs phase with a suggestion that single room houses occurred during this time as well (page 88).

It therefore appears that as a result of the recent discourse and investigations of Bluffs phase house remains on the northern Alaska Peninsula, both multi-room and single-room Bluffs phase configurations are now established. This circumstance ironically supports both sides of the prior argument.

However, Bluffs phase house forms still have not been adequately characterized at the present stage of research and many questions remain that beg further investigation, in a fashion in which the attributes and configurations are evaluated in systematic ways. In this respect, Dumond's comments (pages 83, 88-89) that the modal Bluffs phase house plan was multi-room of a Leader Creek type therefore seem premature, considering the range of variations illustrated in 'Leader Creek' in Figures 2.2, 2.3, 6.3, 6.4 from the Alaska Peninsula, and a house plan from Kodiak Island, in Figure 6.5 (see also, Harritt 1997: Figure 3).

As suggested previously in this review, the questions asked of the archaeological data and history shape new knowledge to a substantial degree. In this respect it is clear that existing knowledge could be considerably advanced from new investigations of the Old Savonoski site or perhaps at other settlement sites located along the outlets of the Colville Lake and Grosvenor Lake in the upper Naknek drainage (Harritt 1997: Figure 4; Figure 1 in this review). Old Savonoski contains both multi-room and single-room houses, a circumstance that Davis (1954:64-65) suggests, *de facto*, reflects changes that occurred in house configurations over time. In fact, Davis' (loc cit.) comments seem remarkably germane to the present discussion:

*Two dwelling types are represented at [Old] Savonoski, a modern, e.g. post-contact, single room, semi-subterranean house and an older, multi-roomed semi-subterranean structure. Depressions left by the latter occur throughout the length of the site. These depressions are oval in appearance, but enough of their outlines remain to indicate that the rooms were roughly rectangular. The floor plan shows a square central room (roughly three by four meters or a little larger) with one or more entrances from the river side, and having two or more rectangular rooms opening off of it at random intervals...The single-room modern house was in use at the time of the Katmai eruption. Fifteen of these houses are situated in a row along the river bank at the modern village site...*

By all accounts the site was a residence for elements of the society inhabiting the area during late precontact and contact period times up to the time of the Katmai eruption in 1912 (e.g., Harritt 1997, 2003, with references). Under these circumstances, rather obvious questions could be posed concerning the cultural remains and history of Old Savonoski, such as:

1. What is the age range for the multi-room houses at the site, and concomitantly, what is the age range for the single-room houses at the site?
2. Do the multi-room houses reflect a detectable design template for their configurations and other attributes?
3. Assuming that a succession from multi-room to single-room house forms is indicated by the archaeological data, does the transition coincide with the arrival of Russian Orthodoxy in the Naknek drainage?

Clearly, many other questions could be posed as well that would illuminate the ethnohistorical significance and evolution of house forms across the northern Alaska Peninsula.

In returning to 'Leader Creek' I will note a few small quibbles with the volume. In Figure 5.1, feature C 2 is mislabeled as C 3, and in Figure 6.1, feature C 1 is presented as a part of House A instead of the free-standing, covered grouping of clay-lined pits described on page 77. The footnote at the bottom of page 104 states that "Harritt (1997) maps dialects somewhat differently, showing distinct dialects in northern and southern Kodiak...." Although I am not certain about the reference in this case, I will assume that the remarks in question are:

*"The late prehistoric Pacific Eskimo can be divided into regional groups on the basis of historically documented dialectal differences in some cases and socioterritorial designations in others ... For example, at least two regional Sugpiak-speaking groups occupied the northeastern and southwestern ends of Kodiak Island...."* (Harritt 1997:56).

It is important to note that my comments concerning Kodiak Island are not new, but follow closely statements made previously by Clark (1964:118). The caption for the map that accompanies the text (Harritt 1997:Figure 2) states, "Documented and postulated Late Prehistoric re-

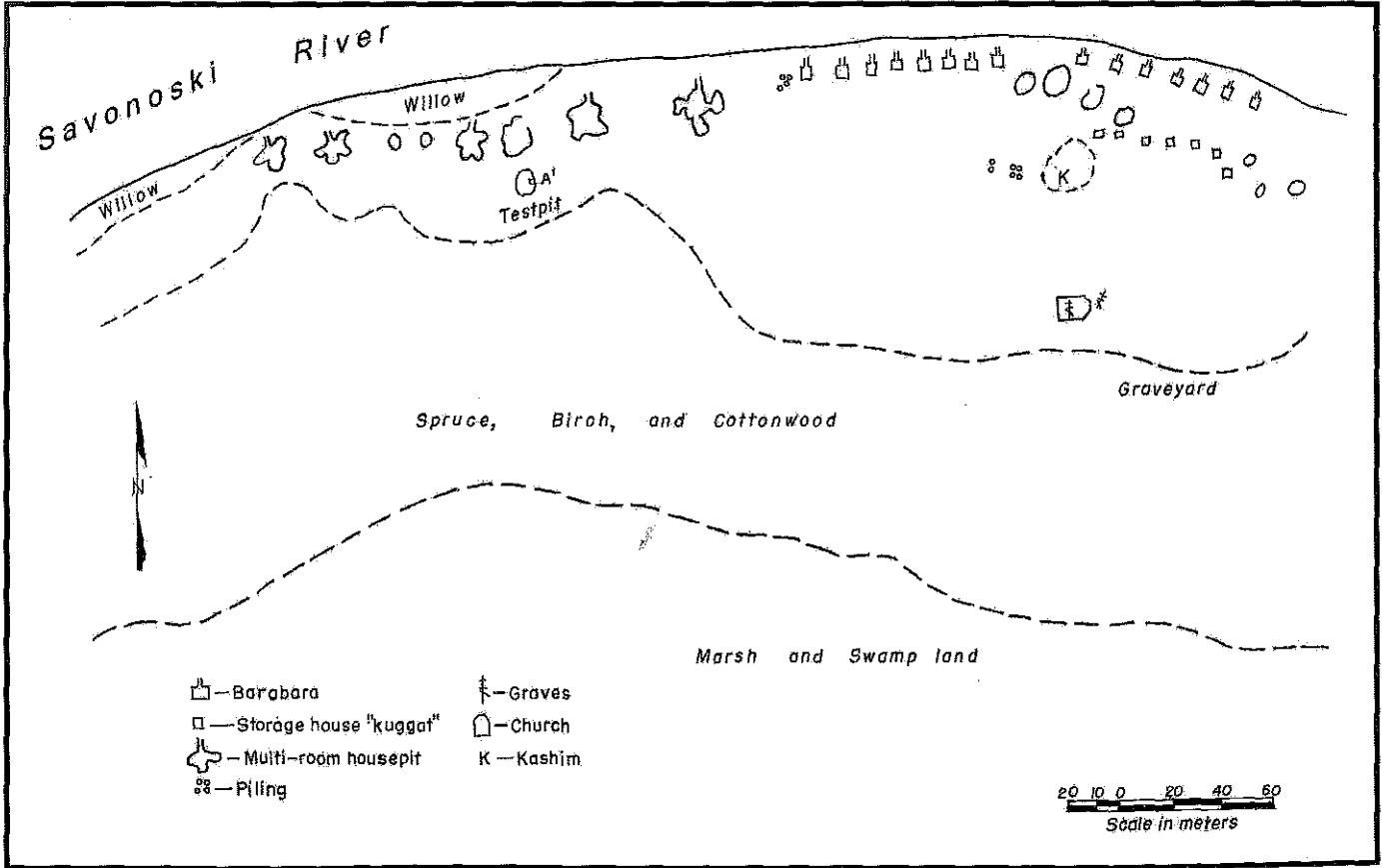


Figure 1. Plan Map of the Old Savonoski Site (XMK-002) Showing Both Multi-Room and Single-Room House Depressions (after Davis 1954:Map 5).

gional Eskimo groups in Southern Alaska....” In my opinion, although the reference to dialects in the footnote is incorrect, the gist of it is in agreement with the suggested divisions described in the 1997 passage.

The final chapter of the volume (pages 103-110) consists of discourse on the (pre) historic relations between the Alaska Peninsula and Kodiak Island, prehistoric demography, geography and cultural diversity, a recapitulation of the significance of multi-room houses, and a final conclusion. Much of the content of this section has appeared previously in the literature (e.g., Clark 1984, 1988; Dumond 1987; Jordan and Knecht 1988), but is updated in the present volume to reflect a shift in the direction of cultural influences across the Alaska Peninsula that appears to coincide with a major volcanic eruption, ca. 500 years ago. During the Bluffs phase period that ensued, the conclusion suggests in essence that the inhabitants of the Naknek drainage were under the sway of a culture centered on Kodiak Island.

In addition to the main body of the report, a total of three appendices are included:

Appendix A, pages 113-134, summarizes the artifact collections from the 1998-1999 effort in tabular form and includes descriptions of artifacts recovered from the Leader Creek site. Table A.1 provides a cross reference of Bluffs phase artifact classes from Dumond’s 1981 monograph, my initial report (Harritt 1988), and from the most recent effort by Dumond. Appendix A, therefore will be of considerable aid for future analyses.

In Appendix B (pages 135-140), the analysis of the faunal collections from 1998-1999 by Robert Losey, is presented in a direct, straightforward fashion. An evaluation of the seasons represented by the various species, normally perfunctory in this type of analysis is not presented by the faunal analyst. A secondary evaluation of the analysis is presented by Dumond in the main body of the report (pages 59-61) as a part of the review of the 1998-1999 collections.

Appendix C (pages 141-164), an analysis of Leader Creek site geomorphology by Jared Erickson, is abstracted from his University of Oregon Master’s thesis. No citation is given by Erickson or Dumond in the main volume bibliography for the Master’s thesis — should one wish to see the original report, alternative sources must be used. A footnote on page 141 states that Erickson’s original analysis was altered to correspond with important points Dumond makes elsewhere in the volume, and on page 164 a footnote indicates that a section from the origi-

nal thesis that treats changes in climate regimes is excluded. Nevertheless, the presentation in the present UOAP volume clearly reflects extensive background research (pages 143-145), including an abbreviated vegetation history, before getting down to cases of archaeological relevance. The analysis itself is thorough and professional and should be useful as a reference for the major processes that affected the landforms of the area.

On the whole, and the critical comments of this review aside, Dumond’s latest analysis of the late prehistory of the northern Alaska Peninsula should be required reading for students of southwest Alaska prehistory. It is a rare thing indeed for an archaeologist to sustain interest and active research in the same geographic area over a period of 45 years, during which fundamental changes in the discipline have occurred. Dumond’s contributions to Northern prehistory over the course of his long career are legendary, and the 2003 Leader Creek report is once again an achievement in further illuminating prehistory. I encourage Northern specialists to read it, and buy the complete set.

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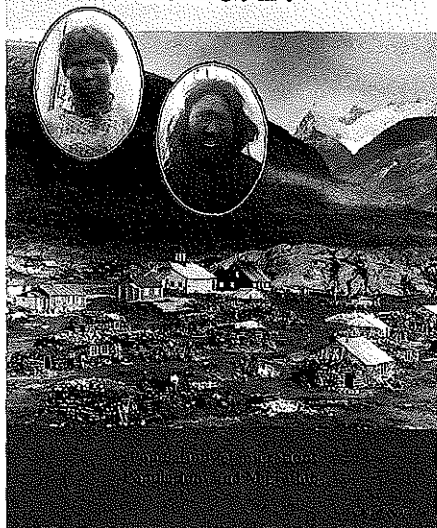
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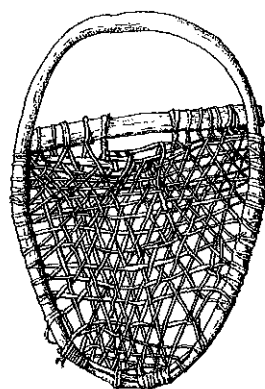
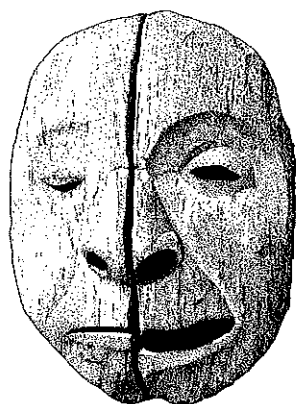
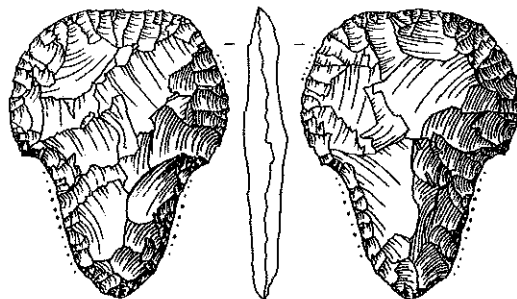
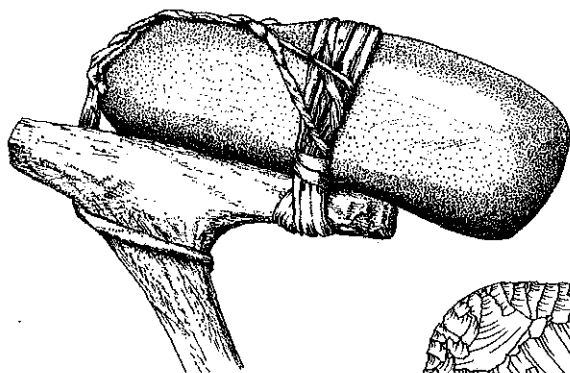
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