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Makahs, Quileutes, and the Precontact History of the Northwestern Olympic Peninsula, Washington

Gary C. Wessen

Abstract *Variations on the related ideas that: (a) the Makah people arrived on the northwestern Olympic Peninsula of Washington as recently as 1,000 years ago and (b) they displaced Quileute people who had previously held those lands have appeared on a few occasions during the last century. As offered, such claims rely heavily on ethnographic and linguistic arguments. A detailed examination shows that all of these arguments are flawed. The currently available archaeological data is not sufficient to address these ideas in an unequivocal way, but may still offer relevant insights. Doing so, however, requires some ability to recognize these groups in the archaeological record; this possibility is explored using the artifact and faunal assemblages from this region. Preliminary findings suggest that Makahs and/or other Wakashan speakers have been present for at least 3,000 to 4,000 years and that there is no credible evidence for an earlier presence of Quileutes and/or other Chimakuan speakers.*

Introduction

Claims that the Makah people crossed from western Vancouver Island to the northwestern Olympic Peninsula of Washington as recently as 1,000 years ago, and that they displaced Quileute people who had previously held those lands, have appeared in the anthropological literature on a few occasions during the last century. A case for these ideas has not appeared in a refereed anthropological journal in more than forty years and the ideas have never been widely accepted, yet their advocates have been presenting them as evidence in treaty rights trials and other administrative hearings as though they were established facts. Thus far, archaeological data has played only a limited role in the debate and, given the appearance of a considerable amount of new data during the last forty years, a further look is appropriate.

Background

This paper examines some existing ideas about the late precontact culture history of the northwestern Olympic Peninsula of Washington (Figure 1). The area under consideration includes the coastal margins of the traditional territories of two neighboring groups: the Makah and Quileute peoples. For the most part, it consists of exposed, steep, rocky shorelines marked by numerous nearshore rocks and small islands (Figures 2 and 3). Low, sandy beaches occur in some places, but they are often small. Terrestrial surfaces farther inland are mostly steep and heavily forested. On the north, a few small coastal river valleys break up this pattern. Farther south, the much larger Quillayute and Hoh Rivers reach the ocean on relatively broad alluvial flats.

Figure 1. The northwestern Olympic Peninsula of Washington. Black lines indicate relatively recent descriptions of the western boundary between Makah and Quileute territory.

Figure 2. The mouth of the Waatch River near Cape Flattery on the northwestern Olympic Peninsula. View is to the northeast.

Figure 3. Toleak Point on the northwestern Olympic Peninsula. View is to the east.

The Makah are the southernmost of a large native group who spoke Wakashan Languages (Renker and Gunther 1990). All other Wakashan speakers have traditional territories on Vancouver Island or elsewhere on the mainland of central British Columbia. In contrast, the Quileute—sometimes spelled Quillayute—are one of only two small groups who spoke Chimakuan Languages (Powell 1990). The nearby Hoh people are here considered to be associated with the Quileute and the other group of Chimakuan speakers—the Chimakum people—who formerly held territory on the northeastern Olympic Peninsula. While some linguistic research has suggested a possible remote relationship between the Wakashan and Chimakuan Language Families, this association remains a matter of speculation (Swadesh 1953; Foster 1996).

Following these groups in historic records is sometimes complicated by confusion associated with different early historic group names and what the names refer to. Among Wakashan speakers, archaeological, ethnographic, and linguistic studies all indicate a close historical relationship among the Makah, Ditidaht, and Nuu-chah-nulth (McMillan 1999). In early historic accounts, Makahs are sometimes referred to as the "*Classet*" (Gibbs 1854:35) or "*Kwe-nḗt-che-chat*" (Swan 1870). Similarly, the native people from southwestern Vancouver Island—the Ditidaht—are often referred to in older documents as the "*Nitinat*" or "*Nittinat*" (Irving 1921). At least a dozen additional groups speaking Wakashan Languages to the north of the Ditidaht are now collectively referred to as the Nuu-chah-nulth people. Older documents often used the terms Nootka or Nootkan for some or all of these groups. The situation is simpler for the Quileute as most early historic names for then are simply various spelling of this term.

Given the territorial focus and the interest in the ethnic identities of the peoples in this landscape, it is worthwhile to briefly consider what is meant by the traditional territories of the Makah and Quileute peoples. In broad terms, traditional Makah lands include the Cape Flattery area and southward along the Pacific Coast beyond Cape Alava. To the east, they held the southern shoreline of the Strait of Juan de Fuca at least as far as the Hoko River. In total, their territory includes only a few relatively small watersheds. In the nineteenth century, all of the Makah settlements were located on, or close to, marine beaches. Important Makah communities were located at Neah Bay, Waatch, Tsooes, Ozette, and on Tatoosh Island. Traditional Quileute territory is located to the south of the Makah. It includes the large Quillayute River watershed and the marine shoreline to its west. Important Quileute communities were located at La Push, Toleak Point, and Goodman Creek on the coast and at numerous locations along the Quillayute, Dickey, Soleduck, Calawah, and Bogachiel Rivers. Powell's (1997:29–30) analysis of their likely late precontact demography suggested that as much as 70% of the Quileute population was associated with upriver, rather than coastal, settlements.

It should be acknowledged, however, that the location of the boundary between the two territories has long been a matter of dispute. In 1956, Verne Ray produced a summary discussion which includes fourteen different maps and twenty narrative accounts of this boundary (Ray 1956). Many of the documents in both groups were produced in the nineteenth century and reflect both uncertainties about the geography as well as the group boundaries. Boundaries on the coast from as far north as Point of the Arches to as far south as Cape Johnson have been reported. Most recent accounts, however, place it to the south of Sand Point and more recent documents (e.g., Spier 1936; the Quileute Tribe's 1954 Indian Claims Commission filing [Claim No. 155, Exhibit 73]; Powell 1990) place the northern limit of the Quileute's coastal presence approximately four to six miles south of Sand Point (Figure 1). The present discussion uses this boundary.

The Arguments

The claim that Makahs arrived on the northwestern Olympic Peninsula from western Vancouver Island quite recently, and that what is now traditional Makah territory was formerly held by Quileutes, has been argued using both archaeological and linguistic data. All such arguments have significant problems. Only a few claims cite archaeological data. These are relatively simple arguments, but suffer from supporting documentation issues. Linguistic arguments are more complex, but often rely on assertions which are either inaccurate or incomplete.

Archaeological Arguments

The earliest presentation of the claim that the Makahs are relatively recent arrivals on the northwestern Olympic Peninsula and that they displaced Quileutes appears to be in Reagan (1917). Albert Reagan was a school teacher at La Push between 1905 and 1909 and wrote some of the earliest accounts of archaeological resources in this region. Reagan reported on shell midden sites in what he called the "Ozette-Makah Region" and the "Quillayute Region" just to the south of it. Central to these accounts are interpretations of the culture history of each region, based upon his observations of the depositional structure of these sites.

Reagan (1917) reported that archaeological sites in the Ozette-Makah Region contain four distinct temporal units which he referred to as: Recent, Old, Very Old, and Ancient. The Recent deposits are historic and the Old deposits immediately beneath them are very similar in content except that they lack historic trade materials. With respect to artifacts, Reagan said: "These remains, as with the Recent, contain many stone implements, such as hammers, chisels, knives, daggers, etc. They also contain stone effigies, totems and other household ornaments and curios, all distinctly Makah in make." The Very Old deposits underlie the Old deposits and are distinguishable from the latter "by the lack of stone implements, effigies, totems and other stone curios." Of these, Reagan adds,

> They resemble the older remains at La Push and were likely made by the Quillayutes at the time when they occupied the whole of the Olympic Peninsula west and north of the mountains.[1] These remains, which are mostly middens, show no intermixture with the Indians to the north who used stone implements and who made their household gods, effigies, and totems out of stone. The Quillayutes were not a stone-implement making people; hence the conclusion that they made the Very Old midden remains in this section.

[1] Reagan's claim that the Quileutes formerly occupied the whole of the Olympic Peninsula west and north of the mountains is based solely upon the traditional Quileute story which he referred to, but didn't actually present in 1917. No other evidence is offered in support of this claim. See below for further discussion of this point.

Finally, the Ancient deposits are described as very similar to the Very Old deposits except that "stone implements are plentiful." Reagan said that: "These seem to indicate an invasion of the region from the north, the invaders being later driven out by the Quillayutes; or probably they were the first people in the region and they were dispossessed by the latter." Unfortunately, he did not offer any insights regarding what types of "stone implements" are present nor why he thought that they were indicative of people from the north.

Reagan's proposed culture history for the Quillayute Region is similar, but includes only three units: Recent, Old, and Ancient. Once again, the Recent deposits are historic and the Old deposits immediately beneath them are very similar in content, except that the latter lack historic trade goods. Reagan provides few details about the Old deposits and acknowledges that some have been badly disturbed by historic activity. The Ancient deposits underlie the Old deposits at some locations and may be present elsewhere without associated younger materials. Reagan offers little information about the artifacts associated with each type and no specific details about assemblage differences among them. Rather, he comments, "the relics found in the middens are few in number, and but few of them are stone." He does say that the Quillayutes made arrowheads of agate, but this is the only stone tool he specifically attributes to them. For the most part, the artifacts associated with all of these units are objects manufactured of bone, antler, or marine shell. In his concluding remarks, he states, "it seems, from the available evidence at hand, that the archaeological remains were made by the same race that now occupies the region. This opinion is also strengthened by the fact that the Quillayutes have no tradition of having migrated from any other place."

The extent to which Reagan's ideas are consistent with the archaeological data now available will be considered in the second half of this paper, but it is useful here to summarize specifically what he claimed. He clearly attributed all of the then-known archaeological sites in the Quillayute Region to the Quileute people. He had no real idea about how much time was represented by these sites, but accepted the Quileute's story that they have been present for a considerable period of time. Reagan's ideas about the Ozette-Makah Region are more complex. He attributed the Recent and Old middens to the Makah people and, since the Old middens often suggested "considerable age," this implied that they had been present for a while as well. Reagan clearly did place the Quileutes in the Ozette-Makah Region before the Makah presence, but this assignment is based upon his claim that an archaeological assemblage lacking stone tools could be attributed to the Quileutes. Note, however, that his discussion of the site's contents contradicts this claim. Finally, note that the Quileutes were not the first people on the northwestern Olympic Peninsula. He reports the presence of a still earlier group in the area prior to them; possibly a group which arrived from the north.

Reagan's ideas about the precontact history of this area have rarely been cited or directly examined by archaeologists, but at least one relatively recent discussion also suggests a possible recent arrival of Makahs from Vancouver Island. In a consideration of the prehistory of the Olympic Peninsula from the perspective of "Land Use Systems," Schalk (1988:116–120) suggests that maritime collecting systems (i.e., the sophisticated offshore fishing and hunting technologies of the early historic Makah and other Wakashan speakers) first appear on the northwestern Olympic Peninsula approximately 1,000 to 1,500 years ago. Noting the clear linguistic ties between the Makah and their Wakashan-speaking relatives, he comments, "it should be recognized that the emergence of the maritime collecting system may have occurred through a process of colonization from the north."

Linguistic Arguments

The modern view of a recent arrival of Makahs, displacing the original occupation by Quileutes, is based largely upon discussion presented by Dale Kinkade and Jay Powell (1976). This paper addresses how linguistics may inform studies of the prehistory of North America and—as a part of it—the authors provide a brief example focusing on the northwestern Olympic Peninsula. In doing so, they state, "A great deal of evidence suggests that the entire northern Olympic Peninsula was originally controlled by Chimakuan peoples. The time depth of Nootkan occupation of the northwest tip of the peninsula cannot be determined with accuracy, but an estimate based on linguistic evidence places it at approximately a millennium." In making their case, they specifically consider three types of language data which: "allow us to conclude that Chimakuan peoples originally controlled the northern end of the Olympic Peninsula." The three types of data they consider are: place-names, Chimakuan comparative linguistics, and mythic and legendary corpora.

Kinkade and Powell (1976:95) claim that, "A number of significant features of the littoral included within traditional Makah territory have Chimakuan place-names which are used by both the Quileute and Makah." In support of this statement, they present a list of words which they describe as Chimakuan place-names for locations in traditional Makah territory. Having done so, they say, "the explanation for the existence of Chimakuan place-names for such important features of the Makah landscape appears to be prior Chimakuan occupation, with their names continuing in use among the Nootkan community which displaced them." Both the specific place-name evidence they offer and the larger question of the use of place-name evidence to infer group territories are worth further consideration.

First, it is important to note that the place-name evidence provided by Kinkade and Powell is very limited. In total, they consider eight words; including one that the authors acknowledge "must be reconstructed to be seen as acceptable Chimakuan," and another which they suggest is derived from a Chinook Jargon term and therefore could be quite recent. In contrast, a Makah Traditional Cultural Property Study prepared by Ann Renker and Maria Pasqua (1989) lists more than 250 place-names in their territory, the great majority of which are associated with shoreline or nearshore features.

A comparison of the place-names provided by Kinkade and Powell with the list prepared by Renker and Pasqua shows two names which appear on both lists and are translated in each list as having a similar meaning. Five additional place-names provided by Kinkade and Powell also appear to be on the Renker and Pasqua list, but the two lists give very different translations for each of them. Renker (personal communication), in fact, rejects the Kinkade and Powell claim that these are even Chimakuan words. I readily acknowledge that I am not a linguist and that I do not have an independent opinion regarding whether these place-names are—or are not—Chimakuan words. However, it is clear that the Kinkade and Powell analysis is based upon a very small sample of the existing Makah place-names, and that there is no consensus view on whether or not they are Chimakuan words, or even what most of the cited words mean.

A bigger problem here is the underlying assumption that traditional place-names imply something about the territories of the peoples who use them. While I am confident that traditional place-names may imply something about the territories of the peoples who use them, this relationship is neither simple nor direct. Kinkade and Powell argue that because a few Chimakuan place-names are present in traditional Makah territory, this is evidence that it was formerly the territory of the Quileutes or other Chimakuan peoples. Assuming that these really are Chimakuan words, I suggest that this is neither the only—nor even the most reasonable—explanation for them. Rather, I suggest that traditional cultures never restricted their use of place-names exclusively to their own territories. Evidence of this can be seen in place-name lists for both Makahs and the Quileutes. Powell and Jensen's (1976) list of Quileute place-names includes names for such places as the City of Seattle, the mouth of the Columbia River, Canada, and California. None of the latter are in Quileute territory nor is it likely that any were formerly the territory of the Quileutes or any other Chimakuan peoples. Similarly, the Renker and Pasqua list of Makah place-names includes names for such places as Vancouver Island and the Cities of Victoria and Port Angeles. None of the latter are in Makah territory.

In sum, the place-name data provided by Kinkade and Powell is, at best, weak evidence that traditional Makah territory was formerly held by Quileutes or any other Chimakuan speaking group. While arguably consistent with the claim of a prior Chimakuan presence, it is not actually proof. Moreover, to the extent that such a claim is supported by these place-names, the argument is silent regarding whether Chimakuans were the original occupants of the northwestern Olympic Peninsula or simply the more recent of at least two prior occupants.

Another type of language data cited by Kinkade and Powell (1976) is what they refer to as: Chimakuan comparative linguistics. They briefly compare the Quileute Language to Chimakum, the only other known Chimakuan Language. They note some differences between the two which "reveals that the two languages had been separated long enough for phonological, grammatical and lexical innovations to appear." Despite this, they add that Quileute elders say that the two languages were, to some extent, mutually intelligible and then they conclude: "comparative linguistics, while providing us no specifiable timetable of divergence for Quileute and Chimakum, allows us to decide that the two languages could have separated approximately one to two millennia ago, but little more than that." To this, however, they add: "it is altogether possible that the Chimakuans split long before the arrival of the Nootkans, with Quileute and Chimakum representing the ends of a continuum of Chimakuan communities or neighbors who were forced further apart when the newcomers settled between them." Note that the authors acknowledge that the Chimakuans could have split for reasons which had nothing to do with the Makahs. If the separation occurred for reasons unrelated to the Makah arrival, there is no basis to suggest a temporal relationship between these events. Finally, if Makahs had anything to do with the early historic distributions of the Quileute and Chimakum, they can be no more than a part of the story as historic Makah territory does not actually separate the latter two groups (Spier 1936). Thus, no real inference about the Makah arrival can be drawn from this discussion.

More recently, Powell added another linguistic argument to the claim that Makahs are recent arrivals. Powell (2015:18) says: "Jacobsen (1979:776) reports that Makah separated from their relatives at Vancouver Island approximately 1,000 years ago." This is not an accurate description of Jacobsen's statement. Jacobsen (1979:776) says: "Makah is a separate language from Nitinat, with the time depth separating them apparently being in the vicinity of 1,000 years." He was not referring to the movement of people, he was discussing when the Makah Language was first discernable from the Nitinat Language. No discussion of the movement of people from Vancouver Island to the Olympic Peninsula appears in Jacobsen (1979:776). Further, no basis for the temporal estimate is provided. It appears to have an intuitive—rather than analytical—origin. Indeed, Jacobsen used the qualifying word "apparently" when referring to it. Powell's argument here is essentially that the Makah people began to speak a different language when, or very shortly after, they arrived from Vancouver Island. This is unlikely. Rather than an abrupt change in language co-incident with their move, I suggest that the change is more likely to have developed after they were physically separated from their Wakashan-speaking relatives on Vancouver Island for some period of time. Recall that Kinkade and Powell argued earlier that differences between the Quileute and Chimakum Languages could have taken as much as 2,000 years to develop, after the groups were physically separated. Thus, if the Makah Language first became distinct from the Nitinat Language approximately 1,000 years ago, it is more reasonable to suggest that this is evidence that their presence on the Olympic Peninsula significantly predates this time.

The last type of evidence cited by Kinkade and Powell (1976) is what they refer to as: "mythic and legendary corpora." This term is unfortunate as the word "mythic" suggests that the stories may not be true. In fact, they clearly present them as documenting events which actually occurred (i.e., as history). I believe that this type of information is better described as "oral-historical evidence." Kinkade and Powell cite two traditional stories which they say document the recent Makah arrival on the northwestern Olympic Peninsula. They acknowledge that the Makah also have a story which says that they have always been here, but they emphasize the former two. One is a specific reference to a published story, the second is a more generalized reference without a specific citation.

The specific reference to a story is *How the Makah Obtained Possession of Cape Flattery* (Irving 1921). Kinkade and Powell (1976:97) tell us that in this story: "Neah Bay was first owned (i.e., claimed, not necessarily settled) by the Nitinat, along with Tatoosh Island and several camp-sites." After a contest between a Makah individual and a Nitinat ended in a dispute, the Makah "went to war against the Nitinats and captured the present site of Neah Bay and Tatoosh Island." Kinkade and Powell offer no estimate of how old this story might be. Their discussion of it, however, fails to mention some important details. This story clearly and unequivocally states that the Makahs who went to war against the Nitinats were from "Waatcht, Sooes, and Ozette" (Irving 1921:6); all Makah villages on the northwestern Olympic Peninsula. Thus, this story is not about a Makah arrival on the Olympic Peninsula; Makahs were already present when the events in this story occurred.

A second detail which Kinkade and Powell fail to comment on is that the people who held Neah Bay and Tatoosh Island at the beginning of this story were Nitinats, not Quileutes or other Chimakuan peoples. In fact, this story offers no basis to suggest that either Quileutes or other Chimakuan speakers were present anywhere in Makah territory when these events occurred.

The second story Kinkade and Powell consider is one in which a Nitinat princess gave birth to a litter of dog-children and was forced to leave her home on Vancouver Island. After arriving in Neah Bay, she discovers that they are really normal children in dog suits and they subsequently become the Makah people. The authors acknowledge that the story has numerous variations, but don't specifically cite any of them. They add that the variations occur mostly in Quileute and Clallam communities and suggest that this indicates that these neighboring groups consider: "the Makah to be newcomers to the Neah Bay area." Kinkade and Powell conclude this discussion by saying: "Such mythic evidence suggests that the Makah moved to the Cape Flattery area and that this migration occurred long enough ago to have become factually dim, but has not eroded completely from folkloric memory." While this story is about a connection between the Makah and a Vancouver Island Wakashan-speaking group, it is not as simple a story, nor does it have the same meaning, as these authors suggest. It says that the ancestors of the Makah people came from Vancouver Island, but it also clearly indicates that they did not become a distinct community until after they arrived on the Olympic Peninsula. Thus, Makah creation stories which say they have always been on the Olympic Peninsula are accurate because a self-identified Makah community never existed on Vancouver Island. While no basis for dating the story exists, this is an older story than the previous one. This is a story about how the Makahs became the Makahs; the previous story occurs at a time when they already are Makahs. Like the latter, this story offers no basis to conclude that Quileutes or other Chimakuan peoples were present at Neah Bay—or anywhere else in traditional Makah territory—when these events occurred.

The only other oral-historical argument for a recent arrival of Makahs which displaced an earlier Quileute population of which I am aware was also presented by Powell, in a legal filing (2015:18) rather than an anthropological publication. Here, he followed a brief reiteration of the Kinkade and Powell (1976) arguments by adding: "Swan (1870:58) reports that the Makahs arrived in Neah Bay 12 generations ago, approximately the year 1550." Note that an event that was described as having "occurred long enough ago to have become factually dim" and estimated to be about 1,000 years ago in 1976 is now suggested to have occurred less than 500 years ago. In fact, this single sentence statement significantly misrepresents Swan's remarks.

The cited remarks begin with Swan saying: "The only genealogical record that has been related to me is one commencing twelve generations ago, beginning with Deeaht and his brother Obiee or Odiee." Swan never describes Deeaht, his brother, or any other Makah person as having just arrived from Vancouver Island. Rather, the account of Deeaht says that, in his (Deeaht's) time, "the Nittinats came over with a mighty host and attacked the Makahs, driving them away from all of their villages, and forcing them to retire to their strongholds at Flattery Rocks." Deeaht was killed in this struggle, but his brother was ultimately able to expel the invaders and restore Makah control over the Cape Flattery area. The story of

Deeaht and the genealogical record which begins with him is not about a Makah arrival on the Olympic Peninsula. They were clearly already present at the time these events occurred.

It is important to add that the discussions of oral history offered by Kinkade and Powell (1976) and Powell (2015) focus almost exclusively on Makahs. They present very little information about Quileute oral history. The only comment Kinkade and Powell offer on this subject is to cite a Quileute creation myth which "suggests that the tribe was created on the Peninsula where they continue to live." The cited story—*The Origin of the Tribes* (Andrade 1931:85)—credits kwá·ti with creating the Quileute people at La Push. A number of other relevant details in this story are, however, not mentioned by Kinkade and Powell. The story describes Kwá·ti traveling north along the west coast of the Olympic Peninsula and stopping at a number of clearly identified locations. Kwá·ti encounters people already living at Hoh, Ozette, and Neah Bay, but finding no one living at La Push, he then created the Quileutes. Nothing in this story specifically addresses a Makah arrival on the Olympic Peninsula nor suggests that Quileute people occupied those lands before Makahs arrived. In fact, this story suggests that the Quileutes themselves may have been among the more recent people to appear on the coast. Another story in the same collection which Kinkade and Powell do not discuss is *The Separation of the Quileute and the Chimakum* (Andrade 1931:200). While they suggested that the separation may have occurred "when the newcomers settled between them," this story makes no mention of arriving Makahs or anyone else. Rather, the separation is attributed to a flood event. In fact, neither Kinkade and Powell (1976) nor Powell (2015) actually cite a Quileute story which places them on the northwestern Olympic Peninsula before the arrival of Makahs.

Yet another account of Quileute history ignored by Kinkade and Powell is in manuscript materials prepared by Leo Frachtenberg, who worked in La Push shortly after Albert Reagan. This is a brief text—dated August 1916—entitled *Early History and Distribution* by Quileute Tribe member Arthur Howeattle. In it, he states, "Quileutes lived in former times on James Island, a small island near the mouth of the river. This was long before the coming of the white people. According to tradition, q!wä′ti created the world, he put the Quileutes on this island and they have lived there since immortal times, winter and summer." This account closely parallels the story recorded by Andrade. Nothing in it suggests that the Quileutes once held a much larger territory which included the northwestern Olympic Peninsula and it is completely silent on the subject of when—or even if—the Makahs arrived.

Finally, I noted earlier that Albert Reagan (1917:20) made a passing reference to a Quileute story about a time when they occupied a much larger portion of the Olympic Peninsula. Reagan did not explain the comment in 1917, but a collection of Quileute and Hoh stories he published later appears to contain it. The story *The Battle of Chimakum* begins by stating,

> We were once a powerful people and had possession of the Quillayute and Hoh rivers and all the rivers that flow into them. Our women also gathered fern roots from all the prairies of the region. Not only that, but our possessions extended over the Clallam mountains to the north to the long water that goes out to meet the big water towards

> the setting sun. Moreover, along that water our possessions stretched from the mouth of the Hoko River to Chimakum, a distance of three long days' canoe journey. (Reagan 1929:186)

While this story does report a much expanded former Quileute presence on the Olympic Peninsula, one detail of it is of particular significance. This story does not actually say they occupied the whole of the Olympic Peninsula west and north of the mountains. Rather, it explicitly says that the lands then held by the Quileutes along the Strait of Juan de Fuca (i.e., "the long water") extended from "the mouth of the Hoko River to Chimakum." Recall that Chimakum is located on the northeastern Olympic Peninsula. Makah people dispute the mid-nineteenth century treaty-based claim that their territory did not extend eastward beyond the mouth of the Hoko River (e.g., Colfax 1987), but it is also widely recognized that the core of their traditional territory lies to the west of the Hoko River. Thus, this story explicitly contradicts the claim that Quileutes held the northwestern Olympic Peninsula before Makahs were present.

Looking at the arguments themselves, what can be said about them? Reagan's ideas are of considerable interest and appear to offer some support for Kinkade and Powell. Nevertheless, important uncertainties remain about his findings. Both Reagan's and Schalk's ideas are best evaluated in light of what is currently known about the region's archaeological record. Discussion of them from this perspective is presented below.

Evaluating Kinkade and Powell's ideas is easier as it is possible to examine their sources and consider them in context. While the discussions presented here do not prove that they are wrong, I suggest that their claim: "A great deal of evidence suggests that the entire northern Olympic Peninsula was originally controlled by Chimakuan peoples" significantly overstates the facts. Similarly, I suggest that their evidence for the claim that the Makahs arrived approximately 1,000 years ago is, at best, very weak. Further, it is fair to say that Kinkade and Powell's ideas have been largely ignored by archaeologists. Recent summaries of Northwest Coast archaeology (e.g., Matson and Coupland 1995; Ames and Maschner 1999; Moss 2011) do not mention them. Alan McMillan's (1999:37) overview of the archaeology of western Vancouver Island and the northwestern Olympic Peninsula does describe their ideas and finds that the cited evidence for their 1,000 year estimate is: "inconclusive at best."

Archaeological Perspectives

An appreciation of what archaeology might offer to this discussion requires several steps. First, it is necessary to review what archaeological data is actually available for the northwestern Olympic Peninsula. This section of the paper will consider the archaeological findings from both Makah and Quileute territories, but before doing so, it is useful to offer some summary discussion of the region as a whole. While the earliest account of the region's archaeology is a century old, the total effort has been relatively limited and most of the data is quite recent. Moreover, the data is influenced by a number of significant biases. Despite these problems, this data is relevant to the ideas of interest here. Once these issues have been addressed, I turn to the questions of how ethnic identities may be reflected in the archaeological record and what this record may be suggesting about the precontact history of the northwestern Olympic Peninsula.

The first published account describing the archaeological resources of the region was the Albert Regan (1917) report considered earlier. While this report offers numerous observations which suggest that he actually conducted controlled excavations into sites in the region, no discussion of such excavations is presented.[2] Thus, we don't know if he actually undertook such excavations and, if so, we know almost nothing about which sites he investigated, how his investigations were made, or the details of what he found. Similarly, while he offered numerous taxonomic identifications for bones in the middens at La Push, it is unlikely that he was qualified to do so.[3] As such, while Reagan's account contains much of interest, caution must be used in relying upon his conclusions. Even if his taxonomic identifications are correct, it is not possible to reconstruct the specific archaeological assemblages he described in other than extremely cursory terms.

No additional archaeological research was conducted here until the late 1940s. Richard Daugherty's survey of the outer Washington Coast in 1948, and then Fred Pennoyer working along the Strait of Juan de Fuca in the early 1950s, identified nine sites in the area of interest here. All of the latter are shell midden sites located along modern marine shorelines. Neither Daugherty nor Pennoyer excavated during their surveys and thus this pioneering work provided little information about the contents of these sites. The first report of a controlled excavation on the Olympic Coast was Thomas Newman's description of work at Toleak Point (45JE9) in 1959. A second, similar, site report describing work at White Rock Village (45CA30) was prepared by Stanley Guinn in 1963. These efforts and Daugherty's initial excavations at Ozette Village (45CA24) in 1966–1967 can be considered the "Early Period" of modern archaeology on the Olympic Coast. All of them excavated relatively large volumes and produced basic insights into the artifact assemblages and depositional structures of the sites they examined. At the same time, none of them actually screened the sediments they were excavating and there was little interest in the recovery of faunal remains. The work at Ozette, however, included the participation of Carl Gustafson, a zoologist who provided some of the first well-documented studies of archaeological faunal remains from this region.

Archaeological research on the northwestern Olympic Peninsula changed dramatically in 1970 with the discovery of extensive water-logged cultural deposits at Ozette. Daugherty returned and subsequently led an eleven-year effort which produced one of the largest controlled archaeological samples from the southern Northwest Coast. In addition to a very large assemblage of normally perishable plant fiber artifacts and structural remains, this effort also generated a very large faunal assemblage which has been extensively studied. The Ozette Archaeological

[2] In an effort to clarify this matter, I have recently reviewed more than 400 pages of unpublished Albert Reagan records on file with the Harold B. Lee Library at Brigham Young University and with the Smithsonian Institute. These materials strengthen the impression that he dug into numerous archaeological sites, but they offer no additional information regarding the specific details of his activities.

[3] Reagan claims to have identified six species of salmon and six species of whales at La Push. This is unlikely to be possible unless he assembled a large comparative collection or collected DNA from the bones. There is no evidence he did either.

Project also stimulated a number of related efforts. In 1973–1974, Edward Friedman conducted small-scale test excavations at five additional sites on the Makah Indian Reservation. While the excavated volumes of these efforts were small, faunal materials were systematically recovered and examined as a part of the analysis. At about the same time, Dale Croes began to investigate another area with water-logged cultural deposits near the mouth of the Hoko River (45CA213). This initial work became the second extensive effort in the region, later demonstrating that this site contains adjacent "dry" deposits and expanding to also consider a nearby rockshelter (45CA21) identified earlier by Pennoyer. The combined result of these efforts produced considerable data on the artifact assemblages, faunal assemblages, and depositional structures at these sites. Radiocarbon dating of cultural deposits also became common during this period.

The pace of site excavations waned after the work at Ozette terminated in 1981 and at the Hoko River in 1987, but several conditions have driven additional activities in recent years. The first of these was the rise of CRM archaeology in the 1970s. These efforts have been more limited in scope, generally consisting of small-scale test excavations and associated construction monitoring. Efforts of this type were reported by Mary Ann Duncan at La Push (45CA23) in the mid 1970s and again by Gary Wessen in 2006. Another important factor has been the emergence of the Makah Cultural and Research Center in Neah Bay after the Ozette Project closed. This institution has supported small-scale test excavations by Wessen at several locations on the Makah Indian Reservation (45CA1, 3, 22, 400, and 420) in the early 2000s. Finally, the National Park Service has also supported small-scale test excavations at several locations in the Coastal Strip of the Olympic National Park. The first such effort was conducted by Wessen near Sand Point (45CA201) in 1988 and additional efforts have been made by David Conca at Cedar Creek (45CA29) and Cape Johnson (45CA32) in the early 2000s.

There are currently almost 100 recorded archaeological sites on the northwestern Olympic Peninsula. This total represents the efforts of a number of different researchers over a period of more than 60 years. As such, site boundary protocols vary significantly and some of the sites consist of aggregates of nearby—yet wholly discrete—archaeological deposits. Almost a third of these are historic sites representing nineteenth and/or twentieth century cultural activity. A few of the undated prehistoric sites here are lithic scatters of chipped dacite or basalt debitage with only small numbers of formed tools and no organic preservation. None of these sites are located on, or close to, marine shorelines and only one (45CA432) has been sampled (Conca 2000). It is likely that these represent Early to Middle Holocene occupations and they are not directly relevant to the issues addressed by this paper.

The majority of the recorded sites here, however, are precontact sites which represent cultural activity during the last ca. 4,000 years. The recorded precontact inventory contains 66 such sites; 53 of which are shell midden deposits. Encountered in far smaller numbers are petroglyphs, intertidal fish traps, lithic sites, and culturally-modified trees. Beyond this strong bias toward shell midden deposits, there are also strong geographic biases. All of the shell middens, all of the fish traps, most of the petroglyphs, and many of the culturally-modified trees are located on, or relatively close to, marine shorelines. Precontact sites in interior settings are rare on the northwestern Olympic Peninsula. Another important bias in the data is that most of the recorded precontact sites—48 of the 66—are located in Makah territory. Not surprisingly, the larger sample from Makah territory also contains a greater diversity of sites. It is likely that the greater number of sites in this area is due to several different factors. First, Makah territory contains significantly more marine shoreline; approximately 40 miles to 30 miles for Quileute territory. Second, the Ozette Archaeological Project and the various efforts which followed from it have resulted in considerably more research activity in Makah territory. It is also likely that Makahs occupied the marine shoreline in higher densities than Quileutes did.

The bias toward sites located in Makah territory is also strongly apparent in the available excavated data from the northwestern Olympic Peninsula. Twenty sites with precontact deposits have been investigated by controlled archaeological excavations (Figure 4). Fifteen of these sites are located in Makah territory; five are located in Quileute territory. As a few of the sites have been sampled more than once, a total of twenty-four precontact assemblages are available; eighteen from sites in Makah territory, six from sites in Quileute territory. Basic characteristics of these assemblages are summarized in Table 1. This table reflects a number of conditions and trends, but before considering them, it is useful to comment on some of what is presented. First, note that the artifact summaries only address chipped stone, ground and pecked stone, and bone and shell artifacts. The very large numbers of preserved wood, bark, and other plant fiber objects found only in the water-logged cultural deposits at Ozette and Hoko River are not included. Also note that the faunal assemblage summaries report the size of the quantified bone samples available.

The first condition worthy of comment is the overwhelming dominance of the samples generated by the work at Ozette and the Hoko River sites. Work at both was driven largely by the presence of the water-logged cultural deposits and large scale efforts were conducted. In contrast, many of the other excavations were only small-scale testing efforts. The latter examined much smaller volumes of cultural deposits and thus generated much smaller individual samples. If the relationship of excavated volume to recovered sample size is considered in a temporal context, a clear trend is evident. The earliest recorded excavations dug large volumes of cultural deposits, but they were primarily interested in the recovery of artifacts. As such, we know relatively little about the faunal assemblages from some of these sites. More recently, archaeologists working in this region have taken a more resource-frugal approach to their sampling. Excavated volumes have become much smaller and the recovered materials have been much more extensively studied. This has led to the generation of significant quantified bone assemblages for most of the sampled sites and greater use of these materials to investigate cultural behavior. In almost all cases, the quantified bone sample is dramatically larger than the artifact sample from the same site.

While some significant variation is indicated, this table reflects several basic trends in the artifact assemblages. A few sites do contain large numbers of chipped stone objects, but many others lack evidence of this technology. Almost all sites, however, contain some ground and/or pecked stone tools and relatively large numbers of artifacts made of bone or antler. Marine shell artifacts also occur but are not abundant. Table 1 also indicates some significant variation in the relative frequencies of different classes of animals represented in these sites. (Note that

Figure 4. The locations of northwestern Olympic Peninsula archaeological sites mentioned in the text.

Table 1. A Summary of Excavated Assemblages from Precontact Sites on the Northwestern Olympic Peninsula

Site	Name	Approximate Age Range (Cal BP)	Excavated Volume (M^3)	Artifacts CS[a]	GPS[b]	B&A[c]	Quantified Bones	Fish	Mammals	Birds
Sites in Makah Territory										
CA22[3]	Neah Bay	150	7.0	-	4	36	294	95	107	92
CA206[3]	Archawat	200 → 150	9.8	-	6	10	251	189	35	27
CA1[6]	Waatch	200 → 150	1.3	-	-	13	2,121	1,655	345	121
CA204[3]	Warmhouse	250 → 150	8.0	-	4	13	927	830	97	-
CA30[12]	White Rock	500 → 150	~50.0	-	30	157	-	-	-	-
CA24[3]	Ozette A75	700 → 150	4.4	-	-	27	2,018	909	1,014	95
CA22[5]	Neah Bay	750 → 150	1.9	-	3	4	2,606	1,105	437	1,064
CA24[11]	Ozette B70	800 → 150	~1,300.0	79	1,272	~4,500	76,119	22,061	52,938	1,120
CA207[3]	Tatoosh Island	900 → 150	11.8	-	9	35	923	231	635	57
CA21[1]	Hoko Rockshelter	900 → 150	~20.0	116	224	1,051	54,306	47,940	3,518	2,848
CA25[3]	Tsooes	1100 → 150	17.6	-	11	102	1,680	808	577	295
CA24[10]	Ozette A Trench	1500 → 150	~220.0	41	52	472	-	-	-	-
CA22[4]	Neah Bay	2200 → 1900	2.3	1	1	7	1,206	1,145	14	47
CA201[13]	Sand Point	2200 → 1300	1.5	24	19	34	2,628	1,932	642	54
CA217[2]	Hoko Wet/ Dry Site	2900 → 1700	~50.0	~40,450	314	-	4,368	3,818	23	527
CA420[9]	Clara Tyler	3300 → 1900	3.1	355	3	11	1,218	1,091	83	44
CA3[7]	Upper Waatch	3700 → 1600	3.0	770	8	8	592	495	45	52
CA400[8]	Paul Parker	4000 → 2800	6.7	225	19	17	3,530	3,229	88	213
Sites in Quileute Territory										
CA29[14]	Cedar Creek	1000 → ?	0.5	-	-	1	179	162	14	3
CA32[15]	Cape Johnson	1100 → ?	1.6	-	-	4	1,043	836	176	30
CA23[16]	La Push	800 → 150	5.6	34	24	18	-	-	-	-
CA23[17]	La Push	800 → 150	6.4	-	-	11	1,971	762	1,147	56
JE8[18]	Strawberry Point	200 → 150	2.5	-	-	20	707	394	144	13
JE9[19]	Toleak Point	? → 150	~1,000.0	5	86	81	-	-	-	-

Site Reference Key: 1 – Croes 2005; 2 – Croes 1995; 3 – Friedman 1976 and 1980; 4 – Wessen 1991; 5 – Wessen n.d. a; 6 – Wessen n.d. b; 7 – Wessen 2006a; 8 – Wessen 2006b; 9 – Wessen 2003; 10 – McKenzie 1974; 11 – Samuels 1994; 12 – Guinn 1963; 13 – Wessen 1993; 14 – Conca n.d. a; 15 – Conca n.d. b; 16 – Duncan 1981; 17 – Wessen 2006c; 18 – Wessen 1995; 19 – Newman 1959

Artifact Key: CS[a] – Chipped Stone; GPS[b] – Ground and Pecked Stone; B&A[c] – Bone and Antler some of this variation reflects the sample sizes chosen by different faunal analysts and thus the internal site frequencies may be slightly misleading in some cases.) Typically, the fish bone assemblage represents the largest fraction of a recovered bone sample. Mammal bone assemblages are usually at least somewhat smaller, but still relatively large. Bird bone assemblages are usually much smaller.

Finally, consider how clearly the available data from sites in Makah territory dominates the available data from those in Quileute territory. Makah sites are represented by more than 50,000 artifacts, exclusive of the 10,000s of preserved wood, bark, and other plant fiber objects from Ozette and Hoko River. In contrast, Quileute sites are currently represented by less than 350 artifacts from controlled excavations and no preserved wood, bark, and other plant fiber objects. An additional 1,275 bone, stone, and shell artifacts recovered from construction spoil at La Push also represent Quileutes, although they have no stratigraphic context and cannot be demonstrated to represent a single cultural unit (Wessen 2006c). Even more dramatic, Makah sites are represented by more than 154,000 identified animal bones; Quileute sites by less than 4,000.

Makah Territory

Table 1 indicates that the sampled sites in Makah territory can be thought of as members of two separate groups. Nine of the fourteen sampled sites contain cultural deposits representing occupation during the last 1,000 years (45CA1, 21, 22, 24, 25, 30, 204, 206, and 207). The oldest of these is approximately 1,500 years old and all continued to be occupied into the early historic period. At least eight of these nine sites are also known to us through ethnographic and oral-historical sources; four represent large traditional villages and four more represent smaller seasonal camps. A second group includes six sites which contain cultural deposits representing occupation between approximately 1,500 and 4,500 years ago (45CA3, 22, 201, 213, 400, and 420). Of these, only 45CA22 is known to us through ethnographic and oral-historical sources. Members of each group share a number of characteristics.

All sites in the younger group are located very close to current marine shorelines. Most are located on low marine terraces which are actively eroding. All contain assemblages which are heavily dominated by bone and antler artifacts including barbed and toggling harpoons, wedges, and large numbers of small bone unipoint and bipoint forms. Some ground and pecked stone artifacts also occur, with various types of abraders and line weights being particularly common. Ground stone fishhook shanks and ground slate knives and/or points also occur, but are less abundant. Chipped stone objects—either tools or debitage—are very rare or wholly absent. Shell artifacts include ground mussel shell knives and harpoon blades, purple olive shell beads, and dentalium. Of particular note, the bone, antler, stone, and shell artifacts encountered in these deposits are indistinguishable from those recovered from the contemporaneous water-logged deposits at Ozette.

The younger deposits also contain large quantities of faunal remains including marine shell and the bones of fish, mammals, and birds. The fish assemblages usually contain large numbers of greenling, rockfish, and sculpin species; relatively small nearshore fish. Greenling often dominates this group, typically accounting for 30 to 50% of each assemblage. Larger well represented fish include salmon, halibut, lingcod, and dogfish shark. The bones of these fishes are present in relatively smaller numbers but, given their substantially greater body sizes, they actually represent a significantly larger quantity of food (Huelsbeck 1983:114–118). Halibut is particularly prominent as a food resource in this group, although the frequency of halibut bones in these deposits is often less than 10% of all fish. Orchard and Wigen (2016) have suggested that this apparent underrepresentation is due to a combination of factors including both the likelihood that most halibut were butchered on the beach and taphonomic issues affecting those bones which actually arrive in the site. Finally, note that all of the recovery of fish bones from these sites was done using ¼ inch mesh screen and therefore very small schooling fish are undoubtedly underrepresented. In fact, very small quantities of Pacific herring bones have been recovered at four of the nine sites and it is likely that they are actually present in very large numbers in them, and possibly at still other sites in this group as well. The mammal bone assemblages are heavily dominated by marine mammals and fur seals are particularly prominent in this group. In some cases, they represent more than 80% of all identified mammal bones. Also commonly encountered are sea lions, harbor seals, porpoises, and whales. In contrast, terrestrial mammals are typically only a small fraction of the assemblage; deer and elk are usually the most abundant animals and dogs are also common. Finally, bird bone assemblage, though frequently small, often exhibit greater species diversity than either the fish or the mammals in the same site. This suggests that bird hunting was much less focused (i.e., more opportunistic) than fishing or mammal hunting. Still, the overwhelming majority of the identified birds are marine birds; both offshore and nearshore species are very common. Terrestrial birds are rare.

In sum, both the artifact and faunal assemblages from the younger precontact deposits in Makah territory reflect economic and technological behaviors which closely resemble those of the early historic Makah people. In this sense, they are essentially equivalent to Reagan's (1917) "Old" deposits. These assemblages also share many characteristics with those recovered from the group of significantly older sites, yet exhibit important differences as well.

The following discussion considers the assemblages recovered from six of these older deposits, but it is important to note that more than six such locations are known to exist here. At least two more (at 45CA1 and 45CA509) have been identified, but have yet to be sampled by controlled excavations (Wessen 2003b:38–41).

Unlike the younger deposits, none of the deposits in the older group are located on or close to active marine shorelines. Rather, these sites are located on surfaces which are at least somewhat higher in the landscape and farther away from modern shorelines. Some are located interior to exposed outer shorelines and others have been found along the flanks of small coastal river valleys. Sites in both settings are typically located at elevations which range from approximately 20 to 45 feet above sea level. On the exposed shoreline, they typically occur at least several hundred feet interior to the modern beach; in coastal river valleys, all are situated farther inland and some are more than a mile from the modern beach. At least a few other conditions seem to follow this distinction. Sites located in the coastal river valleys contain two distinct components: a mass of shell midden deposits overlain by cultural deposits containing very little or no shell. Sites located closer to exposed shorelines represent a more complex group, but they do not include any in which shell midden deposition is replaced by a younger non-shell component. Further, the older sites in the coastal valleys appear to have been abandoned some time ago and they are not located close to any of the younger sites while the older shell middens nearer the shorelines are all located close to—or have been considered a part of—a still younger site. The distributions of these sites and apparent changes in their functions are thought to be responses to a significant tectonic uplift in the region in recent millennia (Wessen and Huelsbeck 2015). The rising landscape pushed the western shoreline farther to the west and drained local bays, turning them into small coastal valleys. Thus, settlements on the coast appear to have moved as the shoreline did; settlements associated with the old bays first changed their function and then later were wholly abandoned.

Most of the older shell middens are relatively large sites with thick internally complex deposits. Accumulations more than 3 feet thick are common. The activities at the oldest shell midden yet dated (45CA1) began ca. 4,500 years ago. Shell midden deposition was occurring at all—except 45CA213—by ca. 3,500 years ago. Shell midden deposition at most of them ended ca. 1,600 to 1,800 years ago. In contrast, 45CA213—the Hoko River Wet/Dry Site—does not contain shell midden deposits. It was occupied between ca. 2,900 and 1,700 years ago.

The assemblages represented in the older shell middens include ground stone, bone, and shell artifacts much like those from the younger middens and chipped stone artifacts in strong contrast to them. The most commonly encountered ground stone artifacts are whetstones. Anvils, hammer stones, and a few fragments of ground slate have also been found. Bone artifacts are dominated by small unipoint and bipoint forms; unilaterally barbed points, composite toggling harpoon valves, and wedges are also common. Shell artifacts include ground mussel shell knives, purple olive shell beads, and dentalium. Unlike the younger middens however, the older shell middens also contain large numbers of chipped stone artifacts. The great majority of these objects are debitage and formed tools are rare. Examples of direct free-hand percussion are very common. Both bipolar percussion and pressure flaked objects are also present, although the former are less common and the latter are very rare. The dominant tool stones include basalt or dacite, quartz, and a heterogeneous group which includes shale and schist. Also present—but rare—are examples of a dark greenish-gray cryptocrystalline silicate material and obsidian from at least four different sources in southeastern Oregon (Newberry Crater, Obsidian Cliffs, Whitewater Ridge, and Wolf Creek). Chipped stone formed tools encountered in these middens are limited to small flaked cobbles and ovoid knives or preforms.

Despite this difference, the fish, mammal, and bird bone assemblages from the older shell middens are also much like those from the more recent sites. Fish bone assemblages are invariably the largest and they contain similar proportions of the same species. The relatively small nearshore greenling, rockfish, and sculpin species are again the most abundant, typically ranging between 40 and 60% of each fish assemblage. Larger well-represented fish again include salmon, halibut, lingcod, and dogfish shark. Dominance within this group varies with either lingcod or salmon usually at the top. The earlier noted comment about the use of ¼ inch mesh screen and its resulting underrepresentation of very small fish is equally relevant here. Very small quantities of Pacific herring bones have been recovered at five of these six sites and finer screen sampling at two of them (45CA400 and 420) demonstrates that very high densities of these bones are actually present. Mammal bone assemblages are heavily dominated by marine mammals and fur seals are particularly prominent in this group. Also common are sea lions, harbor seals, porpoises, and whales. Terrestrial mammals are only a small fraction of the assemblage; deer and elk are often most abundant animals and dogs are also common. Bird assemblages are usually the smallest fraction, yet invariably exhibit greater species diversity than either the fish or mammals in the same site. Marine birds; both offshore and nearshore species, are dominant.

A slightly younger component overlies the shell middens at 45CA3, 400, and 420; and probably at 45CA1 and 509 as well. At the first three, it is represented by a deposit containing numerous pieces of chipped stone and other lithic objects, but few examples of bone or other preserved organic materials. The "dry" site deposits at 45CA213 share many characteristics with the latter. Unfortunately, while the Hoko River site has been studied in detail, our knowledge of this component at the former sites is much more limited. Not only sampled in much smaller volumes, the deposits overlying the older shell middens at all of these sites have been disturbed by a combination of affects including bioturbation, logging, and road building. As such, there is at least limited evidence of the reworking of sediments across the boundary of the two components. This condition calls for caution and has limited our ability to assess some details of the upper component deposits. An approximate date for the beginning of the younger occupation is available at some of these sites, but no direct information is available regarding when it ends. All that can be said is that the abundant chipped stone materials encountered in these younger component deposits are not found in the shell midden sites which post-date ca. 1,500 years ago.

The most abundant cultural material recovered from these younger component deposits is chipped stone debitage. Examples of both direct free-hand percussion and bipolar percussion are very common, with bipolar percussion being the dominant at 45CA3, 213, and 420. The principal focus of this activity at all of these sites was the reduction of vein quartz pebbles to produce small microliths. The same bipolar vein quartz debitage is also present at 45CA1, 400, and 509, although it's relative frequency compared to direct percussion debitage in these sites has not yet been established. (This same bipolar vein quartz debitage has also been recovered in smaller numbers from the upper layers of the underlying shell midden deposits at 45CA3 and 420, although it is unclear if they are actually associated with the middens or if they have been re-deposited from the disturbed strata above them.) Excavation of the "wet" deposits at 45CA213 has produced a number of examples of these quartz microliths mounted in small wood handles and experimental replicas of such tools have been shown to be effective knives for cutting fish (Flenniken 1980). Presumably, their presence at 45CA1, 3, 400, 420, and 509 reflect similar activities. Evidence of the use of direct percussion on essentially the same range of raw materials found in the shell middens is also encountered in these deposits. In contrast to the latter however, the chipped stone assemblages from the younger components contain significantly more formed objects, although they are still uncommon. The most abundant of these are crudely shaped ovoid to roughly circular bifaces which are probably not finished tools. The most common apparently finished tools are ovoid to roughly circular bifacial knives. Also present are a few large stemmed projectile points, scrapers and/or utilized flakes, and flaked cobbles. Ground stone artifacts in these deposits are uncommon and those which do occur are indistinguishable from those in the shell middens. Whetstones are the most common and a few anvils, hammer stones, and a single fragment of ground slate have also been found. Neither bone nor shell artifacts have been found; a condition which is attributed to the less favorable depositional environment here.

The faunal assemblages recovered from the younger component sediments have likely also been affected by both the less favorable depositional environment and obvious disturbance at these sites. Only very small quantities of fish, mammal, and bird bone and marine shell have been collected at 45CA3, 400, and 420. Essentially the same condition is evident at 45CA213, where a substantially larger recovery of fish, mammal, and bird bone has been recovered, although none of the latter represent the "dry" portion of the site. The specimens from the younger components at the former sites are less well preserved than those from the middens and thus, often less diagnostic. Nevertheless, the represented fish, mammal, and bird assemblages appear to be much like those from the underlying shell middens. While some of the latter may, in fact, be associated with the earlier occupation, it is also likely that materials from the top of the midden have been displaced into the upper matrix by any of the above-noted disturbance mechanisms.

The preceding remarks have argued for a connection between 45CA213 and the older sites farther to the west and southwest while still noting contrasts between them. Some of these contrasts are undoubtedly real. Both its environmental setting and the range of deposits it offers are unlike any other site in this group. 45CA213 also contains cultural materials—other than the perishable organics—which are not reported from any site in this group. For example, both quartz crystal microblades and relatively large, faceted, lance-like ground slate points have been recovered at Hoko River, but nowhere else on the northwestern Olympic Peninsula. Both types of objects are rare at the site and—as such—their absence in the much smaller collections from the other sites could be no more than a sampling error. Other differences are very unlikely to sampling errors. The fact that very small quantities of obsidian have been recovered from the small volume tests at 45CA3 and 420, but not from the much more extensive work at 45CA213, suggests that this distinction is real. Still other variations are also apparent; note that the bipolar production of quartz microliths evident at many of these sites has not been reported at either 45CA22 or 45CA201. No explanation for these variations is offered here and more than one condition is likely involved.

Overall, the older sites in Makah territory are a more complex group than the recent sites. Still, they are inconsistent with Reagan's Very Old deposits, given that they contain abundant evidence of stone working. Whether they approximate his Ancient deposits is uncertain as the latter are not clearly described. All of the older shell midden deposits share numerous characteristics which are also consistent with the more recent shell middens here. The ground stone, bone, and shell artifact assemblages and the faunal assemblages are all very similar. The only area of strong contrast is the chipped stone assemblages. With this exception, most of the available record argues for strong continuities in the economic orientations and technologies of native culture on the northwestern Olympic Peninsula over the last ca. 4,000 years.

The significance of the chipped stone is therefore of particular interest, but the currently available view is both limited and complicated. The chipped stone assemblages indicate that the use of both direct free-hand percussion and bipolar percussion was common. Evidence of direct percussion flaking is always dominant in the shell midden deposits, although it appears to have been focused on a relatively limited range of tools; mostly ovoid knives and flaked cobbles. To date, chipped stone projectile points have not been found in the older shell midden deposits. Evidence of bipolar percussion is found in the middens but it is less common and to what extent the bipolar production of vein quartz microliths was occurring is uncertain. It may have been associated with the more recent of the older midden deposits or it might have post-dated them. In either event, evidence of the bipolar production of vein quartz microliths is abundant in the deposits overlying shell middens in coastal valley settings and in both the "wet" and "dry" deposits at 45CA213.

While much about the origins and significance of the chipped stone traditions remains uncertain, it is important to emphasize the overall continuities within which they occur. Neither the appearance nor the disappearance of particular stone chipping technologies appears to have altered the range of other technologies employed or the basic character of the economic orientation. Thus, there is little reason to suggest that significant changes in the represented population are indicated.

Quileute Territory

Archaeological excavations in Quileute territory have examined only five sites. All five are shell midden sites located wholly—or partially—on low terraces immediately adjacent to the active beach. Further, all five of the sites are located in places where ethnographic and/or oral historical sources report the former presence of traditional Quileute settlements. La Push (45CA23) is usually describe as the principal Quileute village on the Olympic Coast. Smaller significant settlements may have been present at both Cape Johnson (45CA32) and Toleak Point (45JE9). In contrast, settlements represented at Cedar Creek (45CA29) and Strawberry Point (45JE8) may have been smaller, less diverse, seasonal occupations.

Five of the six excavation collections are supported by radiocarbon dates and summaries of the approximate temporal ranges for the site occupations are summarized in Table 1. All four of the dated sites represent occupation during the last ca. 1,100 years. While no radiocarbon dates are available for the Toleak Point occupation, it is likely that it also contains deposits representing this period. Midden deposits containing early historic objects are also known to be present in at least two of the sites and it is likely that most of them were occupied into the nineteenth century. Thus, all five of these sites represent essentially the same period as the group of younger Makah sites described above.

The total precontact artifact assemblage from these excavated sites is small and contains significant numbers of both stone and bone objects (Table 1). Thus far, no Quileute artifact assemblage has been studied in detail, but a number of observations about them can be offered. Bone artifacts dominate all individual collections and all bone tool collections are heavily dominated by a variety of small unipointed and bipointed forms representing harpoon points, fishhook barbs, herring rake teeth, awls, needles, and combs. These objects frequently account for more than half of all the artifacts in a site sample. Other widely occurring bone tools include several forms of composite toggling harpoon valves, unilaterally barbed points, and wedges. Note that this closely resembles the bone artifact assemblages from the shell middens in Makah territory.

While never encountered in large quantities, stone artifacts have been recovered at both 45CA23 and 45JE9 and they are more abundant than the available bone and antler artifact samples at each of these sites. Chipped, ground, and pecked stone technologies are all represented. Evidence of both direct free-hand percussion and bipolar percussion is present at both sites. The most commonly occurring tool forms are flaked cobbles, scrapers, and utilized flakes. Chipped stone projectile points have not been recovered from these sites. Nor have the bipolar vein quartz microliths found at most of the older sites in Makah territory. Raw materials used for the recovered tools either vary widely or are poorly understood; Reagan said they used agate, Newman said stone tools at 45JE9 were made of Greywacke, Duncan said those at 45CA23 were mostly basalt, stone tools in the 45CA23 construction spoil collection are mostly fine-grained sandstones although a few cryptocrystalline silicate and quartzite artifacts are also present. Exotic chipped stone materials such as obsidian have not been reported from these sites. Ground and pecked stone forms are also relatively common.

By far, the most commonly reported objects are tabular pieces of sedimentary rock variously described as abraders, anvils, and whetstones. Other commonly occurring tools include grooved and/or perforated line weights and fishhook shanks.

Finally, Quileute artifact collections also contain objects made of marine shell, but these are rare. The most common objects are fragments of ground mussel shell which probably represent knives or harpoon points. A few purple olive shell beads and a single fragment of a large Weathervane scallop shell rattle have been recovered from 45CA23. Dentalium has not been encountered in Quileute sites.

All of the sampled Quileute sites are also rich in faunal materials, but quantified data is only available for four of them. In most cases, fish bones are most abundant with mammal bones also being well represented. In contrast, individual site bird bone assemblages are invariably small compared to the other two groups at the same site. All quantified fish bone samples exhibit a considerable range of species and share a number of common characteristics. In considering them, note that—again—nearly all such samples were recovered with ¼ inch hardware mesh and thus, very small fish bones are under-represented. Finer screening has only been conducted at 45CA23, and has shown that this site contains very large numbers of surf smelt bones. All identified Quileute fish bone samples are dominated by the relatively small greenling, sculpin, and rockfish species. This group accounts for at least 65% of all identified fish bones at 45CA29, 32, and 45JE8 and slightly more than 37% at 45CA23. The only relatively common large marine fish in these sites is lingcod; ranging from approximately 6% of the fish bones at 45CA32 to 22% of the fish bones at 45JE8. Other relatively large fish represented in the Quileute sites include salmon, halibut, and dogfish, but evidence of these species is much more limited; occurring only in some sites and only at very low frequencies. Three of the four quantified mammal bone samples contain fur seal bones and this animal heavily dominates the assemblages from 45CA32 (75%) and 45CA23 (96%). In contrast, fur seal bones account for only 2% of the identified mammal bones at 45JE8 and were not found at 45CA29; as yet unidentified fur seal bones may, however, be among unidentified marine mammals from each of the latter sites. A number of other marine mammals are also represented, but in far fewer numbers. After fur seals, Steller's sea lions and sea otters appear to be the preferred prey species. The bones of terrestrial mammals are uncommon; the most commonly reported taxa being canids. Finally, the aggregate bird bone sample from the Quileute sites is quite small. The group contains a mixture of both nearshore and offshore marine birds, but no particular group of marine birds really dominates. Some gulls, ducks, and terrestrial birds are also present, but these are rare.

Overall—and despite some obvious variations—the archaeological assemblages from Quileute territory all appear to be members of a single relatively consistent group representing recent precontact Quileute activity and can be considered to be roughly equivalent to Reagan's "Old" Quileute deposits. While it is likely that early historic materials are also present in at least some of them, it is also likely that Reagan was correct when he suggested the only difference between his "Recent" and "Old" Quileute assemblages is the presence of nineteenth century Euro-American trade goods in the former. These assemblages share much with the recent shell midden assemblages described earlier from Makah territory.

The apparently much more limited time depth in these Quileute sites is a potentially interesting and important condition, if it is real. As noted earlier, there is currently no evidence of cultural activity on this portion of the Olympic Coast until approximately 1,100 years ago. Given the very limited extent of the work, it is tempting to suggest that the absence of older materials is merely an expression of the fact that only relatively young landforms have been investigated.

In fact, such an interpretation is probably not correct. Newman's Area A excavation at 45JE9 was located on a terrace approximately 30 feet above sea level, on the slope interior to the lower surface where the rest of the work occurred (Newman 1959: Map 3). This part of the site contains midden deposits similar to those on the point below, including both ground stone objects and a few chipped stone scrapers. Newman suspected that relatively older cultural deposits were present here, but had little basis to estimate ages for different parts of the site. Thus, there is at least one case of a shell midden deposit on an elevated surface interior to the active beach; essentially the same landform expression as the older sites in Makah territory. Whether these deposits really are older than the rest of the site is uncertain, and worthy of further study. If these really are significantly older deposits, one further observation from Newman's work is important: Area A lacks any evidence of the bipolar reduction of vein quartz pebbles seen in the older Makah sites.

Another aspect of the antiquity of Quileute sites worthy of comment concerns 45CA23. Reagan reported the presence of "Ancient" deposits at La Push, but had little basis to assess how old they might be. Duncan (1981:57–64) said that neither the sewer trench nor the associated test excavation encountered culturally-sterile deposits—and that deeper and older cultural deposits are likely to be encountered here—but her claim that culturally-sterile deposits were not encountered is contradicted by the stratigraphic profiles in her report. Schalk and Powell (1997) have also argued that deeper and older cultural deposits are likely to be present at 45CA23. Nevertheless, both of the dated excavations at the site suggest occupation began ca. 800 years ago. While much remains to be learned about this important site, ideas about its antiquity should recall Arthur Howeattle's 1916 account: *Early History and Distribution* (noted earlier). There, he reports that the Quileute people's original home was atop James Island and that they only moved to La Push relatively recently. Thus, archaeologists seeking early deposits representing the Quileute people might consider shifting their focus from La Push to James Island.

Signatures of Ethnic Identity

The relevance of the archaeological record to questions about the precontact history of the Makah or Quileute people is heavily depended upon our ability to accurately recognize these groups in that record. In the context of the present discussion, I assume that the late precontact (ca. 1500 to 150 BP) assemblages from Makah territory represent Makahs and that the late precontact (ca. 1100 to 150 BP) assemblages from Quileute territory represent Quileutes. The important question is: who is likely to be represented by the ca. 1500 to 4000 BP assemblages from Makah territory?

Ethnic identifications for archaeological assemblages are difficult for all but the most recent materials, even under the best of circumstances (Jones 1997). For the most part, efforts to do so have focused on distinctive artifacts and/or technologies which can be taken as signatures of ethnic identity. Some suggested signatures have already been proposed for the northwestern Olympic Peninsula, although none are widely accepted. The following sections review the earlier suggested signatures relevant to a consideration of Makahs and Quileutes and then offer some additional perspectives.

Albert Reagan

Reagan (1917) claimed to be able to distinguish Makah from Quileute artifact assemblages on the basis of their contents. The difficulties with Reagan's findings, however, have already been described and need not be repeated here. Suffice it to say that his claims fail to support the idea that Quileute assemblages predate Makah assemblages in Makah territory in two crucial ways. First, Reagan argued that a Quileute presence could be inferred from cultural deposits lacking stone tools because: "The Quillayutes were not a stone-implement making people." Reagan himself contradicted this claim at the time and subsequent archaeological activities at both 45CA23 and 45JE9 demonstrate that late precontact Quileute people produced a variety of chipped, ground, and pecked stone tools. Second, while Reagan reported that such stone tool-free middens predated the late precontact Makah deposits, we now know that the older sites in this area actually contain significantly more stone artifacts than the late precontact Makah deposits.

The West Coast Culture Type

In 1971, Donald Mitchell revised three existing archaeological units from the Gulf of Georgia region of British Columbia—the Locarno Beach, Marpole, and Gulf of Georgia Phases—into three essentially equivalent units he referred to as "Culture Types." Mitchell (1990) elaborated on the descriptions of these units and proposed three additional regional types for other parts of British Columbia. One of latter is called: the West Coast Culture Type. This unit was applied to what Mitchell called the "Nootkan Area," essentially the outer coast of Vancouver Island, although it is important to stress that he only had data from excavated sites at Yuqout (DjSp1) and Hesquiat Harbour (DiSo1, 9, and 16) (Figure 5). The West Coast Culture Type assemblage he described is heavily dominated by bone and antler artifacts including barbed and toggling harpoons and large numbers of small bone unipoint and bipoint forms. Some ground and pecked stone artifacts also occur, with various types of abraders and line weights being particularly common. Chipped stone objects—either tools or debitage—are very rare or wholly absent. In referring to them, Mitchell (1990:357) commented: "The archaeological assemblages are so like the described Nootkan material culture that a lengthy reconstruction of the technology is not necessary." Using the earliest dates from DjSp1 as his baseline, Mitchell suggested that the West Coast Culture Type was present by ca. 4100 BP and persisted until the early historic period. Given both its geographic location and duration, the West Coast Culture Type quickly came to be regard as the archaeological signature of a long-term relatively stable presence of Wakashan-speaking peoples on the west coast of Vancouver Island.

More recent excavations on the west coast of Vancouver Island have complicated this picture. While late precontact West Coast Culture Type assemblages have now been described at numerous additional locations on the central and southern portions of the island's outer shoreline, examples of a very different assemblage predating ca. 2000 BP have also been identified (McMillian 2003a). Included in the former group are at least six sites in the vicinity of Barkley Sound (Figure 5). In at least three of these sites, a late precontact West Coast Culture Type assemblage is associated with the earlier component. Of particular note, the older materials were found on terraces located above and interior to the more recent deposits; a pattern also noted in Makah territory on the Northwestern Olympic Peninsula. While some variation within the group is apparent, all of these assemblages contain significant quantities of chipped stone artifacts along with bone tools which are consistent with the West Coast Culture Type. The chipped stone artifacts include stemmed, triangular, and leaf-shaped basalt projectile points, ovoid bifacial knives made of schist, shale, or slate, quartz crystal microblades, and bipolar microliths made of vein quartz and cryptocrystalline silicate. A few of these sites also contain limited quantities of obsidian from southeastern Oregon. Many of these artifacts are also present in contemporaneous culture types from the Gulf of Georgia region of British Columbia. In particular, parallels can be seen with the Locarno Beach Culture Type, dating ca. 2500 to 3500 BP. Since the latter has been attributed to precontact Salish speakers, these older assemblages have been seen as suggesting an early Salish presence or influence on some parts of western Vancouver Island that is subsequently replaced by the later Wakashan presence. Added to this is

Figure 5. The locations of western Vancouver Island archaeological sites mentioned in the text. The rectangle indicates the northwestern Olympic Peninsula.

recent work at the Hiikwis Site Complex (DfSh15 and 16) in Barkley Sound where chipped stone artifacts first appear ca. 2,800 years ago and persist until the historic period (MacLean 2012).

These findings indicate that the West Coast Culture Type—as a conceptual unit—does not adequately reflect the prehistory of the west coast of Vancouver Island. While Mitchell was clearly influenced by Dewhirst's (1980, 1982) view of long-term cultural continuity represented at DjSp1, subsequent work now shows significant temporal and areal variations in the artifact assemblages from the west coast. Further, given that almost all of the post-1990 excavation efforts have addressed sites located to the south of Yuqout and Hesquiat Harbour, it's unlikely that the full range of these variations is well understood yet. Another important limitation of the West Coast Culture Type identified by Yvonne Marshall (1993) is that it relies exclusively on artifacts. Her work suggests that significant changes in economic orientations and settlement patterns are also evident on the west coast of Vancouver Island during the last few thousand years. In addressing this matter, it is important to emphasize that migrations or invasions of foreign populations are not the only possible explanations. Some or even much of the observed variations could represent in situ evolution or accommodation by local groups rather than either an influx or a remnant of a different population.

The above limitations notwithstanding, the West Coast Culture Type and associated ideas about its significance for the culture history of western Vancouver Island are relevant to this consideration of the northwestern Olympic Peninsula. Note first that the late precontact assemblages from Makah territory are easily recognizable as additional representatives of the West Coast Culture Type. This is not surprising given the very close cultural ties among the historic Makah, Ditidaht, and Nuu-chah-nulth peoples. The value of this observation in the present context, however, is diminished as the late precontact assemblages from Quileute territory also closely resemble the West Coast Culture Type. While this condition is undoubtedly influenced by our much more limited knowledge of precontact Quileute assemblages, it is likely that the material cultures of late precontact Makahs and Quileutes were quite similar.

The Hoko River Wet/Dry Site

A variation on the recent discussions regarding the West Coast Culture Type on Vancouver Island concerns the Hoko River Wet/Dry site on the northwestern Olympic Peninsula. Croes (1977), using preserved organic materials from 45CA213 and ten other "wet" sites on the Northwest Coast, has shown the existence of broad regional artistic and technological traditions in the production of cedar bark baskets. The distributions of the various traditions approximate those of the major language families on the Northwest Coast and Croes used these findings to argue that basketry is a particularly sensitive marker of ethnic identity. Further evaluations of these idea by Croes (1989, 1995) and Carriere and Croes (2018) have supported the original findings and strengthened the case for them.

These analyses indicate that the ca. 2,900-year-old basketry from 45CA213 is very closely related to the ca. 300-year-old basketry from 45CA24 and that the tradition represented by them is significantly different from a contemporaneous long term tradition of basketry found in several "wet" sites in Puget Sound and the nearby Gulf of Georgia region. Croes associates the basketry present at Hoko and Ozette with Wakashan speakers and those from the Puget Sound and Gulf of Georgia sites with Salishan speakers. Unfortunately, examples of precontact Quileute basketry are not available, so no inferences regarding Chimakuan speakers are possible. Note, however, that the strong continuities between the 45CA213 and 45CA24 basketry argue that Wakashan speakers have been present on the northwestern Olympic Peninsula for at least 2,900 years.

Shortly after these findings began to appear, Mitchell (1982) argued that the 45CA213 lithic assemblage was consistent with the Locarno Beach Culture Type; thereby suggesting an association with the Gulf of Georgia and Salishan speakers. Specific artifacts types from Hoko attributed to the Locarno Beach Culture Type include: stemmed, triangular, and leaf-shaped basalt projectile points, ovoid bifacial knives made of schist, shale, or slate, quartz crystal microblades, microflakes of cryptocrystalline and fine-grained rock, produced mainly by bipolar techniques, large faceted ground slate points, and a single finely finished "Gulf Islands Complex" object. The presence of these objects in an assemblage whose bone and ground stone artifacts fall easily within the West Coast Culture Type is therefore much like the roughly contemporaneous condition found at some sites on the west coast of Vancouver Island. What is to be made of such assemblages? Croes accepted a possible Locarno Beach attribution for the lithic assemblage while rejecting the presence of a Gulf of Georgia population. Rather, he explained the situation by suggesting that different classes of artifacts reflect different types of information. Specifically, while stone, bone, and shell artifacts on the Northwest Coast are most likely to reflect widespread economic trends, basketry styles are more likely to express ethnic information. On this basis, he argued: "the common phase designations are economic stages, or plateaus, developed across broad coastal areas, mostly through the rapid diffusion of good ideas that improved the maintenance of populations and the standard of living. The complex basketry styles, on the other hand, are more reflective of ethnic trends through time" (Croes 1995:228). The implication is that the people represented at 45CA213 were not Salishan speakers, they may simply have been in contact with Salishan speakers.

I believe that this type of explanation is relevant to both the Hoko River and much of the rest of the region. In particular, I agree that many of the common stone, bone, and shell artifacts are most likely to reflect widespread economic trends, rather than markers of ethnic identity; witness the very broad distributions of many types of bone and ground stone tools across much of the Northwest Coast.

While the presence of Locarno Beach Culture Type objects at the mouth of the Hoko River mouth is significant, it is important not to lose sight of the very limited presence they have there. The total number of recovered chipped stone projectile points, microblades, and large faceted ground slate points at 45CA213 is 51 specimens. This, in a collection which includes more than 4,500 tools or tool fragments and more than 40,000 pieces of chipped stone debitage. Moreover, at least seven of the 18 chipped stone projectile points from Hoko are made of lithic materials that are not represented in the chipped stone debitage sample from the site. The latter are very likely trade items. MacLean (2012:109) reports the same condition for chipped stone projectile points recovered at Uukwatis (DfSh15). This should be seen in contrast to sites like Shoemaker Bay (DhSe2), where the early deposits contain large numbers of stone artifacts consistent with the Locarno Beach Culture Type.

Current Status and Possible Future Directions

The above sections capture the current status of efforts to identify possible signatures of ethnic identity in the archaeological record from this region. Clearly, there's not much to work with here. Nevertheless, a few observations can be offered. First, archaeological assemblages dating to the last ca. 1,000 to 1,500 years from Makah and Quileute territories are very similar to each other and both are consistent with the West Coast Culture Type. This is problematic as the latter is strongly associated with Wakashan speakers, yet the Quileute were not Wakashan speakers. As such, there is little basis to distinguish between late precontact Makah and Quileute assemblages at this time. Second, strong technological and stylistic continuities in the basketry from 45CA24 and the 45CA213 have been used as a basis to suggest that the occupants of the Hoko River site were probably Wakashan speakers. Taken together, these are at least possible indications of Wakashan speakers on the northwestern Olympic Peninsula for about 3,000 years. In contrast, no arguments based on well-documented archaeological data have yet been offered for the claim that Chimakuan speakers formerly occupied Makah territory.

The preceding statements represent about as far as the pursuit of this matter has proceeded, but I believe that some additional useful observations are possible. Additional insights relevant to the recognition of ethnic signatures may be obtained in at least two different ways. The use of artifacts for this purpose is already well established and I believe that more can be done with the assemblages already available to us. Further, the substantial faunal assemblages from these sites may also offer insights into the ethnic identities of the represented populations.

Artifacts

The earlier discussion of 45CA213 described how the presence of artifacts attributed to the Gulf of Georgia complicate ideas about who may be represented at this site and noted that this condition is similar to that for some sites on the west coast of Vancouver Island. While much of the focus has addressed the artifacts cited above by Mitchell (i.e., stemmed, triangular, and leaf-shaped basalt projectile points, ovoid bifacial knives made of schist, shale, or slate, quartz crystal microblades, microflakes of cryptocrystalline and fine-grained rock, produced mainly by bipolar techniques, large faceted ground slate points, and "Gulf Islands Complex" objects), some recent discussions have suggested obsidian to be an additional indicator of contact with the Gulf of Georgia (e.g., McMillan and St. Claire 2005; MacLean 2012). Mitchell's 1982 claim about Locarno Beach materials at Hoko, and Croes's response to it has already been noted, but there is still more to be considered. Thus, beyond noting that the projectile points reported at some of these sites are rare and likely to be trade items, the underlying claim that a Gulf of Georgia influence is represented is worth further examination in some cases.

For example, recent discoveries of obsidian sourced to Glass Buttes and Newberry Crater in southeastern Oregon need not have come to western Vancouver Island via the Gulf of Georgia. Examples of ca. 3,000 to 4,000-year-old obsidian from these and other southeastern Oregon sources have also been found in some of the older sites in Makah territory (Wessen and Huelsbeck 2015) and roughly contemporaneous or undated examples are known from elsewhere on the Olympic Peninsula (David Conca, personal communication) and at numerous other locations in western Washington (e.g., Hughes 1995; Skinner 1998, 2015). While it certainly remains possible that Oregon obsidian reached western Vancouver Island via the Gulf of Georgia, there is no reason to assume that this was the only possible route.

A still more important characteristic to consider is the bipolar reduction of vein quartz pebbles to produce microliths first reported at the 45CA213. Presumably, Mitchell (1990:341) equated this with the "microflakes of cryptocrystalline and fine-grained rock, produced mainly by bipolar techniques" which he saw as an attribute of the Locarno Beach Culture Type. While this description does appear to be at least broadly similar to the Hoko assemblage and those from some of the older Makah sites, a closer examination shows that they are actually quite different. In fact, the use of bipolar percussion is widespread on the Northwest Coast, but its application has different expressions. At some sites in western Washington—45CL1 (Ames et al. 1999) and 45SJ24 (Close 2006)—bipolar percussion was used largely in the initial stage of pebble/cobble reduction in order to obtain cores which offered better opportunities for direct free-hand percussion. Alternatively, at other sites—including 45CA213 and some of the older Makah sites—bipolar percussion was used to produce large numbers of small relatively straight-sided flakes which were subsequently used as microliths. Moreover, in most of its expressions on the Northwest Coast, bipolar reduction is applied to a range of raw materials. While one or two types of stone are often preferred, a broad range of materials are commonly represented (e.g., 45KI464 [LeTourneau and Stone 2001] and 45IS2 [Schalk and Nelson 2010]).

As such, bipolar percussion is an expedient and flexible system often employed in support of more complex stone working technologies (Cotterell and Kamminga 1987). Nevertheless, I argue that the bipolar reduction of vein quartz pebbles to produce small microliths at the mouth of the Hoko River is an unusual expression of this technology. It is not accurately described as "microflakes of cryptocrystalline and fine-grained rock, produced mainly by bipolar techniques." What Mitchell may have understood by "microflakes" is uncertain. Avoiding the semantics of possible distinctions between microflakes and microliths, nearly—if not in fact—all of the small bipolar microliths and associated debitage from 45CA1, 3, 213, 400, 420, and 509 are made from vein quartz pebbles (Figure 6). Neither bipolar microliths nor bipolar debitage made from cryptocrystalline rocks have been reported from these sites. In fact, cryptocrystalline debitage of any type is very rare in these sites.

When this particular technological system was first described by Flenniken (1980), it had no close analogs anywhere on the Northwest Coast. As described earlier, however, it has now been shown to be represented in at least six of the older sites in Makah territory. Further, and of particular note, the use of bipolar percussion to produce small microliths from vein quartz pebbles has also been reported at a few sites in the Barkley Sound region of western Vancouver Island;

Figure 6. Bipolar debitage from vein quartz pebbles recovered at 45CA420, northwestern Olympic Peninsula.

Ts'ishaa (DfSi16 and 17) (McMillan and St. Claire 2005) and Uukwatis (DfSh15) (MacLean 2012). At each of the latter, however, bipolar reduction overall appears to be relatively less important and the primacy of vein quartz is less dominant. The deposits containing these artifacts date between ca. 3400 and 3600 BP at DfSi16 and 17 and between ca. 2700 and 2900 BP at DfSh15. That is, the activity at DfSi16 and 17 predates its appearance at 45CA213. At DfSh15, it is roughly contemporaneous with that 45CA213. Given the present uncertainties about its timing elsewhere in Makah territory, its appearance at 45CA400 may be ca. 2,600 to 2,900 years ago and possibly somewhat more recently at 45CA1, 3, 420, and 509. To my knowledge, similar bipolar vein quartz microliths have not been reported anywhere else in western Washington. I am similarly unaware of additional examples of them from sites in the Gulf of Georgia or elsewhere in British Columbia. Thus, to date, this particular variation of bipolar stone working is only known from eight precontact sites; six located close to the northwestern tip of the Olympic Peninsula and two more in the Barkley Sound area of western Vancouver Island. All eight are located within an area approximately 75 miles in diameter. Moreover, all eight are located within the traditional territories of Wakashan speakers.

I find this relatively concentrated group—spatially and temporally—interesting. The likelihood that its members are related to each other seems great. What might it represent? Flenniken (1980) has demonstrated that the hafted quartz microlith tools found at Hoko are very effective knives for fileting fish and could probably be used for cutting other materials as well. Accepting this as their purpose, it can be noted that Middle and Late Holocene archaeological assemblages from the

Northwest Coast contain a number of other tool types which are probably effective knives as well (e.g., various chipped stone, ground slate, and ground mussel shell forms). Further, since it is very likely that both the ancestors and the descendants of the users of these hafted quartz microlith tools also butchered fish, it appears that we are talking about a group of people who developed a "new" type of knife, used it for a while, and then abandoned it for something else.

Another line of thought regarding the group notes that—at 45CA213—this stone working tradition is directly associated with a basketry tradition attributed to Wakashan speakers. I suggest that this is sufficient grounds to consider that small vein quartz bipolar microliths and debitage associated with their production might also be an indicator of Wakashan speakers. Two thoughts follow quickly from this. First, given the paucity of excavated sites on the west coast of Vancouver Island which actually contain these artifacts, they are clearly not an archaeological signature of all Wakashan speakers, nor even all southern Wakashan speakers. Nevertheless, some sort of association with some subset of Wakashan speakers seems to be indicated. Second, individuals who participated in this stone working tradition were present on the northwestern Olympic Peninsula ca. 3,000 years ago.

Faunal Assemblages

The idea that different cultures have different economic adaptations is central to anthropological thinking. It is therefore neither original nor surprising to suggest that faunal assemblages recovered from these sites might offer insights into the ethnic identities of the populations that generated them. In fact, the idea that different native groups on the Olympic Peninsula had different economies has been a common feature of the anthropological literature. While rarely quantitative in any sense, the available accounts offer some basis to at least rank the relative importance of different resources for different groups. The most extensive such study relevant to this discussion is Singh (1956) which examined the available ethnographic and historic literature and interviewed ten tradition elders from the Makah, Quileute, and Quinault Tribes. His analysis includes an assessment of the most important resources for each group and the most important resources for Makahs and Quileutes are summarized in Table 2.

Singh found strong differences between the Makah and the Quileute resource base. The two groups targeted many of the same resources, but each relied upon a different mix of them. Overall, the Makah appear to have had a much stronger emphasis on marine resources. Note that the top five ranked Makah resources are all marine animals; only one of the top five Quileute resources is a marine animal. Ranked resources which only appear in one of the group lists are also revealing. Halibut and cod—both offshore fish—are among the top five for Makahs; neither is listed for Quileutes. In contrast, camas and fern roots, steelhead, and smelt only appear on the Quileute list. While the available archaeological faunal assemblages contradict some of these assignments, the distinction in economic orientations is nevertheless clear.[4] Classifying the native cultures by their economies, or related economic ideas, continues to be a part of how archaeologists think about them. Recall that Schalk's (1988) overview of the prehistory of the Olympic Peninsula

[4] Why the faunal assemblages differ from Singh's reconstructions is not completely understood, but

Table 2. Singh's (1956) Rankings of Important Resources for Makahs and Quileutes

Ranking	Makah	Quileute
1	Whale	Silver and King Salmon
2	Halibut	Elk and Deer
3	Harbor Seal	Whale
4	Cod	Camas and Fern Roots
5	Shellfish	Berries
6	Fur Seal	Steelhead Trout
7	Silver and King Salmon	Fur Seal and Smelt
8	Sea Lion and Porpoise	Shellfish
9	Berries	Harbor Seal
10	Elk and Deer	Sea Lion and Porpoise

classified the early historic Makah as maritime collectors. The Quileute were not directly addressed by this classification; neither maritime nor riverine collectors, presumably they were intermediate between the two types. While Schalk's discussion of these land use strategies focused heavily on the issues of mobility and seasonality, it is not unreasonable to suggest that the visualized types expressed in the landscape of the Olympic Peninsula would generate detectably different faunal assemblages.

Efforts to use archaeological faunal assemblages to examine possible ethnic differences on the Northwest Coast have thus far been limited, but McKechnie and Wigen (2011) have shown regional differences with long-term temporal continuity in assemblages from sites in British Columbia which probably have an ethnic dimension. Specifically, while marine mammal assemblages from sites on the west coast of Vancouver Island are dominated by fur seals, those from sites in the Gulf of Georgia are dominated by harbor seals and sea otters. The former pattern is thought to be associated Wakashan speakers, the latter with Salishan speakers. While of note on a large scale, this distinction is of little help in the present context as the marine mammal assemblages from Makah and Quileute sites are very similar.

Proceeding from this base, the effort to review Makah and Quileute faunal assemblages for possible ethnic signatures must still be approached with caution for many reasons. Not the least of these include both the very large differences in the extent of sampling at different sites and the fact that actual sampled volumes are small in most cases. The small sample volumes have been seen by some researchers (e.g., Schalk 1988:133) as: "far too small to produce samples necessary to document frequency differences in faunal assemblages." More recently, researchers like McKechnie and Wigen (2011) have been less concerned by this condition and were comfortable using faunal assemblages with NISP values of 50 or more. Reviewing the values in Table 1, note that all of the site faunal samples from the northwestern Olympic Peninsula considerably exceed this threshold. Most fish bone assemblages

at least two factors likely are important influences. First, virtually all of Singh's sources (living or written) reflect the late nineteenth and early twentieth century while the overwhelming majority of the archaeological assemblages are significantly older than this. Second, Singh's reconstruction of the Quileute economy addresses the tribe's entire territory while archaeological data is only available for a few of their coastal settlements.

alone dramatically exceed it, as do many of the mammal bone assemblages. Thus, I do not agree that the northwestern Olympic Peninsula faunal assemblages are too small to be of value. Still, small volume samples obtained from large site areas do raise concerns about whether they adequately address internal variability. It's a fair question and the truth is that small volume samples will always have difficulty addressing internal variability. In fact, even our expectations about the extent of internal variability we might encounter are colored by our view of the site's function, and our knowledge of site function is quite limited in some cases. The effect of all of this is that it is often difficult to independently assess how "representative" a sample is. Realistically, no excavator working in this region has ever claimed that their small volume samples were "representative" of the entire site they worked at. Presumably, they are "representative" of the deposits they excavated.

The preceding remarks are not offered to dismiss the problem. The problem is real, but—I believe—not disqualifying. I will shortly turn to a direct consideration of some aspects of the Makah and Quileute faunal assemblages, but no statistical analyses will be offered. Rather than attempting to reconcile or compensate for the above-noted deficiencies and then conduct detailed statistical assessments of individual sites, at this time I simply look for possible distinguishing characteristics. To this end, the ubiquity (given as a percentage of the sites in each group which contain it), the frequency range (given as a percentage of similar animals in each site), and the mean value for that frequency range for selected animals are presented in Table 3.

While all of the fish bone assemblages share many characteristics, there do appear to be some possible distinctions between Makah and Quileute sites. At least four fish—halibut, dogfish, cod, and herring—have both significantly higher ubiquity and relative frequency values for Makah sites. Only smelt exhibits a similar pattern for Quileute sites, although its values there are low. In considering the evidence for small schooling fish species (i.e., herring and smelt) it is important to note that their values represent samples recovered using ¼ inch mesh screen. Limited use of ⅛ inch mesh at 45CA23, 400, and 420 indicates that the former technique dramatically underrepresented the presence of these very small bones. Densities of greater than 100,000 per cubic meter are common, making them—by far—the most abundant fish bones in these sites.

The mammal assemblages are quite similar and closely approximate the western Vancouver Island pattern noted earlier by McKechnie and Wigen (2011). Marine mammals dominate all of the assemblages and fur seals are particularly prominent. One possible distinction among mammals may be porpoise and/or dolphin bones. Evidence of these animals have been identified in most of the Makah sites, but they have not been reported from a documented excavation in Quileute territory. Another apparent distinction is deer and elk bones. While both groups of sites have high ubiquity values for cervids, these animals represent a substantially larger fraction of the mammal assemblage in most Makah sites. The relative frequency of deer and elk bones in the Quileute sites is very low.

The bird bone assemblages from these sites are usually both the smallest and the most taxonomically diverse of the represented faunal groups. Some of the older Makah sites depart from this pattern, but it is true of all of the younger sites and some of the latter as well. Given these conditions, possible trends in them are

Table 3. Faunal Resources Which May Distinguish Makah from Quileute Sites

	Makah Sites			Quileute Sites		
	Ubiquity	% Range	$\bar{x}$ of %	Ubiquity	% Range	$\bar{x}$ of %
Halibut	92.3	0.4 – 60.3	10.3	25.0	2.5	-
Dogfish Shark	84.6	0.9 – 11.3	5.1	75.0	0.2 – 0.8	0.6
Pacific Cod	61.5	0.04 – 11.0	2.6	25.0	1.1	-
Pacific Herring*	61.5	0.1 – 17.2	4.7	-	-	-
Surf Smelt*	-	-	-	25.0	3.8	-
Porpoise/ Dolphin	76.9	0.1 – 43.2	7.2	-	-	-
Elk/Deer	92.3	0.6 – 35.0	13.4	75.0	0.2 – 7.1	3.0
Scoters	100.0	6.6 – 38.4	18.4	50.0	8.9 – 16.6	12.7

* ¼-inch mesh recovery only; see comment below.

harder to discern. Nevertheless, at least one possible distinction may be apparent here as well. Scoters—large sea ducks—represent a significant portion of the bird bone assemblage at every Makah site. They have only been identified at two of the four sampled Quileute sites, where their relative frequency values are low compared to most Makah sites.

In sum, the available faunal assemblages suggest that there may be some consistent differences between Makah and Quileute sites. Bones representing halibut, dogfish, cod, herring, porpoise and/or dolphin, deer and elk, and scoters all appear to be more common in Makah sites. They are all also frequently encountered in sites on the west coast of Vancouver Island. Evidence of them in Quileute sites is more limited or wholly absent. In contrast, at least one Quileute site contains evidence of the use of smelt. Smelt bones have never been identified in Makah territory and are very rare in sites from western Vancouver Island. Assuming for the moment that such differences are real, what might they mean? They are unlikely to reflect differences in resource availability as dogfish, cod, herring, porpoise and/or dolphin, deer and elk, and scoters are all widely available in the region. While Makahs are often cited as having better access to halibut banks, halibut are available in Quileute waters as well. Given the differences in sample size involved, it is certainly possible that some of the apparent differences in assemblage content are due to the more limited Quileute samples. This is most likely to be the case for taxa which are only present in small numbers in the larger Makah samples. Sampling effects of this type, however, are unlikely to account for the apparent patterns exhibited by halibut, dogfish, herring, smelt, deer/elk, and scoters. Thus, some differences in the preferred economic targets may be indicated. Note that some of the possible distinctions are consistent with the ethnographic-based reconstructions offered by Singh (1956). For example, halibut was very highly ranked for Makahs and not listed for Quileutes. Smelt was listed for Quileutes, but not for Makahs. A detail which is not consistent is the role of elk and deer. Singh gave these animals a very high ranking for Quileutes and a much lower one for Makahs, yet their ubiquity and frequency values are higher in Makah sites. If this pattern is real, it may be an indication that Quileutes conducted most of their terrestrial hunting from settlements located in the interior and focused primarily on marine resources when they were out on the coast. In contrast, Makahs—lacking interior settlements—probably conducted most of their terrestrial hunting from their coastal settlements.

While none of the latter appear to dramatically alter our overall view of the represented economies, a number of differences are apparent. In comparing the two groups, yet another inherent bias must be kept in mind. The much larger sample of Makah sites includes the relatively wide range of the settlement types and environmental settings occupied by Makahs and their ancestors. This is not true for the sampled Quileute sites. All of the Quileute faunal assemblages currently available represent shell midden sites on the outer coast. As yet, there have been no significant investigations of Quileute sites in coastal river valleys or elsewhere in the interior. This is unfortunate as ethnographic accounts clearly indicate that most Quileute settlements and the majority of the people were located in the latter areas. Thus, it should be clear that the available Quileute faunal assemblages have a strong coastal bias and offer only limited insight into Quileute economic activity in the broader sense.

The preceding discussions represent an exploration of ideas regarding the possible ethnic identities of populations represented by sites on the northwestern Olympic Peninsula of Washington. No definitive findings are claimed. While the data sets employed are flawed and/or otherwise limited, potentially important patterns may be indicated. In particular, multiple lines of evidence suggest connections between the precontact Wakashan speakers of the west coast of Vancouver Island and the precontact occupants of Makah territory. This concurrence is seen as strengthening the overall case even if individual elements of it retain problematic details.

Discussion and Conclusions

This paper has examined the related ideas that: (a) Makahs arrived on the northwestern Olympic Peninsula as recently as 1,000 years ago and (b) they displaced Quileutes who had previously held those lands. These ideas rely heavily upon linguistic and ethnographic—rather than archaeological—data and a detailed review finds them to be deeply flawed. The available archaeological data from the northwestern Olympic Peninsula—while less than definitive—offers no support for either claim. The case presented here is that the ca. 1,500 to 4,000-year-old assemblages from Makah territory are far more likely to represent Wakashan speakers than Chimakuan speakers. If this proposition is accepted, then it has potentially important implications for the histories of Makahs and Quileutes, and for the Wakashan speakers on western Vancouver Island as well. The following sections address both groups and consider Vancouver Island first as it is the context for the northwestern Olympic Peninsula.

The Origins and Spread of Wakashan Speakers

Ideas about the origins and spread of Wakashan speakers in British Columbia have been generated by both linguists and archaeologists. As is the case with the Olympic Peninsula, linguists claimed to have identified a number of historical conditions prior to the existence of relevant archaeological data, and thereby provided a context to frame the latter. Recent archaeological research has made important contributions to understanding the precontact history of western Vancouver Island and the available data from this region significantly exceeds that for the Olympic Peninsula in most respects. Nevertheless, the available data sets do not clearly and unequivocally support earlier linguistic reconstructions. To some extent this may be due to reliance on questionable linguistic techniques such as glottochronology, but other strictly archaeological issues are also involved.

The Wakashan Language Family includes two major groups: the northern Wakashans (the Kwakwaka'waka, Heiltsuk, Ooweekeno, and Haisla) and the southern Wakashans (the Nuu-chah-nulth, Ditidaht, and Makah) (Thompson and Kinkade 1990). Some linguists have suggested that the original homeland of Wakashan speakers was on and/or near the northern end of Vancouver Island (e.g., Suttles and Elmendorf 1963; Foster 1996). Swadesh (1954)—using glottochronology—estimated that the split between the two groups occurred ca. 2900 BP. More recently, Embleton (1985)—using a revised form of glottochronology—estimated that the split occurred ca. 5500 BP. Regardless of the timing, Wakashan speakers are seen as dispersing out of this ancient homeland with the southern Wakashans spreading southward along the west coast of Vancouver Island. Ultimately, this movement extended across the Strait of Juan de Fuca to the northwestern Olympic Peninsula. Given this broader context, the Kinkade and Powell (1976) claim that Makahs arrive from Vancouver Island ca. 1,000 years ago can be seen as the final step in this larger southward movement of Wakashan speakers and/or Wakashan ideas.

If the linguistically-generated temporal estimates are compared with the radiocarbon dates associated with West Coast Culture Type assemblages, two contrasting models appear to be possible. A principal point of distinction between them is which estimate of the antiquity of the split is used. The Swadesh estimate of ca. 2900 BP is a problem for the West Coast Culture Type as it is significantly younger than the ca. 4,100-year-old deposits at DjSp1, thought to represent a very early Nuu-chah-nulth culture. Given that this site is more than 100 miles south of the northern end of Vancouver Island, a still earlier data might be expected for the earliest southern Wakashans. In contrast, the Embleton estimate of ca. 5500 BP easily accommodates a Nuu-chah-nulth presence at Yuquot ca. 4100 BP. This same uncertainty has very important implications for the question of when Wakashan speakers arrive on the Olympic Peninsula. If the Swadesh estimate is accurate, this is a fatal flaw for the idea that the older assemblages from Makah territory are likely to represent Wakashan speakers. At least five of the sites have dated deposits older than 2900 BP. Alternatively, suggesting an association with Wakashan speakers is not a problem for the Embleton estimate. It easily accommodates these sites.

Unfortunately, the available data from Vancouver Island are not sufficient to resolve the matter. A principal difficulty is the absence of data from the linguistically-identified homeland of Wakashan speakers on and/or near the northern end of Vancouver Island. While there has been a significant amount of recent work in Barkley Sound and elsewhere on the southwestern coast of the island, we lack excavated data from sites north of Yuquot. Thus, archaeological corroboration of the claim of a Wakashan homeland on and/or near the northern end of Vancouver Island has yet to occur. This uncertainty notwithstanding, there are still clear indications that Wakashan speakers and/or Wakashan ideas were spreading southward on the outer coast in recent millennia.

Beyond the admittedly critical issue of timing, it is important to ask: *what is going on here?* While originally visualized as a movement of Wakashan speakers from the north displacing earlier Salishan speakers, more recent discussions of this subject consider that an adoption of Wakashan cultural traits by some local Salish groups may also have occurred (McMillan 2003a). Actual movements of people on the coast have typically been thought of in economic terms. Swadesh (1948) suggested that much of it was associated with control of important salmon streams. Presumably, movements of this type were often associated with warfare and the presence of what appear to be defense sites on the west coast of Vancouver Island supports this view. Precontact fighting between Makahs and Nitinats on the northwestern Olympic Peninsula should been seen in this light. Alternatively, Arima (1988) has argued that the development of effective offshore fishing and hunting technologies favored Wakashan expansion into new territories on the outer coast. While this latter dynamic may have included warfare as well, it also embraces the idea that the new Wakashan economic technologies were adopted by nearby non-Wakashan groups. Note that this is closely related to Croes' idea that economic stages developed across ethnic lines over broad areas of the Northwest Coast. In fact, McMillan (2003a:257) cites several examples in oral histories from western Vancouver Island where the adoption of Wakashan ideas went considerably further and local Salish groups began to speak Wakashan languages. He refers to this as "linguistic capture or cultural assimilation of other groups." MacLean (2012:139) has suggested that a very similar process is represented in some of the older Barkley Sound sites and described it as "cultural fusion."

Clearly, additional work is needed on Vancouver Island. In particular, the investigation of older sites in the linguistically-identified homeland of Wakashan speakers at the northern end of Vancouver Island is needed in order to directly evaluate this idea. Equally clear, Mitchell's original views of the prehistory of southern Wakashan speakers and the archaeological assemblages which represent them need to be refined. Significantly more variation, both spatial and temporal, is now apparent. This is not taken as an indication that Mitchell's view of long-term continuity is necessarily wrong. Rather, it may simply be an indication that the history of this region is more complicated than he appreciated. Since the focus of this paper is really the northwestern Olympic Peninsula, I will make few predictions about future findings on Vancouver Island, but if the ideas considered here are correct, at least two thoughts can be offered.

First, I believe that work at older sites on the northern end of Vancouver Island will show that the offshore economies and technologies associated with southern Wakashan speakers significantly predate the Swadesh estimate of ca. 2,900 years for the separation—and inferred southern expansion of—these people. While such a finding would not necessarily mean that the timing of the movement is wrong, it would certainly indicate that it could have happened earlier. In my view, the idea that the development of offshore fishing and hunting technologies favored the spread of Wakashan speakers is correct and this was likely happening prior to 2900 BP. Finally, while Yuquot is not located within the linguistically-identified homeland of Wakashan speakers, the early deposits at DjSp1 are also important to our understanding of their origins. Further study of this site is needed.

The only other prediction I would offer is that additional sites containing evidence of the production of bipolar microliths from vein quartz pebbles will be found on the west coast of Vancouver Island, but not elsewhere in British Columbia. I do not think that they will ultimately be found to be numerous or widespread, but I do believe that at least a few more will be found. I expect them to be found in deposits ranging from ca. 3,500 to 1,500 years ago. They will not be found in late precontact middens.

The Northwestern Olympic Peninsula

The available archaeological data from the northwestern Olympic Peninsula, while less than definitive, offers no support for the related ideas that Makahs arrived as recently as 1,000 years ago and that they displaced Quileutes who had previously held those lands. I accept that the Makah's ancestors crossed from western Vancouver Island to the northwestern Olympic Peninsula at some time in the past; the unresolved questions concern the nature of this movement, when it occurred, and how it affected groups already present there. In examining the record, the first thing to be noted is that there is no evidence suggesting the arrival of a new group on the northwestern Olympic Peninsula approximately 1,000 years ago. The older assemblages in Makah territory do exhibit a clear difference from the more recent ones: a significantly greater use of chipped stone and, particularly, considerable use of small bipolar flakes made from vein quartz pebbles. Precisely when this earlier pattern ends is uncertain, but it appears to have persisted until ca. 2000 to 1500 BP. Thus, the basic character and timing of this transition resembles that seen on southwestern Vancouver Island.

The apparent similarities and timings of these conditions on both sides of the western end of the Strait of Juan de Fuca beg the question: *to what degree do they reflect the same process?* Indeed, it is difficult to imagine that they are wholly unrelated. Nevertheless, current thinking about them emphasizes different ideas. As noted already, the change in assemblages on Vancouver Island is usually associated with the movement of Wakashan speakers and/or the diffusion of Wakashan ideas to non-Wakashan groups. On the Olympic Peninsula, the change in site assemblages has been suggested to reflect a change in site functions, driven by local environmental changes (Wessen and Huelsbeck 2015). Given the strong continuities in the bone and ground stone technologies and in economic orientations, the change in the use of chipped stone may have been a relatively small change in the represented cultural system. Several different characteristics of the older Makah assemblages suggest ties to the north. They include connections with both western Vancouver Island and the Gulf of Georgia region. The associations with western Vancouver Island are clearly dominant however and this likely indicates a Wakashan presence. Further, if Croes' ideas about basketry's ability to reflect ethnic identity are sound, then it is not unreasonable to suggest that these assemblages represent Wakashan speakers on the northwestern Olympic Peninsula (as opposed to an unrelated local group adopting

Wakashan marine fishing and hunting technologies). An important point should be emphasized here: the inferred ethnic identification is for Wakashan speakers in a general sense, not for Makahs specifically. Oral historical sources place the Makah, the Nitinat, and possibly other Wakashan speakers on the northwestern Olympic Peninsula prior to historic contact. This suggests a significant amount of movement by Wakashan speakers between the northwestern Olympic Peninsula and western Vancouver Island. Given the current difficulties associated with merely establishing the presence of Wakashan speakers, no effort is made to distinguish among them.

What, if anything, do the ideas considered here suggest about the origins of the Quileute people or Chimakuan speakers generally? The principal obstacle for this question is that the data available from Quileute sites offer only a very limited basis to suggest possible archaeological signatures for Chimakuan speakers. Indeed, much of this paper has focused on the proposition that it may be possible to recognize Wakashan speakers. Given our current capacity to suggest possibly distinctive assemblage characteristics for Chimakuan speakers, it is difficult to say much about Quileute origins.

While accepting that the earlier Makah assemblages represent Wakashan speakers means that their arrival was at least 4,000 years ago, it is unlikely that they were the first people to occupy this area. Very little information is available about still older occupation of the northwestern Olympic Peninsula, but recall that 45CA432—a mid-Holocene site excavated near the northern end of Lake Ozette is considered to be a representative of the Olcott Complex (Conca 2000). The latter is a localized western Washington expression of a widespread Early to Middle Holocene archaeological assemblage also referred to by such terms as the Old Cordilleran Culture (Butler 1961) and the Pebble Tool Tradition (Borden 1975). Ethnic identifications at this time depth are particularly tenuous, but a few researchers have suggested an association with Salishan speakers (e.g., Carlson 1990). Thus, the evidence for a prior presence of Salishan speakers on the northwestern Olympic Peninsula, albeit limited, is stronger than the evidence for a prior presence of Chimakuan speakers.

Currently, there is no archaeological reason to suggest that Chimakuan speakers were ever widespread on the Olympic Peninsula or elsewhere on the Northwest Coast. In this light, the strong similarities in Late Precontact Makah and Quileute assemblages may be seen as an indication that Quileutes were adopting cultural traits from their Wakashan neighbors. Quileute oral history also records evidence of this. Andrade (1931:204) presents a story where Quileutes learned how to hunt fur seals from the people at Ozette. Pettit (1950:5) cites a Quileute informant who claims that whale hunting was also learned from the Makah. Similarly, Frachtenberg (1921) reported that the Whale-Hunter Society—and at least two other Quileute ceremonial societies—were introduced to them by the Makah Indians. Thus, it is possible that a process similar to the cultural assimilation of local Salish groups on Vancouver Island described by McMillan (2003a) had also effected the Quileute. This is not to suggest that Quileute people were destined to become Wakashan speakers—had historic contact not occurred—but it is likely that Quileutes did adopt cultural behaviors from their Wakashan neighbors. Frachtenberg's (1921) analysis led him to conclude that the Quileutes originally had an adaptation oriented primarily toward terrestrial hunting in the interior of the Olympic Peninsula and only later expanded onto the coast. This interpretation is consistent with the ideas considered here. Specifically, the Quileute people may have had only a limited interest in marine resources prior to their encounter with the sophisticated maritime technologies of the Wakashans and only expanded their coastal presence after this occurred.

Confirmation of these ideas will require additional archaeological data. In this regard, while Makah territory is hardly well-studied, there is a much greater need for further research in Quileute territory. Beyond a better appreciation of the existing sample of sites, study of both older coastal deposits and sites representing riverine and other interior settings is also badly needed. In the absence of such activity, it will be difficult to learn much more about the precontact history of the Quileute people.

If the ideas considered here are correct, then it is possible to offer some predictions about what future archaeological work in the region is likely to find. First, I believe that additional faunal analyses will provide more support for the view that there are some consistent differences between the economies of Makahs and the Quileutes and that fish such as halibut, dogfish, herring and smelt, as well as deer and elk, and some bird species may be important distinguishing traits in their coastal sites. These conditions notwithstanding, we will still not have a balanced view of the precontact Quileute economy until data from their interior occupations is also available.

Second, I suspect that additional research will show that the late precontact marine fishing and mammal hunting activities in Quileute territory are older than we currently appreciate, but that they are not as ancient as similar activities in Makah territory. While evidence of still older coastal occupation will probably be found in Quileute territory, the earlier use of marine resources will be less intensive and less offshore-oriented than contemporaneous coastal occupations farther to the north. On a still broader level, I expect that a much improved data set for Quileute territory—including better dating of both coastal and interior sites—will provide evidence of an expanding coastal presence after the arrival of Wakashan speakers (as first suggested by Frachtenberg). Given that Quileutes claim to have learned fur seal hunting from Wakashans and that this activity is represented at 45CA32 ca. 1,100 years ago, a still earlier date for the predicted expansion onto the coast is implied.

Finally, the ideas considered in this discussion also suggest somethings about the character and distribution of certain artifacts. Of particular note, I have suggested that the bipolar reduction of vein quartz pebbles to make small microliths may be a marker of Wakashan speakers. I therefore expect that additional sites containing evidence of this activity will be found in Makah territory, but not farther south in Quileute territory, nor anywhere else in western Washington. Similarly, if older wet sites are found in Quileute territory, I expect that Chimakuan basketry will exhibit some differences which allow it to be distinguished from Wakashan basketry.

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Not Even Hearsay? The Oral Narratives of the First Nations of British Columbia

David Henige

Abstract *Numerous claims have been advanced recently that some content in the oral traditions of the First Nations of British Columbia accurately recollects periods as long ago as the last Ice Age. Since such claims have not been validated anywhere else, it seemed worth looking into the matter. I discuss the possibility that virtually all are products of feedback from outside sources that has been assimilated into First Nations' stories since as early as the seventeenth century. I regard this hypothesis as considerably more plausible than a scenario requiring such large numbers of oral transmissions. Astonishingly, proponents use no written sources for their arguments, or attempt to trace any traditions farther back than the nineteenth century. Great swathes of crucial materials have been ignored—missionary journals, travelers' accounts, newspapers, and relevant archives. Under the assumption that all sources for a given argument should routinely be addressed, I conclude that First Nations' traditions cannot be accepted as historical before the early nineteenth century, possibly even later.*

I

In December of 1997, the Supreme Court of Canada issued its ruling on a case (= *Delgamuukw* III) first adjudicated six years earlier by the Supreme Court of British Columbia (= *Delgamuukw* I), in favor of the appellants, members of the First Nations. The case featured a number of academic supporters, denominated "expert witnesses" for the occasion. While the evidence and the arguments in 1997 were much the same as they had been in 1991, this time the verdict was just the opposite, and the testimony proffered by the plaintiffs/appellants about their communal past was upheld.

Given that I, and many others, had all failed to demonstrate that oral traditions were credible historical sources, I was taken aback by this intrusion by the courts, and in this manuscript I discuss the reasons why.[1] I am not qualified to express any opinion on, nor do I have any interest in, the legal ramifications of the 1997 verdict. Instead, I concentrate on the extraordinarily tenuous relationship between the judgment and other evidence purporting to support the 1997 verdict—not least of all, evidence that was never brought to the table. No doubt it was gratifying to read in the court's verdict that oral narrative, "passed on through an unbroken chain across the generations," can lead the way to "historical truth," but can it? This is akin to pontificating on the niceties of medieval canon law without being able to read Latin.

It is not very surprising that advocates of the long-term stability of oral materials take this as a judicial *imprimatur,* as perhaps it was intended. Unfortunately, this claim completely lacks evidentiary support, at least before the period ending ca. 1800. The "expert witnesses" consulted only a small proportion of the relevant evidence and offered no excuse for completely ignoring the careers of these traditions from the Ice Age until the nineteenth century, or, for that matter, how they reached the twenty-first century without a scratch.

One awkward problem with this reasoning is that it breathes new life into the long-discredited notion of "the ethnographic present," which anthropologists devised to explain the lack of reported history in oral societies. In effect, it automatically abolishes the past of these societies by claiming that, during centuries of endless equilibrium, they never changed, so that studying them in the 1930s was tantamount to studying them in the 1630s. Other scholars put paid to this disparaging notion by supplying an amplitude of evidence for such change, but there was damage to undo. The UBCers (as I dub them for convenience) are at pains to emphasize the dynamic and pragmatic aspects of First Nations' culture at present, so that, when looked at holistically, the proponents' arguments reveal that unrecognized incongruities abound.

Observations about First Nations' resilience reverberate throughout the contemporary literature. Contrast this characterization with the implicit conclusions of the UBCers, which relegate the First Nations virtually to automatons, spending more and more of their time and energy presenting and repeating texts that meant less and less to them as time elapsed. This portrayal smacks of H. Rider Haggard more than it does real life.

By aprioristically condemning written sources as irrelevant or worse, the expert witnesses rid themselves of the considerable burden of canvassing thousands of printed pages. By treating outside contamination ("feedback") as both late and light, they limit the extent of the First Nations' exposure to outside influences to a much more restricted body of evidence than they can justify. By limiting all comparisons to western Canada and environs, they forego the chance to test the plausibility of their findings by drawing from a comparative sample of hundreds of similar cases around the world. By failing to develop a consistent nomenclature, they confuse their audiences. The first and foremost purpose of this essay is to apply overdue devil's advocacy, which should be a central fixture of all scholarly endeavor, but is not—certainly not in this instance.

The first argument here is that echo chambers have no place in the academy. The second is to suggest that, to make a case for the continuity of oral tradition, it is essential to track oral traditions *over time* and, in trying to do so, never to eschew exploiting any source simply because it relies on written evidence. Finally, I look at the potential for feedback in today's traditions, as well as in those in the past, and judge it to be longstanding, dangerously high, and routinely unaccounted for.

II

When early observers began to write about First Nations' oral literatures, they referred to them variously as "myths," "legends," "folktales," and "stories," and assigned them no historical value. When these materials later came to scholars' attention, they were rechristened "oral tradition," "oral history," "oral records [*sic*]," and the like, which, when properly decoded, can take audiences as far back as ten millennia or more.[2] As a result, it is believed in some circles today that, by listening to stories told by the First Nations of British Columbia, we can learn about things that happened as far back as the latest Ice Age. This belief necessarily presupposes oral sources that can *demonstrate* both antiquity and continuity on an unprecedented scale, so I looked forward to learning at last how this was possible; perhaps studying the evidence and arguments proffered by the supporters of these claims would dispel my reservations.

I discovered instead that these texts were seldom oral traditions *proprement dites* at all, but only oral texts glossed and re-glossed by modern hands . There are great numbers of unsupported assertions, but no references to the myriad studies that have been, and continue to be, carried out elsewhere, probably at least fifty to one hundred at any given time. Finally, I found that much argumentation has been carried out in, or in the toxic shadow of, courtrooms, where winning is always more important than being right, where there are very few shades of gray, and where legalistic protocol fatally circumscribes the amount of effort than can be expended in terms of evidence introduced or arguments advanced. Still, there are points in this enterprise worth noticing, and I devote the rest of this paper to discussing these, as well to justifying the above remarks.

In a tribute to Susan Marsden, Jay Miller encapsulates the guiding goal of the UBCers well:

> Her greatest insight has been the recognition that this sequence fills thousands rather than hundreds of years of culture chronology and consists of actual "history," in the sense that these events involved real (biological) people, places, emotions, and motivations. (Miller 1998:671 n5)

To some, however, this expedient might well seem more like a latter-day twist on the age-old expedient of euhemerism. In any case, it has the same stimulus—to fill a vacuum (Henige 1975).

For the Ice Age window to be open, certain assumptions are required, of which the most crucial—as well, presumably, the most obvious—is that we know enough about the reasons and circumstances that create what become oral traditions with the passing of time. It then becomes imperative to propose *plausible* ways (Henige 2004) that such texts can withstand scores of transmissions, yet remain "intact." The concomitant hypothesis is that these kinds of situations have routinely happened among the First Nations, and that stories can indeed be transmitted unscathed indefinitely, even within largely literate environments. In turn, this requires at least two further guiding presumptions—that effective mechanisms for faithful transmission existed—and still exist—and that outside influences, however defined, however intensive, and however prolonged, were in sum insufficient to deter these mechanisms from being repeatedly and successfully applied. Absent these being at least arguably true, matters reach an epistemological stalemate and should not proceed.

III

I begin by reminding readers of a similar flurry of interest in learning about precolonial African history by interviewing native informants. Although a generation of African historians emerged in the 1960s and 1970s using this *modus* *operandi*, in the end the implausibility of the transmission of factual information, and especially the inability to prove (in the usual restricted sense of the word) that any claims backed by oral tradition were either right *or* wrong, led most subscribing Africanists to drop their claims and turn their attention to other, more prosaic but more certain, means of studying the past. For all practical purposes, pre-contact African oral historiography was moribund by 2000, although it lingers on as an example of the wasteful effects of uncontrolled enthusiasm (Henige 2007).

Other aspects of fieldwork practice by Africanists raised concern as well. To be "efficient," for instance, many researchers resorted to testimony gathered from several informants at the same time, creating opportunities for the worst excesses of small-group dynamics, in which force of personality or facile acquiescence so often prevail over reasoned discourse. Then, too, it proved impossible to persuade all researchers that asking leading questions was not actually a short-cut in the long run. Most unnerving of all, there was a belated realization that different, even contradictory, responses to the same questions, by the same informant(s), was normative, allowing researchers to choose the ones they preferred with good consciences. Along the way, they also learned that informants were eager to please their interlocutors—not really a good thing.

IV

Whereas even the most fervent Africanist proponents of the potential of oral tradition as historical evidence restricted their efforts and arguments largely to studying the past few hundred years, the UBCers, as noted, have a vastly more exalted goal—to trace the content of some traditions back a hundred centuries or more. While it is repeatedly alleged that First Nations' memories extend back as much as several thousand years, and have been preserved virtually pristine during this period, such affirmations are not accompanied by justifying arguments about motives, means, and ends. Readers might notice, as well, that the most extravagant claims are not proffered by First Nations members themselves, but by others on their behalf.

Andrew Martindale offers what appears to be the most uncompromising argument; referring to "oral records" ("records" not defined) as

> [l]egal documents [*sic*] owned by Tsimshian house groups, formally constructed by the lineage, validated by other lineages and passed on through word by word memorization and repeated iteration in ceremonial feats by the chief and his heir. (Martindale 2017:288)

Martindale does not specify what happens when a particular group is between heirs—or between chiefs for that matter, but this seems a fair question, since one or the other—or both—circumstances was likely to have recurred every fifteen or twenty years on average.

However, Martindale is far from alone in his assessment of First Nations' oral traditions. Here is a sampling of similar thoughts:

> These laws go back thousands of years and have been handed down from one generation to another... (Duff 1989:36)

> There can be little doubt that the ancestors of the Dene witnessed [the eruption of 20 CE or 720 CE] and that memories of this terrifying experience, ... were transmitted orally from generation to generation." (Moodie and Catchpole 1992:165)
>
> *Remembering 10,000 Years of History: The Origins and Migrations of the Gitksan*. (Harris 1997)
>
> [e]very generation of Gitksan chiefs is responsible for ensuring the full transmission to the next generation of the adaaw... (Sterritt et al. 1998:12)
>
> For 2,000 years, extended kinship groups... have passed on the history of a period of warfare and migration. (Marsden 2000:50)
>
> [t]his adawx most probably refers to a time when the mouth of the Skeena River was first inhabited after the end of the last ice age ago [or... at 8000 BP]. (Marsden 2002:109 n23)
>
> Some oral histories recorded events that occurred at least as far back as the last Pleistocene [~9000 to 15000 ybp]. (Kii7ijuus and Harris 2005:123, 124)
>
> It seems clear, however, that these texts also speak of real and not just imagined history with demonstrable accuracy perhaps thousands of years into the past. (Martindale 2006:159)

Somewhat oddly, some contemporary stories have thirty or more versions or variants, as though, after centuries of faithful, but mindless, repetition, individual creativity suddenly blossomed. Where there was once a single text carefully preserved, it became (when, how, or why not stated) a textual free-for-all. Many versions of particular stories appeared in print over the years, and became all but canonical by virtue of that alone.

V

According to Lorraine Weir (2016:182), Tsilhqot'in protocols enjoin that "[e]lders should not be asked why questions." In many ways this is sensible etiquette, since the whys of anything are often the most difficult to fathom and the most contested, and so the least likely to provide assurable answers. Just the same, asking questions of particular scenarios can generate further questions that might also be unanswerable, but that broaden horizons by their very posing. Doing this helps scholars make the strongest defensible case, along whatever lines they are led by the evidence, and might lead to sharper arguments.

This said, we can look at the current and continuing efforts to raise the status of orally-transmitted materials among the First Nations. The case for upgrading requires heavy lifting. In particular, it must, as already noted, overcome the absolutely critical issue of the chain of custody, by *demonstrating* that today's oral testimonies passed from mouth to ear to mouth for scores of generations without losing their virginity. It also seems to involve, if only by choice, disparaging and ignoring written sources as biased by the very act of being written.

There has been little or no discussion in the First Nations' enterprise about such basic methodological and fieldwork aspects as contamination through inappropriate (especially leading) questioning, or any systematic use of repeated interrogations to check for discrepancies. Nor (to my knowledge) have fieldnotes been deposited for easy access by interested parties, even though providing untrammeled access to sources is a *sine qua non* of responsible scholarship (Henige 1980b). Would a scholarly article relying on *written* sources and proposing a major reinterpretation of some text or a part of human experience be granted the same latitude? That seems unlikely, but more on written sources later.

Although they are seldom asked and never answered, the study of oral traditions as historical sources must deal constantly with "why?" and "how?" issues. Why does anyone (singular or plural) decide that something memorable has just happened? How is a tradition born—for the first time? Is the community gathered together and presented with an "official" version, and other versions are promptly bludgeoned out of existence? If so, by whom? Then how does the ensuing transmission process get under way? Do the original transmitters (who were?) get the role through seniority, wealth, or demonstrated memory skills?

At any rate, a particular group eventually stockpiles enough memories to achieve critical mass. Then along comes another event—a spectacular volcanic eruption or a total eclipse, for instance—that demands a similar response, and so on. Eventually the collective memory runs out of steam, and some memories have to be jettisoned, but which ones? Despite all the attention latterly paid to natural phenomena, no one at the time (which could have been *any* time after the "first" eruption) could understand just how formally and/or collectively recalling an eruption or an eclipse several generations after the fact had proved beneficial to the greater good. No one then alive had ever seen an eruption, even knew what an eruption was, and so were predisposed to forget it, and poof! a few generations later it was gone. These cycles of experiencing, deciding, and preserving would go on at ever greater social, economic, and political cost, as local carrying capacity is reached, requiring constant modifications to the must-remember list.

A second corollary implicit in the claims noticed above is that oral traditions were able to exist in an impenetrable vacuum from their inception until the continuing present—that no external influences were permitted that might exercise a distorting influence. A little thought—and innumerable counter-examples—should have served either to torpedo this notion at birth, or at least to move its deniers to defend their denials.

VI

One imagines that any scholars working on the career and meaning of Indigenous oral traditions would dedicate at least some of their efforts on acquiring as many of these as possible from as many sources as possible, and from the very beginnings of contact and proselytization. Trying to do this should rate high on UBCers' to-do lists. For instance, it seems to me to be worth knowing (courtesy of

Streit and Dindinger 1916, vol. 24:392) that the prolific and highly regarded Fr. A. G. Morice (Carrière 1972; Mulhall 1986) published a text entitled "The Great Déné Race" in several instalments between 1906 and 1910 and aggregating to about 250 pages. This appeared in *Anthropos*, a German anthropological journal widely available in North America, as well as on JSTOR. It might even be worth comparing the stories told there with his other work published at almost the same time (Morice 1904), looking for suggestive differences, on the premise that the printed word does *not* speak for itself. Another opening gambit might be to compare the nearly 150 "traditions" recorded by Ḗmile Petitot (1886, 1970) to determine how many are still in circulation and in what shape,

Certainly, this is what occurred in other areas of the colonized world, as scholars sought to understand the reciprocal impacts on colonizers and colonized. In the process, missionaries served as the first anthropologists and linguists, so it is no surprise that their writings garnered attention once the academic study of oral societies began to thrive. It is regrettable that UBCers have resisted the manifold opportunities offered by this extremely rich and copious body of literature, which is unparalleled in its continuity and detail, but in the UBCers' writings, conspicuous only by its absence. To a person, it appears, they reject utilizing or even consulting the plenitude of potentially useful printed literature from the period before about 1950. As a result, we have no sensible idea for western Canada about how germane and voluminous these unmined materials actually are.

Take the case of the major Roman Catholic presence in early British Columbia, the Oblates of Mary Immaculate (O.M.I.), who published about a dozen different journals at one time or another, often in overlapping runs (Henige 1980a). Extensive to complete holdings of these are housed at the Université Laval in Quebec, waiting to be consulted. The O.M.I Archives Deschâtelets in Ottawa (omifamily.org) has similar, though more archival-like, holdings. There is even an O.M.I. center in Vancouver. We cannot know just what kinds of materials research at these, and other, repositories would uncover. It is possible—just—that these thousands of pages happen to include no useful observations on First Nations' lifeways, even by those immersed in First Nations matters, but it is incumbent on those who choose not to exploit these sources to state clearly why they have not.

Then there are the effects of missionary schooling to be considered. (e.g., Bradford and Horton 2016). In 1900 there were some forty O.M.I. residential schools in British Columbia, and a total for all denominations of more than one hundred such schools (McNally 2000). Indoctrination was intensive and continuous, and designed to wean students from their backward ways by controlling the content of the curriculum. Is it reasonable to insist that First Nations students were willing and able to withstand this onslaught in order to preserve oral traditions unaltered, as though they sensed that in due course various scholars would be interested in them? If so, this too would have been unique, but this does not prevent the UBCers from implying such a scenario by avoiding consideration of the course of oral traditions before, say, the 1950s. Last and least, there are two gargantuan bibliographies of French local history journals (Lasteyrie du Saillant 1914; Gandilhon 1944–1961).

Dealing with these is likely to be excessively boring, but eliminating possibilities is an unavoidable chore of academic life. One way to minimize this task would be to accept well-conceived bibliographies as master's theses, or possibly as a project by library science students.[3] Whatever expedients are employed, it is necessary grunt-work to ensure that critical materials do not continue to evade detection. Catechizing, compiling dictionaries, and introducing new and seemingly more efficacious rituals were all activities that required close day-to-day contact. The results of this are probably most obvious in the numberless cases where Indigenous peoples borrowed from the Bible and sundry other printed sources to fill in gaps, but it occurred across the globe, and affected existing traditions in any number of ways, none of them auspicious from the historian's perspective.

Hypothesizing early and reciprocal acculturation, for example, allows us to suggest an interpretation of enigmatic observations like that of Daniel Williams Harmon (1904:251), who, early in the nineteenth century, noted that the "airs of many of these [in this case, Cree] songs, which have been composed, and set to musick by their poets... greatly resemble those which I have heard sung, in Roman Catholic churches." At the least, it broadens the interpretative path, by bringing more evidence to the table, making it more difficult, but more rewarding as well.

Fortunately, one of the greatest bibliographies ever compiled deals with Roman Catholic missionary activity throughout the world from the end of the fifteenth century through the middle of the twentieth century. Published in thirty massive volumes over half a century, *Bibliotheca Missionum* (*BM*) covers an enormous range of sources duplicated nowhere else (Streit and Dindinger 1916), and also incorporates considerable biodata, as well as thousands of citations—hundreds of which treat North America (Henige 1978). Consulting *BM* can hardly be avoided by anyone claiming any interest in this genre of sources. Using it will not be boring, whereas not bothering to use it is entirely unjustifiable.

This leads to one of the most significant *X*-factors of all—school textbooks, and their pervasive, but unaddressed, role in this process. Most of us have been influenced by our school textbooks more than by any post-adolescent reading. Yet no one has trained their fire on textbooks as a fulcral problem. In effect, they are the 800-lb gorillas in the study of this species of post-colonial orality. Comparing five versions of a Nlaka'pamux story about Simon Fraser's arrival in 1808, Wendy Wickwire (1994:20) characterizes them as accurate, and "span[ning] several generations." That is certainly one possibility; here is an alternative. Many Nlaka'pamux attended local schools, and among their textbooks were one or more that extolled the wondrous exploits of the intrepid British explorers of North America, complete with appropriate, but imagined, dialogue. As a result, Simon Fraser was introduced to Nlaka'pamux students, who assimilated the textbook account into their existing repertoire of stories, after adding their own touches.

This strikes me as rather a more plausible scenario than the incumbent one, in which traditions about minor occasions are scrupulously handed down for centuries unscathed. Testing this hypothesis will not be easy. It will require determining which textbooks were used in the schools Nlaka'pamux attended before 1950, then locating copies of them (and textbooks have notoriously fugitive half-lives), then scouring them to determine if any of them contain information on Fraser's visit. If so, the feedback hypothesis gains strength; if not, then the orally-transmitted interpretation does, but only by way of an argument from silence.

It is hardly an easy task, trying—hoping—to find nothing, but very much required in cases like this. And if enough textbooks are located, searched, and found wanting, the hypothesis is technically still valid. But we (and I include Africa, India, Oceania, and other former colonial realms) are very far away from being able even to begin this, since no one seems to have grasped this particular nettle, perhaps because, in some measure at least, it would jeopardize the implied position of the UBCers—and similar schools of thought—all by itself.

Finally—and as if to close the circle—we can hardly overlook the contamination introduced by numerous outsiders asking questions. For the past century and a half, fieldworkers of various stripes have been tramping around First Nations' territory gathering information, but also providing it. Marius Barbeau, for instance, held strong views about the Asiatic origins of the First Nations (e.g., Barbeau 1934; Nowry 1995:238–243). Are we to believe that he did not communicate these to the various groups he studied at the first opportunity, hoping they would "confirm" his hypothesis? And would not these "informants" simply add this interesting new titbit to their own ensemble of stories about the past? Then, when the next researcher came visiting, they reciprocated by regurgitating that information as their own, and so on... and on.

Given the numbers of scholars drawn to the area (Maud 1982), it would quickly have become impossible to specify communications channels among "informants" tidily, and impossible, as well, to distinguish among early and late introductions of data. As more and more time passed, so did any opportunities to disentangle points of origin, and it became impossible, and unwise, to consider that any of these details "corroborated" any others. They became impartible pastiches of the original (in the event that there ever *was* an original) and they introduced a *bouillabaisse* of the emic and the etic, the old and the new, the likely and the dubious, all homogenizing as time passed.

VII

My impression is that those studying the earlier experiences of the First Nations have fallen into the temptation to regard both their experience, and their assessment of it, as so unique that very similar processes which have occurred in many other times and other places are not worth the bother of identifying, studying, and evaluating. This is remarkably parochial and ill-advised, especially regarding ideas that influenced Indigenous views of their own past. They at least—and unashamedly—privileged the written over the oral when given a choice, and absorbed what they heard without a blush. We hear little or nothing from UBCers about the role of crucial transcultural agents, whether they were missionaries, hunters, military men, concubines, or traders. Yet these people were influencing—and being influenced by—each other's ideas on a daily basis for decades before the arrival of academic interrogators (e.g., Colpitts 2002)

Concurrently and complementarily, UBCers treat meaningful contact between the "West" and the First Nations as beginning only in the nineteenth century. Yet, there are no reasons for believing that the collective experience of the First Nations was any different than it was for other times and places in this respect. From all indications, the most turbulent and vulnerable period in the career of a typical oral tradition is the period from first contact until shortly after formal imposition of authority, a span of time that historically has lasted from a few years to several centuries, and is always subliminal. It is a period in which local interest groups scramble to secure the goodwill of their new or potential senior partners, not least by inventing new "oral" traditions designed to forestall rivals' efforts to do the same. In the process, of course, there were casualties, which usually comprised the evidence proffered by the unsuccessful claimants. While this has not happened in every one of the scores of cases for which data are available, it is easily the default scenario. Literate societies throughout history have acted and reacted in very similar circumstances, with the proviso that for written sources we have a greater chance of discerning this problem if it does happen.

The case for the *voyageurs* playing the role of trans-cultural agents is compelling. True, this contact seldom rose to being particularized in the surviving archival record, but there is more than enough evidence to suggest an overwhelming degree of contact and cultural transfer (e.g., Sleeper-Smith 2009; Havard 2016). Recent authors on the fur trade estimate that at times there were as many as 3,000 *voyageurs* swarming throughout western and central Canada and the U.S. Northwest. These are not irrefutable estimates handed down orally, but are based on voluminous (if not always complete) records from the time (Grabowski and Saint-Onge 2001). This tempts one to calculate some probabilities.

Carolyn Podruchny (2006:4–6; cf. Havard 2016:250–253) regards her estimate of 3,000 *voyageurs* in the late eighteenth and early nineteenth centuries as "a conservative approximation," but to be on the safe side, we can cut it in half—to 1,500. Even this, probably artificially low, number means that—assuming just a single contact a week on average—1,500 x 52) or 78,000 contacts *per annum* took place, even more if we assume that many of these encounters were more than one-on-one occasions. And that is just the men (e.g., Van Kirk 1983; Barman 2014; Barman and Watson 2014). What took place on these occasions? Again, it would be downright perverse to assume that nothing of interest occurred that could have led members of either side to rethink and recast previous worldviews—as, for example, most of Christendom was forced to do beginning the day after Columbus' discovery letter was published in 1493.

The Iroquois were key players—even superstars—in the western fur trade (e.g., Nicks 1978). Long involved in the northeastern Atlantic trade, they fanned out across northern North America, and soon hundreds of them were employed by the various fur-trading enterprises, during and after which many served in various roles in the trade, thereby bringing yet other player to the multicultural table. Largely Roman Catholics, some acted as catechists, sometimes for longer than twenty years (e.g., Mellis 2009).

Hawaiian Islanders also partook in the fur trade along the west coast of Canada and the United States, and even ventured at least as far as Lake Superior (e.g., Quimby 1972). From before 1775 until after 1820, numerous Hawaiians served one or another of the fur trading companies, and they were hardly alone. Many Hawaiians who engaged in the fur trade also traveled to the Far East, trading other goods like sandalwood (e.g., Shineberg 2014). For the UBCers, the premise can only be that on all these occasions each side merely incuriously observed the other from a distance (i.e., the ubiquitous myth of silent trade) and did not indulge in social intercourse. Again—just possible. It is much more likely, however, that members of all groups consorted with each other on virtually a daily basis, exchanging views on such matters as origins, as all parties took on board new information, transaction after transaction after transaction. This is not idle speculation, but has been a well-documented phenomenon accompanying westernization and the imposition of European rule every place in the colonized world.

Clearly, then, from at least the middle of the seventeenth century there existed a transcontinental—in fact, intercontinental—communications and mercantile network(s) ranging from New York state to Alaska and beyond, and operating off the grid. Despite their isolated location, the First Nations played a long and consequential role in the new trading patterns that emerged. In this intensive process, authentic First Nations' testimony about many things evanesced long before missionaries and western scholars reached the area, and, once lost, could not be retrieved. The uncongenial, but inescapable, fact is that oral narratives are not—in fact, *cannot be*—better sources for the distant past than written evidence, with all its inherent weaknesses and, even if it were, it would be impossible to know/show this. As many hundreds of examples from throughout space (the entire globe) and time (as early as ca. 1500 BCE) tell us, Indigenous authorities updated their views of their past *whenever* it seemed expedient, to correlate better with newly-unfolding presents (Henige 1982).

VIII

Is there any way that oral testimony purporting to be generations old could be *verified* as authentic and perhaps even as reliable? None comes to mind, and if it did, it would be another certifiable instance of *sui generis* in the historical record, despite more than a century's efforts. This state of affairs can occasionally be palliated, but never abrogated. Affording "equal footing" in any court does nothing to find the truth because, in this instance, all such claims are, as noted above, neither verifiable nor falsifiable—expressions of faith rather than of knowledge (Henige 2014). Exponents of the sufficiency of oral data to retrieve the past beyond certain limits are free to plead their case, but this should be accompanied either by successful efforts to disentangle the chain of custody problem—which would be yet another first—or build a case that doesn't require that. Neither prospect is in sight.

Without doubt, cases like these are epistemological nightmares, but they are hardly unprecedented. Each party has different conceptions of both "proof" and "truth." By the normative standards of scholarship, the plaintiffs have come nowhere near proving their case, if "proof" be taken to mean independent strands of disparate evidence that converge to mandate a single covering interpretation. On the contrary, there was no evidence before about forty to fifty years ago, or rather no argument, since there still remains no credible evidence over which to argue. Efforts to reach defensible decisions are hamstrung by the law's defective conception of the whole truth, which has more to do with points of law, precedents, and persuasion than with weight of evidence and/or argument.

IX

In 1976 and 1977 the small coastal village of Klemtu was wracked by a succession dispute—no small matter. Jay Miller, a linguistic anthropologist, happened to be in Klemtu during the height of the dispute, and found himself "in the midst of a fascinating social process, whereby two unrelated tribes... were fusing through the creation or at least the reorganization of matrilineal exogamous moieties." This was being achieved by "the suppression of certain kinds of knowledge by members of elite families." Miller went on that "the method adopted by the elite families was the expedient of "'forgetting' almost everything not congruent with matrilineal moieties." (Miller 1981; Roth 2001:82–85; cf. Miller 2014; Stapp and Powers 2014).

In light of the UBCers' extravagant defense of the fidelity of tradition for centuries, no matter what, this is interesting, bordering on fascinating. Here was an expedient—purposeful and calculated nepenthe—that can only be anathema to the UBCers; after all, it is diametrically opposed to their core tenet that very little was forgotten and certainly none of it intentionally. Maybe Miller got it all wrong? Maybe this happens to be the only known instance of such rewriting of history among First Nations? Maybe this only shows the baneful effects of colonialism? Maybe still something else? All possible, I suppose. But I prefer to see it as a microcosm of a very much larger, but no longer available, universe of examples of traditions being changed as often as necessary to keep in sync with changing local exigencies.

I suspect that any number of similar examples from recent times could be dredged up without much effort. But does anyone take the trouble to search out evidence that might damage their hypotheses? The typically widespread incidence of confirmation bias says no. There has been no effort to refute Miller, only to ignore him. Miller ignored himself as well. He was later to speak of "a sequence of at least fifteen episodic overlays across ten thousand years" (Miller 1998:657).

The Klemtu case seems to subvert entirely the notion of oral traditions passing smoothly from one generation to the next, allowing for heroic extrapolations of several centuries, even millennia. Which is it to be? It is counterintuitive to believe that there never was an inclination to change a few words in a tradition. Indeed, every occasion at which oral traditions were recited was also an opportunity to tamper with them to fit ever-changing circumstances. Has there been a restudy of this particular incident to determine which set of traditions is *now* incumbent?

X

As would be expected, the flurry of activities that accompanied the Delgamuukw hearings produced a large body of evidence for interested parties to peruse, question, accept, or reject. Only the third of these has actually happened. Most of these materials proved untestable and have no value for either side of the argument. For instance, in the opening statement to the first hearing, the two authors (Gisday Wa and Delgam Uukw 1991:32), wrote, with emphasis, that the names of hereditary Chiefs "...are not personal names. They are names which the Gitksan and Wet'suwet'en *can trace back* over the centuries..." They concluded that these names "stand among the oldest continuously held titles that exist anywhere in the world." The last sentence is clearly a case of feedback, but what matters is that these statements were never challenged; no one was asked actually to "trace back" the succession of chiefs or chiefly titles. Here was an excellent chance to gauge the value of such unattested statements by, for instance, having three chiefs, separately and simultaneously, "trace back" the "succession of Chiefs," presumably a simple matter. Instead, all were accepted as true on goodwill alone.

Later in the same publication, the authors tried to describe how the content of oral tradition passed from one generation to the next, thus "validat[ing] historical facts." They began with a rhetorical question: "[m]ust the Plaintiffs [i.e., themselves] own accounts of events which took place hundreds and thousands of year ago be deemed unscientific and mythical, mirages of reality, rather than the evidence of history?" They continued with a short *apologia* on how tradition was passed down:

> For the court... to deny the reality of Gitksan and Wet'suwet'en history except where it can be corroborated by expert evidence in the Western scientific tradition is to disregard the distinctive Gitksan and Wet'suwet'en system of validating historical facts. (Gisday Wa and Delgam Uukw 1991)

This is followed in turn by a brief exposition of how oral tradition was physically passed across the generations.

> ...the totality [*sic*] of the historical record exists in the minds of the chiefs that feast together.... In this way, the record of Gitksan or Wet'suwet'en history exists in its totality in the minds of those whose duty it is to remember it... when a Chief describes the events that took place long ago, the events that he or she could not possibly have witnessed, these can be told as established truth by virtue of having been tested [*sic*] and validated at a succession of narrations... (Gisday Wa and Delgam Uukw 1991)

The nature of this testing is not specified.

While this description of "remembering" is terminally incoherent, one thing is clear. There is zero accountability built into the system—no checks, no balances. Chiefs always know and tell the truth as they know it, simply by virtue of being chiefs. The failure to indulge in low-grade verification exercises suggests that the UBCers are not confident that any answers would have helped their ca(u)se. These lost opportunities—which never were seen as opportunities—preclude coming to a frank appraisal of the First Nations' oral narratives' historicity. The moment for doing this has now passed, since the roilings of the past thirty years have created new orientations and configurations for the pliable past.

XI

Chief Justice Allan McEachern's (in)famous declaration that written documents "speak for themselves" has inevitably drawn the fire of the UBC School, whose members also seem to believe that this attitude is rampant among those with an opinion on the matter. If so, they are emphatically out of touch. Thousands of literary scholars and historians make their living arguing about every aspect of written texts.

Hundreds of thousands, oh yes, commentaries on the biblical text have appeared in the past two centuries. Dozens of books and hundreds of articles just on Shakespeare appear annually. Historical texts, including inscriptions, are added or subtracted from the canon because of new readings, based on new evidence. No written sources have gained perpetual immunity in this enterprise, and none ever will. They will always be in danger of being spoken for.

Stories circulating among the First Nations at contact were about events considered memorable or were merely confabulated. Whichever the case, they were impacted and enriched by a wealth of cultural contacts between Europeans and First Nations' peoples for two centuries or more. Creation and transmission of such stories antedated "first contact," but were invigorated by the new opportunities that presented themselves. On these occasions, expedient changes took place to keep up with new exigencies. Meanwhile, during the lifetime of these stories—retroactively upgraded to "oral traditions"—they were shared among neighbors, and many of them became common stock.

After about 1900, newspapers and similar sources increased the capacity for the efficient exchange of information. That is how the First Nations first learned about the Ice Age. To speak of "corroboration" by contemporary oral traditions then is hopeful, but essentially pointless, given the extensive flow and counter-flow of information, both intramurally and extramurally, for centuries, so that there are no uncorrupted oral narratives available for study. In fact, the argument can be made that no uncorrupted texts exist after the first few days of their existence (e.g., Henige 1974; Ramsay 1977; Maud 1982; Merrell 2006). Thus, what exists today is an amalgam of version 5.0 texts that have proved helpful and malleable in the past, and those that bid to become so now and in the near future, although almost probably in different guises. One of the advantages of the argument here is that it precludes the need to devise mechanisms for handing them unchanged down the generations.

Looking at the matter from the perspective of probability theory, which, although not entirely comparable, can suggest some useful analogies. All would agree that at every transmission, informants have several choices, only one of which could be the "truth" as they know it. If we assume that the non-true choices average only one per transmission—surely often far too few—then the probability that the original truth emerges after fifty transmissions is 0.50 to the fiftieth power. And, of course, the probabilities would only get tinier and tinier as alternative options are multiplied. For instance, having two non-true options increases the odds to 3.33 to the fiftieth power, truly homeopathically tiny, etc.

If the objective in this debate is to pursue knowledge relating to the potency of oral traditions as historical sources, then it is effectively a waste of time; advocates can—and will—choose from among the propositions being offered, secure in knowing that they can never be proved wrong, but overlooking that they can never be proved right either. If the goal has nothing to do with learning about the past, then this doesn't really matter, but skeptics still deserve answers to pointed questions about fundamental assumptions. The UBCers' arguments cannot be falsified by anyone because no credible evidence to support or refute them is available and never will be available. On the other hand, neither can my scenario. However, the latter does have the advantage of plausibility on the one hand, and a wealth of supporting comparative evidence on the other. In the interim, there are entertaining intellectual pratfalls to enjoy.

XII

The past has frequently been characterized as "a foreign country," but I suggest that a more apt analogy would be a giant archaeological site—say, Vancouver Island—after thousands of years of despoliation by way of war, natural disasters, disease, and the routine tolls of time. There are fewer than twenty edifices still standing in various stages of dilapidation throughout the island, which encompass almost as many architectural styles. There is no obvious way to distinguish governmental from religious from residential structures. There are numerous works in writing that have survived in subterranean sites, but for some reason the archaeologists dispense with these as inferior and useless. These archaeologists of the future realize how tiny a portion of the once-existing whole they will ever have, but they try to reconstruct the site according to taste anyway. After all, they tell themselves, who will be able to dispute them?

At the same time, historians are investigating much the same area, but have not yet for some reason discovered any troves of writing. This allows them to *believe* that much remains to be unearthed. They are also aware that, like the archaeologists, they can build non-falsifiable hypotheses without bothering to defend them. After all, they tell themselves, who will be able to dispute them? The impasse continues, even though no one can limn a resolution without further evidence becoming available, and the unwinnable/unlosable debate grinds on. Frankly, from this perspective it is not easy to understand why the UBCers have chosen to defend their clients with such untenable arguments, redolent of reasonable doubt, at least when playing the game under the normative rules of evidence. Perhaps they sensed that the time was ripe to make an emotional appeal, and, ultimately, they were proved correct, seemingly rendering doubts about their tactics moot.

Winning in court has presumably lent an aura, at least temporarily, to the UBC enterprise, but at the same time it has lowered the bar—only temporarily, one hopes—in the larger debate over how evidence and argument can most productively be integrated and deployed. This paper is designed to temper the view that grants one body of oral narratives a cachet that they have not earned, and can never earn unless it can be *demonstrated*—at least once!—that 300+ faithful transmissions not only can, but do, happen (but see below). As it stands, however, well-documented historical experience, combined with modern experimentation, have pretty much shown that they never actually have.

Why write a critique like this many years after this case was closed, with an outcome that many people—including myself—find congenial? Simply put, it is because the process that led to this success was so deficient as to prove embarrassing to all those who value thoroughness and the practice of devil's advocacy as indispensable features of historical method, and who regard admitted ignorance as preferable to a deceptive posture of certitude, fostering the sense that we *know* much more about our past than we actually do. The results of this attitude are to stop looking for something because we believe we have already found it.

XIII

Were I asked to epitomize and characterize the UBC movement, I would refer readers to just a few words by Julie Cruikshank. In scolding Justice McEachern for ruling against certain First Nations' claims in the BC phase of *Delgamuukw,* Cruikshank charged him with "ignor[ing] *established* bodies of *knowledge*" (Cruikshank 1992:26, emphases added) and went on to deplore his use of "evidence." This is a disturbing point of view. It implies that the Ice Age claims have long been accepted by the experts, and so the subject is now closed, whereas it was only a scholarly generation or so before 1992 that the whole idea was first seriously bruited. This does not constitute being "established."

Cruikshank's choice of "knowledge" is particularly inapt, but also symptomatic, presumptuous, and wrong. "Knowledge," or rather its illusion, is a parlous and perilous concept, and the occasion for much unnecessary and unrewarding discourse. Many scholars who had the temerity to question the Clovis First orthodoxy in its heyday were hounded out of academia for their trouble. Now that the Clovis Man theory has been discredited, can we say it was *ever* "knowledge?" Of course not; in retrospect it was largely a waste of time—a chance to exploit the herd instinct to play out scholarly rivalries. The same can be said for the geocentric and rather compact universe, a 6,000-year-old planet, an Arthurian Age, angels, phlogiston—literally *ad infinitum*. In all these cases, a little doubt would have saved a lot of bother.

The most precious attribute of any source, including oral tradition, is its demonstrable independence, that is, a total lack of input from any outside sources, and therefore in a position to "corroborate" other testimony, written *or* oral. Unfortunately, this is impossible in today's communication-crazed world, including the First Nations of British Columbia. With a century or more of sharing and resharing, including absorbing feedback, there is simply no longer any way to identify individual strands.

In short, Cruikshank's confidence is largely misplaced, and frankly, unsupportable by the evidence. Believing in something does not by itself make it real; however illusory, the notion that we can wrest some aspects of human activity from the otherwise blank slate of prehistory is bound to be attractive. As a result, the notion of timeless but true oral tradition will prove popular to a wide readership, which might not appreciate either the wild implausibility of hundreds of verbatim or virtually verbatim transmissions of oral narratives, or the exiguous research on which these claims are based. Thus it will become "settled knowledge," a.k.a. myth, and the cycles of credulity will begin yet again, resulting in a need to remind interested parties that compelling conclusions require compelling evidence, rigorous method, and open debate.

For many, this paper will seem to be yet another occasion of what David Milward (2010:295) calls "a systematic devaluation of oral history evidence," and the present essay will certainly be seen as unwarranted and tendentious interference in a closed case. I regard it, however, as non-partisan. The argument is that a Manichaean approach to this particular topic—First Nations v. Europeans, oral v. written evidence—has led only to the present *status quo,* where a failure to find incontrovertible evidence has resulted in grandiloquent and unfounded claims about the power of oral tradition over very, very long periods of time. In that sense, it is largely a matter of apparently irreconcilable disciplinary epistemologies.

XIV

Both friends and foes of the reliability and/or authenticity of deep-time oral traditions agree that the only way these could have traveled over time, no matter how much time, would have been from the mouths of one generation to the ears of the next. Here, in the long run, outsized claims featuring highly implausible and untestable hypotheses will not appeal without supporting evidence. Still, there might be one saving approach: viz, validating the foundational premise that word-perfect oral testimony can be, and actually has been, faithfully transmitted hundreds of times. Astonishingly, there are no recorded attempts to do this, either in British Columbia or anywhere else. In light of that, let me propose a scenario by which this can be attempted—anywhere. It might prove cumbersome at first, but proponents of oral historiography could regard it as a bargain, whatever the results, since, for instance, practitioners could devote themselves to other pursuits if results are unfavorable.

This testing could be done—sort of—in rigorous and forensic fashion if, say, twenty-five to fifty people, a cross-section of society, were assembled and arranged, say, alphabetically. This test group could be enlarged as required to accommodate the periods hypothesized, now in Australia as well, where even longer custody chains (as many as 350 transmissions) on even flimsier evidence are now being posited (Nunn 2018:107, 240), but again not convincingly defended. The first person in line would be given a text orally of, say, thirty-five to forty words, including at least two proper names, on any subject, even those already familiar to some of the transmitters. To begin with, the tenth person would produce the text he/she receives. If it agrees substantially (subject to a mutually agreeable definition) with the *Ur*-text, the experiment would continue, twenty-five transmissions at a time until the final transmitter, or cease at some intermediate point, where the message has lost contact with the *Ur*-text. All forms of social-media communication would be forbidden, and the ban rigorously enforced. Like most experiments, the occasion would be proctored at every turn. A number of such experiments would need to be carried out to avoid outliers. The results could then be disseminated—and repeated?—widely.

Of course, the advocates of faithful transmission could object that *their* hypothetical transmitters were a selective group, more intelligent and more dedicated to the process. As usual, this is yet another self-serving but largely vapid speculation, advanced in a highly-circuitous matter. At best, such experiments can refract reality only imperfectly, inasmuch as the problematics of real life, primarily the corrosive effects of passing of time and the destabilizing perturbations that accompany it, cannot be part of the exercise. So it is really a best-case opportunity for the Pros. Should the Pros outscore the Cons often enough, this would eliminate, or at least seriously hamper, efforts by sniping skeptics. On the other hand, the balance could tip toward the Cons. No one knows for sure, since no one has ever tried it. To my mind, though, the burden of proof—and the burden of organizing and vetting such occasions—lies with the Pros, since it is they who have made such extravagant claims on oral tradition's behalf. Until a series of such experiments is carried out, no one has the right to press strong claims for *or* against. Should the Pros outlast the Cons often enough, this would eliminate grounds for excessive skepticism. Should the reverse result occur, there is always next time.

ENDNOTES

1. The present essay is a historian's take on the current debate over the durability and reliability of oral narratives over very long periods of time. I am concerned largely with the sufficiency of the research, the nature of the relationship between evidence and argument, and the lack of effort either to support specific arguments or to justify far-ranging conclusions. I do not consider the effects of memory on any of these issues (but see Mason 2006:45–66 and below). I would point out, though, that memory has been given rough passage over the past forty years or so, as experimental and other evidence have shown—time after time—that we all distort our memories, often within minutes of the event(s) being recalled. Those dubious of this claim can consult online (and in print) bibliographical databases such as *PsychINFO* for overwhelming evidence for this unpalatable truth.

2. For the record, I use the term "oral tradition(s)" to define those accounts of a receding past recited in oral societies and passed down to and by future generations. For what it is worth, the frequency with which I cite my own works is not from a bloated sense of self-regard, but only to point out more extensive versions of the arguments raised here.

3. This collective antipathy to the printed word could easily be mitigated. A cadre of qualified staff and students is organized, which sets out to divide the bibliographical universe of the First Nations by allocating subsets of the project to troll through systematically—all the missionary journals, archives, newspapers, scholarly books and articles, etc. These would then be organized as a *wiki*-like website, to be enlarged as and when new data become available. An editor would constantly evaluate both new and old matter. Despite its magnitude, the sheer number of trained contributors would ensure a fairly prompt completion of the core project, and once established, individual workloads (even for the site's editor) would be modest. As an online bibliography, this could be arranged by author, by keyword, or by First Nations group.

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ABOUT THE AUTHOR

David Henige (Ph.D. Wisconsin, 1974) founded and edited *History in Africa* from 1974 to 2010, and otherwise has published extensively on oral tradition as a historical source, as well as on historical method, exploration texts, scholarly publishing, numbers in history, and American Indian historical demography.

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"I Was Surprised": The UBC School and Hearsay—A Reply to David Henige

Charles R. Menzies and Andrew Martindale

David Henige opens his critique of the University of British Columbia (UBC) School (Henige 2019) expressing surprise that the Supreme Court of Canada could have gotten it so wrong when it overturned Chief Justice Alan McEachern's Gitxsan and Wet'suwet'en title and rights decision in 1997 (commonly referred to as Delgamuukw). We're with Henige. We too are surprised, but not by the court decision nor the years of subsequent case laws and research that have followed it. No, we are surprised by the blithe manner by which Henige tosses together snips and snaps of material to make an assertion that essentially renders Indigenous peoples to a perpetual present without history, and UBC scholars as ill-informed or complicit in a grand effort to hoodwink scholarship.

To paraphrase Henige's complaint, the members of the UBC School (more on who this is later) disregard reality and act according to a misguided belief in a romantic (and false) conception of the Indigenous past. This assertion is supported through a series of thinly-researched just-so examples that purport to demonstrate the possibility of counter explanations to the so-called irrational beliefs of the UBC School. In each case Henige assures us his counter explanations are just as, if not more, plausible than those of the UBC School. Henige misunderstands published work and ignores published examples of the very evidence he demands. His paper is lacking the very substance that he asks of the members of the UBC School.

Take for example the following statement: "When early observers began to write about First Nations's oral literature, they referred to them variously as 'myths,' legends, folktales, and stories and assigned them no historical value." Only later, Henige tells us, were these accounts "rechristened 'oral tradition,' 'oral history,'" or "'oral records [*sic*],' and the like." This opens the reader's expectation and anticipation that Henige will provide a detailed empirical review of 40 to 50 years of the UBC School's work in which he documents all the ways in which members of the school engaged in rechristening. But despite the promise, nothing materializes. It would have been useful if Henige had shown us how, for example, the members of the school changed the accounts recorded by William Beynon in Lach Klan in 1916 (fields notes of which are easily available from the Canadian Museum of History). Beynon records accounts explicitly described by his interviewees as their lineage history. How did the UBC School transform these "myths" (as Henige calls them) into an oral history record? Or why didn't Henige count the number of times, in the several hundred pages of publicly available field notes recorded by Beyon in 1916, that he used the term folklore, myth, or legend. Henige could then have compared how members of the UBC School transformed those terms into an oral history record. It would have been instructive to see Henige's detailed graphs and tables documenting the evolution of terminology deployed by the UBC School wherein they began their rechristening process, or some refutation of the corpus of scholarship from the late nineteenth and early twentieth centuries (see below for a summary of some of this) that explored Indigenous history through oral traditions. But our anticipation is left unsatisfied and our excitement diminished.

Instead of the detailed and empirical review of rechristening and historical refutation one would have anticipated, Henige instead skips to Africa. Rather than delving into the hard work of effective scholarly critique, we are treated to a self-referenced criticism of African historiography wherein he manages to castigate not just the UBC School, but several generations of African historians and then concludes by saying the African historiographers' efforts are moribund. That may well be true. How, though, does that say anything about the specific ways in which scholars working across many departments and disciplines and universities have explored the ideas and conceptions of Indigenous historiography in Canada and, more specifically, in the Northwest Coast (NWC) of Canada? Put simply, it doesn't.

Let's actually consider an example Henige touts as (1) proving the "extravagant defence of the fidelity of tradition for centuries, no matter what" by members of the UBC School and, (2) demonstrating that that even when one of their own (Jay Miller 1981) brings up evidence that "might damage their hypothesis" they all, even Miller, artfully ignore the evidence.

Miller's article is an account of the emergence of Klemtu as a contemporary hybrid Indigenous community. According to Miller,

> The mixed Tsimshian-XaiXais Kwakiutl village of Klemtu, British Columbia, provides a vivid example [of social transformation]; over the past three generations, its elite has managed to transform two co-existing tribal divisions into exogamous, matrilineal moieties, suppressing and modifying traditional knowledge and symbols to accord with recent and present needs and conditions. (1981:23)

Here is a case in which clearly contradictory and antagonistic aspects of the two originating communities' histories and cultural practices were suppressed to facilitate community cohesion in the face of colonial intrusion.

Henige tells us, however, that this is a blatant example of members of the UBC School ignoring reality. Henige says,

> The Klemtu case seems to subvert entirely the notion of oral traditions passing smoothly from one generation to the next, allowing for heroic extrapolations of several centuries, even millennia. Which is it to be? It is counterintuitive to believe that there never was an inclination to change a few words in a tradition. Indeed, every occasion at which oral traditions were recited was also an opportunity to tamper with them to fit ever-changing circumstances. (Henige 2019:65)

No one among the UBC School, says Henige, has done anything but to ignore Miller's 1981 paper. But that's not really the relevant point (besides, it's factually incorrect; for example, see Menzies 2016). Henige is using this modern example from the twentieth century to argue that all oral history is contrived and made up almost on the spot. He ignores the historical details that provide context and setting for the feast that Miller observed and the interviews he conducted.

Klemtu was a village of about 250 people at the time of Miller's visit in 1977. The village was settled by people from both Kitisdzu (Tsimshian) and XaiXais

(Kwakiutl) communities. The XaiXais "and the Kitisdzu had been much reduced in numbers by the time that they settled together at Klemtu [about 1875]. The location was selected because it was a harbor sheltered by Cone Island, and on the steamship route, so that the early inhabitants could earn, at first, vouchers, and then money by supplying firewood for the steamship boilers" (Miller 1981:25). Klemtu had been a place of refuge to survive the depredations of colonialism in the late 1880s in the face of economic and population collapse of Kitisdzu and XaiXais communities. Then again, in 1918 half of the community died in the course of the "great influenza epidemic" (Miller 1981:31). The community was bolstered economically with the establishment of a salmon cannery "which functioned between 1927 and 1968" (Miller 1981:31). Today, fisheries, forestry, and eco-tourism support the community's economy; all industries are managed and directed locally.

Heigne ignores the historical context and simplifies the social transformation that Miller describes in detail. Pulling from interviews and historical documents, Miller documents details of the traditional territories of both Kitisdzu and XaiXais lineages. Nowhere does Miller state that this information has been altered, silenced, and forgotten, by people in Klemtu. This is important, as it is this kind of oral history—that related to lineages territories and social alliances and origins—that most of the people Henige is critical of are actually discussing. Much of Miller's paper is a detailed documentation of lineage histories. What Miller notes as having being transformed is of a different order of social narrative that he himself calls "Myth and Ritual" (1981:29). (So much for Henige's worry that the UBC School has been rechristening "myth" into "history"). Indeed, by the latter half of the twentieth century, the term myth had moved beyond the vernacular for fiction and taken on historical scholarly heft in western and Indigenous culture (Campbell 1972).

The fundamental social transformation that Miller is considering concerns the very important protocols for intermarriage between two communities with different underlying social models of whom is appropriate to marry whom. As Miller documents, this was about reconciling Kitisdzu with XaiXais crest groups for the purposes of marriage to accommodate the tumultuous and erosive effects of colonialism. So, while Henige interprets this to be a big reveal about how Klemtu shows that oral history is invented, in fact Miller's paper is about how customary protocols and longstanding lineage narratives are used as part of legal process to adapt to changing conditions of the present. What is more revealing than his lack of research is Henige's underlying bias to simplify Indigenous cultures and narratives to an artefact of the present unable to innovate or generate complex ideas on its own. It is as though he cannot believe Indigenous peoples had the capacity for complex sophisticated cultural systems of practice and knowledge.

How then, do Indigenous peoples ensure the robust maintenance of oral history? I suppose it could be asked how on earth does a singer, a thespian, or any number of public performers manage to repeat the same performance time after time. As Yates (1966) explores, systems of memory were formally developed and commonplace in ancient Greek and Roman times, and often put to use in the performance of long narrative poems. Similarly, a quick Google search reveals hundreds of examples of communities dedicated to and demonstrating how complex texts, such as the Rig Vedas, are learned by rote. Kitselas hereditary chief, Walter

Wright, related in 1936 how he had learned the oral tradition of his house,[1] whose major crest was the Grizzly Bear, M'deek:

> When I was a boy my Grandfather, who was Neas Hiwas, taught me the history of Medeek. It had been his duty of carrying it through his generation. His was the responsibility of choosing one of the Royal Blood to keep it safe after he had died. As a lad I sat at my grandfather's feet. Many times he told me the story. It was long. In the Native tongue it takes eight hours to tell. So several times each year, I sat at his feet and listened to our records. I drank the words. In time I became word perfect. So I became the historian of Medeek. So I took my place in the long line that had gone before me. For so it is. In our land of Ksan there is no written word; the record had to be passed down from man to man by word of mouth. (Wright 2003:11–12)

Menzies also recalls that his late mother would talk at length about her grandparents who apparently could recite hundreds of lines of poetry by heart. As a young child he was very impressed. However, he wonders if now he must reconsider her claim in light of Henige's assertion that no such feats of memory are possible. Menzies considers his mother to have been sincere and genuinely honest with regards to the oratorical prowess of her elders. So allow us to assert that humankind is capable of holding a great deal in memory if trained and practiced. After all, Menzies' mother told him it was so.

Menzies has written about the Indigenous ways and means of maintaining oral histories that are of particular social and cultural value (Menzies 2016; see, in particular pages 69–85). He has laid out what he calls a Gitxaała theory of history. History and historical conceptions are integral to Gitxaała identity. There is little about Gitxaała that is not touched by history or historical references. In *People of the Saltwater*, Menzies provides three stories of oral history research that exemplifies a Gitxaała approach to the documentation of history. This commentary does not allow for a detailed review. We refer the interested reader to the original source. Suffice to here to outline the general features:

> In the course of conducting field research with Gitxaała people related to traditional territories and their use I [Menzies] was regularly advised that the appropriate approach to research involved requesting permission of the named title holder to the territory in question and that any conversation with community members should include groups of people who held the rights to tell the history. The emphasis was that even in direct communications, such as interviews and conversations, the transmission of history and related information needed to take place in a collective setting

[1] Wright (2003:11) explains how the consequences of colonization had created "new models of life" that "have drawn the minds of our young men from the habit of peacefully listening to their elders." As a result, he dictated his house lineage's oral tradition in English to Will Robinson, a local non-Indigenous friend. Years later, Robinson's descendants discovered the manuscript, which was published by the Kitselas Indian Band in 2003.

with appropriate individuals in place to acknowledge and witness what was being said.

In conversations with my mentors I have had the opportunity to discuss and learn about the ways knowledge is transmitted. In these settings, which parallel traditional approaches to the transmission of knowledge, I have learned about the processes of learning. This involves the learner listening, not questioning, observing and then doing. Knowledge about history is transmitted in these settings through direct instruction, demonstration, and practice.

Documentary sources also provide corroborating information on oral history and its transmission. William Beynon recorded a great many historical narratives; throughout his work can be found comments and asides related to the nature of Tsimshian oral history and the ways it can or should be related. In his 1916 notebooks of observations of and interviews collected in the village of Lach Klan, for example, Beynon records that his entire research project was placed on hold until the leading hereditary leader, Joshua Tsibassa, granted approval.

> *The people have not advanced as much as the other people of other tribes in matters of education and still adhere to ancient ceremonies. I had difficulty in getting started here on account of this and on going to an informant to get information in the house of Tsybɛsɛ̨ I saw the informant [Sam Lewis, who then] took me to the house of the chief and asked the necessary permission to be able to give me the information and after I had paid him for his work, he handed all the money over to the chief and took only what the chief allowed him for telling me what I wanted.* (Beynon notebook, 1916, vol. 1, bf 419, box 29, cmc)

Then, midway through his research, a number of Beynon's respondents withdrew their participation; they were waiting for further permission to be granted by hereditary leaders to answer Beynon's additional questions.

> *My informant upon being requested to translate other names refused to do so until she had received the consent of the different chiefs.... Informant for the house of 'nagap't, ganhada. Mary 'Áłaxsgɛłs 75 yrs of age.* (Beynon notebook, 1916, vol. 1, bf 419, box 29, cmc)

This process of ensuring approval, proceeding, stopping, and reaffirming approval is a long-standing practice among Gitxaała people. It is part of the internal mechanisms and protocols that ensure the maintenance and continuity of an oral history over time. (Menzies 2016:70–71)

The presentation and transmission of oral history occurs in a range of settings, including but not limited to formal settings such as feasts and training or instruction of heirs and youth. What Henige confuses is his own conception of a people's inability to remember history with the empirical reality of Indigenous histories (and the means and mechanisms of its practice). It is hard to argue against the feigned empiricism of intractable belief. But simply because Henige is unable to accept the possibility of a people who can in fact recall history in detail over millennia does not prove Henige's point.

Henige's Logic and Errors

Henige's analysis contains errors that suggest a lack of familiarity with Indigenous oral records on the NWC and the scholarship that explores them. Most fundamentally, he seems unaware of the legacy of written records of oral traditions that began in the nineteenth century. Oral traditions are actively curated and transmitted today as part of vibrant Indigenous cultural practice, but many were written down, often by Indigenous scholars, as far back as the nineteenth century. Scholars such as Chris Arnett (2008), Marius Barbeau (1928, 1950, 1953, 1961), William Beynon (1939), Franz Boas (1916), Keith Carlson (2001), Arthur Wellington Clah (see Brock 2006), John Cove (1987), Julie Cruikshank (2005), George Hunt (in Boas and Hunt 1902), Viola Garfield (1939, 1951), George MacDonald (see MacDonald and Cove 1987a, 1987b), Susan Marsden (2000, 2008), Charles Menzies (2016), Gordon Robinson (1956), Edward Sapir (see Sapir and Swadesh 1939), Wayne Suttles (1987), John Swanton (1909), Henry Tate (1993 and in Boas 1916), James Teit (1912), Simon Walkus Sr. (1982), and Walter Wright (2003) all present recorded versions of oral records that in many cases date back into the nineteenth century (and there are many more in both published form, unpublished reports, and archives). The suggestion that scholars are prompting these stories in modern settings or that they were created recently in response to European textbooks is undermined by this long historic record. In addition, Indigenous leaders applied Indigenous law by invoking oral traditions as early as the 1860s to Department of Indian Affairs agents as demonstration of their rights when negotiating issues such as reserve allocations. Although the DIA rarely acted on this information, it was duly recorded, and is now available digitally in the DIA archives in the Library and Archives Canada.

Certainly, the influence of colonialism has a potential impact on these narratives as Menzies (2016) has discussed. Henige's unfortunate phrasing that Indian Residential Schools sought to "wean students from their backwards ways" notwithstanding, modern communities in some cases have relied on the earlier written versions of their oral traditions to maintain their legacy of narratives. This is not always the case, and the Gitxan and Wet'suwet'en lineage leaders who presented their oral traditions in the Delgamuukw case, for example, presented an orally curated scholarship (Culhane 1998; Sterritt et al. 1998). Scholars can and do work with contemporary Indigenous leaders whose knowledge of their oral traditions represents a claim to continuity through the upheavals of the colonial era, but there is a long legacy of recorded oral traditions. To suggest that these narratives are substantially transformed or even invented as an Indigenous response to projected European ideas is to misunderstand the clear sequence of continuity in these narratives over the last 200 years (see for example Sterritt et al. 1998).

The Tsimshian case is among the best documented, in part from the work of Tsimshian scholars such as Arthur Wellington Clah (n.d., see Brock 2006), William Beynon (e.g., 1939), Henry Tate (1993; in Boas 1912, 1916) and Walter Wright (2003). Although Maud (2000) notes that Tate's work was complicated by translation issues (he recorded them in English and translated them into Sm'algyax (Tsimshian) for Franz Boas, who subsequently relied on Archie Dundas to translate them back into English), the work of Clah, Beynon and Wright has no such issues. Beynon's records, for example, are interlinear translations, preserving the original spoken Sm'algyax with Beynon's accompanying translated version. Beynon also records the meta data for these narratives (the informant's English and Tsimshian names, their tribal affiliation, and the location and date of the recording). Menzies has noted that Beynon's value is complex, as the extensiveness of his records imply a representativeness that might not be true, especially regarding the Gitxaała narratives (Menzies 2016:21–24). That said, Beynon's work is sufficiently representative, especially when complied alongside the work of contemporary knowledge holders and other historic records, to lay a foundation to understand important aspects of the regional dynamic within Tsmishianic history. Many other scholars have used these sources or made reference to them as part of contemporary ethnographic and historical work (Garfield 1939, 1951; Miller and Eastman 1984; Seguin 1984, 1985; Dean 1993; B. Miller 1997; Anderson and Halpin 2000; Roth 2002, 2008; Brock 2006).

The work of Barbeau (1950, 1953, 1961), Boas (1912, 1916), Beynon (1939, and in Barbeau and Beynon n.d. a, b, c, d), Clah (n.d.), Cove (1985, 1987), Dauenhauer and Dauenhauer (1987), Duff (n.d.), MacDonald and Cove (1987a, b), Robinson (1956), Swanton (1909), Wright (2003) presents records of oral traditions from the region that are easily available. Similarly, the work of scholars who have synthesized and compiled these sources into historical and spatial patterns is also available as reports such as by Marsden (1987, 1997, 2001, 2002, 2011, 2012) and MacDonald (2009), providing a comprehensive descriptive overview of these traditions. Many publications exploring both the historical value of these texts and the methods to explore them refer to these sources (Cove 1987; MacDonald and Cove 1987a, b; Marsden 2000, 2002; Martindale 2006, 2009; Martindale et al. 2017b; Menzies 2016; Roth 2008).

Thus, Henige's argument that these narratives are not available except as superficial and selective summaries of data by non-Indigneous scholars is simply false, as even a quick review of the available scholarship would reveal. Without an understanding of the data that scholars work with, Henige's critique is groundless. However, Henige's most egregious flaw lies in his ignoring published evidence that oral traditions are demonstrably correct. We choose the word "ignoring" intentionally; Martindale provided Henige with three articles presenting evidence of the historical accuracy of Tsimshian oral traditions (Edinborough et al. 2017; Martindale et al. 2017a, 2017b) prior to the publication of his opinion. His choice to not consider these suggests an ideological foundation to his critique, despite his claimed adherence to empirical evidence and logical rigor.

The evidence of the historical value of Indigenous oral traditions from the NWC focuses largely on recent history in which oral traditions are treated with similar care and scrutiny as written documents (Garfield 1939; Cruikshank 1990, 1998, 2005; Marsden and Galois 1995; Martindale 2003; Menzies 2016). Scholars who explore earlier time periods tend to focus on the comparison between archaeological or geological data and oral traditions, as there are few other historical sources that reach comparably as far back in time. These are of two forms: side-by-side comparisons in which evidence from oral traditions is compared to material evidence, and more rigorous comparison of larger scale regional patterns in both oral traditions and archaeological data. The former is more common but creates only a series of anecdotal cases, which while supporting the idea, are difficult to test and are more persuasive in the aggregate. As Henige (2009:192) notes, even a system that does not work will correctly transmit some historical evidence some of the time. Still, these are sufficiently numerous to warrant caution in rejecting the hypothesis that oral records have historical value from a range of contexts along the NWC, (Marsden 2000; Cruikshank 2001, 2005; McMillan and Hutchinson 2002; Martindale and Marsden 2003; Thom 2003; Fedje and Matthews 2005; Angelbeck and McLay 2011; Crowell and Howell 2013; McKechnie 2013; Gavreau and McLaren 2016).

Still, we agree with Henige that an inventory of cases of apparent conjunction between oral traditions and archaeological or geological data are insufficient to fully test the hypothesis that these records have historical value. Thus, Martindale and colleagues present one of the few statistically robust testable scenarios in this field. Working with a series of oral narratives that were recorded in the early twentieth century, primarily Beynon (1939), and published as reports (Martindale and Marsden 2011) and articles (Marsden and Galois 1995; Marsden 2000; Martindale and Marsden 2003), Marsden described how a regional war with northern invaders (whom the Tsimshian refer to as Tlingit) caused a regional settlement disruption. An initial wave of newcomers, who were accommodated within Tsimshian territory, were followed by communities with whom they were at war. A regional conflagration ensued in which the invaders established themselves on the Dundas Islands to the northwest and the local Tsimshian populations retreated south down the coast and east into the interior valleys, leaving a settlement hiatus in the coastal zone. After a successful counter-attack, some of the greater Tsimshianic community reclaimed their lands, but relocated largely to winter villages in the more defended Prince Rupert Harbour area.

A number of previous researchers have seen archaeological patterns that appear to match this historical picture (MacDonald 1969; MacDonald and Inglis 1981; Marsden 2000; Martindale and Marsden 2003; Cybulski 2014), and estimate that the settlement hiatus from the war occurred about 1,500 years ago.

However, Martindale and colleagues, inspired in part by Henige's (2009) own critique (see Martindale et al. 2017a), sought more demonstrable evidence by choosing to test the oral records against well-sampled, appropriate, and representative data. Although the concept of a test of Indigenous oral records is disrespectful to Indigenous communities who know their value, it is the logic of legal and scholarly critics who seek, not unreasonably, to evaluate them. Martindale and colleagues ask the question, can we disprove the hypothesis that a hiatus of settlement occurred in Tsimshian history characterized by settlements across the region preceding the hiatus and concentrated only in the Prince Rupert Harbour area subsequent to it? To do so, they developed new methods of collecting datable samples by percussion coring (Martindale et al. 2009), mapping (Supernant and Cookson 2014), and the compilation of archival data (Ames and Martindale 2014; Letham et al. 2015), primarily the survey, mapping, and dating projects of David Archer (1989, 1990, 1992). To be sure that the data were not skewed by post-glacial relative sea level effects, they compiled a local sea level curve and confirmed that relative sea level (RSL) effects predated the hypothesized hiatus by more than 3,000 years (McLaren et al. 2011; Letham et al. 2016, 2018). They compiled existing radiocarbon dates and expanded the set to better sample for variation in settlement type and location (Martindale et al. 2017a; Letham et al. 2017). Since they were including marine sourced carbon in their analyses, they complied a local delta-R value for the Marine Reservoir Effect (Edinborough et al. 2016) and published a defense of best practices using new methods and statistics from geochemistry (Martindale et al. 2018a). They compiled regional settlement patterns (Martindale et al. 2017b) that showed that a settlement hiatus beginning about 1,300–1,100 years ago was clearly visible in both the spatial data and in a composite summed probability distribution (SPD) plot of the radiocarbon dates. SPDs are synthetic compilations of radiocarbon date distribution probabilities that have been shown to be proxies for demographic trends, including the movement of large numbers of people (Chaput et al. 2015). However, Martindale and colleagues were unsatisfied with the composite SPD, and developed a new mathematical model to test whether SPDs could detect a settlement hiatus of this duration and whether the pattern was visible in the marine and terrestrial dates separately (Edinborough et al. 2017). All of the primary data for these analyses are available in Martindale et al. 2018b.

This work was framed as an attempt at disproof: an exploration of the archaeological data to reject the hypothesis that a settlement hiatus matching the oral traditions was visible in material evidence. The test failed to disprove the historical accuracy of the oral record, and while this is not in and of itself proof, it does set the bar high for anyone who proposes that the oral records are not historically valid. Henige clearly does not reach this bar. Henige's critique thus demands the very evidence that clearly already exists and is easily discoverable in major international journals, the bibliographies of which would lead him to the primary data of both oral traditions and archaeology. If his opinions were simply erroneous, his paper would not likely have been published and we would not have been asked to provide a response. The stakes for this debate are high. The denigration of Indigenous people, their achievements, and scholarship have been ongoing since the beginnings of colonization. The fanciful and ethnocentric narratives such as Henige subscribes to circulate in some non-Indigenous quarters with a clear and longstanding purpose: to disenfranchise Indigenous peoples from their rights, including their title rights. Challenging how Indigenous people recorded history without writing is scholarship. Assuming that they could not do so, especially in the face of clear evidence that they have, is a double standard.

Henige seems to believe that scholars who find historical value in Indigenous oral traditions are steering the courts as expert witnesses, creating acceptance for their views via manipulation rather than evidence. In fact, the opposite is true. Court rulings since Delgamuukw have consistently challenged the historical value of oral traditions (Miller 1992, 2011; Culhane 1998; Martindale 2014), demanding similar kinds of evidence and logic to Henige, and ruling against Indigenous communities when these data are not presented. Thus, there is no conspiracy to falsely substantiate Indigenous oral records, no collective plan to force the courts to accept fabricated evidence. Instead, there are individual Indigenous communities with long traditions of orally transmitted knowledge willing to form research partnerships with individual scholars who are seeking to assess and evaluate the provocative claim that humans can and have recorded millennia of history without writing. There is no UBC school of thought on this subject, but we like Henige's pejorative: perhaps there should be.

The UBC School

So who constitutes this UBC School Henige focuses upon? For one thing it doesn't include Menzies who is not cited, named, nor even obliquely referenced in Henige's paper. One suspects Henige is unaware of Menzies and many other folks who have written a great many papers on this subject over the last few decades since Delgamuukw. As an Africanist bibliographer, Henige would have no reason to be familiar with much of this work. As a retired librarian, he would have little cause to search out (or think to search out) work by Menzies or others such as our colleagues Bruce Miller (2011) or Leslie Robertson (2012).

It seems that Henige is jousting with an old ghost. He is peeved by a 1997 court decision that relies upon research conducted in the 1980s and earlier. Some of the folks he names as members of the UBC School had engagement with the original Delgamuukw trial as commentators, expert witnesses, authors of a special edition of *BC Studies* edited by Bruce Miller (1992, ironically not cited by Henige), or close associates. Menzies's own involvement was simply as a graduate student observer who visited court proceedings in the 1980s at the Vancouver Law Courts. So let's just take a look at the list of the UBC Schoolers (as Henige calls them). Who constitutes the UBC School as determined by Henige, what are their interconnections, where is their manifesto, how are they located in time and history? Let us take Henige's call for scepticism and empiricism seriously and ask ourselves—Is there a UBC School?

So, by order of appearance in Henige's paper, here are the folks he lists as members of the UBC School.

- Susan Marsden, mentioned by way of a tribute from Jay Miller. Marsden was an expert witness for the Gitxan and Wet'suwet'en court case and has published extensively on the subject of oral history. She has written many reports for legal counsel on behalf of First Nations. With Andrew Martindale, she has co-authored papers on how Tsimshianic oral history and archaeological knowledge interlink. Marsden is the curator of the Museum of Northern BC and has no direct connection to UBC
- Jay Miller (as above). Miller conducted ethnographic research with Gitga'at and Kitasoo community members and wrote a full-length monograph, *Tsimshian Culture: A Light Through the Ages* (1997). He was part of a cohort of researchers in the 1970s and 1980s who focused their efforts on BC's North Coast (for collections of their work see Miller and Eastman 1984; Sequin 1984; Mauze, Harkin, and Kan 2000), though he had no formal association with UBC.

- Wilson Duff. A luminary of Northwest Coast anthropology. He worked both at the Royal Museum of BC and as a faculty member in anthropology at UBC. One of his most important pieces of work was to review and collate the voluminous work of Marius Barbeau and William Beyon on the Tsimshian (broadly conceived as including the Nisga'a, Gitxan, and Coast Tsimshian) and create an archival record used by a great many researchers and housed at the Museum of Anthropology, UBC.
- D. W. Moodie and A. J. W. Catchpole. To the best of our determination, they are historical geographers formerly of the University of Manitoba. The bulk of their work stretches from the 1970s into the 1990s and considers how historical records can provide information about climatic and other environmental concerns. Aside from an interest in the Hudson Bay Company records, there appears little overlap with others on this list, or any connection to UBC
- Heather Harris. Completed a Ph.D. in anthropology at the University of Alberta (2003) on 12,000 years of oral history. Harris' work is by far the most literal in terms of the priority she places on oral history as a literal fact. Her dissertation focused on Gitxsan narratives. She was briefly associated with the First Nations Studies program at UNBC.
- Neil Sterritt (former president of the Gitxan/Wetsuweten Tribal Council in the 1980s). His place on this list is in reference to the book, *Tribal Boundaries in the Nass Watershed* (Sterritt et al. 1998). The book deals with how the Nisga'a Nation encroached upon Gitxan and Gitanyow traditional territory through the Nisga'a treaty process. The co-authors of this book are Susan Marsden (expert witness), Peter Grant (legal counsel), Robert Galois (expert witness), and Richard Overstall (research coordinator). Sterritt has no direct association with UBC.
- Kii7ijuus (Barbara J. Wilson, Haida Nation) is a co-author with Heather Harris of a chapter in an edited book on Haida archaeology and history.
- Andrew Martindale, faculty member of the Department of Anthropology at UBC and archaeologist who trained at the University of Toronto.
- Loraine Weir, UBC faculty member in the Department of English. It would appear Weir's inclusion stems simply from the fact she studies, among other things, Indigenous literature.
- Owen Mason, archaeologist. This is a maybe. It's not clear to us from the way Henige deploys Mason if he is to be properly considered a member of the UBC School or not.
- Wendy Wickwire. Emeritus University of Victoria faculty member in Environmental Studies. Wickwire's work is incorrectly used by Henige to introduce the idea that the Nlaka'pamux narratives she discusses (1994) are more plausibly a product of residential school textbooks than as actual and genuine Nlaka'pamux narratives.
- Julie Cruikshank. Emeritus UBC faculty member who is deployed by Henige to assert: "believing in something does not by itself make it real" but in so doing Henige doesn't really engage Cruikshank's work in a serious way at all.

We may have missed a name or two or added one or more not intended by Henige, but that is partly due to the ambiguity and lack of clarity on Henige's part in clearly defining what and who is meant by the moniker UBC School. To quote Henige back to Henige: "Believing in something does not by itself make it real." Of his list, only three (Cruikshank, Duff, and Martindale) have both published on oral traditions and are directly associated with UBC. Since Duff died in 1976, Cruikshank retired in 2003, and Martindale did not join UBC until 2005, there was not much opportunity for them to create a collective intellectual movement.

Could there be a UBC School? That is an interesting question. As one of the top ranked anthropology departments in Canada, those of us from UBC would like to think there may well be something unique about our program and some quality and impact among our members past and present. But rankings, desires, and aspirations do not make a school. According the Henige, the core aspect of the UBC School is the fundamental (fundamentalist?) belief in the validity without question of all and sundry Indigenous oral accounts (however recorded, relayed, observed, or remembered). Henige presents eight brief quotes and explores three cases in a little bit of detail (J. Miller, Wickwire, and Cruikshank). We are expected to simply accept Henige's assertion that there is such a school and that there is unanimity amongst the members of said school. Let's consider the evidence.

UBC's anthropology department was founded by Harry Hawthorn in 1947. Under his direction the department produced volumes of theses and dissertations concerning Indigenous peoples in Canada. While the department's research focus has expanded geographically, we do retain a strong cohort of faculty and graduate students working with and among Indigenous communities in Canada and abroad. Our program is also entwined with both the Museum of Anthropology, founded by Audrey Hawthorn in 1949, and the Laboratory of Archaeology (LOA), founded by Charles E. Borden in the same year. However, though some of the Museum's faculty are co-appointed in anthropology, not all of them are and the Museum is a stand-alone institution with it's own institutional character and mandate. As with the department, the museum has a strong focus on research with and among Indigenous communities. LOA is instead a unit within the Department of Anthropology, and it has had a history of increasing engagement with Indigenous communities, especially the Musqueam.

While it is possible that the UBC School idea is simply a red herring tossed out to distract us, given the manner by which Henige cites it throughout his paper, we are obligated to consider the sense of its empirical reality. Henige refers to the UBCers, the UBC School, and UBC movement at least seventeen times in the paper. He mentions "them," and "they" almost as many times, in an apparent unvoiced reference to a UBC School. In most cases no actual people are named, except in his list of eight exemplary quotes of the School wherein the authors are described as either members or supporters of the UBC School.

Names not mentioned, but that one might have expected to see listed as members of the School include, but are not limited to: Wayne Shuttles, Michael Kew, Marjorie Halpin, Michael Ames, James MacDonald, and John Pritchard. Also overlooked are current colleagues such as John Barker, Carol Blackburn, Michael Blake, David Pokotylo, Jennifer Kramer, Bruce Miller, Patrick Moore, Leslie Robertson, Daisy Rosenblum, Sue Rowley, Patricia Shaw, and Mark Turin. We could go on, but our point is that if one were to do a thorough job of this, one needs to know who the actors are and what they say. As Henige himself says, one needs to do the work to collect the evidence and analyze it.

We can see at least four strands of work emerging out of UBC Anthropology's engagement with Indigenous-based research: an empirically-based tradition of ethnography linked to provision of expert testimony, an empirically-based traditional of field archaeology, a structuralist Levi-Straus influenced ethnographic practice, and a materialist tradition of political economy. These are not hermetically-sealed categories and our colleagues may not necessarily agree with our groupings, but when one examines closely the corpus of our department's publications relating to Indigenous communities on the northwest coast of North America, we submit the evidence supports these general streams of work.[2]

Under Harry Hawthorn's direction, several decades of empirically grounded graduate studies of Indigenous communities were produced. Hawthorn himself led two major government funded projects "The Indians of British Columbia: A Study of Contemporary Social Adjustment" (Hawthorn, Belshaw, and Jamieson 1955) and "A Survey of the Contemporary Indians of Canada" (Hawthorn 1966). These, and other similar reports, examined the socio-economic state of Indigenous peoples with recommendations for accommodating Indigenous peoples within the mainstream economy.

Hawthorn was not alone among his colleagues of the day in engaging in applied, policy, or expert witness research (see Kew 2017 for his personal reflection on a history of applied research). Wilson Duff, whose work was pivotal in making William Beynon's fieldwork accessible to several generations of students, was a key expert in the Nisga'a land claims, commonly called The Calder Decision (Foster, Raven, and Webber 2007). Duff, who worked at the Royal Museum of BC before taking up an appointment at UBC was a thorough, empirical researcher interested in not simply what was, but also how Indigenous communities found their way in the contemporary moment. Michael Kew, who began teaching at UBC in 1965, already had amassed a strong history of applied anthropology. With a B.A. from UBC (where he had studied with, among others, Harry Hawthorn), Kew found work with Duff at the BC provincial museum in 1956 (Kew 2017). From the museum he went to work for the Centre for Community Studies, University of Saskatchewan. He returned to graduate studies in 1963 in the doctoral program in anthropology at the University of Washington (Ph.D. completed 1970). All the while his work focused understanding not simply reconstructions of past cultural practices, but the ways Indigenous communities persisted and adapted in the face of fundamental social transformation.

These early members of the department were trained in an anthropological approach that prioritized detailed empirical fieldwork with community-based knowledge holders. Their work involved both a consideration of historical practices predating colonialism and the contemporary adaptations of Indigenous peoples (see, for example Duff 1964; Hawthorn 1966). Closer in sensibility to the British structural functionalists than with Boasian particularism, they were very much

[2] For a complementary description, see Miller 2018.

interested in how things worked and how change wrought by colonialization emerged within the contemporary period.

Archaeology was not originally part of the anthropology program at UBC. Instead, an amateur archaeologist and Germanics Studies professor, Charles E. Borden, initiated it (West 1995).

> In the 1960s Borden would reflect that Drucker's words [see Drucker 1943:128]… instigated his early amateur involvement in B.C. archaeology: "Drucker's report… had a profound influence on the present writer. It was the direct impact of his publication which in 1945 prompted me to initiate a series of salvage projects at potentially important but rapidly vanishing sites within the city limits of Vancouver." (Quoted in West 1995:6–7)

Wilson Duff (previously mentioned) was an undergraduate student of Borden's. Working together in the 1940s and 1950s, Borden and Duff conducted some of the earliest scientific archaeology in the province. They also joined with the Musqueam Indian Band in 1946 to initiate one of the earliest archaeological partnerships between a university and a First Nation in the province (Roy 2010). Later, as an employee of the provincial museum, Duff created the journal *Anthropology in BC* that came to play an important role in the professionalization of archaeology (West 1995).

Duff, Hawthorn, and Borden collaborated in establishing a provincial research program that linked social anthropology and archaeology. Duff, from his position at the provincial museum "sent Borden and his UBC colleague, Harry Hawthorn, a series of recommendations based on the assessment that provincial archaeological sites were in danger of destruction, by both urban expansion and proposed hydro electric dam projects" (West 1995:27). They also coordinated in having legislation set in place to regulate and professionalize archaeological excavation. They also lobbied to have developers, not the government, pay for the cost of archaeological research (West 1995:27). A decade of lobbying resulted in the passage into law of the Archaeological and Historic Sites Protection Act in 1960, the forerunner of today's Heritage Conservation Act.

As West (1995) observed the early UBC anthropological tradition (circa 1945–1970) closely linked socio-cultural anthropologists and archaeologists in a common pursuit of the scientific study of Indigenous peoples in British Columbia. These foundational figures of the UBC School placed a higher value in scientific study than the beliefs of their Indigenous research collaborators—at least in terms of historical truth. Duff's and Kew's expert opinion research, for instance, relied upon interviews with Indigenous knowledge holders to document historical practices, but they also drew upon archival records and archaeological and (in some cases) ecological data to triangulate their conclusions. Clearly these anthropologists are not part of the UBC School that Henige is so concerned about. Where might the turn from science to fabrication and revisionism be located?

The mid-twentieth century anthropological stability was shaken by the rise of new ideas in the academy ushered in on the tails of national liberation struggles in the heartland of anthropological fieldsites (Latin America, Africa, Asia, and Indigenous North America) and the new social movements of the metropole

(Patterson 2001). At UBC these changes first appeared in the form a Marxist influenced political economy (the later more evident among the students than the faculty) and a theoretical interest in Levi-Strausian structuralism.

Marxist influenced political economy had few faculty adherents in anthropology at UBC, most of the Marxist influences came from new hires in the sociology side of the program (circa 1970s). There were other materialists and empiricists, within anthropology in the form of archaeologists and carryovers from the Hawthorn-Duff period, but they were not necessarily advocates of Marxist theory. The most explicit political economists were among the graduate students.

Between 1977 and 1995, five dissertations (Kobrinsky 1973; Pritchard 1977; McDonald 1985; Boxberger 1986; Littlefield 1995) and at least three M.A. theses (Wake 1984; Legare 1986; McIntosh 1987) engaged in some significant way with Marxist influenced political economy (though the authors may well eschew a Marxist label). There were additional theses, such as Sparrow's (1976) work on the life history of her paternal grandparents or Brown's (1993) analysis of Indigenous cannery work, that while not specifically political economy, did engage with a common subject matter (labor, labor organization, and working-class experience). While Kobrinsky's dissertation combines elements of political economy theory, his could also be considered an example of French structuralism.

The political economists, though considerate and respectful of Indigenous community sentiments, were also interested in documenting processes of change and transformation and analyzing such change in the context of a theoretical model external to Indigenous systems of knowledge. Pritchard examined how Haisla involvement in the industrial capitalist economy undermined their traditional social organization. McDonald analyzed how the development of industrial resource capitalism in north western British Columbia simultaneously underdeveloped Kitsumkalem's Indigenous economy. Boxberger's dissertation also examined the way the Lummi's incorporation within a capitalist economy served to disadvantage them vis-à-vis their access to elements of the mainstream capitalist economy. Littlefield differed from the other three with an explicit feminist analysis in her study of Sne-nay-muxw women's employment, though she too was interested in how this Coast Salish community was incorporated into the capitalist economy. In each of these cases, the notion of truth was not so much about the truth vested in Indigenous oral narratives, but the truth of specific transformation in material conditions of life and how that was shifting Indigenous social and cultural organization.

The structuralist approach, represented on faculty by Pierra Maranda, and among graduate students by people like Marjorie Halpin (1973; who became a faculty member in 1973), Martine Reid (1981), and Dominque Legros (1981), had an effervescence quickly displaced by the growing interest in interpretive and post-modern anthropology, a tendency that has gripped mainstream anthropology for several decades now (in various, and often competing, forms). The French structuralist movement was driven by Levi-Straus's ideas of binary oppositions and the notion that the meaning of ritual, myth, and cultural institutions did not reside in what people said they were but were rather notions that emerged from the structure of mind. While key local knowledge holders were valued, the analytic frame was one that located meanings and truth somewhere other than the surface statements. The French structuralists did not accept that Indigenous oral history was in any way a true history (or that historical truth was of central importance); for them, the truth lay in what the "myths" revealed about cognition.

This kind of structuralism was fairly short-lived, compared to other approaches within anthropology, and was replaced in the 1980s and 1990s with a discourse, narrative, and community-focused kind of anthropology. While the externalist idea of applying theories and models to Indigenous peoples, narratives, and communities continued, now it was done with an eye toward giving "voice to the voiceless." These developments occurred within the context of a discipline that was turning to a consideration of how one might write as being as important (if not more so) than what one might write about (Marcus and Fisher 1986). The earlier empiricism of UBC's founding anthropologists was gradually being displaced by a more post-modern or cultural studies approach that was less interested in interrogating knowledge holders as to the veracity of their statements and more interested in revealing and celebrating internal cultural logics and expressions.

Even with the post-modernist turn, anthropology at UBC continued to be primarily driven by theoretical frames and models that saw Indigenous peoples and communities as a source of data to apply their external theories to, and by which to evaluate these ideas (see for example, Martindale and Nicholas 2014). Clearly the works of UBC anthropologists such as, but not limited to, Michael Ames (1992), Julie Cruikshank (1990, 1998, 2005), Bruce G. Miller (2001, 2004, 2011), or Robin Ridington (1978, 1988) demonstrate a deep-seated respect for Indigenous peoples and societies. Yet the concerns they focus on, while respectful and imbued with an Indigenous sensibility, were not beholden to a literalist interpretation of Indigenous narrative, as Henige so dismissively would have us believe. These are not scholars who have simply ignored or overlooked real empirical evidence that challenges, undermines, or critiques their scholarly interpretations. They engage with real Indigenous communities, consider their perspectives, and apply their academic training to making sense of the actually lived worlds of people they care about. Respectful research has deep roots at UBC.

Leona Sparrow, currently Director of Treaty, Lands, and Resources at Musqueam, described the importance of documenting Indigenous perspectives of work through a life and work history of her paternal grandparents (1976:1–4). In the opening section of his dissertation, James McDonald (1985) describes the parallel process of gaining permission to conduct research with an Indigenous community, in his case the Kitsumkalum First Nation.

> At the time when I was considering specific topics and seeking a study area, there occurred a happy coincidence: Kitsunkalum Band Council decided it wanted an anthropologist to make a study of their social history that would assist them in their land claims and economic development. Since I intended to do an historical study of the political economy of an Indian population, our paths came together in a mutually beneficial way. A relationship developed between the Council and myself in which the Band Council provided me with contacts, material support, guidance, and encouragement that not only facilitated the study greatly, but also lent it an orientation that incorporate Indian as well as academic expectations. (McDonald 1985:22)

Sparrow's approach prefigures, and defines, what can clearly be claimed as one of the core attributes of Indigenous-focused research by UBC anthropologists. McDonald's dissertation shows the practice in full form: respectful of community expectations, field-based, focused on long-term relations that take into account Indigenous perspectives while being firmly rooted within the protocols of scholarly discipline-based research. Members of the actual UBC School (not the ones tossed so glibly together by Henige) may well approach our research questions from different theoretical perspectives or personal experiences, but we do share a common commitment to respectful fact-based and community-grounded research. This is not, of course, what Henige meant when he called us the UBC School.

The UBC School, if we can be said to exist, can be summed up as our colleague Bruce G. Miller has recently done: "The persistent theme at UBC,… for all of us, independent of where we were trained, was *engagement* and the department decided around 2014 that the collective identity was of "grounded" researchers, whose research questions arose primarily from pressing questions derived from work with living populations" (2018:18). Miller goes on to say it would be incorrect to suggest we are "simply applied as opposed to theoretical or that these two stand in opposition" (2018:18). Rather, our approach reflects the fact that we are very much engaged with the "highly dynamic situation regarding Indigenous rights and their place in Canadian society. Especially over the last two decades First Nations have achieved a significant level of self-governance along with key legal victories concerning the Crown's obligation to consult with them concerning economic development and the Crown's fiduciary obligations" (Miller 2018:18).

The UBC School's principle of respectful engagement can be seen throughout and beyond (such as in the work of Susan Marsden) the Department of Anthropology and across much of its history. Borden's partnership with Musqueam, and specifically with Andrew Charles, Sr., created a relationship that persists and was recognized in UBC's Memorandum of Affiliation with the Musqueam Indian Band, on whose unceded territory UBC sits, in 2006. Department scholars have developed and maintained long-standing partnerships with communities (many Indigenous, but not all) around the world. This work strives for equitable and respectful rapport toward research that is empirically based, theoretically thoughtful, cognizant of historical and structural asymmetries, and directed toward meaningful and mutually beneficial goals. Work of this order facilitates, rather than impedes, science by advancing our cumulative understanding of complex issues and assessing our vulnerabilities to ethnocentric assumptions and bias. Though we did not think of ourselves as a formal movement until Henige's intemperate and poorly-researched screed, when we look around at the work of our colleagues, we see a shared vision of humility and thoroughness toward respectful engagement of complex issues that we are proud to be part of as the UBC School.

David Henige has awoken the sleeping giant, as Menzies's elders are wont to say when an issue of concern leads them to stand up and speak. Henige dismisses without due care or attention to detail scholarship that directly contradicts his beliefs and assertions. Yet we are left wondering why, if he claims an empirically-grounded approach to history, he does not even considered the scholarship that challenges his beliefs. We have offered Henige the thoroughness he did not provide to us. In our response we document in detail the thorough and extensive peer-reviewed research that makes the case that Indigenous histories are in fact history. More than that, we have laid out the outline of the actual UBC School—an approach to anthropology with Indigenous peoples that has always been empirically grounded, thorough, and respectful. It is a school that is there on the ground in community doing mutually beneficial research with real people, about real histories, and with real consequence.

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Portable Engravings of the Northeastern Paleoasiatics (Late Neolithic and Paleometal): An Attempt at Semantic and Ethnic Interpretation[1]

Margarita A. Kir'yak (Dikova)
Translation by Richard L. Bland

Abstract *In the world of prehistoric values, small art is as important as monumental art. Small art embodies ideological characteristics of the people who produced it. It is a mnemonic device for passing information from one generation to the next, both in verbal form and as a physical object. This art is very often concerned with religious/ mythological ideas; thus, understanding it contributes to an understanding of the people who created it. This article attempts to do just that.*

In the context of world cultural values, along with objects of early monumental art, graphics of small form are also important as a cultural-historical resource. Symbolic in essence, graphic art not only embodies ideas of an ideological character in a certain coding key but also appears as a mnemonic device, by means of which knowledge and memory of the past were communicated from generation to generation.

In the relics that have come to us from the depths of remote millennia, the world that surrounded early people—people and animals, sky and sun, earth and water—was focused in a simple "graphic formula."

This stratum of early representational activity reflects a certain stage in human perception of the surrounding world, demonstrates the capability of consciousness toward abstraction and creation of universal ideological concepts, and confirms the appeal to categories of early philosophy that sprouts from mythologizing everything existing on earth and in the cosmos.

In the collection of pieces of portable art from the northern Far East, the cosmological system of the universe, with its cosmic and spatial reference points, stands out as dominant. From the collection of graphics (on slate slabs, small flat pebbles, everyday tools, and flakes) it is possible to distinguish four subjects on our interesting theme. Three of these I obtained during excavations at the Rauchuvagytgyn I site, located above the Arctic Circle in western Chukotka. The fourth object was found by the archaeologist R. S. Vasil'evskii on Nedorazumeniya Island near Magadan, Russia.

Description of Subjects

The first subject is a complex composition engraved on a slate slab and framed by an oval that has a vertical tripartite division (Figure 1). Each part contains linear figures: in the upper section are an anthropomorphic pointer and an obtuse angle with broken sides having paired segments symmetrical to it that are intersecting and straight-lying; in the middle are a honeycomb illustration of double lines, an

[1] This article has not previously been published, as has been the case with *JONA*'s previous translated articles.

Map 1. Three subjects were found at the Rauchuvagytgyn 1 site; a fourth was found on Nedorazumeniya Island.

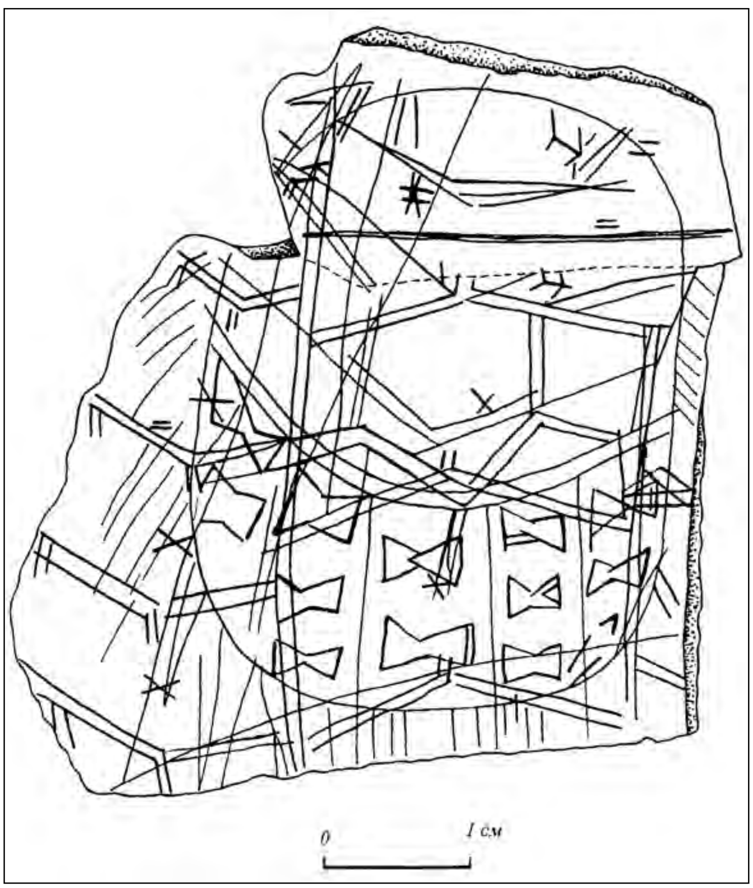

Figure 1. Graffiti with a complex three-tiered composition in an oval. From the Rauchuvagytgyn I site. Chukotka.

anthropomorphic pointer, and a straight cross; in the lower are figures in the form of triangles joined at the tops, distributed in five zones, and grouped in vertical rows in twos or threes. Superimposed upon the oval and its representational field is a multilayered stepped construction in the form of a double zigzag with paired projections at the top and evenly spaced, sloping crosses. The dimensions of the slab with the illustrations are 4.8 x 4.6 cm (Figure 1).

The second subject is a tripartite composition on a small flat stone. Engraved above is an illustration of a "dwelling" (a cylindrical base, conical roof with triangular bifurcation in place of a smoke hole), reminiscent of the outlines of a yurt of the Sayano-Altai peoples (Figure 2). The internal space is occupied by a

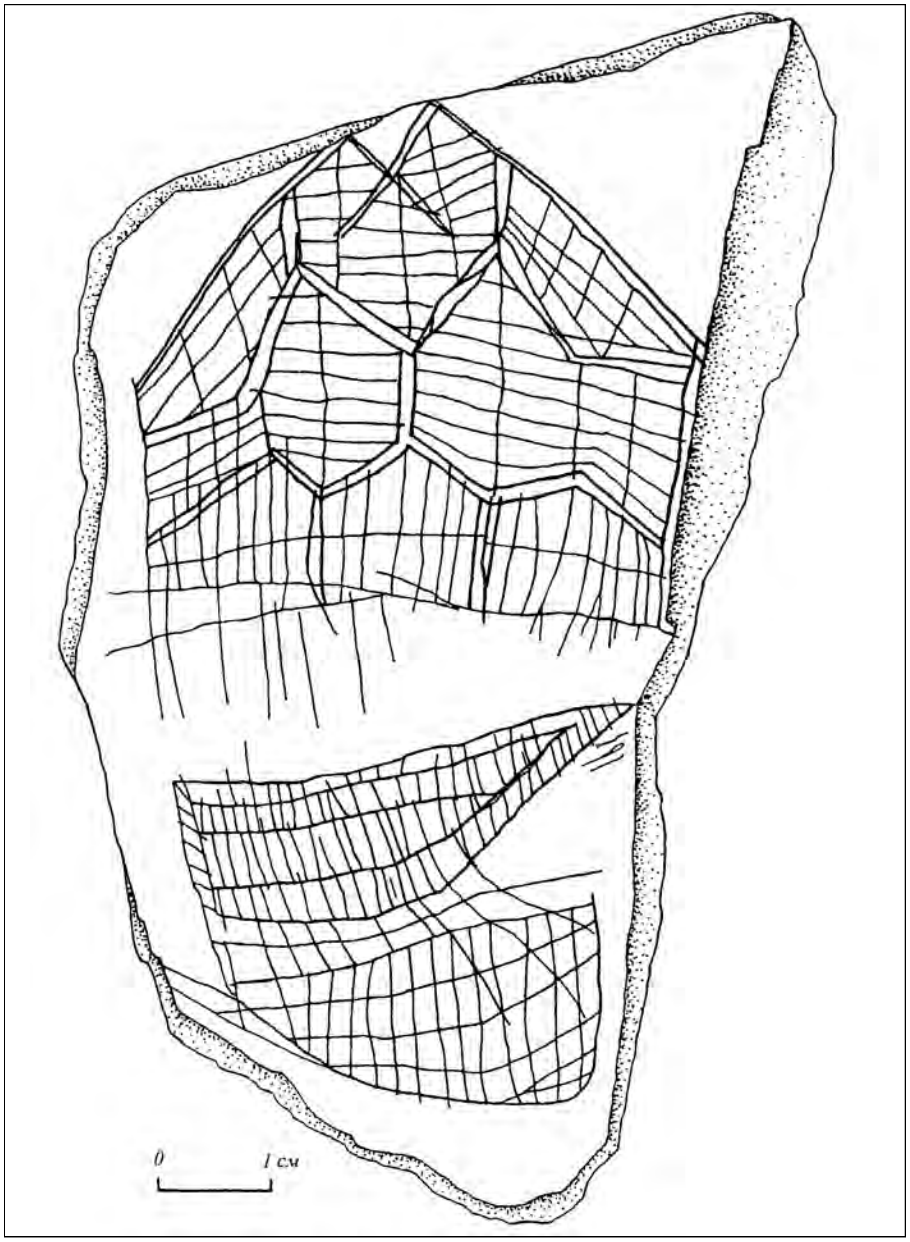

Figure 2. Graffiti depicting a dwelling-shaped design in the top tier. From the Rauchuvagytgyn I site. Chukotka.

honeycomb illustration and a multitude of vertical and horizontal line segments. In the middle part of the expositional surface is an illustration in the form of a thickly hatched enclosure. In the lower part is an illustration reminiscent of a fishnet. The dimensions are 9.9 x 5.5 cm.

The third subject is based on the mythological idea of dwelling-cosmos. It is close to the preceding subject. On a small flat pebble a three-stage geometrically regular latticed construction reminiscent of a truncated pyramid is illustrated (Figure 3), at the base of which (the upper plane) lies a square (in this projection it is a rhomb). On symmetrically placed lateral surfaces under the "roof," two spiral-like scrolls are scratched. The illustration attracts attention due to the image of the "construction" together with the system of the spatial coordinates; that is, it was made axonometrically. The dimensions are 4.3 x 3.9 cm.

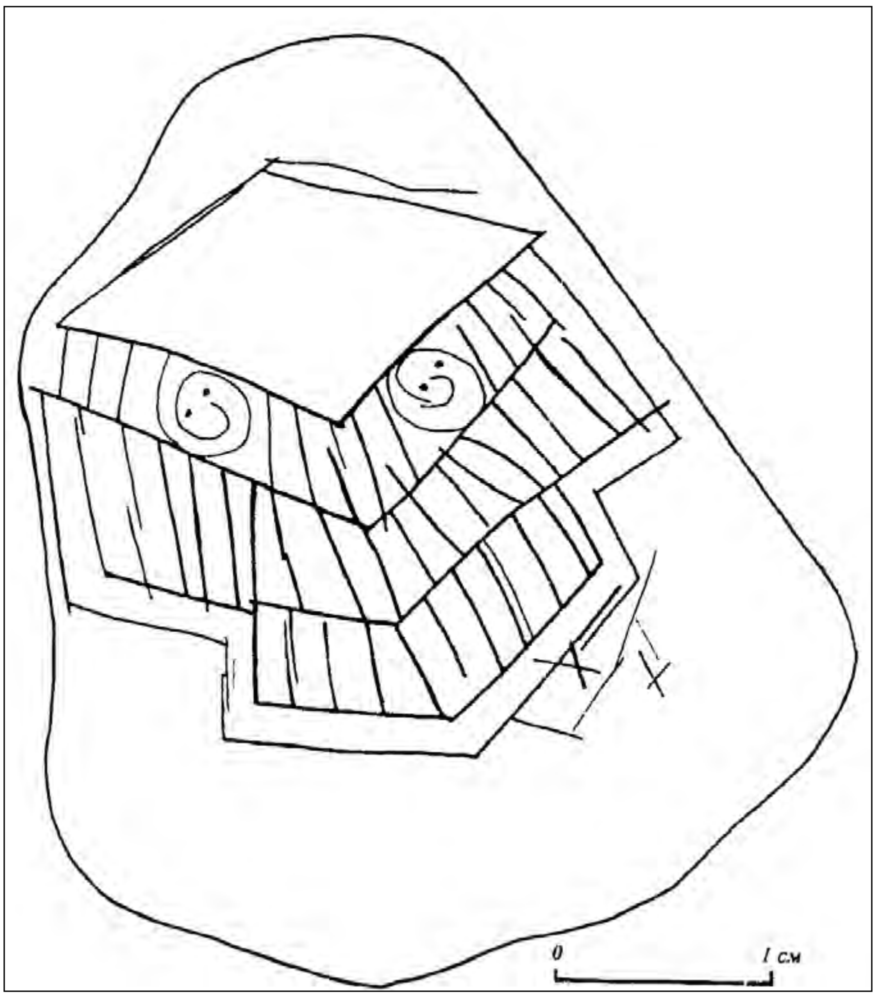

Figure 3. Graffiti with the image of a truncated pyramid. From the Rauchuvagytgyn I site. Chukotka.

The fourth subject is on a small, round pebble-pendant from Nedorazumeniya Island (Figure 4). It is a concise engraved illustration: a linear phallic figure with erect "hands" under which two dots are engraved symmetrical to the vertical axis, one of them transected by a straight cross. On the whole, the graphics are reminiscent of a mask. The dimensions are 2.7 x 2.8 cm.

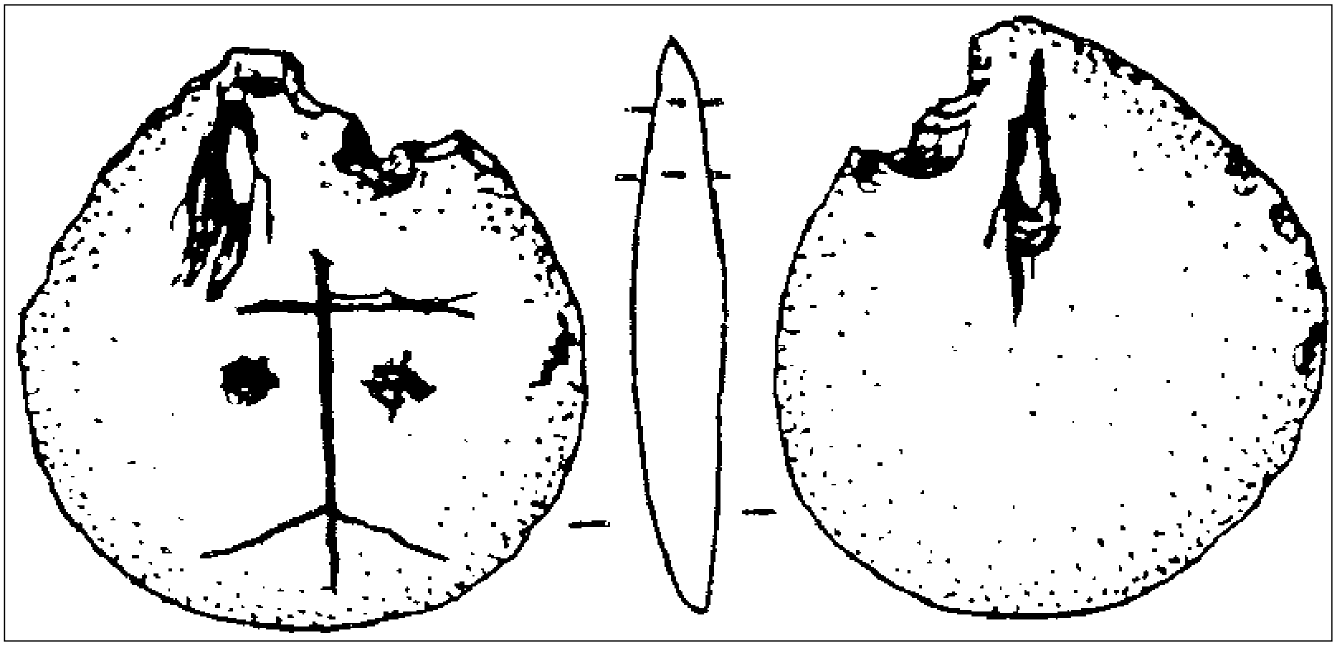

Figure 4. Graffiti with a linear shape with an anthropomorphic image. Nedorazumeniya (Magadan region). By R. S. Vasil'evskii.

Semantics

I will touch on the first subject, the composition with an oval as the basic form-making element. The oval, filled with linear images, is reminiscent of a shaman's drum with its pictographic illustrations and cosmic symbolism. Most productive in an associative plan are parallels in attributes of the drums of Sayano-Altai peoples (the Shortsi, Kachintsi, Sagaitsi, and others) as well as the Saami, Sel'kup, and Nganasany (Prokof'eva 1976; Potapov 1981; Alekseev 1984; Popov 1984). It is known that among Siberian peoples, the shaman's drum symbolized a planar model of the Universe.

In the Rauchuvagytgyn subject, the oval was made in three tiers: the upper is separated from the middle by a straight line, and the middle from the lower by a double inverted arc. Each tier has its own symbolism. The figure of the oval, composed as if of two mutually opposing arcs (the hatching is drawn from the lower) and just like the form of a drum, is correlated with the World Egg, the symbol of the Universe, in the mythology of many peoples of the world. It can be supposed that the ideas of the early Rauchuvagytgyn people about the cosmos and cosmogonic events are graphically reflected on the slab, and to these events belong "the establishment of the cosmic expanse (the separation of Heaven and

Earth, the forming of three cosmic zones, and the like), the filling of the expanse with concrete objects or abstract essences, the combining of everything real into one and separation of everything from one" (*Mify narodov*... 1988:7).

According to the canons of Chinese philosophy, from the World Egg emerged the primal ancestor, the cosmic person Pan-Gu, after whose birth the halves of the shell became Heaven and Earth (Okladnikova 1995:65). The symbolic expression of Heaven in the creation myth of the Yukagir appeared as a convex arc with the ends turned down, while the symbolic expression of the Earth is a concave arc with the ends turned up. The mythological marriage of Heaven and Earth gave birth to the Universe (Zhukova 1996a:31). This event is poeticized in the Kalevala (1956:25):

> From the egg, from the lower part,
> Emerged mother—the damp earth;
> From the egg, from the upper part,
> Arose the high heavenly vault...

In the Rauchuvagytgyn composition we are examining, an inverted arc is perceived as the symbolic border between the middle and lower worlds. In the views of the Yukagir, the sign of the inverted arc symbolizes the base of the world, the hearth, and is correlated with the maternal womb (Zhukova 1996a:30).

Above the basal components of the Rauchuvagytgyn graphics, the early artist engraved a structure in the form of a three-stage figure of double angles with projections at the top, which on the whole can be treated as the cosmic support—the World Tree in one of its variants. Among many peoples, images of the World Tree and the World Egg are used as symbols of the verticality that joins the upper and lower worlds, along which a shaman or mythological person moves (travels) (*Mify narodov*... 1987:1–234). In the Tungus-Manchurian languages the term that denotes a tree can be translated literally as "road" (*Mify narodov*... 1987:1–234).

The threefold structure of the World Tree includes the entire sphere of existence and brings opposites together: the upper world (Heaven), the middle world (Earth), the lower world (the subterranean kingdom); the past, the present, the future; predecessors, the present generation, descendants; day, night; north, south; etc. (*Mify narodov*... 1987:399–400). Among many peoples of the world, the circle (or oval) was considered the protective boundary against the influences of malignant forces. The three-stage figure of double angles with projections on top, as well as a few details of the image in our composition, fall outside the boundaries of the oval. Among some groups of Indians of the Northwest Coast of America, mystical objects (like the Milky Way—road of souls) could be transferred beyond the boundary of a circle as the protective zone (Okladnikova 1995:266).

Thus, the shaman's universal model of the Universe is in all probability represented by the symbolism of the World Egg and World Tree in the graphics from the western Chukotkan site of Rauchuvagytgyn I.

In the two subsequent subjects, the idea of the World model has a different graphic resolution.

In the collection from Lake Rauchuvagytgyn, there is a composition in which an image of a yurt-type dwelling of the Sayano-Altai peoples is present—with a high cylindrical lower part and a low conical form of the upper part with a circular bifurcation in the center. The illustration was made in X-ray style: the contour of the dwelling and internal details are outlined—the supporting structure, smoke hole, and hearth arrangement. In spite of the simplified schematic style, the image gives an idea of a real living structure, analogous to the second image in the western Chukotkan collection, which presents one of the components of the tripartite composition engraved on a flat stone with stepped fracture. On the top is a figure whose outline typologically repeats the preceding one (cylindrical base, conical roof, triangular bifurcation in place of the smoke hole), but the "living" area is filled with a multitude of linear signs, on the background of which a "honeycomb design" appears.

In many traditional cultures the dwelling is a mythological model of the World. Researchers assign the dwelling to the earliest metaphor of the Universe, while the planar model is the most recent (Okladnikova 1995:199). The form and structure of the dwelling in the views of the peoples of Siberia and America are associated from earliest times with "the mythological schema of the structure of the Universe." Semantically adequate comprehension of the internal area of the living structure and the methods of comparison of its elements with the modeled components of the Universe are characteristic both for Siberian peoples—Samoyed, Nganasani, Obsk Ugor, and Evenk—and for North American Indians (Okladnikova 1995:222).

The regularity and stability of this connection over a long period of time is explained by the fact that the cosmogonic schema seemed to be the basic unconscious arrangement forming the archetypes of the psyche (Okladnikova 1995:228).

The permanence of archetypes, which were formed in the Old Stone Age, is confirmed by ethnographic parallels. The ideas of the ancients about the model of the World, the structure of the Universe, the World Tree, were most conservatively preserved in the modeling—the decorations of the shamanic clothing of the peoples of Siberia and the Far East and attributes of the shaman's costume (Mazin 1984). The everyday clothing, but in larger degree festive (or burial) attire, also helps reveal the ideology of remote ancestors. Yukagir decoration on coats, footwear, and in the structure of details and decoration on the apron is a kind of code of cosmic symbolism (Zhukova 1994:51). Of all the parallels that can be drawn for deciphering graphics with complex construction of the dwelling, those with the best prospects, in my view, are analogous to Yukagir aprons.

The apron is divided into three parts by zones of decoration: upper, middle, and lower. The upper part as a rule does not have a design, but rather silver and copper disks symbolizing the sun, moon, and planets are sewn on the breast. Sometimes small pieces of blue silk symbolizing Heaven are sewn under them (Zhukova 1994:24). In the center of the apron or just below are sewn an ornamented band of rectangular or semilunar form with the ends raised upward (or sometimes several ornamented bands)—"the heart of the apron" or "the palette of life"—symbolically absorbing in itself everything essential for a person's normal living activity: native land, hearth, kinsmen (Zhukova 1994:51).

The closed outline of the apron (trapezoidal top and rectangular or square bottom) can be viewed as the unity of Heaven and Earth, in which the model of the Yukagir's Universe was graphically reflected (Zhukova 1994:32).

The Rauchuvagytgyn illustration is amazingly similar to an image on birch bark from the dwelling complex of the Ymyyakhtakh culture, investigated by Yakutian archaeologists at the Belaya Gora site (Indigirka River basin, Yakutia). Specialists believe that an article of clothing is engraved on the birch bark—a bib-apron, traditional for the Even, Evenk, and Yukagir (Everstov 1999:55)—though, in my view, the upper illustration on the birch bark is very similar to a dwelling structure.

The parallels cited permit treating the composition from Lake Rauchuvagytgyn as a material embodiment of a mythological model of the World through the symbolism of a dwelling.

Thus, we have examined two variants of the mythological Universe that were embodied in Rauchuvagytgyn graphics. A third variant, close in mythological view to the second, is also possible. On a small flat pebble a three-stage geometrically regular lattice structure is illustrated, at the base of which lies a square or rhomb. The structure is reminiscent of a truncated pyramid. A dwelling in the form of a truncated pyramid was encountered among certain groups of people (Sagaitsi, Shortsi, Kachintsi, Altai-Kizhi Teleut; Dolgan; Sel'kup; Ket; Khant; Yakut; Evenk; Yukagir), among whom eight had a winter (permanent) dwelling of such form (*Istoriko-etnograficheskii*... 1961:132–135).

The sacred meaning of the Rauchuvagytgyn image is in the spirals located in the upper tier of the two (geometric) planes (sides) of the presumed dwelling, below the figure of the rhomb. The ancients' combined view of the Universe is possibly engraved in this composition. In many world mythologies the Universe has a four-part horizontal and tripartite vertical partition.

The world constructed horizontally was represented as four sides, a square or a rhomb, modeling the lands of the world and having two coordinates (two horizontal axes): left-right and front-back (*Mify narodov*... 1987:403). In the Rauchuvagytgyn graphics the axes pass through the angles of the rhomb (each of the angles indicates the direction north-south, west-east). In the mythology of the Yukagir that comes to us, the formula of the Universe occurs according to which the upper land is correlated with the rainbow or the dwelling (see above), the lower is represented by a "truncated rectangular pyramid," and they are separated by a concave (in relation to the upper) middle land (Zhukova 1994:49).

The spiral in the mythological ideas of some peoples of the world played the primary role in cosmogenesis. Thus, in the mythology of the Bambara and the Dogon (Mali), the primary role in the creation of the world was allotted to vibration and spiral-like movements. "In the cosmogonic myths of the Bambara, during the course of creation of the universe... the spirit Io, 22 basic elements, and 22 coils of the spiral appeared in one of the stages" (*Mify narodov*... 1988:161). The image of the spiral is also treated as solar symbolism. Thus the third composition of the Rauchuvagytgyn graphics can be placed in the same semantic series with the preceding.

A simplified linear embodiment of the model of the Universe, as it is represented on the pendant from Nedorazumeniya Island, is entirely different. The image is superficially reminiscent of a mask or a miniature copy of a shaman's drum.

I will dwell on the constituent elements of the image presented by the corresponding symbolic code and try to decipher it. The vertical segment with two flanking dots (on one of them—the right one—a straight cross is engraved) is a graphic reflection of the phallus. In world cultures, this symbol is known as the sign of the lingam, where the dots at the sides denote testicles (Demirkhanyan 1985:141). The triangular symbol with the point directed upward, as well as with the turned-down top (with and without transecting line segment), is, based on its semantics, the female symbol. Thus, the central figure has two halves—male and female—in the combination of which one of the principles of organization of the vertical component is reflected, and specifically, the "transmission of movement directed upward, imitating the growth and formation of a tree."

The form of the tree (one of the basic mythological constants) in the mask we are examining is reinforced by the transverse line segment located at the top, above the two dots, and the "female" transected triangle below (correspondingly, the crown and the root). The symbol in the form of the line segment growing from the triangle-bosom of the tree transmits the idea of vertical growth and has a sacred meaning (Demirkhanyan 1985:132).

The straight cross is a solar symbol and in this composition marks the sun. Correspondingly, the second dot, located symmetrically to it, is perceived as the image of the moon.

All the elements of the composition we are examining create features of an anthropomorphic mask. In many cultures of the world, the image of a face is represented by a graphic symbol—a vertical line segment and two flanking dots. This symbol is widespread, for example, in Armenian petroglyphs as a sign of the phallus, having the meaning of the World Tree (Demirkhanyan 1985:141). As analysis of the details of the mask shows, it is also polysemantic, and its creator was not only an artist but also a philosopher, who was able to show a complex ideological concept of the universal model of the Universe in its three-part structure by a simple graphic formula, symbolizing "continual renewal of life." In the center of the universe he constructed is the person in two of his roles—male and female—which, joined in one, personify the inexhaustible spring that nourishes humanity.

Ethnic Interpretation

It is well known that traditional elements of culture are retained the longest in the spiritual sphere. In the view of investigators, "sacred ideas are first mythologically formed and therefore are preserved rather long in the consciousness" (Gening 1989:163, 164). Some elements of world-view and of the religious ceremonial system of early tribes, which were materially embodied in cliff art and portable art during the Neolithic and Paleometal periods, can be found in the ethnographic material.

Above, I cited the comparison of archaeological materials (graphic illustrations on stone from the Rauchuvagytgyn I site in Chukotka and birch bark from the Belaya Gora site in Yakutia) with the ethnographic (through a series of Yukagir aprons), which permitted interpreting the archaeological representational material as pre-Yukagir. Yakutian archaeologists independently came to the same conclusion. In addition to the cited image, a representational block of graphics from the Chukotkan site mentioned also attests to a Yukagir ethnos: this is confirmed by decorative motifs and a multitude of brandlike signs in compositions. The ethnic interpretation is also strengthened by artifacts from sites (Rauchuvagytgyn I and Belaya Gora) belonging to the terminal stage of the Ymyyakhtakh culture, whose burial complexes contain traces (decorations) of clothing characteristic of the Yukagir.

Of greatest prospect in the attempt to find ethnographic parallels in the graphics from Nedorazumeniya Island are Evenk materials. The closest analogies can be traced in them not only in graphic form but also in the semantic signs, by means of which this form was created.

Above all, the similarity with drums and drum covers of different groups of northern (Yenisei) Evenk, both in form and in the system of pictograms, is revealed. As already stated, the form of the drum in the mythology of many peoples of the world corresponds to the World Egg—the symbol of the Universe. In the cosmological structure of the Universe, represented by a variant of the World Egg, are the sun, moon, and stars. Based on the laws of mythological oppositions, the sun and moon as symbols of day and night, light and dark, and ultimately life and death are obligatory components of the Universe.

On the drum of the Zeisk Evenk, these heavenly bodies are represented by two circles: night is marked by a female anthropomorphic image (in correspondence with a legend widespread in Siberia), and day with a straight cross intersecting a circle in the center and the ends of the cross lines extended beyond the circle's boundary. The early master used an identical method in the illustration of the solar symbol (a cross engraved on one of the two flanking points) in the making of the pendant from Nedorazumeniya Island, reminiscent of a shaman's drum in miniature.

Direct parallels to this image, symbolizing the base of the Universe or World Tree, are encountered also in the decorations of the aprons of some groups of Evenk. S. V. Ivanov, who has studied the graphic art materials of peoples of Siberia and the Far East, revealed the semantics of this symbol and used the semantic meaning of the drum and its parts as his basis for writing that "in the indicated painting a reflection of cosmogonic ideas should most probably be seen" and "it is hardly possible to doubt that there is a connection between the graphic figures on drums and images of the Universe and sun... on the metal pendants of Evenk shamans' caftans" (Ivanov 1954:180, 181).

Comparing the already-cited archaeological object from Nedorazumeniya Island and ethnographic sources, one can see the great similarity in the graphic formula reflecting the idea of the World Tree, as well as in the arrangement of several details having a definite semantic accent. It is also impossible to ignore the fact that, first, the enlisted ethnographic sources are represented not by individual objects but by large series of them (this excludes coincidence), and second, they belong to the northern group of Evenk.

In 1996, materials from the late Bronze Age site of Torgazhak (Minusinsk depression, Yenisei basin) were published by D. G. Savinov. The researcher considers the discovery of a large number of engraved pebbles—222 specimens—as the main result of the excavations. Graphics with geometric images were distinguished as a separate group. In the context of the theme that interests us here, I want to turn attention to illustrations that render a model of the world. Among them one is especially attractive. The graphic formula of the image is absolutely identical to the tracings on the drums of the Yenisei Evenk, which is not a simple coincidence. In the Torgazhak graphics, the Universe is modeled in the same semantic key and possibly reflects mythological ideas of the same ethnocultural community.

The similarity of the illustrative elements of the pendant from Nedorazumeniya Island and the Yenisei materials (ethnic and archaeological) may attest to cultural contacts and interactions of pre-Evenk and pre-Koryak cultures in the territory of northwestern Priokhot'e, not only on the level of borrowing but also in deeper ethnic connections.

Thus, it can be concluded that in the Neolithic and Paleometal, conceptually formed ideas of the surrounding world existed in the portable graphics of the ancient "Paleoasiatics" (in the broad sense). One of the most important ideological paradigms was the model of the Universe.

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A Comment from Mark G. Plew on Kir'yak's Portable Engravings of the Northeastern Paleoasiatics

Mark G. Plew

Jan Kee and I published a note on incised stones from Idaho (Kee and Plew 2015). The paper was based on a presentation on portable art of Western North America presented at the 2014 SAA meetings and published in *JONA*. We described four distinct types that occur in different geographic settings in association with different site types and over a period of several thousand years, though more common in the Late Holocene. These included stones with parallel lines located on the face or margins of stones with horizontal, vertical, or diagonal lines—or a combination. A second type was characterized by centrally placed hachure, while a third type consisted of irregular/multidirectional lines lacking discernable patterning. A final type included what appeared to be more decorative—combining zig-zags, ladders, parallel lines and chevrons. Reviewing the Kir'yak paper, there appear some similarities in design motifs to those in Idaho—though all would fit our Type 4 (Figures A, B, and C)—being more decorative items.

Kir'yak views portable or graphic art as a mnemonic device by which knowledge and memory are communicated across generations. Though I find this interpretation and her specific interpretations of the individual items to be a bit of a reach, I find her assertion that these "graphic formula" reflect a "certain stage in human perception of the surrounding world" more so. This is not to argue that prehistoric peoples were not capable of abstraction and the creation of universal ideological concepts, as there are many good examples of non-portable art that substantially pre-date the time of these items. We found ethnographic documentation of the use and importance of incised stones in ceremonial/ritual contexts in Northern and Southeastern Idaho—some in Northern Idaho associated with waterways and in one instance, a design has been interpreted as possibly reflecting a landscape feature. This speaking to the likelihood that portable art served multiple functions not all relating to cosmos. The dilemma we commonly face when thinking about the meaning of and the underlying functions of non-portable art is clear in this paper. Although some motifs common globally are undoubtedly reflective of common human abstractions, I am uncomfortable with the author's attempt to decode these "graphic formula."

Figures from Kee and Plew 2015 are shown below for comparative purposes.

Note from the Editor—Readers may interested to know that an article on incised stones recently appeared in *American Antiquity*.

Thomas, David Hurst
2019 A Shoshonean Prayerstone Hypothesis: Ritual Cartographies of Great Basin Incised Stones. *American Antiquity*, 84(1):1–25.

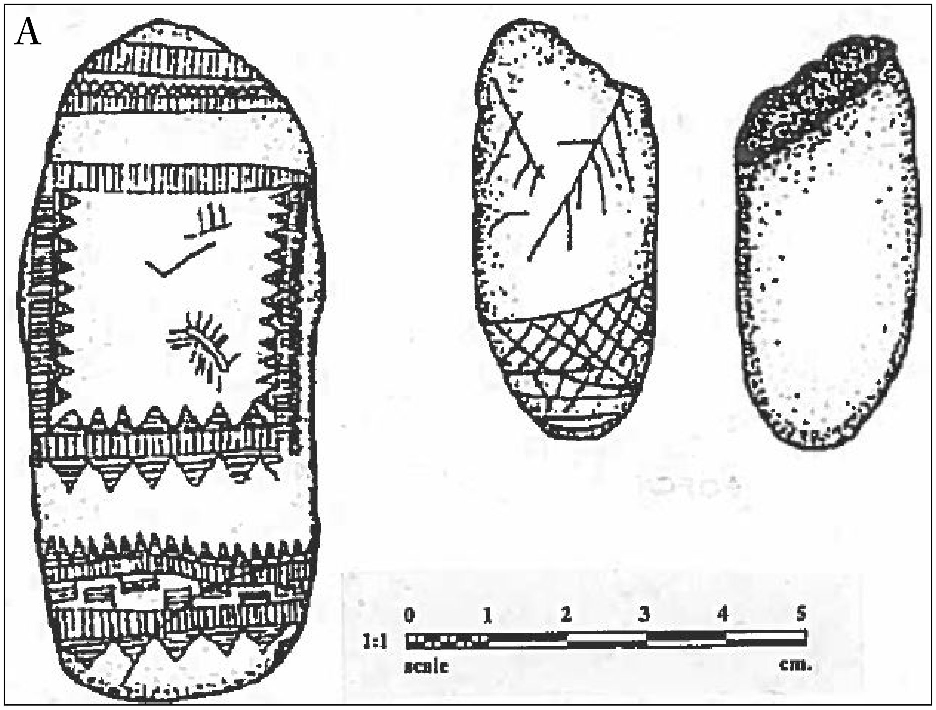

Figure A. Type 4 incised stones, Pend Oreille River, northern Idaho. This item measures 5.5 x 2.0 x 0.3 cm (Kee 2004).

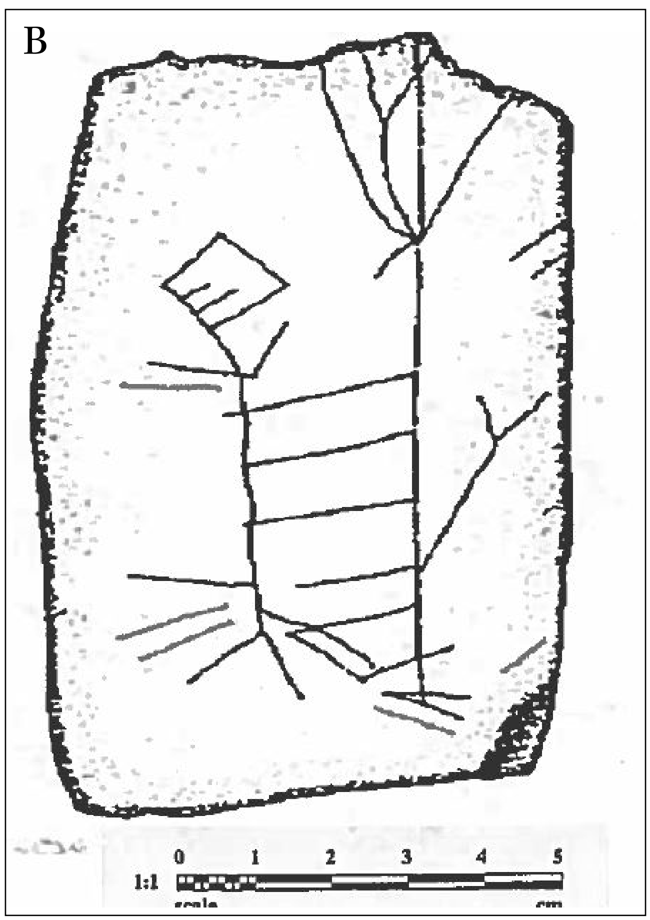

Figure B. Type 4 incised stones, Pend Oreille River, northern Idaho (Kee 2004).

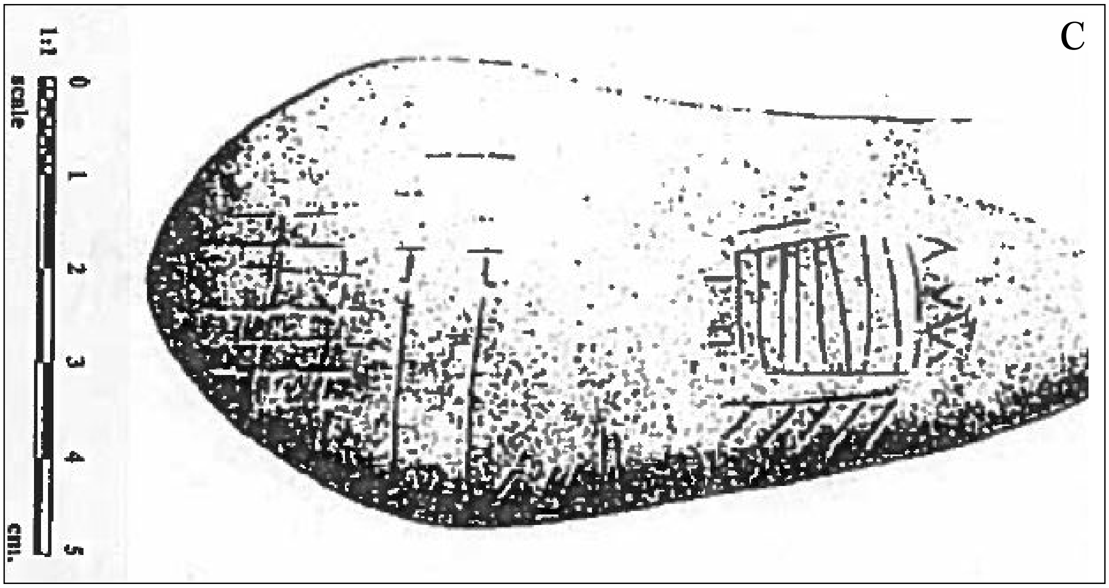

Figure C. Type 4 incised stone, Bliss Site, southwest Idaho. Artifact measures 8.5 x 4.1–2.0 x 1.2 cm (Plew 1981).

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Why Don't We Write More? Essays on Writing and Publishing Anthropological Research

Introduction—Darby C. Stapp and Julia G. Longenecker

Part I Essays—Virginia L. Butler, Kenneth M. Ames, Roy Carlson, Alice B. Kehoe, R. Lee Lyman, Robert R. Mierendorf, and Mark G. Plew

Part II Essays—Thomas F. King, Dennis Griffin, Dale R. Croes, Kevin J. Lyons, Madonna L. Moss, Mark S. Warner, and Dennis Dauble

Part III Essays—Bruce Granville Miller, Jay Miller, Nathaniel D. Reynolds, Astrida R. Blukis Onat, and Rodney Frey

Conclusion—Tiffany J. Fulkerson and Shannon Tushingham

Abstract *The editors of the Journal of Northwest Anthropology invited twenty-five colleagues to share their perspectives on anthropological writing and publishing in an essay format. The purpose was to collect experiences, insights, and suggestions from experienced authors to assist other professionals in writing and publishing their own research. Nineteen of those invited accepted the challenge. The group includes academic and practicing anthropologists, archaeologists, and ecologists. Collectively, the group has written or co-written more than 150 books, 150 chapters in books, and more than 1,100 articles in professional journals. The essays contain personal writing-related anecdotes and philosophies, describe the changes occurring in the publishing industry, explore the benefits that can accrue from writing, and provide tips to improve one's writing to increase the chances of getting published.*

Introduction—Darby C. Stapp and Julia G. Longenecker

The world of publishing, especially academic-related publishing, is in a state of flux. The changes that have resulted from the advances in digital computing and communication technology have revolutionized the way we write, the way we read, the way we present our information, and the way we disseminate our information. Not surprisingly, the economics of publishing have been turned upside down. All of these factors are impacting the way that scholars—especially young scholars—write. The impact on disciplines such as anthropology, for which technical books and academic journals are very much the lifeblood, has been, and will continue to be, significant.

The *Journal of Northwest Anthropology* (*JONA*) has a vested interest in the changes that are occurring throughout the publishing industry and the impacts that will be felt in the academic and business sides of anthropology. As a regional anthropology journal, we must incorporate new technology where we can, adapt to market conditions as they shift, and provide our readers with the products they expect, be they books or journals, printed or digital. We must balance our mission to disseminate anthropological research with the need to stay economically viable. More than anything, however, we need content. Without manuscripts to publish, none of the rest of it—subscribers, journal design, publishing software, websites, etc.—matter. For this reason, we spend a considerable amount of time throughout the year talking with colleagues and staying up on developments in the field to keep a steady and diverse stream of quality manuscripts arriving in our inbox.

The collection of articles presented below is a result of one of these efforts, which started as an email discussion between one of us (DCS) and Tiffany Fulkerson and Shannon Tushingham (both *JONA* authors) at Washington State University concerning their research on gender, profession, publishing, and the peer-review process. At one point, the topic shifted to their recent suggestion for creating a new non-peer review journal for the Pacific Northwest, which they think might address some of the publishing problems that appear related to the peer review process (Fulkerson and Tushingham 2018). An additional thought Tiffany had was to devote an issue of *JONA* to the various forces that impact anthropological writing and publishing.

This idea struck a chord with us and within a week we decided to ask our *JONA* family to share their experiences and ideas about professional anthropological writing. We would ask for relatively short essays (1,000 words, plus or minus 500) about writing from a diverse group of *JONA* authors and peer reviewers and publish them in an upcoming issue of *JONA*. We would write an introductory essay and, since Tiffany had come up with the idea, she and Shannon could write a concluding essay.

But would anyone do it? To find out we composed the e-mail and then started down the *JONA* mailbox selecting individuals who we knew had published a lot or who we knew had a strong interest in the challenge of publishing anthropological research. We sent the email to twenty-five colleagues on October 26, 2018. To our surprise we had acceptances from a dozen colleagues within a few days, and by the deadline in December, we had nineteen completed essays (53% came from academic settings, 47% from applied settings; 79% are male, 21% are female; 63% are archaeologists, 26% cultural anthropologists, 11% ecologists (one terrestrial, one aquatic); 100% are white with average age of approximately 65). Collectively, this group of writers has written or co-written more than 150 books and more than 1,300 book chapters and articles in professional journals.

To help put the contributed essays in context, we provide the actual email sent to colleagues below to show exactly what was asked of each writer:

> Dear Colleague,
>
> I have been in discussion with Tiffany Fulkerson and Shannon Tushingham (WSU) about anthropological writing and publishing. While Tiffany and Shannon are focusing on issues surrounding gender and publishing (or lack thereof), part of our discussions have morphed into tangential topics: the (declining) state of anthropological writing in general, our perceptions that professionals are writing less these days, and ways we might encourage professionals to continue/begin writing (young, mid-career, retired).
>
> At one point Tiffany suggested that perhaps *JONA* could play a role in promoting anthropological writing. Julie and I have thought about

that suggestion in the last few weeks and come up with the concept of publishing a series of short essays by writers about writing. I'm writing to ask you, as someone who has published a variety of things in *JONA* and elsewhere, to write something relatively short for us, say 1,000 words (plus or minus 500 words), concerning your personal thoughts on anthropological writing. Below are some questions that might ignite some thoughts:

- what has been your writing philosophy?
- what has motivated you to write professionally?
- what challenges have you had to overcome to get published?
- what challenges do you see today in the world of anthropological publishing?
- what suggestions do you have for would-be writers?
- how can we as a profession ensure that anthropologists in the Pacific Northwest of all types and backgrounds continue to publish?

These questions are not intended to be answered in toto, but they could be. But more so, the intent is to offer some ideas and let you go with what moves you.

If interested, our suggestion is for you to let the words flow as they might. We see these essays as written in an informal style, inspirational, and of a personal nature (i.e., what you think, what you have experienced, what you would like to see). We're interested less on explaining the nature of the problem, and more on exploring potential solutions to the dearth of manuscripts being produced for both peer review and non-peer review outlets.

Please think about our request and let us know if you can participate. We would like something around December 15 so we can get it all put together for the next *JONA* in spring 2019 (v. 53, n.1).

The current plan is to include the essays along with an introduction to the set. Each essay will include a brief introduction about the writer with a summary of writing accomplishments.

If per chance we get an overabundance of responses, we can include more or perhaps continue the discussion in the next *JONA*.

Thank you for considering the request on this important topic.

Darby

As you read through the essays, you will see that they vary widely in both content and style (Table 1). Almost every essay provides anecdotes concerning the authors' writing past, professional responsibilities to write, and the challenges that writers face. Many of the essays provide suggestions to young professionals to help them hone their writing craft and get published. The essays provide insights to the past and present publishing world and the technological advancements that are stimulating change. Several authors address specific topics such as gender and Indigenous peoples.

The essays are divided into three parts. Part 1 essays were written by archaeologists and tend to be comprehensive. Part 2 essays were written by archaeologists and an aquatic ecologist, tend to be short, and focused on two or three topics. Part 3 essays were written by cultural anthropologists, an archaeologist, and a terrestrial ecologist, are longer, and explore cultures outside the academic world. Following the essays, Fulkerson and Tushingham present a conclusion, which draws on the essays and addresses the future of anthropological publishing.

We encourage you to read all of the essays and believe you will find them as interesting, insightful, and inspirational as we did. We are most thankful to the nineteen authors who took time from their busy schedules to share their thoughts on writing and publishing. Clearly, this group of prolific writers feels strongly on the need for anthropology to continue its strong tradition of documenting and sharing what we learn. We can only hope that the new generations of anthropologists will follow in their footsteps and adapt to this rapidly changing world where new technology is upending the way we communicate.

This collection of essays has also been formatted as a standalone publication and can be found as:

Stapp, Darby C., Julia G. Longenecker, Tiffany J. Fulkerson, and Shannon Tushingham, editors
2019 *Why Don't We Write More? Essays on Writing and Publishing Anthropological Research. Journal of Northwest Anthropology* special reprint, https://www.northwestanthropology.com/publications. Richland, WA: Northwest Anthropology LLC.

Table 1. Topics Discussed in the Nineteen Essays

Essay	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	
Author	Butler	Ames	Carlson	Kehoe	Lyman	Mierendorf	Plew	King	Griffin	Croes	Lyons	Moss	Warner	Dauble	B. Miller	J. Miller	Reynolds	Blukis Onat	Frey	Total
Anecdotes		X	X	X	X		X		X	X	X	X		X	X	X	X	X	X	15
Writing Philosophy	X		X	X		X	X	X		X	X		X		X	X	X	X	X	14
Why Publish/ Write? Motivation	X	X		X	X	X	X	X	X	X		X			X	X			X	13
Publication Obstacles/ Challenges	X	X		X	X	X		X				X	X		X	X		X		11
Publication Trends			X	X			X	X	X			X	X			X	X		X	10
What to Write		X	X	X		X			X						X	X	X	X	X	10
Writing Tips/ Suggestions	X	X			X	X		X	X		X				X			X	X	10
Publication Outlets		X	X	X			X	X				X			X	X			X	9
CRM Impacts		X	X				X	X	X											5
Indigenous Perspectives						X				X					X	X	X	X	X	7
Writing for the Public					X	X			X					X						4
Publishing, Editors, Peer-Reviews												X				X		X		3
Gender				X								X								2
Total	4	7	6	8	5	7	6	7	7	4	3	7	3	2	8	9	5	7	8	

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Fulkerson, Tiffany J., and Shannon Tushingham
2018 Equity, Multivocality, and the Need for a Non-Peer Reviewed Journal for Pacific Northwest Archaeology. *AWA News*, 22(3):9–10.

Part 1

Part 1 contains the following essays:

- "Reflections on Writing" by Virginia L. Butler, professor and chair of the Department of Anthropology at Portland State University, where she has been since 1994. She is an archaeologist specializing in long-term relationships between people and animals, which she explores mainly using zooarchaeology. She earned her M.A. and Ph.D. from the University of Washington. Butler has published sixty-eight peer-reviewed articles and book chapters, as well as twenty-nine contract reports—mainly chapters and appendices to which she has contributed.
- "On Writing and Publishing" by Kenneth M. Ames, Professor Emeritus of Anthropology at Portland State University. He is an archaeologist. He received a Ph.D. in Anthropology from Washington State University in 1976. Ames has authored seventy-one peer-reviewed articles and chapters, and has written or edited five books; he has authored or contributed to thirty-three CRM reports.
- "Writing and Publishing Research and the Electronic Revolution" by Roy Carlson, founding Chair (1970) of the Department of Archaeology, and Professor Emeritus at Simon Fraser University. B.A. 1952, M.A. 1954 U. of Washington. Ph.D. U. of Arizona 1961. Field research and publications on the Northwest Coast, Plateau, Southwest, and north Africa. Carlson has published eighty articles, including chapters in books and monographs; sole-authored six books; and edited or contributed to seven multiple-authored books.
- "It's Writing, or Vacuuming" by Alice B. Kehoe, Professor of Anthropology, emeritus, Marquette University, Milwaukee, WI. Her Ph.D. 1964, from Harvard University drew upon fieldwork with a Ghost Dance religion congregation in Saskatchewan. Besides ethnographic and ethnohistorical research, she carried out archaeological fieldwork in Montana and Saskatchewan with her husband, Thomas F. Kehoe. Kehoe has written seventeen books and has coedited four.
- "On Writing Paleozoology, Zooarchaeology, Archaeology, and in General" by R. Lee Lyman, paleozoologist and archaeologist, Professor Emeritus at the University of Missouri-Columbia. He received a Ph.D. in Anthropology in 1982 from the University of Washington. Since 1975, Lyman has published 20 books (5 as sole author, 9 as co-author, 6 as co-editor), more than 170 journal articles, and 51 book chapters. He has also authored 85 contract reports (mostly chapters in reports).
- "Writing to a More Inclusive Readership" by Robert R. Mierendorf, a career anthropologist since the 1970s, specializing in precontact period archaeology of the Pacific Northwest. Following retirement after twenty-plus years as park archaeologist at North Cascades National Park, he continues researching and writing through his private consultancy. Mierendorf has published thirty-two articles, two books, and five chapters in books.
- "A Writing Philosophy" by Mark G. Plew, Ph.D., Indiana University. He is University Distinguished Professor of Anthropology at Boise State University. His primary research interests are in hunter-gatherer archaeology of the Snake River Plain and Northeastern South America. Plew has published 253 books, monographs, and journal articles.

Reflections on Writing—Virginia L. Butler

I am pleased to have been asked to contribute to this *JONA* issue that includes essays about the importance of writing in anthropology. I think about writing a lot as a researcher and college professor. Writing is so fundamental to my scholarship, it is difficult for me to express why it is important. It's almost like breathing or eating. If you decide you want to be a scholar or "do research," you have a responsibility to share what you have learned to a broader audience. Otherwise—only *you* have the knowledge that comes from research. In another fundamental way, the act of writing crystalizes what you have learned, forcing you to logically explain your insight. Sometimes (maybe often), it exposes important flaws in your logic. Writing forces you to link various threads (initial goals, larger theory, and empirical findings) in a logical, coherent way. Writing is not an independent act after you complete laboratory or field work; it is an integral part of the research process. As you write, you are "figuring things out." Noted author Elie Wiesel captures this idea well: "I write to understand as much as to be understood."

In the text that follows, I share reflections on what motivates me to write, my general philosophy about writing, how I overcome challenges when writing, and suggest things developing writers can do to support their practice.

Motivation

Various factors motivate me to write. Since I like doing research, I feel a responsibility to share what I do as noted above. Whether it's the tax-payer that funds my research or a private foundation, I feel guilty if I do not "write something up" and get it published so it is part of the public record. Beyond guilt, I enjoy the sense of community in writing articles, building on other scholars' ideas and then thinking that someone will build on mine. Perhaps this is odd to admit, but posterity motivates me too. When I'm long gone, I like thinking that maybe someone will read something I've written and build on an idea there; and then pass on a kernel of me into the future.

Philosophy

My writing is guided by principles articulated in Strunk and White's classic, *The Elements of Style*. My 11th grade English teacher recommended this book to me, and I've had a copy ever since. Rules I draw on especially include the importance of active voice and the need to use specific, concrete language. But perhaps "rule 13," which highlights brevity, is my favorite.

> Vigorous writing is concise. A sentence should contain no unnecessary words, a paragraph no unnecessary sentences, for the same reason that a drawing should have no unnecessary lines and a machine no unnecessary parts. This requires not that the writer make all sentences short, or that he avoid all detail and treat his subjects only in outline, but that *every word tell.* (Strunk and White 1962:17; emphasis added)

As I write and then edit my own writing, I sense Strunk and White at my shoulder, whispering in my ear: "is that paragraph, sentence, phrase, or word necessary? Are these helping to *tell your story*?" If I can't answer yes, I omit the words. Many writers (including students) suggest that stripped down writing is dull. I will counter this criticism with "less is more." Writing that tries to convey ideas simply and clearly and uses active voice and active verbs, creates vibrant writing that can compel a reader to read more. To me, the best writing is active and concise, includes all the important detail, but nothing extra.

Challenges to Overcome

Writing is hard. It takes time, patience, and discipline to write. I can spend half a day on one paragraph. I can write 2,000 words and then decide most of them do not contribute to the paper I'm *really* trying to write. Some people are faster than others are at writing, so the hardship varies. But practically everyone who commits to writing knows it can be hard and lonely. So, how do researchers (or writers of any kind) overcome this very real challenge? Why do we persist in doing something that is so hard? I'm not sure how others do it, but I've become friends with "delayed gratification." I get a huge rush of satisfaction when I complete a paper, then again when it is published. This helps me push through challenging writing times, it keeps me inside when the sun is shining outside. I keep telling myself how wonderful I'm going to feel when I'm done!

Some aspects of writing I truly enjoy, especially when I can view a writing project as a puzzle. The components of the manuscript for example, like the research context, project goals, results and implications, are pieces of the puzzle (and parts of these in turn are smaller units of the puzzle). I ask, what is the best way of arranging these puzzle pieces to tell the story in the most compelling way? When do I introduce this idea, or that line of evidence? I like moving pieces around, trying them out in different places to see what works best.

Suggestions for Developing Writers

First, I recommend that developing writers read widely—across all kinds of writing—and read a lot. Read academic books and articles and essays and fiction written for popular audiences (the *New Yorker* is one of my favorites). As you read, critically reflect on the writing, asking yourself whether the writing "is working"—driving you to read more, or not. Analyze how an author develops an argument, presents their ideas. Find time to read while you are writing. This practice will connect you with the writing process and give you a sense of community with others that are writing.

If you are getting started on a writing project, make yourself sit in front of the computer and commit to writing on a regular schedule. Do not wait for inspiration—you may wait a long time. The simple act of trying to write will generate some writing, promise. That start will then get re-written likely many times. That writing start will then join with the next piece of the project, and then the one after that. When you are "stuck" in your writing, try to figure out what is blocking you, but don't obsess over it. Maybe you need to work on another section for a while. When you return to the section in which you were stalled, you'll often have a new perspective and can find ways to power through. Be patient, keep trying. Think about how happy you're going to be when you have a finished draft.

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1962 *The Elements of Style*. Originally by William R. Strunk, Jr., with revisions, an Introduction and a new chapter on writing by E. B. White. MacMillan Paperback Edition, New York.

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On Writing and Publishing—Kenneth M. Ames

My 1981 *American Antiquity* article started its life as a seminar paper in the fall of 1969. I was encouraged to revise and publish it, which I did eleven years later. I spent those intervening years massaging, tweaking, completely revising, massaging, and tweaking some more. After all that stressing and work, when it was accepted, the editor of *American Antiquity* called to say I had to cut it by a third. I asked which third. He said, "Any third," which I did in four days over a Thanksgiving vacation. So much for eleven years of obsessing: "When in doubt, cut it out." The published version bares only a passing resemblance to the original and I am not sure it is better, and, had I gotten it out in a timely manner (say after five years of tweaking), I would have been well ahead of the "complex hunter-gatherer" curve. Of course, I did other things during that period: got my first two jobs, moved twice, started teaching and developing classes, finished my dissertation, got married, got divorced, ran survey and data recovery projects, wrote reports, wrote two papers that got rejected (one was eventually published) and published a couple of chapters. But still I tinkered.

The charge for these essays is to write about writing and issues such as why people are writing/publishing less, if indeed they are. I am not sure what "writing/publishing less" means; does it mean that people are writing less, or that the pace of journal submissions is declining? The proliferation of journals, growing stacks of CRM reports, and bulging files of archaeobureaucratic paperwork points to lots of writing. More journals may mean submissions are spread more thinly among them. And then there's writing, and then there's writing. Take CRM reports; some are excellent, contain significant data and intellectual capital, while others are stultifyingly boring, mechanical, and useless. Of course, the same can be said for journal articles/book chapters. So, by writing, do we mean putting words on paper; or good, productive writing, writing that moves the discipline forward in some fashion?

There are impediments to good writing, perhaps more than to bad writing. Good writing is hard work (for most of us); requiring time, discipline and focus. In academia, where writing is a job requirement, there actually isn't much support for it beyond lip service, at least in my experience. Faculty time is eaten up by a myriad of increasing demands; some essential (teaching, advising, research), some a waste of time (assessment). The structure of the faculty has changed. Gone are the days when most were tenured or tenure track, with the security to pursue long-term research and writing projects. Now, in many schools, a significant portion of the faculty are fixed-term or sessional faculty, contractors who teach one, two or more courses here, there, and elsewhere. Hard to write that journal article while commuting between campuses or teaching four courses per term. In CRM, there is less incentive to publish, especially after a day of cranking out reports or surveying wind turbine pads. That CRMers and agency archaeologists do publish is a testament to their professionalism and dedication to the discipline. The impediments also include fear, insecurity (I have nothing to say, I will look stupid, I can't write), a sense that there's no point (no one's ever going to read this), misplaced perfectionism, laziness, lack of professional commitment. And finally, as my personal anecdote shows, there's life.

So, why write? It is of course a professional obligation; publishing our work, making it available to our colleagues and ultimately the public, is one of the crucial things that separates us from antiquarians and worse. That is not to say we do that particularly well, but it is the ideal. Two additional aspects of that professional obligation are the quality of the work and of the writing itself. Neither well written poor work, or badly written good work are useful. What is good work? Rigorous scholarship of some kind, be it library research or a well conducted field survey. Good writing is, to my mind, simple, straightforward, and readily understandable, which is difficult to achieve, but more of that below. Other reasons to write are job requirements, ego, competition, ambition, desiring to contribute to the field, to shape the field, and combinations of these and other motives. Or, just because you want to, or because you have to; it's who you are. At this point in my life, that's why I write. I have other motives as well: ego, wanting to contribute—but writing is now a fundamental part of who I am.

But that doesn't necessarily ensure either getting writing done or producing good writing, as the anecdote that opens this essay shows. It took me 11 years to get that paper out because of insecurity hiding behind perfectionism, with continual revision as a way to avoid submission, avoiding risking rejection or criticism. As long as I held on to it, it was a good paper, maybe a great paper, most of all a safe paper.

How did I get past that? I did publish one paper before the *American Antiquity* paper. I coauthored it with Alan Marshall and we published it in *North American Archaeologist* after it was turned down by *The Journal of Anthropological Research.* At that time, *NAA* was new, and I figured it was a safe place to submit. It was, but the paper was well received. A confidence builder. But the most important step was participating in the 1985 volume *Prehistoric Hunter Gatherers, the Emergence of Cultural Complexity*, edited by T. Douglas Price and James Brown. Price is an excellent, but brutal editor. If I survived that, I could survive anything. My writing and publishing took off after that, partially because of the attention drawn by the volume, but partially by the increased confidence gained from that experience. But even with all that, I still struggle sometimes and get bogged down.

I am pressing my allotted 1,000 words, but I want finish with a few more words about good writing. American academic writing is notoriously bad, turgid, opaque, dense, but there are good archaeological writers. In my generation these include Kent Flannery, David Hurst Thomas, Brian Fagan, and Bob Kelly, among others. Good writing can be done, but I hesitate to give advice, since writing is a very personal activity. I once asked Jim Deetz, another excellent archaeological writer, what his secret was. He said he wrote one draft, and never revised. If it wasn't coming out the way he wanted, he set the piece aside and worked on something else for a while, and then went back to it when things felt right. Amazing, but not very helpful, at least for a mere mortal. My few pointers on writing well by mortals:

- Have your goal in writing the piece clearly in mind.
- Have a clear idea what you want to say and how you want to say. I try to know what I want to say, but often don't until I've written it. Leads me to:
- Pound out the rough draft. Expect it to stink. Don't revise while you're writing.
- Revise the rough draft. It may take three or four revisions, but don't hide behind the revisions, use them to move the paper/report along.
- Keep asking yourself "is this useful, how can I make it more useful."
- Ask yourself "How can I make it shorter, simpler? Can this text be a table?"
- Write in the active voice, avoid the passive voice. Never, ever write "Due to the fact that…." "Because" works well and it's four fewer words.
- Kill your babies. Go through and cut out all the prose and vocabulary you think is especially nifty. It is distracting. The paper is not about you, it's about the subject.
- Shorten it some more.
- Keep in mind, you may never be finished, but at some point you are done.

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Writing and Publishing Research and the Electronic Revolution—Roy Carlson

The system of which anthropological writing and publishing is a part has changed, and is continuing to change, during my sixty-five years as an anthropological archaeologist. Anthropologists should be more aware than most people that culture and society do change in response to technological innovations, of which the most influential today is the Electronic Revolution that has brought rapidity and bargain-basement cost to the preservation and communication of research data and results. As in the earlier Bronze Age and Iron Age revolutions, the effects of the Electronic Revolution on society are far reaching as the socio-cultural system attempts to achieve equilibrium of its component parts, and all kinds of changes take place. Anthropological writing for publication is a small part of this system, but since its purpose is preservation and communication of both data and ideas, it was and is bound to take advantage of new technology that facilitates these goals, and will change accordingly.

My philosophy of professional writing is that the data recovered from survey and excavations are the most important part of archaeological research, even though the interpretations may at the time of publication have been much more interesting. This philosophy probably stems from the Boasian ideal of careful and precise collection of data as a prelude to synthesis. I look back at the first thing I ever published (with Warren Caldwell) on stone piling in the Plateau, published in the *American Anthropologist* in 1954, and realize it is still being cited today because of its substantive content. The data from another of my early publications on the Archaeology of the San Juan Islands (*American Antiquity* 1960) is still being used today in new and different cultural-historical syntheses (i.e., Terry Clark 2013 *Rewriting Marpole*). My doctoral dissertation, *White Mountain Red Ware*, written in 1961 and published in 1970 (University of Arizona Press), has a heavy substantive content, and is still frequently cited. The nature of archaeology as an inexact science, and the need for re-synthesis by merging the old data with the new, could until recently only be accomplished through a printed written record.

Anthropological writing of the late nineteenth and twentieth centuries arose from different intellectual streams, natural history on the one hand and social philosophy on the other. Societies and cultures past and present were described and compared with goals of determining where they fit in history or how they worked. The historical school, founded by Franz Boas, frowned on premature speculation and emphasized the necessity of carefully collecting data on cultures and societies past and present before attempting synthesis. The goal, however, was synthesis and integration of the data about human beings derived from carefully collected data on human biology, prehistory, linguistics, society, and culture. The method of both preserving these data and communicating them to other researchers was mostly by writing supplemented by photographs and other illustrations, and publishing in journals and monographs. Writing is still required for communication today, but the internet offers a vastly expanded audience at little or no cost, whereas printing and distribution costs continue to accelerate. In 2017 the SFU library agreed to make electronic copies of all previously published *Archaeology Press* research monographs, and to put them online where they could be accessed by anyone for free. Are the types of printed journals and research monographs of the pre-electronic era still necessary, or has this method of data storage and communication become obsolete?

As the managing editor of *Archaeology Press* at Simon Fraser University for forty-five years (1972 to 2017) I oversaw the production, printing, and distribution of all our research monographs. The purpose was to make the information they contained available so it could be used by students and professionals. Many of these monographs were theses or derivatives of theses. The need to print theses disappeared when the university instituted the requirement that all theses be in electronic format so they could be put on line. Many other *Archaeology Press* monographs resulted from conferences or research projects. Today SFU Archaeology hosts few conferences and undertakes more archaeological research elsewhere than in the Pacific Northwest, and this change has also contributed to the demise of *Archaeology Press*.

Another factor in the decline of printed research in archaeology is the rise of the consulting industry in the last thirty years. Academic archaeology and consulting archaeology arise from different motivations. Practitioners of the former are motivated by competition and cooperation with their peers and solving or adding to the puzzles of prehistory through wide distribution of their research. Consultants, on the other hand, need to make a monetary profit by satisfying the demands of a client, and wide distribution of their results is sometimes prohibited, usually unnecessary, and costly if published other than on the net. Academic publications do not usually require a profit, have sometimes been heavy on supply and low on demand, and still been available 50 or more years after they were initially printed. The Smithsonian was giving away free the remainder of its large surplus stock of BAE *Annual Reports* and *Bulletins* in the early 1950s (I ordered one of each), and many of the *Memoirs of the Jesup Expedition* were still available as least as late as 1956 (I ordered Tait's Thompson Indians).

The data of anthropology has become significantly different. When I was a student at the University of Washington (1950–1954) the Department of Anthropology was one of the four top anthropology departments in North America. The ethnography of non-literate native peoples was the primary database and was supplemented by physical anthropology, linguistics, and archaeology of these same peoples. By the time (1970) I instigated the split of archaeology from anthropology at Simon Fraser University, and formed the Department of Archaeology, there were very few non-literate native societies left anywhere in the world, and any salvage ethnography still being done was of questionable validity as an indicator of pre-contact culture. Socio-cultural anthropology had lost its data base, had shifted to the study of acculturation and current social problems, and had become less relevant to archaeology. Archaeology, on the other hand, had expanded during this period, largely triggered by incorporating many new techniques derived mostly from the hard sciences such as ^{14}C dating; there were still millions of archaeological sites throughout the world capable of supplying new data on the past. Cultural-historical and cultural-ecological synthesis has remained the ultimate research goal by anthropological archaeologists.

The primary purpose in writing and publishing research is still to make new data and ideas available to other scholars working on the same or similar problems.

The internet now provides a more rapid and inexpensive technique for accomplishing this goal than does printing, and it is probable that institutions involved with the preservation and dissemination of archaeological and anthropological knowledge, as well as researchers themselves, will adapt even further to this technology. Writing, however, is still essential regardless of whether it is destined for the printed page or the air waves.

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It's Writing, or Vacuuming—Alice B. Kehoe

Learn to Write Badly sums up the problem with academic writing. Students are still being taught to use the third person passive, to choose words barely anglicized from Greek and Latin, and, author Michael Billig's *bête noir*, to nominalize what doesn't in fact exist. Successful, sought-after academics disdain simple thoughts about actual data. For example, from current fads, "relational ontologies," "meshworks," "agency of substances and things," and "poiesis" describing "the Mississippianization of mid-continental... North Americans [which was] rhizomatic and afforded a more dramatic territorialization of relations once coordinated by people and cosmic forces at a higher scale. Whether some or all people intended at the beginning for this to happen seems both unlikely and beside the point" (Pauketat and Alt 2018:75). Did only "materialities" have agency, people did not?

Nowadays, with permanent academic positions continuing to be cut in favor of universities endlessly selling online courses monitored by TAs, fewer anthropologists need to publish. Fewer still have support from employers for time and research funding for fieldwork, followed by time to work up fieldnotes and data and construct publishable papers and books. Textbooks, more than ever, are products of a few large publishers whose staffs provide Technicolor photos, charts, online links, instructors' outlines and notes, and test questions. Anthropologists forced to teach four courses *a day* at two or three colleges each week, need such packages. They certainly have no time to write. Into the void come blogs, hastily done.

When I had my Ph.D., in 1964, I had published, in *American Antiquity*, a graduate independent research project analyzing sherds of the Northwestern Plains into wares and types. It was the first overview of ceramics in the region, my taxonomic labels have scientific priority, but only once were they cited, by a woman archaeologist; they were supplanted by rival systems created by ambitious Canadian men archaeologists. This has been the story for me, and for many women, the "invisible college" is a boys' school, women are literally not in their class, not heard nor seen (e.g., Bardolph 2014, Bardolph and Vanderwarker 2016). Women are less likely to write for publication when their published work, like themselves in person, are overlooked. This may change now that, since 2017, women are in the majority in Society for American Archaeology, and trending toward increasing numbers.

Why have I continued to write and seek to publish? I enjoy writing in scholarly style. I write easily, and those I consider true colleagues, actively engaged in research and thinking rather than career-building, compliment me on my writing.

The bottom line, for me, is that when I am writing professionally, I feel I am taking a break from housework. Growing up, the expectation was clear that I would marry, have children, spend my life as a homemaker, little time for reading. "Nevertheless she persisted" in going to college and through graduate school. I obtained also the degree that seriously was openly advised for women wanting to do archaeology: the Mrs. That title offered opportunity to accompany the husband into the field, do the fieldwork chores, help with analyses, help with preparing reports, type them. Of course my husband did no household or childcare tasks. Nor could we afford hired help. Working intellectually instead of manually, hearing the typewriter ping instead of the vacuum's roar, was fulfilling, like eating chocolate.

It does help to feel impelled by wanting to say something. When I've worked from observational data through inference to what comes out as best explanation ("IBE"), I want to tell it to people. I'm skilled at that craft (I read history/philosophy/sociology of science, read history of our discipline, understand the principles of historical science). So I publish, though often not cited as I should be. A backward acknowledgment tends to happen when I comment at a lecture or presentation. The lords of the paradigm in vogue glare at me, make it clear that what I said was not welcome—once I was even physically pushed away (misdemeanor assault, it was). So I know that my assessment of the lords' work as fundamentally unscientific in terms of historical science, was correct: pushing dogma, they can't discuss what I offer.

This so far is my confession. It doesn't go into the difference between edited books of papers, and journal publication. For about twenty years now, most of my publications are essays or chapters in edited books. Colleagues invite me to contribute to volumes they're developing, on themes or topics or out of conference presentations; in a couple of cases, it's been an invitation to co-edit. An important distinction here is that submitting to journals is submitting to "peer" review by persons I might not consider my peers, whereas invitations to edited books are from like-minded colleagues. Established journals have high rates of rejection, because they get many more submissions than pages allotted to each volume of the journal. Journal editors tend to select papers that stay within the box, don't rock the boat. Edited book invitations spare me rejections because my paper doesn't fit mainstream expectations.

To sum up, I write easily and enjoy thinking as an anthropological archaeologist. Everything human is within our purview, there's no way to exhaust research possibilities nor interpretations. That's so much more rewarding than vacuuming the same damn rooms—now I can leave it to a student roomer, vacuuming in lieu of rent. For other anthropologists/archaeologists, the world is changing with online access. Anyone who enjoys writing can post. Ivory-tower denizens will go on with the latest obscurations (my online dictionary: "make unclear and difficult to understand"). It does bother me when the Theorists' juggernauts roll over people I know from the field, labeling them "animists" or "living always spiritually" or "foragers." With my books, I've tried to counter the imperialist impositions of such characterizations. I do wish more people read my efforts. When the end comes, I shall say that scoreboards on readers and citations didn't matter so much, writing was my pleasure stolen from my ordained life of housekeeping.

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On Writing Paleozoology, Zooarchaeology, Archaeology, and in General—R. Lee Lyman

I published my first professional archaeology journal article in 1977. I was excited to be entering the ranks of professional archaeology, and publishing in one of the profession's peer-reviewed journals was, in my mind, symbolic of my credentials. I had completed my Master's thesis the year before and my first technical CRM report the year before that. I like to think I have become a much better writer since then, given the help of numerous astute reviewers of things I have submitted, including numerous technical reports, book chapters, book reviews, journal articles, and books. Another thing that helped along the way was the diversity of topics about which I have written. Different subjects and venues (and editors) require different structures to arguments and reasoning. I still get excited when my peers and journal editors think what I have written is worthy of publication, and I still get perturbed when they think otherwise (just ask my wife!), though not as much as I used to. There are things about writing and publishing that colleagues and students have prompted me to consider over the years that someone just entering the writing and publishing game should at least be aware of (forewarned about?) and, perhaps, think about. I outline a few of the more important of these things in the following.

First, readers of these comments are likely anthropologists, and as such, they are fully aware of the reality of individual differences. In the present context, my point is that some people write well from day one and others, such as myself, must work at it to become competent. Practice, and lots of it (along with feedback from others), is the only way to become adept at it. Academicians at many universities must write and publish because that is ~40% of their job; if they want to be promoted, win a raise in salary, or even hold their job into the future, they must publish in top-tier journals (these days measured with citation indices, impact factors, and rejection rates). That is a rule of the game made and enforced by university administrators. It is those administrators who seek prestige for their university because prestige allegedly encourages state legislators, donors, and funding agencies to pour more money into university coffers. But writing can also be gratifying to the author (see below). Of course I wrote because I wanted to be promoted and to earn raises. And I wrote for other reasons as well (see below).

Second, writing of any kind takes time, energy, thought (mental gymnastics), reading the pertinent literature, research, analysis, stringing words together, editing, and rewriting. It also requires development of a thick skin, to withstand criticisms of reviewers, many of whom force you to rethink and rewrite in a clear and efficient manner, others of whom you wonder if they actually read what you wrote. The most important point here is that reviewers will expose your weak thinking, incorrect analyses, mathematical mistakes, grammatical errors, every place you made a mistake from typographical errors through omission of a key reference to logical fallacies. This helps you become a better researcher as well as a better writer. Reviewers can be hurtful, whether or not that is their intention. This is so because writing for the public means you are exposing the intellectual and professional part of yourself, the part you hope your peers think is superb. As my doctoral advisor Donald Grayson said to me many years ago, "It is ok if your friends know you are an idiot, so ask them to unofficially review your manuscripts before submitting them to unknown members of your profession. You do not want the rest of the profession to know you are not very bright." Peer review is a good way to have your ego deflated, but it is also an exceptionally good way to learn both that you are not omnipotent and how to do research and write better.

Third, notice I said "learn you are not omnipotent." If you want to truly learn a subject, write something about it and submit it for peer review. If you are lucky, the peer reviewers will in no uncertain terms identify gaps in your knowledge, and the really good reviewers will also tell you how to fill the gaps they identify. Hence, you learn by writing. And that is another reason I write: I learn what I write about by reading as much as I can about the topic first. Then I have to write concisely and clearly about the topic; writing forces me to think clearly. Finally, reviewers tell me where I failed to learn sufficiently and think clearly. I learn some more by reading the literature I should have in the first place, doing the analyses I should have done in the first place, and writing about those things. Good writers are also good learners, and in any research field, it behooves us to never stop learning. Another benefit, one for my students rather than me, is that writing has helped me become a better teacher precisely because writing (and the research it requires) has forced me to learn topics inside-out, upside down, and frontwards and backwards. When a student asks a question, I either have a good answer at hand or I know how to find one *if* I have written a paper on that topic that withstood peer review. And this is yet another reason I write; it helped me be a better teacher.

Fourth, something you learn when trying to publish is persistence. I had a manuscript I thought was pretty good in the 1980s. I submitted it to an individual with knowledge of the topic and asked if he thought it was worthy of publication. He had a few comments and indicated that the manuscript was indeed worthy of publication. I submitted three versions of it to one journal; it was rejected every time, not always for good reason (in my view). I then sent the fourth edition to another journal that rejected it as inappropriate for that journal. So I then sent three other versions of it to a third journal that finally accepted for publication the seventh edition. It took nearly four years from the time of the first submission to acceptance. Not every experience I have had has been that lengthy; sometimes the second edition of a manuscript has been accepted. You will nevertheless learn persistence, just as I did. This does not mean, as Robson Bonnichsen once commented to me, that if a bit of research is worth doing and writing up, it is worth publishing. I do have several unpublished manuscripts that after a couple of unsuccessful submissions, have, since the last rejection, been sitting in my files for decades. Perhaps I could today write them in such a way as to make them acceptable to a journal editor somewhere, but my interests have shifted a bit from the topics of those papers and my current thinking about those topics is a bit murky and certainly out-of-date.

Finally, a couple students have over the past thirty-five years asked "How do you write (given it is time consuming and may require innovative thinking)?" A former graduate student by the name of Matthew Boulanger, whom I was working with at the time, laughed and gave an answer that I here share with you (with his permission). Boulanger provided the following series of steps (my elaborations are parenthetical) that describe the writing and publishing process:

How to Write and Publish:

1. Identify a research topic, phrase it as a question or testable hypothesis (this comes from discussions with your advisor or colleagues, or reading the pertinent literature).
2. Devise a way to evaluate or answer the question or test the hypothesis (this is the stage known as putting together a research design).
3. Do the research and analysis, and answer the question or test the hypothesis.
4. Write a paper discussing the question or hypothesis, describing your research design, and your analytical results. (It sometimes helps to know to which journal you are going to submit the manuscript for consideration as this may influence how you structure the paper.)
5. Format your paper for the journal you have chosen. (Each journal has a more or less unique format [e.g., the form of section headings, how references are cited, how reference lists are constructed].)
6. Submit to the chosen journal. (Virtually all journals now have online submission systems. You will need as well to have decided on 3–5 potential reviewers to recommend to the journal editor.)
7. Receive reviews of the manuscript (and either celebrate its acceptance and proceed to step 8, or have a stiff drink and begin figuring out how to appease the reviewers and make the editor happy). Revise and resubmit (hopefully only a time or two before celebrating).
8. Return to step 1.

Before I retired from academia, I taught a 1-credit course to first-year graduate students. One of the things we covered in that class was how to write research papers. There are many articles and books available that tell you how to do this. I examined a lot of these and chose several articles to have the students in my class read. These references are listed below. Take a look at them, then go to step 1 above and begin.

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Writing to a More Inclusive Readership—Robert R. Mierendorf

As an anthropologist who uses mostly archaeological data to make inferences about past behavior, I've considered the questions about publishing anthropology from a perspective developed through career experiences, first as university-based consulting archaeologist, later as federal archaeologist and cultural resources manager, as teacher of field-based adult seminars on cultural history, as board member of an environmental education non-profit, and as private archaeological and cultural resource consultant, all in the Pacific Northwest. Many of these career activities overlap considerably with those of my professional colleagues. Practicing a form of public anthropology, I interacted closely with non-anthropologists and professionals in different disciplines, with tribal and first-nations representatives, with administrators and managers, and with the general public.

What Has Been Your Writing Philosophy?

Write with clarity and precision, and write anthropology not only to other professionals, but also for other public, nontechnical, educational, nonprofit, and governmental entities and audiences. Many such entities seek to learn from anthropological insights and how they might change perspectives, teaching curricula, and agency policies and planning. For writing to a more inclusive audience, I found mentors in disciplines outside of the one that molded my professional writing style. Individual writing styles usually adhere to professional standards and terminology, often defined more by particular disciplinary paradigms and less by any need to communicate to a larger, nonprofessional audience. Making the results of our efforts more comprehensible to other audiences fosters engagement of diverse communities in the work we do. The Society for American Archaeology and state offices promote professional outreach efforts, exemplified annually in the widespread practice of state-sponsored archaeology events geared to reach public audiences.

Archaeologists collect information and data to make inferences about places and natural spaces in a way few other disciplines do, but the challenge is to make insights available and understandable to audiences with no technical background. Many readers are discouraged by heavy data presentations, technical jargon, and abstractions that obscure the linkages between people in the past from those in the present, which in effect suppresses the local traditions and lived experiences of Indigenous communities.

What Has Motivated You to Write Professionally?

"So why would they do that?" The question was asked of me during one of many chance encounters with another backpacker, alone in an alpine meadow in the wilds of North Cascades National Park. The question came after I replied "archaeologist" in answer to his first inquiry. Squinting, he then asked, "Well what would you do?" These questions rank among the most frequent from park visitors over years of fieldwork in the park's backcountry. I learned that such inquiries reflected sincere attempts to understand why "they"—the National Park Service—would need anthropological or archaeological services in the middle of Wilderness.

Good questions from curious people made for stimulating impromptu discussion, but also made apparent to me the disconnect between popular perceptions of Wilderness and who might have been to such remote places before them, or not. The common appellation spawned in "sublime" alpine scenery that "I was surely the first to cast eyes on…" represents another common denial of the landscape's prior human history. Hence the notion of an Indigenous history would often arouse expressions of surprise or sometimes momentary meditations on what such a history might mean. It made no sense to me that this notion should surprise, yet it highlighted again the need of interpreting the cultural past for general audiences.

I also gained from other more enduring discussions, many of them lighted by camp fires in the Upper Skagit or Stehekin River valleys, in field seminars attended by an array of participants seeking knowledge about mountain history. These campfire and classroom exchanges and knowledge sharing moments revealed to me high levels of interest and appreciation, conveyed in particular by elementary and high school teachers, who found it difficult to first access, and then make classroom use of current knowledge about pre-contact histories and Indigenous ethnographies in their own teaching districts, localities, valleys, and watersheds.

These encounters motivated me to reach audiences, from outside of the profession, who sought a deeper understanding of the archaeology and ethnography of Pacific Northwest places. My employer at the time, the National Park Service, further encouraged outreach activities aligned with its mission of public "interpretation" (education) of national parks. My interactions frequently entailed the simple breaking down of stereotypes of archaeologists (mostly perpetuated by media, some by archaeologists), of what they do, and of what they look for—conservation archaeology, site protection and stabilization, and traditional cultural values and landscapes are foreign concepts at first-hearing. Sometimes the questions came from my government co-workers with expertise in other fields and sometimes from members and affiliates of Tribes and First Nations. Others included high school teachers, college faculty, and book writers. The need for answers sought by these audiences seemed to match closely the educational and "outreach" imperatives of several disciplines, including archaeology: to encapsulate the results of research in a form comprehensible to a general audience. Increasingly specialized studies producing new data coupled with its rapid dissemination only deepens the need for synthesis and for the technical to be made less so, if goals for expanded audience insight, appreciation, participation, and support are to be attained.

What Challenges Have You Had to Overcome to Get Published?

Challenges include finding the "right" publisher, i.e., one that meets author needs for audience, data presentation format, open access versus other options, color printing, and cost. Too often, publisher style guidelines and international journal formats dictate how data are displayed in ways incompatible with the scope of a study. Other options, such as a monograph (e.g., *Journal of Northwest Anthropology's Memoir Series*) or occasional series, may offer lower cost and higher readership access compared to international journals. More than ever, electronic access factors into selection of a publisher.

What Suggestions Do You Have for Would-be Writers?

As in any writing, be certain of your target audience, of what to convey to them, and of the means for doing so. Beyond proficiency in publishing technical reports and peer-reviewed articles, there are opportunities to write to a less technical and broader readership. If that is an interest, be open to other publishing formats, or to those with a general audience or theme, such as John Miles' edited book of "Naturalist" essays of reflections on varied environmental and cultural perspectives of North Cascades landscapes (Mierendorf 1996; Miles 1996). Some publishers seek writers who make comprehensible and give contextual meaning to current issues that anthropologists and archaeologists are equipped to address. In one example, editors of an archaeology encyclopedia published concise essays about archaeological places in America, including in Washington State (McManamon 2009: V. 3 and 4), in order to fill a gap in student and public education about archaeology. In another, the Gilder Lehrman Institute of American History's online journal for teachers published a theme issue on archaeology's role in reconstructing the American past (History Now 2017). Sometimes agencies commemorate events or anniversaries of special places, and in the case of Manning Provincial Park's 50th anniversary, seek to acknowledge the long Indigenous history and ties to the lands being commemorated (Mierendorf 1991). Though of small format, such low circulation special publications reach a select audience and reassert Tribal and First Nation's narratives of traditional places in today's mountain preserves.

Experience suggests that a career in archaeology and anthropology means it's likely you will be asked to write nontechnical narratives of several sorts or genres—such requests can present opportunities. Like a second language, a writing proficiency exercised in a less technical genre than that of one's profession enlarges the circle of sharing and compels increased appreciation for anthropology's contributions. To start, it's easy to enroll in writing workshops and to talk with writers about writing, including creative and informed nonfiction, "interpretive" materials, "popular" brochures, commemorative publications, and assorted journalistic genres. In addition to colleagues in archaeology and anthropology, I've consulted cultural and natural history writers including Ruth Kirk, Robert Michael Pyle, and William Dietrich. Gary Snyder, environmentalist, poet, and anthropologist, encouraged and advised to "write much" during our tour of the Upper Skagit River valley in 2002.

How Can We as a Profession Ensure That Anthropologists in the Pacific Northwest of All Types and Backgrounds Continue to Publish?

Lend support to publishers of journals, monograph and memoir series, books, and occasional publications that anthropologists and others in related fields use to disseminate results of investigations. A stable membership base that readily submits articles for publication is necessary to ensure viable publication outlets. Such support may be even more critical for publishers of smaller regional journals compared with those supported by large national and international memberships.

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A Writing Philosophy—Mark G. Plew

I think my writing philosophy has much to do with the importance that people around me placed upon it. In my case, I had the good fortune to study with and be around people for whom reporting and publishing were paramount—even more importantly believed as my mentor did that publishing on one's work should be undertaken promptly. I was, I believe, fortunate to have been encouraged even as an undergraduate to write and to think about publishing. Having been encouraged to read exhaustively has helped improve my writing many fold. Though we are professionally motivated to publish for a number of reasons, not the least of which is promotion and tenure, I have found that consistent writing has made me a better thinker. I have seen writing as an important part of my professional and personal development and something that I have always thought should always be contributing. For that reason, I have very rarely undertaken projects that I didn't think would lead to better or more complete understandings of a problem. For that reason, I have rarely presented papers that I did not intend to see through to publication.

There are today a number of issues relating to anthropological publishing that pertain to individuals but also journals. As a state journal editor, I have seen a marked reduction in the number of submissions over the past decade. Although we might attribute this in part to more grey literature reporting or an increasing number of publishing outlets, there are notably fewer contributions from graduate students and academic faculty, and even fewer from those working in cultural resource management. Although there are notable exceptions, there are too few within the community who publish—something problematic given the quantities of data generated by their efforts. I find that there is and has been a growing sense of this not being a requirement of their professional lives; academics probably deserve some responsibility for this. To ensure that regional anthropologists continue to publish requires those of us in academic positions to encourage our students to begin thinking about publishing early in their careers. This may require us to invest more time in helping students conduct research projects that lead not to posters and conference presentations alone, but to publications. My personal experience is that those who get an early start don't fear the review process that drives many from submitting their work. I also think that editors need to be more proactive in encouraging submissions—especially from cultural resource managers. Finally, I wonder if the trend toward multiple authored papers is not a factor in the reduction of submissions.

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Part 2

Part 2 contains the following essays:

- "Eschew BS and Insist on Disclosure" by Thomas F. King, Ph.D. in anthropology from the University of California, who has worked for the last 50+ years in archaeology and historic preservation, in government and in the private sector, in the United States and the Pacific Islands; a reformed former U.S. government employee, now self-employed as a cultural heritage and environmental impact assessment consultant based in Silver Spring, Maryland. King has written twelve textbooks and tradebooks, nine monographs, two novels, and sixty journal articles.
- "To Publish or Not to Publish—The Changing Nature of Archaeology" by Dennis Griffin, the State Archaeologist with the Oregon State Historic Preservation Office where he has worked since 2002. He received his Ph.D. in Anthropology from the University of Oregon in 1999 and has spent over 40 years working throughout the Pacific Northwest. His areas of interest focus in the Pacific Northwest and Alaska with a specialization in oral history, tribal collaborative research, and more recently, Oregon's early military history. Griffin has published two books, twenty articles in refereed journals, one book review, thirteen articles in non-refereed publications, two book chapters, and well over one hundred-forty technical reports.
- "If You Dig a Site, You Must Record in Detail *and* Write Up Results, Since Your Site Area is Now Gone Forever…." by Dale R. Croes, B.A. in anthropology from the University of Washington (UW), M.A. and Ph.D. in anthropology at Washington State University. He did his Ph.D. dissertation research on basketry and cordage artifacts from the Ozette Village wet site. Adjunct Professor, Anthropology, Washington State University, Director, Pacific Northwest Archaeological Society and Services . Croes has authored eight books and fifty-eight articles.
- "*Caveat Emptor*, Anthropology is a Lifetime of Writing" by Kevin J. Lyons, thirty-year practitioner of ethno-archaeology in Interior Pacific Northwest anthropology. Serving as the Kalispel Tribe of Indians' Cultural Resources Program Manager for the past twenty years; good days are filled with primary ethnographic research, medium days are filled with archaeological analysis, and tedious/necessary days are filled with administrative and/or policy work. He has contributed to Pei-Lin Yu's 2015 anthology "Rivers, Fish, and the People" provided editing assistance to John Ross' 2011 ethnography "The Spokan Indians," and has penned the usual administrative detritus that is cultural resources management copy.
- "Some Hidden Facets of Writing Archaeology" by Madonna L. Moss, Professor of Anthropology at the University of Oregon and Curator of Zooarchaeology at the UO Museum of Natural and Cultural History. She received her Ph.D. in 1989 from University of California, Santa Barbara. Moss has authored or co-authored over 80 peer-reviewed articles, written two books and two monographs, and has published dozens of non-peer-reviewed articles.
- "Writing Tensions: Voices That Help—and Those That Don't" by Mark S. Warner, Professor of anthropology at the University of Idaho and the president of the Society for Historical Archaeology (2018–2019). He is a historical archaeologist and his research interests include issues of inequality, the American West, zooarchaeology and foodways and collections management. He is the author

of *Eating in the Side Room: Food, Archaeology and African American Identity* (2015), and co-editor (with Margaret Purser) of *Historical Archaeology through a Western Lens* (2017). Warner has authored or coedited four books and two thematic issues of journals, as well as authored or coauthored nineteen articles and book chapters.
- "From Writing Science to Writing for the General Public" by Dennis Dauble, retired fisheries scientist and adjunct professor at Washington State University Tri-Cities. He earned a B.S. and doctorate in fisheries from Oregon State University and a M.S. in biology from Washington State University. Dauble has authored sixty journal articles, forty-eight technical reports, the natural history guidebook *Fishes of the Columbia River Basin*, and three short-story collections.

Eschew BS and Insist on Disclosure—Thomas F. King

Writing—particularly writing fiction—vies with field archaeology as the most fun I've had with my pants on. I write because I like to, and I've been blessed to be able to make a living doing it.

I don't know that my writing's guided by any explicit philosophy, but I'm happiest with work that I think is elegant, that's clever, that communicates with people in plain language about something that's important at some level. If I have a writing philosophy, I guess it would be to eschew BS.

Getting published has often been a challenge for me—usually for good reason. Now that I'm no longer trying to build a resumé, I pretty much write for publication only if asked, and otherwise satisfy myself with my weblogs, occasional Huffington Post pieces, and postings to Academia.edu.

My impression is that in today's world of anthropological publishing, there are lots of places to publish but a good many discouragements to doing so. One of these is what seems to me to be the widespread demand for adherence to postmodern styles, which combine a snooty insistence on inclusive discourse with terminology that enforces exclusivity. Another is the discomfort that some communities feel for being written about, and for having their ancestors' lifeways plumbed. Another is that much anthropological—especially archaeological—writing is done under contract for commercial entities and others who reflexively seek to impose non-disclosure requirements, to which government and the professional community have offered only the most flaccid pushback.

My advice to would-be anthropological writers, I suppose, would be "just do it." Write about what interests you. Try to have fun with your writing, whether it's a journal, field notes, an email to a friend, a social media posting, a contract report, an essay, or a novel. Keep a journal, and in it try to practice "thick description" (c.f. https://en.wikipedia.org/wiki/Thick_description). And read some good anthropological writing; off the top of my head I'd suggest Keith Basso's "Wisdom Sits in Places" (1996) and Margery Wolf's "Coyote's Country" (2018), but there are plenty of other examples. Keep throwing your writing at others, for publication or just to share. Throw nothing away that you've written; you can digitize and discard hard copies, but don't let your words be destroyed. They'll be useful to someone, someday, as a record of the times if nothing else.

As to what the Pacific Northwest anthropological profession can do to encourage publication, I'm of at least a couple of minds. I think it's worth discussing what does and does not NEED to be published in a traditional sense. How many dead-tree journals do we need any more? What alternatives to publication should the academic and related communities accept to forestall perishment?

As I've watched my own dead-tree publications fall into disuse and obscurity, and seen the price of acquiring or accessing them ascend into the ionosphere, I've become more and more convinced that academia's fixation with traditional publication is misguided and counterproductive. Publication has traditionally served two purposes in and around academia. It has preserved and communicated data and ideas, and it has been a context for discussion and debate. There are now many alternatives to the dead-tree book or journal as means of serving both functions. Some are more cost-effective than others, but I'm guessing that dead-tree publication is about as cost-ineffective as options come. *JONA* has not only a distinguished journal but a fine worldwide website with lots of links; maybe there are ways to make that site more accessible, more widely used by the community, and use it to encourage writing of all types.

Then there's the matter of those non-disclosure provisions in CRM contracts. There's seldom much reason for their inclusion; it's just something that private firms tend to do. The anthropological community, I think, should take a firm stand against the use of such instruments except where their use serves a pretty clear public interest. Sometimes they do serve such interests, but often they do not. It's obviously a discouragement to publication when one writes something that one thinks is useful and worthy, and then cannot share it with others who might find it so. I imagine that CRM practitioners in the Northwest would benefit from *JONA*'s taking a seriously skeptical attitude toward prohibitions on public disclosure.

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To Publish or Not to Publish—The Changing Nature of Archaeology—Dennis Griffin

There are many reasons one can hypothesize why writing has fallen off among archaeologists, young and old. Such a change could be due to changes that have occurred in academia and the mentoring relationship that historically developed between students and teachers, or the evolving world of cultural resource management (CRM) where most students find themselves working after graduation. Since the time that I was a young college student, universities have changed their expectations of teachers requiring them to place an emphasis on publishing over teaching in order to earn tenure, with such publications often being solo authored and directed toward national over regional publications to attract a wider audience. This change occurred at the same time as an increase in general class size, a switch to computer graded exams over the earlier required essays and class papers, a rise in on-line degrees, a reduction in research opportunities for students to work with faculty that equates to reduced opportunities for students to develop close mentoring relationships with teachers, and less of a focus on writing within the classroom environment. All of these changes, in addition to the effects of less focus on grammar and writing skills during primary and secondary education add up to students who often do not see publishing as an integral part of their career unless they choose to be a teacher in a university setting. Fewer graduate students appear to recognize the importance of publishing an article on their thesis research; something they have spent two to three or more years of their lives slaving over and only want to put it behind them and get on to gainful employment.

In the world of CRM since the mid-1980s, project related archaeological contracts have largely passed from the universities to private CRM firms with work becoming highly competitive and now tied to stricter budgets and tighter time frames for completion. Report writing has also changed where it has now become rare for project reports to be written by a single author with writing being divvied up among multiple writers, not in a collaborative way that had been the norm within a university setting, but where different authors are assigned their own chapter of a report with little opportunity to deal with a project in its entirety. Analyzes have become more compartmentalized with no single person being able to summarize the totality of research. Due to the competitive cycle of CRM projects, staff are forced to move directly on to other pressing projects with little time to write articles for journals that aren't required by a contract. As a result, fewer authors choose to publish the results of their research.

I was fortunate to have been taught early in my professional archaeological career that if a job is worth doing it is worth telling others about. This sentiment often came from professors that did not themselves publish much aside from grey-literature reports but they chose to use their classroom as a forum to disseminate the results of their own research as well as that of others that impressed them about the topic of the daily classroom discussions. These teachers strongly encouraged students to present at local regional conferences with their university providing free transportation to such conferences, cheap shared lodging (often filling the floor with students in sleeping bags), employed students as research assistants, and encouraged them to author or coauthor papers on the projects they assisted with. University anthropology clubs often had forums available where we were able to share our research with fellow students, while working to perfect our analyses and results and sharpen our delivery. Having a general fear of speaking in front of large audiences, I forced myself to try and present at least one paper at a conference each year in order to combat such fears, and if the responses were good to try and later publish it. I find that there is no better way to force oneself to pool one's thoughts on a topic into a cohesive document then facing a conference schedule or classroom assignment. We were given many opportunities to write in graduate school, as it was not unusual to have three 20-page papers due each term. My writing may not have been very good but I certainly learned to write *a lot*.

After working at a State Historic Preservation Office (SHPO) for over sixteen years, I have had the rare opportunity to review and read through hundreds of reports each year summarizing archaeological projects within Oregon. Many of these are associated with small CRM projects that lack a larger regional perspective and would not normally be suitable for publication in journals or monographs.

However, each year a number of larger projects produce multi-volume reports with many different research variants that would be excellent for publication. Unfortunately, few ever see the light of day aside from grey literature reports that are shared with the company that hired the authors, and if shared with SHPO, are scanned and available only on the state's on-line archaeological inventory to qualified archaeologists. I feel this is largely due to the pressure authors have to be able to move quickly on to new projects and a general lack of desire in scholarship where funding is not available to pay for the time it would take to write up and share the results of projects.

What suggestions would I have for would-be writers? First, I would remind students that if they thought their master's thesis research was important enough to spend several years completing, that they aren't the only one that would think so. Each year I hear many excellent thesis or project-related presentations at NWAC and other regional conferences and I strongly encourage authors to attempt to share their findings. Many of us would love to learn more about their research and if we missed their conference presentation or were unable to decipher the richness of their research from their conference title or abstract we may never hear of it. Finding a journal to publish your writing is not the problem; it is getting your thoughts down on paper and seeking to share your research in the first place. Many state archaeology associations have journals that are begging for articles.

Second, remember why it is that you chose archaeology as your profession. It certainly wasn't for the money or the ability to spend your days working in an 8am to 5pm office environment. You probably read something that caught your interest and got you thinking this is what I want to do! For me, archaeology provided me with a means to travel the world working with and studying people of different cultures. Seeing the changes that occurred to cultures over time, whether they be large-scale changes resulting from outside culture contact and disease, changes in resource availability or the effects of climate change, or the simple evolution of a particular style of can or bottle label was both exciting and stimulating. Having an insatiable curiosity, I find topics that draw my interest almost on a daily basis and the problem is not what topic to research and write about but which other topics do I have to put aside in order to focus on getting the first written.

Third, consider working with a tribe or different cultural group in order to gain a different perspective from your own. I think writing stems from the realization that your perceptions, ideas and insights are different than what you read every day.

What can we as a community do to encourage publishing? One thing might be to offer to publish a proceeding from each year's Northwest Anthropology Conference, much like is done at the Chacmool Archaeological Conference in Calgary, Alberta. I don't think the publication of such a proceeding would seriously affect submissions to existing regional journals such as *JONA*, while it would provide an opportunity for students and non-student archaeologists to see their research in print. The proceeding would not need to be peer-reviewed, thus reducing an author's fear of their research being seriously criticized. After seeing their work in print, authors would be encouraged to submit their work to peer-reviewed journals as the next logical step. I know that NWAC moves around each year and places many demands on the school or agency that sponsors each year's conference, but the publication of a conference proceeding could be offered to the local Anthropology Club as an opportunity to gain hands-on experience in editing and publishing while gaining them recognition for their editing efforts.

A second idea would be to require the publication of the results of all archaeology mitigation projects. If a significant site is being adversely affected by a project and mitigation is required, aside from data recovery or other mitigation measures, make the publication of an article in a regional or national journal or local newspaper a stipulation of the MOA.

If the public does not understand and support cultural resource protection, future administrations may make legislative changes that limit its continued protection. Sharing the results of mitigation projects will serve to educate the public about local history, increase support for archaeology, and encourage authors to publish future articles about their research even when not required.

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If You Dig a Site, You Must Record in Detail *and* Write Up Results, Since Your Site Area is Now Gone Forever—Dale R. Croes

Trained as an archaeological scientist, it was instilled early (I think by Dr. Robert Ackerman, Washington State University (WSU) who has an exemplary record of reporting his work) that one "should not dig, unless you write-up your findings." The excavation part, and for me hydraulic wet site excavations, were always an adventure—you never knew what wood and fiber artifacts you might find—though painstaking in terms of the scientific recording. The meticulous recording was also primary, since we certainly destroy the part of the site we excavate as a "non-renewable resource." Therefore, writing started with the documentation so one could synthesize all the notes into a summary report. I guess if you follow all the rules of the scientific approach, especially recording one's observations and detailing how one develops and classifies systematically what you find, writing starts there. Then you try to present the hard part, explaining what you found, then you need to try to outline eloquently your hypotheses.

I don't consider myself a natural writer, put do enjoy the process. I had some good English teachers as a University of Washington undergraduate and was introduced to the "little" book by William Strunk, Jr., and E. B. White, *The Elements of Style* (1959). Fortunately it was "little" and I still have that tattered copy I used to carry around in my back pocket while in Basic and Advanced Infantry training in the Army. When we had breaks, I often pulled it out and learned a new lesson. I had joined the reserves and so I returned to graduate school to work on writing my WSU M.A. thesis and Ph.D. dissertation after the 4.5 months of training. So, I had to work at becoming a writer and still must go through several drafts when writing, though I enjoy editing and back-and-forth with reviewers/editors too.

What Has Motivated You to Write Professionally?

Again, my professional motivation has been to be sure and write-up the archaeological sites I have been responsible for directing and all the endless pieces of data recovered from the site, therefore fulfilling my archaeological and scientific professional responsibility. I proudly say that I have finished, with lots of help from teams of researchers, all four of the sites I have directed in my career, so can retire without guilt or the ongoing responsibility of getting it written up. Now it is writing what I want to compile, which has been a synthesis of my and Ed Carriere's, a Suquamish Elder and Master Basketmaker, life histories, testing my career hypotheses, and re-assessing old basketry collections like Biderbost (see next part).

Benefits of Writing Memoir 15—Re-Awakening Ancient Salish Sea Basketry, Fifty Years of Basketry Studies in Culture and Science (2018) with Ed Carriere, Suquamish Elder and Master Basketmaker.

[I ended the last submission saying that if we don't publish our archaeological field work, we should not have even excavated, since now it is gone forever; I kept this to the allotted 1,000 words. Then I was asked to write about the trials, tribulations, and hopefully benefits of writing a *JONA* memoir (*Memoir* 15) in partnership (50/50) with Ed Carriere, where we described our lifetime work on basketry in culture and science:]

What started out to be a post-retirement scientific re-assessment of 2,000-year-old Biderbost wet site (45SN100) basketry at the UW Burke Museum, (a collection I recorded in a rush in a Washington Archaeological Society (WAS) garage "lab" in 1973 for my Ph.D. dissertation), took a totally unimagined turn. Normally I would re-examine each ancient basketry piece (after 45 years of growing experience and new assistance of Kathleen Hawes conducting cellular ID on each piece (not possibly before)) and write it up as a technical report. Then a flash of enlightenment crossed my mind; I needed to call my friend Ed Carriere, Suquamish Elder and Master Basketmaker, and suggest he join us and try to replicate what represented his 100th grandparent's baskets from Biderbost. The thought certainly caught his imagination and triggered the writing of a book (*JONA Memoir* 15) on his and my 50 years of focus on basketry, me as an archaeological scientist and him as a cultural career expert.

After Kathleen, Ed and I met at the Burke, having secured permission from Laura Phillips, the Archaeological Collections Manager, the magic happened; Ed analyzed each piece of basketry from Biderbost, synthesizing in his mind how to replicate the most complex pack basket that Laura challenged him to re-construct, and Kathleen let him know that the basketry material used at the site to make these baskets was almost exclusively split cedar root. That is all he needed to know, and he went to work while on vacation at a time-share he has in Mexico, packing his bag full of split and processed cedar roots.

Upon return we talked by phone and he was excited to show me the large, replicated, fine-gauge, open-twined pack basket he carefully made; I quickly got into my car and drove the hour and a half north to his home studio and was truly impressed and knew we were onto something anthropologically big. He loved learning from his ancestors, which was usually up to five to six generations back, and now he did something he had never dreamed of: work with a master weaver, and no doubt grand relative, one-hundred generations back. My statistical analyses of all ancient Northwest Coast basketry clearly showed the linkage of style in Ed's traditional Salish Sea territory up to one-hundred fifty generations back, verifying scientifically the strong cultural transmission of basketry traditions through Ed's teacher, who raised him from infancy, his Great Grandmother Julia Jacobs, to him. The book was a matter of synergizing this combined culture and science work through our life stories.

Ed and I thought this to be a good idea to compile a book, and he was very patient in our transcribing, in his voice, over half of the manuscript presentation. Since the Ozette project (my M.A. and Ph.D. work), I had learned to work in equal partnership with Tribes in my ongoing professional career, and knew this book with Ed had to be an equal partnership, which it is. This included my replicating the basic, checker-work, ancient pack baskets from Biderbost, with Ed's guidance and from what I had learned in Makah basketry classes as part of my Ozette research.

I also had to "excavate" through vast amounts of Ed's and my images/photos to capture this history, which somehow worked. Fortunately I had visited Ed as often as possible over the past twenty-four years while teaching at South Puget Sound Community College and began recording a lot of his early works in photographic detail. Ed was an amateur photographer through his life, which really helped in compiling his amazing history. Fortunately I was retired in 2013 from the time consuming and hard task of teaching, and have been blessed with the time over the past five years to work essentially full time with Ed on this project.

Writing is one thing, publishing is another. So much of this involves being lucky, and persistent. We compiled a manuscript fully illustrated with photographs and line-drawings and began showing potential publishers. The Northwest Tribes had taken a great interest in this project, especially the Northwest Native American Basketweavers Association (NNABA) and had us present our progress on replicating the ancient Salish Sea baskets at each of their main annual meetings. Their NNABA Board passed a resolution to write a letter supporting the detailed publication of our work. We had meetings with both Western and Eastern Washington publishers associated with Universities and the first said "there is no audience for this book" and the second said that we would have to strip most of the illustrations and take out anything that was not basketry (such as Ed's detailed canoe carving). I pondered the "no audience" statement and told Ed the Tribes wanted this published and could care less about who the audience is, and the greatly reduced (illustration-wise) idea would loss a lot of the detail the Tribes wanted too.

As mentioned, publishing takes a lot of luck and persistence, and I thought of checking with an old friend, Dr. Darby Stapp, owner of Northwest Anthropology LLC, and the publication started to materialize. He embraced the idea and the over 300 color images seemed no problem to him. He took our five-chapter manuscript and worked it over into ten chapters with our time-line of Ed's and my lifetime study of basketry. The book finally came together better than Ed or I had imagined. Darby Stapp and Julie Longenecker have an amazing and talented staff of designers and editors, bringing together a book NNABA and Tribes liked very much.

And we realized we were taking a new approach in Northwest Anthropology/Archaeology that could be applied elsewhere; we demonstrated how this equal partnership, both cultural experts and archaeological sciences, could show how these ancient basketry artifacts create ideational links via shared ideas through hundreds of Salish Sea generations. Truly what archaeology strives to do is show how their database, artifacts, reflect how ideas are shared through long-term cultural transmission; how humans actually operated through the generations as demonstrated by the artifacts we find. This is more than *experimental archaeology* or *ethnoarchaeology* and we decided to call it ***Generationally-Linked Archaeology*** (GLA).

Knowing the Tribal interest was our main book audience, I submitted 1% casino grants to the Squaxin Island Tribe and they provided us funds to send book to all Northwest libraries and colleges. I also submitted to other tribes to subsidize the book so Tribal members could get the book at about half price. The Siletz, Tulalip and Snoqualmie gave Northwest Anthropology LLC, this support.

Following the book release Ed and I have enjoyed sharing the work with both scientific and Indigenous audiences, taking all our replicated baskets and samples with us, finding good reception from both. Our first coming out and book signing was with the Musqueam Band in Vancouver, B.C., Canada followed by the UBC Museum of Anthropology (MOA) opening the next night. Since Musqueam and UBC MOA 3,000–4,500-year-old wet site basketry really crystallized our idea for the GLA approach, and since the UW Burke Museum was busy moving to their new museum, we were glad to open in B.C. Following this opening we went to the Northwest Anthropology Conference (NWAC), The Society for American Archaeology (SAA), and a Wetland Archaeology Conference in central France—all professional groups. With an invitation from the Maori National Weavers Gathering we went to New Zealand with Ed's apprentice Josh Mason, Squaxin Island Tribe, to present our work. One of our favorite presentations is with the Northwest Native American Basketweavers Association, and we consider this a NNABA book because of their ongoing support. We also recognize that the Biderbost site is in Snoqualmie Tribe traditional territory and presented our book and work to their Elder's Honoring, where they purchased 100 of our books and gave them out for signing to their members; they are truly excited to get back their 100^{th} grandparent's and Master Basketmaker's work.

We also recently traveled to Juneau Alaska through Sealaska corporation to present our work and to be in classes with their Master Haida basketmaker, Delores Churchill. The American Museum of Natural History asked us to come to New York and help them with the re-model of their Northwest Coast Hall, originally developed by Dr. Frans Boas (father of American Anthropology), for its 150^{th} Anniversary of this Hall.

Ed's Suquamish Museum is opening a show on our work starting in mid-January 2019 through mid-June 2019. His community is very proud of his accomplishments through the years and excited about the new book documenting his amazing cultural contributions. They got loans of the ancient wet site baskets from UBC MOA, UW Burke, and Squaxin Museums—try to attend!

Upcoming professional programs will be the NWAC, SAA and the 11^{th} Experimental Archaeology Conference (EAC11) in Trento, Italy, where Ed and I have been asked to keynote the event. All this currently upcoming events will feature discussions of our approach, *Generationally-Linked Archaeology*.

Therefore writing this book has provided Ed and I many opportunities to travel and visit with Indigenous artisans and professional archaeologists throughout the world. When we think back in the process to the publisher that said "there is no audience for this book," we are happy to report there has been both Indigenous and professional interest in our writing this *Generationally-Linked Archaeology* book in equal partnership.

A good review of the book is from BC Booklook: https://bcbooklook.com/2019/01/08/462-baskets-across-the-border/ and a Hakai Online Magazine article is available at: https://hakaimagazine.com/features/the-basketmaker/

Some past publications with some links from author's WSU Anthropology web page: *https://anthro.wsu.edu/documents/2017/09/dale-croes-2.pdf*

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Caveat Emptor, Anthropology is a Lifetime of Writing—Kevin J. Lyons

Anthropology is not an essential. Bet no one in graduate school mentioned that to you. It, and the many social activities that we lavish our time with, does not defend the frontier, protect the innocent from injustice, feed the hungry, or mend the ill. Nonetheless, there is a social good that is provided by its various practitioners through the exposition of how humanity is a unifiable whole with both a common ancestry and destiny. That social good can only be delivered when the time spent results in a publicly accessible product that makes the efforts of our peers more efficient and demonstrates the virtues that anthropology can uniquely communicate to our sponsors the public. With those admissions voiced, why then is the state of anthropological publication in such a shabby state? It is not for a lack of venues; how many top tier journals are publishing content well outside their titled domains? It is not a technological issue; portions of this essay I scribbled on post-it notes at a laundromat, on a memo application on my cell phone while riding a bus, forwarding such content to my cloud account and complied on to a free online word-processing platform. When the means of production are both free and ubiquitous, coupled with content starved channels, I infer the lack of production to be the fault of the anthropologists. When I see such behavioral signatures among the novice, I assume one of two fundamental causations: fear of ridicule, or an inability to connect the strategic vision of anthropology with the necessary and mundane acts that deliver anthropology to the sponsoring public. I am less forgiving of myself and other seasoned anthropologists when we fail to write. At the end of days, being an anthropologist is committing to a life of research, reading, writing, editing, and coaching the successor generations of anthropologist. This is the trade craft of anthropology and pursuing the strategic vision that humanity is worthy of understanding in all its variety circumstances.

As per the instructions of our beneficent editor, I'm to keep this essay short, useful, and blunting my vicious pen; each instruction contrary to my norm. If you are new to the trade and overly concerned that you'll not hit a home run at your first at bat and that's what is holding you back, then I hope this serves. All writers' first drafts are crap, it's editing that shapes the rough and not ready for others into something that has the potential to shine a light on the good that anthropology provides. Many novices are aware of this and make a fundamental error of editing while drafting copy. This cannot be done with any efficiency, consistency, and/or economy. Editing is a repetitive act that follows the act of writing. Writing is fundamentally a creative and deeply emotional act; your voice will leak out of your pen, which always tempts a writer to defend each verb and noun with too much zeal. Editing is the coldhearted act of "killing darlings," adhering to publication ethics and standards, assuring the writer's ego does not abuse the readers' attention, and demands narrative economy and clarity. In short, they are very different uses of your brain and contrary to any self-delusion you and others suffer, humans don't multi-task well. I could cite all the neuroscience research that supports this opinion but *tempus fugit*.

As to making that home run on your first at bat, it's not going to happen. Epic performance is the result of epic preparation; the anthropological "industry" does not expect a novice to be a master of the trade on their first day. Mastery, the effortless command of hundreds of micro-skills, only develops after years of continual use of those micro-skills. When I look at the copy I wrote some three decades ago, I cringe at the clumsy, inconsiderate, and inefficient narration of simple questions and the methods used to explore them. Accept that in your early days as an anthropologist, you are traveling a long road that consists of researching, reading, writing, and editing. Call these the essential macro-skills of being an anthropologist. Nested within the writing macro-skill there are the following micro-skills: organization, consistency, staying on topic, closing loops, and following through.

As to the micro-skill of consistency, for comparative purposes, modern American novelists have an average daily production of 1,000 word count a day with the average novel consisting of 100,000 words. The more prolific modern novelists (e.g., Stephen King) self-report an average of 6,000 words a day. My best performance was 8,000 words a day for 14 consecutive days, a feat I shall not repeat as it was mentally exhausting and in retrospect the product was self-indulgent dribble. There are two points to underline here; consistent measured effort nets actionable results and the motivated writer can achieve considerable volume of rough copy in reasonable time. If just starting out, set a modest personal goal of 500 word count a day; do this every morning (earlier in the day is always better when writing [yes there's uncited science that supports this opinion]). Do this habitually and sequentially as the habit develop; demand of yourself more words per day—you'll know what that upper limit is when you hit it. In the beginning, take the first five consecutive days' copy and on the fifth day compile that 2,500-word copy and start ripping it apart (a.k.a. editing). If you are inferring the use of calendar with committed blocks of time for these tasks, bright girl/boy you are. Your first pass should be reductive with the goal of finding more economical ways of describing/explaining the topic. Your second pass of editing should emphasize points of clarification, often you will know far more than the reader on the topic, so a modest amount of remedial explaining is necessary (added copy); do this without being a pedantic priss. There after your third pass (you didn't think editing was the easy task, did you?) review for voice and meter. I have found it easier to read the copy out loud; if the copy lands awkwardly on the tongue, it means there is narrative tissue missing or the voice is all wrong. Reading copy out loud provides the opportunity to review what is written rather than what you thought you wrote (I have a nasty habit of dropping articles, conjunctions, and a rapturous tendency for run-on sentence and parentheticals). Reading out loud saves me the embarrassment of an editor sending back drafts with a quip "seek medical attention."

As to organization, this precedes writing and in this example is a case of me not following my own advice. Start with a mind map of the topic and drill down to the various didactic questions, issues, data, methods, and whatever. After that visual exercise, weigh the branches of the mind map; which side needs more development/which is more interesting? Decide which side of the map to follow, turn it into outline, and then guess how much copy needs to cover each topic/sub-topic. The outline (table of contents) is not only a reader's finding aid—it's also your production schedule. For the love of God, don't feel the need to start your way from page one; hop and skip through the various sections, writing minimally a topic sentence for each and then dive into the stuff you are more comfortable with. Writing is emotional; booking an early win (getting copy on page) motivates a virtuous cycle of putting the other, less glamourous stuff on the page. You can flesh those sections out later when you are focusing on them. Remember you'll be editing later; in as much as humans don't multi-task well, we are also not very linear and long spells of focus can tap the tank.

As to the remaining micro-skills, our High Lord editor is holding me to word count, I only have the room to briefly stress the importance of follow-through. Lots of people want to be an anthropologist. The ones that have jobs and careers are the ones that not only can do the needed task but do the needed task. Yes, they write. Pick up your reluctant pen and write, only through the sharing of anthropological perspectives and its data shall the public, currently enamored with a celebration of ignorance and easily baited by divisive rhetoric as they are, can be served. And remember "done" is always better than perfect. Perfection is one of those fine notions that is seldom required. Developing the micro-skills on your way to be a Master of the trade, that's a far more tenable objective than perfection. The only one holding you back from that outcome is you—by not doing the necessary sets and reps that get you there.

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Some Hidden Facets of Writing Archaeology—Madonna L. Moss

When I was first hired at the University of Oregon, Don Dumond gave one piece of advice: "write like hell." In Darby Stapp's email message of October 2018, he asked a group of us contributors whether there has been a decline in anthropological writing. I believe your answer depends on the type of anthropological writing under consideration. While competition is tight for publication of articles in certain journals, regional journals have experienced declines in submissions, at least in archaeology. There has been a simultaneous steady decline in the publication of archaeological monographs in favor or narrowly focused journal articles. We may also see a decline of edited volumes in the near future, except for those in which the editors are senior graduate students. All these trends are new ways to "play the game" of publishing and are the consequence of pressures in the academy where administrators are most interested in hiring "stars," with over-the-top performance metrics that value international and national outlets over regional journals like *JONA*. In our annual (and other) reviews, faculty are asked to evaluate ourselves on our "research-related output efficiency," which is most easily measured by article counts, journal rankings, and the "h-index." The h-index (Hirsch index) measures the "impact" of an author based on the number of publications that have received *h* or more citations. As an example, my h-index is currently twenty-six, meaning I have twenty-six publications that have been cited at least twenty-six times. My i10-index is fifty-three, but I admit to having to look up the definition of this index. It means fifty-three of my articles have at least ten citations. Personally, I am appalled that one's academic "output" gets reduced to such numbers. I find such reductionist ways of evaluating one's contributions both insulting and demoralizing.

The most damaging trend of those described above is the decline of the archaeological monograph. Producing a monograph requires leadership, organization, and industriousness. Over the past fifteen or more years, many archaeological sites have been excavated, but the pressure to publish is so intense, that scholars focus on single (and often small) problems that can be addressed relatively quickly in journal article form. They may chip away at the larger analysis, but people seem to have forgotten the long-term value of thorough and complete reporting of archaeological investigations. A concomitant trend is the completion of article-based dissertations instead of monographs. I believe it is our solemn responsibility to report all aspects of an archaeological investigation, and that because monographs present primary data within a holistic context, they have enduring value. Archaeologists have to be trained to be good writers; we owe it to the archaeological record. Good monographs stand the test of time and will be consulted for years to come. Although most monographs won't garner headlines in *National Geographic* or other splashy media outlets, they will be consulted and cited in the future and they are a lasting legacy of our collective investments in recovering archaeological data. Our students who will work in the heritage industry need to know how to document their field projects. These are essential parts of the archaeological record that are not adequately valued in the academic arena today.

I think most of us have experienced a surge in one area of writing: email. Unfortunately, the more responsible you are at answering emails, the more work you generate for yourself. The more you do email, the more email you do. Every week I spend so much more time on email than I would like. My email is perpetually "out-of-control;" I have concluded it is uncontrollable. Although email has facilitated communication among scholars and has (perhaps) hastened the pace of journal article review and publication, I think that we have also witnessed degradation in the tone of scholarly communication. Everyone is under such time pressure, it is easy to be overly blunt; I know I am guilty of this. I recall with great affection thoughtful letters scholars used to write to one another. I still have letters written to me by colleagues R. G. Matson, R. Lee Lyman, Aubrey Cannon, and James Petersen from the 1990s. These were thoughtful and substantive responses to recent publications, and raised important questions for us to consider in future work. These represent the type of scholarly feedback one craves, but rarely receives. They were acts of intellectual generosity that I treasure. I have received few emails that compare in depth or insight.

This brings me to aspects of writing that are even more deeply hidden: the manuscript review process. As a manuscript reviewer, I know I've spent untold hours helping writers clarify their arguments and improve their writing. This work is rarely acknowledged, and sometimes over the years, I've probably spent more time improving an author's writing than they did. This dynamic occurred especially in the first decade after my Ph.D., when I was reviewing works of senior male scholars. On the one hand, I was able to read new and emerging work, on the other hand, I would spend 60+ hours reviewing a book-length manuscript and writing up detailed comments, in some cases, for individuals who had not taken sufficient time to write carefully. In my experience, male and female authors also tend to respond differently to reviewer comments. When I submit something for publication, I almost always comply with suggestions and re-work a ms. following the editor's and reviewers' comments. May I suggest that this is not how male authors always respond; in my experience they are more apt to be defensive and explain why they don't have to pay attention to reviewer comments. This behavior has a clear gendered dimension and may not have been experienced by everyone. I hope that it is changing. Also note that my words "may I suggest" are a feminized figure of speech, intended to soften the edge of my observation. As women, we learn to speak and write this way, sometimes to our detriment. I recall one more senior female scholar admonishing me to "write like you have a penis." This wise woman will remain unnamed here.

Over the years, I have been worried about the under-representation of women authors, particularly when it comes to theoretical work. Consider this: if I were to come up with a new approach to the topic of the initial settlement of the Americas or a new take on the coastal migration theory, am I likely to get it published or cited? I am betting the first attack I would face would be: "where is her evidence?" Yet some male scholars can put together the flimsiest of stories and get them published (sometimes in multiple places). I know that my hypothetical narrative is less likely to get published because I am female. For many women, it is harder to get theoretical work published, and much easier to focus on empirical work that is of undeniable, durable value and is less easily dismissed. I recognize this in my own work and I am more comfortable sticking with the empirical. But doesn't this perpetuate and reproduce the patriarchal structure of our discipline? Of course I have been supported by many male colleagues throughout my career. I am very grateful for their encouragement and advice. If any of you reading this essay have reviewed my submitted work, I extend my sincere appreciation for your efforts.

Dr. Stapp also asked contributors if we could share suggestions with writers. I will close with one recommendation related to writing conference presentations. This is a practice I've followed for years that has always helped me. It is particularly valuable in crafting the fifteen-minute conference paper. I always read my paper aloud, sentence by sentence (multiple times), which allows me to hear all the superfluous words. Then I cut out those words as I aim for clarity and succinctness.

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Writing Tensions: Voices That Help—and Those That Don't—Mark S. Warner

Part I: Finding my Voice

Thank you Mike Agar.

In my first semester of graduate school I had a class taught by the cultural anthropologist Mike Agar. In that class he had us read a book he wrote called *The Professional Stranger* (1980). We read the book partway through the semester after I had listened to him lecture in class for a few weeks. What struck me when reading his book was how there were places where I could literally hear his voice while reading. Reading Mike's work was, at times, remarkably like hearing Mike talk. I found that to be a revelation. Up to that point almost all of my school readings had never come across as sounding anything like a class lecture (try reading a typical journal article out loud—see how it sounds). The distinctive parallel between Mike's writing and hearing his voice has periodically come back to me over the years.

I went on from Mike's class and several years later produced a dissertation. Looking back on that not-so-classic work it is full of everything I grumble about today. Overall it is a fairly defensive piece of work, I make an argument and then spend many pages justifying why it was appropriate to use particular data. I also spend pages explaining potential contextual problems as well as preemptively refuting anticipated counter arguments to some of my claims. Years later I was able to clean up some of that writing (primarily through cutting sections) and my 350 page dissertation was turned into a 180 page book. My point here is that sometimes it takes a while to find your voice and have the confidence to write in a way that is comfortable for you rather than writing like you think something should sound.

Now to be honest, I really think that it takes time to find your voice, writing changes in part from simply growing as a professional. Personally I've experienced two things that come with time. The first is that the longer you do something, the more confidence you have in your abilities. Over time, I am increasingly confident in simply stating the positions that I want to take and explaining the rationale/evidence behind my arguments. I stopped worrying about trying to please everyone by anticipating what they *may* want to comment on or critique. The second benefit of time is that the longer you are in a profession you begin to identify people who are somewhat more like you in their writing. You find people like Jim Deetz, Adrian Praetzellis and my colleague Rodney Frey who have actively tried to tell stories through portions their written work. Let me be clear here, *in no way* am I implying that I have anywhere near the rhetorical eloquence of those folks. Rather, I embrace their boldness to write as they want to.

So to return to Mike Agar, when I re-read portions of *Professional Stranger* today there are still portions of that work where I can hear him speaking what I am reading—His writing and speaking came to me as a single voice—My goal is that my writing continues to move in that direction.

(Michael Agar died in May of 2017, as happens all too often, I never got around to reaching back out to Mike to tell him about my impressions of his writing and teaching.)

Part II: Losing my Voice/the Evils of Email

> Darby,
> Apologies for not being clearer about that
> Mark w.

(Full text of an email from Mark Warner sent to Darby Stapp on September 6, 2018. It was one of thirty-three emails I sent that day).

Sharp eyed folks may notice a couple of minor issues in the email—namely that I didn't put a period at the end of the sentence and that I didn't capitalize my last name initial. So I made two punctuation mistakes in ten words. People who are in regular correspondence with me through email will nod their heads knowingly. Frankly my emails are kind of sloppy and I acknowledge some responsibility for that. However, this little vignette is also the tip of a broader and somewhat insidious issue which is that our lives are dominated by expedient forms of communication such as email and/or twitter. On a daily basis we are jotting out quick missives such as my example. The question is what is the impact of this form of writing?

To explore this issue a bit further I went back and tabulated the word count for the 30 emails I actually sent on that day (three were emails I that forwarded on without comment). I wrote 1,327 words in those emails, the longest email consisted of 206 words. Put another way those emails amount to roughly four to five typed pages of work—or a good solid start on an article or book chapter. If you want to be really depressed about your productivity, extrapolate out those numbers over time. At that rate one could have a 200-page manuscript done in about 45 working days!

So what are the impacts? I think there is a corrosive effect on writing when you are slapping out emails all day. Specifically I would note three issues. The first is that "writing" becomes a quick and dirty thing. I end up pounding out emails during fifteen-minute lulls between meetings or while travelling or while waiting for someone, etc. In other words I am repeatedly jotting down responses. The result of just typing and sending is that work becomes a series of (poorly punctuated) snippets/bullet points and, speaking personally, it becomes sloppy. To be clear, there are times where I will spend a day crafting and editing an email before sending it, but what is typical are emails such as the one that opened this section—acknowledge it and move on to the next one in your in box.

A second issue is that a fatigue factor that comes into play. If I spend a big chunk of the day on a computer typing emails in fifteen-minute spurts, by the end of the day I am thoroughly done staring at my computer and typing—particularly since I have also been staring at the same screen working on presentation slides, spread sheets, etc. Physically I need to do something else—a state that doesn't help structured writing at all.

A final, and somewhat related point is that I think email has an impact on the discipline needed to write. As mentioned, I already spend a great deal of time staring at a computer every day. Beyond fatigue I also find that when writing almost anything these days I am readily distracted by incoming email pop ups (I know I can turn that off) or anticipating a time sensitive response to an email. I bang out emails all day but I now struggle to sit down and write 500 words in a dedicated block of time. My pop psychology diagnosis is email-induced attention deficit issues.

A final caveat: I am somewhat of a technological Luddite, I am on email, but I do not have Facebook, Twitter, or Instagram accounts. Email and all of these other social media platforms have absolutely transformed our ability to keep in touch and readily communicate with many, many people. In many regards that is a huge positive for the workplace. However I also think that a world increasingly consumed by staccato writing is not a world that fosters the creation of eloquent prose.

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From Writing Science to Writing for the General Public—Dennis Dauble

During my thirty-five-year career as a fisheries scientist, I wrote over one hundred fifty peer-reviewed articles and technical reports. I did so both because I learned the craft, and because it was a requirement for my profession. I have continued to write during retirement because, to paraphrase what Robert Barrass wrote in his self-help book, *Scientists Must Write*, "Writing helps you remember, helps you observe, and helps you think." One difference is that I no longer sit down at my computer to describe the results of laboratory or field studies. Instead, my current interest focuses on writing for the general public.

There are discrete differences in the two genres. For example, the organization of a typical scientific article is more prescriptive. The introduction includes a problem statement and a review of related literature. A methods section that follows is basically a description of what and how. Results include summary tables and figures along with explanatory narrative text. The final section or Discussion is the most important. Inference is drawn from key results and summary thinking is backed up with citations from the scientific literature.

Similar to a scientific article, narrative non-fiction requires a theme or a thread that takes the reader through the story from beginning to end. What's different though, is non-fiction authors have more opportunity to opine or wax poetic. Emotions can be bared; conversation revealed. One step further down the literary trail takes authors into creative non-fiction, an emerging genre that allows you to embellish facts "for the sake of story."

My current business card reads, "scientist, writer, educator." I chose those words carefully. Along with consulting on contemporary fisheries issues, I am a Board member of the Northwest Outdoor Writing Association, and I speak to conservation groups on such topics as the impacts of dams to salmon, bird-fish interactions, and the history of fish and fishing. These public interactions reinforce the importance of me being able to relate to an audience, especially when the goal is to educate them on a scientific topic.

According to the Oxford Dictionary, *journal* means a logbook, daily record, or diary. One thing I learned as a practicing scientist was the importance of maintaining data and formal observations in a logbook. I relied on logbooks (often with a quality assurance manual by my side) to record field and laboratory observations. These records served the basis for later scientific articles and reports. In contrast, my current collection of field journals capture details of outdoor adventure. For example, I might record the condition of weather, stream flow, phenology of native plants, wildlife observed, geological landscapes, fish caught (or lost), and whatever else comes to mind.

I keep a leather-bound journal beside my bed, write-in-the-rain notebooks in my truck and boat, and a pocket journal in every jacket and vest I take into the field. Over the past several decades, this habit has led to a pile of mismatched journals stored under lock and key in my den. Some evenings, I pull a journal from the bookcase and am reminded of poignant moments. Like the frosty autumn morning when my son caught his first trout, the camping trip when I accidentally broke wife Nancy's ankle while busting up firewood, and the August night when I woke to the scream of a cougar while a full moon rose over the Blue Mountains.

But more important, journaling provides fodder for non-fiction magazine articles and books that I write. The process of taking detailed notes allows me to capture the moment, tie down fleeting thoughts, and attach images to a time and place. I also record conversation that would otherwise be impossible to recreate. In this manner, my journals serve as a logbook of activities, settings, and feelings that would otherwise be lost.

In 2009, I wrote a natural history guidebook for people who wanted to know more about fish. I incorporated what I learned as a practicing scientist, classroom teacher, and avid angler. It wasn't easy. I studied writing aids, worked on my grammar, and developed a more consistent voice. These same writing skills might come natural to someone with a Masters Degree in Fine Arts, but not when you have a Ph.D. in a science discipline.

There are similarities in my previous and current writing life. For example, careful introspection and rigorous peer review are essential. Reading within and outside of my area of expertise continues to be important. Meeting deadlines and managing word count come into play, as does making friends with editors. I strive to include science in my stories whenever possible through the use of historical and life history facts. Admittedly, eliminating scientific jargon can be a challenge. Much like writing for other scientists though, writing for the general public is all about story. Tell a good story and you can communicate with any audience.

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Part 3

Part 3 contains the following essays:

- "A Commentary on Publishing" by Bruce Granville Miller, Professor, Department of Anthropology, University of British Columbia, and Canadian Anthropology Society Fellow. Miller's research concerns Indigenous peoples and their relations with the state in its various local, national, and international manifestations. Miller has authored or edited eight books and some two hundred journal articles, chapters, and reviews.
- "Why Write" by Jay Miller, an anthropologist in the old-school Americanist tradition, rescuing, researching, sharing, and writing about cultural contexts, archaeology, history, beliefs, kinship, lifeways, and languages of the Indigenous peoples across North America. Miller has written or edited fifty-five books and one hundred twenty articles.
- "Unearth and Heft" by Nathaniel D. Reynolds, an ethnoecologist and Interim Cultural Program Manager with the Cowlitz Indian Tribe. He received a M.S. in Environmental Science and Regional Planning in 2009 from the Vancouver Branch Campus of Washington State University.
- "The Language of Writing" by Astrida R. Blukis Onat, an archaeologist and ethnographer who received a Ph.D. in Anthropology from Washington State University in 1980; taught anthropology at Seattle Central Community College for 27 years; and founded BOAS, Inc., a CRM firm, in 1982. She has authored and coauthored more than 30 major data recovery investigations and monographs, more than a dozen published papers, three short archaeology teaching films, several brochures about archaeological sites, and too many CRM reports to enumerate. For the past 30 years, she has worked with certain tribes conducting ethnographic research for legal proceeding.
- "The Tin Shed: Why I Write" by Rodney Frey, Professor Emeritus in Ethnography at the University of Idaho, having received his Ph.D. in Anthropology from the University of Colorado in 1979. He has conducted collaborative, applied ethnographic projects with the Crow, Coeur d'Alene, Nez Perce, Warm Springs and Wasco Tribes, among others. Of primary concern has been the role and the significance of oral traditions, particularly as those traditions influence a people's relationships with their "landscape" and mediate the impact of Euro-American influences. As collaborative projects, he has also been concerned with the ethical issues associated with Tribal sovereignty and cultural property rights. Frey has authored five books, four internet books, five journal articles, and six book chapters.

A Commentary on Publishing—Bruce Granville Miller

My first anthropology publication came with the help and guidance of my dissertation supervisor at Arizona State University, Brian Foster. He had an insight into the work I was doing regarding Coast Salish social networks that I couldn't yet see; he showed me how I might mathematically operationalize concepts used metaphorically by Suttles and Elmendorf. That same year I published a piece about the federal recognition project, an interest I developed while working with the then-unrecognized Samish tribe. I noticed that not much was being written about an important, largely invisible process. I fell into both of these publications.

My first thought, then, is that instructors and professors should help students to develop some small, interesting corner of their thesis or dissertation research projects that might make a tight, engaging paper. This worked for me with Tad McIlwraith, now a professor at Guelph, and what I think was his first publication. This concerned the movement of Plains/Prairie ritual practices into the Coast Salish territories. We talked about this while he was a student in my ethnographic fieldschool run with the Stó:lô Nation. We were living in a longhouse in Chilliwack and a ritual leader there told us of his dismay about the use of tobacco. Tad used it as an opportunity to write about ideas of culture, responses to the contemporary world, and other issues which were of interest beyond the longhouse. On another occasion a graduate student I supervised gave a talk to a small departmental audience. Listening, I recognized a very strong insight into the issue of Israeli immigration to Vancouver. Just as in my own case, he hadn't yet realized, but it soon produced a nice publication.

Students at UBC ask where they might publish something and what the publication process is about. I spend time in a graduate pro-seminar on this and typically students don't know much about it. I go over nuts and bolts—reading journal guidelines, the length of time before they might inquire if there is progress on reviewing their paper, the sorts of responses they might get and how to reply (promptly, I tell them, before the editor changes his or her mind). I suggest that they lose their inhibitions and fears about publishing and recognize that even at the M.A. stage, most of them have something to say. They don't quite realize that. Part of this might be called finding your voice.

There are different stages in creating a writing career. A significant difficulty that I had early on was realizing when to stop in a paper and not to cram too many issues into too little space. An editor wrote in response to an early submission that the reviewers liked it, but there was too much content. I am thankful to those people for the good advice; I divided the paper in half and got two publications out of it. Eventually I learned to write papers which fit both my interests and the formats of particular journals. A few years later I published my first book, *The Problem of Justice*, which concerns Coast Salish historic practices and their responses to colonization. The Stó:lô Nation leadership had asked if I would do some background research on their justice practices. In the course of interviewing a Vancouver Island chief, I had the sudden realization that what he was telling me about their ancestral practices was significantly different than what I was told by Coast Salish leaders in Puget Sound and on the Fraser River. I realized on the spot that this must reflect differences in contact history and public policy aimed at Indigenous peoples. *That is a book*, I thought right then. A lot of publishing is about the unexpected.

My own approach is to write about things that matter in the present-day world. With few exceptions, the ideas come from the Indigenous communities with which I work. For example, I have worked with several non-recognized tribes and bands and found that there was no world-wide review of why there were so many of these groups. I came to realize that just as Indigenous populations were growing world-wide, resource extract was occurring in marginal locations where the Indigenous peoples had been pushed. Many countries came up with ways to administratively erase them and I wrote the first world examination of the issue. This arose initially from my work with the Samish. I fell into this, too. I didn't set out to do this.

I can't separate my writing philosophy from my research approach. More generally, my research strategy is to get inside the playing out of a social controversy, see it from the perspective of participants and write about it. Strangely, this seems to be an unusual practice. Here is an example: some years ago, an attorney contacted me to see if I could would provide expert testimony regarding the case of a First Nations woman harassed by security guards in a downtown mall. I thought about this and tried to write about the long-term adverse effects of surveillance and racialized segregation in Vancouver on Indigenous people now. This resulted in my testimony in *Radek* in the BC Human Rights Tribunal, testimony since cited by the Supreme Court. More of our students today are working on real-world issues and, I think, ought to write about them from the inside. Real issues identified from the ground up make good research programs and publications. These generally come from the application of very basic anthropology, such as how kinship and exchange systems work.

The next point about writing again concerns research methods: our students in anthropology are taught formal research methods and go through elaborate human subjects protocols as part of their graduate training. They learn, then, about creating one-off, complex research agendas, the results of which become thick dissertations. I did this, too, to get a doctorate. But I rarely do anything like that now. More likely, I am involved in small-scale events or productions which, taken together, provide the material for journal publications. It is the old bricolage approach once familiar to anthropology and memorialized by Lévi-Strauss. More publications might result if anthropologists were alive to the various things they have learned, just by living, watching, and participating in, as in my case, the Coast Salish world. This is a form of generalist practice which, I think, is well suited to the anthropology of today. A great example is a recent *Current Anthropology* paper by Bill Angelbeck, John Welch, Dave Schaepe and others, about understanding the therapeutic effects of engaging in archaeology for Indigenous community members.

A book I wrote, *Oral History on Trial*, emerged after I was goaded into giving expert testimony about the use of oral history evidence in Canada by the crown expert, following a Supreme Court decision which gave oral history the "same footing as written history." Here, the challenge of writing a book about all of this was sharing space with people with a strong sense of the importance of their discipline. In my case, this was legal scholars. My strategy was to ask some retired judges to explain legal concepts and to respond to ideas I had about how oral history evidence could be used. It worked. People like to explain their fields, I find, and we might seek out more allies and collaborators as a way to get into print and to take advantage of our discipline's rare groundedness. Part of my point here is that our students may not fully appreciate how distinct a grounded perspective is, and how valuable it is to our contemporary society, which is generally studied from above, in the abstract, and divorced from living people. I am struck by the frequency with which I hear ideas graduate students have which should be developed into journal submissions. To *JONA*, in many cases.

In brief, senior anthropologists might help students find the message in their own work and in community identified projects, and encourage them to submit to journals. I'm sure many do this already. We can inform them about the publishing process and how to work with it. We can point out journals for them to consider. And, we can encourage them to look beyond large scale projects to assembling the smaller bits of knowledge they gain as they go along. In addition, we can encourage participation in team publications and in projects with real connection to current social problems, and to embrace the unexpected. Above all, we can help our students understand that our disciplinary approach of looking at issues from the ground up is distinctive and valuable and worth writing about. Nobody else is doing it, and nobody will, if we and they don't.

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Why Write—Jay Miller

As a second generation Boasian, the eternal importance of write up was instilled in me repeatedly. This was further reinforced when I tried to find reports of important archaeological excavations in the Southwest for my own Ph.D. only to end in the disappointing realization there were neither notes nor final write ups of crucial sites. Often the only source of any information at all came informally in bars over drinks, often many of them to loosen tongues and long buried memories.

Finally, technology began to provide another solution as timing, people, place, and growing annoyance with a shirking profession glibly denying our ancestors; all converged to urge me toward self-publish my dozen long-languishing book drafts based on intensive work with key knowledge-holding elders, making them widely and readily available to tribes and scholars who have been so very helpful over the years and deserve to have their contributions on record. Following Smithsonian recommendations for the Handbook, these works are intended to be standard references for fifty years out.

Approaching her 100th birthday in 2016, Amelia Susman Schultz (Columbia Anthropological Linguistics Ph.D. 1939, chaired by Franz Boas) wanted more brain stimulation to accompany her regular tai chi and yoga and was told that "proof reading" was among the best mental challenges. She began by asking several friends and colleagues if they had manuscripts for her to work on, and, approaching me, I was only too happy to oblige with something I knew would interest her, bringing her abreast of Americanists decades on. After her requisite three reviews—for obvious typos and mistakes, for grammar, and for sense—of each manuscript, she returned a superbly corrected copy. Others soon followed, though she is always urged to keep to her own pace.

Of note, Amelia wrote two dissertations, one on acculturation at Round Valley that she was asked to withdraw so seven of her peers could get their Ph.D.s with the publication of a book featuring chapters that were their dissertations. Her second was a grammar featuring Aspect in Ho-Chunk ~ Winnebago, with the famous Crashing Thunder Sam Blowsnake, Big Ho-Chunk as her native speaker, approved in 1939, but not published until she used her first paycheck as a WWII WAC to make off-set printed copies that she sent to libraries and department to finally qualify for an awarded Ph.D. in 1943. Indeed, publication counted from the very beginning for Boasians.

The *Journal of Northwest Anthropology* (*JONA*), where I am associate editor, shifted in 2015 to Amazon self-publishing for its journal issues and memoirs, including a collection (*Memoir* 9) of twenty-five of my own articles. This shift introduced me to the digital procedure as well as provided me with hands-on guidance as I began publishing my own works and improving my CreateSpace skills. Near the end of 2018, CreateSpace was moved over to Kindle, where my paperback and E-books reside for sale on Amazon.

Another precipitating factor was the review and acceptance of an earlier manuscript by the academic press that has published several of my other volumes. This time, however, my Mounds draft was cut in half, and the reviewers (some my friends) were less than helpful, if not overly caustic and clueless. Self-publishing sidesteps these personal difficulties and preserves otherwise fraught friendships. In part, their startled reactions derive from my own limited participation in academic conferences, where my progressing analysis of data was expected to be marshaled and interpreted so as to be vetted in public during the solitary writing-up process.

Along with these ongoing pressures and traumas, are factors of aging. Medical concerns arose that urged quick action, carrying me through awkward and frustrating misadventures with computers, programs, texts, and PDFs.

As an active reader, I have also benefited from interviews with professionals. P. D. James taught me to write about what I am most interested on that day and then weave together the many pieces at the very end, writing the beginning overview at the very end. Tony Hillerman taught persistence in pursuit of publication, even as he was repeatedly told to leave out the Indians by earlier reviewers of his Navajo series. Many other writers have since woven ethnic themes and peoples into series now popular in the US, Canada, and internationally.

Focusing on outcomes, distractions and conflicts were held off as I concentrated more and more on final edits, revisions, and hard copies. While time and money have usually been mutually exclusive for me, I suddenly had a bit of both, as more and more scholars espoused "digital humanities" despite incongruence within Indien country, where electricity can be beyond the means of families and native churches still rely on candlelight.

Finally, by making quantities of my books readily available to native families fulfills mutual pledges with scholarly elders, spanning decades, half a century in cases. Thus, my life burdens are lifting and my future options include more freedoms, sharing, promise, and flexibility among wider choices.

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Unearth and Heft—Nathaniel D. Reynolds

An invitation by *JONA's* editors to contribute an essay to a volume on anthropological writing is an unexpected honor, especially because I never set out to do anthropology—I stumbled into the field by luck and happenstance. Instead, my education, field training, and personal interests focused on natural history and ecological conservation. I came to science as a memorizer, able to recall Latin names and obscure facts. Picture the classic Scottish naturalist collecting and preserving specimens, preparing them for shipping from some distant shore back to the halls of Edinburgh. These were my heroes, and their tools were fieldbooks of jotted notes and Victorian-era curiosity cabinets packed with artifacts, fossils, and oddities. I was dismayed during the early stages of my master's thesis research to learn that making species lists and recognizing patterns was no longer *de rigeur*. I was told: "No, you'll need to find a project where you can look at pattern, hypothesize what process causes the pattern, then test the hypothesis to determine whether or not your beliefs about the process are valid. We kick the tires these days!"

Twelve years ago, I was hired by the Cowlitz Indian Tribe to work in their Natural Resources Department. I focused on conserving and restoring species and habitats that are culturally-relevant to the Cowlitz People. I assembled long lists of ethnobotanical references, and learned the names of places in the landscape that are traditional resource-gathering sites. I apprenticed with Cowlitz knowledge, learning how to roast camas roots in an earth oven, how to dip and smoke-dry eulachon, what season is right for pulling cedar bark. I swung stone adzes and hefted fishing weights unearthed from archaeological sites. In the discipline of anthropology (in a comparative sense to ecology) these details are pattern.

Old-style anthropology, like old-style natural history, often emphasizes artifacts and cultural materials. But these items no more adequately represent a living culture than a moth-eaten, taxidermied museum wolf represents its lithe, wise, and fierce incarnation. Heritage items of material culture are silent and asleep in glass museum cases. They come alive when they are talked to, honored, used, and put into a cultural context, but only by the people who made them, loved them, and lived with them for generations.

I recently toured the American Museum of Natural History in New York, and the Northwest Coast Hall (opened in 1899 under direction of Franz Boaz, the "father of American anthropology") is getting a prominent update. On October 15th, 2018, the museum announced the appointment of *Nuu-chah-nulth* artist and cultural historian *Haa'yuups* (Ron Hamilton) as co-curator for the redeveloping Northwest Coast Hall.

In the press release announcing the appointment, Peter Whiteley, the museum's curator of North American Ethnology, stated: "With the reimagining of the Hall, our goal is to present the art and material culture of the Pacific Northwest in a way that highlights the ideas, voices, and perspectives past and present behind these wonderful historical pieces." Museum President Ellen V. Futter observed: "*Haa'yuups* will bring an important perspective for millions from all over the world who will visit the reimagined Northwest Coast Hall and its updated presentation of cultural treasures."[1]

This opportunity to give some space and voice to the dynamic, living culture, the processual agent, the cause of the artifacts and art-effects, is progress! But the language of the museum representatives still primarily focuses on the trappings of culture, the glass cabinets commodifying "wonderful historical pieces" and "cultural treasures," rather than putting the rich political, cultural, and social identities of the makers foremost.

The discipline of anthropology, like ecology, is slowly learning it is an error to pit process versus pattern. Rather, process and pattern (like cause and effect) are a dualism. They complement, inform, and rely on each other, and can best be understood in an integrated, holistic way. In ecology, the route to understanding process begins with recognizing and describing pattern; effective anthropological analysis likewise should proximally begin with pattern, but ultimately illuminate the meaning of culture and identity, and be reported in a hybrid and intersectional manner. The objects alone cannot speak, and the tenacious people who made them are the ones who testify best to the heritage of use, of the cultural meaning imbued in the physical material.

Nazarea[2] introduces ethnoecology as "a way of looking at the relationship between humans and the natural world" including the cultural "schema, scripts and plans that orient people," and yes, looking is good. But listening may be even better. I believe a successful path to understanding Indigenous ways of being—or perceiving what knowing and living with non-dominant cultural heritage means in our modern world—is to directly hear the voices of the Indigenous in the narrative.

My philosophy of writing, my "always a beginner" entry to Indigenous process/pattern dualism, is this query: "What does it mean to be Cowlitz, in this time and place?" I ask Cowlitz citizens endless, bothersome questions: What does it mean to the Cowlitz that mountain goats are returning to the slopes of Mount St. Helens/*Lawetlat'ła*? What does it mean for Cowlitz identity that eulachon population numbers are low? What does the act of digging camas roots mean for you? What do you feel when you hear your great-grandmother, her voice recorded on a wax cylinder, sing her huckleberry-picking song? I record their answers and file them away in the Cowlitz Tribal Archives. They speak their own words. I only listen.

I adopt this approach in my writing, and recently co-authored a *JONA* article titled "The Pacific Crabapple and Cowlitz Cultural Resurgence."[3] The first half of the article presented pattern: a review of crabapple harvest and processing techniques along the Northwest Coast. The second half of the article detailed a modern Cowlitz crabapple harvest. It explored what it means for resurgent Cowlitz identity and Cowlitz People to be doing the act of harvest, at this time, in that place. Christine Dupres, co-author and Cowlitz citizen, was fundamental in expressing opinions and Indigenous frames of thought that I, as a white researcher, cannot

[1] https://www.amnh.org/about-the-museum/press-center/haa-yuups-renowned-nuu-chah-nulth-artist-and-cultural-historian-named-co-curator-in-restoration-of-historic-northwest-coast-hall

[2] Nazarea VD, 1999, Preface, in *Nazarea VD (Ed.) Ethnoecology: Situated Knowledge/Located Lives.* University of Arizona Press, Tucson AZ, USA

[3] Reynolds ND and Dupres C, 2018. The Pacific Crabapple (*Malus fusca*) and Cowlitz Cultural Resurgence, *Journal of Northwest Anthropology*, Vol 52(1):36–62.

fully know. When we sent an initial draft out for review and comment, one editor commented, "This first part is fascinating, but the lengthy section about Cowlitz identity at the end should be significantly reduced." Another editor said, "Remove all this dry ethnographic first part and focus on Cowlitz identity. That's where the paper really happens." In the end, we tightened and kept both sections—and let process and pattern, cause and effect, entwine on the page.

This is my approach to writing anthropology. I strive to find compelling perspectives—to unearth and heft meaning in the material. Then I ask pattern and process to dance with each other in order to more closely express the through-line and truth of the narrative. And as much as possible, I try to get out of the way so the voices of the People themselves come through.

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The Language of Writing—Astrida R. Blukis Onat

The following essay contains personal experiences with learning language, considers structural elements of writing, and suggests the extent to which writing is culturally circumscribed. It is a reflection of a lifetime trying to write in English.

As a child, one is not aware of just how much is learned very early about using language. Accents, idioms, and metaphors that float in adult conversations are picked up as children grow. Children's books tell cultural stories. These bits of communication will be used in speaking and writing in later years. A child placed in situations where multiple languages are being spoken may absorb all of them simultaneously and master the particulars of each as they grow up. They can code switch at will, and will continue to do that as adults. They will be truly bilingual and bicultural. If childhood languages are learned sequentially, confusion may result. As a new language and culture displaces the old, each will be known incompletely. English was the fourth language I learned before the age of ten.

My first languages, Latvian and Russian, were learned simultaneously. At age four, my family left Latvia for Germany. Russian was left behind. Living in Germany for the next five years, I learned to speak German and spoke it fluently from ages four to nine. We continued to speak Latvian with parents and in our refugee community. When I was nine, we emigrated to the US. At home, we continued to speak Latvian. Since there was little opportunity to speak Latvian outside a family context, I never managed to become truly articulate in adult Latvian.

By the time I finished high school, English was my major language. My fantasies of being a good writer were frustrated for lack of the basic elements of a language most children learn as they grow up. What I call the "story bits," were missing. The best I could do was follow the rules of grammar and attack writing assignments with correct spelling. It became clear that my writing would be limited to term papers, theses, and a dissertation in the social and natural sciences.

The structure of Euro-American writing is linear, with a beginning, a middle, and an end. Scholarly writing begins with an argument, provides data, and ends with a conclusion. A personal voice is missing and not encouraged. Because such writing is meant for a limited audience of fellow professionals, writing can be full of professional jargon—a sub-language. Conclusions are hesitant and couched in equivocating clauses. The language used is specialized and is not understood by the general public. I have written many cultural resource reports and authored papers within this structure.

Midway through my archaeological career, I conducted research in post-Soviet Latvia. I found that Latvian archaeologists used ethnographic and historic data to both search for archaeological sites and to interpret what was discovered. "Story" was a part of this research and writing. Given this model, I determined to use local ethnographic studies to address investigations in the Puget Sound area, become more informed regarding contemporary tribal culture, and include both in my interpretation of regional archaeology.

The earliest ethnographers spoke to tribal people at a time when an older generation of native speakers was being lost. The ethnographers all made some effort to learn the native language. The next generation of native Lushootseed speakers learned the language from their parents, meanwhile learning English outside the home and in school. These bilingual persons often translated for their elders. Information was recorded mostly in English.

My first serious attempt to incorporate tribal story with archaeological data came in an unfortunate context. We were conducting an archaeological survey of an area associated with a geologically unique rock promontory. Adjacent to the promontory, we found a petroglyph showing two snakes carved into a large boulder. An archaeological site also was located nearby. The owner of the property wanted to mine it for the rock and was intent on determining that associated archaeological materials were not important. The Upper Skagit Tribe was trying to preserve the feature because of its cultural significance. In preparing for a legal hearing, we gathered ethnographic information about the location to support the archaeological data.

In a Nookachamps story anchored in this landscape, the promontory is identified as Snake. Two other rock features in the floodplain on the small river are named Mouse and Frog. Another character, Beaver, also is featured. He lives in Beaver Lake nearby and has his own house. Two versions of the story had been documented in the 1950s.[1] The two pages of the story describe the natural environment, give details of the effects of major flooding, and include a humorous discussion of marriage customs as represented by the animal actors. Features of the landscape are used to structure the story. The connections between the story, the archaeology, and the place were very obvious in this context.

However, explaining the importance of the rock promontory to non-Indian people with an agenda that was focused only on removing the promontory for profit was an exercise in disconnected communication. Telling the story of Snake had no effect at the hearing. The story was dismissed as "just a myth" and had

[1] Snyder, Sally
2002 sgʷaʔčəɬ syəyəhub *Our Stories, Skagit Myths and Tales Collected and Edited by Sally Snyder*. Lushootseed Press, Washington.

nothing to do with the promontory. There were no cultural bridges that could be crossed, even with a common language. Snakes, mice, frogs, and rocks were of no importance. Neither was the spiritual significance of geologic remnants. In fact, it seemed that bringing the story into the process made it harder to validate the archaeological finds. The petroglyph was disfigured, the archaeological site ignored, and the promontory has been mined down to a nub. I could not understand why the obvious connection of story to place was so vigorously dismissed, even by some in the archaeological community.

Therefore, I delved ever more deeply into the ethnographic record. I examined the wealth of field notes made by individual ethnographers. Most notes were direct quotes from tribal participants, sometimes backed by audiotapes. From these, it was evident that the information as spoken and written down was structured not at all like how it was presented in publications. The notes revealed the accustomed tribal story form. They were teachings tied to features of the landscape. As such, the notes contained much more information than what was summarized in any publication. Even while providing information about cultures not our own, the academic writing style was structuring interpretation for non-Indians. It was difficult to hear Indian voices in that context.

Perhaps an entire book written from a bicultural perspective would communicate better. Two tribal elders I knew had retired from tribal work and were assembling a number of stories they had written into a book. Edith Bedal had served as tribal historian and recording secretary during the time the Sauk-Suiattle Tribe was acknowledged, Jean Bedal Fish was tribal Chairman. The mother of the sisters, Susan Wawetkin, was Indian; and their father James Bedal was a pioneer. The sisters spoke the native dialect of the Sauk people, learned from their mother and their Indian relations. The sisters learned to speak English from their father and pioneer neighbors. They learned to write in a school established at the Bedal homestead. The sisters grew up bicultural and bilingual.

Both sisters had written about the tribal history they were a part of. They wrote down the customs and legends as told by their mother. Edith Bedal also wrote about experiences living at the homestead and working as a guide in the mountains around Glacier Peak. Jean Bedal Fish wrote about the early pioneer history of the upper Sauk and Stillaguamish River area. She supplemented her own knowledge with information in early history books, often using phrasing dating to that time. The mix of Sauk legends, family history, tribal history and pioneer history was told in a series of stories. The Sauk River valley was described as a beloved place and the persons who had lived there were endlessly fascinating.

With support from a USFS grant, we organized the writings and selected photographs. I copy edited the stories and wrote a preface. Before they passed away, I promised to get the book published.[2] *Two Voices* was first privately printed in 2000 for a memorial service that included the sisters and other tribal elders.

When I approached three different publishers with the first printing, I was told that the stories could not be published as presented, that the writing style of the Indian legends and that of the historic information was too disparate, and that the collection of stories needed a "context." In other words, the book could not stand

[2] Edith Bedal (1903–1995); Jean Bedal Fish (1907–1997).

on its own and the work needed some explanation. I was somewhat taken aback at this response. The book made total sense to me as I was by then more familiar with tribal customs, local history, and the Sauk area. We even conducted an archaeological excavation at the Bedal homestead. There continued to be demand for this book in Indian country, in the local community, and among a few researchers, so extra printings were made, the last of which was published with a new preface in 2016.[3]

Presently, I am attempting to write the "context" book to go around the stories told in *Two Voices*. I cannot change the nature of the original. I have gathered more information about the area, local history, and the family. I have found more stories and photographs in the materials the sisters left in my care.[4] The writing structure will be familiar to scholars, with lots of references to *Two Voices*, the information amplified by voluminous footnotes.

The essence of *Two Voices* for me was that the authors wrote in disparate styles but with one focus, telling the story of their homeland and its inhabitants, hence the title. It was clear that they held both the Sauk Indian and the Pioneer perspectives in their beings. They did not try to explain one in terms of the other. The sisters grew up knowing two cultures. They learned local pioneer history, written in linear English. They also wrote down their oral history as they had heard it. They code switched as needed.

Writing the history of a landscape and a people in a written language that is not structured for the task is difficult at best. How does one use a linear structure to present stratified and diffuse information about a homeland full of personal history spanning millennia? There is no beginning or end to the Sauk stories for those accustomed to hearing them in the context of the landscape that is a homeland. Rocks, hillsides, peaks, stream, and rivers tell the story of a people whose ancestors have lived among them. Legends tell of the relationships among people, land, and animals through metaphor. The natural and spiritual world are connected. Social interactions are carried out by the living and non-living. Daily life and work are memorialized in rock promontories. Stories connect to other places and people. Traditional family areas have a local history, the telling of which belongs to people who have remember their experiences in reference to the landscape. It is the setting of the story that structures other elements. That is how the stories in *Two Voices* were written. One could assemble them in any order and pick out one or another as wanted.

And so, I do not have a structured ending for this essay. I cannot reconcile the way the English language is used in scholarship with the way it is structured to tell the old Indian stories. Nor do I feel it is necessary. It is possible to hold both in the heart and see the world from more than one direction. I can read that Snake is a tall guy with grey eyes and stinks and know that the greywacke stone from the promontory has been found in many archaeological sites in the region. I know that when South Wind and North Wind are fighting, there will be toppled trees and the weatherman will tell of dramatic changes in atmospheric pressure.

[3] Fish, Jean Bedal and Edith Bedal
2016 *Two Voices, a History of the Sauk and Suiattle People and Sauk Country Experiences.* BOAS, Inc. Seattle.

[4] The Bedal materials are slated go the Special Collections at UW once I have finished cataloging them.

I can see that Tahoma is the mother of all waters and understand that if the ice all melts our rivers will go away. I like to hear the context of a story as told by the original inhabitants. Organizing data to end with a conclusion about an idea is also satisfying. Communicating between these approaches may be difficult but the effort is so very rewarding.

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The Tin Shed: Why I Write—Rodney Frey

The following is a compilation of actual events and experiences, chronologically rearranged, to create a personal essay in narrative form. It's a story that seeks to address some perennial questions, through the lens of ethnography. What motivates me to write and what's the philosophy behind that writing? What challenges have I had in getting published? What suggestions do I have for would-be writers? In this storytelling the "answers" are often implicitly embedded in the narrative, many rephrased as questions that can help guide.

I'd arrived early that hot June day, anxious to get started. The interviewee, a Tribal elder of some fifty years my senior, asked if I'd wait on an old wooden bench under the shade of a cottonwood just out front of his modest home. He had a few chores he wanted to first finish. As I sat there, fidgeting with my cassette tape-recorder, going over my many questions, my mind kept wondering, reflecting on just how this urban, middleclass, white-guy, who had a pretty easy upbringing, got to this place. What did I ultimately seek, and why? How would I begin to convey to others what I might learn from this elder? The reflections went beyond simply that I was here now, having been invited by the Tribe, to research what could be done to improve health care delivery.

In the rush of seemingly random thoughts, one stood out. I'd gone to an inner-city high school, with a graduating class of some 800, a quarter of whom were African-American, with significant numbers of Asian-American and Hispanic students. It was a wondrous mix of stories, in the classroom, throughout our community, and on the track. I was a runner, and for this "white boy," pretty good, a member of our state-champion track team. My senior year I anchored our mile relay. We traveled together to meets throughout the state, practiced hard and depended upon each other. Together, we endured disappointments and celebrated accomplishments, together we told stories. I had a sense even then, though not fully comprehending it, that at the core of our humanity, we're all storytellers—*Homo narrans*. On this predominately Black-team, I participated in difference, yet in those fluid moments as the baton was handed off, there was no difference, and in the stories we told, it made all the difference. And I waited for the Native elder.

With the recorder on, I asked about kinship, ceremonies, language, bombarding him with youthful enthusiasm. After a while, enough was enough, and the elder held out his hand, stopping me in mid-sentence. Silence. Then he pointed to a corrugated-metal building, some fifty yards to the north. It housed highway equipment, trucks and tractors, or so I imagined. And turning directly to me, he asked, "Do *you see* that tin shed?…it's kinda like our way of life…you can sit back here and talk about it…but *not really understand*…it's not 'til you get off your bench….go *inside…listen…feel it*…feel it with your *heart*…*see* it from the *inside* looking out…that you really know what it's all about…you've *gotta go inside*." Later he added, "And I'll come along…*with you*…be with you," and then asked, "What are you *gonna do* with what you've learned?"

There could be no better questions asked, no better preparations offered to a neophyte ethnographer. This little story would guide my journey over the next four decades, as I and my students engaged with the Apsáalooke, the Niimíipuu, the Schitsu'umsh, along with other Native communities. Their stories rich with diversity yet imbued with shared humanity. And in their re-telling, hopefully a difference could be made.

"Do *you see* that tin shed?" The elder could have gotten up, terminating the interview. Instead, he offered a sort of permission, as a host, to engage. It illustrated the critical role in acknowledging Tribal sovereignty and the cultural property rights of your host, and of following the Tribe's research protocols. Would I be invited, as a guest? And I act as one? Would I adhere to a Tribal review of the concluding research, that assessed its authenticity and appropriateness, before going public? Of course, a Tribe might deny permission, deeming a proposal unsuitable, or having their own ethnographer, no need for a guest.

"I'll come along…*with you*." Would I work in collaboration with my host, he a guide, showing the way, and I willing to follow, avoiding mis-steps? Would I become a trusted ally? It's a collaboration from the start, beginning with the research design, continuing as we interviewed, coded, interpreted and formatted the final paper, concluding in co-authorship. None of my annual evaluations or promotion and tenure reviews were ever weakened because of co-authorships, just the contrary. While seemingly trite, isn't research and publication ultimately about respect, relationship and reciprocity, as we *together* explore our differences and reveal our common humanity? And challenging it, a growing essentialism?

"Get off your bench….go *inside…listen…feel* it with your *heart...see it* from the *inside* looking out." If I was to "*listen*" and "*feel*," I mustn't view from afar, but with empathy and my best ethnographic skills, experientially engage in relationships with members of my host community. Relationships built on trust and respect. But even then, perhaps the greatest challenge is in attempting to "*see*" and "*feel*" from the perspective of those I engaged. While ultimately an impossibility, the journey to an "insider's" perspective is nevertheless essential, for the alternative is to continue in ignorance, bias and prejudice. Can I begin to "*feel*" "*heart* knowing," what the Schitsu'umsh call *hnkhwelkhwlnet*, "our ways of life in the world,"—Indigenous ontology and epistemology? Could I begin to grasp a world view so completely alien to my upbringing? Let go of how I've been taught, replacing it with an Indigenous pedagogy? Let go of my own preconceptions about the nature of reality. Let go of Cartesian Dualism and Aristotelian Materialism, replacing it with the "transitory intersection of those participating—human, animal, plant and spirit peoples—anchored to place-based teachings?" A reality not of discrete objects, reducible to material forms, but of unfolding events, co-created by those in relationship? And the elder asked, "What *brings forth* that rainbow?" Taking it deeper, at each juncture of the research and writing, I needed to acknowledge, deconstruct and adjust my own white, male privilege, my own colonizing predications, and challenge the same in the environments within which I traveled. Could I critically self-reflect, and critique the academy? Could I get off the security of my old wooden bench? Could I be as a child wrapped in the cradleboard of my host, and truly listen and feel with my heart?

And then, "how would I begin to convey *hnkhwelkhwlnet* to others?" The challenges continue. Early on I appreciated the unequivocal relationship between *what* I was researching, i.e., relating to orality-based narratives and behaviors—*how* that content was conveyed, i.e., acts of verbal and symbolic discourse, such as storytelling and ritual procedures—and *who* were the "others," i.e., the recipients of the storytelling. If I was to begin to convey *hnkhwelkhwlnet* content, applying my best word-sculping skills, I needed to attempt to use an appropriate means, aligning the *how* with the *what*. And as with a good storyteller, I needed to know my audience, rendering the story accessible without impairing its authenticity. To do otherwise would only distort what I sought to convey, "whitewash the Indigenous."

Here comes the rub. How would I and my co-authors publish through a media, e.g., professional journal or book manuscript, that in its literacy-based nature could undermine the orality-based message we wished to convey? In seeking a better alignment, the means we'd use needed to come closer to acts of traditional storytelling, of *baaéechichiwaau* (Apsáalooke) and *'me'y'mi'y'm* (Schitsu'umsh). To help provide an oral nuance and sense of the rhythm in the telling, when opting for a written media, we transcribed narratives using a poetic style, complete with intonation, pause and hand gesture markings. We even encouraged the reader to first access the story through the voice of someone reading to him or her, to better experience the orality of the unfolding story. We've also "published" via the web, on internet modules that streamed elders being interviewed and retelling stories, providing a more authentic auditory and visual experience. And most recently, we've found an intriguing alignment in the application of orality-based content through an interactive 3D virtual reality web module. The "user" becomes an avatar, interacting via a joystick with an elder, as a traditional root is gathered. If not "listening" to the elder's dialogue and responding to his subtle directions, the story ceases and the module must be initiated anew. In the experience, the user is offered implicitly conveyed Indigenous teachings. Surprisingly, this cutting-edge media technology has parallels with the structures and dynamics of Indigenous orality-based storytelling. Regardless of format, publishing comes with financial costs, often contingent on marketability, a benefactor or grantsmanship. Ultimately, there cannot be a substitute for the experience of engaging directly with an elder's storytelling. But in our ethnographic endeavors seeking a wider audience, can we at least better appreciate the challenges and begin to address them? Can engaging a book begin to create an experience conveying placed-based teachings through the transitory intersection of the elder with the Coyote with you, the reader?

"What are you *gonna do* with what you've learned?" Fundamental to the Indigenous way is sharing with those in need, to give back to others. In our anthropological history, there's been too much taking. The "give back" certainly needs to be defined by the community. I've been involved in a range of applied projects, addressing such issues as health care delivery, a language arts curriculum, a natural resource damage assessment, climate change, and an Indigenous perspective on Lewis and Clark. Stories that seek to make a difference.

Does not successful ethnographic publishing really start with an invite and permission, moves to collaboration and relationship, adding a dose of deep self-reflection along with some suspension of disbelief, a dash of aligning the *how* with the *what* with the *who*, culminating in giving back? With challenges faced at each juncture of the journey. Can publication be other than the offspring of a labor of love, and certainly not the impetus for the research? While there are other routes to publishing, I suspect if you've successfully traveled the "tin shed," you could be well along your way to publishing.

I *baaéechichiwaau*, aka, write, to re-tell the cherished stories that celebrate diversity and reveal the ubiquitous, in the hope of making a difference. I've found no greater satisfaction than in the act of re-telling the stories, be they in oral or written form. Late into the evening, after sharing his favorite stories, to be included in an anthology we were putting together, the elder turned to me and affirmed, "If all these *great* stories were told…great stories *will come!*"

Ahókaash to all my teachers, co-researchers and co-authors. To the reader, may this story bring you a difference.

You can glimpse my publication history at: http://www.webpages.uidaho.edu/~rfrey/

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Conclusion

Writing and Publishing in Anthropology: Voices, Insights, and Disciplinary Trends—Tiffany J. Fulkerson and Shannon Tushingham

We appreciate Darby Stapp and Julie Longenecker flipping the typical organization of contributed volume compilations—here, we (the most junior authors of all the participants) have been given the position of commenting on essays by a group of prolific and influential senior writers. This is in keeping with the intent of this endeavor: as stated in the introduction, the purpose of this special section is to encourage and inspire other professionals to write and publish. The call was met with an enthusiastic and impressive response. In in just two months, nineteen writers sent in their essays, with commentary ranging from practical tips on how to write, to views on research ethics and the struggles of publishing. The essays are not only entertaining and often deeply candid, but also the individual stories offer profound insight into the reasons why these individuals write. While much of the imparted wisdom is directed at early career professionals, this effort is also aimed at encouraging mid-career and senior authors to share their wisdom and life's work for the purposes of posterity and ushering valuable hidden knowledge into the light.

Accounts such as these are important, not just because they are inspirational, but also because they can provide insight into larger disciplinary patterns. Understanding how and why people write is central to our research, which examines trends in the production and dissemination of knowledge in North American archaeology. We are specifically concerned with who dominates discourses in STEM sciences and the systemic factors that influence individual decisions to write, while advocating for multivocality and equity. We have compiled large-scale datasets that demonstrate that women, cultural resource management (CRM), and agency archaeologists publish significantly less than men and academics in peer-reviewed journals. The reasons for these inequities are many and include the simple cost-benefit dynamics of publishing, which vary according to one's occupation, gender, and other intersectional identities (e.g., Tushingham et al. 2017; Fulkerson and Tushingham 2019). We believe that promoting multivocality will provide many benefits to our discipline, and by shedding light on these dynamics and addressing some of the systemic issues, it is possible to promote a broader range of perspectives and voices in archaeology discourse.

Writing can be hard, lonely business, as is navigating the peer-review process. This set of essays provides a great deal of insight into the detailed mechanics of writing, as well as writing philosophies, motivations, and approaches. Below, we review some of the major themes that are covered. For the seasoned scholar, writing may come as second nature, but it goes without saying that such knowledge is not magically imparted on writers (except, perhaps, for a few charmed individuals), and without proper mentorship, it can often be very difficult for young writers to break into and establish a career in writing. It is also important for mentors, editors, and senior scholars to acknowledge that young anthropologists face a very different professional and publishing landscape than they were once socialized into, and so we conclude our essay with commentary about the changing landscape of writing and publishing and some thoughts on how individual writers may navigate this brave new world.

Reviewing the Essays: Sampling Considerations

The nineteen contributing authors in this compilation of essays encompass a variety of occupations in anthropology (see Stapp and Longenecker, introduction to this collection), which affords a diversity of professional perspectives. It bears noting, however, that only four of the nineteen authors are women, which amounts to 21% of participants. This, of course, is not representative of the high proportion of women anthropologists working in the northwest today, but it is consistent with observations made by some of the few women authors who contributed to this special section (see below). It also parallels research findings demonstrating that women remain inadequately represented in anthropology publishing. For example, Bardolph (2014:527) found that women comprised only 29% of first/single authors in regional publication venues in North American archaeology from 1990–2013, while our own research demonstrates that women accounted for a mere 27% of first/single authors in peer-reviewed journals from western North America from 2000–2018 (Fulkerson and Tushingham 2019).

The disproportion of women to men authors in this special section was not an intentional editorial slight—Stapp initially contacted those former *JONA* authors and peer reviewers who he knew published a lot or cared about writing. Of the original invitees, eight (32%) were women (Darby Stapp, personal communication). Not all who were given the opportunity chose to or could participate. While the intentions were not to obtain a representative sample of people who write in anthropology, the demographic makeup of the nineteen authors who ultimately participated mirror other trends in anthropology beyond men/women ratios. North American anthropology has historically been and continues to be dominated by white, heteronormative, and cisgender people, and there is a strong male bias among older generations of practitioners (e.g., see Zeder 1994). As Stapp and Longenecker noted in the introduction to this special section, 100% of the contributing authors are white, while the average age of the authors is approximately 65. Thus, the demographic makeup of the contributing authors is illustrative of some of the complex historical and idiosyncratic factors that can factor into issues surrounding publication and demography. Future essay compilations may garner insights from Indigenous people/People of Color and LGBTQ+ individuals, which would not only help to clarify the dynamics of writing and publishing for these underrepresented groups, but would also help to illuminate why such people are not well represented among practitioners and those who publish in the discipline. Future research and solicited insights from people with identities that are underrepresented or have been historically marginalized, as well as from younger authors and even non-anthropologists, will help to provide a more inclusive understanding of writing and publishing in northwest anthropology.

<u>Essay Insights on Writing and Publishing.</u> The essays cover several themes: 1) writing philosophies, approaches, and insights; 2) challenges of writing and publishing; 3) motivations and rewards of writing and publishing; and 4) advice and

recommendation for students and colleagues. In some cases, writing philosophies/approaches and advice were implied rather than explicit, being illuminated through anecdotal experiences. Often, they were closely tied to or indiscernible from one another. While challenging, we made a good faith effort to summarize some of the key insights from the nineteen papers with careful consideration of the messages conveyed by the contributing authors.

Writing Philosophies, Approaches, and Insights. A recurrent theme in the compilation of essays is that of change. Carlson, J. Miller, Mierendorf, and others observed that technological advancements have precipitated considerable change in the world of writing and publishing. Online access to journals and other internet resources have provided new solutions to knowledge dissemination and can influence decisions of where and how to publish. As noted by Carlson, the digital revolution has or may be rendering some print journals obsolete as online methods of research communication change the cost and speed of publication.

Many essays emphasized the importance of writing well. Becoming a good writer requires time, dedication, and training. It often necessitates the development of an effective writing style. Several papers criticized common approaches to writing that employ excessive jargon that isn't accessible to a wider audience. Numerous authors noted the importance of writing clearly and concisely. Several stressed the utility of committing to a writing schedule and reading widely, both within and outside of anthropology. Adherence to these approaches affords developing writers the opportunity to find their voice, which Warner and B. Miller reminded incipient authors to put time and effort into developing.

A number of essays emphasized the need to consider the impact of research and writing on the many stakeholders of anthropology. Anthropology is a collaborative enterprise that includes multiple participatory and invested communities, from Indigenous groups and the general public to agencies and clients. Blukis Onat, Frey, Reynolds, Kehoe, Mierendorf, and Dauble pointed out the need for writing that reaches Indigenous communities and the public, as well as writing that incorporates Indigenous perspectives. While anthropologists are (or should be) well aware of the past and even present transgressions of our discipline, we see from these essays that decolonized and intersectional approaches to writing may serve to mend grievances and build cultural bridges moving forward.

Insights into gender disparities and patriarchal systems that continue to limit the participation of women in publishing come from Kehoe and Moss. Kehoe reminded us that women are less likely to publish when they, as people, are overlooked by society. Moss provided anecdotal experiences to highlight gendered dimensions of writing and publishing that often disadvantage women. Consistent with our own research and the research of others (e.g., Bardolph 2014; Tushingham et al. 2017; Fulkerson and Tushingham 2019), Moss and Kehoe spoke to the underrepresentation of women in publishing and the underappreciation of women's writing, especially within the realms of theory.

While there was a wide diversity of writing philosophies and approaches that materialized throughout the nineteen essays, it is clear that persistence is a shared quality of the prolific writers who contributed to this special section. Lyman, Croes, and J. Miller highlighted the importance of persistence when pursuing publication.

Indeed, it is fair to assume that nearly every writer who has gone through the painstaking process of publishing their work has been faced with the challenges of putting "pen to paper" (or rather, "fingers to keyboard"), tackling the drudgery of editing, and maneuvering through obstacles set forth by reviewers, committee members, and editors. As it were, a recurrent theme throughout the compilation of papers has been the modern-day challenges of writing and publishing—challenges which, no doubt, account for much of the contemporary lack of writing output that several authors observed.

<u>Challenges of Writing and Publishing.</u> In the frank words of Butler, "Writing is hard." Many authors observed that the writing and publishing process is time and energy consuming. Personal limitations include fear of ridicule, lack of drive or commitment, perfectionism, and life in general. Then there are the systemic and institutional barriers to publishing that our aforementioned research addresses. Anthropologists are employed in a wide variety of professions including academia and CRM, the latter of which comprises 90% of archaeologists in the U.S. (Sebastian 2009:7). King, Griffin, Plew, Carlson, Moss, Kehoe, Warner, and Ames spoke to changes in the professional landscape of anthropology that have complicated publishing in modernity. These difficulties include but are not limited to: fewer tenure-track positions and lack of proper mentorship in academia, lack of incentives and support for publishing for those in CRM, the necessity of time-consuming emails, and the increasing emphasis on high-impact journals, which has reduced incentives to produce monographs and publish in regional venues (see also our discussion below).

<u>Motivations and Rewards to Write and Publish.</u> Regardless of the many challenges, there is a resounding consensus among the authors that writing and publishing can be deeply satisfying and rewarding. Many of the prolific writers indicated that they enjoy communicating their research and collaborating with colleagues. Lyman, Plew, and Dauble observed that writing and publishing make us better thinkers and teachers. As Kehoe noted, it allows us to have our voices heard. Authors share a desire to contribute to and shape their field, as Ames pointed out. Butler indicated that writing helps to build a sense of community and ensure posterity, while Butler and J. Miller discussed the delayed gratification of seeing a project through to completion. For those who work closely with Indigenous and host communities, satisfaction can come from Indigenous collaboration and conveying knowledge through Native perspective, as detailed by Frey, Blukis Onat, and Croes. Importantly, numerous authors reminded us that anthropologists have a professional obligation to disseminate their work. We maintain an ethical obligation to communicate the results of our studies not only to each other, but also to the public and the communities that we write about.

<u>Advice and Recommendations.</u> The contributing authors of this issue imparted invaluable advice about research, writing, editing, and publishing that students, junior colleagues, and even seasoned authors will undoubtedly benefit from embracing. We briefly reference or directly quote some of their wisdom as follows:

- Read a lot and widely (Butler, King).
- Recognize the value of your research. Gain confidence and lose your inhibitions and biases (Griffin, Warner, B. Miller, Frey).
- Make your data and fieldwork results available (Carlson, Croes).
- Do not wait for inspiration to write. Just do it (Butler, King).
- Commit to writing copiously and consistently (Butler, Lyons, Mierendorf, King).
- Find your audience and know them (Croes, Dauble, Frey, Mierendorf).
- "When in doubt, cut it out" (Ames).
- "Kill your babies" (Ames).
- "Eschew BS" (King).
- All first drafts "are crap" (Lyons).
- "…'done' is always better than perfect" (Lyons).
- "Epic performance is the result of epic preparation" (Lyons).
- Don't edit while you write. The editing and revision processes are essential components of writing (Ames, Butler, Kehoe, Lyons).
- Learn and improve from reviewer feedback (Lyman).
- Build a thick skin when it comes to the peer-review process (Lyman).
- Writers: support publishers. Editors: be more proactive (Mierendorf, Plew).
- Encourage more compliance archaeologists to publish. Challenge non-disclosure provisions in CRM contracts that inhibit publication. Use publication as a form of mitigation (Plew, Griffin).
- Share with and give back to the descendant and host communities that you research or collaborate with (Blukis Onat, Frey, Reynolds).

The Present and Future of Knowledge Production and Dissemination for Anthropologists in the Pacific Northwest (and Elsewhere)

<u>The Changing Landscape of Writing and Publishing.</u> Stapp and Longenecker presented a basic question to the authors: "Why don't we write more?" The answer is not straightforward. We contend that people write a lot—the volume of written output has grown exponentially, particularly if one counts technical report writing, which has exploded after the growth of CRM. We have found that extra-academic professionals publish more in non-peer-reviewed venues than peer-reviewed ones (e.g., Tushingham et al. 2017; Fulkerson and Tushingham 2019), and it is for this reason that we have suggested that such an outlet, if introduced, might benefit dissemination of knowledge in the northwest region (Fulkerson and Tushingham 2018). The landscape of publishing is different for academic professionals, where pressures to publish remain constant and have even heightened. Publication output for academics is especially important in today's job market, where coveted tenure-track faculty positions are becoming increasingly scarce (Speakman et al. 2018; see also Ames, this volume). Taken together, it seems that people are still writing and publishing in high numbers. So how do we explain the lack of writing output that was observed by the contributors to this special section? As some of the contributing authors have hinted at or suggested, we believe that what we are seeing is a trend towards reduced writing and manuscript submissions among specific

research dissemination outlets, which, at least in part, is a response to changes in technology and the standards for measuring professional success in modernity.

Impact Factors, Online Dissemination, and Today's Hyper-Competitive Academic Market. There has been an accelerated push in academia to embrace quantitative metrics as a measure of productivity and research impact. Such measurements include the Journal Impact Factor score, the h-factor, and altmetrics. These measurements consider both the quantity and presumed quality of papers. Consequently, peer-reviewed papers, especially those with higher impact scores (which tend to be national/international in scope), typically hold greater weight in academia (see Moss, this collection). In general, those with higher frequencies of influential publications are more likely to be selected for tenure-track jobs and to secure tenure once they have obtained a faculty position (see Griffin, this collection). Students also feel these pressures—at least those who seek academic faculty positions. In today's hyper-competitive academic job market, candidates with a low number of publications will have a tough time getting a job, much less an interview.

The hyper-competitive job market and emphasis on publication output influences decisions of where and what to publish, while encouraging a new world of active dissemination. In the age of digital open access publications, altmetrics (a measure of public impact as measured by online news stories, Twitter, Facebook, blogs, and other social media outlets, etc.), and for-profit repositories that allow researchers to make their works available online for free (Academia.edu, ResearchGate, etc.), researchers are navigating new territories that allow them to actively publicize their work in unprecedented and more highly visible ways.

All of these variables have both positive and negative effects on publishing. On the one hand, this phenomenon can be beneficial as it pushes individuals to circulate their work and provides more opportunities to communicate research than ever before. On the other, it encourages many writers to focus on "high impact" journals at the detriment of regional and "low impact" ones. Importantly, papers in journals that afford high impact scores do not necessarily equate to high *quality* papers. Indeed, some of the most valuable works that we're familiar with come from technical reports, theses/dissertations, monographs, and regional journals like *JONA*—all of which typically either garner low impact factor scores or are not rated at all by the modern quantitative metrics reviewed above. Today, writers may opt to "salami slice" their research, cut corners, or engage in other behaviors that will strengthen their publication output, even at the expense of research and writing quality. While we don't agree with all of these practices, it's critical to understand the current dynamics of writing and publishing and the pressures and constraints that young scholars face, and to acknowledge that this is a very different landscape than older generations of writers grew up in.

The Growth of Extra-Academic Anthropology. It is important to acknowledge that conversations about writing and publishing typically revolve around academic publishing, yet only a small proportion of anthropologists are in academia, and an even smaller number are in research-intensive universities. In archaeology, as with many other fields, there has been a major growth in the private sector and in agency work since the mid-1970s. Key regulatory developments led to the expansion of "compliance archaeology," which involves Section 106 and related management activities conducted by government agencies and CRM outfits. Those who work in this field produce a great deal of written work—technical reports, National Register of Historic Places nominations, etc. While some of the best archaeology work is done by compliance professionals, too often this research is not widely disseminated, and thus remains hidden from much of the archaeology community, let alone the public (Lipe 2009:50). Although individuals in non-academic anthropology professions may produce the greatest volume of written work, academic anthropologists continue to publish a great deal more in both regional and national peer-reviewed journals. Realistically, it can be a challenge for CRM and other professionals to find the time to write up their results in venues such as *JONA*, but finding ways to facilitate and encourage such submissions would only serve to benefit the discipline (see also King, Griffin, Carlson, Ames, this collection).

The Future of Knowledge Production and Dissemination. While it is difficult to predict the future of anthropological writing and publishing, evaluating past and current trends in the discipline provides insights into potential future directions. For most of us, writing is and will continue to be a non-negotiable skill. Anthropologists and early career professionals must learn to write, write a lot, and write well. They must also navigate a new landscape of publishing, with both opportunities and constraints that are quite different from earlier generations. Successfully maneuvering through the formidable publishing landscapes of today and the future will require individual persistence and the ability to adapt to the many obstacles that will invariably be encountered.

In order to maintain the relevance of anthropology as a discipline, and out of obligation to the stakeholders that frequently fund or contribute to anthropology projects, it is critical that we become more proactive about conveying our research and its importance to each other, the communities that we study, and the public. This will require strengthening collaboration efforts, supporting regional journals that allow us to communicate research to our colleagues in visible ways, and using online resources as well as digital media and non-traditional communication forums in order to disseminate research through new, ethical, and innovative approaches.

There remains a persistent gap in the visibility of the work of certain groups in anthropology, which inhibits the diversity of voices and perspectives that contribute to and shape our field. Today, roughly half or more of anthropologists are women, and most work in extra-academic settings. If the current market share and hiring environments are any indication (see Speakman et al. 2018), these trends are likely to continue or grow. Moving forward, we should cultivate an environment that encourages the voices of not only women and non-academics, but importantly, Indigenous people/People of Color, LGBTQ+ groups, and other periphery groups that remain underrepresented in publishing. Ensuring a more robust and equitable future for anthropology writing and publishing will necessitate strong mentorship, support for CRM and agency professionals to publish their work in accessible forums, the dissolution of institutional impediments to publishing for certain demographics, individual persistence and perseverance, and certainly not least, taking to heart the invaluable wisdom that the contributors to this special section graciously imparted.

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