

THE PLACE OF OZETTE IN NORTHWEST COAST ARCHAEOLOGY

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Introduction

For many non-archaeologists and archaeologists, Ozette is Northwest Coast archaeology. It is the only Northwest Coast site that is regularly described, or even mentioned, in many textbooks, including introductory archaeology texts. It is one of only eleven North American sites discussed in Price and Feinman's *Images of the Past*, a glossy "who's who" of major archaeological sites worldwide (Price and Feinman 1993). Ozette is mentioned in Renfrew and Bahn's encyclopedic and widely used text: *Archaeology: Theories, Methods and Practice* (Renfrew and Bahn 2000). It is one of 18 sites, and the only west-coast site, covered in Thomas' *Native North America* (Thomas 2000). When Herbert Maschner and I were working on our own recent book on Northwest Coast archaeology (Ames and Maschner 1999), the editor and peer reviewers insisted that we include more on Ozette than we originally had. While we did not at all initially ignore Ozette, we had deliberately focused on other sites to counter what could be termed "Ozettopeia", or the inability to see the rest of the coast because of Ozette. Nevertheless, they were right and we expanded our coverage. Why is Ozette so important?

Ozette's contributions to Northwest archaeology and anthropology and to archaeology generally can be assessed in terms of three broad areas: the archaeology of water-saturated sites, ethnoarchaeology, and public archaeology and the relationships between Indian people and archaeologists. It is useful in some of the discussion that follows also to distinguish between contributions made by the archaeological project and the impact and effect of the site itself. Obviously, without the project, the site might have washed out to sea in a series of storms; but the site has, I believe, some intrinsic qualities that the project illuminated, but which are not consequences of the project. The single most important of these inherent qualities is that Ozette is an extraordinarily well-preserved water-saturated village that was seemingly buried in an instant.

Most archaeological sites represent the slow accretion over long periods of bits and pieces of people's material lives. When archaeologists expose them, these bits and pieces have been winnowed by rot, human activity, and the slow grind of geological processes. Not so Ozette. Everything, down to the leaves of plants behind the houses, was sealed in an instant and preserved. Thousands of wooden and fiber artifacts, normally lost to decay, survived, many reasonably near their original location. For this reason, Ozette often has been called "America's Pompeii."

However, I think people's fascination goes far beyond this extraordinary preservation. Ozette has a human immediacy that most archaeological sites lack, particularly for the non-archaeologists. It is often difficult to see the people behind the accumulated strata and tools in a rockshelter, for example. However, it is possible, even easy, to visualize life in the Ozette houses and to empathize with the disaster's victims and their families. This immediacy makes Ozette a riveting, human story that transcends time and culture. There are other Ozette stories: the Makah reclaiming significant parts of their history, archaeologists working year-around on a stormy coast to salvage the town, and the science that is the topic of the rest of this essay. But the heart of all of these stories, I think, is that human immediacy.

In the rest of this essay, I will briefly assess Ozette's contributions and importance in the three

areas mentioned above: the archaeology of water-logged sites, ethnoarchaeology, and public archaeology and the relationships between Indian peoples and archaeology. However, the human immediacy is the warp that holds the story together.

Wet-site archaeology

Virtually all of Ozette's significance rests on its being a water-saturated site with extraordinary preservation, so most of the following sections could be subsumed under this heading. However, in this section I stress two points. The first relates to the site's fame. Ozette made wet sites visible to both archaeologists and the public. There is both a positive and a negative side to this fame. People became aware of the potential contributions these sites can make to archaeology, and they became aware of the expense of wet site excavation, particularly of the costs of analysis and curation. The second point is methodological. The Ozette project proved the utility of hoses and water streams to excavate a wide array of perishable artifacts, including cordage, basketry, and planking. It also, and I think more importantly, confirms the value of large exposures and samples.

Wet site archaeology did not begin with Ozette. Europeans have been excavating water-logged sites since the last century (Coles 1984, 1998, papers in Coles and Lawson 1987), with Clarke's excavations at Star Carr among the most famous (Clarke 1954). Japan is another country with a long history of wet site archaeology (e.g. Aikens and Higuchi 1982). In North America, extremely important, on-going research on water-logged sites has also been conducted in Florida, for example (e.g. papers in Purdy 1988). Ozette was not the beginning of wet-site archaeology on the Northwest Coast (papers in Croes 1976). The first hydraulic excavations on the coast may have been at Hoko River in 1967 (Gleeson and Grosso 1976). When excavations began at Ozette in 1969, other wet-site excavations were also underway on the coast (e.g. Hobler 1976).

However, the Ozette story attracted attention, as these other projects did not. Part of this was timing. Ozette was excavated as modern news, and entertainment media organizations were rapidly evolving and competing for good stories. Widely published and broadcast images of crew members with hoses exposing baskets, whaling hats, folded blankets, rows of pegs, bundles of whaling gear, whale saddles and engraved planks were compelling evidence for what water-logged sites could produce. The sheer number of perishable artifacts recovered was persuasive evidence that ordinary, dry land archaeology recovered only a minor fraction of the potential material culture record. Other Northwest Coast sites, including Old Musqueam (Borden 1976) and Lachane (Inglis 1976) for instance, produced examples of such items, but only Ozette yielded them in such extraordinary numbers, and as parts of entire houses.

Archaeologists looking at the news reports, attending conference papers, or reading popular magazine accounts, the annual grant reports, and dissertations, as they emerged learned of the extraordinary contextual evidence preserved in the Ozette houses. Since then, the analytical evidence has been slow to develop – partially because of the overwhelming amount of data – and has yet to attract equivalent public attention. There are reasons for this: contextual data are complex, and analyses must be well advanced for them to be easily popularized; media reports of archaeology focus on the discovery of amazing things, presenting a picture of archaeology as a discipline that is entirely discovery driven. The patient refinement of computer maps covered in dots and triangles is much harder for reporters, even well informed ones, to cover, albeit perhaps more important in the long run than beautiful things. Additionally, this subsequent work is woefully under funded relative to the fieldwork (a typical problem in archaeology, only magnified here by curation and the size of the data set). In any case, the excavations, the artifacts recovered and the subsequent and ongoing

analyses – some described below – clearly demonstrate the potential significance of water-logged sites.

This potential has been only partially realized in the Northwest. Its realization has been due, in part, to the persistence of a few individuals, including Dale Croes and Katherine Bernick. The failure to fully realize the potential is, I think, due to some pervasive issues and problems in archaeology generally, as well as problems peculiar to the Northwest Coast.

Since Ozette, research on waterlogged sites in the Northwest has taken two directions. The first of these involves continued work on single sites, such as Ozette, Hoko River (Croes 1995), Little Qualicum (Bernick 1983) Old Musqueam (Borden 1976), Musqueam Northeast (Archer and Bernick 1990), Lachane (Inglis 1976), Montana Creek (Betts 1998) and so on. The second direction grows out of the first, but conceptualizes the research in terms of wetland landscapes (e.g. Bernick 1998a). The Hoko River project is an excellent example of the former approach.

Hoko River, like Ozette, is on the Olympic Peninsula, but on the northern side, facing the Straits of Juan de Fuca (Croes 1995). The site contains two localities. One – The Hoko River locality – has both a wet and a dry component. The second locality is a rockshelter with a dry midden. The wet component was excavated from 45 vegetal mat layers that had been tectonically uplifted from the bottom of the Hoko River. These spanned a period between ca. 3,000 and 2,600 B.P. – considerably older than Ozette – and placing this component in the Locarno Beach phase of the regional cultural historical chronology (Mitchell 1971, 1990; Matson and Coupland 1995; Ames and Maschner 1999).

Croes (1995) conducted excavations between 1977 and 1989. The site produced a wealth of perishables, most of which appear to be associated with fishing. The site is interpreted to be a fish camp. The project was also the basis for an ambitious model that simulated economic changes on the northern Olympic Peninsula during the Holocene (Croes and Hackenberger 1988). The modeling was based on a detailed site catchment analysis of the Hoko River site and the Hoko Rockshelter. This model placed Hoko River in a regional context.

Using Hoko as an example of on-going research on water-saturated sites actually distorts that research. Most projects are much smaller than the Hoko River project and less well funded. Many are accomplished as salvage work, sometimes under difficult conditions.

The second direction investigating water-saturated sites has taken attempts to place these sites in a regional context and to use small sites or projects to good advantage. The emphasis here is on water-saturated sites as part of wetlands, and on the ecological, social, and cultural importance of wetlands and wetland landscapes generally (e.g. Bernick 1998a). On the Northwest Coast, wetland research has focused primarily on intertidal zones and estuaries, particularly on locating, mapping, and dating of fishing facilities such as wooden weirs and traps (e.g. Byram and Erlandson 1996). This research, as well as that at Ozette and Hoko River, contributes significantly to our knowledge of the evolution of Northwest fishing technology and the coast's fishing economy. It is also a response, I believe, to some of the problems that excavations like those at Ozette and Hoko River pose. The root of these problems is the immediate and ongoing costs of excavating water-saturated sites.

Water-saturated sites are expensive in money, labor and/or time to excavate (e.g. Singley 1998). The very richness of Ozette, for example, imposes an extraordinary analytical and curatorial burden. All of the thousands of artifacts have not only to be studied – measured, described, etc. – they have also to be preserved in perpetuity. Research on fishing facilities can take advantage of the positive aspects of water-saturated sites (stakes pounded into tidal mud flats can last for millennia), making significant contributions, but at relatively low cost. One can map the stakes, collect a small, random sample of them, have the wood and technology analyzed and run AMS dates on the wood at far less

up front and on-going costs than excavating large numbers of baskets.

These comments are not intended to minimize the importance of this line of research; it is very important, particularly its stress on the idea of wetland landscape; nor are they intended to minimize the importance of large-scale excavations like Ozette or Hoko River. I believe both directions are extremely important for several reasons. One of them is sample scale.

Archaeology is facing a major methodological conundrum. Large-scale excavations of both wet and dry sites have become increasingly expensive. This has happened for many reasons, some having nothing to do with archaeology. There is also increasing opposition to large-scale excavations both within and outside the discipline because of their inherently destructive nature. Some Native American groups, for example, oppose any excavation unless a site is threatened with immediate destruction. There are also archaeologists who feel our duty is to preserve and conserve sites, rather than to excavate them. If we excavate them, we should minimize our impact.

Within the discipline, there is also increased emphasis on recovering what might be termed micro-scale data, microdebitage, seeds, etc., and the use of techniques that emphasize such data (e.g. 1/8 mesh hardware cloth). These techniques raise the costs of excavation, analysis, and curation, making large-scale excavations increasingly difficult. Micro-scale data also can be collected from small excavation units (1x1m units or auguring). What is sacrificed is macroscale data such as architecture.

However, it is clear from Ozette (Samuels 2001) and other work (Lyman 1991) that large exposures are necessary to even approach adequate sampling of houses, and to acquire samples large enough to draw reliable conclusions. Lyman shows that on the Oregon coast, one needs to excavate a minimum volumetric sample of 100 m³ to be assured of recovering house features or to confidently conclude, in the absence of such features, that no houses are present. Ozette plays two significant roles here, as an exemplar of the crucial evidence that only large excavations can acquire and as a large-scale excavation at a time when such excavations are increasingly rare. In other words, the Ozette data can be used to pursue questions that only data at its scale can answer, data which are not likely to be replicated any time soon.

I am not advocating unlimited large-scale excavations. There are many routes to answering archaeological research questions that can and should be pursued in lieu of automatic excavation. There are, for example, many excavated collections which have never been fully analyzed or described, or which will significantly repay reanalysis. It is possible, however, to overestimate the number of these, or to underestimate the difficulties. There are important questions that only new, large exposures can answer.

Ethnoarchaeology on the Northwest Coast

It may initially seem odd to describe Ozette as an ethnoarchaeology project, but that is how I see it, particularly now, and I want to persuade others to see it that way as well. This is perhaps the most important "hidden agenda" in this paper. I had originally called this section "Paleoethnoarchaeology" but that term is too awkward and ugly a neologism and ethnoarchaeology is close to my meaning here.

Ethnoarchaeology began in the late 1960s as an effort by archaeologists to observe material culture dynamics and site formation processes among living people. The intention was to provide firm observational bases for making inferences about the archaeological record and the human behaviors that are the ultimate interest of all archaeologists. However, the potential field of those observations is narrower than the archaeological record itself. Because of many events over the past

500 years or so, modern cultures, globally, are far less diverse than they were, perhaps even a century ago. For this reason ethnoarchaeology has been limited primarily to some kinds of hunter-gatherers and peasants. Among the societies that are unavailable for this research are complex hunter-gatherers, particularly on the Northwest Coast. For some issues, that limitation may be irrelevant, for others it is not.

The Ozette project was originally conceived within the framework of the Direct Historical Approach: i.e. on the assumption that pre-contact life at Ozette was not qualitatively different (Trigger 1989) from the “ethnographic present” of the Makah and other groups on the coasts of Washington and Vancouver, and the ethnographies could be used to illuminate the former. However, it was also clear that the extraordinary archaeological record at Ozette could be used to elucidate, test and to revise the ethnographies. As the project has developed, this line of research has taken the direction of using the site to test many of our preconceptions not only about the ethnographic record, but also about the archaeological record and how it forms. I think this direction is almost inevitable, given the wealth of the Ozette record. This direction is also extremely important. Before discussing Ozette as Ethnoarchaeology, it is important to place that in a broader context.

Since Boas, Northwest Coast archaeology has been an exercise in the direct historical approach in one-way or another. There are many reasons for this, but among them is the now abandoned but long-held assumption that human occupation of the coast was relatively short, perhaps no longer than 2000 years, and that there has been relatively little change during that time. This assumption underestimated the impacts of contact. Thus, the ethnographic pattern could be easily extended back into time. Even when it became clear after World War II that people have been on the coast for a very long time, a primary goal of Northwest Coast archaeology remained documenting the history and evolution of the ethnographic pattern. That goal may be recast, for example, as explaining the evolution of complexity on the coast, but it is still the ethnographic pattern of complexity we are explaining. There is in this a great strength, but also a great weakness. Without great care, our reasoning becomes inevitably circular. Ethnographic patterns (of inequality for example) are explained based on the presence or absence of the relevant ethnographic objects (e.g. ethnographic status markers e.g. labrets). The argument can become we know they had ranking because the ethnographic form of ranking appears to be present. The only escape from this circularity is to test, using independent data and theory, the inferences and links we make between the ethnographies and the archaeology. Despite these problems, the direct historical approach is virtually forced upon Northwest Coast archaeology by the special place of the Northwest Coast in the history of Anthropology and by the vastness of the ethnographic literature itself.

The Northwest Coast has played a central role in the development and testing of Anthropological theory in North America, from Boas’ initial field work through Eric Wolf’s recent use of the Kwakwaka’wakw as an exemplar of his theories on the relationships among power and ideas (Wolf 1999). Native societies on the coast were exceptions to many rules: stratified societies based on hunting-fishing-gathering, hunter-gatherers with large, permanent towns who moved around a great deal, small-scale societies that produced monumental art of exceptional quality, and so on. Every archaeologist and anthropologist working on the Northwest Coast, and elsewhere, is familiar with its traditional role in Introductory Anthropology text books: as the inevitable exception to most of the generalizations about cultural variation generated by a century or more of anthropological research. This “specialness” makes the history of these societies an important archaeological issue. Ironically, the analyses of our sociocultural colleagues are often uninformed by Northwest Coast archaeology. In fact, modern Sociocultural Anthropology, despite its current

affair with particularism and historicism, is often remarkably uninterested in long-term history as revealed by archaeology. However, there are two additional ironic and more important points I would make here.

The first point is that the coast's very importance as an example and exception and the very size of the ethnographic record can obscure the coast, and obscure its history. In some ways, studying the ethnographic record is like using a powerful spotlight; it is marvelously illuminating but it also blinds you to, or conceals in its shadows, whatever is outside the beam's ambit. We are forced to use either the "best" examples (e.g. the Kwakwaka'wakw which is a "best example" because of the historical accident of Boas' work), or a distillation of a typical Northwest Coast (e.g. Drucker 1955), which is often a "best" example in disguise. These approaches obscure crucial variability along the coast. When these approaches are applied in archaeology (through the direct historical approach), then variability in time and space, the stuff of history and evolution, is obscured. There are exceptions to this. Donald (1997) recently analyzed Northwest Coast slavery using a sample of societies from the entire length of the coast. His sample was based on criteria of relevance and completeness, not on the apropos anecdote. The book is, I hope, a model of future explorations of the ethnographic and ethnohistoric record. It is also important to stress that I am not at all arguing that the ethnographic record be ignored. Rather, I am suggesting it needs to be treated with greater respect and care.

In any case, the ethnographic record is THE record for interpreting Northwest Coast cultures, past and present. It is often privileged in archaeological works. If the archaeological record does not fit the ethnographic record, than the flaw rests with the archaeology. However, by yet another irony, it was that ethnographic record and the coast's stereotype that had important consequences for global archaeology in the past 30 years.

It has become overwhelmingly clear in the past 25 years that modern hunter-gatherers represent a limited sub-set of the range and diversity of hunter-gather societies and economies that have existed over the past 50,000 years, or even the past 12,000. Many of these societies have been termed "complex hunter-gatherers," because they share a number of features that had long been assumed to be exclusively associated with farmers. These include large populations, residential stability (some variety of sedentism), large settlements, relatively elaborate and costly material culture, intensive food production and, usually, but not inevitably, permanent social inequality. These societies include (but are not limited to): the Mesolithic of northwest Europe, the Jomon of Japan, and perhaps other, early Holocene societies of Northeast Asia, the Natufian of southwest Asia, possibly some Upper Paleolithic cultures of Europe, the Chumash and other groups in California, the Calusa of Florida, some middle Archaic groups of the southeastern USA, and so on. It is very likely that these societies would not have been recognized as complex hunter-gatherers, or at least recognized at the time they were, without knowledge of the Northwest Coast.

In sum, the Northwest Coast has been of extreme importance to Anthropology (including archaeology) since its inception in North America for many reasons. It remains theoretically and substantively important. However, the very ethnographic record that is the basis for that significance creates problems for archaeology on the coast. This archaeology as presently practiced is inconceivable without the ethnographic record (including ethnohistories) but archaeology also struggles with major problems created by that same record.

What has this to do with Ozette? Ozette presently is the crucial link between the ethnographic record and the archaeological record of the coast. Because it pre-dates contact by only a very few decades, and because it is extraordinarily rich in both artifacts and contextual data, Ozette provides our best laboratory for testing the connections between ethnography and archaeology as very

different kinds of data, in what could be termed paleoethnoarchaeology. There are a large number of topical areas where the Ozette excavations have played or can play this role. I cannot review all of them, but will discuss several in the context of Household archaeology, and try to show how the kinds of inferences being built up about the Ozette houses constitute an ethnoarchaeology.

Household Archaeology

Household archaeology has become increasingly central to Northwest Coast archaeology during the past 20 years or more. It crystallized both theoretically and methodologically in 1981 with the publication of an issue of the *American Behavioral Scientist* devoted entirely to the topic (Wilk and Rathje 1982). The roots of household archaeology are firmly in the archaeology of Mesoamerica (Wilk and Ashmore 1988) and studies of peasant societies, some among the earliest examples of ethnoarchaeology (Kramer 1982). Related concepts include Sahlins's "domestic mode of production" (Sahlins 1972) and Levi-Strauss' "House" societies (Levi-Strauss 1983, Joyce and Gillespie 2000). Here, I will include all of these under the rubric of "household archaeology." These approaches were quickly applied on the Northwest Coast (e.g. Coupland 1985a, 1985b, Ames 1985).

Household archaeology is a logical framework within which to study issues of Northwest Coast cultural evolution. The household was the fundamental economic, social, and political unit of the Early Modern period. To understand the evolution of Northwest Coast institutions then, we must understand the history of Northwest Coast households, the cockpit for that evolution. Ongoing research suggests that this role is at least 3000 years old. Activities associated with houses, including, but not limited to their construction, maintenance, and final abandonment, were among the primary site formation processes that created Northwest Coast archaeological sites. Household archaeology was anticipated by the Ozette excavations, although excavations of the wet site at Ozette began a decade before the concept of "household archaeology" was formalized. Houses were the fundamental unit of excavation and remain the central focus and unit of the ongoing analyses.

The Ozette excavations produced data crucial to five interrelated aspects of household studies:

- Plank Houses as Deposits: Household taphonomy
- Internal arrangements and the distributions of activities
- Northwest Coast architecture and house construction techniques, including construction histories
- Household social organization
- Intra and inter-household variability.

It is the ability to investigate these issues using both archaeological and ethnographic evidence (e.g. photographs) that makes the Ozette research ethnoarchaeology.

Northwest Coast sites, particularly shell middens, are notoriously complex (e.g. Stein 1992). Much of this complexity is likely due to site formation processes produced by house construction, repair, rebuilding, cleaning, and abandonment. Blukis Onat has suggested, for example, that shell middens themselves may be constructions to create flat, well-drained platforms for houses (Blukis Onat 1985). One aspect of the complexity of Northwest Coast sites is their heterogeneity. The contents of sites (artifacts, ecofacts and features) may differ markedly from one portion of a site to another, sometimes over relatively small distances. The Ozette excavations clearly demonstrate how

houses can contribute to that. There are, for example, differences between the deposits that accumulate under benches and those that develop on the earthen floors; interior deposits differ from exterior deposits. These differences appear to be the consequences of differing activities carried on in each these areas, and of differing patterns of housecleaning across a house and between dwellings.

One of the things that does emerge from a variety of the Ozette reports is the importance of floor sweeping as a site formation process in these structures. As a consequence of sweeping, house walls and wall trenches may be important artifact sinks, where objects which might otherwise end up in the middens will remain for long periods. The recognition of sweeping is not based on observations of living Northwest Coast households by ethnographers, but on the observations of Hayden and Cannon (1984) of earthen-floored Maya houses (Samuels 2001.). This suggests the more general importance of sweeping in structuring artifact assemblages forming on and in earthen-floored contexts.

The Ozette analyses are organized around a model of the interior of shed-roofed houses. The model recognizes several zones; including walls, benches, hearths and surrounding open areas. It also takes cognizance of the interior social organization of these houses by identifying residential compartments based on hearths. This framework requires modification to accommodate other interior arrangements (for example Ames et al. 1992). This model is not an inevitable consequence of the Ozette excavations: it could have been easily formulated by other researchers based on ethnographic descriptions. However, Ozette provides an opportunity to both operationalize that model (what does a bench look like archaeologically?) and to test it (what other ways of organizing data spatially would be productive?).

The Ozette work also shows how the operation of social organization and economy can structure deposits. Wessen (1982, 1988 and 1994) for example, relates the mollusks composing midden deposits to different habitats in the littoral zone in front of the villages and to the practice of different houses owning the rights to different stretches of beach. Another, striking example of these dynamics is the differences between House 2 and House 1 in the volume of faunal remains recovered from their respective floor middens, with House 2 having far more faunal elements in its floor midden. House 1, the larger, probably higher-status house, appears to have been regularly or more meticulously cleaned. Despite this cleaning, bulk processing (butchering, filleting etc.) also occurred on these house floors and the debris incorporated into the floors.

Northwest Coast architecture and construction methods have long been the subject of continuing, albeit not intensive, interest, among cultural anthropologists (e.g. Vastokas 1966). They are of increasing interest among archaeologists, for whom reconstructing the internal arrangements of houses is central in reconstructing the distributions of activities within the house and for locating the positions of persons and families of differing status and perhaps with differing economic specializations. Investigations of construction methods and construction histories provide evidence for the amount of labor invested in these dwellings and in the duration of their occupation, which provides some evidence for the life spans of the households occupying the structure (e.g. Ames 1996).

In dry sites, of course, such evidence is restricted to postholes, post molds and plank molds. Occasionally, when a structure burns, a plank is preserved. Simonsen (1973) excavated a structure on the northern British Columbia coast, for example, and recovered what appears to be a charred plank from a sleeping platform, the first direct evidence for that widespread house fitting on the northern Coast. At Ozette many of the structural members, like pegs, are still in place, and others, while overridden by the mudslide, were still present, some in their appropriate relationship to others. In some senses, the Ozette houses can be said to have been found partially articulated.

Why does this matter? The Ozette data permits archaeologists to directly link archaeological features, such as post-holes, to the objects, such as posts, that produced the features. Mauger's analysis of the planking used at Ozette (Mauger 1978, 1991), for example, provides a strong basis for saying that similar features at a dry site were also produced by a plank and to estimate its size with reliability. At Ozette, the interior edges of the sleeping platforms are marked by linear groups of small pegs and posts, which were part of the support system holding up the platforms (Samuels 1983, 1991b). At other sites, where we lack both the actual platforms and the pegs, we can, by analogy with Ozette, establish the inner position of the platforms (and their presence) by the distribution of small postholes. This is not the kind of detail that ethnographies or early paintings or photographs can give. In short, the Ozette data provides strong inferential warrants for interpreting house features. It allows us to translate ethnographic description into archaeological data, not archaeological inferences, but evidence. Why does this matter?

A central issue in Northwest Coast archaeology over the past 25 years has been the development of ranking and stratification on the coast. Much of the earlier work on the issue involved analyses of burials. While excavations of burials continues in some portions of the coast, archaeologists have turned increasingly to the analyses of houses, including their size and their contents to document the presence not only of elites, but occupational specialists and other aspects of Early Modern Northwest Coast social and economic organization. Many of the correlates of high rank used in this research have been developed using archaeological data sets from elsewhere in the world, or the Northwest Coast's ethnographic record. Ozette, of course, cannot inform us directly about the evolution of social ranking on the coast, except to demonstrate its presence on the southern coast 70 years before direct contact with Europeans. However, the site can provide warrants for inferences about broader issues, as well as the opportunity to test hypotheses developed using other data sets.

I have already mentioned Wessen's work on the differential distribution of shellfish across middens associated with different houses and the inter-house differences in the volume of debris in the floor middens of houses reflecting relative household rank (Wessen 1982, 1988, and 1994). Other analyses include explorations of the apparent relationships between high status and access to preferred foods (Huelsenbeck 1983, 1989, 1994), and the possible role of hearths as reflections of relative rank (Samuels 2001). However, I do not think the research on this topic has reached its full potential yet. Analyses of the differential distributions of different crafts within and between the houses might be quite informative. Some work in this direction has been done with baskets (e.g. Croes 1977, 1989) but much remains to be done. A key issue is to what extent can high rank actually be recognized in the archaeological record even under these exceptional conditions, and from that, what the excavator of a more typical site might expect.

Woodworking is an example of the potential. Friedman's dissertation research focused on the woods selected for different kinds of tools and uses by the site's occupants (Friedman 1975 and present volume), while Gleeson looked at the debitage of woodworking (wood chips) (Gleeson 1980 and present volume). Woodworking was perhaps the central craft, or even industry, for Northwest Coast life. It is also a craft that does leave some evidence in dry sites in the form of adzes, chisels, and other tools. Ozette provides sufficient data with which to model how woodworking was conducted, whether it was done by specialists or part-time artisans, and how the craft was structured. For example, Friedman's data could be coupled with data on the collection of terrestrial resources (hunting, collecting basketry supplies, etc), including plants (Gill 1983 and present volume) to determine the extent to which selection of wood was embedded in other activities, or needed to be conducted as a separate task. This in turn would provide insight into the demands on task organization and labor in the Ozette houses. Gleeson's data are tantalizing in that they suggest a

relationship between high rank and woodworking, a relationship that I have also noted in a Chinookan house on the Lower Columbia River (Ames 1995).

In this context, it would also be quite interesting to build on Draper's analysis of the lithic artifacts from Ozette (Draper 1989). While it is true that lithics are a tiny proportion of the artifacts found at Ozette (and that probably were present in most Northwest Coast houses) ground and chipped lithics, along with bone tools, are what most Northwest Coast archaeologists find in their sites. These data would allow investigation of the relationships between the distributions of those materials that comprise virtually all of the world's archaeological record and the patterns visible in the organic artifacts.

Northwest Coast Art and Technology

Ozette offers a unique opportunity to develop our understanding of the ethnoarchaeology of Northwest Coast. The excavations produced a significant assemblage of artifacts bearing Northwest Coast motifs (Daugherty and Friedman 1983). Northwest Coast art is best known from museum collections acquired between the late 18th century and the present (Cole 1985). Taken *in toto*, the global assemblage of Northwest Coast art is enormous. Cole (1985) estimates 500,000 objects scattered across the world. In contrast, the archaeological sample of Northwest Coast art is extremely small, especially if we limit our count to items with zoomorphic or anthropomorphic motifs. Margaret Holm's study of the entire body of zoomorphic and anthropomorphic archaeological specimens excavated in British Columbia before 1990 included 243 zoomorphic and anthropomorphic items dating to the last 4500 years (Holm 1990). Similarly, of the more than 10,000 artifacts recovered from sites in Prince Rupert Harbor, British Columbia spanning the last 5,000 years, less than 180 bear any kind of motif, and of those, only 20 have zoomorphic or anthropomorphic designs. These rare items are extremely important for our understanding of the history and evolution of Northwest Coast, particularly in terms of techniques and the chronology of particular motifs and design elements. However, our understanding of the contexts within which that history occurred is extremely limited. For example, Ames and Maschner (1999) note that zoomorphic and anthropomorphic objects can be associated with human burials on the southern coast archaeologically, while they are not on the northern coast. Only such limited statements are possible at present using artifacts from dry sites.

Ozette, and to a much lesser extent, other water-saturated sites, is our only archaeological window into the daily context of motifs, designs and motif-bearing artifacts, given that the great bulk of these items are perishable. At Ozette, it is possible to determine not only what kinds of designs and motifs are present, but also how and where they were distributed, at least within one or two houses. It is possible to establish their associations? Are there consistent patterns in the kinds of objects associated with particular motifs? Are particular motifs or designs consistently associated with other motifs and designs? Are they consistently associated with particular places in the house, or particular architectural features, or particular artifacts? Is there perishable and imperishable debris that was produced by production of the art? What are the spatial relationships between the locations of the tools used for making (carving) decorated objects and the locations of the objects themselves? As far as I know, this work has yet to be done, but the potential is clearly there.

The same comments apply to technology more generally. As the sample of excavated water-saturated sites on the coast increases, it is becoming increasingly possible to examine both the spatial and temporal distributions of certain classes of perishable artifact, including basketry (e.g. Croes 1989, Bernick 1998b) and fishing tackle to begin to write a history of some aspects of Northwest

Coast technology (e.g. Byram and Erlandson 1996, Moss and Erlandson 1998, Stevenson 1998). However, that history should not be limited to perishable artifacts.

Temporal changes in technology are the framework upon which much of Northwest Coast culture history is hung. Despite that, I suggest, we do not really have a firm grasp of the history of that technology for its own sake. We use technological changes (changes in artifact form) to infer economic or social changes, but lack a firm grounding in the spatial and temporal distributions of artifact types on the coast. We all have an idea about certain, historically significant classes of artifacts, such as microblades, but, for example, do we have as good a knowledge of the temporal and spatial distributions of different classes of harpoon heads (are bilateral barbed heads more common than unilaterally barbed ones? Do they have similar or dissimilar temporal-spatial distributions? How are harpoon valves distributed temporally and spatially? Where are they more common?)

These kinds of questions are central to developing a history of Northwest Coast technology, which in turn, is central to understanding the evolution of Northwest Coast societies. Central to any evolutionary understanding is grasping patterns of variation, which, for archaeologists, means grasping the patterns of variation among different classes of artifacts and understanding the different spatial and temporal scales at which this variation occurs. That knowledge will enable us to better understand the kinds of sorting or selective mechanisms (or drift) at work on the coast over the past 11,000 or more years. To do that, we must also better understand how the kinds of artifacts we more commonly recover, the ground and chipped stone, and bone and antler tools, articulate with the technology that was less commonly encountered, the wooden and fiber tools.

The bulk of the artifacts retrieved in Northwest Coast sites were parts of compound tools. The ubiquitous bone bipoint is a case in point. These objects may have armed toggling harpoon heads and fishhooks, studded herring rakes, been used as fish gorges, and so on. However, their distributions in archaeological sites do not correlate well with the distributions of harpoon valves and other artifacts that may have been part of fishing or sea mammal hunting gear (Raetz 1989).

What have these issues to do with Ozette? Certainly, Ozette cannot answer questions about temporal changes in technology. However, it can help us answer questions about how the objects we commonly find articulate with the rest of the technology that we do not find. Where, for example, do bone bipoints occur at Ozette? Does their distribution bear any straightforward relationship with the distribution of harpoons, of fish hooks, of baskets, or of wood chips? In what kinds of deposits are they recovered? While these may not seem to be the most pressing questions to be answered by Ozette project researchers at this moment, ultimately they are questions whose answers will materially assist archaeology on the rest of the coast by giving us a basis for linking the things we usually find with the technologies we reconstruct.

Public Archaeology and the Relationship between Native Americans and Archaeologists

These could be legitimately seen as separate topics. Native Americans are not a special subset of the general public. Indian people (or First Nations people, depending on which side of the US/Canada border this is being read) are the living descendents of the people whose lives and histories archaeologists study. There are other descendant communities but none with the special legal status or claim to the history of the entire hemisphere that Native Americans have. However, there are certain core issues that I think link the public and Native Americans, and Ozette can teach us and help us with those issues.

I was recently asked by an Indian person "how much knowledge is enough?" If one knows, for example, that a site is a village, or a cemetery, or a special use site, why do we need know more? That knowledge is enough to manage the site, and to preserve it. Why do archaeologists need to collect more information; why do they need to dig at all? His view of sites is very different from mine and that of most archaeologists. He sees sites as places to be managed, as they are, not as sources of knowledge about the past. Knowledge about the past, for him, comes from oral traditions and from his elders. Archaeological sites are manifestations of that past, but primarily as intact sites. He recognizes the importance of archaeology and history regardless to the sources of information, but for him, and his particular history, archaeology is far less important than oral traditions. It is his question, "how much knowledge is enough?" that links Public Archaeology and Native Americans, at least in this essay, and that I think Ozette can help answer.

Almost all archaeology in North America is "Public Archaeology" in the sense that it is publicly funded, either through Cultural Resource Management (CRM) or through public funding agencies (e.g. the National Science Foundation). Ozette was, in this sense, public archaeology, since much of the work was funded through the National Park Service. There is another, related, sense of Public Archaeology. Archaeologists need, nay must, make what they do publicly accessible and publicly available. This is, in part, recognition that, since the work is publicly funded, we have an obligation to bring that knowledge "home." If we do not, people will begin to wonder why they should pay for it.

However, bringing the knowledge "home" goes beyond a responsibility based on funding sources. One of the major criticisms of anthropologists and archaeologists by Indian peoples and others has been that we take information out of communities, which are often economically poor and politically weak, translate that information into academic knowledge through analysis and writing, which we then use to build careers. Our careers, while not lucrative by the standards of the broader society, are certainly much more so than what often is available locally. A similar criticism, however, is also sometimes made of us, not by native communities, but by local and often rural Euro-Americans. Farmers, ranchers, loggers, and others usually feel a very close connection to the place where they live, and one aspect of that connection has been collecting artifacts. When university or government archaeologists come and conduct projects, we remove the artifacts we find to some distant place, where it is hard to visit and see them. We often do not display them, and the information we develop is either not available or available only in dense technical reports. Many people see little difference between collectors (however they are characterized) and archaeologists, and, at least with collectors, the artifacts often stay "home".

The Ozette artifacts did stay "home". A centerpiece of the Ozette story has been the development, construction, and continuance of the Makah Culture and Research Center, where the Ozette collection is housed. Of course, the project did not itself create the Center; that was done by the Makah, for the Makah, from the Makah point of view. Other tribes, particularly those with gaming incomes, either have, are developing, or are wishing to develop, tribal interpretative centers where archaeological collections can be curated.

However, there is more to Public Archaeology than curating and displaying artifacts. One of its widely recognized purposes is educational, teaching people about both the results and the methods of archaeology. This is accomplished at the Makah center with displays, interpretative texts, and the like. It was accomplished by the extensive media coverage of the project when it was in the field. It is accomplished more broadly in archaeology through lectures, popular books, and the like (although in a recent survey commissioned by the Society for American Archaeology, a distressingly small percentage of the American public gets their information about archaeology from those sources

[Ramos and Duganne 2000]). We are not yet very successful at that, but we need to go beyond presenting our results, as valuable as they are. We need to make clear how we get those results, and why both the results and the methods are important and valuable, in short, to show how and why archaeology as a science accomplishes what it does, show why that matters and answer the question “How much knowledge is enough?”.

There are several reasons why we need to popularize archaeological methods. First, there is widespread concern about the state of science education and what is often termed “scientific literacy.” The argument (and worry) is that we live in an age in which science, scientific issues, and scientific solutions are increasingly important to public policy; we also live in a democracy, and it is crucial that the public be able to make informed public policy decisions (or be able to vote for people who can). It becomes problematic if that public is scientifically ignorant. Some (e.g. Levett 1999) argue that “scientific literacy” is not truly possible for the population at large, and argue such issues need to be left to experts. Others (e.g. Wilson 1998) maintain that something like scientific literacy is essential, that, in fact, even our ethical systems must be grounded in scientific reality about human nature.

“Scientific literacy” is often conceived as Physics, or Biochemistry, or Molecular Biology literacy. However, more humbly, and more broadly, it can be conceptualized as critical thinking. Archaeology may be uniquely placed to exemplify critical thinking (Daehnke 2002). It is enormously popular; people are fascinated by what we do, and so we have an entry to the public that other sciences may lack. The intellectual process that archaeologists go through in constructing, testing, and evaluating hypotheses is more accessible to the layperson than is, say, quantum mechanics. We propose multiple, competing ideas about the past, accumulate evidence, often fragmentary, with which to test those ideas, discarding some, holding others in abeyance, supporting others as we winnow through them. We evaluate and test the ideas, the methods used to collect and analyze the data, and the arguments made about the data and the ideas. Our evaluations of each other’s thoughts and results are critical, even skeptical. This skepticism is perhaps sharpened by our knowledge that archaeology is destructive, so a poor or stupid project is irremediable. However, the intellectual process, while often hard work, is broadly accessible.

It is also easily misunderstood and can be a source of confusion, even anger. One key source of misunderstanding between Native Americans (and others) and archaeologists is, I believe, the scientific skepticism that is central and necessary for evaluating ideas about the past. I think Indian people frequently see our often-public disagreements as rude. In societies where decision making is by consensus, and consensus is often strengthened and reinforced by deferral to the knowledge of elders, archaeological debate can be troubling and seen as disrespectful behavior. This becomes particularly complex when dealing with oral traditions, which are increasingly being put forward as legitimate sources of ideas and hypotheses about the past and as sources of evidence about the past. This is made complicated because traditions contain far more than information about the past, and it can be difficult, if not sometimes impossible, to separate the history from the other, more metaphysical content. However, when oral traditions are advanced as scientific evidence, then they must be tested and evaluated just like any other ideas. This can seem rude and disrespectful. It would be even more difficult and vexing, in the long run, to grant them immunity to this treatment.

However, it is incumbent upon us to show the value of debate, of testing, of skepticism. In a very real sense, it is necessary to demonstrate the value of intellectual processes that themselves have been developed and refined over 2500 years, and which are not always or even generally accepted throughout the dominant society. We often take these processes so much for granted that we never think about them, and so, when challenged can be at a loss to explain their value and

importance. It may be that archaeologists, even those who plan a career in local Cultural Resource Management (CRM), need to take more philosophy before leaving college so they can better explain what it is they are doing and why.

Secondly, it is important to show the value of cumulative knowledge. At the heart of the scientific enterprise is the core belief that knowledge is something requiring constant work and tending; that it is possible and good to both increase our knowledge and to improve that knowledge (accumulate more while throwing some out). It is also not a bad thing (it can be a very good thing) to be wrong. We can sometimes figure out why we are wrong; it may hard to know why we are right. These are not abstract values. They are inherent to the CRM process (e.g. Section 106), although that is not always recognized. Management decisions based on ignorance are bad decisions, bad both for the archaeological resources and for science. The key problem here is to make explicit the linkage between the continuing accumulation and testing of knowledge with good management and conservation of archaeological sites, a linkage even many archaeologists do not understand or at least appreciate.

This is not to argue that archaeologists can or should operate without limits. Society has concluded that ethical science involves limits on what can or cannot be done. These limits can be the subject of ferocious debate (i.e. use of fetal tissues). The current ongoing debate over how to treat human remains is part of the continual process of determining what is ethically appropriate and what is not. However, the fundamental point remains, that a case needs to be made for the value of cumulative knowledge and testing of that knowledge.

Third, and last, is the value of the knowledge itself. Archaeologists often appeal to the humanistic ideal that archaeological knowledge contributes to our understanding of the human condition, and to what it means to be human, and that the past of all humans is part of the universal human heritage. A loss of knowledge or the destruction of a site in one place diminishes the heritage of all. Archaeology, like history, art, literature, and physics, enriches the soul, however that is conceived. This is a very important value and it needs to be stressed. However, it is often not stressed enough. American culture generally takes a very utilitarian view of the value of knowledge, and so we are measured against many apparently and actually more practical fields and often found fascinating but irrelevant. Native Americans may respond that they know their past, thank you very much, and not only do not need whatever knowledge we may have, but that the practice by which we accumulate it is destructive of their cultures and damaging to their relationships with their dead.

It is hard to argue that archaeological knowledge has direct, utilitarian value, in the sense of developing a better wing design for passenger aircraft, or a cure for AIDS. However, archaeological evidence and an archaeological frame of reference (long time spans) have significant roles to play in, for example, environmental policy. Redman (1999) demonstrates the utility of long-term archaeological and environmental data for documenting and understanding human-environmental dynamics as essential to understanding current environmental issues. Butler and Delacourte (2001) show the same on a much more local scale for lake fish in the Great Basin and how knowledge based on archaeological samples of these fish permit a reconstruction of fish dynamics in Great Basin lakes and provide essential context for current efforts at habitat and population restoration.

It is in this kind of knowledge that the archaeological response to Native American objections can be productively based: in the development of sound, scientifically-based knowledge relevant to questions of direct and immediate concern to Native American communities. Environmental and ecological questions come immediately to mind here, but archaeology can also be an element in community building (e.g. The Makah Museum). These examples illustrate a crucial point about archaeological knowledge: its significance rests at multiple scales. Archaeology at some level is

immediate and personal. Many of us have had the experience of excavating something that clearly was the result of a single gesture or action, something where it is easy to imagine the ancient actor putting this material in the ground, or throwing it away. However, by its very nature, understanding in archaeology requires comparison of multiple samples and use of data from several different scales (site, locality, region etc.). These multiple scales allow us to link the immediacy of material remains to broader questions.

This is not to say that all archaeological research needs or should be directly in the service of Native American interests, or that Native American interests in history are exclusively or even generally limited to their own past, or that the humanistic goals of the discipline must be abandoned, or seen as an embarrassment. However, it does mean that research needs to be actively designed collaboratively when and where possible, appropriate or necessary. It is in the process of such collaboration that the archaeologist has an opportunity, even an obligation, to present and explain aspects of archaeological practice that may seem alien or offensive, such as skepticism and testing of ideas, and that “truth” is contingent. It is also in the process of such collaboration that the archaeologist has the opportunity, even the obligation, to listen and to learn as well.

These suggestions are not at all original to me. They are encoded in the legal framework within which we work and in the code of ethics that members of our professional society subscribe to. These considerations return us to Ozette. I argued in my opening remarks that Ozette’s importance rests with its human immediacy. That immediacy provides a basis for using Ozette as an instructive example for the issues raised here: how archaeologists build knowledge, how that knowledge needs to be cumulative and, the value that knowledge has in many venues, at many scales.

It is possible with the Ozette data to take a single object, a whaling harpoon, for example, and link it to a whaler, to whaling and whales, to also link it to issues of status, social organization, household economics, and regional trading systems (Huelsbeck 1988). One can go from floor laminae to housefloors to how artifacts accumulate on floors to house cleaning to differences in household status to economics and social organization (Samuels 2001). In either example, one can show how weighing of evidence, testing of multiple hypotheses, skepticism about one’s own favorite ideas and so on are required to construct our knowledge. It is also possible to show, while doing this, the importance of this knowledge to the Makah community and to our understanding of the heritage of all humanity, and that, in this, Ozette is not unique.

Summary and Conclusions:

I have argued three major points in this essay: First, that the Ozette site, because of how it formed and what it contained – recognizable houses and their contents – has a human immediacy that few archaeological sites possess. It is easy to imagine the homes of loved ones swept away by a catastrophe. This immediacy also comes from the essential dailiness of Ozette. While the site is taphonomically complex, it is possible to see the daily lives of the people who lived at the site. It is this immediacy upon which the other contributions of the site to Northwest Coast archaeology and archaeology in general rests.

Second, I have argued that the Ozette’s project’s scientific value derives from its being conceptualized as a project in ethnoarchaeology, as perhaps the only opportunity Northwest Coast archaeologists have of really integrating the coast’s archaeological and ethnographic records to answer the kinds of questions addressed by ethnoarchaeology: questions about site formation processes, material culture dynamics and so on.

Third, I suggested that Ozette’s already significant contributions to public archaeology and to

the relationships between archaeologists and Native Americans can be enhanced by using the site as an exemplar of the kinds of knowledge that archaeologists build and of the intellectual methods we use to construct that knowledge.

However, at the simplest most fundamental level, Ozette is important because it is many stories, many really good human stories.

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