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Constructing Native American Identity within the Context of the Native American Graves Protection and Repatriation Act

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Constructing Native American Identity within the Context of the Native American Graves Protection and Repatriation Act

By

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Abstract

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The Native American Graves Protection and Repatriation Act of 1990 (NAGPRA) established a mechanism for repatriating ancestral Native American human remains and three other categories of special objects already curated by federally funded institutions. NAGPRA is undoubtedly an important piece of human rights legislation recognizing the historical mistreatment of Native American dead. Yet, the process of repatriation, arguably the most salient piece of NAGPRA, highlights larger questions about the construction of tribes, as an analytic category through archaeological and ethnographic evidence. Indeed, this federal law changed the nature of the archaeo-legal landscape. Before NAGPRA, archaeological expertise was used in the context of the National Historic Preservation Act or Archaeological Resources Protection Act, for example. Post NAGPRA the political involvement of archaeology has expanded and archaeological and anthropological methods and theories now occupy a unique place in the archaeo-legal landscape. In particular, the corner stone methods and theories by which archaeologists and anthropologists link contemporary social and cultural groups to their ancestors, together commonly known as cultural affiliation, have become particularly important.

The most salient example this is Bonnichsen v. U.S., 367 F.3d 864 (9th Cir. 2004). In 2004, after almost a decade of litigation, the 9th Circuit in decided the final disposition of approximately 9,000-year-old human remains. Popularly known as “Kennewick Man” or “The Ancient One” the remains were inadvertently discovered by four students watching a boat race from the banks of the Columbia River in Kennewick, Washington. The county coroner initially identified the remains as Caucasoid. However, the discovery of an a Clovis spear point in Kennewick Man’s hip suggested this identification might not be correct. After further testing, the age of the remains were found to be approximately 9,000 years old. The Army Corps of Engineers, on whose property the remains were found, decided, based on the age of the the remains, that Kennewick Man should be repatriated to area tribes. Subsequently, a group of archaeologists and anthropologists sued claiming that the remains were so old and because the original characterization of Kennewick Man was Caucasoid, cultural affiliation to a modern tribe could not be established within the meaning of NAGPRA and thus the statute did not apply. The

central question for the court to consider was whether or not Kennewick man was a Native American.

Relying heavily on archaeological and anthropological evidence, the Court decided that Kennewick Man was not Native American. It is not surprising that the Court would draw heavily upon these disciplines. Indeed, much of the justification for their existence and importance has been their ability to tell us about ancestral pasts and cultural lineages. While archaeology has long played a key role in contributing to national narratives, in both positive and negative ways, NAGPRA paced a new emphasis on political involvement of these disciplines in defining who is Native American. As a result, their taken for granted's are called into question.

The starting point for my broader inquiry into the traditional models and methods of cultural affiliation, is a single site, CA-SJo-42. The objects excavated from CA-SJo-42 are curated at the Phoebe A. Hearst Museum of Anthropology at the University of California, Berkeley (PAHMA) and date from roughly the Late to the Historic Period (A.D. 1500-1830). CA-SJo-42 is located in a border area separating the Sacramento and the San Joaquin Valleys (The Delta) - a border that coincides with the anthropologically, linguistically, and archaeologically defined "cultural" border separating the Plains Miwok and Northern Valley Yokuts peoples. CA-SJo-42 is the ideal starting point for exploring the empirical foundations of cultural affiliation because it is a collection that the Tachi Yokuts Tribe, a federally recognized tribe, has requested be repatriated but is tenuously labeled culturally unidentifiable by the Hearst Museum. The reason for this designation points to the heart of my analysis.

Despite the best efforts of PAHMA, traditional culture area maps and archaeological typologies provide few answers to the complex interplay of people and objects their data suggest. The goal of this dissertation is to better understand the empirical footings for cultural affiliation within American archaeology. Specifically, I ask two related questions: can we discover cultural differences in the archaeological record and how might dynamic cultural interaction and between-group differences be remodeled to better understand border interactions and group identity? To do this, I address two sets of interrelated issues. First, I examine the theoretical underpinnings for cultural affiliation and examine how data were used to construct and reify a certain notion of cultural boundaries. Second, I reflect on the ways in which cultural interaction and between-group differences might best be represented. I ask how complex social relationships, especially in border areas, can be remodeled so they better incorporate and interpret the variation that often exists among archaeological, linguistic, and ethnohistoric evidence. This, I hypothesize, could provide the foundations for more nuanced archaeological and legal understandings of identity.

For All My Relations

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A dissertation is not the work of just one person. Rather, it is the result of the collective efforts of a community of scholars, friends, and family. This section is a cursory nod to the various communities who deserve more credit and praise than there are words.

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I would also like to acknowledge and thank the Tachi Yokuts Tribe, Yokuts ancestors and specifically Lalo Frano and members of the Cultural and Historical Preservation Department. Without their approval, support, and guidance, this dissertation would not have been possible and I would not have been able to learn the many lessons the process has taught. I will be forever indebted. Wanìshi!

Before entering graduate school you are told a lot of things about what to expect and what will be expected of you. Amongst the innumerable pieces of advice there is one that stands out. Intuitively you understand it, but it is nevertheless hard to comprehend; that your advisors will become your colleagues. This is very true, and if you're lucky they also become life long mentors and friends. My advisors, Kent Lightfoot and Margaret Conkey are certainly that. They deserve more thanks and gratitude thank I can convey. They have both invested an incredible amount of time and energy in my intellectual, professional, and personal development. They push me to do my best, give me feedback and advice on a range of topics, and provide space, encouragement to read widely and be curious, and find importantly, find balance in the process of intellectual growth. I cannot thank them enough for their patience with this process, with my sometimes-unconventional approach to graduate school, and for their understanding regarding my decision to enter law school.

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It is only fitting to thank my mother and father. They provided more support and inspiration than I can ever put into words. I hope to eventually "make good" on their sacrifices.

Chapter 1
Introduction

INTRODUCTION

Excavating, collecting, and analyzing Native American human remains and associated burial gifts are practices older than the United States and a cornerstone of American archaeology. The earliest recorded disinterment took place in late November of 1620. While attempting to relocate a stash of buried corn, Miles Standish and several other Pilgrims instead found several Pokanoket burials. Too curious to ignore the potential "treasures" buried with the graves, they "resolved to dig [them] up" (Philbrick 2006:61-67). Having unceremoniously done so, they left with "sundry of the prettiest things" (61-67). Although this historical moment is unconnected to the formal development of North American archaeology, it exemplifies an unsatiated curiosity about the North American continent's first peoples that underpins much of modern discipline. Particularly famous in the annals of North American archaeology is Thomas Jefferson's systematic excavation of a burial mound near his home at Monticello in 1787. His first goal was to understand who and when the mound was created (Jefferson 2002 [1781]: 147-151). Jefferson's excavation and subsequent analysis of the mounds stratigraphy earned him the unofficial title of "America's first archaeologist."

Jefferson also hoped the results of his excavation would demonstrate that Native people were, albeit slowly, moving from a state of savagery to one of civilization. This was part of Jefferson's ongoing political debate with European scholars who questioned the viability of the nascent American state. Scholars like Bufon argued that an important metric for evaluating the success of a 'civilization' was its ability to 'tame' the land. If Jefferson could show that even Native people, who were inferior to Europeans because of their "savagery" could make progress toward a civilization, then surely the newly formed American government and people would succeed.

In the 1830s, Native people and their ancestral remains' continued to be simultaneously objects of curiosity and fodder for larger political narratives. Instead of trying to connect Native people to the American continent, as Jefferson wanted, amateur and professional excavators of the early 19th century attempted to divorce them from it. If Native people were not the first inhabitants, then westward expansion and conquest was more easily justified. At the same time as westward settlers attempted to destroy Native peoples and cultures they became objects of scientific study as members of a "dying race." Both amateur curio collectors and members of the fledgling academic discipline of archaeology carried out this type of preservation. One private collector wrote Samuel Morton, a Philadelphia doctor and then leading proponent of phrenology:

> It is rather a perilous business to procure Indians' skulls in this country—the Natives are so jealous of you that they watch you very closely while you are wandering near their mausoleums and instant and sanguinary vengeance would fall upon the luckless—who would presume to interfere with the sacred relics…There is an epidemic raging among them which carries them off so fast that the cemeteries will soon lack watchers—I don't rejoice in the prospects of death of the poor creatures certainly, but then you know it will be very convenient for my purposes (qtd. in Thomas 2000:56).

By the time of the Indian Wars in the later 19th century, the national narrative was such that Native people needed to be eradicated because they were a backward people impeding American progress, but at the same time were also pitied as a dying race. Collecting and preserving Indian remains and "relics" was a way to bring synergy to these conflicting ideas. Archaeology became the tool by which anthropologists documented the erasure/eradication of Native peoples and Native cultures.

For example, in 1868 the surgeon general of the US Army requested that the heads of Native combatants be sent to him for phrenological study (Sonneborn 2007:181). The collecting of Native bodies as war trophies took on a particularly gruesome face during and after the massacre of Cheyenne and Arapaho men, women, and children at Sand Creek, Colorado. On the morning November 29, 1864 Calvary officer and Methodist minister John Chivington led an attack on a quiet encampment which resulted in the brutalization of Native bodies, specifically that of Chief White Antelope whose scalp, nose, and ears were removed, as were his testicles for future use as a tobacco pouch. In other cases, women's vaginas were worn as bloody hatbands, and heads were removed from bodies and sent to the Army Medical Museum for scientific study so that the Surgeon General could continue his "Indian Crania Study." This collection later became one of the Smithsonian Institution's founding collections (Thomas 2000:53; Mihesuah 1996:154).

United States history is littered with countless examples of both overt and clandestine acts of violence towards Native peoples and their bodies. Whether those remains were collected by sword or trowel, their study has been a cornerstone American anthropology and archaeology. Many of these acts resulted in the curation of Native American ancestral remains and grave gifts for scientific study. From the first instances of their cemeteries being desecrated, Native people voiced their opposition. Famously, in 1969 Vine Deloria Jr. penned a caustic review of cultural anthropology and archaeology. His work embodied this long history of Native American frustration and anger and was an important catalyst for change (Deloria 1969).

Perhaps the most important tangible result of the American Civil Rights and accompanying Red Power movements of the 1960s and 70s was the passage of the Native American Graves Protection and Repatriation Act (NAGPRA) in 1990 (25 U.S.C. §§ 3001-3013 [2006]). For the first time in United States history, legal protection was extended to Native American human remains and associated burial items of federally recognized Native American Tribes, Alaskan Natives, and Native Hawaiians (Hutt and McKeowan 1999; MacManamon 2000). The Act has applies only where there is a federal nexus: on federally controlled or administered lands, to federally recognized tribes, or federally funded institutions. NAGPRA cannot, for example, be applied to private lands or for the benefit of federally unrecognized tribes. For the purposes of this dissertation, I emphasize two main components. The first component established guidelines to protect Native American human remains discovered and excavated after 1990 (25 U.S.C. § 3002(c) & (d) [2006]). The second component, which is at the heart of this dissertation, created procedures for repatriating ancestral remains and three categories of associated objects (funerary objects, sacred objects, and objects of cultural patrimony) that entered federally funded institutions before 1990 to federally recognized tribes.[1]

[1] Federally recognized tribes are those tribes that have been acknowledge by the Bureau of Indian Affairs having a unique political relationship to the United States Government. With this status come benefits found in section 25 of

NAGPRA is a consequence of Native people voicing their opposition to, and demanding justice for the misdeeds leading to the curation of Native American human remains in publicly funded museums and institutions. Indeed, the passage of NAGPRA marked a turning point in the national narrative of the United States about Native people, and more specifically, it substantively changed the nature of the relationship which existed between Native people and archaeologists. NAGPRA acknowledged that the beauty and vitality of Native American cultures was not limited to the trowels edge, but rather resided in the living descendants of the people on whose backs the discipline was built.

During the final stages of the review of the Act, co-sponsor and Member of Congress Morris Udall (D, Az.), reflected eloquently, "in the larger scope of history this is a very small thing, in the smaller scope of conscience it may be the biggest thing we have ever done" (136 Con. Rec. E3484 1990). Richard West, founding Director of the National Museum of the American Indian, expanded on this idea when he stated, "repatriation is the most potent political metaphor for cultural revival that is going on at this time. Political sovereignty and cultural sovereignty are linked inextricably, because the ultimate goal of political sovereignty is the protecting of a way of life..." (qtd. in Bordewich 1996:171-172). Almost a decade after the passage of the Act during a hearing held by the Senate Committee on Indian Affairs, then Maricopa County, Az. Judge and now director of the National NAGPRA program Sherry Hutt declared NAGPRA to be "one of the most significant pieces of human rights legislation since the Bill of Rights" (qtd. in Thomas 2000:214). Indeed, the repatriation process finds support in international human rights documents such as the Universal Declaration of Human Rights, (1948); the International Covenant on Civil and Political Rights (1966); the International Covenant on Economic, Social, and Cultural Rights (1966), and the Universal Declaration on the Rights of Indigenous Peoples (2007). and may accurately be claimed as an expression of the rights and norms expressed therein (e.g., the right of self-determination, the right to culture, and freedom from discrimination). The comments by Udall and Hutt highlight both the morality and humanity that underpin the Act. Similarly, West's statement points out the political importance of the Act. Not only do Tribes have the right to decide, but because of NAGPRA, they have the ability to participate in decisions regarding their cultural heritage.

This process of consultation is the cornerstone of tribal political power in NAGPRA It is a legally mandated discussion between collections curators, anthropologists, archaeologists, and tribal representatives over how and by what means a collection might or might not be repatriated. It is also the specific legal apparatus through which tribes are able to share in the decision making process about the final disposition of the remains of their ancestors. The rapidity with which the consultation process catapulted Native voices and perspectives into conversations about repatriation had a profound and destabilizing effect on the discipline of archaeology . NAGPRA mandated that archaeologists sit face to face with the descendants of the people whose ancestors they studied. In this new dialogue, the discipline was open to more direct critiques of its practices and methods, which inspired creative retooling of codes of ethics, field practices, and even archaeological theory (e.g., Bruchac et al. 2010; Colwell-Chanthaphonh 2008;

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the U.S. Code as well as certain responsibilities. The corollary to being federal recognized, is to be "un-recognized." To be an unrecognized tribe means that legislation found in Section 25 of the U.S. Code does not apply. These tribes are similarly situated to every other citizen in the United States.

Dongoske et al. 2000; Fforde et al. 2002; Gonzalez et al. 2006; Kerber 2006; Scarre and Scarre 2006; Smith and Wobst 2005; Zimmerman 1996; Zimmerman et al. 2003). For others, this interaction and the resultant repatriation of ancestral remains from federally funded institutions to tribes was an egregious affront to academic freedom (e.g., Meighan 1992). Apart from the internal battles over the merits of NAGPRA, the Act helped open the doors of archaeology to increasing numbers of Native archaeologists. This increased participation and voice continues to press critical reevaluations of core assumptions in archaeology that are fundamental to executing NAPGRA.

Inspired by these critiques and the creative remaking of archaeological practice engendered by NAGPRA I investigate a process and category that lies at the heart of both the law and the discipline of archaeology: cultural affiliation. If a cultural connection between a NAGPRA-related collection and a federally recognized tribe is not established, no ancestral remains can be repatriated. NAGPRA defines cultural affiliation to mean that "there is a relationship of shared group identity which can be reasonably traced historically or prehistorically between a present day Indian tribe or Native Hawaiian" (25 U.S.C. § 3001[2]). Practically this means that in order for repatriation to occur, a cultural connection between past and present must be made. In the context of NAGPRA establishing this cultural connection is a two part process. First, information is assessed in light of the totality of the circumstances surrounding the acquisition of the collection (provenance) (25 U.S.C. 3003[d][2][C]). If this is not level of scrutiny cannot establish a link between ancestral remains and a present day tribe, the standard is heightened and becomes a "preponderance of evidence" (25 U.S.C. 3005[a][4]). A preponderance of evidence, is a balancing test and is established when 51% of the evidence favors one party or the other. Evidence that can be used to establish this preponderance includes, but is not limited to geographic, archaeological, anthropological, ethnohistorical, historical, and linguistic data, as well as oral traditions. If a collection can be culturally affiliated to a modern day group, by either method, then it is eligible for repatriation.

If, on the other hand, a collection cannot be culturally affiliated it is labeled "culturally unidentifiable." If the collection is designated as unidentified, one of two things may occur: the collection either remains in the possession of the museum or institution until affiliation can be established, or it may be repatriated according to Title 43 part 10.11 of the Code of Federal Regulations.[2] Section 10.11 describes the process by which a culturally unidentifiable collection may be, but do not have to be repatriated to a contemporary unrecognized tribal group claiming an ancestral relationship.

Despite the clear administrative rules describing the procedures of repatriation under NAGPRA, there is no rule outlining the process of cultural affiliation. This gap in NAGPRA exists for good reason. Legislators are not experts in making decisions about specific processes, especially processes as unique and specialized as cultural affiliation. Instead, they defer to the expertise of archaeologists and anthropologists.

First, describing and interpreting the characteristics of indigenous groups and tracing similarities and differences between them through time is indeed the raison d'être of these disciplines. In this sense, they produce expert knowledge about social categories and ethnic

[2] The Code of Federal Regulations is the administrative procedure detailing how to carry out a particular statute. It is referred to especially when the statute itself is not clear on its face.

distinctions. When it comes to determining final ownership of ancestral remains and associated burial objects for the purposes of NAGPRA, archaeological and anthropological methods and theories advance powerful technologies of categorization. Second the methods of categorization developed and used by these disciplines (e.g. artifact typologies which are discussed in more detail in Chapter 1) assume that groups, rather than individuals, are the appropriate unit of analysis. This meshes nicely with the focus of NAPGRA on "shared group identity." Third, archaeology and anthropology provide not only the methods for describing past social worlds, but also the tools for mediating and validating the same information. In this sense, these disciplines function as lawmaker and judge.

By emphasizing archaeological knowledge NAGPRA simultaneously re-situated archaeological theory, method, and practice in the political arena, and fundamentally changed the nature of the archaeo-legal landscape; a unique space where archaeology and law interact. Before NAGPRA, archaeological expertise was used with regards to procedural or administrative questions about compliance with the National Historic Preservation Act (NHPA) (16 U.S.C. *et. seq.*) or and in the prosecution of illicit antiquities dealers under the Archaeological Resources Protection Act (ARPA) (16 U.S.C. 470aa-470mm). Post NAGPRA this is still the case but because of its passage and the need to culturally affiliate collections, archaeological and anthropological methods and theories occupy a place in the archaeo-legal landscape that they did not previously occupy. What was taken for granted now has to be scrutinized. The most salient example of the central role played by archaeology and anthropology in determining the cultural affiliation of Native American ancestral remains is *Bonnichsen v. U.S.*, 367 F.3d 864 (9th Cir. 2004). In 2004, after almost a decade of litigation, the 9th Circuit in decided the final disposition of approximately 9,000 year old human remains. Popularly known as "Kennewick Man" or "The Ancient One" the remains were inadvertently discovered by four students watching a boat race from the banks of the Columbia River in Kennewick, Washington. The county coroner initially identified the remains as Caucasoid. However, the discovery of an a Clovis spear point in Kennewick Man's hip suggested this identification might not be correct. After further testing, the age of the remains were found to be approximately 9,000 years old. The Army Corps of Engineers, on whose property the remains were found, decided, based on the age of the the remains, that Kennewick Man should be repatriated to area tribes. Subsequently A group of archaeologists and anthropologists sued claiming that the remains were so old and because the original characterization of Kennewick Man was Caucasoid, cultural affiliation to a modern tribe could not be established within the meaning of NAGPRA and thus the statute did not apply. The central question for the court to consider was whether or not Kennewick man was a Native American.

The court conducted a two-part analysis to answer this question. First, the court focused on NAGPRA's definition of Native American. That definition states that Native American means one who is "of or relating to, a tribe, people, or culture that is indigenous to the United States" (25 U.S.C. §3001[9]). The court noted that the present tense "is indigenous" mattered greatly. The court then reasoned that NAGPRA required a significant relationship between the human remains and a presently existing tribe, people or culture. In other words, if Kennewick Man were going to be repatriated, it had to be shown that he shared cultural characteristics with a present day federally recognized tribe. This is a reasonable conclusion given the purpose of NAGPRA. Indeed, the goals of the statute would not be served if ancestral remains were returned to individuals bearing no relationship to the remains.

It is the second prong of the court's analysis that raises the most issues. Here, the court

analyzed the "cultural" link that might exist between The Ancient One and modern day tribes. The court gave carefully considered archaeological evidence relating to diet, trade, subsistence, settlement patterns, biological diversity, and mortuary practices of people living in the area where The Ancient One was found. In particular it focused on settlement patterns, burial patterns, and biological evidence. Archaeological settlement pattern data showed that at the time Kennewick Man was alive, populations living in the area where he was found were mostly nomadic. This evidence conflicted with what archaeologists knew about the antecedents of modern tribes - that they were sedentary. The court found that this difference conflicted with the finding of a shared relationship between The Ancient One and modern Native people. As with the settlement patterns, the archaeologists showed that burial practices were not consistent over a 9,000-year period. The court again found that the disruptions in a consistent pattern indicated that different 'cultures' moved in and out of the region rather than demonstrating changes in a single or related practice over time. Lastly, the court considered the biological evidence presented by the archaeologists. Here, measurements of The Ancient One's skull were compared with measurements taken from modern Native populations. Although the coroner who first looked at The Ancient One throughout he was "caucasoid," subsequent analysis argued he had morphological features found in Polynesian populations. In either case, his features differed from those of modern Native populations. The court again found that the evidence suggested no shared relationship between The Ancient One and modern Native people. Last, the court gave a little attention to tribal oral traditions. Instead of evaluating this evidence with the same depth and care as the archaeological evidence, the court simply announced:

> Oral accounts have been inevitably changed in context of transmission, because the traditions include myths that cannot be considered as if factual histories, because the value of such accounts is limited by concerns of authenticity, reliability, and accuracy, and because the record as a whole does not show where historical fact ends and mythic tale begins, we do not think that the oral traditions…were adequate" (*Bonnichsen* 2004: 882).

In the end, the court agreed with the archaeological analysis. Because burial and settlement patterns were discontinuous and because The Ancient One didn't look like a modern Native person, the unbroken chain of relationships needed to establish affiliation was not met and The Ancient One was not Native American for the purposes of NAGPRA. In drawing this conclusion the court did several things. First, by deferring to the archaeological analysis it entombed the idea that for Native culture to be authentic—concepts the court was unwilling to ascribe to oral tradition—it must be static and timeless. Second, the court implicitly rejected the section of NAGPRA that allows for Native knowledge to be an equally valid source of information for establishing cultural affiliation. By relying on archaeological evidence as fact, the court reified the symbolic power of archaeologists to define what it means to be Native American (Bourdieu 1994). Third, the court's decision implies that human remains found in the US that are older than 9,000 years are not Native American. This is a highly problematic position in that it (1) potentially creates a bright-line date for Native American identity, and (2) opens Native identity to an origins debate that could be used to undermine the special relationship that exists between tribes and the United States (Tsosie 2005; Waldron 2003).

To expand on the first point listed above, in relying on the archaeological evidence, the

court authenticates (in a legal sense) a mixture of three common anthropological models used to define cultural affiliation: 1) ethnolinguistic groupings; 2) culture areas; and 3) archaeological cultures (each of these models will be discussed in greater detail in Chapter 1). Each model assumes that permanence and stasis is inherent to Native American cultures. These models also posit that Native groups are discrete entities with clearly defined political and cultural boundaries, which can be defined with Linnaean-like precision (Bieder 1996; Read 1974; Trigger 1989). These three models are anthropological bedrock for linking people and objects from the past with contemporary groups and are so foundational that despite recent well founded critiques of their anachronistic methodological and theoretical underpinnings, they remain in wide use (Diaz-Andreu et al. 2005; Hodder 1982, 2001; Jones 1997; Lucy 2005; Shennan 1989; Trigger 1989; Wolf 1982). One reason for the persistence of these models is that they make a great deal of intuitive sense, are easy to use, and find concordance with contemporary understandings of Native people. Another reason is that these models are conduits projecting into the past contemporary political and legal understandings of Native American tribes as groups that occupy reservations, that have definite geopolitical boundaries akin to Nation-States, and that actively distinguish themselves from their neighbors. Until the American Civil Rights and accompanying Red Power Movement that helped to engender NAGPRA and its mandate for consultation with tribal communities, archaeology and anthropology remained largely insulated from close scrutiny.

Eric Wolf's (1982) work provides a excellent starting point for discussing the theoretical problems associated with ethnolinguistic groups, culture areas, and archaeological cultures. Indeed, his discussion highlights what is missing: the complex constraining and enabling processes that are at the core of social interactions (for example Bourdieu 1984; Barth 1969). In his analysis, Wolf uses billiard balls to describe how these different anthropological models conceive of social groups. Each ball is unique, bounded and separate from the others. When contact between balls occurs there is some transfer of force but each retains its essential characteristics. Wolf's analysis also raises four important and interrelated questions.

First, to what extent do the social and political boundaries separating one group from another have permanence and rigidity like a billiard ball? Native Californians for example, are typically labeled "complex hunter-gatherers." That is to say, they were not dependent on agriculture, did not live in one place, and their social organization, to varying degrees, was loosely hierarchal (Ames 1994; Arnold 1996; Blundell 1980; Hodder 1982; Layton 1986; Myers 1988; Peterson 1974; Sassaman 2004; Wissner 1984; Wilmsen 1973). Given worldwide research concerning the nature of hunter-gatherer social organization and boundary maintenance over the past twenty-five years, we might expect traditional models of boundary maintenance to be inadequate for describing the more permeable, fluid, and flexible cultural boundaries between these groups.

Second, there is the issue of how cultural change through time is measured. Traditional models often rely on theories of diffusion which themselves maintain notions of unchanging or "essential" cores. Although these models allow for some change, change is still presented within a zero-sum economy of cultural and social interaction. For example, as the style of projectile point of one group changes, neighboring groups respond by adopting or rejecting the new item. There is an ebb-and -flow to these models that does not quite capture they dynamism of these interactions.

Third, there is confusion regarding the types of boundaries being discussed or measured in these models. When archaeologists use diagnostic artifacts or remnants of a language or

languages to define culture areas, archaeological cultures, or ethnolinguistic groups it is not always clear what type of boundary is being measured. Do the patterns highlighted by archaeological evidence and the differences in material culture or language represent the ethnic differences we assume them to be? Might not these patterns also represent social, political, economic, marriage, or kin-group boundaries? Close analysis and careful attention needs to be paid to these subtle differences if we are going to speak knowledgeably about cultural affiliation.

Finally, traditional models simplify a great deal of data and emphasize differences or disruptions rather than continuities. That is to say, they are artificially exclusionary and homogenizing. Yet boundaries, like fields or structures in the sociological sense, are multiple and overlapping (Green and Perlman 1985; Hodder 1982; Jones 1997; Sewell 1985, 1992; Shennan 1989; Stark 1998). On the one hand, neatly defined boundaries makes volumes of information digestible and understandable, and the differences between groups appear to be clear-cut and straightforward. While homogenizing groups highlights internal consistencies, it also pushes complex interactions and blurred social boundaries to the periphery of the analytic gaze. For the purposes of NAGPRA, cultural affiliation becomes a matching game favoring easily classifiable data so that when easy or straightforward correlations cannot be made, further attempts to explore the complex social interactions are set aside in favor of expediency.

NAGPRA inserted a "real world" exigency into the discipline of archaeology that has resulted in the need to substantively address the limitations of anthropological and archaeological models for establishing cultural affiliation. At its core, this dissertation is an epistemological response to that need. In the analysis that follows I question the nature, scope, presumptions, and reliability of the interpretations about Native people inferred from the three primary models used to define cultural affiliation: 1) ethnolinguistic groupings; 2) culture areas; and 3) archaeological cultures. I do this by reanalyzing numerous extant museum collections from the Phoebe A. Hearst Museum of Anthropology at the University of California, Berkeley. Importantly, these collections represent a substantial portion of the data set originally used to create these three models. My goal is to present a different model of cultural affiliation; one that can be used within the administrative boundaries of NAGPRA, reflects complex social interactions, and avoids the pitfalls of traditional models which relegate unaffiliated collections to what I call NAGPRA purgatory, a place where unaffiliated collections are marginalized—remaining unappealing to researchers and inaccessible to descendant communities.

THE CALIFORNIA DELTA, A CASE STUDY

The starting point for my broader inquiry into cultural affiliation and the models used to establish it, is a single site, CA-SJo-42. The objects excavated from CA-SJo-42 are curated at the Phoebe A. Hearst Museum of Anthropology at the University of California, Berkeley (PAHMA) and date from roughly the Late to the Historic Period (A.D. 1500-1830). CA-SJo-42 is located in a border area separating the Sacramento and the San Joaquin Valleys (The Delta) - a border that coincides with the anthropologically, linguistically, and archaeologically defined "cultural" border separating the Plains Miwok and Northern Valley Yokuts peoples (Bennyhoff 1977). CA-SJo-42 is the ideal starting point for exploring the empirical foundations of cultural affiliation because it is a collection that the Tachi Yokuts Tribe, a federally recognized tribe, has requested be repatriated but is tenuously labeled culturally unidentifiable by the Hearst Museum. The reason this designation points to the heart of my analysis.

Despite the best efforts of PAHMA, it was unable to culturally affiliate CA-SJo-42 because the objects and stylistic patterns found within the collection did not fit neatly into the known typologies for either the Miwok or Northern Yokuts Tribes (Bennyhoff 1977; Fenenga and Heizer 1939; Fredrickson 1973, 1984; Heizer 1966, 1978; Jones and Klar 2008; Klimek 1935; Kroeber 1908, 1923, 1925, 1936; Latta 1977; Lillard, Heizer, Fenenga 1939; Merriam 1955; Schenck 1926; Schenck and Dawson 1929). One of the first resources PAHMA used to culturally affiliate CA-SJo-42 were culture area maps for the Plains Miwok and Northern Yokuts Tribes provided in the Handbook of North American Indians (Elssasser 1978; Wallace 1978) and The Ethnogeography of the Plains Miwok (Bennyhoff 1977). While the maps provided in these sources, which are often considered definitive, situate CA-SJo-42 within the cultural area of the Plains Miwok Tribe, the archaeological and ethnographic record are less clear. Close scrutiny shows that several artifacts found at CA-SJo-42 are diagnostic of the Northern Yokuts Tribe. Further investigation also showed diagnostic Plains Miwok artifacts had been found in Northern Yokuts territory (i.e., Bennyhoff 1994:69). Additionally, early Mission records and other historic documents discussing the groups occupying the Delta during the Late and Historic periods were equivocal as to who which group occupied this area of the Delta. Because there were no clear markers indicating to whom CA-SJo-42 was affiliated, the museum labeled it culturally unidentified.

The uncertainty which, in part, led to the collection being labeled unaffiliated highlights the difficulties of employing traditional models— the longevity of the intellectual paradigms underpinning archaeological models for cultural affiliation which do not easily accommodate variation. Determining the cultural affiliation of a museum collection is, at the very best, highly technical and laborious process. At worst, it is a politically contentious Gordian Knot of fragmentary provenience information, false presumptions based in unquestioned paradigms, and contemporary tribal politics.[3] Yet, it is precisely because of these difficulties and challenges that I found the opportunity to creatively reexamine not only the nature of this cultural boundary, but also the broader issue of how cultural affiliation is determined.

The goal of this dissertation is to better understand the empirical footings for cultural affiliation within American archaeology. Specifically, I ask two related questions: c*an we discover cultural differences in the archaeological record* and *how might dynamic cultural interaction and between-group differences be remodeled to better understand border interactions and group identity?* To do this, I address two sets of interrelated issues. First, I examine the theoretical underpinnings for cultural affiliation and examine how data were used to construct and reify a certain notion of cultural boundaries. Second, I reflect on the ways in which cultural interaction and between-group differences might best be represented. I ask how complex social relationships, especially in border areas, can be remodeled so they better incorporate and interpret the variation that often exists among archaeological, linguistic, and ethnohistoric evidence. This, I hypothesize, could provide the foundations for more nuanced archaeological and potentially legal interpretations of identity. Recent studies in the archaeology of California by Dillion (2002), Groza (2002), Jackson (1986), King (1974), and Shackley (2001) exemplify

[3] Arguably a Tribe could use evidence for a determination of cultural affiliation to establish or expand the boundaries their traditional territory, which may in turn help or hinder federal recognition claims of other groups, or in extreme cases the information might be used to argue for the citing of gaming facilities.

these ideas through their exploration of the specific ways the Native California landscape was dynamic and highlight that cultural interaction took place locally as well as across extensive regions.

Additionally, this dissertation tests four commonly employed boundary models through close analysis of four artifact types that have implications for examining the relationship between style and identity: shell beads, basket impressed baked clay cooking balls, obsidian chipped stone objects, and engraved bird bone ear tubes. Throughout this dissertation, I employ archaeological, ethnographic, linguistic, and oral traditions to map out the nature of the Plains Miwok/ Northern Yokuts border area in an effort to determine to whom CA-SJo-42 is affiliated.

Robert Hitchcock and Lawence Bartram wrote, "In the best of all possible worlds, we would be excavating fences" (Hitchcock and Bartram 1998:13). Certainly, it would be convenient to think of cultural borders as fixed in time and space. Yet, the available evidence suggests this is not typically the case. In this case, complexity is more convincing than simplicity.

This complexity of interactions is best explained by prioritizing *relations*. This concept is an especially fruitful one. It is an idea borrowed from the work of Winona La Duke (1999) and Lalo Franco, Tribal Historic Preservation Officer for the Tachi Yokuts Tribe, as well as from my own experiences. This idea accepts that intermarriage, kinship, and ceremony among other factors were foundational in shaping everyday life. This concept frames border areas not as places that limited and constrained affiliations, but rather zones of interaction. The works of Marcel Mauss' *The Gift* (1990), Anthony Cohen's *The Symbolic Construction of Community* (1985), and Fredrick Barth's *Ethnic Groups and Boundaries* (1969), also make this point eloquently. As Barth points out, we should study boundaries for their *meanings* and not necessarily their *forms*. Or, to paraphrase James Clifford (1986), the cultural units we bound are temporal, emergent, and variously contested. With this in mind, boundaries are the unfolding traces of people's daily practices, not hard lines of difference.

DISSERTATION ORGANIZATION

CHAPTER 2: THEORETICAL AND EMPIRICAL PUZZLE

In the first part of this chapter I discuss the theoretical assumptions that undergird anthropological and archaeological models of the culture area and archaeological culture. I provide a survey of the intellectual history of these models, points of conceptual overlap, the assumptions upon which they are based, and the limitations of their use. When appropriate, I draw the reader's attention to the particular place of California archaeology in this broader narrative. In beginning my analysis with a discussion of culture areas and archaeological cultures I am better able to demonstrate why the empirical evidence from CA-SJo-42, and the California Delta more broadly, are so difficult to cultural affiliate using traditional models and paradigms. The implications of this analysis are discussed in the concluding chapter.

CHAPTER 3: BEADS AND BOUNDARIES

In this chapter I turn my attention to the first of four types of artifacts analyzed in this dissertation: *Olivella biplicata* and clam shell beads. Shell beads are ubiquitous within the archaeological record of California and are the focus of a great deal of archaeological attention.

Numerous studies demonstrate that stylistic changes in shells beads vary consistently over time. Indeed, California archaeological interest in shell beads has been dominated by an interest in refining typologies that fit individual bead types into an increasingly accurate chronological sequence. My primary interest in shell beads is not to construct or refine existing chronologies, but rather to use the different types of beads to reconstruct social boundaries. In this chapter, I map the distribution of different types of beads and demonstrate that particular beads indicate unique social relationships and choices.

CHAPTER 4: OBSIDIAN IN THE DELTA

The second class of artifact I analyze are those made from obsidian; in particular I focus on "Stockton Curves." Generally, obsidian artifacts are some of the most well studied artifacts found in the California archaeological record. In particular however, Stockton Curves, unique objects shaped into a form that resembles a bear's claw, are understudied in the archaeological literature, but reveal a great deal about social relationships. In this chapter I begin with a broadly framed review of current interpretations of obsidian objects in California. As part of this discussion I evaluate in what ways different obsidian objects have been used as indicators of group difference. In the second part of the chapter I present the results of energy dispersive x-ray florescence (EDXRF) analysis conducted on two categories of obsidian object: utilitarian objects composed of whole and partial projectile points and production flakes, and non-utilitarian objects limited to Stockton curves. Pairing my EDXRF analysis with the ethnographic record suggests there was bi-directional flow of raw materials and ideas that can be associated with obsidian in the Delta.

CHAPTER 5: THE BASKET PALIMPSEST

To date, there has never been a close analysis of basket impressed baked clay cooking balls, which are unique to the Delta region. While there are three main studies that map the distribution and create basic typologies of stylistic difference for baked clay cooking balls, none analyzes basket impressions found on many of them. In this chapter I review the three previous studies of baked clay in the Delta Region, develop a unique methodology for analyzing basket impressed baked clay, and show that far from being useless, these common items provide key insights into the cultural and social relationships constructed between individuals and communities?

CHAPTER 6: ENGRAVED BIRD BONE EAR TUBES

Like the basked impressed baked clay, engraved bird bone ear tubes are unique in their ability to illuminate social relationships, yet they are understudied. The ethnographic and historical record suggests these objects were used similarly to contemporary earrings by men and wealthy women. To date, only James Bennyhoff has attempted to interpret them, but the substance of his analysis was never published. In this chapter, I reanalyze most of the same objects and for the first time recreate his typological categorization. I discuss these findings in light of ethnographic and historical data and explore the relationship between engraving style and group affiliation. I conclude that while Bennyhoff's bird bone tube typology was intricate, his methodological approach did not yield the findings he claimed.

CHAPTER 7: CONCLUSIONS AND IMPLICATIONS

The final chapter provides a summary of what California archaeologists know about social relationships in the Sacramento Delta area, what they do not know, and what gaps this dissertation fills by exploring social interaction through exchange. I consolidate my findings from the four data chapters and test them against five boundary models I proposed in chapter one. I provide a new model for interpreting social and cultural interaction in border areas based on the more fluid and open concept of *relations*. Finally, I discuss the implications of this research within the contemporary sociopolitical climate of twenty-first century archaeo-legal practice.

Chapter 2:
Theoretical and Empirical Problems

INTRODUCTION

In the preface I briefly discussed an important point of intersection between archaeology and law: the cultural affiliation process enumerated in the Native American Graves Protection and Repatriation Act (NAGPRA). I also introduced the culture area and an archaeological culture. In this chapter I provide a more complete discussion of these two concepts and add a discussion of the ethnolinguistic group. I review the constituent elements and the relationships between them. I also discuss the steps traditionally taken by archaeologists to culturally affiliate and assign ethnic identities to archaeological materials. I will demonstrate that anthropological culture areas and archaeological cultures are linked through the ethnolinguistic unit. In part, it is this link that allows archaeologists to use material remains to attribute and derive ethnic identities. The following discussion will highlight the broader theoretical problem of cultural boundary making in archaeology. It will also direct the reader's attention to one possible explanation for why these models do not work and why the Phoebe A. Hearst Museum of Anthropology (PAHMA) could not affiliate the human remains and associated burial objects from CA-SJo-42 to a descendant community.

Tracing the particular histories and origins of objects from the present into the past to assign ethnic and cultural identities to those objects and the territories they come from has and continues to be one of, if not the most important, interpretative tasks undertaken by archaeologists (Jones 1997:1). Impetus for this work can, in large part, be linked to the shared history of archaeology with late nineteenth century nation building (Beider 1996; Trigger 1989). As Trigger (1989) deftly notes, the discipline's early methods and theories are closely tied to two key ideas: 1) that ethnic differences and similarities are reflected in the patterned distribution of archaeological materials, and 2) that by tracing the stylistic history of certain types of artifacts from the present to the distant past archaeologists can link contemporary ethnic groups to ethnic groups in the past.

In the middle to late nineteenth century when the United States struggled to create a unique national identity, archaeological materials and interpretations played an important role. The infamous Mound Builder debate, for example, championed the impossibility that Native American ancestors were the architects of the great mound sites dotting the American landscape. Mounds were thought to be the monuments of great and mythical lost civilizations—the ten tribes of Israel for example—whose architects were driven off by the "savage" ancestors of then contemporary Native Americans. Variations of this idea remained influential until almost the turn of the 20th century. Additionally, this interpretation created an image of United States history that stretched deep into a mythical and glorious past and provided a justification for denying Native claims to land by denying them access to the principal of the right of first occupancy. If the Native people living in the United States in the 19th Century were not really the first inhabitants, then westward expansion and military conquest had both a moral and legal foothold.

While our understanding of the history of the relationship between contemporary and ancestral Native American people has changed greatly since the Mound Builder debate, many of the early methods developed for establishing historical and cultural links between people in the present with those of the past, as well as demarcating their ancestral territories, are still used today. In particular, I reference the specialized systems of classification called culture areas,

archaeological cultures, and ethnolinguistic groups. These systems were developed for specific use in cultural anthropology, archaeology, and linguistics. They share eight key premises:

1. Each unit of analysis has a temporal and geographic component. Their boundaries can be marked on a landscape for a particular time-period.
2. The extent to which groups of traits or diagnostic types are distributed across a landscape mark boundaries. For example, an ethnographic unit would be defined by the presence or absence of particular words, word endings or phrases. Likewise, a culture area can be defined by aggregations of types of objects, social practices, ceremonies, and life ways such as food gathering techniques. The geographic distribution of these characteristics corresponds to the territorial limits of the unit.
3. Key traits or diagnostic types are individual artifacts or words, for example, with repeated similarities found within a given geographic and temporal range.
4. Each diagnostic type or trait has its own stylistic genealogy, or family tree, that can be traced across space and through time. This is usually done by progressing from the known to the unknown, or from present to past.
5. When stylistic changes occur in a trait, one of two things occurs: either the change is representative of a normal historical development (temporal), or the change is substantial enough to represent a new trait (geographic).[1] If the latter is true and an adequate number of new traits are encountered, then one is within the confines of a new ethnolinguistic group, culture area, or archaeological culture.
6. Because stylistic genealogies can be mapped, long-term changes and historical links can be drawn between very old objects and more contemporary ones.
7. Culture is epiphenomenal. In this case, changes in a style precede changes in culture and culture is a proxy for ethnicity.
8. By tracing stylistic genealogies, one can link contemporary ethnic groups with their antecedents. Ethnolinguistic groups, culture areas, and archaeological cultures from the past can be given the same name as contemporary groups and thus be considered ancestral to the modern group.

While specific methods used to describe culture areas, archaeological cultures, and ethnolinguistic groups vary, the goals of each are similar: to trace and explain the historical and cultural connections within and between contemporary ethnic groups into the past, to find the ancestral territories of contemporary groups, and to explain the relationships that existed between groups. In short, each of these classificatory systems provides a mechanism to describe the temporal and spatial histories of groups of people in the past and give those groups names that have contemporary meaning.

These foundational organizational frameworks and the assumptions that undergird them were developed with a keen eye toward a Linnean system of classification and Darwinian evolutionary theory. Culture areas, archaeological cultures, and ethnolinguistic groups, each posit a telescoping hierarchal scalar organizational structure like a Matryoshka doll. The history of each unit (or an individual doll within a set) and the relationships between them (all the dolls in the set), as well as between different groups (sets of dolls), are organized similarly to familial genealogies. In the case of archaeological materials, there are certain artifacts with unique styles

[1] Deciding what the adequate number of traits were to signal the movement from one culture area to another was usually based upon aggregate evidence. Sometimes statistical regressions were used (e.g., Klimek 1935)

or groups of styles that are patterned and repeated through time and across a geographic space that archaeologists assume reveals an inherent relationship among them. These objects are often called "diagnostic types" and are used as emblems of, and markers for, culture areas, archaeological cultures, or ethnolinguistic units. The distributions of these diagnostic types across a landscape are used to demarcate social groups. In central California, diagnostic types include, for example, *Olivella* shell beads, projectile points, and engraved bird bone tubes (these objects are discussed in Chapters two, three, and five). It is from diagnostic types that the histories of culture areas, archaeological cultures, or ethnolinguistic groups are built.[2] It is also through diagnostic types that cultural and ethnic labels are given to these organizational units. Using Linneaus' hierarchal system of classification and Darwin's ideas about the relationships between different data, North American archaeologists claim to be able to assign ethnic identities and trace the genealogies of objects as well as define the ancestral territories and boundaries of contemporary Native American people by mapping the distribution of these artifacts.

This organizational schema is one of the epistemological foundations of archaeology and is often relied upon by contemporary archaeologists to frame discussions and organize both simple and complex interpretations (Kossina 1911; Childe 2009). This schema is also the subject of some of the most serious recent theoretical and epistemological critiques in archaeology (e.g., Diaz-Andreu et al. 2005; Hodder 1982, 2001; Johnson 2000; Lucy 2005; Orser 2001; Shennan 1989; Trigger 1989; Willey and Sabloff 1974).

The continued uncritical use of culture areas, archaeological cultures, and ethnolinguistic units creates four fundamental problems. These problems are emphasized by the requirements of NAGPRA. The first concern is the analytically imprecise use of culture. In the framework I briefly outlined above there is confusion about whether culture means a society, an ethnic group, a group of artifacts, or a system of shared values. The second problem concerns how social groupings are temporally and spatially organized and perceived. Culture areas and archaeological cultures for example paint an image of groups neatly bounded, internally consistent and homogeneous. They are inadequate non-agentic models of human action and ignore the interplay between overlapping social structures, fluid boundaries, the existence of different types of boundaries, and human agency. Third, change is under-theorized. Change may be understood to occur, but the ideas undergirding culture areas, archaeological cultures, and ethnolinguistic groups do not fully account for changes in social interaction or cultural or group formation and maintenance over time. Rather, they project the Westphalian model of the nation-state into the past in ways that seem empirically and epistemologically reasonable. In doing so boundaries seem to be fixed in time and space in the ways similar to modern geopolitical boundaries that change little over time and only under dramatic circumstances (e.g., Alvarez 1995; Kearney 1991; Wilson and Donnan 1998). This monolithic view of cultural groups provides a synchronic snapshot of social worlds in the past and inherently limits what and how we understand social group interactions their histories. There is also a related problem to under theorizing change; that boundaries between groups are seen as clearly marked and discernible in the archaeological record. The boundaries separating groups have traditionally been demarcated by dramatic breaks or interruptions in the patterned similarities and differences found in constellations of diagnostic objects. Refashioning an understanding of change calls these premises into question. Fourth, it is not always clear what type of boundary is being discussed by these models or even if the

[2] This process generally ignores the problem highlighted in Schiffer (1996). That is culture areas, ethnolinguistic units, and archaeological cultures are defined by what can be discovered and analyzed. These remains, linguistic or material, may or may not have been important for marking differences between the people who used them.

particular boundary being discussed has been located. While archaeologists speak about cultural boundaries often it is not clear whether they are referring to cultural, social, or political boundaries.

While some authors explored these problems in the context of the discipline of archaeology, few have questioned the consequences of the continued use of these frameworks within the context of cultural affiliation and NAGPRA. I argue that continuing to circumscribe groups in ways similar to our anthropological predecessors is conceptually inadequate, practically inappropriate, and philosophically and empirically untenable. Given what we currently know about the dynamics of societies in the past and present we should be thinking of border areas as constellations of arrays or trajectories of people interacting on and in a landscape of the past, not homogenous groups with entrenched boundaries (Barth 1969; Cohen 1985; Green and Pearlman 1996; Hodder 1982; Sackett 1982; Shennan 1989; Stark 1990; Wiessner 1983). The remainder of this chapter is a discussion of the theoretical and empirical puzzles at the heart of why CA-SJo-42 was labeled culturally unidentifiable.

THEORETICAL PUZZLE

CULTURE AREAS

The culture area is an original anthropological contribution to the study of human societies. They are geographically defined regions circumscribing groups of societies that share common cultural traits. Culture areas, wrote Kroeber, "...consist of a number of politically independent tribes spread over a considerable territory and evincing a fairly fundamental similarity of customs and institutions" (1923:295). These traits included, but were not limited to, material objects such as common tools; dance regalia; fine arts including pottery and rock art; and social traits such as particular dances, ceremonies, languages, and traditions (Wissler 1917; Kroeber 1925, 1936). While there was some variation between these shared traits within a give culture area, in the aggregate, objects were thought to be more similar to one another than to the diagnostic traits of neighboring culture areas. Thus one culture area could be distinguished from another by following the distribution of typical or diagnostic traits across the landscape. As the similarities across and between traits diminished and new ones appeared, the researcher could assume the presence of a new culture area (Kroeber 1923:294).[3] The culture area concept assumes that all the social groups living within a particular culture area are, in one way or another, culturally related. The maps representing culture areas meld together vast amounts of data into a comprehensible and simplified representation of the limits of a many social worlds. This process for synthesizing cultural data, however, has come to implicitly structure our historical imagination about the people in the past we study. Culture areas normalize our vision of how people interacted and formalize the boundaries between social groups to such an extent that they become natural and self-evident. These maps even influence interpretations of the degree to which societies seem similar or different, and even how we think about what culture is and what it represents.

[3] Diffusion was the dominant model used to explain stylistic changes in artifacts in both time and space. Scholars assumed that an idea, technology, or style developed in one place, then through cultural contact, war, trade, etc., that trait spread. Furthermore, the greater the distance a trait was from its developmental place of origin, the less likely it was to resemble its original form and the further in time it was from when it was first developed.

The origins of the culture area can be traced to two key individuals. Culture areas draw from Corolus Linnaeus (1738) and his framework for classifying living things and their relatedness to one another (Trigger 1989) though the culture area concept is indebted to Franz Boas' desire to reorient the ideas structuring museum exhibits from a progressive evolutionary approach to one that emphasized cultural relativism (Stocking 1974). But it was Boas' student, Alfred Kroeber, who most fully developed the idea of the culture area in a series of influential publications (i.e., Kroeber 1925, 1931, 1936, 1939). For Kroeber, a culture area was more than just a way to organize museum exhibits or Native American cultures and artifacts (see Kroeber's (1939:4) critique of Clark Wissler's (1917) formulation of the culture area). For Kroeber, the culture area was the foundation from which interpretations about the historical development of Native societies and cultures could be made. Culture areas were seen as natural and preexisting windows into the pre-European past that were being exposed, not created by researcher (Kroeber 1936; Milliken 2007:51).

Kroeber's model of the culture area drew explicitly from the biological taxonomic system developed by Linneaus, and, later as he developed the idea further, used statistical modeling to provide empirical evidence for what he viewed as more "subjective" claims (Klimek 1935; Kroeber 1923, 1935). In Kroeber's framework the broadest level of organization was the American continent (comparable to phylum). He divided the continent into fourteen culture areas or classes. North America (Figure 2.1), for example, was contained ten of the fourteen American culture areas; the (1) Arctic, (2) The Northwest, (3) California/ Great Basin, (4) Plateau, (5) Mackenzie-Yukon, (6) Plains, (7) Northeast, (8) Southeast, (9) Southwest and (10) Mexico (Kroeber 1923:336).

Culture areas were further divided into subareas (order or family according to the taxonomic scheme, but Kroeber is not specific about which one) (Kroeber 1923:295). For example, Kroeber divided California into five subareas (Figure 2.2): the Northeast, Northwest, Central, Southern, and Lower Colorado (Kroeber 1936:102). Last, within each subarea, Kroeber demarcated the territories of individual tribes (species according to the taxonomic schema). But unlike the larger geographic areas that were defined by cultural traits derived from physical objects, these smaller areas were demarcated using linguistic characteristics (Hughes 1992; Klimek 1935; Kroeber 1935; Silliman 1996).

California deviates slightly from this schema. Instead of tribes, Kroeber argued that California native people were organized into political units called tribelets. Tribelets were nonpolitical landowning groups of 100 to 500 people, similar to ethnic nationalities, connected to one another through kinship and language for example (Kroeber 1925). Because there were so many (Kroeber estimated about 500) determining their boundaries would be difficult given the limitations culture area concept. Kroeber felt the data regarding tribelets were too incomplete, the labor required to make the data complete too great, and the significance of making inquiries at such a small scale of such little significance to the discipline of anthropology that it was not worth doing (Kroeber 1936:101).

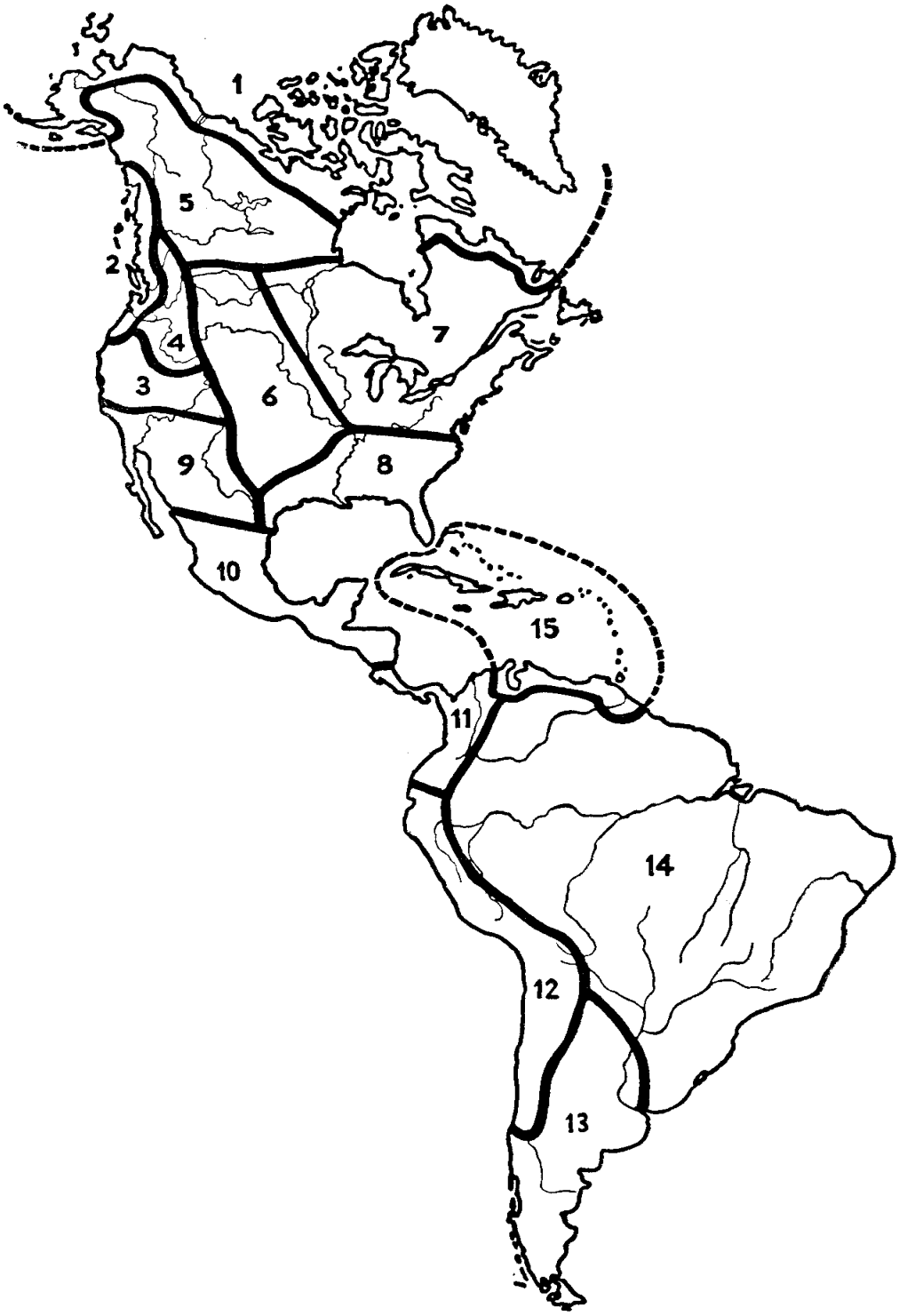

Figure 2.1 Kroeber's culture areas of North America. After (Kroeber 1923:337).

Figure 2.2 Cultural sub-areas of California (heavy borders) and tribal territories (thin borders).

Although not formally part of his organizational schema, Kroeber had two final elements - one geographic, the other temporal. The first was the cultural center or hearth. He defined this as a place with "...the greatest radiation of cultural material has taken place in the area" (Kroeber 1939:226). Cultural centers were areas, usually tribal in scale, which had the most developed and greatest number of cultural traits that seemed to influence surrounding areas. For Kroeber, the goal of anthropology was to describe the "historical process" of mankind. Thus, when one area

influenced another that influence needed to be measured through time. To measure this influence Kroeber developed the idea of a cultural climax. The climax was a period of renaissance for a cultural center. It was period in which a center exhibited the most influence on surrounding areas. There could be multiple and overlapping climaxes within any given subarea (Kroeber 1936:102).

Archaeological Taxonomy and Culture

Archaeological taxonomies are similar to culture areas in that they attempt to temporally and spatially organize data. But whereas culture areas tend to focus on ethnographic data, archaeological taxonomies focus on material remains; and where culture areas are broader in scale, archaeological taxonomies tend to emphasize regional analysis. Willey and Phillips' (1958) *Method and Theory in American Archaeology* was the first codification of the archaeological taxonomy. In it, they organized and integrated previously disparate taxonomies for classifying observed changes in the archaeological record by integrating time and space. They did this through the horizon concept, an idea originally developed in McKern's Midwestern Taxonomic System (McKern 1939; Swartz 1996). Phillips also had the goal of distinguishing archaeological from cultural anthropological methods while at the same time maintaining a common purpose - the explanation of human societies (Willey and Phillips 2001:3).

The Willey and Phillips taxonomy had several different scales of geographic analysis. Progressing from large to small, it included areas, regions, localities, and sites. The area was the largest unit of analysis, akin to Kroeber's culture area, and the site the smallest and most basic unit of analysis (Willey and Phillips 2001:18). Of all these units, the most pertinent to this dissertation is the region. A region is not as small as a site, and not as large as a locality or area. Instead, it is "...roughly equivalent to the space that might be occupied by a...'tribe' or 'society' ... it is a geographical space in which at a given time, a high degree of cultural homogeneity may be expected but not counted on" (20). Willey and Phillips also divided their schema temporally by components, focuses and phases. They defined a component in relationship to a phase. The phase is "an archaeological unit possessing traits sufficiently characteristic to distinguish it from all other units similarly conceived...spatially limited to the order of magnitude of a locality or region and chronologically limited to a relatively brief interval of time" (22). The authors employed temporal units of analysis. These temporal divisions consisted of local and regional sequences. A local sequence was a "chronological series of components, phases, or subphases, within the geographical limits of a locality..." A regional sequence exhibited the same characteristics but was organized at the regional scale (24-25). Lastly, they integrated time and space, using the concept of a horizon. A horizon is "primarily spatial continuity represented by cultural traits and assemblages whose nature and mode of occurrence permit the assumption of a broad and rapid spread" (33).

All archaeological taxonomic units—whether geographic, (region, locality, area), temporal (sequences), or integrative (horizons)—phylogenetically describe and order the development and relationship of and between artifact types in their areal and temporal distributions in a coherent and socially meaningful way. Archaeological taxonomies share many characteristics with cultural anthropological ones and may even be argued to be derivate of them. However, distinguishing between archaeological and cultural anthropological taxonomies is important for two reasons. First, there is a difference in the direction of analysis. For example, starting from the point of view of a region, a geographic area common to both archaeology and

cultural anthropology, a culture anthropologist would have directed his/ her gaze outward, searching for broader connections and interrelationships in the ethnographic literature. An archaeological taxonomy, on the other hand, focuses on closer connections between objects. Archaeologists look more carefully at the connections between things, people, and places at a much smaller scale. As the scale increases the more 'course grained the resolution.'

The second point of distinction has to do with monitoring and explaining change through time. Traditionally in anthropology the mechanism used to account for culture change was diffusion. Although the manner by which diffusion accounts for change is problematic, it nevertheless accepts that culture and thus culture areas were not completely internally cohesive and static (Kroeber 1923, 1936; Lyman and O'Brian 2003). Archaeological taxonomic categories recognize change too, but it is harder to discern. For example, in the Central California Taxonomic System (CCTS), a taxonomy particular to central California, styles or patterns and their distributions change over time, however, the district boundaries in which those patterns were found did not. As a result, boundaries appear to be static. This was, as Fredrickson (1994) pointed out, due in part to the influence of the culture climax idea in California archaeology. The fascination with the climax, as an area from which things and ideas spread, overemphasized the 'center' or 'core' and de-emphasized the periphery (Fredrickson 1994:25-27). The consequence of this, Frederickson notes, is that borders are seen as less important for understanding culture change than cultural cores. The marginalization of borders has its own effect on the interpretation of archaeological remains: it "obscures cultural processes, some of which may be unique to marginal or border regions and some of which may strongly influence the course of development of the climax cultures" (Fredrickson qtd. in Hughes 1994:27).

Just as having a basic understanding of how taxonomic units and culture areas are essential to deciphering how ethnic identities are constructed from archaeological remains, so to is an understanding of archaeological cultures. I argue that archaeological cultures conceptually bridge culture areas and archaeological taxonomies. Consequently, archaeologists have come to identify and name constellations of traits in a space-time framework as "cultures": "The Beaker People," "The Mousterian," or the "Olmec" being classic examples of archaeological cultures. V. Gordon Childe's canonical definition demonstrates this well. He posits:

> We find certain types of remains - pots, implements, ornaments, burial rites and house forms - constantly recurring together. Such a complex of associated traits we shall terms a "cultural group" or just a "culture." We assume that such a complex is the material expression of what today would be called a "people" (Childe 1929:v-vi).

As presented by Childe, and often used in archaeology, the archaeological culture is both a unit of analysis as well as a thing to be analyzed; it both organizes and is organized by the data (Gamble 2001:57). This results in an analytically imprecise tool that confuses a culture area, archaeological taxonomy, ideas of culture, and negatively affects ones ability to cogently understand the relationship between people, things, and identity.

PROBLEM AREAS

The term culture has been employed in a variety of ways in archaeology. It has been used to denote a society, ethnic group, group of artifacts, or in a sociological sense is used as a descriptive term for the rules, values, mechanisms people use to interact (e.g., Bourdieu 1984;

Derne 1994; Geertz 1993; Swidler 1986). In this next section I highlight a few theoretical issues associated with culture areas and archaeological cultures as they relate to the process of cultural affiliation. The first problem has to do with the analytically imprecise use of culture. Second, is the assumption that culture areas and archaeological cultures are neatly bounded, demonstrate a measurable degree of homogeneity, and do not change through time. Last, if cultural boundaries do divide one group from another, they are not easily inferred from material remains.

Analytical uses of the "Culture" concept

Culture as Society or Ethnic Group or Tribe. Archaeological cultures are often equated to social groups such as a tribe or nationality. This tendency in archaeology can be traced to Kossina and his idea of Kulturkreis (Lucy 2005; Shennan 1989; Stark 1998; Trigger 1989; Hodder 1982; Willey and Sabloff 1974). Kossina (1911 in Jones 1997:16) wrote, "sharply delineated archaeological culture areas coincide with clearly recognizable peoples or tribes." Becuase Kossina's goals were explicitly nationalistic and his conclusions later used to argue for the racial superiority of the German State after World War I, the impact and longevity of his ideas in archaeology are all the more troubling. Drawing on the work of Kossina, V.G. Childe presented a similar conclusion, but without the nationalistic or racial overtones. He wrote that an archaeological culture was composed of, "certain types of remains - pots, implements, ornaments, burial practices, house forms - constantly recurring together...Such a complex of associated traits we shall terms a 'cultural group' or just a 'culture'. We assume that such a complex is the material expression of what today would be called a 'people'" (Childe 1929:v-vi). Archaeological cultures, standing for ethnic groups, are treated as if they were independent entities, acting on a historical stage (Shennan 1989:11). This proposition is problematic for two reasons. First, it does not recognize that archaeological entity created by a distribution of artifacts may or may not represent a pattern that was socially relevant to the people who created those artifacts. Second, the distribution of the artifacts being used to denote different ethnic groups could be the product of several different factors, such as kinship, marriage, or ceremonial relationships, not just ethnic differences.

Culture as a Group of Artifacts. The second most common way culture is used in archaeology is as a category for a group of artifacts, for example, "The Beaker People." In this framing, the Beaker People are designated as a single "culture" linked throughout Western Europe based of the similarities in the formal attributes of their pottery (Figure 2.3). This method of grouping people and labeling them a culture is problematic. At the very least, there is no indication that the people who made beaker-style pottery shared and cultural values, language, daily practices, or dispositions (Swidler 1986).

Figure 2.3 Distribution of Beaker Culture in Europe.

CULTURE AS SHARED NORMS AND VALUES. The third way culture is used in archaeology is in its anthropological sense. One of the most common and widely accepted anthropological definitions for culture is provided by Kroeber and Parsons (1958). They "suggest that it is useful to define the concept of culture…[as] transmitted and created content and patterns of values, ideas, and other symbolic-meaningful systems as factors in the shaping of human behavior and the artifacts produced through behavior (Kroeber and Parsons 1958:583). While this is a reasonable if dated definition of culture, it is questionable whether or not it has had any influence on archaeological practice or theoretical inquiry. The continued used of such a definition results in a focus upon the outcomes of cultural action rather than the mechanisms by which culture is created and reproduced (Joyce *pers comm.* 2008). What she points to is that explaining the outcomes of cultural action tends to be the focus of archaeology rather than exploring the specific mechanisms by which culture operates. While it may be true that archaeology has little to say about culture if only because of the limitations of the available data, trying to understand it and its influence on the discipline is not a wasted endeavor. Indeed, if one is to understand and fully explore the inner workings and limitations of culture areas and archaeological cultures the theories of culture that serve as their rationale cannot be ignored.

In this case, a 'normative' view of culture lent credence to the culture area. Simply put, the normative view understands culture to be a set of shared traits or 'norms.' What this means is that cultures are thought to be relatively homogenous and easily differentiated from neighboring cultures. A limitation to this framing is that there is not adequate mechanism to explain change. Although 'diffusion' was the most common explanation for the transmission of cultural traits from one group to another, usually dominant to less dominate, or advanced to less advanced are inadequate, people in this model are "cultural dopes." They unthinking automaton reacting to external stimuli and not active participants (Swidler 1986).

BOUNDED, HOMOGENEOUS, AND STATIC

Several authors point out that culture areas and archaeological taxonomy, underpinned by a normative view of culture frame social groups are bounded and homogenous in space and time (e.g., Diaz-Andreu et al. 2005; Hodder 1982, 2001; Johnson 2000; Lucy 2005; Orser 2001; Shennan 1989; Trigger 1989). Kroeber (1923), Childe (1956), and Wiley and Phillips (1958)

highlight this limitation. As part of his definition of a culture area, Kroeber stated that one should expect the culture area, and subsequently its constituent parts, to have a "...fair degree of uniformity...Yet when the boundaries of such an area are crossed, a new type or culture begins to be encountered, which again holds with local variations until a third district is entered" (Kroeber 1923:296). In *Cultural and Natural Areas of Native North America*, he expanded this point noting that culture areas "...yield only a momentary and static organization of knowledge..." and that they only "...attempt to deal with such cultural wholes" (Kroeber 1939:1, 5). Similarly, Childe (1956) wrote, "a culture must be distinguished by a plurality of well-defined diagnostic types that are repeatedly and exclusively associated with one another and, when plotted on a map, exhibit a recognizable distributional pattern..." (Childe 1956:123 qtd. in Shennan 1989:5). Willey and Phillips (1958) note that what is required of an archaeological unit is that its "...constituent forms be physiognomic, recurrent, and internally consistent. To borrow a phrase from Childe, they must relate to one another in a way that permits us to assume them to be 'the concrete expressions of the common social traditions that bind together a people'" (Willey and Phillips 1958:14 quoting Childe 1950:2). The consequence of this is that that the world appears to be divided into discrete entities, what Eric Wolf (1982) called the 'billiard ball' vision of the social world. A series of tightly compact, rigidly bounded orbits that interacted, but did not necessarily change. When and if change occured, it was usually explained by the diffusion of objects and ideas as a consequence of invasion or migration.

Materializing Borders and Boundaries

The third common problem with archaeological cultures is that clear and definitive boundaries are often difficult to discern. Weissner (1983), Hodder (1982), and Stark (1998) point out that current understandings demonstrate that what once appeared to be stable and definite cultural boundaries no longer seem as clear-cut. A consequence of focusing on cultural 'cores' thus making culture areas appear to be internally stable has been, by association, to imply that boundaries are equally stable and cohesive. Indeed, even Kroeber recognized the difficulties of explaining the edges of culture areas. He wrote, "The weakest feature of any mapping of culture wholes is also the most conspicuous: the boundaries. Where the influences from two culture climaxes or foci meet in equal strength is where a line must be drawn, if boundaries are to be indicated at all" (Kroeber 1939:5). Even with the many theoretical and methodological improvements to the discipline since Kroeber formalized the culture area concept, we have not fully escaped implying that social groups in the past somehow existed within neat and definable spaces.

EMPIRICAL PROBLEM

Archaeology in the Delta of the Great Central Valley

The Delta Region of the Central Valley of California provides an exceptional case study for examining the issues surrounding the process of interpreting cultural boundaries of tribal groups using archaeological evidence. The Delta was a region of extensive human interaction and movement for at least the past 12,000 years (Gibbon and Ames 1998:125; Jones and Klar 2008; Moratto 1984). Much of the evidence for human occupation in the Delta, which drew initial archaeological interest beginning in the late nineteenth century, came from the extensive network

of mound sites along the rivers and tributaries extending from the San Francisco Bay far into the Delta (Cook and Treganza 1947; Cook and Heizer 1951; Cook and Elsasser 1956; Jones 1922; Lillard, Heizer, and Fenenga 1939; Meredith 1899, 1900; Nelson 1909; Schenck and Dawson 1929; Steward 1890; Uhle 1907; Walker 1934). Like much of the archaeology conducted in North America at the time, the goals of these early excavations in the California Delta were twofold; to establish chronological sequences of artifacts to more precisely understanding the cultural and historical development of the people who once occupied the region, and to understand the relationship between the archaeological record and contemporary Native people.

Early on, archaeologists from the University of California at Berkeley attempted to address these goals by excavating a few of the over 400 shell mound sites found throughout the San Francisco Bay (Nelson 1909; Uhle 1907). Although Uhle's excavations at Emeryville for example, demonstrated subtle but significant change in the material assemblage and a concomitant cultural change, it was not until the late 1920s and 1930s that California archaeologists recognized significant cultural change in the archaeological record and made serious attempts at chronologically sequencing the artifacts and the people who made them (Hughes 1994; Kroeber 1925:926).

The work of Uhle, Nelson, and others in the San Francisco Bay Area is significant for the development of California and North American archaeology. However, it was the excavations conducted at mound sites in the Delta that were instrumental in developing a cultural and historical sequence for pre-European Native Californian people (Gibbon and Ames 1998:126). The search for evidence of cultural change and affiliation with contemporary native groups began in earnest in the 1920s. In 1926 W. Egbert Schenck summarized the excavations of amateur archaeologist Elmer Dawson conducted between Stockton and Lodi, California. Based on Dawson's excavations, Schenck correlated the patterned distribution of artifacts detected by Dawson with contemporary tribal boundaries. The boundaries defined by the artifact distribution were then paired with the linguistic boundaries devised by Kroeber (1925). Schenck and Dawson wrote:

> Within our area, the beginnings of a division appeared. In burial customs and material culture the Stockton region showed more affiliation with the south and west, while the Lodi region showed closer connections with the north and west. Here a cultural division seemed to coincide with the linguistic line of cleavage between the Yokuts and Miwok (Shenck and Dawson 1926:406).

Their report is important for three reasons. First, they began to equate a limited number of artifact types and burial practices with tribal groups (Yokuts and Miwok). Second, they assume that the differences between the archaeological assemblages (Stockton and Lodi regions) marked cultural boundaries that could be correlated to the ethnolinguistic groupings Kroeber discussed in his then newly published *Handbook* (Kroeber1925). Third, Schenck and Dawson (1926) assumed that these cultural boundaries changed little through time.

Building on the work of Schenck and Dawson, J.B. Lillard and William Purves published the results of two years of fieldwork in the southern Sacramento River Valley. They recognized three distinctive temporal phases—Early, Middle, and Late—based on changes in burial position and artifact type (Lillard and Purves 1936). A few years later, Lillard, Heizer, and Fenenga (1939) refined and expanded this chronology. They proposed a sequence of Early, Transitional, and Late Periods, and further divided the Late Period into three sub-phases (I, II, and III). Importantly, Lillard and Purves (1936) correlated groups of artifacts with specific time periods

and created what they called culture types. Based on their refinements, subsequent archaeologists paired similar artifacts into their general chronology and attempted to trace the cultural and historical changes of Native peoples throughout the state.

The importance of these two publications cannot be understated. They are the starting points for what became a primary research agenda in the Delta: building a taxonomic system for organizing and interpreting culture change in the archaeological record through time and across space. Using McKern's Midwestern Taxonomic Method (McKern 1939) as a starting point, Richard Beardsley drew from the work of Schenck and Dawson as well as Lillard, Heizer, and Fenenga to develop his own classificatory scheme that we now know as the Central California Taxonomic System (Beardsley 1948, 1954).[4] Although Beardsley's lofty goal of applying the CCTS to all of California proved unrealistic, his efforts were nonetheless a catalyst for research that improved the central California taxonomy and contributed to California archaeological method and theory more generally.

Subsequent refinements, mostly undertaken by David Fredrickson and James Bennyhoff (1969, 1987), and republished by Hughes (1994), demonstrate a commitment to honing the descriptive precision of the taxonomic categories first developed by Beardsley, and improving the accuracy with which the interrelationships and changes in material culture could be mapped through space and time. As part of these revisions the definition for spatial unit locality was modified to resonate with the ethnographic tribelet. Similarly, the definition of a district was adjusted over time to correlate to Kroeber's (1925) language groups. The district, according to Bennyhoff and Fredrickson, "...is a geographical space, normally larger than a locality but smaller than a region, which exhibits a significant degree of total cultural uniformity among its constituent components...we believe there is already evidence available to equate districts with language groups in the Protohistoric and late Prehistoric period" (Bennyhoff and Fredrickson 1969 in Hughes 1994:20). The boundary dividing two districts was generally determined by a disjuncture in number, type, and style of objects found at archaeological sites. In other words, where the "degree of cultural uniformity" is no longer present, a cultural/tribal/linguistic boundary was assumed to exist.

Similar to the premises requiring culture change posited by Schenck and Dawson (1926), when a group of emblematic artifacts for a district faded in its spatial distribution and a second set appeared, a tribal boundary was assumed to exist in that location. As an extension of North American method and theory, the CCTS did a great deal to codify districts in Central California as internally consistent and possessing clearly differentiated boarders. However, as a consequence of later rethinking of the CCTS two inherent problems were highlighted. First, that as part of the creation of any taxonomy there is a homogenizing effect. Bennyhoff and Fredrickson, possibly the most skillful editors of the CCTS note, "we agree that this [a taxonomic framework] is the simplest way to handle broad syntheses but submit that it [a taxonomy] obscures cultural dynamics on any analytic level (Bennyhoff and Fredrickson 1969:19 qtd. in Hughes 1994). Taxonomies, they rightly state, smooth over and sometimes eliminate variability in the record. Second, they recognize that this homogenizing of the data has a concomitant effect on cultural boundaries. Thus, boundaries appear to be fixed in time and obscure the dynamics of cultural change that might be occurring (Hodder 1982; Wissner 1984; Sackett 1985). Fredrickson and Bennyhoff recognized this problem and tried to mitigate it, stating, cultural boundaries are neither fixed in time nor space, and that "...a much clearer view

[4] Hughes (1994:1) notes that it was actually Gerow, in Gerow and Force (1968:5) that was the first to use the title Central California Taxonomic System.

of prehistory is obtained if spatial boundaries fluctuate in synchrony with cultural change" (Bennyhoff and Fredrickson 1969:19 qtd. in Hughes 1994). Yet by normalizing the data that makes up an assemblage representing an archaeological district in the Delta, there is an inescapable and accompanying effect on the boundaries: they appear to be equally definitive.

Despite cautionary statements and the recognition of some inherent problems by some archaeologists (e.g., Milliken 2006), the simple application of district boundaries as tribal boundaries and the homogenization of cultural dynamics continues (e.g., Chartkoff & Chartkoff 1984; Jones and Klar 2008; Moratto 1984). Some of the most dramatic examples of this take place in the NAGRA context. When trying to determine the cultural affiliation of a NAGPRA eligible museum collection, legitimate budget constraints, personnel shortages, or inadequate provenience data often prompt museum staff and archaeologists to use archaeological boundaries posited by the CCTS (or another similar, but regionally specific taxonomy). In practice, these boundaries are applied as definitive and a-temporal. Archaeological boundaries are mapped onto ethnographic (cultural) boundaries and the ethnographic present is projected indefinitely backwards in time. This is an uncritical use of, and misplaced reliance upon, what appear to be but are not, cut-and-paste categories of cultural and historical integration.

THE DATA

As noted early in this chapter, the nature and location of the cultural boundary separating the Plains Miwok and Northern Yokuts tribes is the subject of continued ethnohistoric, historic, and archaeological debate. In this section I briefly discuss the conflicting empirical evidence fueling this debate.

By the 1970s major refinements made to the CCTS and chronological and artifact sequences based on stylistic analysis were generally accepted and the archaeological district in California had been formalized. At the same time, James Bennyhoff (1977) combined these advances in archaeological knowledge with historic documents, specifically Mission registers, to describe the nature and character of Plain Miwok peoples. As part of this research he posited that the Northern Yokuts cultural boundary lay between the Mokelumne and Calaveras Rivers. He further argued that the archaeological and ethnographic data illustrated that the Consumnes and Stockton archaeological districts corresponded to Yokuts and Miwok tribal territories (Bennyhoff 1977, 164; Bennyhoff 1994:106-107; Fredrickson 1973; Morrato 1984; Schenck and Dawson 1926). Central to these two conclusions was his adherence to ideas about the location of the boundary first presented in the early twentieth century. Schenck and Dawson (1926) for example, posited that there are several artifacts types and key variations in stylistic elements that could be used to distinguish between the two archaeological districts, and thus tribal groups. The idea that archaeological districts corresponded to group territory continued through the several iterations of the CCTS (Bennyhoff 1994; Fredrickson 1994). Stockton Curves (obsidian objects with serrations on either side that closely resemble bear claws and are usually thought of as being part of the Bear Dance, steatite and magnesite objects) different engraving patterns on bird bone tubes, different types of abalone pendants for regalia, and the orientation of burials were all thought to be these markers of cultural difference.

The same artifact types used to define archaeological districts also provided tantalizing evidence to confuse and blur them. I was first alerted to this issue while reading Bennyhoff's (1982 in Hughes 1994) discussion of the Cosumnes and Stockton districts during the Augustine

Pattern in the Central Valley (the Augustine is a grouping of temporally coeval cultural traits).[5] Outlining the archaeological evidence exemplifying each district, Bennyhoff noted that an incised elk-cannon hairpin/dagger which is a "typical Yokuts (Stockton District) artifact" was found at CA-SJo-43, a site he posited as the Plains Miwok village of Seuamne (Bennyhoff 1977:113).[6] Bennyhoff thought the presence of the hairpin at the site was the result of intermarriage (Bennyhoff 1982 in Hughes 1994:72). While this is a reasonable explanation there is other evidence that suggest more than just a single case of intermarriage might have been taking place in this border area. For example, my cursory exploration of the distribution of Stockton Curves, another marker type for the Stockton District (Yokuts territory), showed they were not all found exclusively in the Stockton District. Rather, they were distributed on either side of the border and in some cases (i.e., CA-SAC-113) they are found at sites in the center of the Cosumnes District. A similar pattern emerged with engraved bird bone tubes. Bennyhoff posited that the "crisscross" pattern was diagnostic of the Stockton District and the "openwork" design style was associated with the Cosumnes District (Figure 2.4)(Bennyhoff 1994:72). Again, my initial research suggested a broader distribution for both of these patterns. While my preliminary investigations were not enough to rewrite the district boundaries, they did suggest that there might be more complex relationships existed and that more another look at the Plains Miwok/Northern Yokuts boundary was archaeologically worthwhile.

Turning to some of the ethnographic evidence, and setting aside a lengthy discussion of early debates involving Kroeber and his contemporaries, I focus on two authors, Frank Latta (1892-?) and James Bennyhoff (1926-1993). Frank Latta, was not formally trained anthropologist yet Kroeber called him a "natural born ethnographer" (Kroeber 1948). Archaeologist Robert Heizer (1978:303) added that Latta's work was rivaled only by Kroeber and the post-humus work of J.P. Harrington. He is best known for his impressive volume, *Handbook of Yokuts Indians*. James Bennyhoff, an anthropologist specializing in archaeology is well known for his *Olivella* shell bead typology and his 1977 dissertation, *The Ethnogeography of the Plains Miwok*. Each of these authors contributed greatly to, and fueled the debate over the location of the boundary separating the Plains Miwok and Northern Yokuts Tribes.

FRANK LATTA. Frank Latta argued that the most northerly villages occupied by the Yokuts people were along the Cosumnes and Mokelmne River drainages. He named these two groups the Cosumne and Mokelumne Yokuts, respectively (Latta 1977:99-106). Latta used three sources to draw this conclusion. First, journal entries from early Spanish explorers such as Padre Duran from Mission San Jose (1819). Second, conversations with Kroeber and Merriam in which he claims they supported his ideas that the Yokuts occupied the entire Delta just prior to the Mission Period. Last, he relied heavily on ethnographic information he collected from Yokuts elders. His position is perhaps more liberal than Bennyhoff (1977), and he criticizes traditional anthropological approaches relying only on ethnolinguistic records. Making a thinly veiled swipe at Bennyhoff (1977), Latta notes, "...approaching the problem [that of the Plains Miwok/ Northern Yokuts cultural border] form a library in Berkeley, [where]...the relationships between the groups, and to the Mission, Sutter, Webber, Marsh, and other records, appear very different than they do to one coming from the company of many living Yokuts in the South" (Latta

[5] Bennyhoff focuses on the Stockton and Consumnes districts because he thought they corresponded to Miwok and Northern Yokuts territories. Assuming this connection is true, finding the boundaries of the districts would necessarily mean locating the boundaries of the tribal groups.

[6] Recent (1992) surveys conducted by BioSystems Analysis Inc. (now Pacific Legacy), a CRM company, demonstrated that despite being listed as four separate sites, CA-SJo-42, 43, 44, and 45 are in fact a single large site.

1977:95). Here Latta claimed Bennyhoff was naive to prioritize ethnohistoric and linguistic evidence over contemporary ethnographic observations and oral tradition. This passage also hints at a three-part methodological critique of Bennyhoff by Latta about the nature of ethnographic data, how it was collected, and who collected it. While I am not going to address these problems in any depth I will say Latta's evaluation highlights issues of knowledge production, power relations within archaeology and why some data which underpin why some authors evidence is more closely adhered to than others methodological critique of Bennyhoff by Latta about the nature of ethnographic data, how it was collected, and who collected it. While I am not going to address these problems in any depth I will say Latta's evaluation highlights issues of knowledge production, power relations within archaeology and why some data which underpin why some authors evidence is more closely adhered to than others.

JAMES BENNYHOFF. The research of James Bennyhoff (1977), is where many archaeologists, CRM firms, and museum officials turn to when trying to give a cultural designation to sites in the Delta. Like Latta (1977), Bennyhoff used ethnographic evidence, however, he found much of this data suspect for several reasons. He was concerned about the sample of people interviewed by early ethnographers. He was also concerned with the reliability of individual memory, their cultural relationship to the people being asked about, the accuracy of the recordings, and the potential for the information to be excessively generalized.[7] To combat these problems he focused his attention on the linguistic data, believing it to be more reliable. Additionally, he drew extensively from mission registers and early Spanish accounts. He also looked to the archaeological record that he viewed as the material expression of the Plains Miwok culture and an incredibly accurate one.

After combining and synthesizing archaeological, linguistic, and ethnographic data, Bennyhoff concluded that the Plains Miwok/Northern Yokuts boundary was two river drainages further south than Latta's, between the Mokelumne and the Calaveras Rivers. The distance between Bennyhoff's boundary line and Latta's northern most site, Cosumne, is approximately 26 kilometers. CA-SJo-42 is nearly equidistant to these two boundaries. The distances between these boundaries are not great yet the authority and precision with which Bennyhoff demarcates the cultural boundary between the Plains Miwok and Northern Yokuts seems problematic in light of the available evidence.

SUMMARY

Despite determined efforts at finding an accurate and precise location of the Plain Miwok/Northern Yokuts boundary by each of these scholars, an unambiguous border may not exist. In 1926, Alfred Kroeber wrote, "in the northern part of the Yokuts area the map is, however, blank except for a few names of groups of uncertain situation and doubtful affinities" (Kroeber 1926:474). Approximately fifty years later, James Bennyhoff wrote of the same area, "their [native peoples] territorial boundaries have been the subject of more disagreement than those of any other group in California" and that, "…the data available on Plains Miwok society and culture are grossly deficient and unbalanced in coverage, though not as wanting for the Northern Yokuts…" (Bennyhoff 1977:1, 10). More recently, Jones and Klar wrote:

[7] Bennyhoff notes that many of the informants from whom the ethnographic data is drawn were descendants of neighboring groups, not, in his case, the Plains Miwok (Bennyhoff 1977:2).

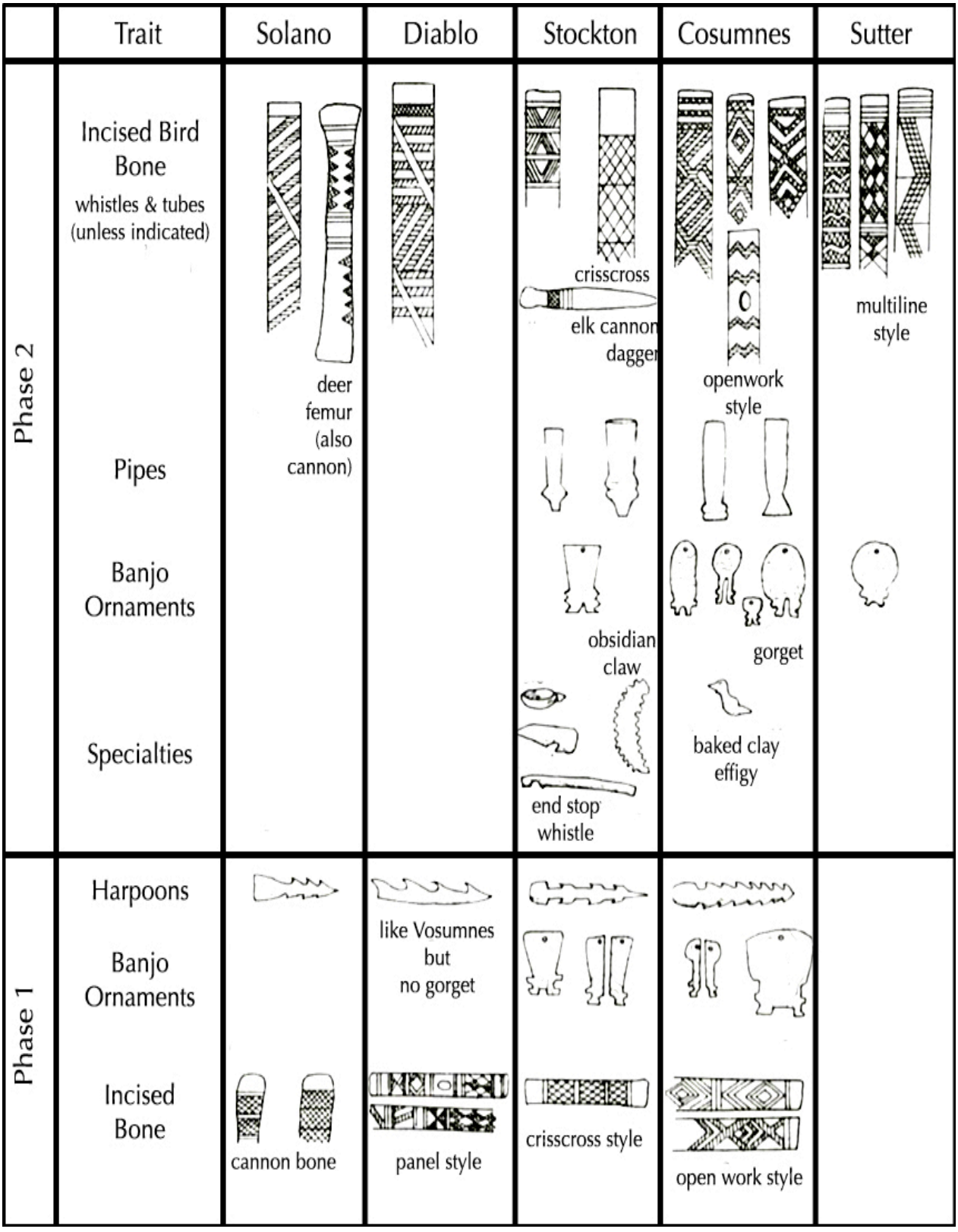

Figure 2.4 Bird bone tube artifact types and incising styles from the Diablo, Stockton, Consumnes, and Sutter districts. After Hughes (1994:72).

> Since Moratto's summary of Central Valley archaeology, basic understanding of culture history in the region [read Delta] has progressed very little, and we continue to lack well-grounded chronologies for large segments of the valley. Extant collections reveal a diverse and complex archaeological record, yet few modern studies have revised these interpretations or synthesized the considerable archaeological information available for this region (Jones and Klar 2008:147).

After almost a century of traditional research in the Delta, the cultural border between the Plains Miwok and Northern Yokuts Tribes is not clearly known. There are three reasons for this. First, while there is ethnographic data for the Nothern Yokuts tribes, there is a paucity of information concerning the Plains Miwok (Milliken 2008). Second, linguists ability to associate particular word endings with groups and find their attachment to village names has proven equally difficult (e.g., Bennyhoff 1977 and Latta 1977). Last, the archaeological evidence has not yet produced definitive results (Bennyhoff 1977; Bennyhoff and Fredrickson 1969; Fredrickson 1973, 1994; Jones and Klar 2008; Moratto 1984; Schenck and Dawson 1926).

In this case, a well informed or educated guess or a call for the collection of more data might seem appropriate. Both of these options, however, are inadequate. They ignore underlying theoretical problems with the paradigms being used and do not address the impact of the continued use of these theories within the context of NAGPRA. The gravitas, scope, and complexity of the relationship between culture areas, archaeological cultures, and cultural affiliation in NAGPRA might seem insurmountable. For me, it was precisely the unsettled nature of the supposed cultural borders in the Delta that gave me hope. In this dissertation I take a second look at several original collections without the burden of traditional archaeological paradigms and demonstrate the complexity of the Sacramento/San Joaquin Delta. My goal and hope is that this project substantively contributes both tribal and museum repatriation efforts.

CONCLUSTION

North American archaeology combines the anthropological concept of culture area and cultural taxonomy into a single orienting interpretative framework, the archaeological culture. The archaeological culture is used to link ancestral Native American sites, and in the context of NAGPRA, individuals to contemporary Native communities. In some cases this works well and in others it is less appropriate. Specifically, cultural affiliation breaks down in border areas where changes in material evidence become more complicated and as a result rarely fit into precisely defined categories. Although widely recognized as a major theoretical issue in North American archaeology, especially after the passage of the NAGPRA, contemporary research still adheres to these traditional processes. Its continued use within the NAGPRA process is especially problematic given what is at stake: the final disposition of the cultural heritage of a formerly colonized people. As article 31(1) of the *United Nations Declaration on the Rights of Indigenous Peoples* (2007) states "Indigenous peoples have the right to maintain, control, protect and develop their cultural heritage, traditional knowledge..." Although NAGPRA certainly fits within Section 2 of article 31: "...States shall take effective measures to recognize and protect the exercise of these rights," blind adherence to culture area maps found in the *Handbook of North American Indians*, for example, fall dramatically short of the standards set by International Human Rights organizations as well as the underlying spirit of NAGPA. The inadequacies of

archaeology's foundational concept, especially in light of NAGPRA, argues for the continual reexamination of these assumptions and the need to explore the practical and sociopolitical implications of these ideas outside the confines of the discipline.

The line of questioning in this dissertation focuses on the difficulties associated with the process of cultural affiliation in cultural border areas. Instead of assuming cultural boundaries are static or arguing they are fluid, I consider five potential models:

1. *Fixed*: This model is based on a traditional framing of cultural boundaries; the territory of an ethnic group as a homogeneous unit. In this case I would expect to see clear differences in stylistic variation of the artifacts. The boundaries should be 'hard'.
2. *Clinal*: The second model is one where I would expect to see a gradient in the distribution of objects, a moving boundary. The territories for different groups should overlap like a Venn diagram and in these overlapping areas there should be a great deal of mixing of artifacts. In this case, everything outside this grey area would suggest, like the first two models, hard boundaries. In this case, cultural permeability only exists in border areas. Boundaries may shift based on context.
3. *Temporally Fixed*: In this model, artifacts distribution patterns should be fairly distinct during each chronological period, but move between periods.
4. *Temporally Clinal*: This fourth type of boundary is similar to the temporally fixed boundary, but changes should be observable within each chronological period. Given my data this type of boundary might be impossible to observe.
5. *Punctuated*: The final model is the most flexible and random. I expect to see some types of objects circumscribed and others less constrained. Boundaries are more permeable than in any of the other models and there is a greater movement of objects. The overlap in material evidence could be similar to the overlap hypothesized in model two, but in this case, it should penetrates deeper into what is usually thought of as the 'cultural core.'

By allowing for the existence of any or all of these boundary models, I leave open the possibility that my analysis could show for each diagnostic artifact class a different type of boundary as well as a different type of social interaction. I do not assume that a combination of some or all the artifact types will show the location of the boundary and fully explain the nature of the interactions taking place across it. Nor do I assume the existence of a singular boundary per-se. Instead I ask, what type of boundary is being observed with a given artifact type and if it is a cultural boundary, what does it look like? At the end of each data chapter I will compare my findings to each of these five models to better understand the nature of any observable difference.

METHODOLOGY: RATIONALE FOR RESEARCH

Four different classes of diagnostic artifact types will be analyzed in this dissertation; shell beads, baked clay, chipped obsidian objects, and engraved bird bone ear tubes. Each artifact class will require the use of a different analytical method. I will describe the method used at the start of each data chapter. Some issues common to all artifact classes are discussed below.

MUSEUM COLLECTIONS. I analyze collections from 32 different sites located in the Delta Region of Central California (see Image 1.5 below). Many of the sites were excavated in the early part of the twentieth century and were central to the development of the Central California

Taxonomic System and the culture area and archaeological culture. Collections from all but two sites (YOL-19 and YOL-182) are extant archaeological collections and housed in the Phoebe A. Hearst Museum of Anthropology (PAHMA) at the University of California, Berkeley. I chose to work with extant collections for three reasons.

First, reanalyzing museum collections addresses several broad issues within the discipline. It positions museums not as oversized curio cabinets of the past but as places where viable and innovative research that is accountable to, and partnered with, a variety of publics takes place. As Rothschild and Cantwell (1981) point out, despite the many limitations of conducting research on extant archaeological collections—commonly a lack of provenience information—a wealth of information remains accessible. While using museum collections takes more work and additional creativity, it is possible and the results can be compelling (e.g., Cantwell et al. 1981). Next, conducting research on museum collections addresses the curation crisis by demonstrating that new excavations lacking explicit or pressing community oriented purpose is untenable. Today there are greater limitations on space, personnel, and funding for museums holding archaeological collections. In part, these issues can be addressed in the field with innovative methodological techniques such as "catch and release" (Gonzalez et al. 2006), or by limiting the amount of excavation taking place by using "surgical operations" (Lightfoot 2008:221). Last, conducting research on museum collections is a way to directly engage with the active and heated NAGPRA issues facing the University of California system. On the one hand, critics of U.C. museums argue the museums should loose their curation privileges because not enough research aiding communities takes place. To put it another way, museums appear to be disengaged from the publics they are required to serve. On the other hand, critics of NAGPRA decry the loss of potential data and the ability to apply new techniques to old collections in the future. It is incumbent for archaeologists, for lack of a better term, "to put their money where their mouths are" and justify the disciplines claims to intellectual necessity for curating objects for future research.

Second, working with extant collections is also a place to develop innovative archaeological methods and theories, test old ones, and return to the discipline's museum based origins (Trigger 1989). Analysis of museum collections is something new in the sense that it recognizes the one person/one site mentality of doing research and graduate student training is largely dead (Thomas 1981:576). Further, it acknowledges the growing participation and partnership with Native collaborators. On both counts the discipline is at a stage where it is appropriate to reconsider many of its traditional assumptions about the appropriate locations for data acquisition, interpretation, and even dissemination (Atalay 2012; Mihesuah 2000; Smith and Wobst 2005; Tuhiwai Smith 1999; Watkins 2000).

Third, the PAHMA is the primary repository for archaeological materials in California. Examination of its collection is thus foundational to creating and revising understandings of the social, cultural, and economic lives of the Native people living in the Delta Region.

SITE SELECTION. CA-SJo-42 was the starting point for the broad questioned addressed in this dissertation for four reasons.[8] First, it is located near an archaeologically and ethnographically disputed boundary between the Plains Miwok and Northern Yokuts Tribes and is listed as

[8] CA-SJO-42 is a NAGPRA collection. Because of this, only limited contextual information will be provided about the sites location, excavation history etc., in order to protect the anonymity of the original excavators and any further looting of the site.

culturally unidentifiable by PAHAMA. Second, the Tachi Yokuts Tribe of Leemore, California is claiming the site for repatriation under NAGPRA. Tachi is claiming the collection because of the sites proximity to this boundary; because the Plains Miwok don't exist as a federally recognized tribe; and because of their oral histories and traditions that speak of relationships between Yokuts, Delta, and Bay Area peoples. Third, the collection from CA-SJo-42 is part of a larger cluster of sites (CA-SJo-42, CA-SJo-43, CA-SJo-44, and CA-SJo-45) and could be the ethnographically known tribelet center of Seuamne (Bennyhoff 1977:113) or Mokelumne (Latta 1977:102-106). CA-SJo-43, CA-SJo-44, and CA-SJo-45 were originally excavated by Schenck and Dawson prior to their 1926 publication. While Schenck and Dawson and later Bennyhoff (1977) posited that CA-SJo-43 was the ethnographically known village of Seuamne, they made no statements about the adjacent sites of CA-SJo-42, CA-SJo-44, and CA-SJo-45 (Bennyhoff 1977:113; Schenck and Dawson 1929:309). However, a recent survey[9] demonstrated that these four individual sites should be considered a single site. For this reason, I combine data for sites CA-SJo-42, CA-SJo-43, CA-SJo-44, CA-SJo-45, where possible, to acknowledge this past social reality. Fourth, the materials from CA-SJo-42 date from the Phase II of the Late Period to the Historic period. This temporal position and possible status as a tribelet center makes this cluster of sites comparable with other type sites originally used to determine cultural boundaries and ethnographic boarders in the Delta (see for example Bennyhoff 1977).

Although the cluster of sites near CA-SJo-42 was the starting point for this dissertation, the complicated process of cultural affiliation required a broader focus. Affiliating a single, or even a cluster of sites is impossible without extensive comparison. Consequently, the research area was enlarged to include the entire San Joaquin and Sacramento Delta (Figure 2.5). The sites chosen for analysis are known or probable tribelet centers or large village sites. Each site dates from the Late to the Historic Period. The Late Period is typically divided into three Phases (I, II, and III). Phase II of the Late Period is the temporal starting point for the analysis contained herein. Additionally, and importantly, analyzing four different artifact classes across the entire Delta created multiple and overlapping artifact-based transects which crosscutting the supposed Plains Miwok/Northern Yokuts cultural boundary. These transects, in turn, create a comparable cross-sectional views of the Delta through the multiple lenses presented by each of the artifact classes.

ARTIFACT SELECTION. Four primary classes of artifacts were chosen for analysis: *Olivella* and clam shell disk beads, basket impressed baked clay cooking balls, obsidian chipped stone objects, and engraved bird bone ear tubes. I chose these objects because of their ubiquity within the archaeological record in the Delta, their historical use as archaeological marker types for different archaeological districts thought to correlate with ethnic group boundaries, and their primacy and importance for building chronological and typological sequences in California.

[9] See footnote 6 above.

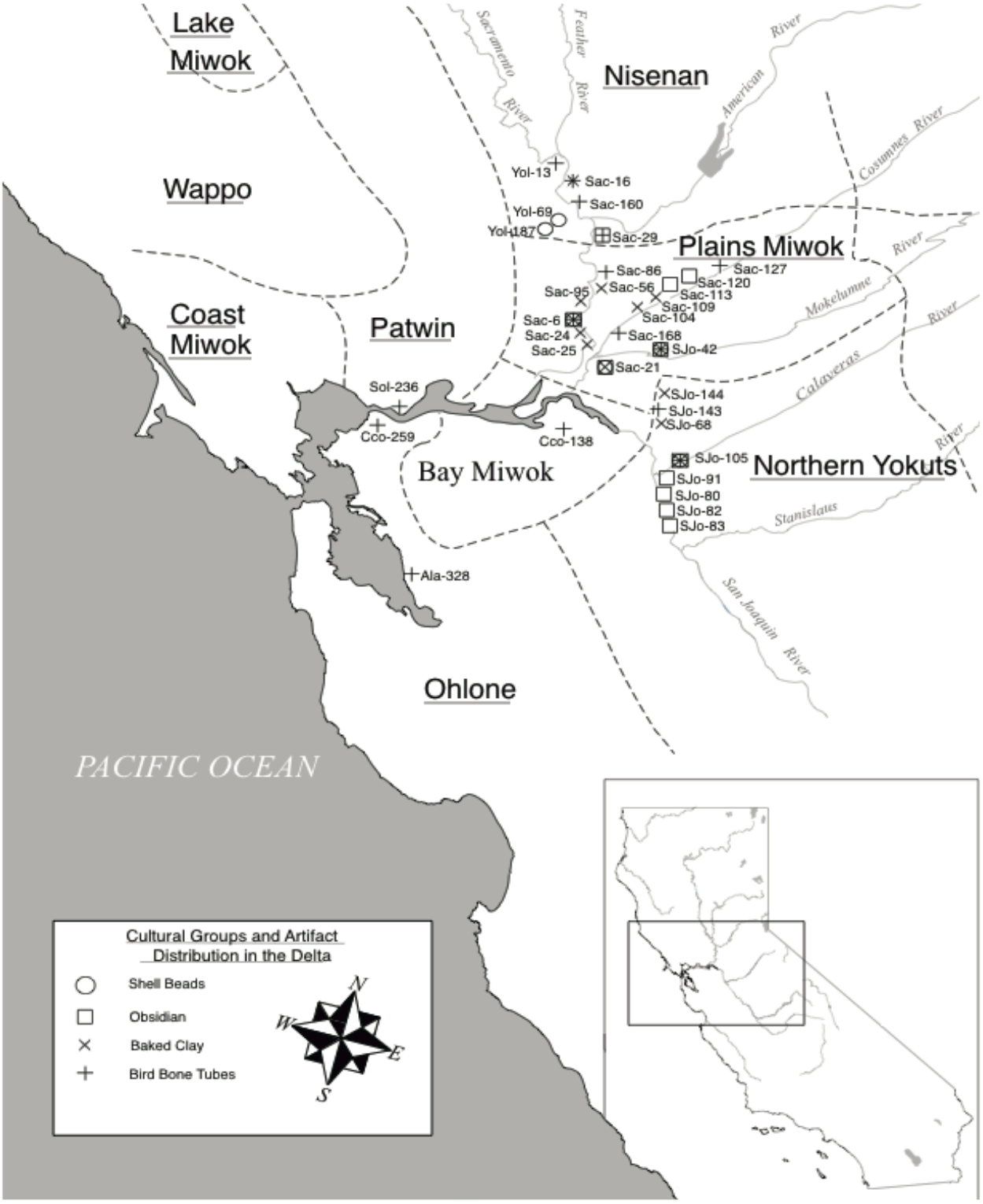

Figure 2.5 Distribution of archaeological sites and artifact classes utilized in the study.

Chapter 3
Olivella and Clam Shell Beads

INTRODUCTION

In the previous chapter I briefly summarized the theoretical and empirical issues underlying the process whereby archaeological collections can be culturally affiliated. In this chapter I present an analysis of the first of four artifact types examined in this dissertation: *Olivella* and clam shell beads. Although I do not interpret them as markers of cultural boundaries, they are accurate chronological markers and help establish chronological control. Moreover, because I did not conduct destructive analysis on any of the objects analyzed during any phase of this project, collections were dated based on shell bead analysis and the established shell bead –based chronologies. Shell bead analysis provides a "big picture" view of economic interactions and the social arenas underpinning them.

Beads, in particular those made from *Olivella biplicata* and clam shell genus *Saxidomus* and *Tresus* are ubiquitous in the archaeological record of California. This ubiquity, in conjunction with their formal variability and consistent change through time, their broad temporal and geographic distribution and the frequent reference to them in many of the early ethnographies of California makes them crucial to elucidating daily practices in pre-European Native California (Barrett and Gifford 1933; Bennyhoff and Hughes 1987; Eerkens 2005, 2007; Gayton 1948; Gibson 1992; Gifford 1947; King 1974,1978,1990; Latta 1977; Merriam 1955; Moratto 1984; Vellanoweth 2001). There are five areas of archaeological inquiry in which *Olivella* and clamshell beads figure prominently: 1) building and refining archaeological typologies and chronologies (Bennyhoff and Hughes 1987; Groza 2002); 2) studies of regional and interregional systems of conveyance (Fitzgerald et al. 2005; Hughes 1994; Jackson and Ericson 1994; Raab and Howard 2001); 3) explorations of social complexity, especially in the Late Holocene (Arnold 2001); 4) provenience or source analysis (Eerkens 2005; 2007); and 5) studies of craft specialization (Arnold 1987, 1993, 1994, 2001; Hartzel 1991; Rosenthal n.d).

The analysis in this chapter builds upon previous scholarship by reanalyzing and remapping the distribution of shell beads (measured in percentages) in the California Delta during Phase II of the Late Period (A.D. 1510 - 1720). To do this, I employ the shell bead typology of Bennyhoff and Hughes (1987) and pay particular attention to the distribution of bead classes, types, and subtypes across five sites, SJo-42, SJo-105, Sac-6, Yol-69, and Yol-182 in the Delta (Figure 3.1). Together, these sites create a rough transect bisecting the Plains Miwok/Northern Yokuts boundary suggested by Bennyhoff (1977).

Throughout this chapter I ask if and in what ways the distribution of different classes of shell beads reflect social interactions and trade between groups across boundaries. In particular, I focus on two dominant areas of California bead analysis, typology and chronology, and trade. First, I provide a literature review of several chronological and typological shell bead studies and include descriptions of the common bead types. Next, I discuss three of the most influential conveyance studies. I also highlight ethnographic and historic evidence relating to shell bead trade and use and their social contexts. After providing a summary of the historical and methodological contexts within which this research is situated, I present my own methods and findings. I conclude the chapter with my interpretations.

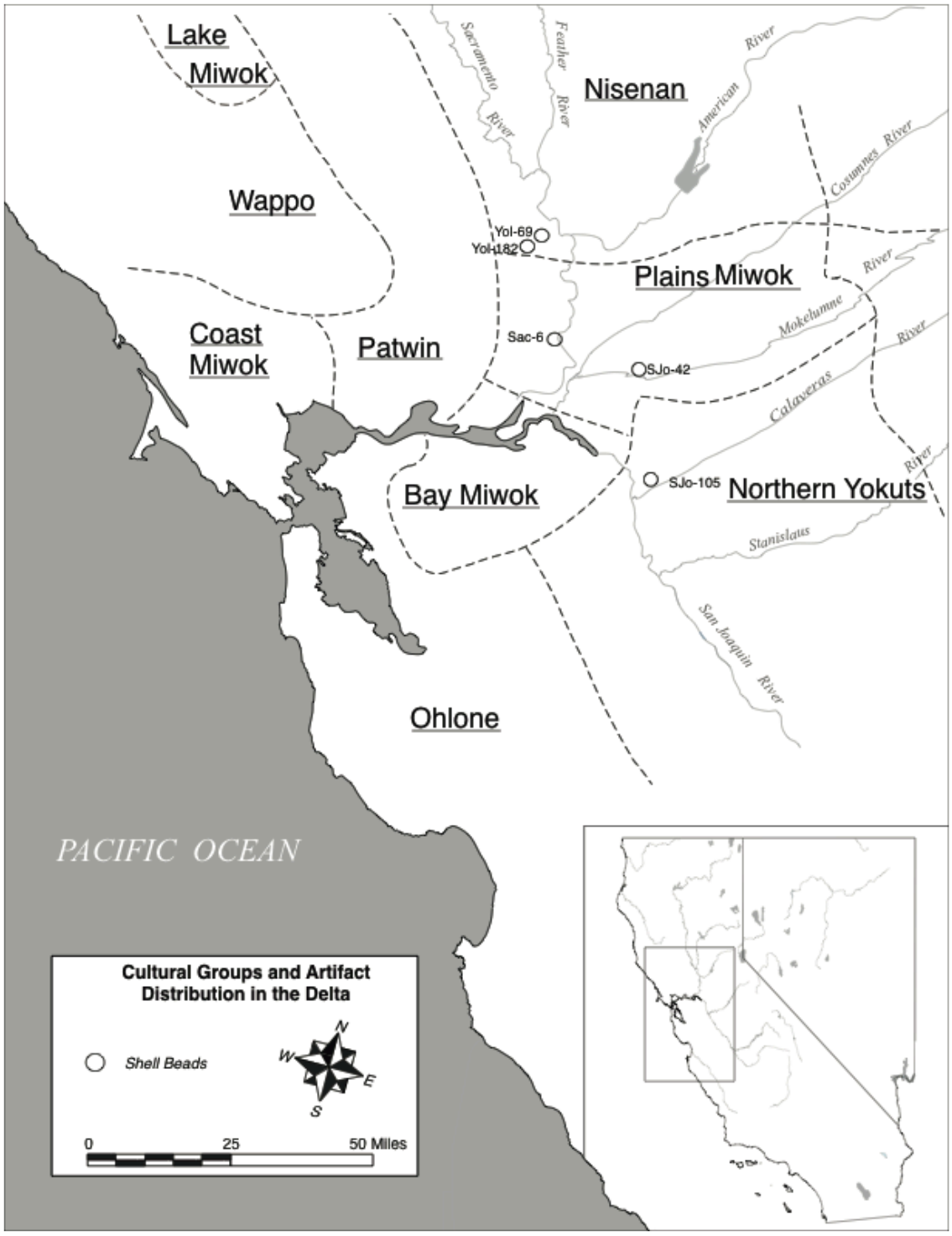

Figure 3.1 Study sites from which *Olivella* and clam shell beads were studied

BACKGROUND

CHRONOLOGY AND TYPOLOGY

California shell beads are important to California archaeology for several reasons. Most importantly, *Olivella* shell beads played a key role in the development of regional and chronological typologies.For example, King wrote that shell beads are "the most sensitive indicators of change over time regularly found in late archaeological contexts in California…" (1978: 58). Even today, this remains true. Secondarily, shell beads provide for unique insights into regional exchange networks and the social interactions underpinning them.

The following statements highlight the importance of *Olivella* shell beads in particular.

> For our part, we would place more reliance on a single, well documented cross-date based on ceramics or shell beads than we would on a single radiocarbon date which might appear to be more exact but which, standing alone and unverified, therefore is a potential avenue of error and misdirection (Bennyhoff and Heizer 1958: 60).[1]

More recently, René Vellanoweth (2001) stated:

> Because of their popularity and continuous use in the past, shell beads now serve as valuable archaeological time markers. Indeed, they are one of the most important artifact classes for the development of cultural chronologies. (Vellanoweth 2001: 941).

Olivella shell beads are used as accurate temporal markers because their formal stylistic characteristics change regularly through time. Shenck and Dawson (1926: 374-377) were the first California archaeologists to observe the consistent changes in the size and shape of *Olivella* and clam shell beads recovered from archaeological contexts. While their work focused on the Delta, they made two observations that are the basis for bead studies in California. First, they hypothesized that changes in the raw numbers of specific types of shell beads found at archaeological sites indicated different archaeological regions correlated with ethnic group boundaries. Second, and more importantly, they posited that the observed changes in shell bead styles were measurable and consistent through time (plate 87 in Schenck and Dawson 1926). While their first hypothesis proved not to be the case, their second has been the cornerstone of California bead studies. Taken together, their two observations represent a significant first step in understanding the culture history and chronology of the California Delta. There are three other major attempts to develop a shell bead typology. These are Lillard, Heizer, and Fenenga's *An Introduction to the Archaeology of Central California* (1939), Gifford's *California Shell Artifacts* (1947), and Bennyhoff and Hughes' *Shell Bead and Ornament Exchange Networks Between California and the Western Great Basin* (1987).[2]

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[1] This last point is of course a dated statement that perhaps says more about the researchers view concerning a newly developed method (Libby and Arnold 1949). Despite significant improvements and adjustments to radiocarbon dating procedures during the past fifty years and its wide spread use, the analysis of shell beads remains an essential chronometric method among California archaeologists.

[2] Table 3.3 integrates chronological components from each of the three primary bead studies discussed in this section as well as from Bennyhoff and Hughes (1987) and Groza (2002).

LILLARD, HEIZER AND FENENGA: INTRODUCTION TO THE ARCHAEOLOGY OF CENTRAL CALIFORNIA (1939)

The first bead typology for California was completed by Lillard, Heizer and Fenenga as part of their *Introduction to the Archaeology of Central California* (1939) (Table 3.1 and Figure 3.2). The goal of this monograph was to provide a synthesis of the entirety of the excavations conducted by researchers from the Sacramento Junior College and the University California, Berkeley in the San Joaquin and Sacramento River drainages. As part of this synthesis, the authors directly challenged the then dominant theory of culture change (or more accurately, lack thereof) championed by Alfred Kroeber, "…that not only the general Californian culture area, but even its subdivisions or provinces, were determined a long time ago and have ever since maintained themselves with relatively little change" (Kroeber 1925: 926). Based on their observations of the archaeological record Lillard et al. challenged Kroeber by proposing a three phase pre-European temporal sequence for California. They divided this tripartite chronology into Early, Middle, and Late periods and characterized each period with a group of diagnostic objects (Table 3.2). Using shell beads, Lillard, Heizer, and Fenenga provided evidence for the assertion that the cultural practices of Native Californian people cultural did not remain fixed in time, as Kroeber argued. They stated:

> As is well known, California archaeology has often been pointed out as a field, nearly unique in one respect – that it is 'timeless'; that cultural sequences are absent, and that there is a striking uniformity in culture from oldest to most recent. This, even in theory, is absurd; we know of no culture which is so static. Culture to judge from its varied manifestations throughout its total range is dynamic…And California archaeology is no exception… This, then, is one of the points we should like to emphasize. (Lillard, Heizer, and Fenenga 1939:1).

In their schema, one that still influences archaeological practice, significant changes in a bead style have come to signal the movement from one archaeological period to another. Lillard et al. opined, "the importance of bead types as cultural indicators in this area [California Delta] cannot be overestimated. Bead styles in this region are the equivalent of pottery types in the Southwest" (Lillard et al. 1939:74).[3] Until the late 1980s, the Lillard, Heizer, and Fenenga bead typology (Figure 3.2) was the most comprehensive and usable framework for temporally organizing beads recovered from archaeological contexts. Importantly, their publication solidified a major research agenda for studying shell beads; beads as chronological markers.[4] They also provided the key rationale for excavating burials where they occur most abundantly; to associate shell beads with discrete temporal events and thus improve the chronological control of their typology.

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[3] For an excellent introductory review of the importance of ceramics in the Southwest for the development of typologies and chronologies in American Archaeology (see e.g., Trigger 1989:148-206).

[4] This is not to say that shell beads are used only for the purposes of chronological sequencing. Since at least the 1980s there has been a push to explore their relationship to trade and exchange networks and their role in increasing social complexity on the southern California coast, for example. Rather, it is to say that their primary role in archaeological analysis has been as a time-sensitive object.

Table 3.1 Summary of Lillard et al. (1939) Shell Bead Typology	
1a. Small, spire-lopped, whole shell bead lb. Large, spire-lopped, whole shell bead lc. Small, diagonally spire-lopped, whole shell bead ld. Side perforated, whole shell bead 2a. Rectangular bead 2b. Large rectangular bead	3al. Lipped saucer bead 3a2. Half shell bead 3a3. Sub-rectangular half shell bead 3b. Saddle-shaped bead 3b1. Large, saddle-shaped bead 3b2. Sub-rectangular bead 3c. Saucer-shaped bead 3d. Small, saucer-shaped bead 3e. Thick, saucer-shaped bead

Table 3.2 Lillard et al. (1939) Shell Bead Chronology		
Early Period 2b 1a 1c	Transitional Period 3b 3b1 3b2 3c	Late Period Phase I: 2a 3a2 3a3 Phase II: 3a1 3d 3e Clam

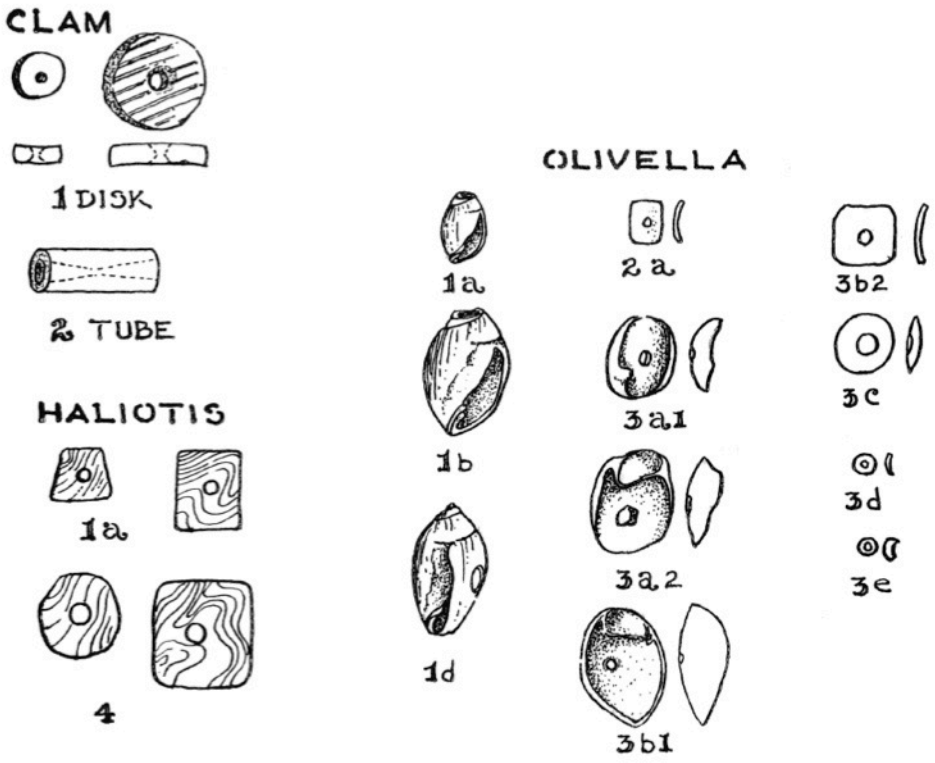

Figure 3.2 Lilliard et al. (1939) shell bead typology. Adapted from Beardsley (1954: Appendix A).

Lillard et al. also recognized that the primary limitation of their bead typology was its lack of time depth and geographic scope. This was a direct result of restricting their analysis to sites from the lower Sacramento and northern San Joaquin River drainages (Lillard et al. 1939: 3). Nevertheless, their work is significant to the archaeological community because they provided extensive descriptions of all the excavations conducted in their study area and created usable typologies for several different classes of artifacts, especially shell beads. The bead typology, developed from their summary of Delta excavations, inspired later regional typologies, and until the 1980s was the most widely used and usable classificatory system available.

E.W. GIFFORD: CALIFORNIA SHELL ARTIFACTS (1947)

Almost ten years later, Edward Gifford attempted to expand the shell typology proposed by Lillard, Heizer, and Fenenga in *California Shell Artifacts* (1947). Gifford had two goals for his monograph. First, it was to combine all shell objects - beads, pendants, and ornaments - made from various materials - *Olivella*, clam, *haliotis* for example - into a single statewide typology (Figure 3.3). Second, and more important was to correlate different types of shell objects found in archaeological contexts with ethnographically known examples so that better interpretations of the unseen cultural practices expressed in the archaeological record could be inferred. Gifford believed that by focusing on the ethnographic record he could better explain the function and meaning of shell objects and thus contribute to archaeology's broader anthropological goal of understanding the culture and history of California Indian people. He noted that attention to the relationship between the archaeological and ethnographic record would provide further chronological insight and substantially contribute to the primary research focus of California archaeologists at the time, namely, creating cultural sequences for the entire state (Gifford 1947:6).

Gifford made a few important additions to California bead studies. He provided extensive descriptions of the ethnographic contexts in which different types of beads were found; on baskets, cloths, necklaces, or as burial gifts. Gifford's extensive descriptions are useful because they lend insight into the range of uses that any single bead might have had and remind the reader of the complex social relationships and worlds that shell beads helped shape.

Yet, the ambitious nature and broad scope of his study resulted in three key limitations. First, the typology is unwieldy. While the attempt to incorporate the totality of shell artifacts into a single workable typology is laudable, the result is a cumbersome and often confusing system of classification. Second, as Bennyhoff and Hughes (1987) point out, many of the subtle time sensitive variations observed among *Olivella* shell beads are lost in both "catch-all" and overly divisive categories. For example, Gifford's X3bI type blends three time-sensitive bead types noted by Lillard et al. and divides their type 3a1 into three different types that ignores their temporal similarity. Third, Gifford focused on examples from Late Period sites employing the Direct Historic Approach (Steward 1942; Heizer 1941).[5]

[5] For more recent critiques regarding the continued use of the Direct Historic Approach, see for example, Hughes (1992), Wobst (1978), Silliman (1996).

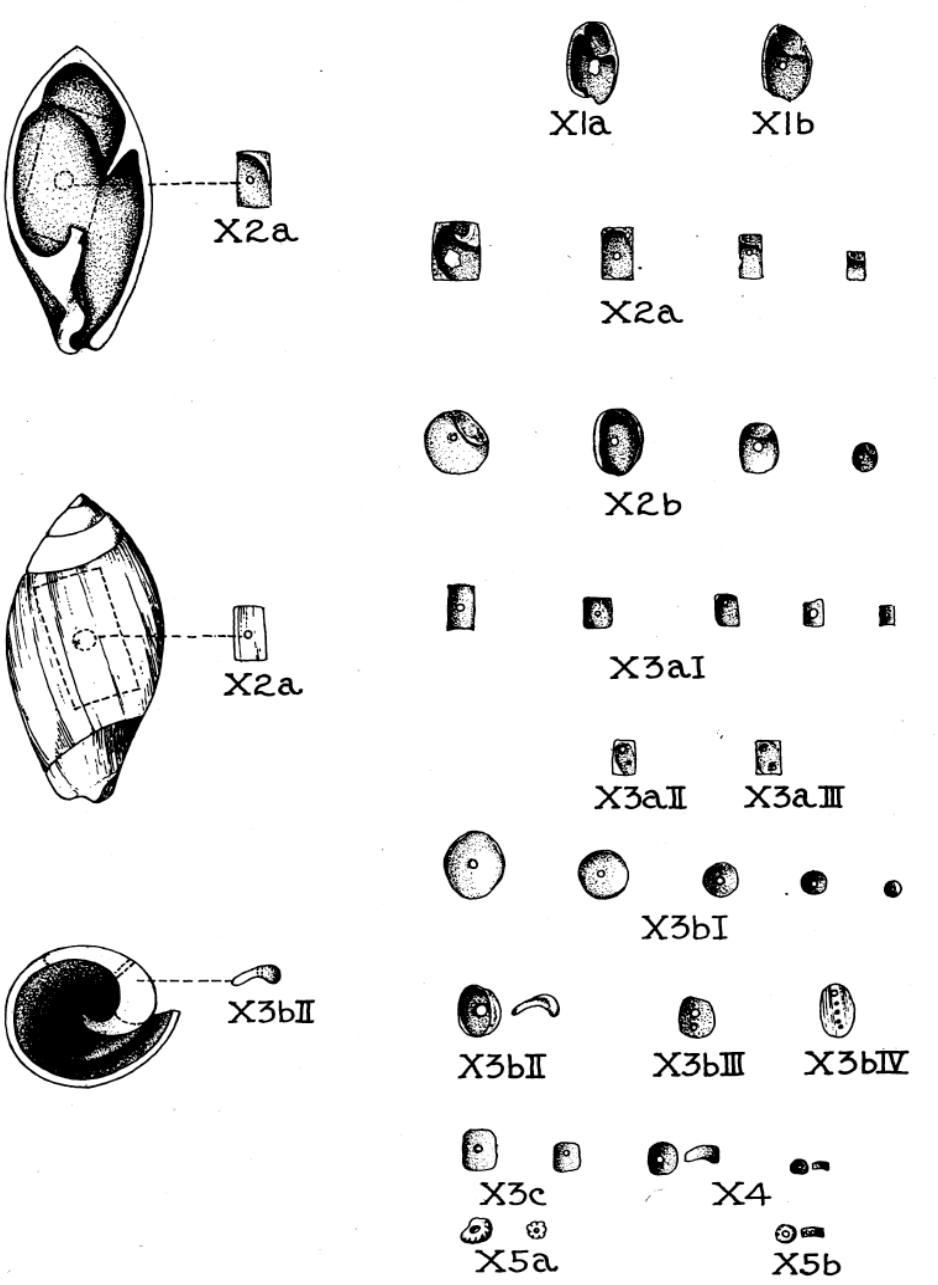

Figure 3.3 Gifford's (1947) shell bead typology. Adapted from Gifford (1947: 97)

Recognizing the limitations of the typologies created by Lillard et al. (1939) and Gifford (1947), Bennyhoff and Hughes (1987) expanded what was originally intended to be a focused analysis of shell beads from Gatecliff Rock Shelter in Nevada into a comprehensive bead typology for California and the Great Basin. Bennyhoff and Hughes (1987) refined and elaborated the temporal and geographic breadth and depth of previous typologies. They analyzed 5,420 individual beads sampled from 81 sites spread throughout California and the Great Basin, which ranged in date from 4,000 B.C to approximately 1830 A.D. Bennyhoff and Hughes acknowledge that much of the complexity of their typology results from the fact that they 'split' rather than 'lumped' descriptive elements of the beads. Nevertheless, by paying attention to subtle details of shell beads, their typology provided a means for rigorous chronological assessments of archaeological excavations without absolute dating methods.

Bennyhoff and Hughes divided their typology into 17 Classes of beads (A-Q), 45 types, and many more subtypes (for an example see Figure 3.4). Classes of beads are marked with a capital letter (for example: *A*), types have a numeric designation (for example A*1*), subtypes a lower case letter (for example: A1*c*), and variations in the subtype are designated by another number (for example: A1c*1*).[6] In rare cases, shell beads had been incised on the width of the bead. To designate this phenomenon, Bennyhoff and Hughes used a terminal (i) (for example: A1c*i*). When they found incising to be temporally significant, the (i) was followed by a roman numeral (for example: A1ci*I,* A1ci*II*) indicating an ordinal sequence. Included in the typology is a temporal range for each bead type and subtype, detailed descriptions and drawings of each bead, and the average measurements of each bead in three dimensions (length, width, and curvature).

While Bennyhoff and Hughes made notable and long-lasting improvements to previous studies, there are three common critiques of their work. First, Moore pointed out that none of the 5,420 samples were radiocarbon dated (Moore 1990: 871). Instead Bennyhoff and Hughes confirmed their sequence by comparing it to other bead studies that provided calibrated radiocarbon dates for beads of the same type. In 1990 Moore's criticism was valid; without having updated radiocarbon dates, bias and error is added to the study. Recently, however Groza (2002) addressed Moore's concern by radiocarbon dating temporally significant beads from museum and Cultural Resource Management firm collections. Groza adjusted Bennyhoff and Hughes' chronology by accounting for the local marine reservoir effect (ΔR 225± 35), something that could not be taken into account during the original study (Groza 2002: 34-36). Groza found that while some adjustments needed to be made, the overall schema remained intact. The second critique is that the beads Bennyhoff and Hughes analyzed were assigned a provenience to several different archaeological contexts; burials and household contexts. Consequently, and this was the issue Gifford attempted to address, the cultural use(s) for the beads are unclear; that is, as the contexts in which the beads derive change, so to do their meanings. The third critique is that despite the title of their monograph—*Shell Bead and Ornament Exchange Networks Between California and the Western Great Basin*—there is very little detailed discussion of trade and exchange patterns; rather, only general descriptions of bead distributions and densities were provided.

[6] Note that this subtype does not exist in the typology and is only used here to illustrate the logic of the system.

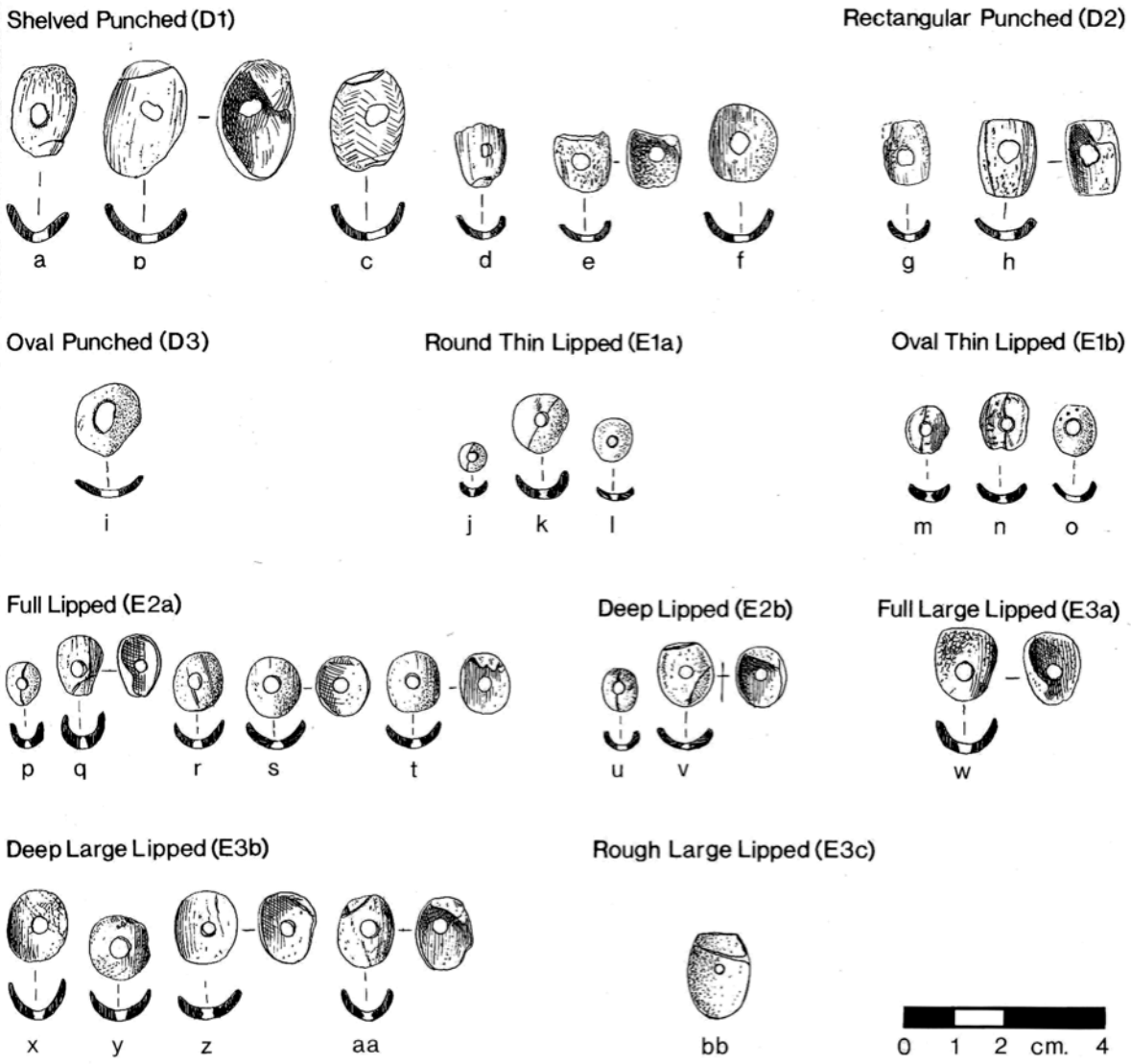

Fig. 5. *Olivella* shell beads, classes D and E.

a. D1a, L-17252 (Sac-16); b. D1a, L-17252 (Sac-16);
c. D1ai, 1-65712 (ch15); d, e, f. D1b, 1-56352b (Sac-66);
g. D2a, 1-56352d (Sac-66); h. D2a, 1-53871a (CCo-250);
i. D3, 1-74718 (Las-1); j, k. E1a1, L-16576h (Sac-127);
l. E1a2, L-16576i (Sac-127);
m, n. E1b1, 1-133721a1 (Sac-168);
o. E1b2, 1-133721a2 (Sac-168);
p. E2a1, L-15691a (Sac-56);
q. E2a1, L-15691a (Sac-56); r. E2a2, L-15691b (Sac-56);
s. E2a3, L-15691d (Sac-56); t. E2a4, L-15691c (Sac-56);
u, v. E2b, L-15576a (Sac-56);
w. E3a1, 1-54321a (Mrn-266);
x. E3b1, 1-54321b (Mrn-266);
y. E3b2, 1-54321d (Mrn-266);
z. E3b3, 1-50699 (CCo-259);
aa. E3b4, 1-54321c (Mrn-266);
bb. E3c, number not available (C. King, 1973a, 1973b).

Figure 3.4 Bennyhoff and Hughes' (1987:126) bead typology. Reproduced with authors' permission.

At this point I refrain from presenting a more comprehensive review of the Bennyhoff and Hughes typology for several reasons. First, a full review would be almost as long as the original text defeating the purpose of a "summary"; in lieu of this, I provide brief summaries as well as images of different bead types relevant to this dissertation. With this typology I have found images are more effective and more readily comprehended than are words. Lastly, there are at least two other excellent reviews, reproducing them in their entirety would be redundant and make this chapter unnecessarily unwieldy.[7]

SHELL BEAD CONVEYANCE

Using the geographic distribution of shell beads as a representation of social and economic interaction (conveyance) is a second focal point for California bead studies. Shell beads, in particular *Olivella biplicata*, are ubiquitous in archaeological contexts throughout the interior of California and the Great Basin. In addition to their broad geographic distribution, numerous references to their use as personal adornment, money, and gifts in the ethnographic record make them ripe with meaning (e.g. Barrett and Gifford 1933; Gayton 1940,1948; Kroeber 1925; Latta 1977; Powers 1976).There are a few questions other than chronology that guide California bead studies: how and where were shell beads manufactured, how did they reach their final destination, and for what reasons did they move. Answers tend to interpret the movement of beads as indicators of adaptive response to changes in available resources, increasing population density, and increasing social complexity and residential stability (Arnold 1994, 2001, 2002, 2008; Arnold and Graesch 2001; Farmer and LaRose 2009; Gamble 2008; Gibson 1992; Hartzell 1991; Hughes and Milliken 2007; King 1978,1990; Rosenthal n.d.; Rosenthal et al. 2009).

For example, Arnold argued that shell bead production was an adaptive response to environmental changes and a key element in the development and maintenance of Chumash society. She wrote that "the Chumash adapted to the environment and its uncertainties by effectively managing risks though strategies that were developed over thousands of years" (Arnold 2009: 248). In this case, Arnold notes that one way the Chumash managed risk was through the production and exchange of shell beads (Arnold 1991, 1992, 1994, 2001, 2002, 2008). In these publications she argues that environmental stresses created by climate change facilitated trade and exchange between the Santa Barbara Channel Islands and the mainland, which was controlled by plank canoe owners. Trading and exchanging shell beads allowed island dwellers to counteract the effects of resource scarcity by trading shell beads made on the island for food with mainland populations. In turn, this exchange system increased the volume of shell beads traded into the interior of California, changing the nature of social interactions and influencing the social structure of different groups as they participated in the growing shell bead economy. For Arnold, it is within this complex interplay of environment, canoe use, and shell bead production and trade under the concentrated authority of burgeoning elites that the beginnings of Chumash social complexity are found (Arnold 1992, 1993).

Milliken and Hughes (1993) have a slightly different interpretation. For them, trading and exchanging shell beads was potentially a way to improve social relationships between groups as well as to alleviate social stresses. Drawing on data from San Francisco Bay and Delta regions, they note that the distribution of beads in the form of "conspicuous gifting" at feasts, along with "conspicuous destruction" during funeral ceremonies, "...may have been necessarily concomitant to the beginnings of a truly monetized regional exchange network by stabilizing beads as

[7] See footnote 15 below.

markers of value and of obligation between families as corporations over time" (Milliken and Hughes 1993: 392). In other words, shell beads cemented the social bonds established through marriages, feasts, ceremony, and other social and political activities.

The transfer of shell beads between groups is often viewed in terms of the broader scale of the interaction area and exchange network (Bennyhoff and Hughes 1987; Farmer and LaRose 2009; Jackson and Ericson 1994; Jenkins and Erlandson 1996; King 1978; Vellanoweth 2001). Building on the work of Caldwell (1962) and King (1978) (Figure 3.5), Bennyhoff and Hughes (1987) view the limits of the distribution of *Olivella* and clam shell beads as indicators of the boundaries of "interaction areas." King (1978: 62) notes, "the limits of these large interaction networks occurred in areas of lower populations density and smaller villages. In these boundary zones, only the most common and low-valued forms of beads were usually owned" (King 1978: 62). However accurate the interaction area maps may be at modeling the large-scale movement of beads, they do not explain the smaller scale social mechanisms and relationships between individuals moving the beads.

Raab and Howard (2001), however, provide a good example of how the social context of shell beads might be seen through the lens of an exchange network. They discuss the distribution of *Olivella* Grooved Round (OGR) beads throughout California and the Great Basin. Several scholars view OGR beads as time sensitive indicators of a Middle Holocene (4,200 - 5,200 Y.B.P.) cultural interaction sphere that possibly parallels the distribution of the Uto-Aztecan languages (Bennyhoff and Hughes 1987; Fitzgerald and Jones 2005; Raab et al. 1995; Vellanowith 1995, 2001). Thus, fluctuations in the trade of these beads are interpreted as indicators of changes in cultural interaction. Raab and Howard write:

> Given these patterns, we suggest that OGR beads might have been transported in a cultural sphere defined by Takic language community. Languages within this group (e.g., the Cupan language dialects spoken by the island Gabrieliño) represent one of the California sub-divisions of the larger Uto-Aztecan linguistic province (Raab and Howard 2001: 593) (see also Harrington 1962; Jenkins and Erlandson 1996:301; Kroeber 1925: 574-580).

Caldwell (1962) first presented the interpretive model of the interaction sphere arguing that they represent an extensive geographic distribution of recognizable, non-local, elite, prestige items found at Hopewell sites in the American Midwest, as a way to understand social and economic interaction. The exchange of goods among distinct Hopewellian sociocultural groups was, Caldwell posited, monopolized by the elite who sought markers for social status in a developing system of stratification. Additionally, the movement of goods between groups within an interaction area administered by people with elite status was underscored and accompanied by information exchange. Regional and interregional relationships encouraged by the exchange of goods and managed by this small group of individuals precipitated and reinforced the development of complex social institutions. In Caldwell's assessment, interaction spheres lend *causality* to formal hierarchy and social complexity. Moreover, interaction spheres are inherently evolutionary in that regional and interregional interactions progress from simple to complex (Caldwell 1962; Freidel 1979; Schortman and Urban 1987; Struever 1971). In California, however, interaction areas are not discussed in terms of social complexity, rank, or systems of exchange among elite individuals. Rather, the underlying conceptual framework

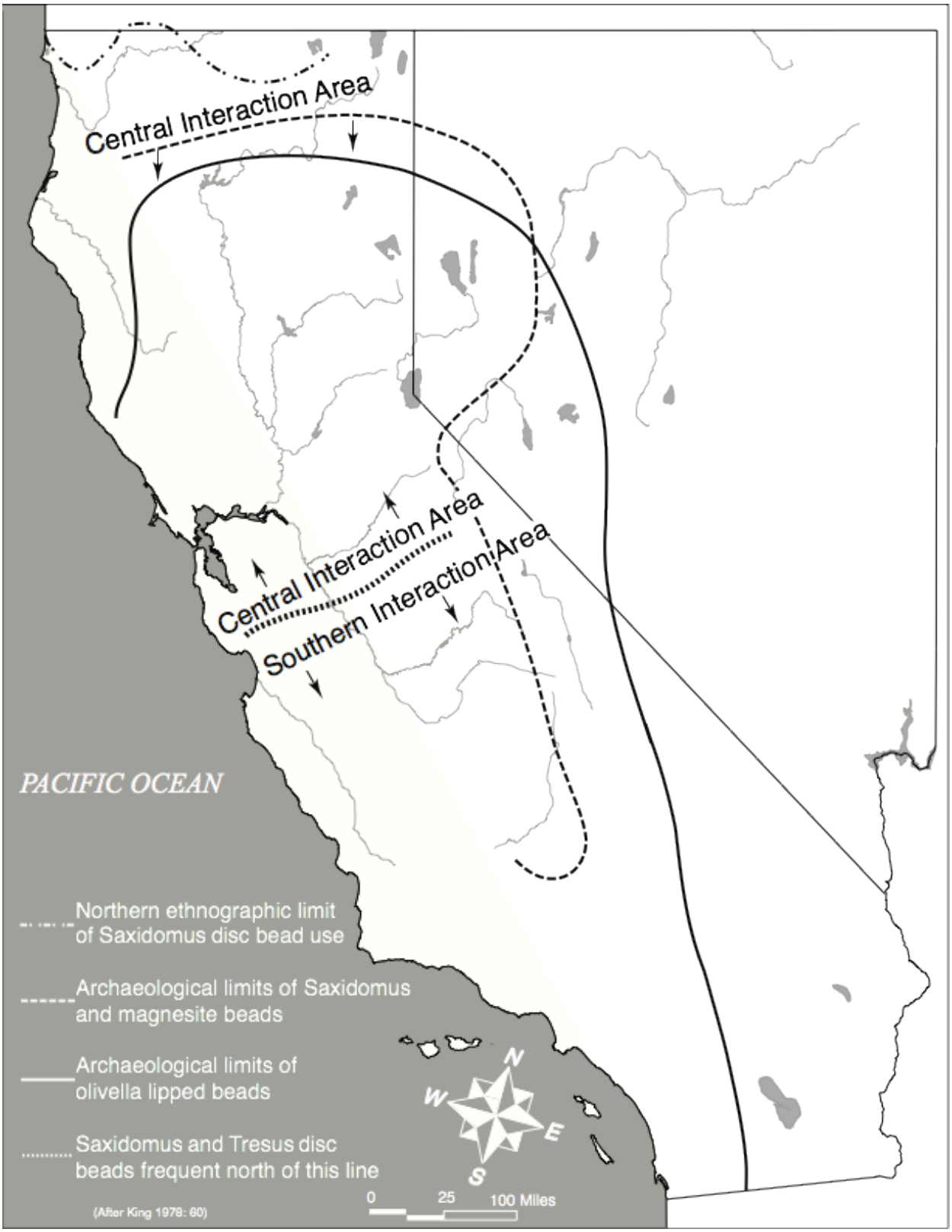

Figure 3.5 Interaction spheres of shell bead exchange throughout California.Adapted from King (1978).

is modified to describe the limits of the geographic distribution of shell and obsidian (e.g. Bennyhoff and Hughes 1987; Jackson and Ericson 1994; King 1978; Raab and Howard 2001). In this framework, specific social interactions facilitating exchange and the mechanisms for the transfer of goods are assumed, but not articulated.

Tracing the main differences between Caldwell's interaction spheres and those used by California archaeologists raises the following proposition: that the idea of an interaction sphere is sufficiently broad to allow for the modification of the explanatory frameworks describing the activities happening within them: i.e., type(s) of interactions, the numbers of people doing the interacting, the range and scale of the interactions, and the social relationships under pinning those interactions (e.g., Freidel 1979). This conceptual flexibility is advantageous for two reasons. First, it can broaden the way in which we understand the nature of inter-group interactions across cultural borders, and second, it has implications for testing existing boundary models.

RATIONALE

As I outlined above, there are two primary focal points for shell bead studies in California: first as temporal markers for regional chronologies, and second, as key elements within regional and interregional exchange networks and interaction spheres. However, focusing analysis on only these two areas of study reduces opportunities to explore the social roles of shell beads. Other scholars have made similar statements concerning the typological and distributional studies of shell beads (for example, Bennyhoff and Hughes 1987; Gibson 1992; King 1978) and I will elaborate on these briefly.

Olivella and clamshell beads provide tangible evidence for a range of complex social and cultural interactions that took place for millennia before the arrival of Europeans. From this perspective I highlight that shell beads are not just "objects" that chart economic exchange networks or mark time; they actively facilitate social relationships in, with, and between people and events. Passing from the hand of one individual to another shell beads played an active and constitutive role in making and possibly even breaking social bonds (Trubitt 2003). In California, as in many other places around the world, the meanings that are embodied by shell beads depend on context as much as on type. Shell beads were used, for example, for economic purposes (as money), as indicators of rank and/or status, and to express the social responsibilities and bonds of affinity linking individuals and groups during perennial feasts and ceremonies. As media operating in broadly social spheres of action they expedited the movement of not only goods but also ideas; in this sense, shell beads were inherently 'social.' The beads I analyzed helped to maintain multiple, complex, and contingent relationships; they were media for both economic (trade and exchange) and ceremonial activities (e.g. actions derived from social responsibilities: participation in dances, mourning ceremonies, or feasts for example, that themselves might be described as particular types of economies). As Mauss (1990) reminds us in *The Gift*, and Durkheim (1984) more generally in *The Division of Labor*, shell beads have a double value; as both 'do-er' and 'signifier'. In the context of California archaeology, shell beads are often situated both as 'do-er' and 'signifier.' On the one hand, they are static objects with inherent value facilitating the movement of other objects (e.g. baskets). On the other hand, they represent labor, social control, and hierarchy.

At the broadest level my analysis reflects the intellectual history of the study of shell beads. I followed and replicated elements of these previous studies for two reasons. First, by

tracing the movements of shell beads though the Delta I illustrate broad-scale trade networks underpinning and facilitating smaller-scale exchanges that might announce social and cultural differences. Second, based on ethnohistoric evidence, this broad scale context serves as a backdrop to more localized interactions that points to a complex tapestry of face-to-face interactions occurring in a number of different and overlapping social contexts. Simultaneously, shell beads allow us to understand both the broad sweep of economic interactions and regional trade networks as well as personal interactions. There is a third aspect that is beyond the scope of this dissertation, but is absolutely essential to interpreting shell beads in this manner; that is the relationship between individuals and the beads they used to adorn themselves. I lacked specific contextual information needed for this dimension in my analysis of shell beads, but it is one I take up in chapter five as part of my discussion of engraved bird bone ear tubes.

Recent bead sourcing studies combined with ethnographic evidence improves archaeological and anthropological understandings of the multi-faceted social worlds shell beads inhabited and helped to create and reinforce. There has been a great deal of evidence pointing to the Southern California Channel Islands as the production center for *Olivella* shell beads found throughout all of California and the Great Basin (e.g. Arnold 1994; Bennyhoff and Hughes 1987). This evidence is derived from a number of production sites with a high density of beads and their raw materials.

More recently, Jelmer Eerkens et al. (2005, 2007, 2009) carried out oxygen isotopic analyses to try to provide accurate source data for California *Olivella* shell beads. In 2005 Eerkens et al. presented data showing that the California coast could be divided into two isotopic regions, north of Point Conception and south of Point Conception. The distinction was based on differences in the carbon isotope ratios found in modern *Olivella* shells. Eerkens (2007 and 2009) then sampled archaeological collections from the San Francisco Bay and San Joaquin/ Sacramento Delta (n=42 beads) and the Owens Valley (n=6 beads). Contrary to what he and his research team expected as based on the 2005 study, none of the Delta beads had isotope signatures consistent with the modern control sample from Northern California. Of the total sample, 69% (n=29 beads) were definitively sourced to southern California (south of Point Conception) and, as for the remaining 31% (n=13 beads) no clear source could be determined (Eerkens 2009). Additional provenience analysis of *Olivella* shell beads needs to be completed and should be expanded to include clamshell beads. However, current evidence supports ethnographic accounts recorded in the early 20th century highlighting the southern coast of California as "the" location for *Olivella* shell bead production.

While southern California seems to be the main location for *Olivella* shell bead production, there is evidence that clam shell beads were produced in North Central California, albeit at a much smaller scale (Hartzell 1991, Rosenthal n.d.). As part of a study exploring the function of shell bead exchange in Northern California, Rosenthal (n.d.) compiled a comprehensive list of North Central Coast and Delta sites that yielded clam shell beads and sites showing evidence for *Olivella* shell bead production in the Delta and the greater San Francisco Bay Area. Rosenthal found that the distribution of clam shell bead production sites was limited to areas north of the San Francisco Bay Area and west of the Sacramento River (Yolo, Napa, Sonoma, and Lake Counties), while sites indicating only consumption of shell beads are found south and east of this area (Sacramento, Contra Costa, and Marin Counties) and concentrated in Sacramento County (Figure 3.6). The distinction between production and consumption sites is striking and points to a specialized economic relationship between producers and consumers.

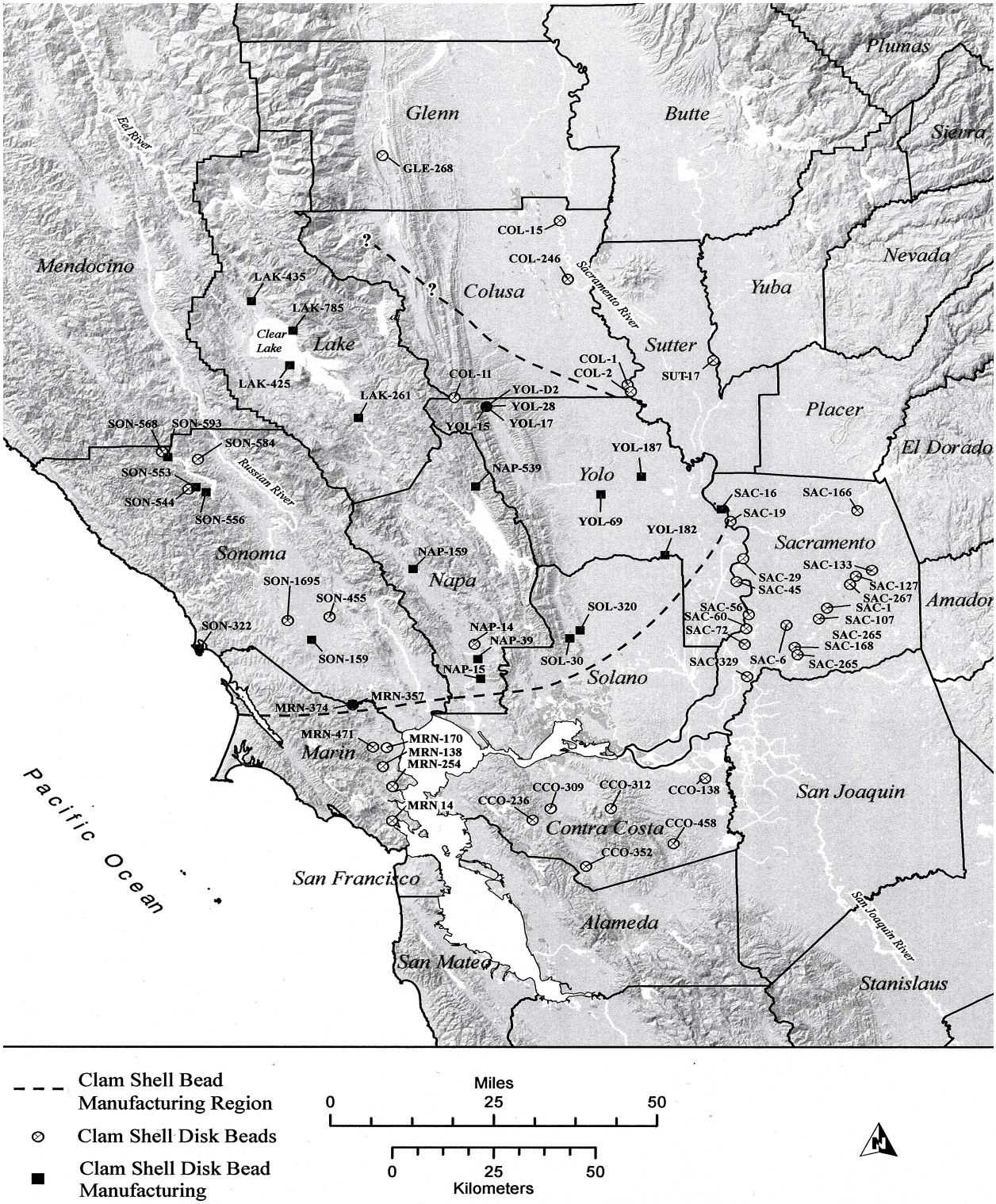

Figure 3.6 Late Prehistoric sites in Northern California with clam shell disk beads and evidence of bead manufacture. Bead production sites located near Pt. Reyes are ommitted from this map. Image reproduced with permission of author (Rosenthal n.d.).

Ethnographic evidence collected in North Central California points to the same pattern of shell bead distribution between northern and southern California as found in the archaeological record. Both Barrett and Gifford (1933) and Gifford (1926) provide key insights into the flow of shell beads through the Delta. Barrett and Gifford wrote about the movement of both clam and *Olivella* beads in Miwok territory,[8]

> Few or no disk beads were made by the Miwok.[9] They obtained the discoidal clam shell beads chiefly from the people living to the north, while olive shell disks and long tubular beads came from the south. Apparently they themselves regularly made only abalone (*Haliotis*) shell ornaments, and chains of whole olive shells and other univalves (1933: 251).

Providing more detail about the movement of clam shell beads Barrett and Gifford wrote:

> [Clam shells] were obtained from the ocean by the Miwok journeying to the coast for them. Whether this occurred before the coming of the Spaniards is uncertain. At any rate, it was a prevalent custom after the coming of the Spaniards. The shells brought home were worked locally (1933: 255).

In *Clear Lake Pomo Society* Gifford presents a similar narrative regarding clam shell beads when discussing money. He recounted:

> The Cigom [Pomo] informant had himself been six times to Bodega bay to obtain the clams. The customary load borne home on the back weighed about one hundred pounds. The journey from Cigom to Bodega bay took three and one-half days, the route being by way of Cloverdale and Sebastopol. At Sebastopol the last night camp was made. The final leg of the journey required all of the following day. The Miwok at Bodega bay neither charged for the clams nor objected to the Pomo digging them (Gifford 1926: 377).

And of *Olivella* shell bead trade Barrett and Gifford learned from informants that

> Olive shell disk beads came from the South as a rule, though sometimes the Central Miwok made them. Often a southerner would bring a string of them to a Miwok friend. After drilling the disks the edges were rubbed smooth on a rock. They were strung on string, made of either a man's or a woman's hair (Barrett and Gifford 1933: 252).[10]

Although ethnographic references to shell bead trade in the Delta and North Central California are scant, their concordance with the archaeological record is remarkable (Rosenthal

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[8] In their study, Barrett and Gifford do not consider Coast and Lake Miwok people; "what little is known of Coast and Lake Miwok cultures indicates that they resembled Pomo culture rather than the culture of the Plains and Sierra Miwok" (Barrett and Gifford 1933: 128).

[9] First, it is not clear to which beads the authors refer, clam or *Olivella* and second, this statement cannot be applied to Coast and Lake Miwok territories.

[10] The fleeting reference to unfinished beads being traded and finished by people living in the Delta is tantalizing and deserving of further research.

n.d.; see also Gifford and Kroeber; 1939 Kniffen 1939; and Stewart 1943 for additional references to shell bead use). Combined, archaeological and ethnographic evidence points convincingly to southern California as the source for *Olivella* shell beads and the Marin and Sonoma County coasts as the origin for clam shell beads during the Late Holocene and early Historic periods.[11] Conventional archaeological explanations describe this pattern as the result of risk buffering activities, an economic response to environmental stress, or a consequence of population pressure and changes in social complexity (Arnold 1987, 1991, 1994, 2004;Chagnon 1970; King 1974, 1978, 1990). These explanations try to link shell beads to large-scale social change but in doing so, they ignore the social roles of shell beads.

Certainly ethnographic and archaeological narratives raise several questions about the large-scale mechanisms and social networks that moved shell money to their final resting places in the California Delta. But even the passing references to a "southerner" bringing beads to a friend or the multi-day trips to the coast by Pomo men to collect shells in Miwok territory at no cost speaks to personal interactions that moved shell beads across the landscape across and within social arenas.

From this point of view, the archaeological record gains texture and lends itself to an interpretation of shell beads as not just pawns in a game of economic stress, but as symbols of friendship, ceremony, and day-to-day interaction. For example, Barrett and Gifford (1933: 251) note of the Miwok, "Beads and shells were employed in necklaces, belts, bandoliers, and shell ropes. No leg or ankle bands were made." They also observed that "In case of toothache a bit of the root [fleabane] was chewed and placed in the cavity. Fleabane came from the North ("Klamath River") by trade, and was paid for in beads, shells, and baskets" and that, "shell beads, baskets, acorns, and flicker feather headbands were used as media of exchange (Barrett and Gifford 1933: 166, 255). Describing what occurred before a Yokuts person was interred, Anna Gayton pointed out,

> ...after a body was put in the grave, its clothes and possessions were added. Gifts from relatives and friends, usually baskets, were added, the baskets being placed one over the head, another over the feet, and little ones along the body. These were always deliberately despoiled by cutting. Then loose beads were scattered all over...The grave was then filled with earth" (Gayton 1940: 196).

Gayton also briefly described elements of the ceremony when a woman had her first period:

> She [the woman] was washed, and dressed with beads and earrings for the first time. (Gayton 1948 vol 1: 30)

And Gifford observed part of the process of catching an eagle to gather feathers for ceremonial activities,

> The actual capture was preceded by a feast on the part of a number of men and women beneath the tree or cliff where the nest was located, everyone scattering seeds and beads

[11] While there is evidence of *Olivella* shell bead production in the San Francisco Bay area it is much less concentrated or developed than shell bead production sites found in southern California.

as offerings. At the nest the eagle catcher scattered more seeds and beads. Gifford 1926: 395).

As items of non-Delta manufacture traded over varying distances and used in multiple and overlapping contexts, (e.g. economic, personal adornment, and ceremonial settings), shell beads simultaneously provide broad and narrow perspectives on the types of social interactions taking place in the Late Period and early Historic California Delta. On the one hand, beads give a birds-eye view on the socioeconomic underpinnings for the movement of local goods. On the other hand, ethnographic accounts flesh out and give context for the myriad social contexts and uses of shell beads. These accounts give a sense of the finer details of daily activities and ways in which shell beads were used. In other words, ethnographic passages remind us that beads are not just objects to be counted, weighed, and "typed" by archaeologists; they are ceremonial offerings and markers of transition denoting an individual's progression from one social role to another. It is important to remember these social and economic transactions occurred between individuals as well as between the living and the deceased.

METHODS

I selected for analysis five Late Period Phase II to Historic Period sites with bead assemblages with more than 1,000 beads.[12] All five sites, CA-SJo-42, CA-SJo-105, CA-SAC-6, CA-YOL-69, and CA-YOL-182 (Figure 3.1) were chosen because, like CA-SJo-42 that was the starting point for this dissertation, they are described in the ethnographic record as either large village sites or triblet centers. Moreover, the sites are arranged along a north/south axis bisecting the Bennyhoff Plains Miwok/Northern Yokuts boundary. I chose this configuration of sites for three reasons. First, it allowed me to examine the distribution of the *Olivella* and clam shell beads across the Delta region. Second, it provides an empirical test for previous archaeological hypotheses and more recent ethnographic evidence I presented above. Lastly, it tests the four proposed boundary models presented in chapter one.

The materials recovered from three of the five the sites I selected are curated at the Phoebe A. Hearst Museum of Anthropology at the University of California, Berkeley. Three of these sites (CA-SJo-42, CA-SJo-105, and CA-SAC-6) were excavated multiple times over the last one hundred years and are foundational to the interpretive history of California archaeology. Data from the remaining two sites were derived from site reports (Atchley 1995; Wieberg 2005). The Yolo county sites were used to expand the study area northward as a way to verify the validity of the trend first found among the Berkeley collections. Several other sites were analyzed, or partially analyzed, during the process of mapping and interpreting shell bead distribution in the Delta (e.g. CA-SAC-107, CA-SAC-21, and CA-YOL-197). These sites are excluded from this chapter because the provenience information was too poor to accurately separate Phase II beads from other time periods and only after complete analysis of the collections were they determined to have too few beads for the purposes of this study (<1,000 beads).[13]

[12] During the course of analysis I found that sites with 1,000 beads showed similar distributions as those with exponentially more beads (compare for example CA-Sac-6 and CA-Yol-182)

[13] I was only able to determine a collection presented too small a sample until after the entire collection was analyzed.

Because the provenience information for each site analyzed in this chapter varied considerably and was often minimal, I organized the data into three broad categories: 'burial,' 'non-burial,' and a combination of the two that I defined as 'all.' While not ideal for refined analysis, this simple division allowed for a 100% sample of the beads curated at the Phoebe A. Hearst Museum of Anthropology. It also allowed for data from site reports for CA-YOL-69 and CA-YOL-182 to be integrated with the museum based information. Whenever possible, the bead assemblages for individual burial lots were recreated.

Using the Bennyhoff and Hughes (1987) shell bead typology, *Olivella* shell beads were categorized by class, type, and subtype. Typological categorization was taken for each bead analyzed (n=204,170). For the benefit of the reader, I reproduced Figure 1 from Bennyhoff and Hughes (1987: 89) (Figure 3.7) to provide visual reference to important landmarks of an unaltered *Olivella* shell as well as vocabulary essential to *Olivella* bead analysis. The chart visually indicates from where on a complete *Olivella* shell specific types of beads were cut.[14] Additionally, given the sensitive nature of many of the beads analyzed for this project, neither AMS radiocarbon dating nor oxygen isotope analysis was conducted. Without primary absolute temporal data gained from destructive analysis, I follow the Bennyhoff and Hughes' (1987) chronology and I incorporated the updates made by Groza (2002) as well as Milliken in Wiberg et al. (2005) to limit the beads I selected for analysis to those coming from contexts within the Late and Historic Periods.

Regarding the Bennyhoff and Hughes (1987) typology, I offer a few general observations: 1) remember these beads were made by human hands and while they are surprisingly uniform, an over emphasis on and adherence to, what is printed can be debilitating While the illustrations in the 1987 publication show the most 'typical' bead for any given type, there is a great deal of variation within any one bead 'type' ; 2) make flash cards first. I found it helpful to redraw each bead type and subtype and write down key characteristics along with their temporal ranges. This made it much easier to recognize different bead types and subtypes as I worked through bags that were sometimes filled with several hundred of all subtypes of beads; 3) recognize that becoming comfortable with the typology takes time and there will necessarily be long periods spent just staring at a bead while trying to decide between one subtype and another. Point three leads me to my final statement, which overlaps with the first point, namely that there are some key characteristics of each bead type that allow the researcher to accept the range of variation in any given type. Recognizing these elements however, takes time. For convenience I provide descriptions of the bead types I analyzed during my research and, whenever possible, provide detailed descriptions of the subtle, but key differences that I used to distinguish types and subtypes within Classes.

My analysis of clam shell beads involved a less complicated process. Clam shell beads were counted, weighed, and the relative size noted (small, medium, large). I made no attempt to measure the exact diameter of each clamshell bead (potentially a time sensitive indicator) because although clam shell beads generally get larger over time, the accuracy for measuring this change is not yet refined enough to make it worthwhile (Wiberg 2005: 5-6). Nor did I differentiate between *Saxidomus* or *Tresus* clamshells. Beads of both these genera were found in

[14] While Bennyhoff and Hughes (1987) is authoritative, two other excellent and straightforward sources for analytical terminology and illustrations related to shell bead analysis are available: Gibson (1992) and Milliken and Schwitalla (2009 especially pp. 10-11). In particular, Gibson's bead descriptions are accessible and synthetic.

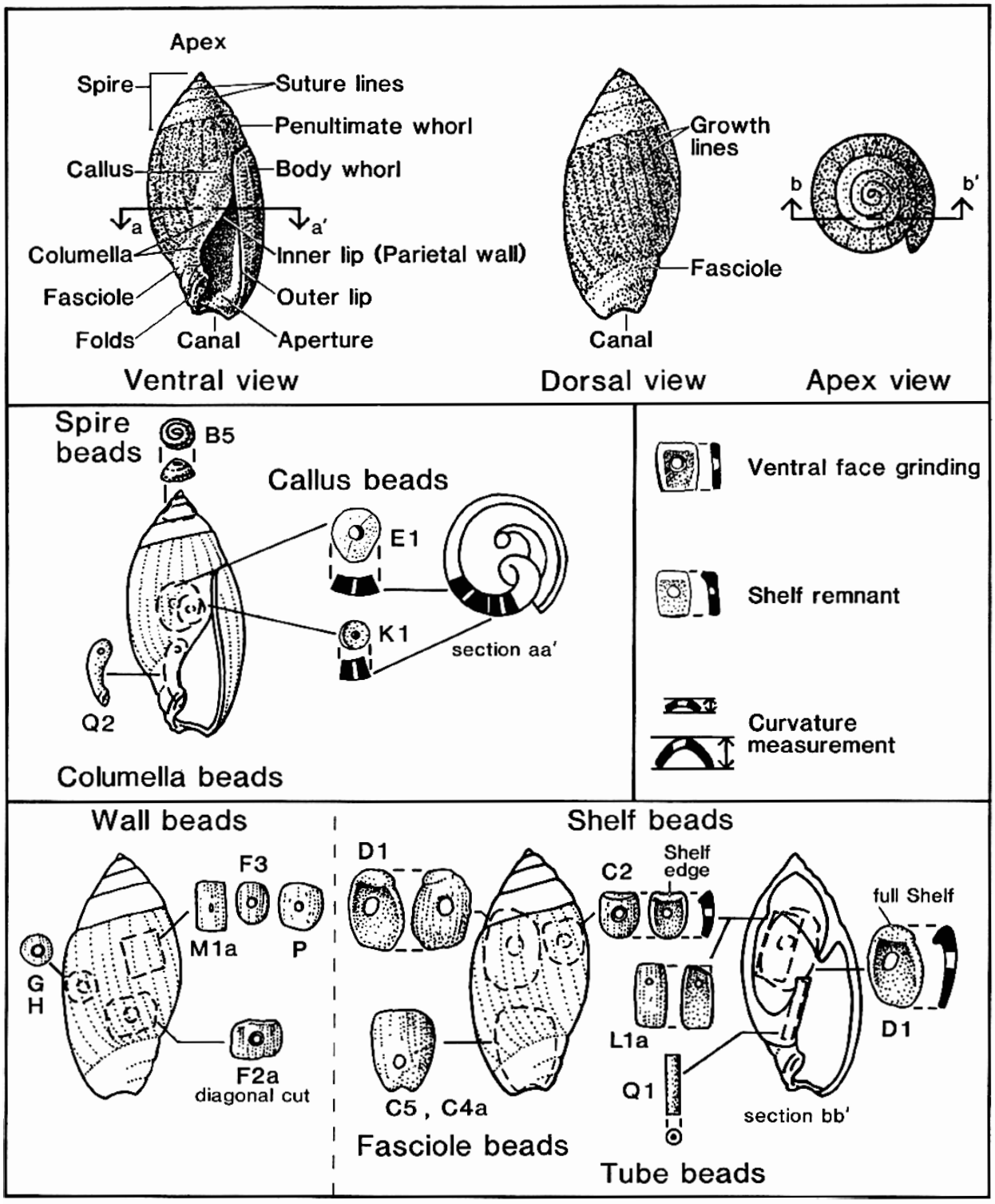


Figure 3.7 Landmarks and bead positions on *Olivella* shell. Adapted from Bennyhoff and Hughed (1987:89).

all contexts, but, given the limited provenience information, little was to be gained by recording this information.

I conducted two types of statistical inquiry following my typological analysis of the shell beads. First, I organized the beads into relative proportions of clam and *Olivella* shell beads for each context and each site. Graphing the relative proportions of clam and *Olivella* shell beads from the three contexts provided an adequate level of resolution to compare archaeologically derived hypotheses and ethnographic accounts.

The second statistical tool I employed was cluster analysis using SPSS (Inc.) Grad-Pack Version 18. Here, beads were organized according to the following rubric for each site: 1) all beads from all contexts; 2) an aggregation of clam and *Olivella* beads from all contexts; 3) *Olivella* beads from all contexts organized by type; 4) *Olivella* beads from all contexts organized by subtype; 5) a simple aggregation of clam and *Olivella* beads from burial contexts; 6) *Olivella* beads from burial contexts organized by type; 7) *Olivella* beads from burial contexts organized by subtype.[15] I employed cluster analysis to test if the Bennyhoff and Hughes (1987) typology created meaningful categories, or at the very least, if the detailed resolution of their typology was potentially useful for addressing questions other than fine chronological distinctions.

BEAD TYPES:

First, I provide a concordance chart (Table 3. 3) that shows the variety of names that have been given to different beads found in this dissertation by key researchers throughout the history of California bead studies. The table also traces changes in chronological assignment for each bead type. This chart compiles data from Bennyhoff and Hughes (1987: 149), Gibson (1992: 6, 17), and Groza (2002: 95).[16] Second, I include brief descriptions and images of the bead types I encountered during analysis.[17] My descriptions are adapted from Bennyhoff and Hughes (1987), Gibson (1992), and Weiberg (2005). Along with formal "class" designation for beads, I include the more common or colloquial names used in press or in conference papers. I also briefly I highlight characteristics that can help differentiate between related types and subtypes.

[15] Two important notes: First, in some cases, class is the finest distinction that can be made because further refinement by type or subtype does not exist in the Bennyhoff and Hughes (1987) typology (e.g.,J Class beads). In the cluster analysis of subtype, I retain the finest distinction the typology allows. Second, although all bead Types are presented in the summary data tables, I removed a total of 730 beads falling outside the Phase II - Historic period temporal range for statistical analysis.

[16] Only bead types relevant to this dissertation are included in Table 3.3.

[17] All photographs of *Olivella* shell beads shown in this section were taken by the author and are reproduced here with permission from the Phoebe A. Hearst Museum of Anthropology.

Table 3.3 Concordance Table of California Shell Bead Typologies

LILLARD ET AL. BEAD TYPE (1939)	*GIFFORD BEAD TYPE (1947)*	*BENNYHOFF & HUGHES COMMON BEAD NAME*	*BENNYHOFF & HUGHES BEAD TYPE (1987)*	*DATING SCHEME B1 (BENNYHOFF & HUGHES 1987)*	*DATING SCHEME B1*	*DATING SCHEME D (GROZA 2002)*
1a, 1b, 1c	F4, F5b	Spire-lopped, small, medium, & large	A1 (a, b, c)	All Time Periods		
1c		Oblique spire-lopped	A2	All Time Periods		
		Applique spire-lopped	A5	Phase IIA Late Period	1500-1600	
1a, 1b	F5b	End Ground	B2	Early Period & Phase I Late Period	900-1500	
	G1c	Spire	B5	Phase IB Late Period - Historic Period	1100-1816	
3b1	X1b, X2b	Split	C	Middle Period and Adjacent Transitions	500-950	B.C. 210-A.D. 420 (C2, C3)
3a2, 3a3	X1a, X2a	Split-punched	D	Middle/Late Transition	100-300	1010-1210 (D1a, D2)
3a1	X3b2	Thin lipped	E1	Phase IIA Late Period	1500-1700	1510-1700
3a1	X3b2, X3b1, X5a, X1b, X2b	Thick Lipped	E2	Phase IIB Late Period	1700-1800	1510-1700 (E2a, E2a2)
3a1	X3b2, X2b, X3b1	Large Lipped	E3	Historic Period	1700-1880	1510-1700 (E3a, E3b)
3d	X3b1	Tiny Saucer	G1	All Time Periods		
3c	X3b1	Saucers	G2, G3	Middle Period	900-100	445-575 (G2a, G2b)
3d	X3b1, X5a	Rough disc	H1, H2	Mission Period	1770-1834	1700-1720 (H1a)
3c	X3b1	Ground wall disc	J	Phase IIA Late Period	1500-1600	
3e	X4, X5b	Cup	K1, K2	Phase I Late Period, Phase II Late Period	900-1500	1390-1510 (K1)
3e	X4, X5b	Cylinder	K3	Phase IIA Late Period	1600-1700	
2a	X2a, X3a1-3	Sequin	M1	Phase I Late Period	900-1500	1010-1510 (M1a)
2a	X2a, X3a1-3	Pendant	M2, M3, M4	Phase IB,C - Phase IIA Late Period	1200-1770	1265-1510 (M2a)

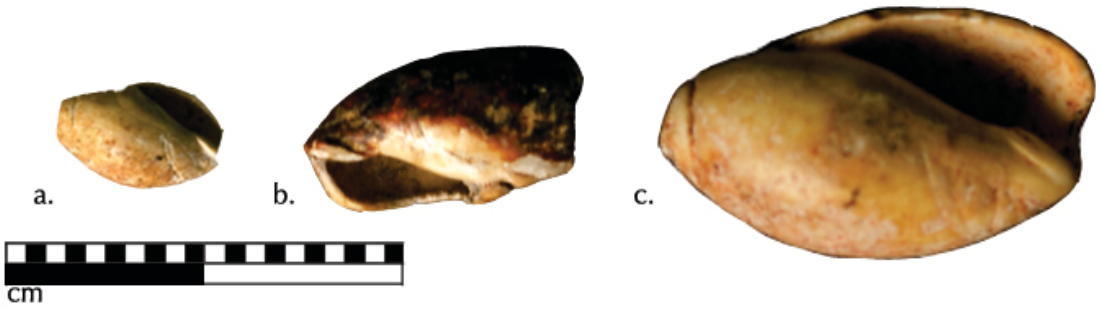


Figure 3.8 Spire lopped *Olivella* beads: a. Type A1a (Catalog # 1-25642); b. Typle A1b (Catalog # 1-73553); c. Type A1c (Catalog #1-25642).

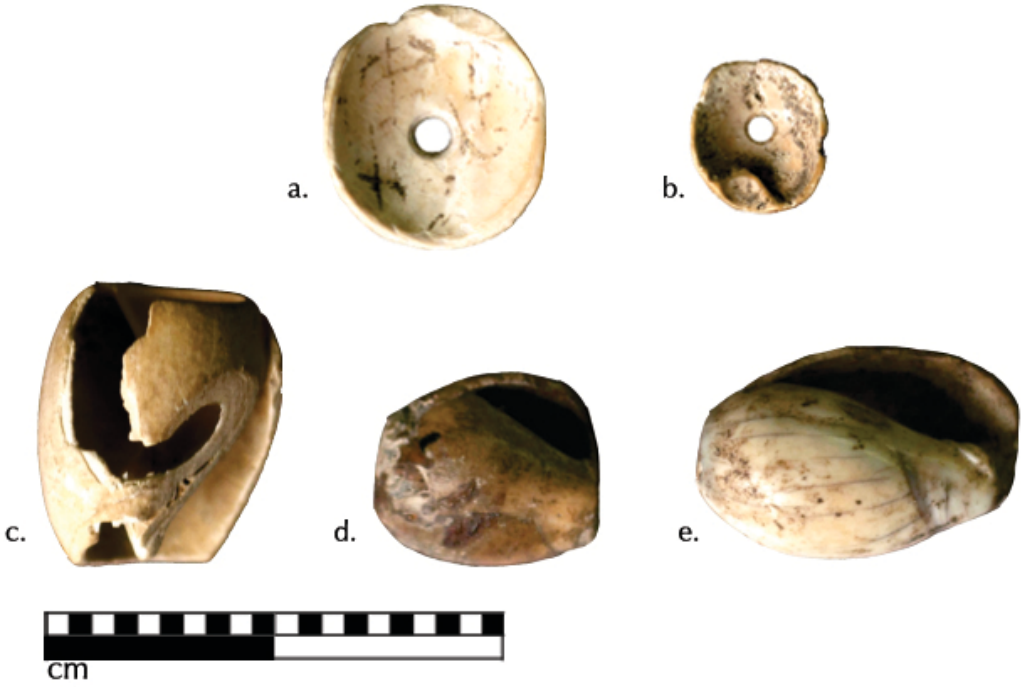


Figure 3.9 End ground *Olivella* beads: c. Type B1c (Catalog #1-339917); d. Type B2b (Catalog #1-39216); e. Type B2c (Catalog #L-17476). Split *Olivella* beads: a., b. Type C2 (Catalog #1-53956).

CLASS A (SPIRE LOPPED). Class A beads are the simplest and most common type of bead found in Californian archaeological contexts (Figure 3.8). Class A beads are made by grinding or chipping off the spire of the shell; there are three subtypes of Class A beads. Following Bennyhoff and Hughes (1987), distinctions between types were made based on relative diameter as measured at the widest part of the bead: A1a (3 mm - 6.5 mm), A1b (6.51 mm - 9.5 mm), and A1c (9.51 mm - 14 mm). Bead size from small to large parallels the temporal progression from Early to Late/ Protohistoric periods. Sometimes it is difficult to distinguish a Class A bead made by human hands from *Olivella* shell beads that lost their spires naturally. On most of the Class A shell beads there is some evidence that callus that has been modified. Close attention needs to be paid to distinguishing between naturally broken or deteriorated *Olivella* shell and intentionally manufactured Class A beads.

CLASS B (END-GROUND). Class B beads span a range of archaeological time periods (Table 3.3) and in most cases are distinguished from Class A beads by having a greater amount of their spire and aperture removed (Figure 3.9). This end-grinding generally gives B beads a distinctive "square," "boxy," or "barrel" appearance. There are six different Types of Class B beads (B1-B6). With subtypes B2 and B3 the type is further divided according to size (B2a, B2b, B2c). There are no further divisions. The exception to the general barrel shape of Class B beads is type B5. These are conically shaped beads made exclusively from the spire of the *Olivella* shell; 362 Class B beads were identified during analysis. Despite their typically earlier date (Early Period and Phase I of the Late Period), all B beads and were found with Phase II beads (clam and E Class) and were included in analysis.

CLASS C (SPLIT). Class C beads are simple split beads, made up of one half of an *Olivella* shell (Figure 3.9). Usually they have one half or one quarter of the interior shelf present and all edges of the bead are ground smooth. They date to the Early and Middle Periods. Only two Class C beads were identified during analysis of site CA-SAC-6. Because they date to the Terminal Middle Period, (C2), they are not included in this analysis, but are included in Figure 3.9 for reference.

CLASS E (LIPPED). Class E beads are generally described as "round to oval beads normally made from the upper callus/inner lip and variable amounts of adjacent body whorl" (Bennyhoff and Hughes 1987: 127). There are several key characteristic questions to ask when analyzing Class E beads; whether they are lipped or lipless, thin or thick lipped, and how much of the callus and shelf edge is present on the interior of the bead. The curvature is also an important variable in distinguishing between types. Class E beads have several types (E1, E2, and E3) that are divided into time-sensitive subtypes (Figure 3.10). For example, E1a beads are marker types for Phase II of the Late Period and are common at the beginning of the protohistoric period (A.D. 1500-1600). E2b beads are also indicators for Phase II of the Late Period but continue into the Historic Period and E3a beads are marker types for the Historic Period. Class E beads were the most commonly represented Class identified during my analysis. Characteristics important for distinguishing between types include: thin versus thick lips, curvature, "stackablility," and uniformity of shape - whether the bead is consistently ovular or has irregularities.

CLASS G (SAUCER). Class G beads are generally known as "wall" discs because they are made from the wall or body of the *Olivella* shell (Figure 3.11). G beads are circular and sometimes oval and have a central hole. The curvature of G beads are much more shallow than that of E beads (.7 mm - 1.2 mm) (Gibson 1992: 30). Additionally the edges of G beads are always ground smooth. This characteristic helps distinguish them from some similarly sized and shaped, but later, H beads with rough, unground edges, or J and sometimes C3 beads. G1 and G2 beads were occasionally found in the sample. Class G beads were manufactured from the Middle Period into the Late Period but are not temporally diagnostic. Sometimes the most efficient way to tell the difference within and between this and other similar Classes of beads is to compare the ratio of the center hole to the surrounding shell body.

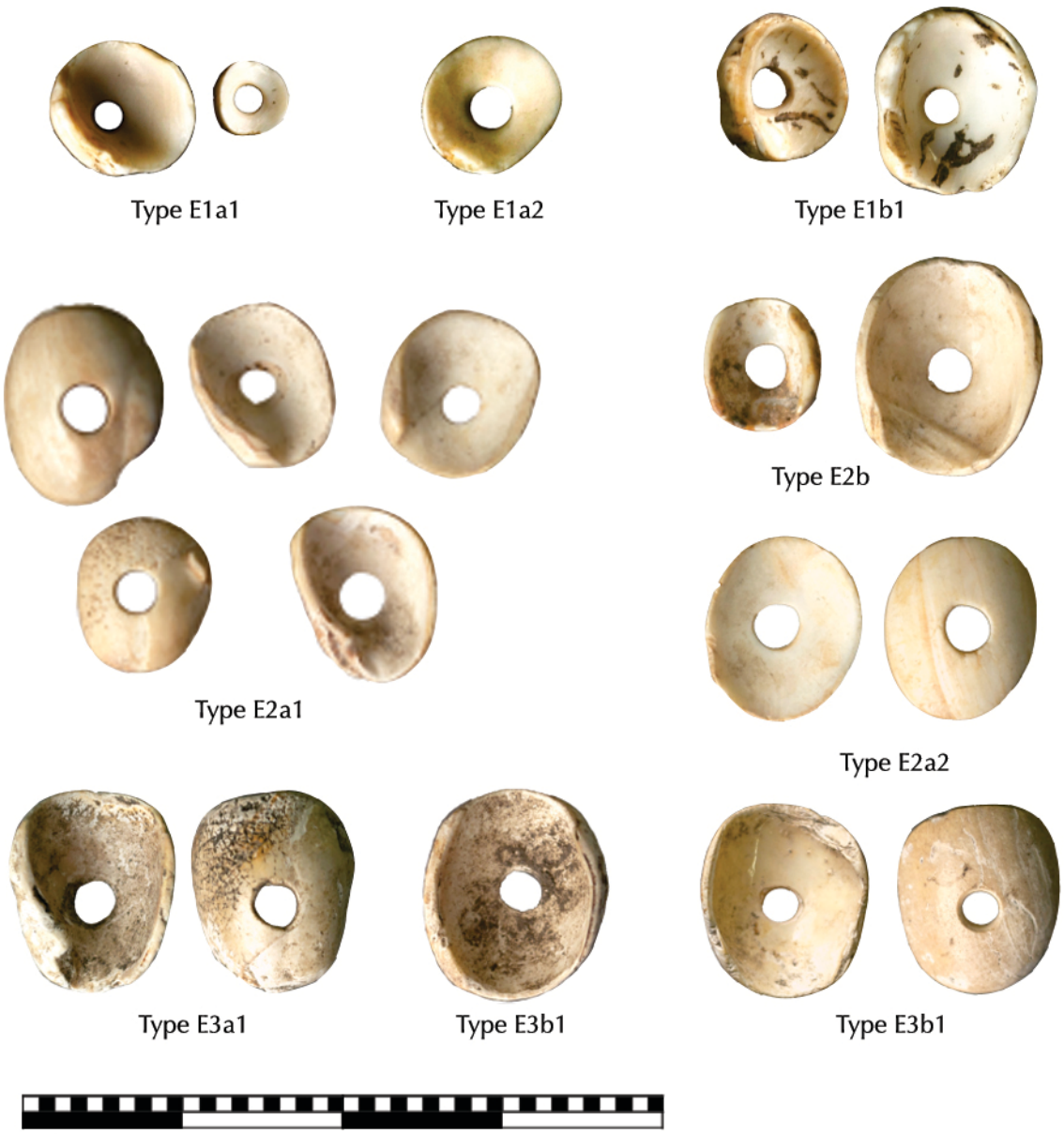


Figure 3.10 Lipped *Olivella* beads: Type E1a1 (Catalogue #L-16576h); Type E1a2 (Catalog #L-16576h); Type E1b1 (Catalog #1-33721a1); Type E2a1 (Catalog #L-15691a); Type E2b (Catalog #L-15576a); Type E2a2 (Catalog #L-15691b); Type E3a1 (Catalog #L-15691b); Type E3b1 (Catalog #1-54321b); Type E3b1 (Catalog #1-54321b)

CLASS H (DISK). Class H beads are circular, shallow, beads similar to Class G beads, but have much smaller and more uniform diameter center holes (the side walls of the center hole are always perpendicular) (Figure 3.11). This smaller and more uniform central hole is the result of trade with the Missions for metal drill bits. The parallel walls of the center hole of a Class H bead are the simplest way to distinguish it from J or sometimes a G1 beads (sometimes this misattribution can be made when first sorting large numbers of small, disk shaped beads). Class H beads first appear during the early Franciscan Mission Period and were made through the nineteenth century (A.D. 1770-1900). The diameter of Class H beads progressively increases in diameter and from smooth ground edges to rough chipped edges over time (H1a-H3).

CLASS J (WALL DISK). Class J beads are also made from the wall of the *Olivella* shell (Figure 3.11). Although they appear similar to Class G beads they are different in that they retain a slight shelf edge on the interior of the shell and they can sometimes appear to be slightly more ovular than J, C, or G beads. Missing this subtle difference often results in their being confused for the lipless Class E bead or the type G2 bead. These beads are temporal markers for the Protohistoric period in the San Joaquin Valley (Bennyhoff and Hughes 1987: 136).

CLASS K (CUPPED/ BUSHING). Class K beads are small thick circular beads with a large central hole in comparison to the outside diameter resulting in a thin wall (Figure 3.11). They are made from the upper callus of the shell and the suture lines are usually visible. An easy way to tell distinguish K1 beads from E1a1 beads is by stacking them. K1 beads will stack uniformly whereas E1a1 beads do not. There are three types of Class K beads, K1, K2, and K3. K1 beads are a marker type for Phase I of the Late Period and can be easily misidentified as Type E1a1. There are two key differences that distinguish K beads from one another. K1 beads stack neatly together. Because the diameter of K1 beads ranges from 3-7 mm it is the "cupped" characteristic of the beads that prevents mistaking the K1 bead for a K2 or K3 bead. Additionally, K2 beads are smaller than K1's. They have a diameter only 3-4 mm with a fairly small center hole and thick wall. I found these beads to have a "boxy" appearance to them and found several that had been inserted into the center hole of large clam or steatite beads - hence their monicker as "bushing" beads. K2 and K3 beads are markers for Phase 2 of the Late Period. K3 beads are the smallest K Type and have a large center hole in comparison to their overall diameter (2-3 mm) making the wall of the bead thin.

CLASS M (SEQUIN). Class M beads are thin rectangular beads with holes drilled either in the middle of the bead or toward one end (Figure 3.11). They can have sharply defined corners, a "rounded" top (nearest where the hole is drilled), or and "angled" top. There are four types (M1 – M4) and several subtypes within each Type. Class M beads are only found north of San Louis Obispo (Gibson 1992: 32) and are generally a marker type for Phase I of the Late Period, although in some cases they are found in contexts dating to Phase II of the Late Period. These types of beads were often sewn to baskets and clothes in overlapping or shingled manner. For this reason they are often referred to as "sequin" beads. Of all the Classes of *Olivella* shell beads besides Class A, Class M beads are one of the easiest to categorize using the Bennyhoff and Hughes (1987) text.

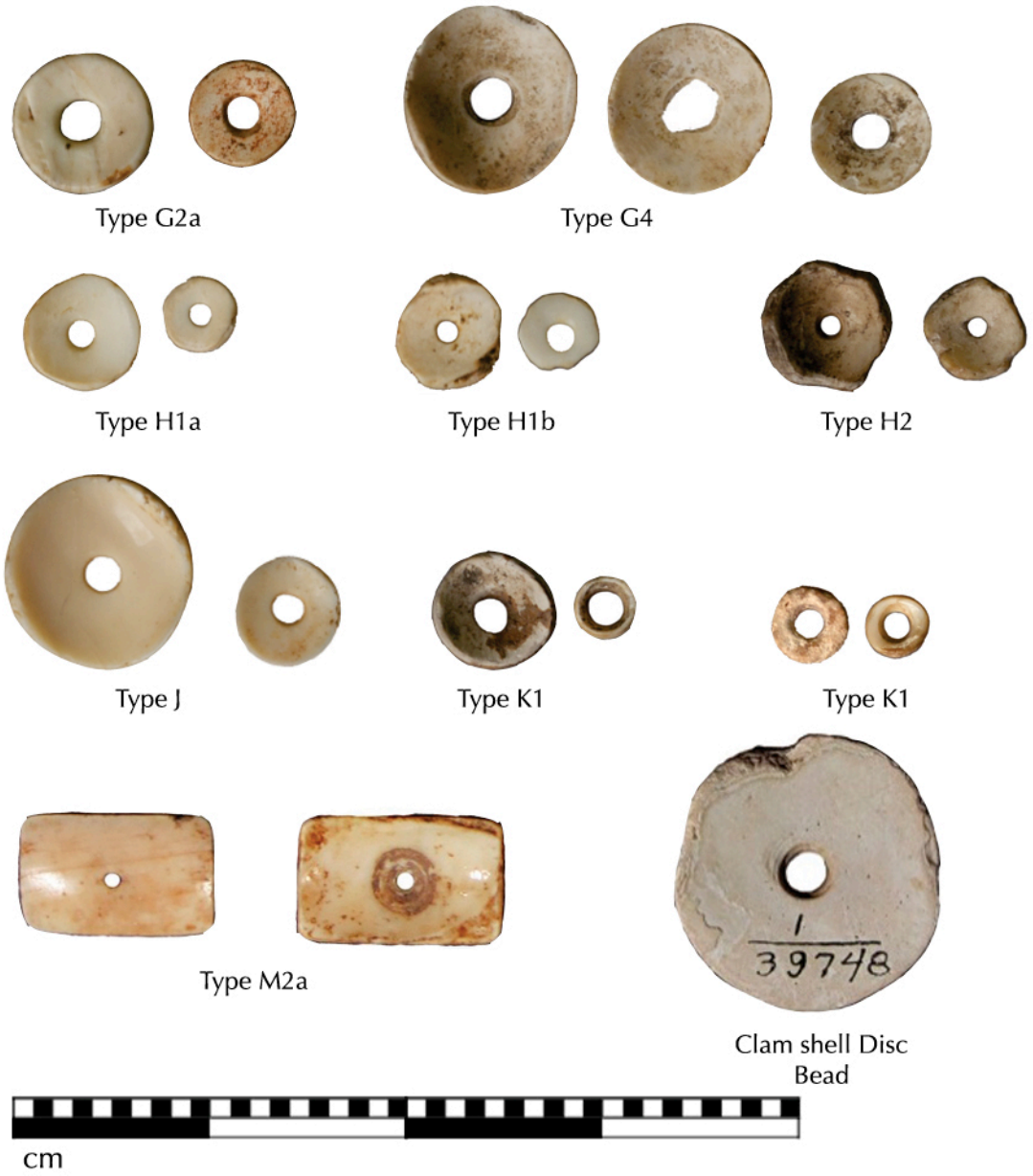


Figure 3.11 Saucer *Olivella* beads: Type G2a (Catalog #1-39912c) and Type G4 (Catalog# F.N.I.). Disc *Olivella* beads: Type H1a (Catalog #L-20080e), Type H1b (Catalog #L-20080e2), and Type H2 (Catalog #B-45). Wall disk *Olivella* beads: Type J (Catalog #1-52379). Cupped/Bushing *Olivella* beads (from left to right): Type K1 (Catalog #L-17505) and Type K1 (Catalog #L-17505). Sequin *Olivella* beads: Type M2a (Catalog #1-82142a). Clam shell beads: Catalog #1-39748.

CLAM SHELL BEADS. Clamshell beads in Central California were most commonly manufactured from *Saxidomus nuttalli* (Washington clam) *Saxidomus giganteus* (butter clam), or *Tresus nuttalli* (gaper clam) shell (Figure 3.11) (Gifford 1947). The only fully articulated clamshell typology is that of Bennyhoff and Fredrickson in 1969 (Bennyhoff and Fredrickson 1994). This is a complicated typology that requires extensive measuring of each bead before assignment to a type can be made. A second and more simple typology was developed by Milliken (Weibeg 2005: 6-8) based on the work of Bennyhoff and Fredrickson. This second typology has seven broad categories and a few subtypes that account for size in the same way as the typology of Bennyhoff and Hughes (1987). Milliken recorded diameter, and the presence or absence of ground edges, chipped edges, blanks, and cylinders. The goal of Milliken's typology was to test Bennyhoff and Fredrickson's (1994) presumption that variations in clamshell bead size varied regularly through time and thus that clam shells, along with *Olivella* shells could be used as chronological markers. While Milliken found the general trend to be true --that bead size increased through time-- he concluded that there were no "natural" boundaries between clamshells of different sizes. For the purposes of this study, I combined all clamshell subcategories since the original excavators of Yolo-69 noted, "In the big picture, however, fine size sorting is probably not necessary to answer important research questions" (Wiberg 2005: 6). Thus, for this study, only approximate size (small, medium, large) was noted during identification, and all sizes were collapsed into the general clamshell category during analysis. Regardless of size, clamshell beads were present only from the Late to the Historic Period.

RESULTS

In this section I summarize the results of my *Olivella* shell bead analysis. The results of this analysis are presented in Tables 3.4 - 3.18 below. In respect for sensitive and ceremonial nature of the shell beads I analyzed, I do not include the Excel data spreadsheets containing catalogue numbers. If further information is needed to replicate this study, the original data can be obtained from the Phoebe A. Hearst Museum of Anthropology at the University of California, Berkeley with permission from the Tachi Yokuts tribe, or from the Tachi Yokuts Cultural Department. The summary tables of the shell beads analyzed from each site are in a geographic order, beginning in at the southernmost end of San Joaquin County and proceeding north. For each site, the data are presented according to the following format: burial contexts, non-burial context, all contexts.

Table 3.4 Summary of CA-SJo-105 Shell Beads in Burial Contexts

TYPE	TIME PERIOD	COUNT	PERCENT
A1	All Time Periods	139	4%
B2	Phase Ia Late Period	28	1%
Clam	Phase II Late Period	1582	47%
E1	Phase IIa Late Period	91	3%
E2	Phase IIb Late Period	176	5%
E3	Historic	65	2%
H1	Mission	1004	30%
J	Not Temporally Diagnostic	6	0%
K1	Phase Ib Late Period	123	4%
K2	Phase II Late Period	100	3%
M2	Phase Ic Late Period	27	1%
Glass	Historic	26	1%
Totals:		**3,367**	**101%**

Table 3.5 Summary of CA-SJo-105 Shell Beads in Non-Burial Contexts

TYPE	TIME PERIOD	COUNT	PERCENT
A1	All Time Periods	7	2%
Abalone	All Time Periods	14	4%
Clam	Phase II Late Period	103	31%
J	Not Temporally Diagnostic	207	63%
Totals:		**331**	**100%**

Table 3.6 Summary of All Shell Beads at CA-SJo-105

TYPE	TIME PERIOD	COUNT	PERCENT
A1	All Time Periods	146	4%
B2	Phase I Late Period	28	1%
Clam	Phase II Late Period	1685	46%
E1	Phase IIa Late Period	91	2%
E2	Phase IIb Late Period	176	5%
E3	Historic	65	2%
H1	Mission	1004	27%
J	Not Temporally Diagnostic	213	6%
K1	Phase Ib Late Period	123	3%
K2	Phase II Late Period	100	3%

Table 3.7 Summary of CA-SJo-42-45 Shell Beads in Non-Burial Contexts

TYPE	TIME PERIOD	COUNT	PERCENT
A1	All Time Periods	1,034	10%
A4	Not Temporally Diagnostic	10	0%
B2	Phase Ia Late Period	6	0%
Clam	Phase II Late Period	7,860	79%
E1	Phase IIa Late Period	443	4%
E2	Phase IIb Late Period	72	1%
G2	Not Temporally Diagnostic	5	0%
G4	Not Temporally Diagnostic	5	0%
K1	Phase Ib Late Period	4	0%
K2	Phase II Late Period	40	0%
L2	M?	3	0%
M2	Phase Ic Late Period	418	4%
Totals:		**9,900**	**98%**

Table 3.8 Summary of CA-SJo-42-45 Shell Beads in Burial Contexts

TYPE	TIME PERIOD	COUNT	PERCENT
A1	All Time Periods	140	9%
A5	Proto-Historic &Historic	157	10%
B5	Phase I Late Period	305	19%
Clam	Phase II Late Period	650	40%
E1	Phase IIa Late Period	219	13%
E2	Phase IIb Late Period	6	0%
K2	Phase II Late Period	18	1%
M1	Phase Ia & b Late Period	2	0%
M2	Phase Ic Late Period	132	8%
Totals:		**1,629**	**100%**

Table 3.9 Summary of All Shell Beads at CA-SJo-42-45

TYPE	TIME PERIOD	COUNT	PERCENT
A1	All Time Periods	1,174	10%
A4	Not Temporally Diagnostic	10	0%
A5	Proto-Historic & Historic	157	1%
B2	Phase Ia Late Period	6	0%
B5	Phase I Late Period	305	3%
Clam	Phase II Late Period	8,510	74%
E1	Phase IIa Late Period	662	6%
E2	Phase IIb Late Period	78	1%
G2	Not Temporally Diagnostic	5	0%
G4	Not Temporally Diagnostic	5	0%
K1	Phase Ib Late Period	4	0%
K2	Phase II Late Period	58	1%
L2	M?	3	0%
M1	Phase Ia & b Late Period	2	0%
M2	Phase Ib Late Period	550	5%
Totals:		**11,529**	**100%**

Table 3.10 Summary of CA-SAC-6 Shell Beads in Non-Burial Contexts

TYPE	TIME PERIOD	COUNT	PERCENT
A1	All Time Periods	5,666	4%
Abalone	Not Temporally Diagnostic	387	0%
Clam	Phase II Late Period	103,084	74%
C2	Middle Period	2	0%
E1	Phase IIa Late Period	7,300	5%
E2	Phase IIb Late Period	2,270	2%
E3	Phase IIb Late Period	154	0%
G	Middle Period	92	0%
Glass	Historic	14,707	11%
H1	Mission	12	0%
K1	Phase Ib Late Period	539	0%
K2	Phase II Late Period	176	0%
K3	Phase II Late Period	374	0%
M1	Phase Ia & b Late Period	66	0%
M2	Phase Ib Late Period	3,894	3%
M4	Proto-Historic	8	0%
Totals:		**138,731**	**99%**

Table 3.11 Summary of CA-SAC-6 Shell Beads in Burial Contexts

TYPE	TIME PERIOD	COUNT	PERCENT
A1	All Time Periods	681	5%
Abalone	Not Temporally Diagnostic	39	0%
Clam	Phase II Late Period	8,854	69%
E1	Phase IIa Late Period	115	1%
E2	Phase IIb Late Period	201	2%
E3	Phase IIb Late Period	13	0%
Glass	Historic	268	2%
H1	Mission	11	0%
K1	Phase Ib Late Period	916	7%
K1i	Phase Ib Late Period	3	0%
K2	Phase II Late Period	206	2%
K3	Phase II Late Period	1	0%
M1	Phase Ia & b Late Period	282	2%
M2	Phase Ib Late Period	1278	10%
M4	Proto-Historic	13	0%
Totals:		**12,881**	**100%**

Table 3.12 Summary All Shell Beads at CA-SAC-6

TYPE	TIME PERIOD	COUNT	PERCENT
A1	All Time Periods	6,347	4%
Abalone	Not Temporally Diagnostic	426	0%
B2	Phase I Late Period	22	0%
Clam	Phase II Late Period	111,938	74%
C2	Middle Period	2	0%
E1	Phase IIa Late Period	7,415	5%
E2	Phase IIb Late Period	2,471	2%
E3	Phase IIb Late Period	167	0%
G	Middle Period	92	0%
Glass	Historic	14,975	10%
H1	Mission	23	0%
K1	Phase Ib Late Period	1,455	1%
K1i	Phase Ib Late Period	3	0%
K2	Phase II Late Period	382	0%
K3	Phase II Late Period	375	0%
M1	Phase Ia & b Late Period	348	0%
M2	Phase Ib Late Period	5,172	3%
M3	Proto-Historic	7	0%
M4	Proto-Historic	21	0%
Totals:		**151,641**	**99%**

Table 3.13 Summary of CA-YOLO-69 Shell Beads in Non-Burial Contexts

TYPE	TIME PERIOD	COUNT	PERCENT
A1	All Time Periods	3	0%
Clam	Phase II Late Period	1,126	99%
G	Not Temporally Diagnostic	4	0%
H1	Mission	7	1%
H2	Terminal Mission	1	0%
Totals:		**1,141**	**100%**

Table 3.14 Summary of CA-YOLO-69 Shell Beads in Burial Contexts

TYPE	TIME PERIOD	COUNT	PERCENT
A1	All Time Periods	303	1%
B5	Not Temporally Diagnostic	1	0%
Clam	Phase II Late Period	23,399	70%
E2	Phase IIb Late Period	61	0%
E3	Historic	4	0%
G	Not Temporally Diagnostic	4,053	12%
H1	Mission	5,250	16%
H2	Terminal Mission	149	0%
J	Not Temporally Diagnostic	5	0%
M3	Proto-Historic	5	0%
M4	Proto-Historic	156	0%
Totals:		**33,386**	**99%**

Table 3.15 Summary All Shell Beads at CA-YOLO-69

TYPE	TIME PERIOD	COUNT	PERCENT
A1	All Time Periods	306	1%
B5	Not Temporally Diagnostic	1	0%
Clam	Phase II Late Period	24,525	71%
E2	Phase IIb Late Period	61	0%
E3	Historic	4	0%
G	Not Temporally Diagnostic	4,057	12%
H1	Mission	5,257	15%
H2	Terminal Mission	150	0%
J	Not Temporally Diagnostic	5	0%
M3	Proto-Historic	5	0%
M4	Proto-Historic	156	0%
Totals:		**34,527**	**99%**

Table 3.16 Summary of CA-YOLO-182 Shell Beads in Non-Burial Context

TYPE	TIME PERIOD	COUNT	PERCENT
A1	All Time Periods	15	6%
Clam	Phase II Late Period	209	83%
F2? Or E?	Intermediate Middle Period	1	0%
Undrilled	Not Temporally Diagnostic	27	11%
Totals:		**252**	**100%**

Table 3.17 Summary of CA-YOLO-182 Shell Beads in Burial Context

TYPE	TIME PERIOD	COUNT	PERCENT
Clam	Phase II Late Period	1,166	99%
G1	Not Temporally Diagnostic	4	0%
J	Not Temporally Diagnostic	1	0%
Undrilled	Not Temporally Diagnostic	1	0%
Totals:		**1,172**	**99%**

Table 3.18 Summary of All Beads from CA-YOLO-182

TYPE	TIME PERIOD	COUNT	PERCENT
A1	All Time Periods	15	1%
Clam	Phase II Late Period	1,375	97%
F2? Or E?	Intermediate Middle Period	1	0%
G1	Not Temporally Diagnostic	4	0%
J	Not Temporally Diagnostic	1	0%
Undrilled	Not Temporally Diagnostic	28	2%
Totals:		**1,424**	**100%**

OBSERVATIONS

In this section I first present the results of my analysis of the relative proportions of clam and *Olivella* beads at each site. Next I describe the results of the cluster analysis. A total of 187,548 *Olivella* and clam shell disk beads were analyzed from five archaeological sites dating to the Late Period in the San Joaquin and Sacramento River Delta. Beads were identified and dated according to the Bennyhoff and Hughes (1987) typology and chronology; subsequent chronological adjustments made by Groza (2002) were also taken into account. During typological analysis I organized the beads according to three contexts: burial, non-burial, and all contexts. For the purposes of statistical analysis I selected only beads dating from Phase II of the Late Period through the Historic Period. This resulted in the removal of 730 beads that were typical of earlier periods from the 'all contexts' grouping. Beads from earlier periods were also found in burial contexts but were retained for analysis because they were associated with Phase II beads and were assumed to be heirlooms. I conducted my analysis in two stages. In the first stage, I divided the beads into two broad categories (*Olivella* and clam) at each of the five sites and according to context (burial, non-burial, and all). I then plotted the relative proportions of beads found at each site within each context across the Delta region according to their geographic location along a north/south axis (left to right on the graphs below).

Table 3.19 shows the total number of clam and *Olivella* beads analyzed from each site. Despite the variation in sample size, the relative proportions remain constant. On the one hand, the proportion of *Olivella* shell beads decreases along a south to north trajectory. On the other hand, the proportion of clam shell beads decreased along a north to south pathway. This trend is illustrated in Figure 3.12.

Table 3.19 *Olivella* and Clamshell Beads from All Contexts

SITE	*OLIVELLA*	CLAM	TOTAL
CA-SJo-42	3,009 (26%)	8,510 (74%)	11,529
CA-SJo-105	1,999 (54%)	1,685 (46%)	3,684
CA-Sac-6*	24,728 (18%)	111,938 (74%)	136,663
CA-Yol-69	10,002 (29%)	24,525 (71%)	34,527
CA-Yol-182	42 (3%)	1,375 (96%)	1,417
Total:	**39,780**	**148,033**	**187,820**

*** Note:** 14,975 glass beads listed in Table 2.12 above were removed for the comparison reflected in this table.

Table 3.20 illustrates the relative proportions of clam and *Olivella* beads excavated from non-burial contexts. The distribution of beads in this context is remarkably similar to the proportions found when burial and non-burial contexts were combined (see above). One explanation for this is that because the overwhelming majority of beads I analyzed were from non-burial contexts this skews the data. This trend is illustrated in Figure 3.13.

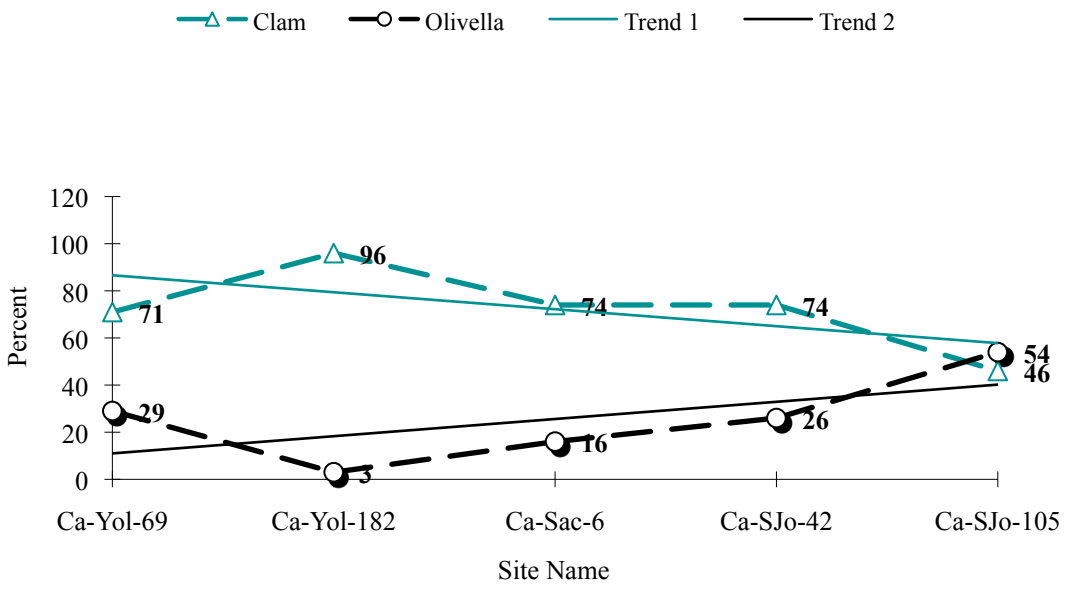


Figure 3.12 Proportions of clam and *Olivella* shell beads from all contexts. Organized by site, from north to south (left to right).

Table 3.20 *Olivella* and Clamshell Beads from Non-Burial Contexts

SITE	*OLIVELLA*	CLAM	TOTAL
CA-SJo-42	2,030 (20%)	7,860 (79%)	9,890
CA-SJo-105	214 (58%)	103 (31%)	317
CA-Sac-6	19,866 (14%)	103,084 (74%)	124,657
CA-Yol-69	15 (1%)	1,126 (99%)	1,141
CA-Yol-182	36 (15%)	209 (85%)	245
Total:	**22,161**	**112,382**	**136,250**

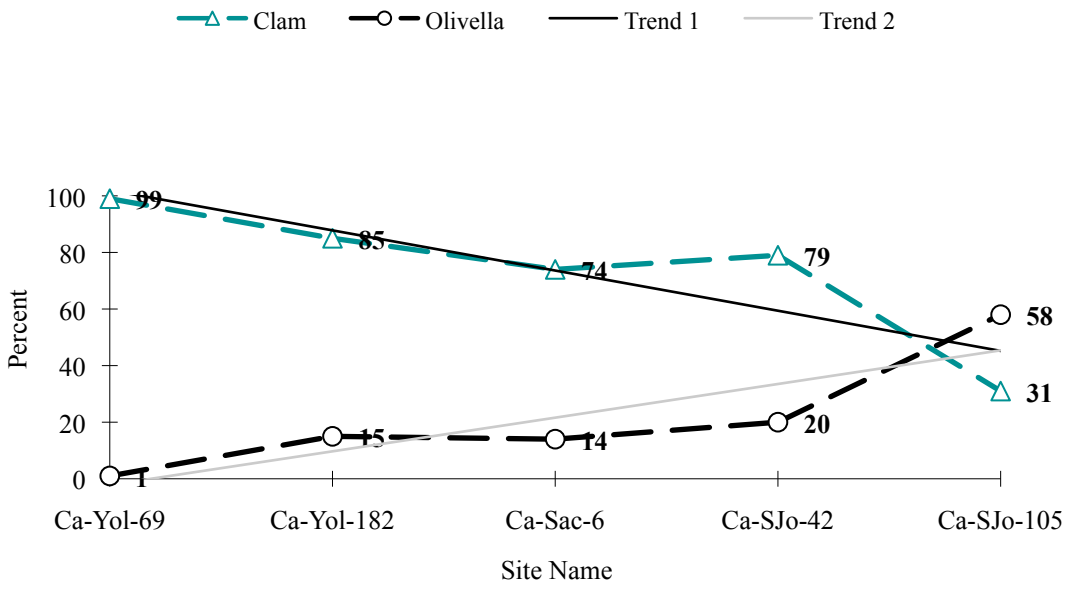


Figure 3.13 Proportion of *Olivella* and clamshell beads from non-burial contexts.

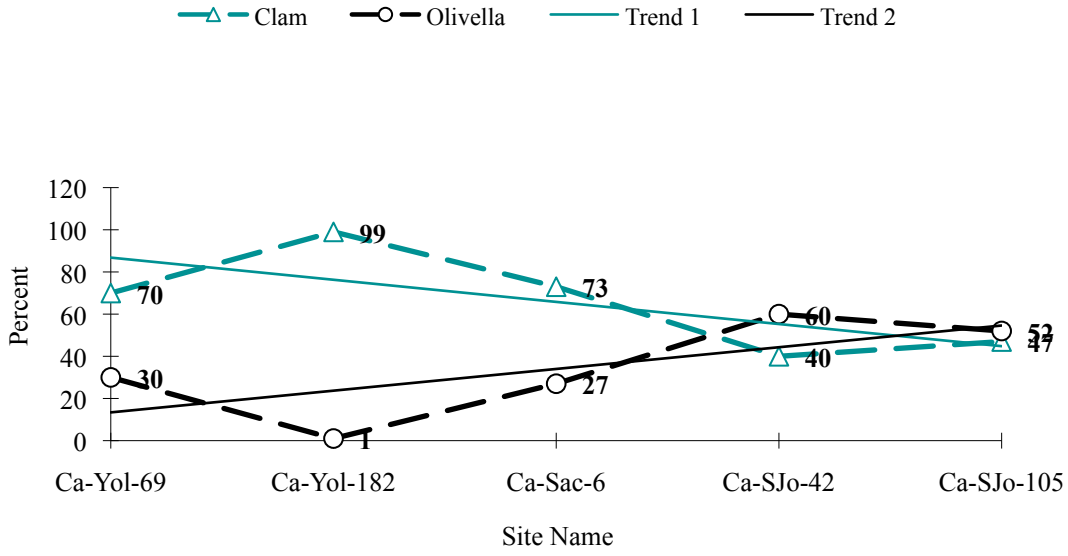


Figure 3.14 Proportion of *Olivella* and clamshell beads from burial contexts. Organized from north to south (left to right).

Despite the difference in sample size, the same trend is seen in the relative proportions of beads excavated from burial contexts (Figure 3.14, Table 3.21). This was unexpected. Initially, I anticipated items of non-local manufacture (*Olivella* shell beads) to occur with greater frequency in burials and thus indicate a reverse of the trend displayed above. However, this was not the case. Raw numbers of clam and *Olivella* shell beads analyzed from burial contexts support this conclusion (Table 3.21).

Table 3.21 *Olivella* and Clamshell Beads from Burial Contexts

SITE	*OLIVELLA*	CLAM	TOTAL
CA-SJo-42	979 (60%)	650 (40%)	1,629
CA-SJo-105	1,785 (52%)	1,582 (47%)	3,367
CA-Sac-6	4,597 (27%)	8,854 (73%)	13,451
CA-Yol-69	9,987 (30%)	23,399 (70%)	33,386
CA-Yol-182	6 (1%)	1,166 (99%)	1,172
Total:	**17,354**	**35,651**	**53,005**

For the second stage of statistical analysis, I conducted cluster analysis. As with the proportional analysis I divided the data for each site into three contexts (burial, non-burial, and all). Unlike the proportional analysis that combined shell and *Olivella*, here I focused exclusively on *Olivella* shell beads types and subtypes. I directed my attention toward the type and subtype categories to test whether or not their co-occurrence might reflect certain preferences within and between groups. Cluster analysis was chosen for three reasons. First, as a tool to explore the relationships between multiple variables, cluster analysis does not assume normality within a sample thus making it appropriate for this data set where the provenience information is limited and there are a large number of variables (Bernard 2002: 653). Second, although the results of cluster analysis do not provide cultural explanations for the data, it can create groupings that, in comparison with other data, may give insight into the preferences for particular bead types. Lastly, cluster analysis has the potential to "push" shell beads toward inferences about the social spheres in which they interacted. Before I conducted this analysis, I had no expectations. This analysis was purely exploratory.

Despite some of the shortcomings in working with museum collections (see chapter 1), the results of the six different cluster analyses, based on the frequencies of different bead types and subtypes, were remarkably consistent and showed a significant trend. In general, in each of the analyses there were two main clusters. First, CA-SJo-105 and CA-SJo-42 paired closely, and second, the Yolo and Sacramento County sites paired, but not as closely as the San Joaquin sites. This relationship changed slightly when conducting cluster analysis for the clam and *Olivella* shells in burial contexts. In this case, CA-SAC-6 and CA-YOL-182 appear to be more closely related to one another and CA-YOL-69 clusters at a greater distance. Nevertheless, the important separation between the San Joaquin and Sacramento and Yolo County sites remains. As a general point, it is important to note that the Yolo and Sacramento County sites clustered at greater distances as the level of typological specificity increased. This is in contrast to the San

Joaquin County sites that clustered at close distances at all levels of typological analysis. In the interest of space and expediency, I am including the results of only three of the clusters that produced interpretable results. The first graph represents the clustering of all beads from all contexts, the second illustrates the results from analyzing clam and *Olivella* beads from burial contexts, and the final graph shows the results from clustering by subtype of *Olivella* beads from burial contexts.

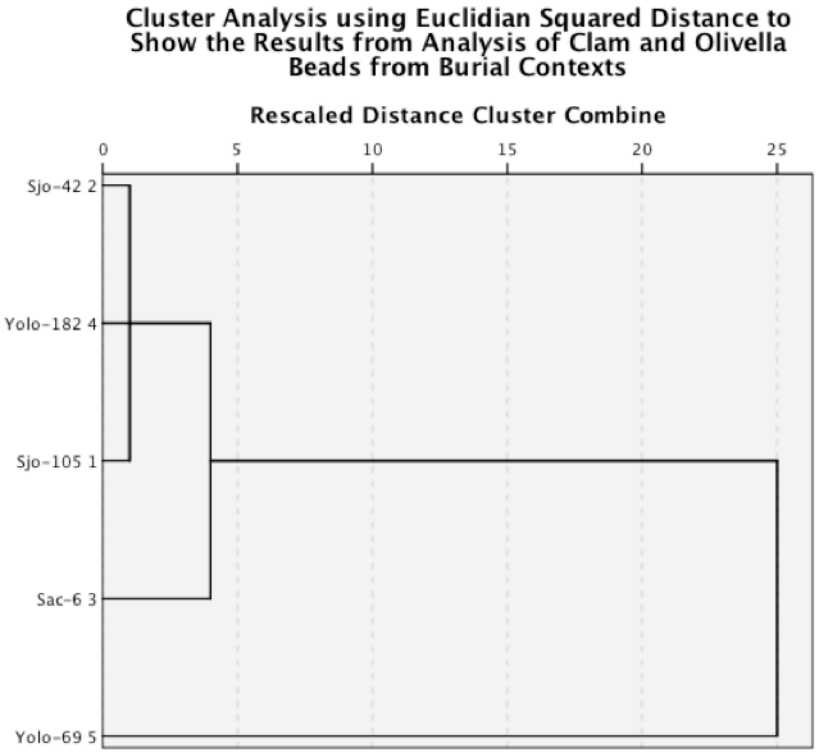


Figure 3.15 Cluster analysis of clam and *Olivella* beads from burial contexts.

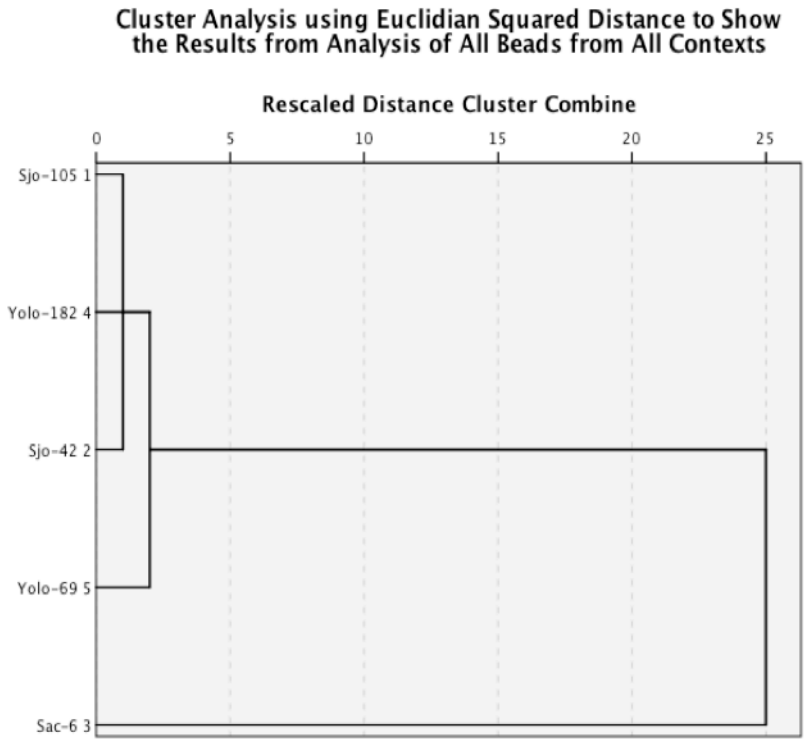


Figure 3.16 Cluster analysis of clam and *Olivella* beads (subtype) from burial contexts.

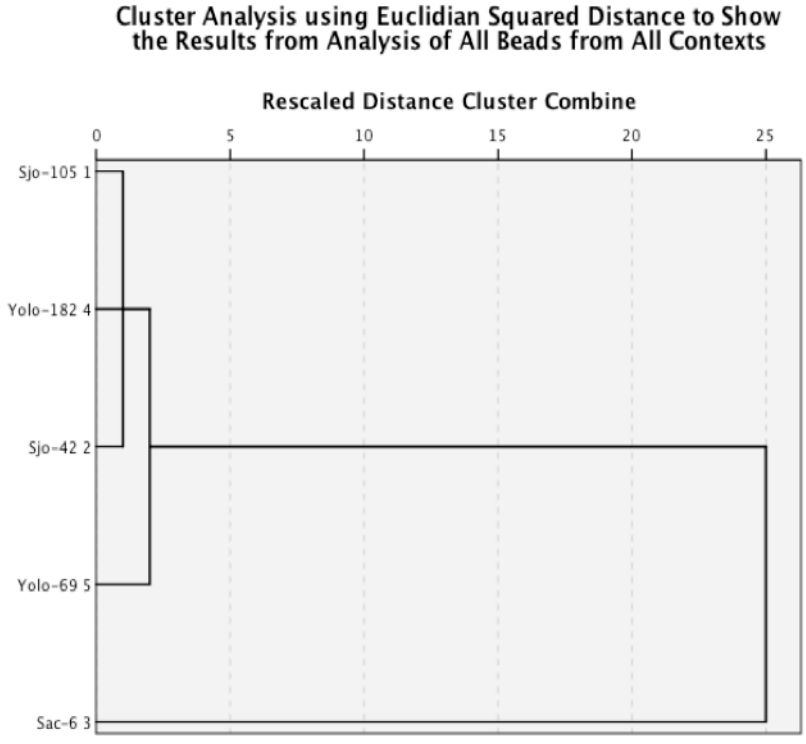


Figure 3.17 Cluster analysis of clam and *Olivella* beads from all contexts.

CONCLUSIONS

Olivella and clamshell beads are an integral part of the California archaeological record and provide key insights into the cultural dynamics of the Delta region. *Olivella* bead typologies have receive directed and sustained attention because of their clearly demonstrated utility as a relative dating tool in building local and regional chronologies (Bennyhoff and Hughes 1987; Gifford 1947; Jones and Klar 2007; Lillard et al. 1939; Milliken 2007, 2009; Moratto 1984). Building on recent scholarship (i.e. Hildebrant et al. 2009; Rosenthal n.d.), I demonstrate that even with an absence of detailed provenience information, shell bead analysis also provides important insights into, and broader understandings of, the cultural practices and intimate human interactions that created the archaeological record.

Given traditional archaeological assumptions about the way in which groups interact with one another across social and cultural boundaries, I expected these data to fit one of the following proposed boundary models as outlined in Chapter 2:

1. *Fixed*: Expect to see circumscribed patterns of distribution.
2. *Clinal*: Expect to see a gradient of in the distribution of objects.
3. *Temporally Fixed*: Expect to see circumscribed boundaries that shift according to chronological divisions.
4. *Temporally Clinal*: Similar to above, but changes should be gradual. This might be impossible to see.
5. *Punctuated*: Expect to see some types of objects circumscribed and others less constrained.

With regards to mapping the proportions of clam and *Olivella* shell beads from three different contexts found at five different sites, the pattern suggests a clinal boundary. Given ethnographic and archaeological data and the relationship between the distributions of these two types of beads an economic explanation is suggested for non-burial contexts. At this broad level of analysis it seems as though the Delta is the place where two shell bead economies (or economic interaction spheres?) met: one more localized and underpinned by clam and the other, more broadly distributed system based on *Olivella* beads.[18] In this sense, it is not at all surprising to see clam shells, as a localized northern California monetary item, decrease in proportion to *Olivella* beads the further away sites are from the manufacturing location.

As mentioned above however, this explanation does not account for why a similar pattern was observed among the burial data. Following traditional interpretations of burial contexts, I began with the assumption that non-local goods had more 'value' than locally manufactured items. Yet, this is not what is observed in the data. A possible explanation for this is that personal connections, perhaps knowing the makers of the beads, or receiving them as part of a shared ceremonial experience mattered more than the economic value of non-local items.

The results of the cluster analysis also point to exciting future interpretive possibilities, but no definite answers. Whether the observed clusters of types and subtypes of *Olivella* beads are culturally meaningful cannot be answered here. Nevertheless, the clusters do exist and

[18] Assuming the intersection of the regression lines in Figures 3.12, 3.13, 3.14 marks the Plains Miwok/Northern Yokuts cultural boundary would be an analytical error similar to saying statistical significance is cultural significance.

provide an excellent opportunity to explore individual preferences or even notions of aesthetics. The cluster analysis I conducted presents an interesting pattern. That is, the San Joaquin sites, despite their positions on either side of the so-called Plains Miwok/ Northern Yokuts cultural boundary, clustered together according to bead type, whereas the Sacramento and Yolo County sites clustered. Yet, because of the lack of detailed provenience information it is impossible to say with confidence how the *Olivella* shells I analyzed were used - as bodily adornment, for clothes or baskets, or something else entirely-- and whether this might influence my interpretation. What I think can be said however, is that 'something' is going on and that 'something' is most likely cultural. In the same way, the ratio analysis from burial beads stood out. Paying close attention to the distribution of types and subtypes, even within the poorly articulated contexts of 'burial' and 'non-burial,' points to the usefulness of a heightened level of detailed analysis in bead studies and the possibility that with better provenience information, a larger and more densely spaced sample of sites, more precise distinctions could be drawn and culturally relevant interpretations made. For now, however, and in terms of *Olivella* beads, we can say the difference exists somewhere north of CA-SJo-42.

Taken together, the ratio and cluster analysis point to three areas for further study. First, continued attention needs to be paid to the fine-grained analysis of shell bead distributions across California. As I have demonstrated, this type of analysis can provide a unique window into the daily practices of people living in the Delta. Second, a reexamination of collections from Bay Area shell mounds needs to be conducted. In particular, attention should be paid to evidence of both *Olivella* and clam shell bead manufacture. Tantalizing data exist for the production of *Olivella* shell beads from the interior coastal ranges of Northern California. Yet, no bead productions sites comparable to those of southern California and the Channel Islands have been found. A reexamination of shell bead production in Northern California and the San Francisco Bay could prove to be instructive. Finally, further study is needed to explore whether there is a correlation between environmental changes in the bay and an increased number of clams and the appearance of clamshell beads in the archaeological record.

Chapter 4
Obsidian In the Delta

INTRODUCTION

Obsidian is formed by the rapid cooling of silica-rich lava that reaches the earth's surface during volcanic eruptions. Rapid cooling of the lava minimizes the inclusion of other geologic materials. The result is a stone that is chemically homogeneous with unique chemical signature. This unique characteristic of obsidian—chemical homogeneity—makes it possible to find the specific geographic origin for an obsidian stone that was turned into a man-made object. Once this 'source' information is obtained, it is possible to hypothesize about how and in what manner an object traveled from its point of origin to its final resting place. Finding the source for an obsidian object also allows for inferences to be made about the social contexts in which that object functioned.

The primary tool for measuring the chemical signature of obsidian excavated from archaeological contexts is X-Ray florescence (XRF). X-Ray florescence measures the amount of energy released by electrons as they 'fall' from the outer orbit of an atom to one closer to the nucleus. Electrons fall from one orbit to the next because the atom is exposed to radiation. This radiation forces electrons with weak atomic bonds to 'jump' orbit. The amount of energy released by an electron-jumping orbit is a unique indicator of a specific chemical element. By measuring the amount of energy an atom releases when it jumps orbit, the type and amount of each chemical element composing a substance can be determined -in this case obsidian. Thus the ratios of elements create a chemical fingerprint for each obsidian source. After a great deal of research, the proportions of many, if not most, obsidian sources in California and the Great Basin have been located. With this source information, and after conducting XRF analysis on obsidian excavated from archaeological contexts, it is a fairly straightforward process to determine from which geologic source the material for an obsidian chipped stone tool originated. The ability to accurately "source" obsidian artifacts make it an excellent material for drawing inferences about pre-European trade networks and even social relationships. Additionally, XRF is an excellent, low-cost, non-destructive method.

Focusing on California, Jack and Carmichael (1969) first demonstrated that obsidian created during the same volcanic event or found in the same localized geographic area, called a flow, had essentially the same chemical composition. They also showed that obsidian from different flows could be distinguished from each other by closely monitoring minor variations in trace elements. Geo-chemical consistency within obsidian flows, recognizable differences between them, and the ability to measure and compare the elemental chemistry or fingerprint, are essential criteria for locating the geochemical point of origin for obsidian objects found in archaeological contexts.

It is important to note that the geo-chemical source of an obsidian artifact may be different from the location where the raw material was given shape and form and where it was collected, modified, and, finally, deposited. In other words, sourcing provides limited insights into the use-life or particular travel history of an obsidian object. After excavation and comparison of an objects and sources chemical fingerprints, all we know is from which geological source the raw material originated and where it was finally deposited, the distance between these two localities, and depending on the study, the distribution of different types of obsidians across the landscape.

Since the mid 1960s, matching the geochemical fingerprint of obsidian taken from archaeological contexts to a geologic source has been a particular point of interest for California archaeologists studying the intersection of pre-European social and economic interactions (Bettinger 1982; Jack 1976; Jack and Carmichael 1969; Hughes 1989; Hughes and Bettinger 1984; Hughes and Milliken 2007; Parks and Tieh 1966; Stevenson et al. 1971; Weaver and Stross 1965). For example, sourcing lends insight into trade and exchange networks and interactions (Earle and Ericson 1977; Hughes and Milliken 2007; Jackson 1986, Jackson 1994; Rosenthal et al. 2007; Shackley 1998, 2001), theories of value (Dillion 2002), helps find the location of social boundaries and exploring how they function (Jack 1976; Jackson 1986, 1989), and helps underpin robust theories of social complexity (Fredrickson 1996). More recently, sourcing studies have used obsidian source data to explore Native Californian resistance during the Colonial era (e.g. Silliman 2005). Despite the varied topics for which obsidian is used to discuss they all share three common variables: the geologic source of an object, the distance an object is found from that source, and the distribution of obsidian sources (measured by percentage) across the California landscape.

Whereas many California archaeological studies use obsidian to explore long-distance trade and exchange, I focus specifically on the Delta and show how the analysis of obsidian objects from more localized settings provide insights into social, cultural, and economic interactions. For this study I compare utilitarian objects—whole or partial projectile points—and non-utilitarian objects—Stockton Curves or hand-chipped obsidian objects shaped to mimic bear claws. Stockton Curves are found exclusively within the Delta region of Central California and are part of the regalia for an annual ceremony called the Bear Dance.

In this chapter, I focus on identifying the geological source for obsidian artifacts from the study area and analyze the distribution of sources and obsidian artifacts across the Delta region. I anticipated three outcomes from the XRF analysis. First, I anticipated that my findings would extend the obsidian source distribution pattern uncovered by Jackson (1986) for the north-central coastal region into the heart of the Delta. Second, I expected the observed pattern of source distribution across Delta would provide insights into the social and political relationships underpinning obsidian trade. This in turn would speak to the permeability or impermeability of the Plains Miwok/ Northern Yokuts cultural boundary. Last, I hypothesized that there would be multiple obsidian sources used to make Stockton Curves and that each source would correlate to a particular tribelet. I further hypothesized that if there were more specific and localized economic and social relationships, they would be observable in variations in source and artifact distribution.

Not surprisingly, my findings did not conform entirely to my expectations. While XRF analysis of utilitarian objects showed a continuation of Jackon's (1986) findings of increasing proportions of Napa Glass Mountain obsidian into the Delta, Stockton Curves were manufactures almost exclusively from Napa Valley sources. I was, therefore, unsuccessful at finding any correlation between obsidian source and tribelet. There are possible two reasons for this failing: 1) inadequate sub-source data for Napa Glass Mountain resulted in my inability to make solid comparisons between tribelet locals; and 2) the variation I thought indicated Napa sub-sources was actually the result of acceptable range of variation in the measurements made by the *Quant-X* machine used for analysis.

Nevertheless, the data I present in this chapter contributes to California archaeology. Using Jackson's (1986) findings as a starting point and for comparison, I continue the analysis of the distribution of obsidian across tribelet boundaries into the heart of the Delta region. To do

this, I provide the results of energy dispersive XRF analysis of two different categories of obsidian objects from the Delta: utilitarian (projectile points) and non-utilitarian objects (Stockton Curves). My analysis of the Stockton Curves is particularly unique because, to my knowledge, it is the first XRF analysis done on these objects. I frame the data presented herein in light of local economic (trade and exchange) and social and cultural interactions (ceremony).

In this chapter I will first provide a brief literature review emphasizing obsidian studies that search for cultural boundaries in Native California. Next, I will present the methods for analysis, my results, and interpretations. Last, I will consider the data presented in this chapter in relationship to the five boundary models I proposed in Chapter 2 and argue for a bi-directional flow of raw materials and ceremonial knowledge. My findings contribute to understanding of one of the most dynamic regions in California and provide additional insight into the social relationships that undergird obsidian trade and boundary relationships in the Delta.

OBSIDIAN STUDIES AND CULTURAL BOUNDARIES

Robert Jack, a member of the Geology and Geophysics department at the University of California, Berkeley conducted pioneering work with XRF. He undertook the first geochemical study of obsidian sources in California (Jack 1976). Jack selected 1,567 pieces of obsidian within the boundaries of ethnolinguistic groups throughout all of California. His goal was twofold. First, to determine which obsidian sources used by Native California's and where there were traded and, second, to correlate these findings with the ethnolinguistic boundaries proposed by Kroeber (Jack 1976:184). Jack identified several sources of obsidian and recorded the relative proportions of artifacts made from different sources of obsidian at each site he sampled. He was not, however, able to identify a unique and exclusive relationship between specific sources and individual ethnolinguistic groups as he hypothesized.

Despite this limited success, using a least-cost analysis Jack concluded that the obsidian used "by the several tribal-linguistic groups of the area...quite simply related to the relative accessibility of each of the obsidian sources from each of the sites studied and to the territory controlled by each Indian group" (Jack 1976:189). Importantly, this passage highlights Jacks finding that there was no correlation between obsidian source and ethnolinguistic group and posits that the relationship between human actors and any give source was simply opportunistic. Jack's study also illustrates a consistent analytic error in obsidian studies in California; not critically evaluating what type of boundary was being explored. Jack was not explicit about whether the boundaries he was observing though obsidian objects were social, political, economic, linguistic, or how those boundaries might interrelate with one another, if at all.

Mapping the changes in the relative proportions of chipped stone objects made from a variety of obsidian sources found throughout California and the Great Basin does not always map cultural boundaries. Rather, these distributions are proxy's, in a limited sense, for different types of interactions enacted by groups and individuals in particular settings resulting from the movement of objects across a landscape. As Jonathon Ericson (1981) noted, "...the spatial distribution of the obsidian is not a system, but rather it is the consequence of a system" (93).

The second significant obsidian study exploring the relationship between source distribution and ethnolinguistic boundaries is Ericson's (1981) attempt to model exchange during the Late Horizon across three dimensions. In this short piece, Ericson rightfully critiques the adherence of previous models to Renfrew's (1977) Law of Monotonic Decrement. Summarized, the Law posits that as the distance from the source of a raw material increases, the relative

proportion of objects made of that source material will decrease. This assumes a trend of consistently declining distribution of source material in all directions from the source and masks any potential variation in the data. Ericson attempts to overcome this limitation by demonstrating what he assumes to be inherent variation in archaeological data by employing a 3-D mapping system called SYMAP (Fisher 1973 cited in Ericson 1981:106). SYMAP produces a type of contour map that statistically weighs and compares data. The product of this analysis is an image of both the relative intensity and direction of flow. This mapping technique, Ericson argued, overcame the two dimensional nature of previous studies that attempted to record the quantity of a particular object and the distance it was from its source. The second portion of Ericson's study compared his SYMAP findings to major pre-European trails (Davis 1961) and ethnolinguistic boundaries (Kroeber 1925). Ericson found some concordance between the contours of the SYMAP output and the location of Native Californian routes. But before comparing the SYMAP data to ethnolinguistic group boundaries he posited, "if it is assumed that the social distance of communities within an ethnolinguistic group is less than between ethnolinguistic groups, then it would be expected that discontinuities in quantity of exchange items would be observed at ethnolinguistic boundaries" (Ericson 1981:114). Like Jack (1976), Ericson found no correlation between ethnolinguistic boundary and changes in the distribution of obsidian objects made from materials that came from different sources (Ericson 1981:114). There are two key take-aways of Ericson's study. First, he adhered to a time-honored assumption about Native Californians: that they had internally consistent (homogenous) cultural characteristics and boundaries that changed little through time, and, second, similar to many researchers before and after him, Ericson assumed that ethnolinguistic boundaries defined daily interactions and group identity.

In an evaluative essay Hughes and Bettinger (1984) focused on these weaknesses. They note that while Ericson's (1981) work represented a major step forward in obsidian studies and improved California archaeologists understanding how material from different sources was distributed, they criticized it for dismissing (or missing) social and cultural explanations for his findings (1984:155). Unfortunately for Ericson, he was Hughes and Bettinger's (1984) straw-man. They used his work to highlight two faulty assumptions underpinning California obsidian source studies more generally—assumptions that persist today. First, that despite ethnographic evidence to the contrary, obsidian objects are generally framed by archaeologists as being valuable only insofar as they have a utilitarian function; and, second, that the twin assumptions that the distribution of obsidian sources reflects the social distinctions and that the ethnolinguistic group was the appropriate unit of analysis, were incorrect (Hughes and Bettinger 1984:155). In proposing a new approach, Hughes and Bettinger suggested obsidian distributions should be explored at the tribelet level. As they argue, the tribelet was the sociopolitical unit of greatest importance in pre-European Native California (citing Kroeber 1955, 1962) and that social, political, and economic obligations essential to explaining obsidian distribution through space and time would be more accurately reflected at this level of organization, not at the larger scale of the ethnolinguistic unit.

Thomas Jackson's (1986) dissertation is perhaps the best-known example of obsidian research that maps the distribution of obsidian chipped stone objects source data among the tribelets of Northern California. In addition, Jackson discusses the social contexts and interactions associated with the life of the object from raw material to its final resting place. Jackson's XRF analysis, which resulted in a map that shows the final disposition of obsidian objects in terms of their source data, allowed him to postulate that at the tribelet level the economic exchange facilitating the movement of raw material was mediated by social

interactions. This allowed Jackson to explore two broad themes: the relationship between economic organization and cultural complexity, and how goods were exchanged across territorial boundaries (2-3).[1] Unlike previous obsidian studies of boundaries, Jackson questions whether and if there is evidence of any effect and archaeologists can distinguish between ethnic and economic boundaries (4).

Jackson (1986) drew eight conclusions from his research, of which three are relevant to this dissertation. They are: 1) that obsidian was traded across some tribelet boundaries and not others; 2) some objects, such as clamshell disc beads, were traded across all territorial boundaries and used primarily by elites (Stockton Curves may be an exception to this); and 3) obsidian was exchanged in a variety of forms but only among certain groups was it used for non-utilitarian purposes (e.g. Stockton Curves and North Coast bifaces used in the White Deerskin Dance) (122). Each conclusion is highly provocative, especially considering the goals of this dissertation.

Indeed these conclusions provide an opportunity to ask three key questions. First, whether obsidian found in burial contexts is from more "exotic" quarries and thus will not demonstrate the same geo-source patterning observed in non-burial contexts. Second, whether the common paradigm in obsidian sourcing studies of assuming a correlation between value and utility is valid. And third, by focusing on a single and unique object, Stockton Curves, make it possible to discern more personal information about the people with whom they were buried.

Jackson's study is groundbreaking because it shifted the scale of obsidian studies and assumed trade and social relationships occurred from the ethnolinguistic unit to the more socially meaningful tribelet. Additionally, Jackson demonstrated that distance decay and rational choice models neither satisfactorily explain variability in the distribution or even use of obsidian, nor do they adequately explain or uncover the social relations associated with cross border interactions.

METHODS

XRF analysis is the most commonly used nondestructive method for determining the geochemical composition or "fingerprint" of obsidian artifacts (Shackley 1998, 2001, 2005, 2011). Since XRF was first used in California by Jack (1976), its precision and accuracy has improved significantly. The reliability of XRF analysis is predicated on the fact that obsidian flows are formed during a single volcanic event. As noted above, this results in a stone with a singular chemical composition. Improvements in the precision of XRF analysis suggest that minor deviations within single flows—something now recordable through EDXRF—might be a place for further investigation. For example, minor variations between sub-sources might lead to more fine-grained analysis of obsidian procurement, distribution, and the social relationships that accompany these activities (Dillian 2002; Eerkens 2004; Jackson 1986). While some effort was made to explore this new research path, my efforts were unsuccessful (see below).

All the objects selected for this dissertation were analyzed using a Spectrace/ThermoNoran *Quant'X* energy dispersive X-Ray florescence spectrometer (EDXRF) at the Berkeley Archaeological XRF Laboratory at the University of California, Berkeley. The *Quant-X* machine improved the analytical speed, efficiency, and accuracy of XRF analysis as compared to previous XRF machines (Shackley 2005). In the *Quant-X*, high-energy X-Rays bombard an object forcing electrons to fall from their orbits. When this happens other electrons

[1] Here, Jackson assumes that ethnolinguistic and archaeological culture boundaries are synonymous with geo-political boundary.

from the outer orbits of the atom "fill" the space created by the one removed. In the process of filling the space left by the removed electron, energy is released (radiation) as a photon that has the characteristics of the atoms from which they belong. This is called florescence. In the *Quant'X* machine, photons are directed to a solid-state detector. The detector creates pulses of voltage proportional to the incoming energy. This energy is registered as a peak intensity. Each peak intensity is unique to the element from which photons were created. The *Quant'X* machine repeatedly bombards each obsidian sample recording both the elements presence as well as its concentrations. The results of the analysis are quantitative and derived from filtered intensity values and ratioed to the appropriate X-Ray continuum through a least squares formula rather than plotting the proportions of the net intensities in a ternary system (McCarthy and Schamber 1981; Schamber 1977). Although several elements were measured during lab analysis, here I present data for strontium (Sr), rubidium (Rb), and zirconium (Zr). These are the most useful elements for distinguishing between geologic sources of obsidian in California (Davis et al. 1998; Dillion 2002; Ericson 1977; Hughes 1982; Jack 1976; Jackson 1986; Shackley 1998). Following analysis in the *Quant'X* machine, data were generated by the WinTrace (TM) 4.1 software and was translated directly into Excel for Windows software for subsequent manipulation in SPSS GradPack v.18. All values are presented in parts per million (ppm),a quantitative measurement by weight. The international RGM-1 standard for rhyolite was included during each analytical run to ensure the machine was properly calibrated (Govindaraju 1989; Silliman 2005).

In the context of this dissertation, EDXRF analysis is appropriate for four reasons. First, using a nondestructive technique was a top priority because all the sampled objects were taken from burial contexts. Second, it is a nondestructive technique that outputs reliable, consistent, and interpretable data. Third, XRF is, in general, an efficient process and a large number of samples can be analyzed quickly. Fourth, EDXRF is comparatively inexpensive and creates little financial burden.

Utilitarian objects and Stockton Curves were chosen from among ten sites. Here site selection falls within the same general method as presented in Chapter One (Figure 4.1). Utilitarian objects were sampled from sites within the general population of sites that had obsidian samples and were reported as tribelet centers (Bennyhoff 1977). This method conformed to Jackson's (1986) selection rational and allowed me to compare my findings to Jackson's. Sites from which Stockton Curves were selected were chosen less systematically. In this case, because the total number of available Stockton Curves was so small I chose to analyze all available samples from the Hearst Museum collections.[2]

[2] All Stockton Curves were found from burials.

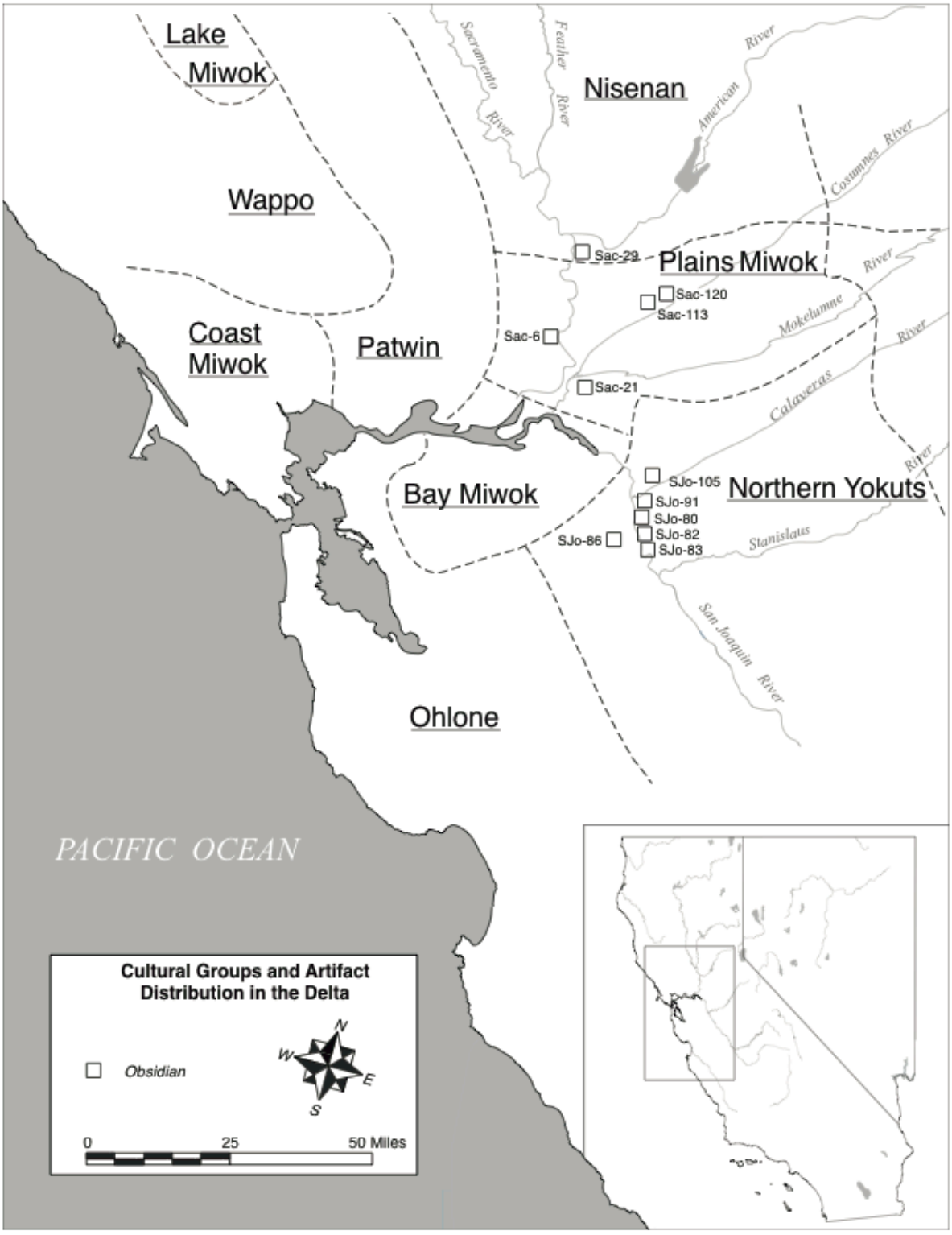


Figure 4.1 Study area and archaeological sites from which obsidian chipped stone artifacts were analyzed.

Before beginning EDXRF analysis on these objects I had three general expectations. First, my analysis would extend the trend noted by Jackson (1986) into the Delta. I also expected that my analysis would highlight, as did Jackson's, the social and political relationships underpinning the obsidian trade. Additionally, I expected that the observable patterns would shed light on the permeability or impermeability of whatever boundary I was observing and allow me to test the four boundary models presented in Chapter One. Specifically, I chose to use objects from burial contexts instead of non-burial contexts to test Jackson's (1986) assertion that obsidian found with burials would necessarily come of more exotic or distant sources and because they allowed for a more defined chronology. Last, I anticipated source data for Stockton Curves would demonstrate more particular preferences in collection or trading strategies and point to more refined distinctions of group membership.

RESULTS

A total of 207 obsidian objects were analyzed from two broadly defined artifact classes: utilitarian and non-utilitarian. The sample of utilitarian objects consisted of 44 projectile points, flakes, or partial points from burial contexts excavated from CA-SAC-6, CA-SJo-105, and CA-SJo-42. A total of 163 Stockton Curves were sampled from burials excavated at CA-SAC-6, CA-SAC-21, CA-SAC-29, CA-SAC-113, CA-SAC-120 and CA-SJo-42, CA-SJo-80, CA-SJo-82, CA-SJo-83, CA-SJo-86, CA-SJo-91, and CA-SJo-105. The sample size for each site within both contexts varied greatly. In some cases, there were as many as 49 Stockton Curves (i.e., CA-SJo-80) and as few as one (i.e., CA-SJo-42 & CA-SAC-29, CA-SAC-113). Similarly, the sample size for utilitarian objects ranged from six at CA-SJo-42, to 21 at CA-SAC-6.[3]

UTILITARIAN ITEMS: PROJECTILE POINTS, PARTIAL POINTS, AND FLAKES

A total of 44 full and partial projectile points and flakes recovered from burials contexts at three sites, CA-SAC-6, CA-SJo-42 and CA-SJo-105, were analyzed using EDXRF. The geologic source for the majority of these samples was Napa Valley (Table 4.1 and Figure 4.2). The site CA-SJo-105 differed from either CA-SJo-42 or CA-SAC-6 because it had single samples from Anadel, Buck Mountain, and two unknown sources (Table 4.1). There are several sites situated in the southeast corner of Jackon's (1986) study area that overlap with my own. These sites are located along the Sacramento River (CA-SAC-6, CA-SAC-56 and CA-SAC-267) and within Contra Costa County (CA-CCo-30, CA- CCo-138, CA-CCo-259 and CA-CCo- 312). According to Bennyhoff (1977), these sites are thought to be within Plains Miwok territory.

[3] Jackson (1986) found similar variation making his attempts to establish a minimum obsidian sample size difficult. Eventually, he settled on a minimum of 15 and no maximum (Jackson 1986: 64). For the purposes of this dissertation, the sample size of 6 objects from CA-SJo-42 represents all of obsidian artifacts associated with burials. Less than rigorous excavation techniques (by todays standards), inadequate or incomplete field notes for the reconstruction of site contexts, and sampling strategies pose some of the biggest limitations for analysis of museum collections and were certainly confronted here.

Table 4.1 Summary of findings for utilitarian obsidian

SITE	*COUNT*	*PRIMARY SOURCE*	*SECONDARY SOURCES*
SAC-6	21	Napa (n=21 or 100%)	None
SJo-42	6	Napa (n=6 or 100%)	None
SJo-105	17	Napa (n=13 or 76%)	Buck Mountain (n=1 or 6%), Anadel (n=1 or 6%), ? (n=1or 6%), ? (n=1or 6%)

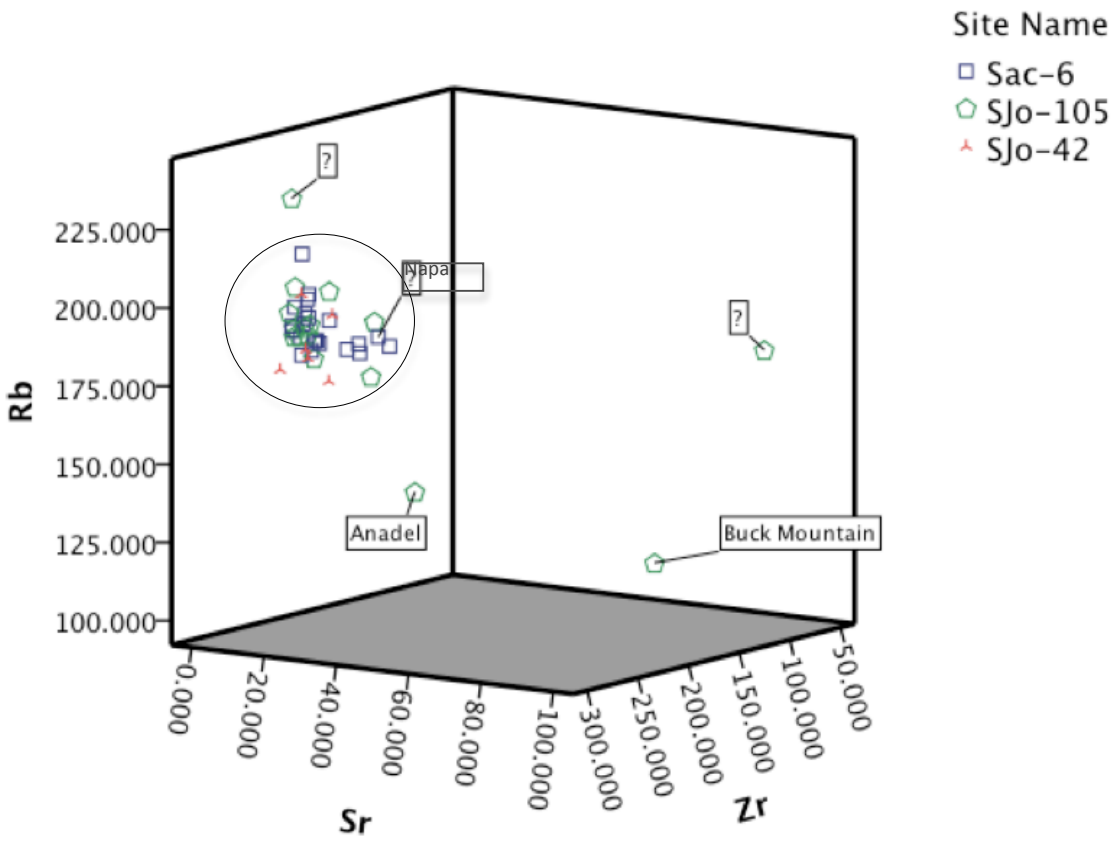


Figure 4.2 Results of EDXRF analysis of utilitarian obsidian.

Jackson (1986) found that the majority (85%) of obsidian sampled from these three Sacramento and four Contra Costa County sites originated in the Napa Valley while most of the remaining samples came from the Sierra Nevada source of Bodie Hills. The origin point for the remaining 15% of the objects came from Casa Diablo and Mono Glass Mountain. My analysis of projectile points supports Jackson's findings. All the utilitarian objects sampled from CA-SAC-6 were sourced to Napa Valley. The same is true of CA-SJo-42. The sample from CA-SJo-105 demonstrates nominally more variation: one sample was from Annadel, which is located near Santa Rosa, California. Another sample was from Buck Mountain along the Oregon border. There were two samples from unidentifiable sources. The minor variation at CA-SJo-105 is not inconsistent with Jackson's (1986:107) findings from a much larger sample.

NON-UTILITARIAN ITEMS: STOCKTON CURVES

The name "Stockton curve" is derived first from the location where these objects were first found in the late nineteenth century, Stockton, Ca., and from their generally curvilinear shape. Most Stockton Curves have serrations on both the exterior and interior arcs of the curve. This characteristic makes them part of the more general "Stockton serrate" type of projectile point. Serrate points, like Curves, are unique to the Delta region and are found almost exclusively in the Delta Region (Justice 2002). A total of 163 Stockton Curves were sampled from eleven sites across the Delta; 156 of these samples yielded usable data.[4] All but five of the sampled Stockton Curves have geologic origins in Napa Valley. The majority of the Curves, 87.5% (n=140), sampled were from CA-SJo-80, CA-SJo-82, CA-SJo-83, CA-SJo-91, and CA-SJo-105, all sites currently understood to be within the Northern Yokuts territory. The remaining Curves came from sites within Plains Miwok territory: 11.5% (n=19) came from CA-SJo-42, CA-SAC-6, CA-SAC-21, CA-SAC-113, and CA-SAC-120, and 9% (n=1) came from CA-SAC-29 (Figure 4.1 and Table 4.2). The results of this analysis confirm Jackson's (1986) hypothesis that Stockton Curves were made primarily from obsidian originating in Napa Valley. Only 3% (n=5) of the total sample originated from sources other than Napa Valley: two from CA-SJo-82 and a single sample each from CA-SJo-83 and CA-SAC-21.

[4] Seven Curves were removed from analysis because heavy dirt layers interfered with the *Quant-X* machine.

Table 4.2 Summary of findings for non-utilitarian obsidian (Stockton Curves).

SITE	*COUNT*	*MAJORITY SOURCE*	*SECONDARY SOURCE*
SAC-6	13	Napa (100%)	None
SAC -21	1	?	None
SAC -29	1	Napa (100%)	None
SAC -113	1	Napa (100%)	None
SAC -120	3	Napa (100%)	None
SJo-42	1	Napa (100%)	None
SJo-80	50	Napa (99.9%)	?(1)
SJo-82	28	Napa (99.9%)	? (2)
SJo-83	19/24 *	Napa (99.9%)	? (1)
SJo-86	0/1*	Unknown	
SJo-91	30/31 *	Napa (100%)	None
SJo-105	9	Napa (100%)	None
* The split number show from which sites samples were removed (see footnote 4) and in what quantity.			

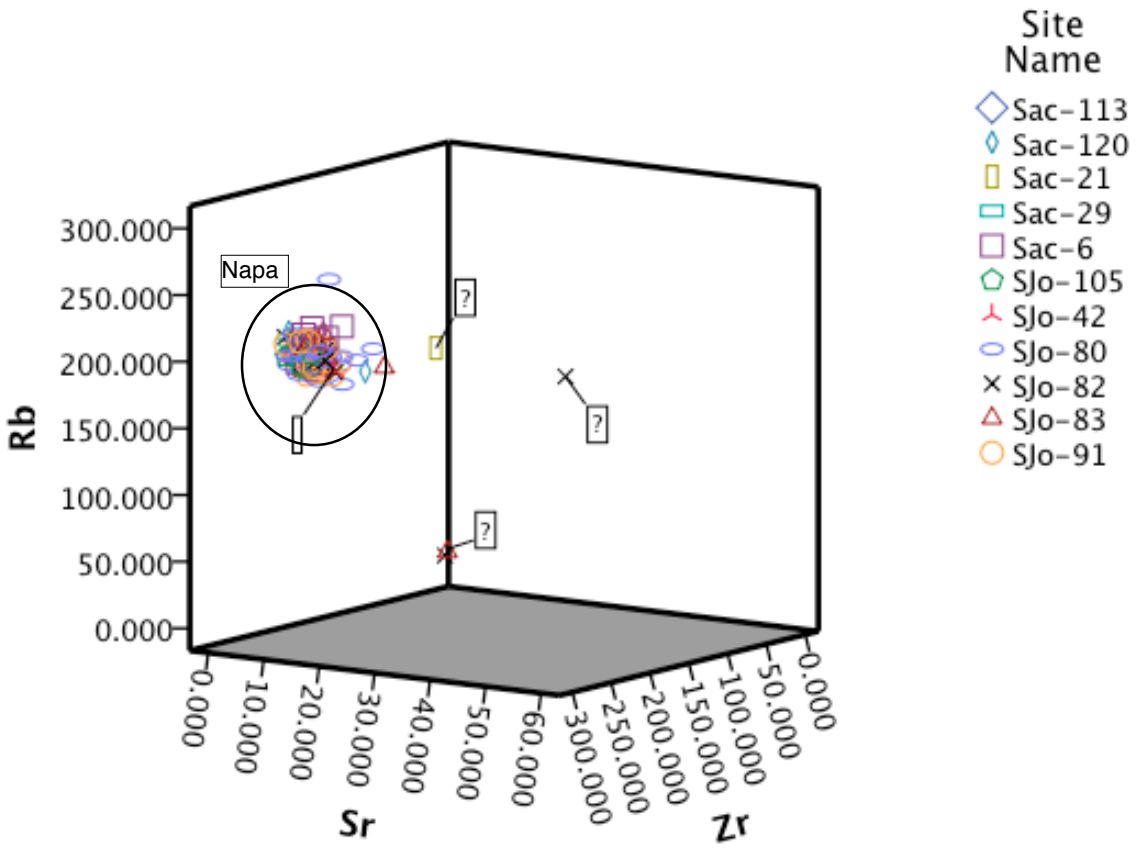


Figure 4.3 Results of ED-XRF analysis of Stockton Curves.

INTERPRETATIONS

In this section I will first interpret the data gathered from the utilitarian items and will draw heavily upon Jackson's (1986) dissertation for comparison. Next, I will focus my discussion on the Stockton Curves.

Despite the small sample of utilitarian objects taken from only a few sites in the Delta, my analysis continues the general trend uncovered by Jackson (1986), that is, of increasing proportions of Napa Valley obsidian at sites to the east and south of the Sacramento River and into the Delta and East Bay areas. Building on Jackson's focus on access to resources at the tribelet level as a way to understand exchange across boundaries, my analysis demonstrates that Napa Valley was the primary geologic source for chipped-stone tools in the Delta. Without direct observation or specific ethnographic references, it is difficult to accurately determine the specific mechanisms through which obsidian moved from Wappo tribelet territories surrounding Napa Valley to the people living in the Delta. Similarly, it is difficult to know if obsidian from Napa Valley made its way into the Delta via a less direct route. Determining what kind of relationships might have facilitated obsidian exchange depends on confirming one of four possible scenarios that are beyond the scope of this chapter:

1. The Wappo collected and distributed obsidian from Napa Valley (down-the-line exchange);
2. The Wappo monitored which neighbors could enter their territory and collect from it (limited direct-access);
3. The Wappo did not collect or distribute obsidian from Napa Valley nor did they limit access, but rather the collection of raw materials was based on other types of relationships (open direct-access);
4. there was a combination of open and limited access.

Some possibilities of this third type include, ceremonial, marriage, and kin or lineage based relationships. There is some evidence for open direct-access (Barrett and Gifford 1933:217; Gifford 1926:377) but Jackson (1986:114) believes this may be an effect of population decline due to Mission recruiting and kidnapping, disease, and genocide during the mid to late nineteenth century.

While Jackson (1986) favors the third explanation—that obsidian exchange was controlled by elites and mediated through marriage partnerships—given the results of my analysis, I am unwilling to support this interpretation. What I think is clear, however, is that when people living in the Delta needed or wanted raw obsidian or pre-made tools, they had limited options or very specific preferences. Franz Valley and Annadel (in the territory of the Gualomi (Pomo) tribelet) sources are not far from the Napa Valley and it is equally probable that obsidian could be obtained from either of these two sources with little change in effort, yet, there are only a modest number of obsidian samples from Franz Valley and Annadel sources recorded at sites in the Delta. It is possible that the people living along the Southern Sacramento River and in the Delta had a stronger economic connection to those controlling the Napa Valley source then they did with those living in the areas surrounding the other two sources.

The reasons for why obsidian in the Delta is almost exclusively from Napa Valley are unclear. It could be that the quality of the obsidian was better/preferable. Or it could have been that social relationships based on marriage or kinship dictated and facilitated trade as Jackson (1986) and others suggest (e.g., Fredrickson 1994; Hughes 1989). It is equally probable that the distribution is based on some other set of cultural preferences or dispositions. Framing this discussion in the language of shell bead studies, one could posit a obsidian "interaction sphere" that included Napa and the Delta. However this framing unnecessarily isolates obsidian conveyance and draws attention away from an exploration of other types of social interactions and settings that may have been more important for determining why Napa Valley obsidian dominates the archaeological assemblage of the Delta.

Despite Napa Valley being the geologic source for a preponderance of Stockton Curves for example, these objects create new possibilities for interpreting the distribution of obsidian source data and understanding the social interactions facilitating their movement. While Warren Moorehead (1900) speculated wildly about the use of Stockton Curves, he did chance upon a few correct interpretations. It is worth quoting him at length:

> ...that the Curves were used to scarify the flesh on ceremonial occasions. Aside from the shape and general adaptability of the implements to such service there is little evidence to support the theory. Nevertheless, as yet I have no reason to revise my views. Scientific men, on examining the Curves usually say: "They must have been for cutting flesh." A California editor, in a foot-note appended to an article of mine on "Art forms in

> Obsidian," remarks "that, as a matter of fact, the 'Curves' are 'merely artifacts' made of that shape because that shape is the natural cleavage of nodular obsidian accessible to those Indians. As they couldn't depend on its breaking straight, they worked it as it did break and made their knives thus sickle-shapes." I am sorry to differ from so skilled an archaeologist and so estimable a gentleman as Mr. Lummis, but familiarity with the local conditions makes his theory impossible. I have examined superficially or otherwise nearly two hundred mounds and village sites in this vicinity, and although these villages all drew their obsidian from the same sources, not one of these sites, excepts the two mentioned, had anything that suggests the "curve." All their chipped implements were perfectly straight. Moreover, the great obsidian beds of the Lake and Napa counties, which have examined several times, could furnish all the Indians of the United States with material for generations. The hills are full of great blocks of obsidian, too heavy for a man to handle, and it breaks as straight as a shingle (Moorehead 1900:262-263).

This passage is one of a handful of interpretations existing in California archaeological literature from the early twentieth century which attempts to understand how and why Stockton Curves were used. Other descriptions of Stockton Curves, made by budding professional archaeologists (e.g., Jones 1923; Lillard, Heizer, and Fenenga 1939; Schenck and Dawson 1926) were less speculative, drawing extensively from the ethnographic literature. There are two passages in particular which provide a great deal of insight into the social worlds of Stockton Curves. The first is from Barrett and Gifford's *Miwok Material Culture: Indian Life of the Yosemite Region* (1933). In a section entitled "Problematical Stone Objects" Barrett and Gifford wrote,

> Three of the archaeological objects called "charmstones" and some examples of the obsidian "Stockton Curves" were shown to Central Miwok. The "Stockton Curves" were declared to be imitation bear claws (tisus uzumatiñ, C) worn on the left hand by dancers of the uzumati or grizzly bear dance. Four of these Curves were attached to sticks and these in turn lashed to the four fingers. (Barrett &Gifford 1933:213).

The first question this passage raises is that if Stockton Curves are so named because they are found near Stockton, Ca. and that area is assumed to be Yokuts territory, why then do the Miwok people from Yosemite know about the Curves and their use?[5] Gifford (1926) and Kroeber (1925) provide an answer. Gifford wrote:

> Both the Central and Northern Miwok agree that certain impersonating dances were introduced by dancers from the region of Livermore and Pleasanton in Alameda County. The Central Miwok attribute to these people the uzumati (grizzly bear) and the hiweyi dances. In the former the Central Miwok used curved obsidian blades as imitation bear claws. Their western attribution of the dance is archaeologically confirmed by such objects found in mounds near Stockton (Gifford 1926:399).

[5] This question arises especially in the context of traditional archaeological models of culture that different groups are essentially cultural isolates and internally homogeneous.

Similarly, Kroeber noted:

> The Uzumati or grizzly bear ceremony came to the central Miwok from the west or northwest, they say; that is, probably, the northernmost Yokuts such as the Chulamni of Stockton, or the Plains Miwok. The performer, who was a dance impersonator and not a bear doctor or shaman, carried curved pieces of obsidian attached to his fingers in place of the bear's claws. He imitated the animal in his dancing. This description accounts for the hitherto unexplained "Stockton Curves," as antiquarians have come to call the semilunar flaked objects of obsidian found in ancient burials in the San Joaquin delta (Kroeber 1925:450).

Although the authors are not in complete agreement over the exact origin of the dance or the ethnic identities of those who shared it with the Miwok groups they interviewed, at the very least it is clear the informants are referencing the area around Stockton, Ca. and to the east, probably close to Livermore, Ca. These passages raise an important second question: to which group the dancers of this area around Stockton can be affiliated, Yokuts or Miwok? Like the Plains Miwok/ Northern Yokuts border being scrutinized by my research, this western border area is equally difficult to understand with certainty. Although it is beyond the scope of this project to fully explore interactions along the Yokuts/ Bay Miwok boundary in Alameda and Contra Costa Counties, some attention to it is useful for gaining insight into Stockton Curves and their meaning and distribution.

Archaeological evidence suggests that the Livermore area is on the northwestern edge of what is currently accepted as Northern Yokuts territory. This is based on linguistic and historical analyses used to affiliate this area to the ancestors of contemporary Yokuts people. There are two key sources for linguistic information. First, in 1880 A. Louis Pinart (Pinart 1955) collected a vocabulary from Yokuts speakers living in Pleasanton who, in no uncertain terms, referred the area to the west as Yokuts. Second, a few decades later, in the early years of the twentieth century, Alfred Kroeber revisited the same area and interviewed different informants also claiming to be of Yokuts ancestry. These individuals made similar claims (Kroeber 1906, 1908). In related efforts to culturally affiliate the Stockton Ca. area, Bennyhoff (1977), Smith (2007) and Milliken (1995, 2008), closely analyzed the vocabularies provided by Pinart (1955) and Kroeber (1906, 1908) in combination with Mission registers and other historical documentation. Each concluded the villages immediately surrounding the Stockton Ca. area and about as far east as Livermore, were within the territory of the Cholovone or Chulumni tribelet, a Northern Yokuts group.

Given this data, and the fact that the vast majority of Stockton Curves are found in the villages immediately surrounding Stockton, Ca., it is reasonable to think Miwok people learned the Bear Dance from Yoktus dancers living around present day Stockton, Ca and to some extent incorporated the use of bear claws made of obsidian as part of their regalia. It is also possible that when Miwok people looked for Bear Dance guidance, they turned south to their neighbors to provide leadership. I may be speculating too far with this last point but I want to highlight what I see as a contrast between the movement of obsidian down the Sacramento River Valley and into the Delta, and the possible foundations of the Bear Dance and sharing of ceremonials occurring in the opposite direction.

CONCLUSIONS

Referring to pre-European exchange systems, Jackson and Ericson intoned, "when archaeological data from the Emergent period are combined with historical data for inter-group relations the picture is often clarified" (Jackson and Ericson 1994:408). My combination of ethnographic and EDXRF evidence does not demonstrate clear cut economic boundaries but it does open the conversation about obsidian trade and exchange to more complicated discussions of cross-border interactions. Typically in California sourcing studies, the value of obsidian objects to archaeologists is contingent upon their utility and the function(s) they serve. Here I present an alternate perspective on this traditional model and argue that the value of an object is derived from its use in interpersonal interactions and the resultant meanings those people attach to it (Graber 2001). In this sense Stockton Curves present an interesting example. I also acknowledge that my analysis is merely a starting in a discussion that pays closer attention to the social contexts in which objects exist. The movement of obsidian into the Delta based on source data for both utilitarian and non-utilitarian grave-goods suggests unidirectional movement from a source to the excavation location. The addition of ethnographic evidence related to Stockton Curves alters this image slightly. The overall movement of obsidian may have been from Napa Valley, down the Sacramento River, and into the Delta, but Stockton Curves and the ideas and ceremonial practices they were a part of seem to go in the opposite direction. There are three possible scenarios for how Stockton Curves and knowledge of the Bear Dance moved from the Stockton area north:

1. Knowledge of how to Bear Dance, as well as make Curves migrate north from the Stockton area; Curves are made in the locations where they were excavated. Only ideas move.
2. Knowledge of how to Bear Dance, as well as to make Curves, migrates north from the Stockton area; Curves are made in the Stockton areas and traded north. Ideas and goods move.
3. Individuals with knowledge of how to Bear Dance, as well as make Curves migrated north (intermarrying/expanding kin relationships) from the Stockton area. Ideas and people move.

Given these three possibilities and the obsidian data presented in this chapter, I would like to revisit the five boundary models I posited in the beginning of this dissertation. Earlier I posited that we might expect boundaries to conform to one of five circumstances:

1. Fixed: Expect to see circumscribed patterns of distribution.
2. *Clinal*: Expect to see a gradient of in the distribution of objects.
3. *Temporally Fixed*: Expect to see circumscribed boundaries that shift according to chronological divisions.
4. *Temporally Clinal*: Similar to above, but changes should be gradual. This might be impossible to see.
5. *Punctuated*: Expect to see some types of objects circumscribed and others less constrained

Initially, I anticipated finding a distinctive pattern in the distribution of obsidian sources used to make utilitarian chipped-stone tools. I also anticipated that this variation would be significant

enough to help determine economic boundaries that might even parallel ethnic boundaries. Additionally, I predicted the pattern of distribution of obsidian sources used to make Stockton Curves would be different from the utilitarian items. While I was able to confirm previously unsubstantiated hypotheses that the most likely source of obsidian found in the Delta region came from Napa Valley, I was not able to separate the geologic source of Stockton Curves from this pattern. In other words, no clear distinctions could be made within the Napa Valley obsidian source.

Comparing these findings to the data gathered from shell beads, it seems two very different economic patterns are apparent. On the one hand the relative proportions of *Olivella* and clam shell beads decrease in the Delta region along a north-south axis whereas the distribution of obsidian sources in the same region is constant. Comparing the data from this study with Jackson's (1986) findings it seems that Stockton Curves complicate the picture. Jackson found that the relative percentages of different sources might indicated that some boundaries were more permeable than others. Using this logic, the data gathered from Stockton Curves would suggest that the boundaries between groups, at least in terms of those items, were completely porous. Where Jackson found some variation in the number of obsidian sources represented at sites in the northern Delta, my research show no variation. It seems that rather than being of a singular character—exclusively social, cultural, or economic—and either fixed, clinal, or punctuated, the Plains Miwok/Northern Yokuts boundary with respect to obsidian was multifarious, bidirectional, and perhaps even contingent upon the type of object the obsidian was going to be turned into (Figure 4.4). This data might also be an indication that completed objects were traded for and not just the raw material. To prove this claim, further study into the stylistic elements of Stockton Curves needs to be made. Unfortunately, that is beyond the scope of this chapter. Nevertheless, considering my analysis, I argue that on the one hand the flow of raw material used to make all obsidian objects was generally north to south whereas the flow of ceremonial information and related regalia was the opposite.

In the context of NAGPRA, my findings in this chapter challenge traditional methods of using artifacts, such as Stockton Curves, as diagnostic of a particular cultural group or economic boundary that leads to claims about social or cultural group affiliation. Importantly, my findings highlight broader questions essential to this project: what are culturally meaningful markers of ethnicity, an emblem of participation in a social event or clan membership or even in economic interactions? What if the material markers of certain practices, traditionally considered emblematic of one group, were shared? If individuals crossed borders to participate in dances, ceremonies or to regularly visit kin, how do we distinguish what is culturally meaningful? When Jack (1976) sought cultural boundaries in his groundbreaking obsidian study he could not find them. He concluded, "[i]n many cases a boundary, as plotted on such a map [i.e. Created by Kroeber or Heizer showing ethnolinguistic boundaries] probably had little significance in the lives of the people whose territory is represented..." (187). The evidence derived from obsidian analysis indicates that there was significant movement of people, things and ideas across the Plains Miwok/Northern Yokuts supposed boundary and that clear distinctions based on obsidian data alone are impossible to draw. The Delta was a place of active and intense cultural, linguistic, and spiritual interactions.

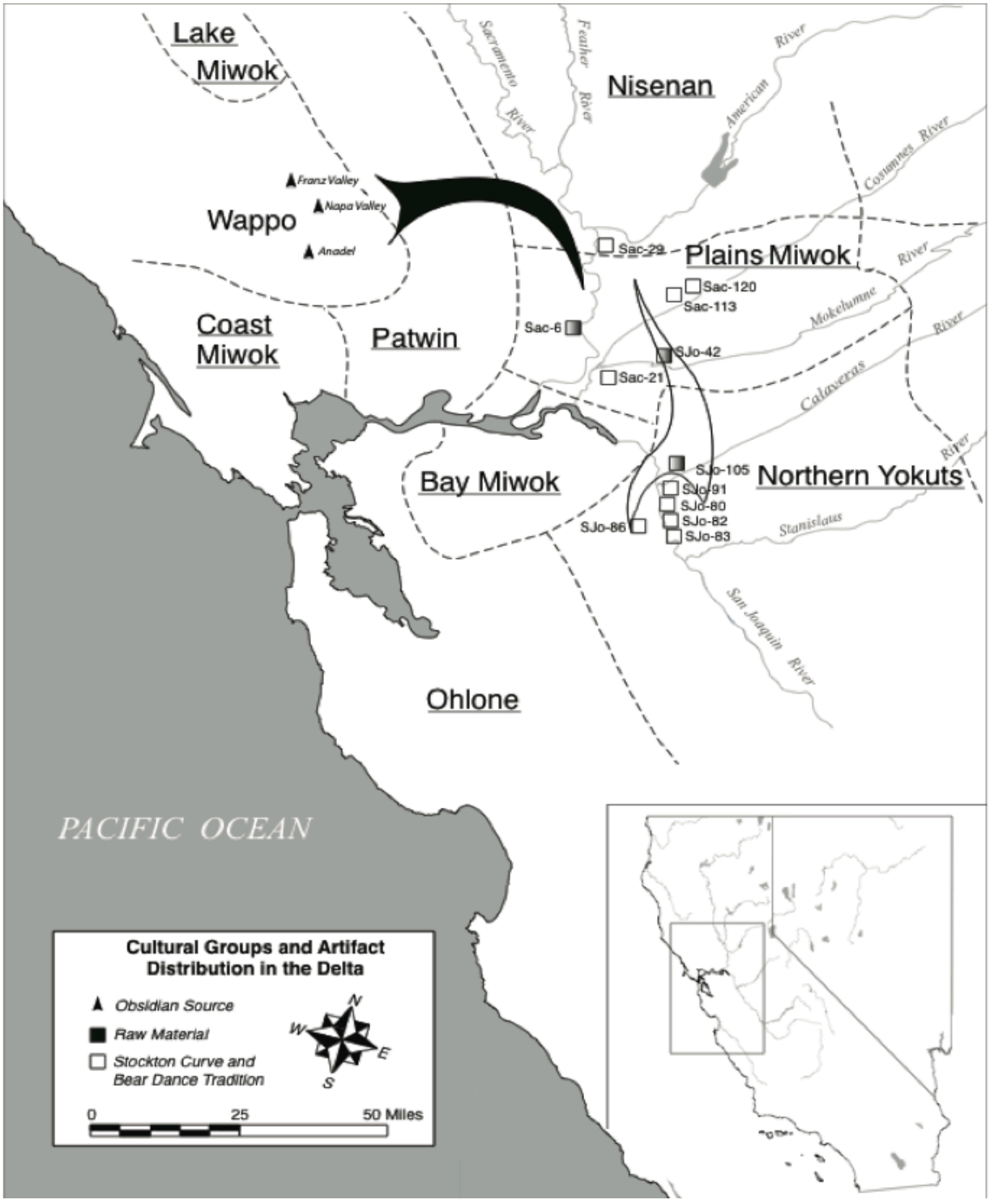


Figure 4.4 Map of sites in study area with hypothesized flow of obsidian and ideas. Shaded boxes represent sites from which obsidian samples and the Bear Dance is known to have taken place.

Chapter 5
Basket-Impressed Baked Clay

INTRODUCTION

There are few artifacts recovered from archaeological sites in the "stoneless" San Joaquin/ Sacramento Delta region that are as ubiquitous and yet as poorly understood as those made from baked clay. While the presence or absence of different forms of baked clay objects have helped define archaeological districts and cultural areas in the Delta (Bennyhoff 1977:48; Schenck and Dawson, 1929:399), they remain enigmatic. There are only approximately one dozen scholarly attempts to understand or describe them in detail (Davis 1959; Elsasser 1963; Heizer 1937; Heizer and Beardsley 1943; Heizer and Pendergast 1955; Holmes 1902; Jones 1923; Kielusiak 1982; Moorehead 1900:230; Pendergast 1957; Schenck and Dawson 1929:360, 399; Sutton 1979; Treganza 1946; True 1957). Baked clay objects are found in a variety of shapes and sizes: from round and triangular, to effigies shaped to resemble birds' heads. There are also vaguely human forms possibly representing women and even clay balls that retain the finger and palm imprints of their makers. Others are much smaller and may have been used as sling stones or ear spools. The leading interpretation of the most common form of baked clay object—balls and ovals—is that they were used as cooking stones. In the Delta pottery was not manufactured. In its place baskets were used for cooking food. Additionally, rocks are absent from the alluvial soil of the Delta. Consequently, it is commonly thought that the clay-balls took the place of actual stones. While this is the most probable explanation for these objects, it has so dominated the literature that most interpretations are reduced to this conclusion. In this chapter I do not focus on baked clay as a tool for cooking. Rather, I explore a unique and undervalued characteristic of some baked clay cooking stones: those inscribed with the impressions of baskets.

Scholarly attention to the broad category of baked clay objects, while limited, focuses on three areas: 1) defining the form and function of these objects (e.g., Heizer 1937; Treganza 1946); 2) using similarities in form among anthropomorphic figurines found in California and the American Southwest to posit cultural connections between the two regions (e.g., Davis 1959; Heizer 1937; Heizer and Beardsley 1943; Heizer and Pendergast 1955; Jones 1923); and 3) attempting to develop time sensitive typologies (e.g., Heizer 1937 and Kielusiak 1982).

In almost every study of baked clay objects in California, the authors mention the presence of basket impressed baked clay yet analysis beyond simple categorization based on shape is nonexistent. The lack of explicit research questions about baked clay artifacts in the Delta is generally problematic. In almost every study of baked clay objects in California, the authors mention the presence of basket impressed baked clay yet analysis beyond simple categorization based on shape is nonexistent. Given the breadth and depth of knowledge about baskets amongst tribal and university scholars in California, considerable archaeological and anthropological evidence concerning subtle details in basket making techniques, and the often lamented dearth of basketry in archaeological contexts (e.g., Hughes and Bettinger 1984:153), it is surprising that more attention has not been given to baked clay objects imprinted with the sidewalls and rims of long since deteriorated baskets or textiles. Although neither the form of the basket nor the design pattern of the basket are preserved, these often neglected impressions provide fleeting glimpses into the lives of individuals who used baskets living in the Delta more than 500 years ago.

Here, I present the results of my analysis of 283[1] pieces of basket-impressed baked clay from ten Late Period sites curated in the Phoebe A. Hearst Museum of Anthropology (Figure 5.1). Building on the work of Larry Dawson (in Heizer 1978), and with the help of his colleague Ralph Shanks (2006, 2010), this is the first substantive analysis of basket-impressed clay objects in California. When I began analysis of basket-impressed baked clay objects I expected to differentiate three levels of cultural affiliation. First, that the observable basket construction techniques would provide information about group affiliation. Second, I searched for social practices that may be highlighted by the impressions. Finally, I hoped to find specific *communities of practice* signaling unique ethnic identities (Lave and Wenger 1991).[2] My analysis demonstrates that cultural affiliation may be expressed in basket-impressed baked clay. However, it only circumstantially indicates social practices, and as of yet, provides no insight into specific communities of practice.

Impressions were found on a variety of shapes of baked clay and the numbers of impressions on an object varied within the group of sampled objects. In some cases there were multiple and overlapping impressions, in other cases, there was a single impression. The amount of surface in which the baked clay impressions covered also varied. Sometimes only a single side of a piece of clay was impressed, other times, the entire object was covered by impressions. Despite these distinctive characteristics, baked clay objects with basket impressions are a neglected subset of the suite of baked clay forms that are themselves often regarded as simply utilitarian items that need no further analysis. Yet, the presence of these impressions combined with the breadth and depth of knowledge about California baskets from contemporary weavers and the ethnographic record is extensive. From birth to death, baskets were and still are important elements in the daily lives of Native Californians. I argue that the application of basket analysis techniques drawn from ethnographic observation to basket impressed clay is a valid method to explore ethnic, cultural, gender, and kinship relationships in the California Delta.

I know of only one other study of basket impressed baked clay: an appendix to the site report for CA-SAC-329 (Soule 1976) written by Larry Dawson. Larry Dawson, a California basket expert, described the basket-impressed clay and observed construction techniques (Dawson 1976:157-159). However, it is not clear from Dawson's descriptions whether he was aware of what I call the *mirroring effect.* This occurs any time an impression of an object with nonlinear (angular features) is made. What is observed in the impression is the exact opposite of what made the impression. If, for example, we observed in a piece of clay stitches from a coiled basket that leaned to the left (\), what we are observing is an impression of a coiled basket that was sewn with stitches that actually leaned to the right (/). It is impossible to include Dawson's data in my discussion without knowing if he recognized this reversal.

This chapter is organized as follows: first I provide a review of the three primary studies of baked clay in California. I will expand on my reasoning for using basket impressed baked clay to approach the study of cultural boundaries. Next, I review the methods I developed to analyze basket impressed baked clay. This section will include definitions of key terms as well

[1] Every identifiable piece of baked clay from each of the ten sites was analyzed. Of the pieces of baked clay I observed, 475 had basket impressions, and only 283 had impressions distinct enough to be analyzed.

[2] Lave and Wenger loosely define *community of practice* as "...a set of relations among persons, activity, and world, over time and in relations with other tangential and overlapping communities of practice" (Lave and Wenger 1991:98).

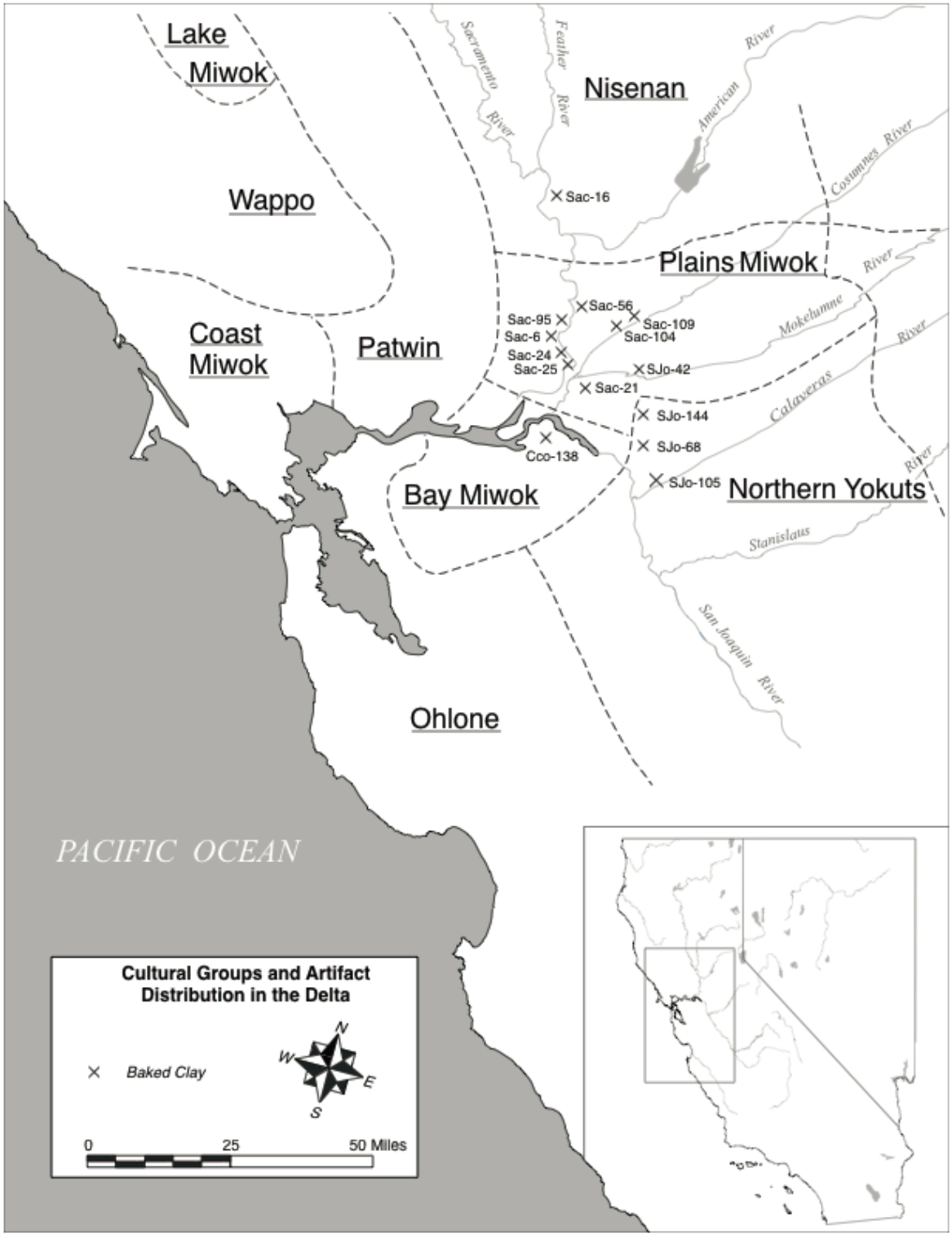


Figure 5.1 Showing the sites from which basket-impressed baked clay objects were examined.

as images and extensive descriptions. Next, I present the results, discussion, and conclusions of my analysis. Finally, I will consider the data gathered from basket impressed baked clay considering the five boundary models I proposed in Chapter 1.

This research makes a significant contribution in four areas: 1) I demonstrate that these baked clay cannot be described in terms of their form; 2) I contribute to the archaeological, cultural, and historical understanding of the pre-European California Delta; 3) I suggest an expansion of the repertoire of California basket studies to include "virtual" baskets as represented through basket-impressed baked clay; and 4) I analyze a unique data set that provides new insights about the possible interactions taking place between people across the accepted Plains Miwok/Northern Yokuts cultural boundary.

BACKGROUND

There are approximately a dozen archaeological studies of baked clay objects found within the Delta (e.g., Davis 1959; Elsasser 1963; Heizer 1937; Heizer and Beardsley 1943; Heizer and Pendergast 1955; Holmes 1902; Jones 1923; Kielusiak 1982; Moorehead 1900:230; Pendergast 1957; Schenck and Dawson 1929; Sutton 1979; Treganza 1946; True 1957). The first references to baked clay in the Delta region of California were made by Warren Moorehead (1900) and P.M. Jones (1923). Jones, for example, noted:

> They [clay balls] seem to be of two general sorts: first, those which are perfectly *plain*, rough, and evidently molded by the hands...; second, specimens of various forms, simply *ornamented*...They [the ornamented type] seem to me rather the expression of the fortuitous moulding of the pliable clay in the hands of one whose occasional occupation was the making of balls used for cooking...I cannot but look upon these simple objects of baked clay as exceedingly interesting specimens representing, as they seem to, a preliminary step toward the discovery of true pottery (Jones 1923:121-122).

This early description of baked clay objects relies heavily upon two categories of classification, form and function. Ultimately, Jones favors a conclusion grounded in social evolutionary theory: the baked clay objects were of intellectual interest only in so far as they represented a stage in ceramic manufacture. The focus on form and function underpinned by evolutionary theory to understand baked clay from the Delta has, in large part, dominated interpretations of these objects.

As with *Olivella* and clam shell beads, in *Archaeology of the Northern San Joaquin Valley* Schenck and Dawson (1929) were the first to engage in an extended discussion of the baked clay found in this region. Schenck and Dawson noted baked clay was exceptional for several reasons: their abundance in the area, limited distribution, variety of form, uncertainty of their use, and that they show innovation in overcoming the limitation of not having any geologic stones to cook with. Their analysis focused on descriptive categories such as quantity, occurrence, material, and size and shape. Additionally, they included a short paragraph on the "purpose" of baked clay suggesting form and function were closely linked. Their greatest contribution came from categorizing baked clay not according to form, but rather the presence or absence of decoration. One of their key findings was that the majority of the cylindrical forms had basket impressions on all sides, whereas most of the other forms, such as triangles, rounds, ovals, were infrequently found with basket impressions (Schenck and Dawson 1929:364). While

focusing on the decorative elements of baked clay as opposed to form presents interesting possibilities for interpreting baked clay, Schenck and Dawson chose not to extend their analysis in this direction and their limited analysis of basket-impressed clay objects remained focused on formal variation (e.g., size, shape, etc.).

Unlike Schenck and Dawson (1929), Heizer's (1937) *Baked Clay Objects of the Lower Sacremento Valley, California* created a more extensive typology of baked clay based on morphological characteristics and traced the spatial and temporal presence of baked clay at sites in the Delta. Guided by the then popular diffusionist model of cultural change, Heizer used his typology and analysis of the spatial and temporal distribution of baked clay to address two popular issues (Figure 5.2). First, to explain the function of the baked clay in terms of a response to the environmental challenges (e.g., not having stones, but needing to cook in baskets) and, second, to posit possible connections between the Delta, Southern California, and the American Southwest. Heizer believed that the variety of clay forms were noteworthy only insofar as they provided evidence for cultural connections.

To address his first question about function, Heizer cites Gayton (1929) and Kroeber (1928) and their ethnographic accounts of the use of baked clay as cooking stones. The crucial point for Heizer however was,

> the relationship between the baked clay artifacts and the nearest true pottery...the appearance of a new physical type associated with the distinctive cultural components indicates rather strongly the actual intrusion of an alien population...to sum up ... [there was] a genetic connection between the Southwestern and lower Sacramento Valley pottery (1937:43, 47).

Following Heizer's work there were few subsequent noteworthy articles that specifically discussed baked clay figurines and clay artifacts shaped to resemble bird heads (e.g., Davis 1959; Elsasser 1963; Heizer and Beardsley 1943; Heizer and Pendergast 1955; Pendergast 1957). These articles were derivative, building on Heizer's hypothesis that there was a cultural and genetic connection between the American Southwest and California. They attempted to show this connection was discoverable through analysis of the patterned distribution of ceramic forms.

To date, Kielusiak's (1982) Masters Thesis provides the most comprehensive review of what is known about baked clay in the Delta region. Her thesis is impressive because it summarizes a variety of scattered manuscripts and site reports into a single location and presents the results of analysis of 3,000 complete and 21,000 fragmentary pieces of baked clay objects from twenty-five sites located within four counties (Sacramento, San Joaquin, Sutter, and Yolo). Her goals were two-fold. First, to provide a comprehensive typology that addressed the inadequacies in previous studies and to create a robust database for further study of baked clay. Second, to note temporal and geographic correlations among types of baked clay within the Delta.

Instead of focusing on *form,* as in previous typologies of baked clay, Kielusiak attempted to interpret their *function*. She noted, "a typology based strictly on morphological descriptions, though being consistent, would in this case result in a cumbersome list of descriptive terms" (Kielusiak 1982:33). To this end her morphological categories, solid, grooved, concave, and

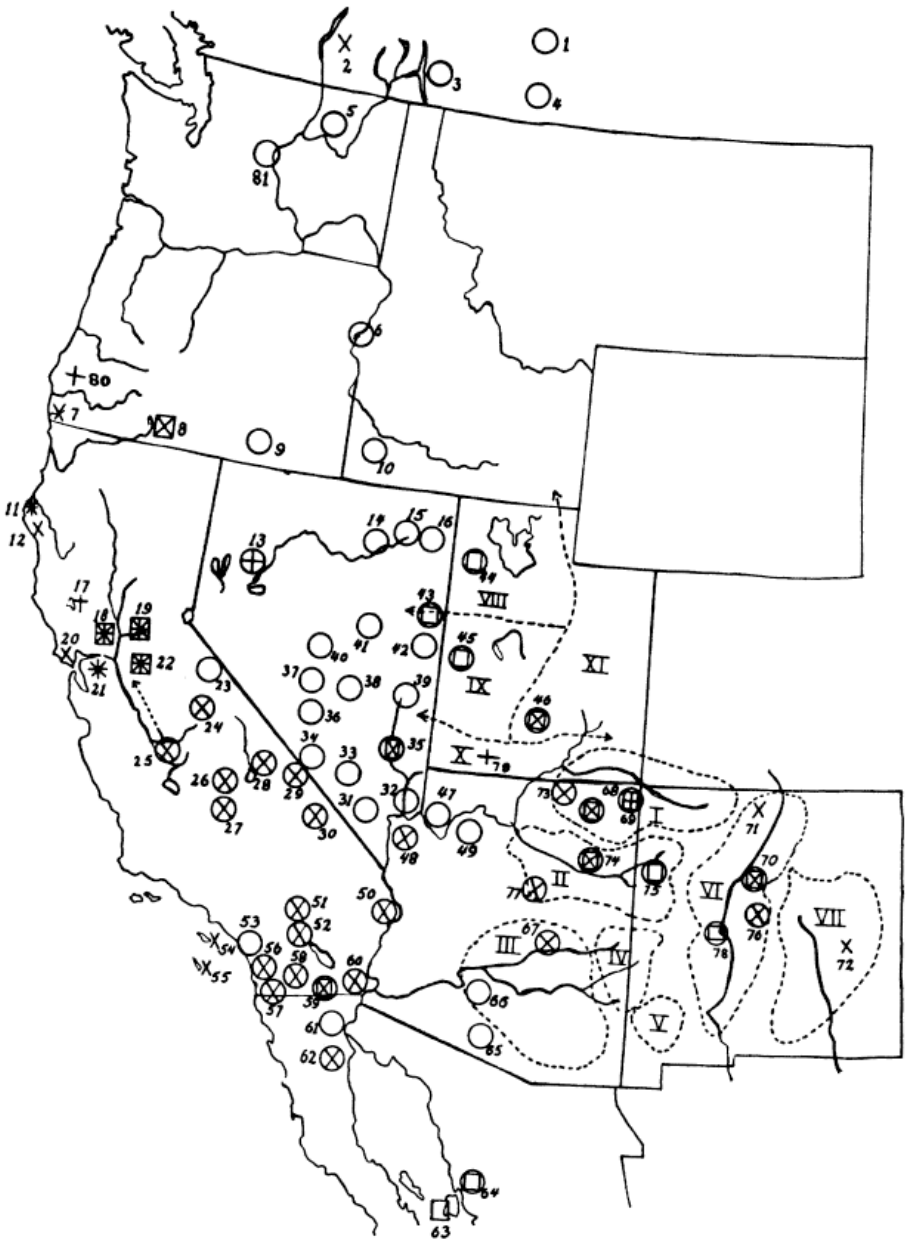


FIG. 4.—Distribution of the Practice of Molding and Baking Clay Artifacts

□—Figurines (clay dolls, animal or bird effigies).

×—Tubular clay pipe.

○—True pottery vessels.

+—Baked-clay objects (balls, etc.)

Starred numbers indicate archaeological occurrence.

Areas I–VII, After Kidder, 1924, fig. 5; areas VIII–XI, After Steward, 1933, fig. 1. (Plain pottery and clay figurines generally distributed in the Northern Periphery. Steward, 1933.)

Figure 5.2 Illustrating Heizer's attempt to map the distribution of and relationship between clay forms in California, the Great Basin, and the Southwest. After Heizer 1937:44.

perforated are merely background description for what she considers functional labels: charmstone, plummet, pipe, bead, etc. Her focus on function reoriented the typology toward explorations of social settings and interpersonal interactions within which baked clay existed. This line of thinking, as Roddick (2009) recently showed, leads to specific insights into potters' communities of practice. Function prioritized over form is an important shift in any typology and a significant improvement over previous studies of baked clay; goals that Schenck and Dawson (1929) and Heizer (1937) set, but could not meet. Kielusiak's (1982) work provided a large database and extensive summaries of the baked clay found at 25 different archaeological sites in the Delta. For these reasons it makes a significant contribution to California archaeology.

RATIONALE FOR STUDYING CULTURAL BOUNDARIES WITH BASKET-IMPRESSED CLAY

It could be that Adan Treganza was correct when he wrote, "of all the lines of evidence upon which California archaeology is based, none is more interesting or offers any more of a problem than that of baked clay artifacts" (1946:3). Unlike many of the other artifact types that occupy the attention of California archaeologists, shell beads and obsidian chipped stone for example, baked clay receives little academic attention. When consideration is given to baked clay objects it focuses largely on creating typological frameworks and debating whether or not form or function should be the organizing principle of the typology. Unhappy with this discussion and the lack of insight it provides to questions central to this dissertation—namely those concerning cultural identities—my interest in baked clay objects diverges significantly. In this chapter, I focus on an often ignored single type of baked clay object: those retaining the impressions of baskets. Basket impressed clay forms are an unexplored resource for creating a more robust understanding of social and cultural interaction in the Delta region during the Late and Historic Periods. I find support for this in two places: California basket studies, specifically, and ceramic analysis, more generally. In this section I will briefly highlight important basket and ceramic analysis that, taken together, point to the applicability and provide justifications for investigating social and cultural interactions though basket impressed baked clay.

Anthropological and ethnographic interest in the manufacture, design, distribution, and formal variation of California Indian baskets has occupied the attention of collectors, scholars, and to some extent the general public since at least the late 19th century (Elsasser 1978; Jordan and Shennan 2003). Analysis of baskets collected by early ethnographers (hereafter referred to as 'ethnographic baskets') engendered discussions of, for example, oral traditions (Turnbaugh 1999) and cultural and linguistic connections between groups (Jordan and Shennan 2003). These studies provide a useful starting point for my own work.

In a short review chapter on California basketry, Elsasser (1978:626-634) summarized basket construction techniques and described various types of baskets and their uses. In his conclusion, he echo's Kroeber's interpretation of California basketry. Elsasser states, basketry typifies "...the tremendous predominance of unmotivated custom and habit over conscious utilitarian, artistic, or religious purpose" (Kroeber 1909:249 cited in Elsasser 1978:641). This statement underscores the cultural dispositions, practices, and orientations enmeshed in the creation of baskets. Kroeber (1925) did not discount the importance of technical or other choices regarding the creation of baskets, but rather highlighted the learned, repeated, and embodied practices embedded in their construction. Kroeber expands on this idea when he noted that weaving technique was an indicator of group identity (414 - 415). Elsasser eloquently

summarized, "...regional differences may be cited where simple choices are involved, as in direction of weft pitch [twist] in twining or in direction of work, use of interlocking or non-interlocking stitches or use of split or non-split stitches in coiled basketry..." (Elsasser 1978:626-627). Elsasser also provided several tables summarizing analysis conducted by Larry Dawson to support these claims. The tables outlined decades of Dawson's close study of California baskets and lend support to the idea that group affiliation and basket construction technique are connected. Recent scholarship by Ralph Shanks, which builds Dawson's study of baskets, replicates and adds both depth and specificity to this earlier work (Shanks 2006, 2010). Importantly, Shanks (*pers. comm.* 2010) in considering remnants of baskets and textiles recovered from archaeological contexts found continuity in construction techniques over long spans of time. This continuity is important in that it provides support for my assertion that the basket-making techniques observed in baked clay recovered from archaeological contexts can be used to link contemporary groups of people to their ancestors as part of the process of cultural affiliation.

Similarly, John Pryor's (1987) dissertation is an excellent example of how an anthropological study of baskets from Northern Central California can lend insight into social and cultural interactions and connections. Pryor pays particular attention to the intersection of basket style (design patterns), weave technique (e.g., twining and coiled), and form (shape) across time and space in the Pomo and surrounding ethnolinguistic areas as a way to explore social and cultural relationships within and between language family boundaries. Pryor maintained that organizing his data by language family as opposed to traditional archaeologically defined cultural boundaries allowed him to better account for what he saw as the movements of people into and out of Northern California through time reflected in linguistic shifts. Second, he argued that a focus on language group better accounted for the social and political reality that existed before European contact and the formal naming of tribes by anthropologists and government agents.[3] That is, there were no "Pomo" in the modern political sense, but rather smaller groups of people with allegiances to their home tribelet that spoke related dialects and allied with one another through ceremony, tradition, and kinship. Organizing his data by language enabled Pryor to circumvent a discussion about political relationships and instead focus on social and cultural interactions.

While there are obvious problems in assuming that shifts in language mirror the movements of groups of people through time and space, Pryor was nevertheless successful in arguing that design, weave, and form were "...the most sensitive [to]...understanding (1) interaction (as measured by geographic distance), (2) shared culture history (measured by a shared language), and (3) technology (measured by similarity in weaving technique)" (Prior 1987:12). From his analysis of 1,222 baskets from Northern California, Pryor found, "...the major language groups (Pomoan, Yuki, and Patwin) are stylistically distinctive. However, boundary groups (e.g., the Hill Patwin and Huchnom), or groups isolated from the major language groups (e.g., the Wappo) show a mixing of stylistic traits" (Pryor 1987:165). His findings demonstrate that attention to the spatial distribution of weave technique, for example, can contribute to understanding group difference, within and across borders. Although Pryor is unable to say which specific types of relationships or interactions engendered the differences and

[3] As I point out in chapter 3, there is close relationship between ethnolinguistic and archaeological boundaries and in many cases the two are inseparable. And as I highlight in chapter 6, it is not always safe to assume that data should be analyzed at the scale of the ethnolinguistic unit.

similarities he observed in his data, that they were observed is important. It points toward the usefulness of basket analysis for teasing apart the existence of group difference.

Research conducted by Pryor, Kroeber, Dawson, and Shanks support a central premise of my own work; that differences in construction technique can provide important insights into group affiliation, personal relationships, and social interactions in the absence of direct observation. Archaeologically, these data are obtained from the impressions in baked clay, allowing us to study a never before considered class of artifacts—virtual baskets.

METHODS

When I began researching basket impressed baked clay there were no substantive descriptions of these specific objects in the archaeological literature. This was disconcerting given what is known about the relationship between construction techniques of ethnographic baskets and their ethnic affiliations. I began my analysis of basket impressed baked clay object by modifying analytical techniques developed in basket studies and applying them to clay.

The first step in basket analysis is to understand the two main categories of baskets made by California Indian people: *twined* and *coiled*. Second, within each of these categories are further refinements. Twined baskets for example, are described as having either *close* or *open* work. Close twined baskets are constructed with *plain*, *diagonal*, and *lattice* twining stitches whereas open work twined baskets are constructed using only the plain twining technique. [4] The orientation of the stitches, or direction of weft twist, also an important characteristic, are herein described as either angled *up and to the right* (/), or *down and to the right* (\ ). On the other hand, the type of stitch used can is used to describe coiled baskets. There are three types of stitches used in coiled basketry: *non-interlocking*, *interlocking*, and *split* stitches. As with twined stitches, stitches on coiled baskets can also be characterized by the direction they lean, either *left* or *right*. The direction a stitch leans on a coiled basket indicates the direction the basket maker worked as they constructed the basket. The stitch direction on a coiled basket is opposite to the work direction, therefore, if the stitch on a coiled basket leans to the left, that means the makers work direction was actually to the right.

There are two limitations in this approach to baked clay basket-impressions. First, and as with all the other artifact types analyzed in this dissertation, the excavation and collection methods as well as the number of times a site was excavated varied. For example, CA-SAC-6 was excavated intermittently between the 1920s and 50‘s and CA-SJo-42 was excavated several times between 1972 and 1975. Whereas CA-SAC-6 was almost completely excavated, many of the other sites were only sampled. This potentially skews the data and calls into question whether or not the numbers of basket-impressed baked clay objects from the other sites provide a representative sample and if basket-impressed clay can provide insight into the ratio of twined and coiled baskets used by the inhabitants of these sites. For example, at CA-SJo-105 no baked clay pieces with coiled impressions were uncovered. This cannot be taken to mean that there were no coiled baskets at CA-SJo-105, but only that none were recovered during excavation. Second, the data presented in this chapter does not speak to the presence or absence of baskets made with other construction techniques. For example, no diagonal or lattice twined impressions were observed, however analysis of charred basket remains from the same area demonstrates that lattice and diagonal baskets did exist. Admittedly, trying to describe different types of baskets using stitch technique and orientation or lean of the stitches is a difficult endeavor. Yet, as previous research demonstrates (i.e., Elsasser 1978:633; Kroeber 1925:415; Shanks 2009, 2010), these are all essential characteristics for determining the ethnic affiliation of baskets. Below I provide definitions, illustrations and a flow chart showing the relationship of the italicized terms in the above paragraph.

[4] In this section I forego providing lengthy, and what will inevitably be confusing definitions of *diagonal* and *lattice* style stitches, because they were not the primary type of stitch observed among impressions. Rather I will describe what they look like to the naked eye.

DEFINITIONS

WARP. Is the foundational material for any basket around which wefts are affixed. Depending on the basket type, twined or coiled, they are oriented differently. For twined baskets warps are oriented perpendicular to the ground (||) whereas the warps on a coiled basket are oriented parallel to the ground (=).

WEFT. Is the material used to bind warps. It is also part of the basket used to determine the kind of stitch used to hold the basket together.

Figure 5.3 Highlighted section of plain and lattice twined close-worked Pomo mortar-hopper. Basket in private collection. Photograph by author.

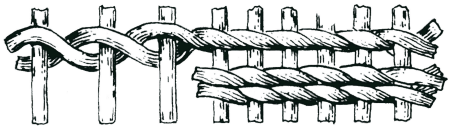


Figure 5.4 Illustration of close work plain twining. After Elsasser (1978:626).

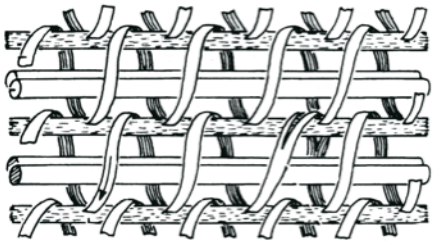


Figure 5.5 Non-interlocking coiling stitch. Right leaning stitch, arrow illustrates direction of work to the left. Split stitch circled at middle right. After Elsasser (1978:626).

TWINING. Is a construction technique that begins with a solid foundation of vertically oriented (head to toe) ridged elements called warps around which two or more supple elements called wefts are crossed, then twisted. This process of crossing and twisting the weft takes place in-between each warp and is what hold the basket together. Twined baskets were generally used for daily tasks (Figure 5.4).

COILING: Is a construction technique that begins by wrapping a base material, the warp, upon itself in spiral rounds. The warp of a coiled basket is held together with stitching. The stitching of a coiled basket is the weft. By definition, coiled baskets are "close"[5] worked baskets. Stitches can be interlocking or non-interlocking (Figure 5.6).

CLOSE WORK: With close work, successive rows of wefts bound about one another. (Figures 5.3 and 5.4).

[5] This is in contrast to twined baskets that can be either close or open worked.

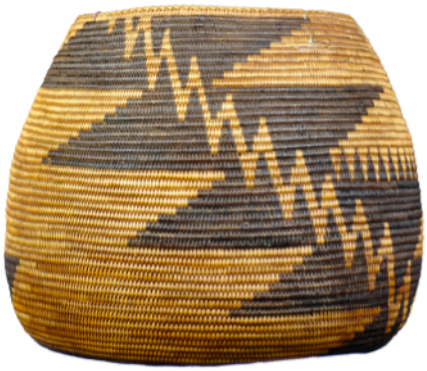


Figure 5.6 Catalogue # 1-2111527 Miwok coiled basket. Image courtesy of the Phoebe A. Hearst Museum of Anthropology. University of California, Berkeley. Photograph by the author.

Figure 5.7 Miwok plain twined open-worked thresher. Image courtesy of the Phoebe A. Hearst Museum of Anthropology. University of California, Berkeley. Catalogue # 1-7237. Photograph by the author.

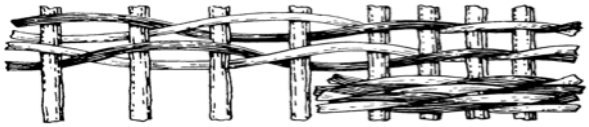


Figure 5.8 Diagonal twining technique. After Elsasser (1978:626).

Figure 5.9 Lattice twining technique. After Elsasser (1987: 626).

OPEN WORK: Twined, open worked baskets have spaces of varying widths between successive rows of wefts (Figure 5.7).

PLAIN TWINING: This twining technique is the most basic and simply describes the process of continuously twisting two wefts around warps. All of the images of twined baskets in this section illustrate plain twining (Figure 5.7).

DIAGONAL TWINING: Is recognized because the stitches appear as if they were courses of bricks laid down as in a wall (Figure 5.8).

DIRECTION OF WEFT TWIST:[6] The direction of weft twist is a term exclusive to describing twined baskets. Direction of weft twist refers to the direction the weft is "pitched" or leans.[7] To put it another way, direction of weft twist is a way of describing whether the wefts on a twined basket were twisted right over left or left over right. Technically in basket analysis, the direction of weft twist is described as either down and to the left (/) or down and to the right (\). For the sake of simplicity however, all descriptions of the direction of weft twist work are given in terms of 'right.' In this chapter, the formal description "down and to the left," and "up and to the right" (Figure 5.11) refer to the same thing: (\). Therefore I used 'right' as the dominant modifier thus simplifying terminology, which reduces the chance of confusion and error—especially later when applying these basket analysis concepts to baked clay. Therefore, direction of weft twist descriptions will be "down and to the right" or "DR" for short and "up and to the right" or "UR" for short.

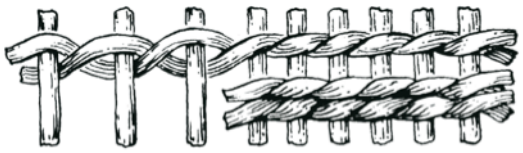


Figure 5.10 Plain twined work illustrating '*down* and to the right' (\) direction of weft twist. After Elsasser (1978:626).

Figure 5.11 Plain twined work illustration '*up* and to the right' (\ ) direction of weft twist. After Elsasser (1978:626).

[6] Direction of weft twist and stitch angle can be confusing. Formal directional indicators up and to the right (UR) and down and to the right (DR) refer ONLY to twined baskets and stitch angle left and right refer ONLY to twined baskets. In an attempt to reduce the number of variables I had to deal with and because of the mirroring effect, I simplified where possible.

[7] This is not to be confused with work direction. Generally in basket analysis, work direction is the opposite of the lean of the stitch. However, for the analysis of basket impressed baked clay, the lean of the stitch and the work direction are the same. This is explained further in the definition of "stitch angle."

STITCH ANGLE: Stitch angle is concept applied exclusively to describe the direction of individual stitches making up *coiled baskets*. Stitch direction is described as either LEFT(\ ) or RIGHT (/). That means that when observing a coiled basket, the top of the stitch (or weft) will lean to either the left or the right. It is from this lean that the stitch angle is described as either left or right. The direction of the stitch angle is also used to indicate in which direction the basket was constructed. If the stitches on a coiled basket lean to the left, then the basket maker originally added stitches by progressing to the right (clock-wise) (Figure 5.13). If the stitches on a coiled basket lean to the right, then the basket was turned to the left (counter-clockwise) during construction (Figure 5.12).

Figure 5.12 Miwok coiled basket illustrating *right* (/) leaning stitches (left work direction). Image courtesy of the Phoebe A. Hearst Museum of Anthropology. University of California, Berkeley. Catalogue # 1-9933 Photograph by the author.

Figure 5.13 Yokuts coiled basket illustrating *left* (\ ) leaning stitches (right work direction). Image courtesy of the Phoebe A. Hearst Museum of Anthropology. University of California, Berkeley. Catalogue # 1-70419 Photograph by the author.

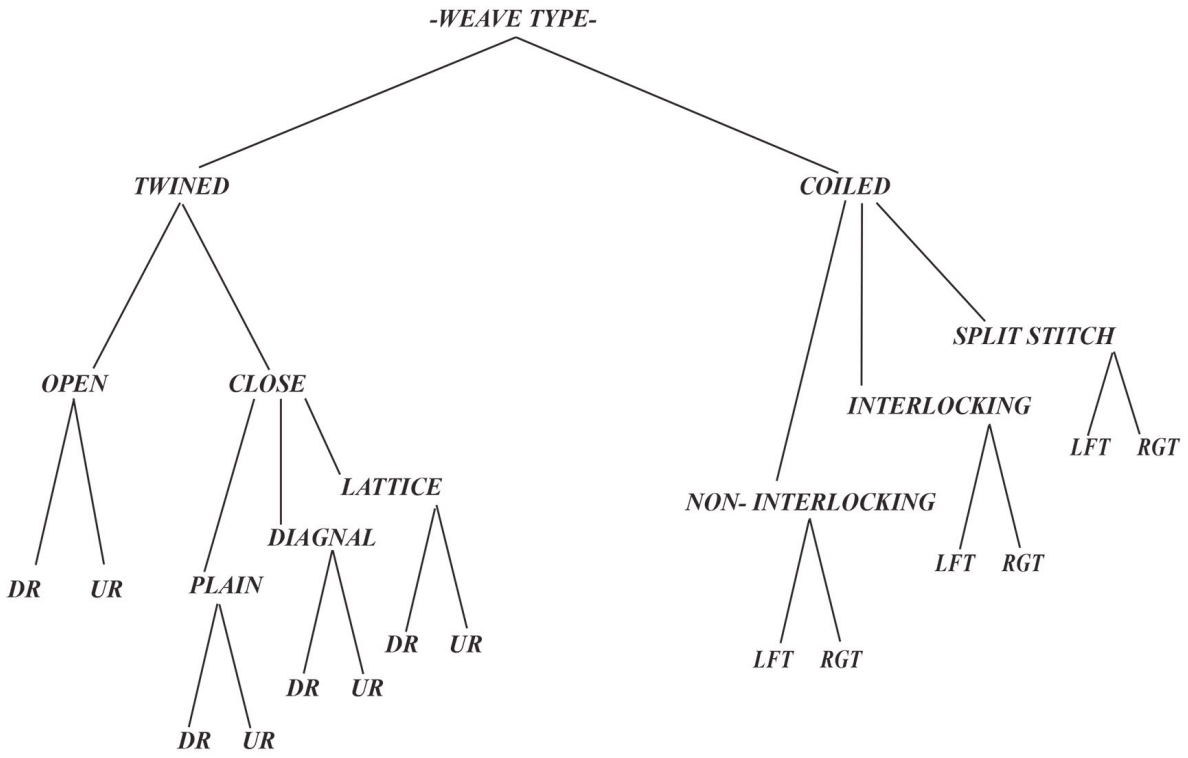


Image 5.14 Flow chart depicting the relationship among the primary components of California ethnographic baskets. The short forms are as follows: Down and to the Right (DR); Up and to the Right (UR); Left (LFT); Right (RGT).

My analysis of basket-impressed baked clay followed the flow chart depicted in Figure 5.14. With each piece of baked clay that had a basket impression I first determined the type of weave, twined or coiled. If the basket was twined, for example, I noted if the work was open or close. Next, I recorded the type of stitch used: plain, lattice, diagonal for twined baskets and non-interlocking, interlocking, and split stitch for coiled baskets. Lastly, I recorded the stitch angle: up and to the right (UR) or down and to the right (DR).

Initially, the transition from analyzing baskets to basket impressions was confusing. The primary issue was how to describe the work techniques. Holding a basket and describing the work techniques is fairly straightforward. The top and the bottom of the basket are obvious, as is the inside and outside. Not having the actual basket that made the impression created a challenging issue in regards to recording relevant information. To address this problem, I created two impressions, one twined and one coiled, from a private collection. To do this, I used Quake Hold™. Quake Hold™ did not leave any residue on the baskets and was sufficiently soft to create an impression without having to apply excessive force to the basket surface. Those impressions are shown below in Figures 5.15 and 5.16.

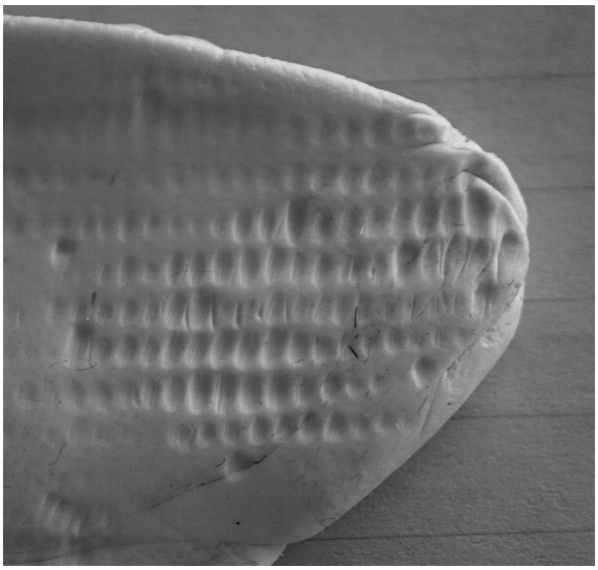


Figure 5.15 Quake Hold™ impression of a coiled basket with right leaning stitches. Photograph by the author.

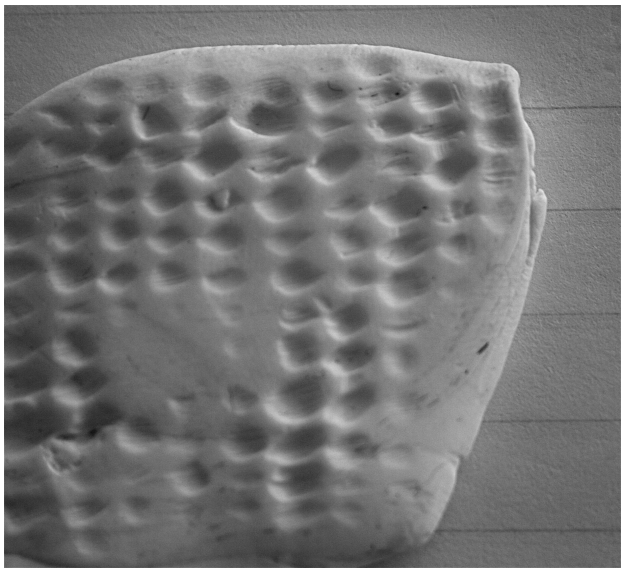


Figure 5.16 Quake Hold™ impression of plain, close-work twined basket. Photograph by the author.

Creating this reference guide led me to four key methodological findings. First, making a type collection highlighted the importance of orienting the warps correctly. When holding a real basket there is no question about the correct orientation. With impressions in clay however, this is not always obvious because the clay is often worn and the impressions difficult to distinguish. Incorrectly orienting an impression means mistaking a twined for a coiled basket and misrepresenting the angle of the stitch and results in incorrect inferences about affiliation. To avoid this problem, I found it important to focus on the size and shape of the stitch as well as point of intersection between stitches. By doing this, each piece of clay was correctly oriented and the technique used to make the basket accurately described. For example, impressions of wefts from twined baskets tended to be globular, boxy, or squarish (Figure 5.16). The length and width of each stitch were approximately equal. Coiled impressions on the other hand tended to be thinner, longer, and more like rice kernels, the length being greater than the width (Figure 5.15). Additionally, the point at which two stitches intersected was different for twined and coiled baskets. The space in between stitches of the twined basket was more uniform and tended to be shaped roughly like a diamond and create the overall impression of a fairly uniform and straight line (compare to Figure 5.16). The intersection of stitches on a coiled basket however created a staggered "V" pattern where one side of the V was thicker than the other giving the impression of a more jagged line (Figure 5.15).

The second crucial observation was the *mirroring effect.* As I noted earlier, the mirroring effect is the result of an impression of an object having the mirror opposite characteristics of the object itself. The best example of this is the deliberate displaying of the word "ecnalubmA" on the hood of an Ambulance on the so that it can be read in motorist's mirrors (Figures 5.17 and 5.18). While this rule holds true for twined baskets, coiled baskets are an exception. As I noted above, the angle of the stitch on a coiled baskets is opposite the work direction. Because of the mirroring effect, the angle of the stitch observed from the impression accurately described the direction of work. For example, if a real basket was described as coiled, with right (/) leaning stitches, the direction of work would be to the left. An impression of the same basket on the other

hand would be just the opposite. The stitches would be described as leaning to the left (\ ) but to describe the direction of work as to the right would be incorrect. As was seen with the real basket, work proceeded to the left. Because of the mirroring effect on the impressions made by coiled baskets, the observed pitch of the stitch accurately describes the direction of work. Admittedly, my observations about how to transition descriptions of the direction of weft twist and the pitch of coiled stitches from real baskets to impressions made by them are based on simple geometric patterning and image manipulation. However obvious this finding may be, it is nonetheless an essential one. Making accurate observations of relevant traits bears directly on drawing appropriate conclusions about the potential identities of the people who made the baskets for which only impressions in clay remain.

Figure 5.17 Basket-impressed baked clay illustrating plain twined close-worked basket. Observed direction of weft twist is up and to the right (⟍). Image courtesy of the Phoebe A. Hearst Museum of Anthropology. University of California, Berkeley. Catalogue # 1-67455. Photograph by author.

Figure 5.18 Impression of basket-impressed baked clay illustrating what the original plain twined close-worked basket that made the impression to the right looked like. Note the mirroring effect, the observed direction of weft twist is down and to the right (⟍). Image courtesy of the Phoebe A. Hearst Museum of Anthropology. University of California, Berkeley. Catalogue # 1-67455. Photograph by author.

Image 5.19 Basket-impressed baked clay illustrating plain twined open-worked basket under normal lighting conditions. The observed direction of weft twist is up and to the right (\), although difficult to see. Image courtesy of the Phoebe A. Hearst Museum of Anthropology. University of California, Berkeley. Catalogue # 1-38507. Photography by author.

Figure 5.20 Basket-impressed baked clay illustrating plain twined open-worked basket under external low-angle lighting conditions. The observed direction of weft twist is up and to the right (\), and much easier to see. Image courtesy of the Phoebe A. Hearst Museum of Anthropology. University of California, Berkeley. Catalogue #1-38507. Photography by author.

Figure 5.21 Basket-impressed baked clay illustrating what Image 5.20 looked like when it was 'raked' with light. Also note, this is an excellent example of the mirroring effect illustrated by comparing the direction of weft twist in images 5.20 and 5.21. Image courtesy of the Phoebe A. Hearst Museum of Anthropology. University of California, Berkeley. Catalogue # 1-38508. Photography by the author.

Image 5.22 Sample light setup used to create a raking light and highlight subtleties of the basket impressions in baked clay. Photograph by the author.

Third, the once highly textured surface of baked clay objects were often smooth. This made it difficult to clearly distinguish different types of stitches. Additionally, natural and overhead incandescent lighting bleached any subtle contrasts that might be visible to the naked eye. To mitigate this, I reduced the lighting in the research area and held an LED flash light at a low angle allowing the light to rake across the surface of the object. With the flashlight in hand, I was also able to rotate the object and the light source. This process created shadows across the surface of the baked clay and highlighted different contours of an impression that were otherwise invisible (Figures 5.19. 5.20, 5.21). I borrowed the idea of changing both the position and angle of a light source relative to an object from the Cultural Heritage Imaging group (CHI). CHI uses Reflection Transformation Imaging (RTI) technique to magnify and render visible contrasts and characteristics of an object that are otherwise hidden by direct light. Their technique is much more sophisticated than mine, but the principal was the same. CHI created a dome with multiple light sources. Technicians take multiple high-resolution photographs of the same object as it is lit from a different angles. They then stitch the photographs together using free software developed by CHI and create an image of a single object with a moving light source. Using a mouse, the researcher can change the angle of the light in relation to the object—similar to holding a flashlight; only better—and instantaneously view the changes in surface relief to the object. While my approach was much simpler, the principle remained the same and was effective in accentuating construction techniques. Adjusting the contrast and lighting angle across the object, even by hand, allowed me to include many pieces of baked clay that I would have otherwise excluded from my sample (Figure 5.22).

Lastly, conversations with Ralph Shanks led to a general observation and a cautionary note about the relationship between my basket analysis and the descriptions of cordage recovered from archaeological contexts. Cordage recovered from archaeological contexts is often reported as having a ‘Z’ or ‘S’ twist. This distinction is made by looking down the length of the cord to see if the twist is to the right (Z) or to the left (S). As with baskets, the orientation of the cord - upside down or right side up - does not matter. While using Z or S twist is appropriate to describe cordage, applying these analytic observations to baskets results in inaccurate and

unusable descriptions. Unlike cordage that is twisted in a straight line, baskets and textiles are constructed by twisting or stitching wefts around warps in a concentric manner. In this sense, cordage can be thought of as having a linear or vertical construction orientation, whereas baskets have horizontal construction orientation. Applying cordage descriptions to baskets or textiles when there is a ninety-degree rotation of construction orientation effects whether accurate descriptions can be made. A Z twist could be said to illustrate a twist that is up and to the right (/) whereas an S-twist is down and to the right (\). But if descriptors used for cordage were applied to baskets and textiles and they are not rotated ninety degrees, a necessity that results from the different orientations of construction technique, incorrect descriptions and inferences will be made.

Understanding the logic of basket making, employing a standardized vocabulary, being able to do basic mental gymnastics and image manipulation, as well as keeping images of actual twined and coiled baskets in my mind, made the analysis of the clay pieces much less confusing and tedious. Doing this also continually reminded me of the time and care placed into making the baskets used to create the impressions. The descriptions and labels on the graphs I provide below take the above findings into account.

RESULTS

I analyzed all baked clay objects listed as having basket impressions in the Pheoebe A. Hearst Museum of Anthropology (n=562). Of the 562 objects, 311 (55%) had sufficiently clear impressions to determine basket type and construction technique (Table 5.1). The 251 objects not included in this analysis are not included for one or both of the following reasons: either the object was listed as having basket impressions but there were none or the impressions were wrongly attributed to human modification and/or there were impressions, but they were too faint to make conclusive statements about basket type and construction technique. The 311 pieces with enough of an impression to analyze were from 15 archeological sites located in the following counties, (the count and percent of the total number of basket-impressed objects from each county are in parenthesis), Kern (1 or .3%), Contra Costa (2 or .6%), Sacramento (280 or 90%), and San Joaquin (23 or 7.4%) counties (Figure 5.23). A few pieces of clay were from unknown locations near the current Sacramento/ San Joaquin County line (5 or 1.6%), these are labeled CA-SAC-SJo in Tables 5.1 and 5.2.[8] Seventy-one (22.8%) of analyzed objects had impressions from coiled baskets and 240 (77%) of analyzed objects were from plain-twined baskets; no lattice or diagonal twining was observed (Figure 5.23). Of the 240 objects exhibiting plain-twined work, direction of weft twist could be determined for 197 of them (Figure 5.24). For the remaining 43 objects, no direction of weft twist could be determined. Of the 71 pieces of baked clay showing coiled work, pitch of stitch was determined for 68, but for the remaining 3 pieces, no pitch direction could be determined (Figure 5.25).

DISCUSSION

Analysis of these objects presents several methodological challenges as well as exciting possibilities. First, deciphering impressions is demanding. The important characteristics that

[8] There are a series of catalogue numbers from an unknown number of unidentified sites along the Sacramento/ San Joaquin County line that were typically collected by local residents during the early part of the 20th Century and donated to the museum. There is no province information for these objects other than this general locational data. I've included them here because specific province data is less relevant to this study than general location.

distinguish twined from coiled baskets are fairly obvious when one holds a basket in their hands; less so when interpreting the basket impressions through the medium of baked clay. Second, preservation and the presence of baked clay with impressions is contingent upon site formation processes and excavation strategies (Schiffer 2002). Baked clay objects from the Delta were made with local river-bottom clay that rarely included a temper. This increases the likelihood of fracturing and disintegration over time and significantly reduces the potential sample size (Heizer 1937; Kielusiak 1982). Moreover, the ubiquity of baked clay in the archaeological record was a reason for some excavators not to collect baked clay. As a result, they unintentionally created a gap in the data. Third, as I noted above, basket impressed baked clay objects cannot provide definitive insights into the total range of different types of actual baskets made at any given site in the past (e.g., lattice, diagonal). Rather, the basket impressions I observed in baked clay provide information about the baskets that made the impressions I observed. Fourth, the distribution of sites sampled for this study favor the Delta region beginning just north of Stockton to just south of Sacramento. Only a few of the sampled sites are near the Plains Miwok/Northern Yokuts boundary.

Despite these concerns, basket-impressed baked clay objects still yield a tremendous amount of information about baskets that no longer exist in material form. Because basket-impressed baked clay objects are excavated primarily from domestic contexts, they offer an excellent alternative to analyzing associated mortuary offerings that have been a cornerstone of California archaeology and the study of ethnicity and identity. Baked clay objects avoid many of the pitfalls and ethical problems associated with analysis of burial objects and requires that more creative questions and analytical thinking be employed to explore the pre-European past of California.

Taking seriously oral traditions and the work of Larry Dawson and Ralph Shanks (2006, 2010) which speak to the longevity and continuity of basket making traditions, several characteristics of basket making are essential to establishing the identity of the makers. Using ethnographic baskets Dawson and Shanks posit that Miwok women created twined baskets with both UR and DR direction of weft twist whereas Yokuts women made twined baskets with weft twists that were exclusively UR. They found similar distinctions with their study of coiled baskets. They also present data suggesting Plains Miwok women tended to work to the left (stitches observed in baked clay will lean to the left), whereas Yokuts women worked to the right (stitches observed in baked clay will lean to the right). Additionally, Dawson found Plains Miwok women tended to split their stitches and preferred to interlock them on coiled baskets while Yokuts women did not split their stitches and preferred not to interlock them (Table 5.2).

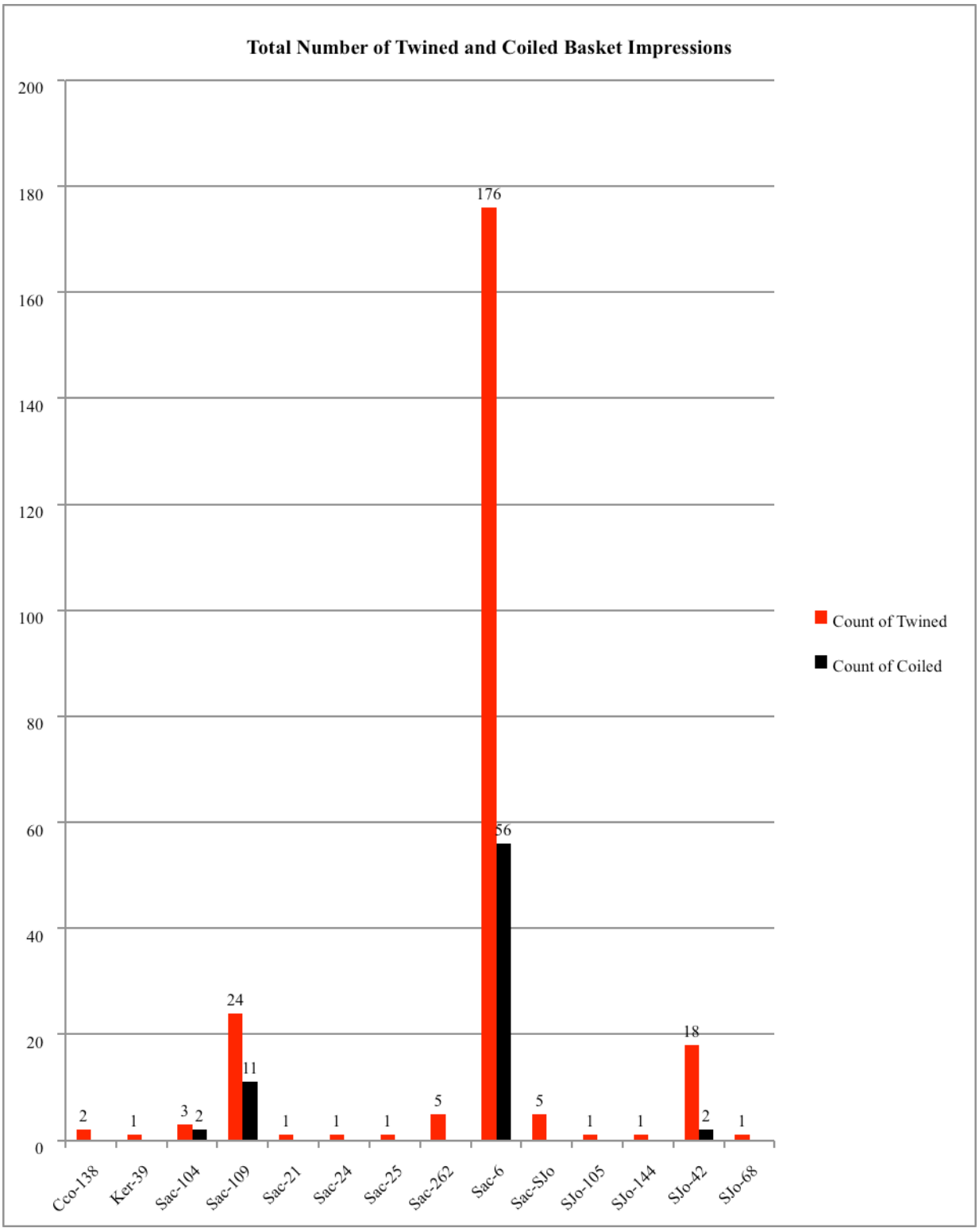


Figure 5.23 Distribution of of twined and coiled baskets found at the sites selected for analysis

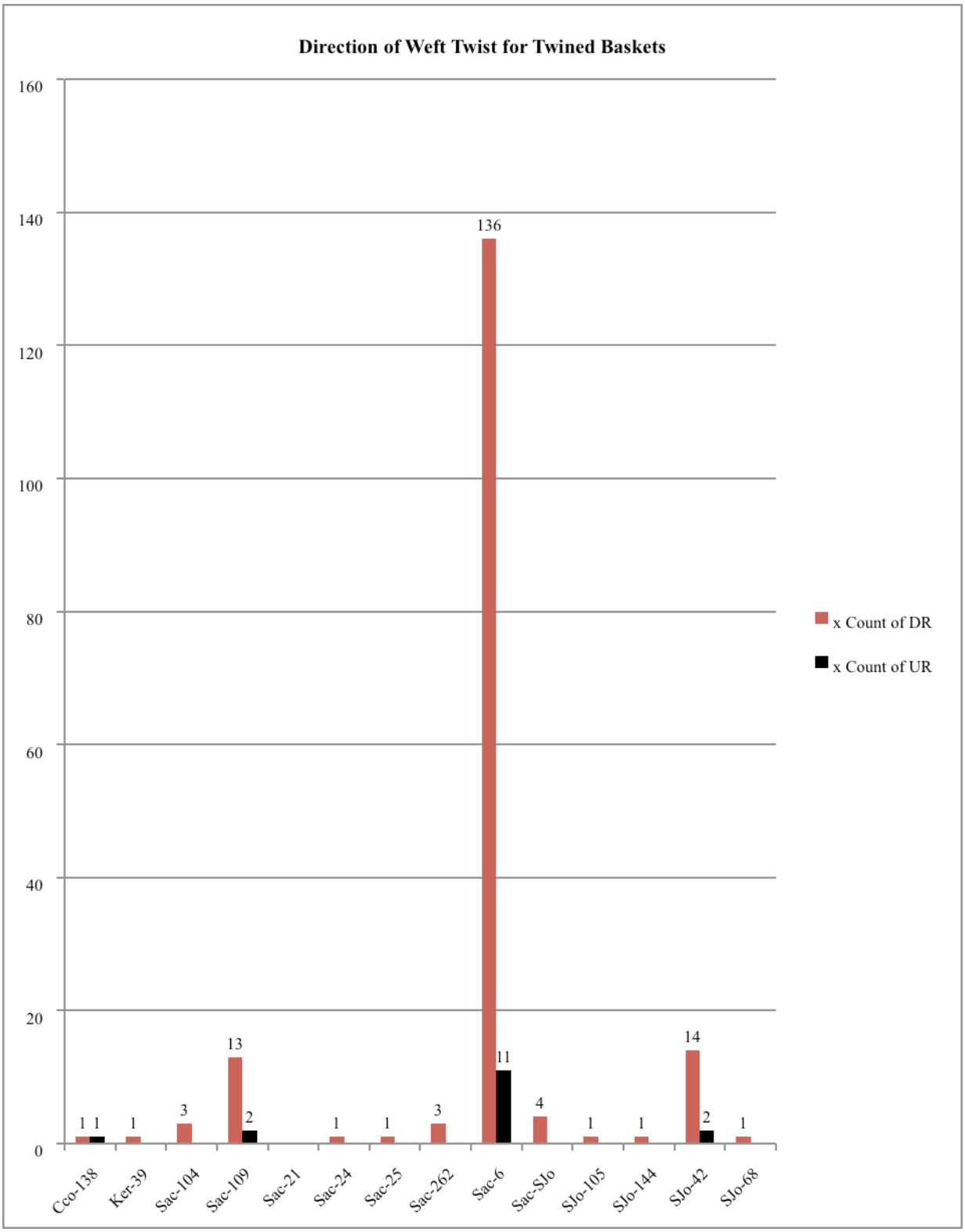


Figure 5.24 Distribution of basket-impressed baked clay samples with direction of the stitches.

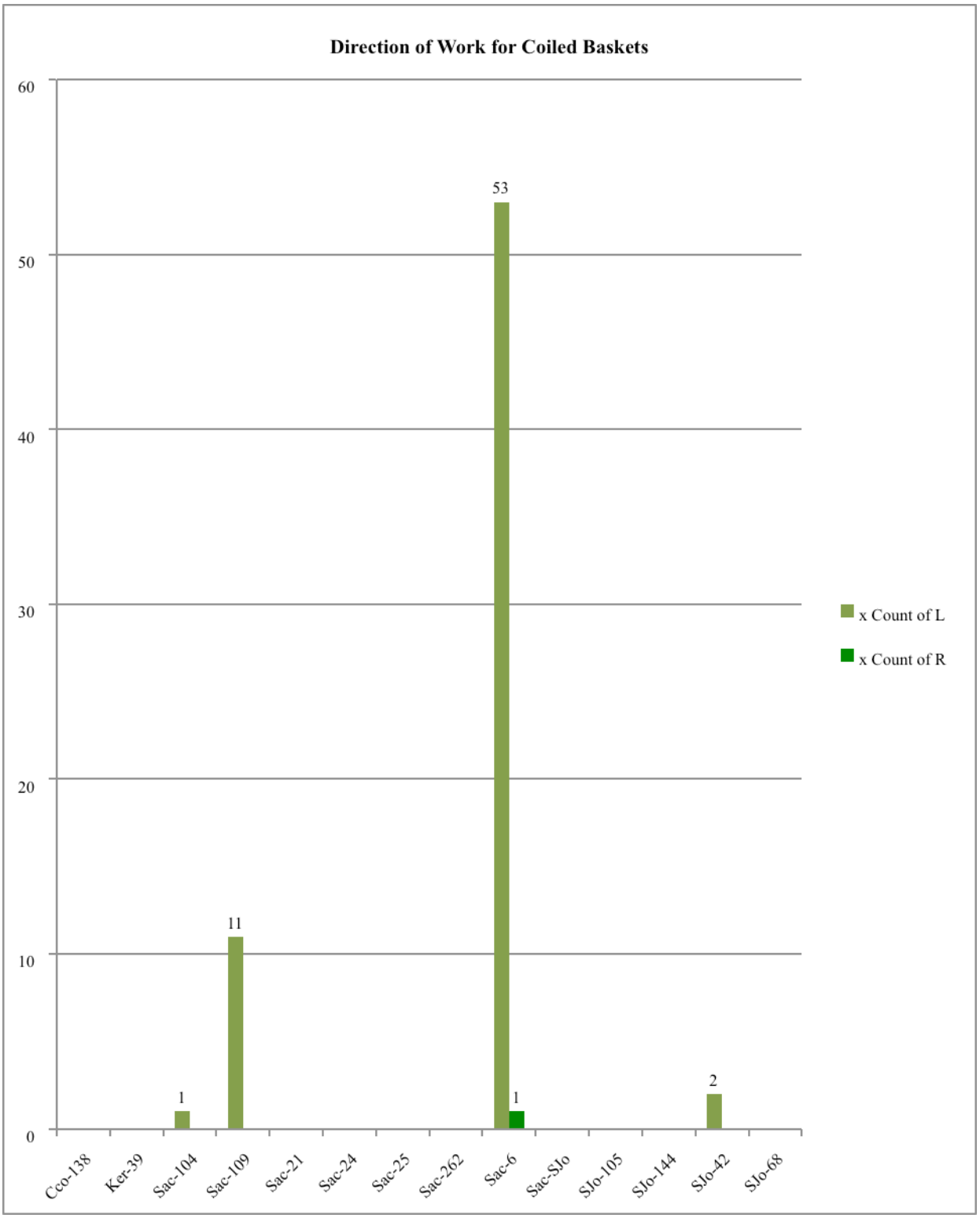


Figure 5.25 Showing the number of baked clay samples that had impressions made by coiled baskets and the work direction as evidenced by the direction of the stitching.

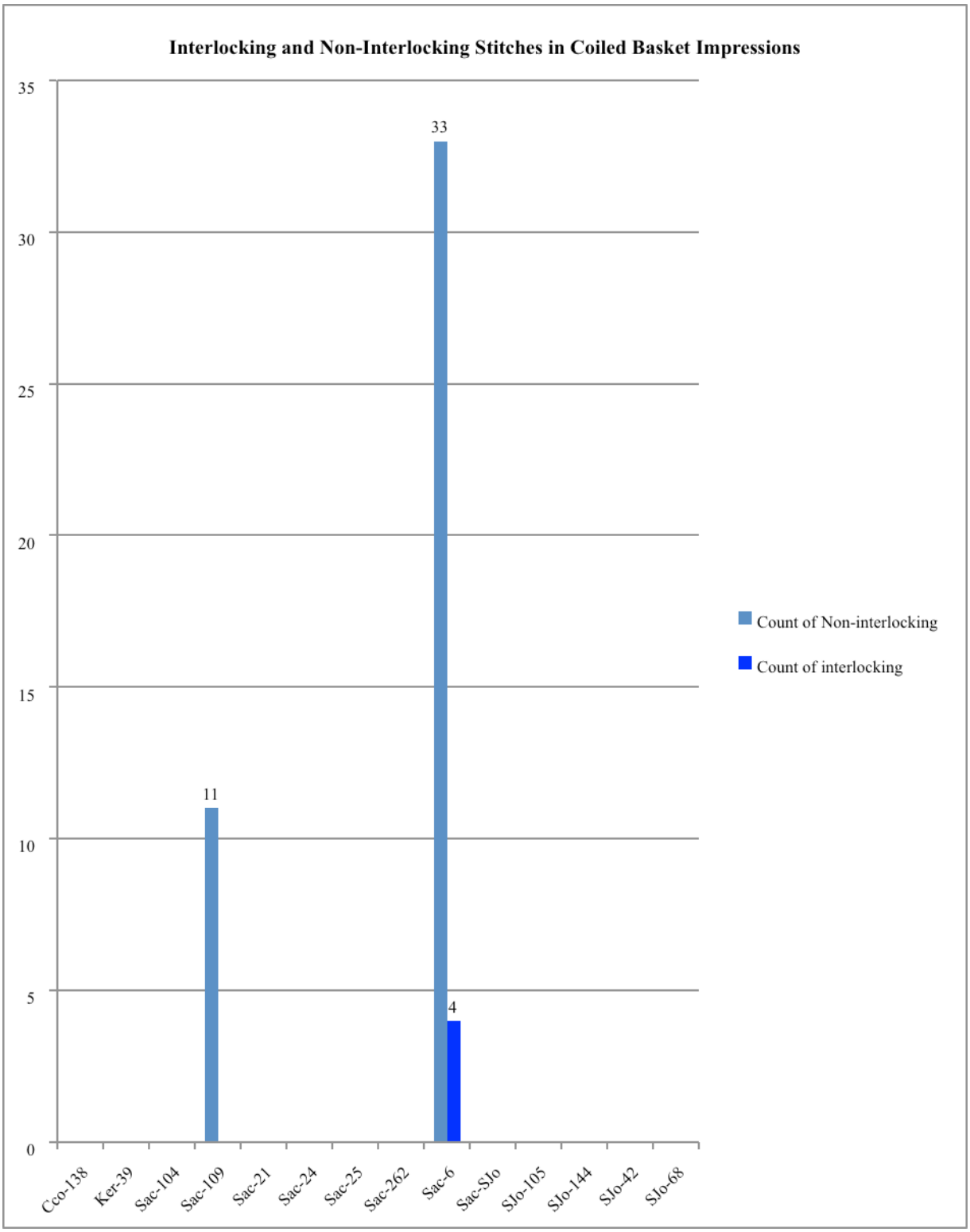


Figure 5.26 Showing the number of baked clay samples with impressions of interlocking and non-interlocking stitches.

Table 5.1 Summary of Analysis of Basket-Impressed Baked Clay

	CCO-138	*KER-39*	*SAC-104*	*SAC-109*	*SAC-21*	*SAC-24*	*SAC-25*	*SAC-262*	*SAC-6*	*SAC-SJO*	*SJO-105*	*SJO-144*	*SJO-42*	*SJO-68*	*TOTAL*
Coiled			2	11					56				2		71
Twined	2	1	3	24	1	1	1	5	176	5	1	1	18	1	240
UR (/)	1			2					11				2		16
DR (\)	1	1	3	13		1	1	3	136	4	1	1	14	1	180
LFT (\)			1	11					53						65
RGT (/)									1				2		3
Interlocking									4						4
Non-Interlocking				11					33						44
Total	2	1	5	35	1	1	1	5	232	5	1	1	3	1	311

Table 5.2 Summary Table of Weaving Techniques Organized by Tribal Group

Tribe	*Twined*	*Coiled*	*Non-Interlocking Stitches*	*Split-Stitches*
Plains Miwok*	(/\)	(\)		X
Yokuts	(\)	(/)	X	

Table adapted from Elsasser 1978:633).
* Many of Dawson's Plains Miwok examples came from basket fragments collected found in archaeological contexts. Shanks (2006, 2010) on the other hand used ethnographic baskets in his Plains Miwok analysis. Neither Plains Miwok or Yokuts basket makers are known to have used interlocking stitches, however, Elsasser (1978:633) reports the nearest group that *did* use interlocking stitches was the Sierra Miwok.

CA-SJo-42 and CA-CCo-138, two sites located along the current cultural border separating Miwok and Youkuts people, have examples of up and to the right weft twist. CA-SJo-42 and CA-CCo-138 are both currently located within Miwok territory and approximately equidistant from the Yokuts border. The comparison of direction of weft twist (up and to the right versus down and to the right) is important for two reasons. First, it could be seen as a clear example of Miwok basket makers showing a preference for one technique, but using both. Or, taking into account the trend from sites further inland that show only down and to the right weft twist, it is plausible that the few examples of up and to the right weft twist along the border indicate a different basket making tradition. Thus their presence might be the result of one of two or both of the following events: intermarriage and kinship relationships, trade, or gifting. Sac-6 is unique within the data set because it is an extraordinarily large sample size. The few examples of impressions made from baskets with up and to the right weft twist, the opposite direction of twined baskets, the dominate type found at SAC-6, could be the result of ceremony, trade, or intermarriage. It is telling however, that no other sites as far or further north than CA-SAC-6 and of comparable size to CA-SJo-42 or CA-CCo-138, yield examples of basket-impressed baked clay objects with up and to the right weft twist.

A total of 71 (23%) pieces of baked clay had coiled baskets impressions. All but one example had a leftward direction of work (Figure 5.25). These came from CA-SAC-6 (n=1) and CA-SJo-42 (n=2). Left direction of work indicates Miwok style coiling technique. Whenever possible, I noted the type of stitch used to construct a coiled basket. In doing so, I found that all but four of the 48 pieces analyzed had non-interlocking stitches, exclusively a Yokuts style of stitching. The remaining four pieces showed evidence of interlocking stitching, a style of stitching that according to Dawson was not used by either Plains Miwok or the Yokuts people. The nearest group to the Delta known to use that technique was the Sierra Miwok (Elsasser 1978:633). The variation is interesting. Work direction points to Miwok weavers but stitching type indicates Yokuts technique. Since there are only a few ethnographic baskets collected from Plains Miwok basket makers (a consequence of missionization, epidemics, and genocide), this archaeological evidence is our primary glimpse into coiled basket making tradition in the Delta (Shanks 2006:174, *pers com*m. 2010).

CONCLUSIONS

The data gathered from basket impressed baked clay objects led to three findings. First, the large proportion of impressions of twined versus coiled baskets suggests that twined baskets were used most often to make impressions in clay. As Pryor (1987) and others pointed out, twined baskets were "workhorse," everyday, utility baskets whereas baskets made by coiling tended to be used for important, special, or ceremonial purposes. It makes sense that there are more twined basket impressions in baked clay than impressions of coiled baskets, since those baskets were most often used for cooking. Second, the direction of weft twist for impressions of twined baskets does not vary enough to draw solid conclusions concerning the ethnic identity of the makers. Of the 196 examples of impressions made by twined baskets, 180 are worked down-and-to-the-right (DR), the remaining 16 are examples are worked up-and-to-the-right (UR) (Table 5.1). The work of Dawson (1978) and Shanks (2006, 2010) suggests Miwok weavers wove in both directions and Yokuts weavers preferred to twin up-and-to-the-right (Table 5.2). The dominance of (DR) weft twists, a direction not found on ethnographic Yokuts baskets, might suggest that the twined baskets used to impress the baked clay I sampled were made by Miwok weavers. The majority of basket-impressed clay objects with impressions showing a (UR) direction of weft twist suggests

Miwok manufacture. Still, given the overall small sample size (n=16 or 8%), I am cautious to draw firm conclusions. Given the location of the sites sampled along what is known as a cultural boundary it is not improbable to think there was a close relationship between Yokuts and Miwok weavers. At this point, however, I am unwilling to draw a firm conclusion based solely on twined baskets. Third, impressions of coiled baskets provide clear distinctions within certain categories (work direction, stitch type), but not between them. On the one hand, the dominant direction of work for impressions made by coiled baskets was to the left (98.5%) (Figure 5.25, Table 5.1). This suggests Miwok manufacture. On the other hand, impressions made from coiled baskets are dominated by non-interlocking stitches (92%), a characteristic of Yokuts weavers.

In the introduction of this chapter I noted that I hoped to differentiate three levels of affiliation. From broadest to most narrow:

1. I expected that basket construction techniques visible in the baked clay would provide information about group affiliation.
2. I expected to find social practices highlighted by the impressions.
3. I expected to find specific *communities of practice* signaling unique ethnic identities (Lave and Wenger 1991).

I was only partially successful in finding evidence to support these three expectations. First, absolute distinctions between Yokuts and Miwok weavers could not be made; second, there was some evidence indicating particular social practices; and third, with further research into basked impressed baked clay objects different weaving communities may become apparent.

Regarding my second expectation, I am only able to highlight what I think is an important point and make suggestions for areas of future research and interpretation. The point is this: that I privileged certain objects and types of information in my analysis. For example, although the chapter is about impressions found in baked clay, the majority of my discussion focuses on virtual baskets, baskets that no longer exist. As a result, I have drawn heavily on the analytical techniques and insights gained from ethnographic baskets.

What I have not done is discuss basket impressed baked clay cooking balls in a social context or paid particular attention to their shape and what it might say about how they were used. There are more impressions of twined than coiled baskets and typically twined baskets are interpreted as baskets of lesser value or utilitarian. The simplest explanation for these observations is that since cooking was a daily activity and baked clay objects were needed for that task, the most readily available baskets were used. However, this explanation does not address why there are any impressions from coiled baskets. It also doesn't answer whey the majority of the baked clay objects that have impressions of coiled basketry are carefully formed into either bricks or cylinders, or why they are impressed on all surfaces and show no signs of maker's finger prints. This is in contrast to the baked clay objects impressed with twining that are often irregular ovals and impressed only on one side.

One possible answer is feasting in either communal or ceremonial contexts. Food consumed during these events would have been considered special or sacred, as would the manner and the items used to cook the food. It is possible therefore that the clay balls used for cooking sacred food also needed to be impressed with baskets that were going to play a role in whatever important social event was taking place. Perhaps the clay was impressed with coiled baskets that were being gifted or interred with a loved one. There is no direct evidence in the ethnographic literature for my interpretation, but reference to feasts during ceremonial and

community events suggest a possible avenue for further research into the forms and functions of basket impressed baked clay.

Regarding my third expectation, the evidence I gathered provides no insight into specific communities of practice as I hoped it might. While Roddick (2009) was fairly successful in his search for these communities in the Tiwanaku Basin of Boliva using ceramics, the analysis I conducted does not lend itself to these types of inferences. This is another area for future research.

At the end of Chapter one I presented five boundary models:

1. *Fixed*: Expect to see circumscribed patterns of distribution.
2. *Clinal*: Expect to see a gradient of in the distribution of objects.
3. *Temporally Fixed*: Expect to see circumscribed boundaries that shift according to chronological divisions.
4. *Temporally Clinal*: Similar to above, but changes should be gradual. This might be impossible to see.
5. *Punctuated:* Expect to see some types of objects circumscribed and others less constrained.

Given the data gathered from basked-impressed baked clay none of these models exactly fits. Evidence from impressions made from coiled baskets shows the overwhelming majority of stitches leaned to the left, a characteristic of Plains Miwok makers. Yet, large numbers of examples of interlocking stitches from the same coiled baskets points to Yokuts weavers. Evidence from the twined baskets is equally mixed. While the majority of twined baskets were woven with stitches leaning down and to the right, (potentially a Plains Miwok weaving trait) a small number of impressions of twined baskets showed stitches leaning up and to the right (a Yokuts weaving trait). Assuming Yokuts and Plains Miwok weavers made both coiled and twined baskets, no clear boundary exists.

While an absolute division between groups based on weaving technique is not discernible, the observable pattern points to an interesting and likely possibility. That possibility is that weavers in the Delta were sharing techniques. To explain the presence of several artifacts found in the Cosumnes District but typical of the Stockton District, Bennyhoff (1994:73) cited intermarriage. The idea that individuals from different groups intermarry is not novel, but what is novel is how it might be observed in the archaeological record. The particular pattern of techniques created by my observations may be the result of intermarriage and the sharing and teaching of techniques amongst newly gained family members. Of course there is yet no direct evidence for this claim, however, it does offer new ways of accessing the social world of the Delta.

Chapter 6
Engraved Bird Bone Earrings

INTRODUCTION

Engraved bird bone tubes are elaborately designed objects with somewhat enigmatic meaning. They were made and worn by Native Californians until at least the middle part of the 20th century. Typically created in matching pairs from the wing or leg bones of cranes, condors, eagles, or other large birds, they were engraved with intricate and complex repeating geometric designs. Fleeting ethnographic, historic, and pictographic references exist in 19th century travel narratives, paintings, photographs and interviews conducted by early 20th century University of California, Berkeley ethnographers also exist. This database strongly suggests that the engraved tubes were worn exclusively by wealthy women and men. It also indicates the tubes were worn as earrings or as ear plugs pushed through a distended hole in the ear lobe. Archaeological interpretation of engraved tubes, on the other hand, typically focuses on recording their presence at a given site and trying to fit them into local and regional typological frameworks and chronological sequences. In rare cases, archaeological analysis attends to the geometric patterns and explores them for their ability to act as stylistic markers of archaeological districts and the culture areas those districts are thought to represent.

While this type of analysis fits squarely within a traditional archaeological mode of stylistic analysis, the certainty with which the geometric patterns are associated with specific archaeological districts is, at best, dubious. There is only one source I know of from which this typological analysis and interpretations are derived: incomplete district typology charts created by James Bennyhoff taken from a hard to find 1962 publication. Recently these charts were reproduced by Richard Hughes (Hughes 1994:72). Without further direct analysis of the objects Bennyhoff used to create his typology and subsequent laboratory notes, the conclusions and explanations regarding these objects are best described as tentative.

To be sure, engraved bird bone ear tubes are breathtaking from both an aesthetic and a technical stand point. In the context of California archaeology, the geometric designs found engraved into tubes have and continue to be useful as stylistic markers for different archaeological districts. Local and regional typologies, which include bird bone tubes, knit together style and ethnic identity by geographically circumscribing the distribution of different design patterns and correlating this distribution with ethnolinguistic and cultural boundaries (e.g., Jones 1997; Shennan 1989; Stark 1998; and Trigger 1989 for excellent summaries of how this use of "style" operates in archaeological analysis). As I noted in Chapter 2, this process is a cornerstone of archaeological analysis and interpretation as well as a source of continued debate. As such, the contours of our understanding of the relationship between style and identity are continually changing. For the most part, this debate has and continues to be productive, continually leading the discipline into new and exciting intellectual territories.

In this chapter I will highlight segments of this body of literature to frame my discussion and interpretation of engraved bird bone ear tubes but it is beyond the scope and purpose of this chapter to present a full review. Where in previous chapters I only touched upon the subject, a central goal of this chapter is to take a closer look at some of the consequences of directly and uncritically associating stylistic variations within a class of artifacts to cultural boundaries. Given recent discussions regarding the relationship between style and identity it is not surprising that the assertion of simple isomorphism between style and cultural boundaries is troubling. In this chapter I argue that the assumption that different geometric patterns *a priori* signal the group

affiliation and are reflections of notions of "us" and "them" should be questioned (Chilton 1999; Wobst 1999). I do not claim to be able to solve the philosophical problems associated the relationship between style and identity in this chapter. However, for California archaeology I do propose a significant first step towards engaging this issue, not by assuming the different geometric patterns engraved in ear tubes reflect cultural differences or indicate boundaries, but to start with a more fundamental question: *do engraved bird bone ear tubes mark cultural differences and borders and, if they do not what do they represent?*[1]

To try to answer this question I analyzed bird bone tubes curated at the Phoebe A. Hearst Museum of Anthropology. As in previous chapters, I focused my attention on Late Prehistoric to Historic Period sites from the Delta region of the Central Valley. Additionally, because of the potential for engraved bird bone tubes to elucidate the social world at multiple levels, from personal style to kin relationships as well as social and cultural group membership, I did not limit my sample to just a few sites. Rather, I attempted to find all known samples within the museum collections. Casting a wider net is appropriate given that I ask if and how these items might signal group affiliation. While bone engraving is found throughout California, this particular form seems to be concentrated in the Delta with some extension into the Bay Area. Widening my geographic scope to these areas, I was able to analyze 98 whole or partial engraved bird bone tubes from 13 sites (Figure 6.1). Twenty-six of the tubes had no provenience information other than having been collected from the "San-Sac" area. I included these unknowns in my sample for two reasons. First, if the engraved geometric designs indicated group affiliation then either I, or basic statistical analysis, would be able to sort them into groups, thus, not having specific provenience information would not matter. Second, using the "unknowns" reminded me to allow patterns to be inferred from the sample rather than fall into the trap of using preexisting categories (cultural, areal, etc.) to organize the sample or my analysis. I will present more specific methodological concerns when I discuss the creation and recreation of specific typologies.

In this chapter, I first contextualize engraved bird bone ear tubes in their social milieu by presenting a brief review of ethnographic and historic observations concerning their use. Next, I present my reworking of Bennyhoff's bird bone tube typology from his original and previously unpublished notes. As I mentioned above, these notes are the basis for the few published illustrations and subsequent interpretations of engraved bird bone ear tubes in California archaeology. I conclude this chapter with a discussion of bird bone tubes in the context of broader theoretical discussions of style and identity in archaeology, show that despite using two typologies grounded in very different theoretical orientations, similar results were achieved, and relate these findings to the five border models discussed in previous chapters. My findings will show that further and more nuanced studies are needed. At this point engraved bird bone tubes do give some insight into the social lives of California Indian people, but they do not seem to correlate with cultural boundaries as they are currently drawn (e.g., Bennyhoff 1994:71-72; Heizer 1978:44-45; Wake 1997:261).

[1] Related questions are: why, and in what ways, would such variations in design and decoration have been meaningful to the wearers and viewers? Although I do not propose to answer this question here, it nevertheless should be asked when assessing the archaeological mantra and often taken-for-granted association between stylistic forms and cultural boundaries.

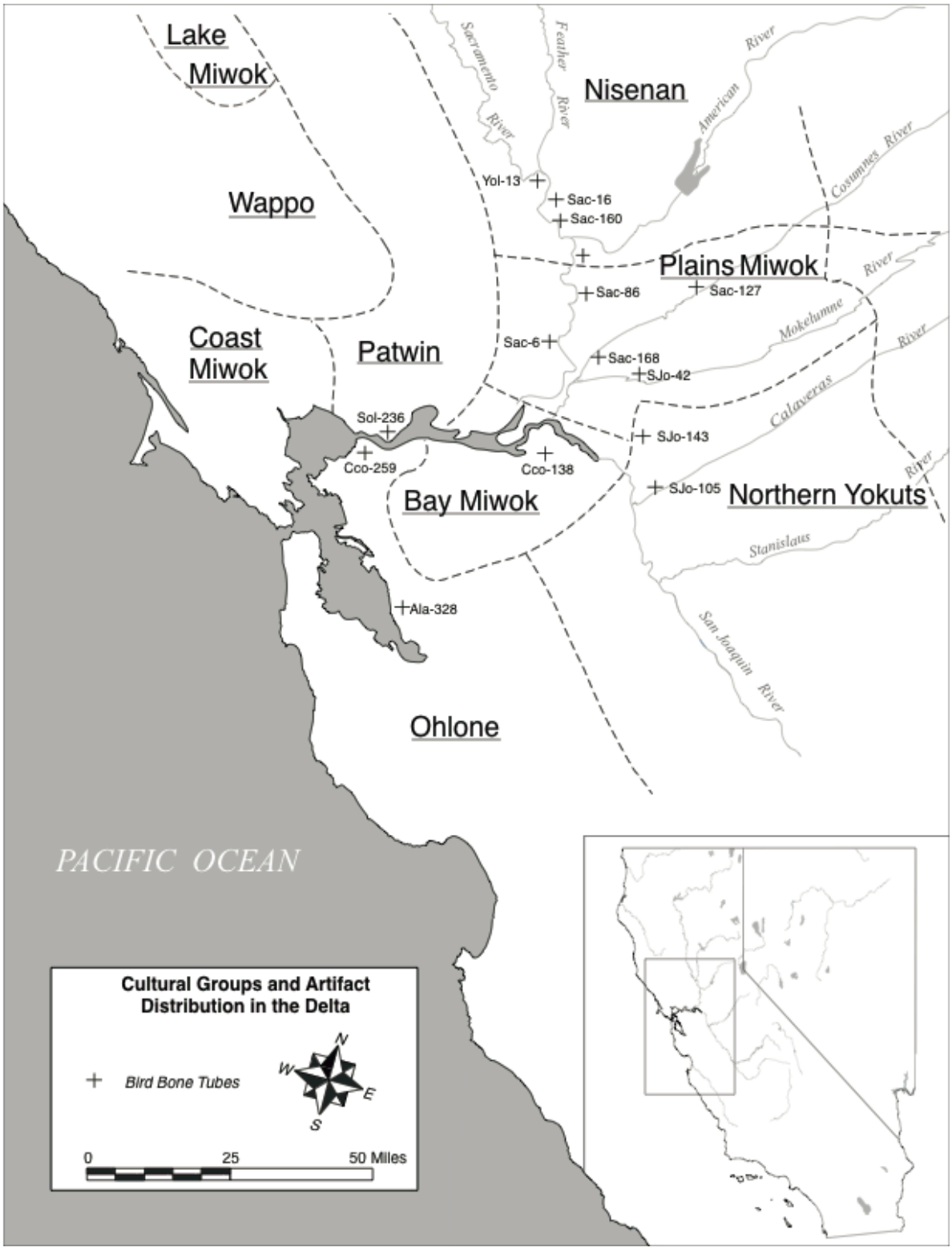


Figure 6.1 Study area and sites from which engraved bird bone ear tubes were selected for analysis.

ETHNOGRAPHIC AND HISTORIC DOCUMENTATION

Ethnographic and historic documentation spanning the 19th and 20th centuries, while not extensive, does provide adequate descriptions (both written and visual) of how engraved bird bone tubes were worn and who wore them so as to warrant plausible interpretations of their use during the Late Period in the Delta Region of California. One of, if not the earliest, written references to engraved bird bone tubes is from Ida Pfeiffer's 1856 travel narrative entitled, *A Lady's Second Journey Round the World.* Pfeiffer described the boom and bustle of mid 19th-century California. This included being charged by a bear that was part of a traveling caravan passing through San Francisco, the "atrocity" of Sacramento gentlemen picking their noses as opposed to using their handkerchiefs (Pfeiffer 1856:302), and a meeting with John Sutter at the gold mines on the Yuba River. Of particular interest to this dissertation is the description of her visit to an unnamed, but most likely Nisenan village near Marysville, California. She notes, "Both sexes have their ears pierced with large holes, through which they pass a piece of wood as thick as a man's finger, decorated with paintings or glass beads" (Pfeiffer 1856:307). This first potential description of engraved bird bone tubes is echoed by H.H. Bancroft (1874). He reflected on his observations of Native people living in the Sacramento Valley, "ear ornaments are very much in vogue; a favorite variety being a long round piece of carved bone or weed, sometimes with beads attached..." (1874:368). While these descriptions are not at all conclusive, they are evocative and potentially point to the likelihood that bird bone tubes as earrings were an established practice and somewhat widespread use of engraved bird bone tubes as earrings.

In 1926 Edwin Loeb, a UC Berkeley ethnographer, described engraved bird bone tubes and highlighted the morphological differences between those worn by men and woman (see also Barrett 1952:300; Gifford and Kroeber 1937:169; Kroeber 1925:240). It is worth quoting Loeb at length. He wrote of Pomo ear-tubes (Figure 6.2):

> The earrings of the men were simpler than those of the women. They were called hai pak'ili, E (wood blackened-by-charring). The ornament was usually made of bottom willow about six or seven inches in length and the size of a pencil. The stick was hollow and a cord ran through the length with a large bead fastened on each end. Around the bead quail plumes were held in place by pitch. From the center of each bead was hung a piece of abalone shell. The wooden portion of the ear plug was colored with red willow leaves which had previously been mashed and mixed with a little charcoal. The coloring was rubbed in with the hands until it became sticky; this gave a glossy finish and helped the color to adhere.
>
> The earrings of the women (hia gapi, E, crane bone stretched) were about eight inches in length by a fourth of an inch in diameter, and were made from the wing bone of the crane. The piece that went through the ears was carefully etched with crossed straight lines. A string ran through the center of the bone and held in place two circular ornaments two or three inches in diameter. The latter were made of coiled basketry into which red feathers had been woven; a couple of pieces of shell hung from the center of the basket, and usually two from the bottom. The bone was packed tightly with tule and pitch applied to both ends; this kept the circular pieces from moving about. On the coast, the ends of the crane bone were decorated with thick beads and green feathers from the neck of the mallard duck. *Only wealthy women used the above elaborate ear ornaments; others were*

> *content with plain unfeathered bones, or quail plumes. Ear decorations were usually made by the men* (Loeb 1926:156, emphasis added).

Barrett and Gifford (1933) also give an excellent description of engraved bird bone tubes taken from Miwok informants. As with Loeb (1926), Barrett and Gifford (1933) note that sex determined the type of ear tube worn, wood or bone and that rank or social status was a further determinative variable.

> An earring, consisting of a small string of beads and shells, was called tc#'kkel*u* (C). An ear plug (lü'sa, P, N; s#'lai*u*, somayu C) was made by charring the surface of a piece of young pine from four to seven inches in length. When this blackened surface was rubbed down properly it became very shiny. The ends were then ornamented with beads and shells, and sometimes with the scarlet feathers of the California woodpecker. Both sexes wore earrings and earplugs; but the former were used chiefly by the women, the latter by the men. *Except among the wealthy and important people these adornments were used only on ceremonial occasions.* The same applies to the nose sticks (p#l#'ki, P, N; t*u*'la, C), some of which were made of white, polished bone, about the diameter of a lead pencil, and from four to nine inches in length; not etched or otherwise ornamented, but highly polished and slightly pointed at the ends. Shell nose sticks (pileki, N; pileku, C) were also worn (Barrett and Gifford 1933:225, emphasis added).

Barrett and Gifford's description reiterates Loeb's findings among the Pomo and further highlights that it was not just sex and rank that influenced who wore which type of ear tube, it was also context. As these authors describe, objects that were usually worn exclusively by wealthy women could also be worn by the non-wealthy during ceremonial occasions. This is an important point, in that it calls attention to the complex social meanings associated with objects made by men and worn by wealthy women in the early 20th century. The life histories of these objects crossed and intersected gender, status, ceremony, as well as public and private boundaries signifying and being signified by the wearer (Joyce 2002). They were simultaneously used to announce an individual's position in a social hierarchy to knowledgeable observers, as well as reinscribe the identity of the wearer.

This study is limited in three fundamental ways. First, by geography. Because of the historical circumstances surrounding decisions made by anthropologists regarding which tribal group was "worth" recording, the majority of ethnographic information regarding ear tubes comes from Miwok and Pomo informants. This is not to say that ear tubes found throughout the Delta region did not have the same or similar rules attached to their use. Rather, it is to underline the possibility that subtle differences between the Pomo and Miwok descriptions (only wealthy women worn them among the Pomo versus the potential of the non-wealthy to wear them at certain times among the Miwok) could be the most important distinctions to be made between cultural groups and yet cannot be extended to the Delta at this time. Second, my focus has, and will be in the next section, on ear tubes as complete objects. In other words, I have yet to discuss the engravings on the tubes. This last point emphasizes a fundamental intricacy of material culture and a core debate regarding style and identity; that there is an elaborate relationship existing between an object itself (the bone or wood), the design added on to and into it (engravings, feathers, baskets-starts, beads), and the different social roles being enacted by one or a combination of all these elements. Last, my descriptions thus far lack visual support. It is

one thing to hypothesize about how ear tubes were worn based on ethnographic images or early ethnographic and travel narratives, it is another to *see* how they were worn.

The two earliest pieces of possible evidence of ear tubes are in images painted by artists accompanying Russian explorations of the California coast during the early 19th century. The first watercolor was painted by Louis Choris, arguably the most talented artist to accompany any

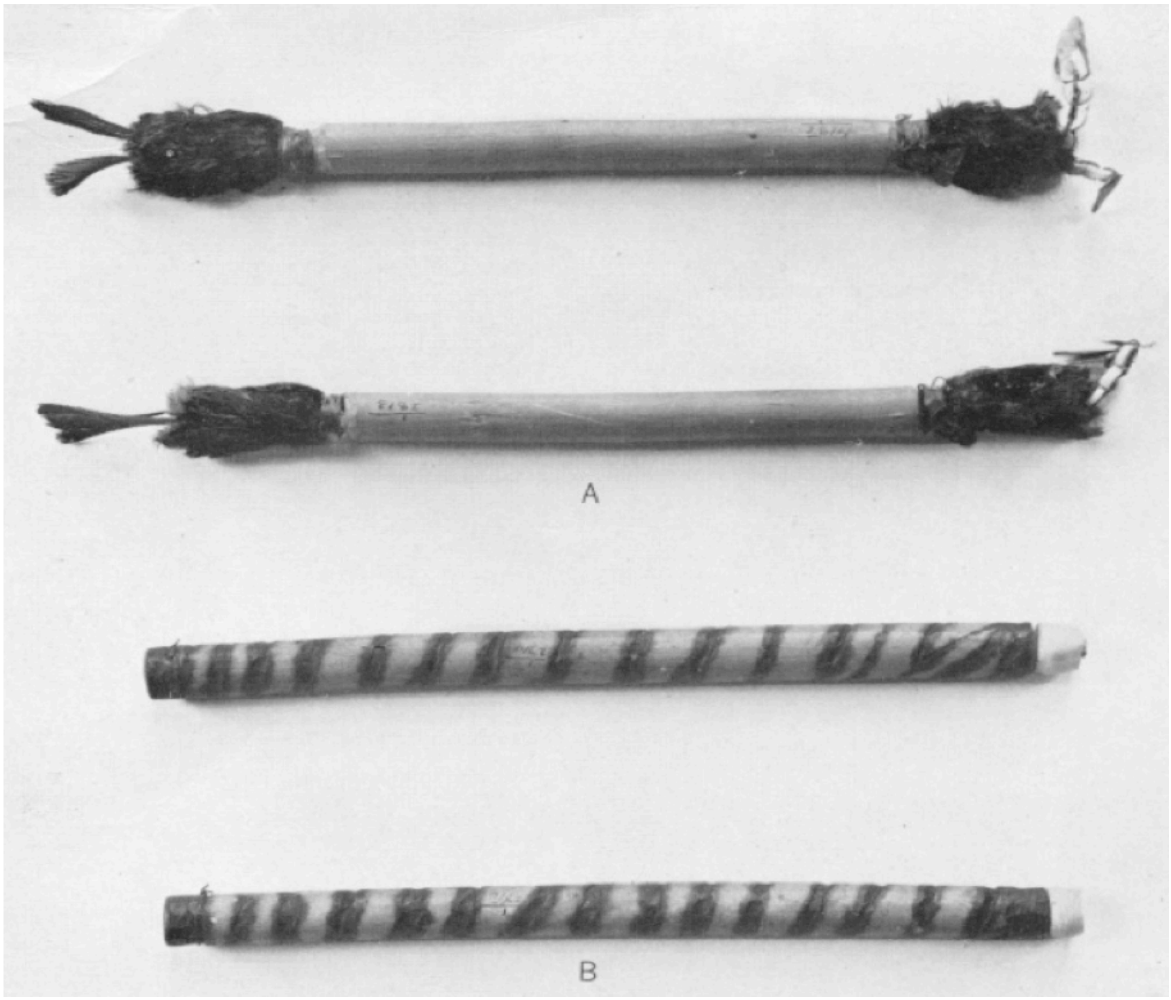


Figure 6.2 Bird bone ear tubes. The upper most set of ear tubes are adorned with mallard and flicker feathers as well as shell beads. From Loeb (1926: plate 3).

expedition into California (British, French, or Russian) in the late 18th and early 19th centuries. Choris, a Russian born artist was famed for 'painting nature as he found it' and his paintings are perhaps the most reproduced and recognizable images of Native Californian's living at Spanish missions (Blackburn 1999). During a brief visit to the San Francisco Bay in October of 1816, while accompanying Otto von Kotzebue, a lieutenant in the Russian Navy, on the *Rurik* as a member of an expedition to find the fabled northwest passage, Choris composed an image entitled "*Cholovonis à la chasse dans la baie de St. Francisco*" (translated, "Cholovonis on the hunt in the San Francisco Bay"). The lithograph (Figure 6.3) depicts two men in hunting poses along the shores of the San Francisco Bay. The men are unclothed, one holds a drawn bow, the other a quiver and there are two more quivers laid at their feet. Both men also wear

Figure 6.3 *Cholovonis à la chasse dans la baie de St. Francisco,* Louis Choris,1816. Courtesy of The Bancroft Library, University of California, Berkeley, BANC PIC 1963.002:0368--B.

ornamentation in their ears. While the detail in the lithograph is not perfect, my observations of ear spools made from clay during analysis of basket-impressed baked clay, supports the idea that the ones depicted here are not clay and thus, most credibly, made from bird bones. The two men depicted in the painting are often mistakenly identified as Ohlone. However, research by Randall Milliken (2006) points to a very different identity of these two men. His exploration of both linguistic evidence and Spanish Mission records locates the Cholovone people as having lived near present day Stockton, California. In other words, from Northern Yokuts territory.

There are two linguistic sources for this claim cited by Milliken (2006). The first linguistic evidence comes from an 1880 vocabulary collected from Yokuts speakers living in Pleasanton, California by A. Louis Pinart (Pinart 1955). The people Pinart interviewed specifically referred to the area around Stockton, California and to the immediate west as Yokuts territory. Second, a few decades later Alfred Kroeber had similar findings when he interviewed different informants, also living in Pleasanton, who claimed Yokuts ancestry (Pleasanton is today

approximately 72.5 kilometers from present day Stockton) (Kroeber 1906, 1908). More recently, Bennyhoff (1977), Smith (2007), and Milliken (1995, 2008:C3), closely analyzed the vocabularies provided by Pinart and Kroeber. In combination with Mission registers and other historical documentation including diaries and travel narratives both concluded that villages immediately surrounding the Stockton area, and about as far east as Livermore, were within the territory of the Cholovone or Chulumni tribelet, a Northern Yokuts group. Given this data, there is a strong inference that the men depicted were Yokuts. This finding makes more sense when considering the statements made to me by Tachi Yokuts cultural leaders and elders. During one conversation they noted, "we traveled North to there [the Bay Area and Delta] often, we had relatives there, we also lived there" (Franco *pers. comm.* 2009).

The second painting is one of five watercolors composed by Alexander Tikhanov in 1818, which depicts Miwok people living at what is now known as Bodega Bay. Tikhanov, a Russian serf given his freedom in recognition of his ability as a painter, accompanied the Golovnin Expedition on the Russian ship *Kamchatka* to record in drawings the native peoples of the places the expedition visited. In particular he was charged with creating profile and full-face drawings and was to pay particular attention to material culture (Farris 1998:2). Of the five watercolors he painted of the inhabitants of Bodega Bay (then called Rumiantsev Bay), one depicting a woman is of particular interest. In this painting, entitled, *Inhabitant of Rumiantsev Bay* the woman carries a basket on her hip and has a necklace of clam shell beads around her neck. Closer inspection reveals she is wearing an engraved bird bone tube in her right ear.[2]

While these visual examples are taken from areas outside the Delta region, they add texture and vitality to the written descriptions. Considering my recounting of historic and ethnographic references to engraved bird bone tubes, the most likely explanation of their function is that of status marker. Commentators point out that only wealthy women or men wore these ear tubes and that in some cases observers noted that men and women wore different types of tubes (wood and bone respectively), but these later references are fewer and may be based on practices unique to a particular group.

Roughly one hundred years later Edward Curtis published a 20 volume set visually documenting eighty tribes west of the Mississippi. Curtis' work is often critiqued as a kind of ethnographic salvage that presents a romanticized image of pre-reservation life, forever changing the public's perception and understanding of Native American people by thrusting them deeper into the pages of history books and denying them a place in contemporary life or access to the ideas of cultural process and historical change. This is a valid critique. It is also true that this narrative rips any individual agency from the hands of Native people and gives it all to Curtis. As Zamir (2007) accurately points out, this story of Curtis and his relationship with Indian people ignores the particular contexts and contours surrounding individual photographs.

Despite the many issues and debates surrounding Curtis and his entho-photographic work, I present two of his pictures, both taken of Pomo women, which show them wearing engraved bird bone tubes quite clearly. The pair of engraved tubes in both images were made by William Benson, a famed Pomo artist from Shaxai on the Western shore of Clear Lake. Curtis conducted his field work in California during 1915, 1916, 1922, and 1924 and focused attention on representing four language groups: Athapascan, Yukian, Hokan, and Penutian (Curtis 1907:xi - xii). In his chapters on the Miwok and Pomo, Curtis summarized the available ethnographic

[2] The image is not reproduced here due to lack of copyright permission. However, a thumbnail can be viewed on the California of Academy's website for "Science Under Sail: Russia's Great Voyages to America 1728-1867" (http://www.calacademy.org/exhibits/science_under_sail/people.html) (visited on 12-12-12).

evidence, most of which was produced by either colleagues or students of Kroeber, and conducted his own interviews. While in Northern California he stayed with the Hudson's who were avid collectors of Indian art in the early 20th. Because Curtis stayed with the Hudson's, Curtis had access to an accomplished Pomo artist and it is Benson's work that appears prominently in Curtis' photographs (*pers. comm*. Smith-Ferri July 14, 2010). The first photograph (Figure 6.4) is entitled "Coast Pomo Bridal Costume" and depicts quite clearly, one of two visible engraved ear tubes. The second image (Figure 6.5) is entitled "A Pomo Girl" and more closely shows a pair of engraved bird bone ear tubes. The third image in this series (Figure 6.6) is reproduced with permission of the Grace Hudson museum and is a close-up of the pair of ear tubes made by William Benson and featured in the Curtis photographs. Figure 5.6 clearly shows cordage passed through the center of the tube and attached at the bottom to a basket start that is decorated with red woodpecker feathers, quail plumes, and abalone and clam shell beads. This arrangement of elements was unique to William Benson and one of the telltale signs of his craftsmanship (*pers. comm.* Smith-Ferri July 14, 2010).

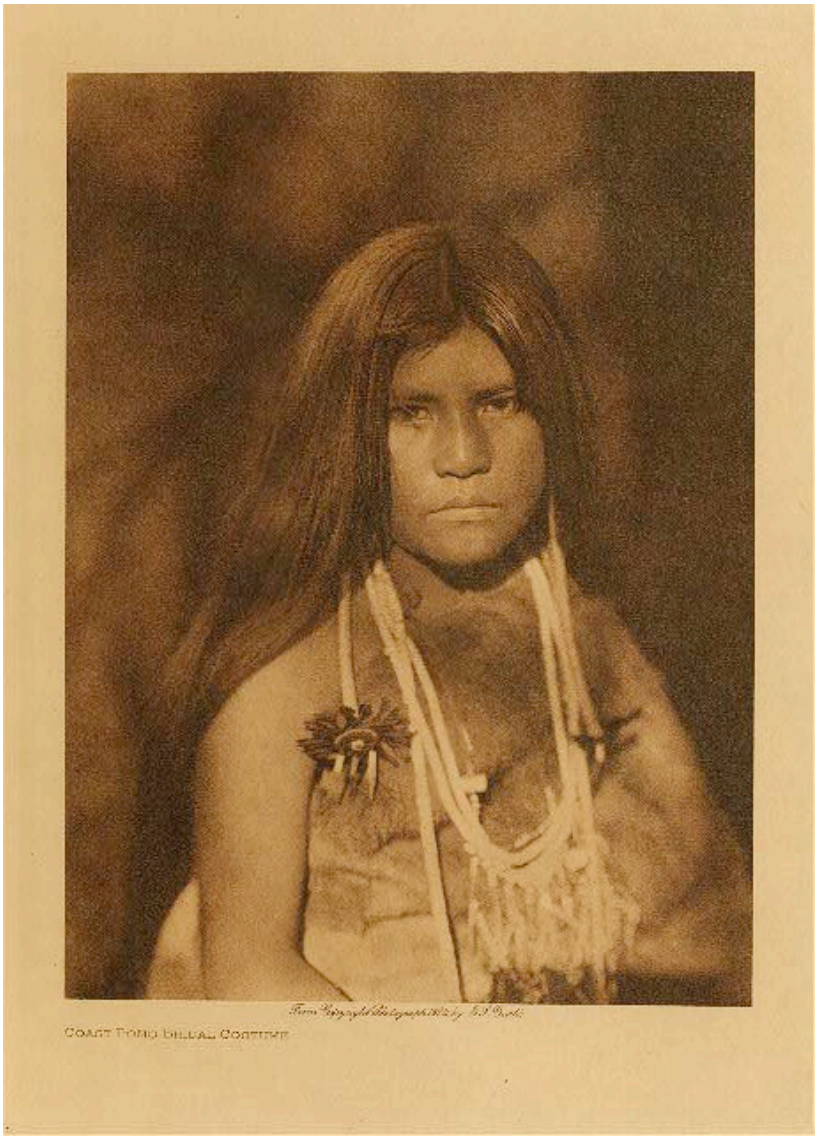


Figure 6.4 "Coast Pomo Bridal Costume" courtesy of Valley Fine Art Gallery, Aspen, Colorado.

Figure 6.5 "A Pomo Girl." Courtesy of the George Eastman House, Rochester, New York.

Figure 6.6 Ear tubes crafted by William Benson and featured in Edward Curtis' prints (Figures 6.4 and 6.5). Courtesy of the Grace Hudson Museum.

PREVIOUS ARCHAEOLOGICAL RESEARCH

I now turn my attention away from ethnographic and historic references of bird bone tubes and toward archaeological research about them. Unlike the ethnographic and historic descriptions of ear tubes, California archaeologists have tended to focus on design elements and, specifically, the design elements of engraved bird bone ear tubes. The most likely explanation for why archaeologists have paid attention to bone as opposed to wood ear tubes is preservation. The bone ear tubes are better survivors of the changing soil conditions than wood that deteriorates more rapidly. Schenck and Dawson (1929) provide the earliest archaeological description of engraved bird bone tubes in their summary of archaeology of the Northern San Joaquin Valley. It is worth quoting them at length:

> ...there were a number of bird bone tubes, elaborately ornamented with geometric designs of etched lines, that belong in an entirely different category. With the exception of the "banjo" pendants these specimens are the most definite expression of the aesthetic sense of the Lodi-Stockton aborigines that has been preserved. It would seem that their purpose was aesthetic or ceremonial, for they are not specialized in form; in many cases one end is still in the rough, and while the other end is squared off the inside gives no evidence of finishing or use. (Two unornamented tubes were noted that had been used as containers. One, uncharred itself, contained a few small charred seeds: the other, small bits of some mineral. This material may have been magical or medicinal.) Several of the pieces were found in pairs...
>
> The designs of the Lodi specimens are shown in figures 5 and 6...Most of these designs suggest basketry origins, but their adaptation to the tubes shows much skill. The application of the same design to pieces varying greatly in size suggests that the design was more or less standardized and was not conceived for the tube work. In plate 78t, probably one of the most handsome specimens, three distinct designs are used, each occupying a third of the tube. The ends were usually finished with a narrow band of different design. In addition to the designs shown in figures 5 and 6 a few variations appear. Four squares are used instead of three as in figure 5a. Plate 78y has six, seven, and eight bands within various squares instead of as in figure 6b. Other specimens show confusion in an apparent attempt to follow design (fig. 6h, pl. 78o). The fine lines and regularity of figures 6 a,c,d, 1 are particularly noteworthy (Schenck and Dawson 1929:353).

It is noteworthy that Schenck and Dawson seem to ignore or not be aware of the ethnographic information that Barrett, Gifford, Loeb, and others were collecting. This is especially puzzling given the argument for the importance of the Direct Historic Approach in California archaeology presented by Heizer (1941) at about the same time. Such an approach advocates the use of ethnographic and ethnohistoric information in tracing “backwards” through time to better account for earlier archaeological materials and evidence. In more recent publications where engraved bird bone tubes are noted as ear decoration (e.g., Chartkoff and Chartkoff 1984:191; Bennyhoff 1994: 72; McLendon and Lowy 1978:311; Ridell 1978:376) ethnographic information about the making and wearing and social lives of the bird bone tubes is ignored. Following Bennyhoff’s incomplete typology California archaeologists have focused on the variation in designs elements on engraved into the bird bone tubes as indicators of cultural difference.

As I noted above there are only a few places (e.g., Bennyhoff 1994:72) where typological analysis of engraved bird bone tubes are presented, and when are, they are based on the incomplete conclusions presented by Bennyhoff. In conjuction with this project, and thanks to Richard Hughes who is in possession of Bennyhoff's original notes I was able to engage deeply with the thought process that resulted in his engraved bird bone tube typology. While there were no definitive conclusions in his notes, they do make it clear that the bird bone tube typology developed as part of a larger project analyzing *all* forms of engraved bone objects found throughout the Great Basin and into Canada. Ultimately Bennyhoff found the attempt to create a comprehensive typology for all engraved forms of bone an untenable proposition and instead focused on engraved bird bone tubes found in the Delta region of the Central Valley. Yet, even this task was daunting, and understandably so. From a purely descriptive standpoint, engraved bird bone tubes are incredibly complex pieces of personal adornment created using a variety of elements, techniques, and combinations of patterns. In the few pages of typed notes summarizing his efforts, Bennyhoff expresses this challenge in what I have come to learn was his characteristically analytical tone. He notes:

> The number of variables is so extreme that no satisfactory system could be found. A great deal of time was devoted to the problem with little to show for it. A compromise was finally taken as a last resort and applied to the designs. Part of the results are typed for consideration. It is still complex, and in the final report I am seriously considering using a simple Type A1, B2, form, depending on wordage to explain most of the variables. Many of the elements appear to be non-diagnostic and need not clutter the final report (Bennyhoff, n.d.).

He goes on to state:

> No satisfactory descriptive typology is possible for the incised art of Central California and the actual illustrations remain preferred. There are so many variables that both word and symbol designations become inordinately long and defeat their own purpose. The compromise presented here, in which consistency has yielded to convenience, is designed to meet the specific need of the existing collection, but it is felt that the typology could be applied to the bulk of future additions (Bennyhoff, n.d.)

Despite these difficulties, Bennyhoff reproduced in visual form, some of his findings. These findings have, in turn, been used to fill out local and regional chronologies and archaeological district typologies. Unfortunately he did not include explanations about how he arrived at his conclusions. I would now like to briefly review Bennyhoff's rationale for his typology, and present my reanalysis of his findings.

From his extensive notes, it is clear that Bennyhoff approached the stylistic analysis of engraved bird bone tubes from a theoretical position halfway between approaches that we today would call Culture History and Processualism. Quoting Walter Taylor (1948:130), Bennyhoff agrees that, "in material culture, as it is found by the archaeologists, there *are* inherent systems; namely, the ones developed by the people who made, used, or possessed that material" (Bennyhoff, n.d.). While on the one hand Bennyhoff was concerned with creating and improving the local and regional California chronologies and typologies and saw their creation as a way of accessing different cultural systems (e.g., Binford 1965). His focus, like many of that time, was on the empirical representation of past human action. That is, he begins from the position that

there were nonrandom statistically and initiatively observable attributes (Chilton 1999). Rather than summarize Bennyhoff's notes and typology, it is more useful to present a transcription of key terms, descriptions of types, and a reconstruction of the typology.

ORGANIZATIONAL TERMS FOR THE BENNYHOFF BIRD BONE TUBE TYPOLOGY

What follows is descriptive and taken directly from Bennyhoff's notes regarding engraved bird bone tubes. His notes are not published nor readily accessed in any way. I include them here as both a foundation for the re-working I have carried out and for making them more widely available. The detailed nature of this typology attests an analytic strategy that resonates more with the archaeological goal of using patterns in material culture to make inferences about probable social group bounded-ness than with an inquiry into the social significances of variation for our understanding of material and visual worlds.

BENNYHOFF'S BIRD BONE TUBE TYPOLOGY DEFINITIONS

Panel: Is defined as an unbroken section of tube usually filled with a single artistic element.

Borders: Are narrow transverse bands which encircle the tube and delimit the panel.

Frames: Are terminal borders, limiting not only the terminal panels but also the design.

Element: Is the minimal artistic unit which is repeated, any reduction of which could result in the formation of a new element

Pattern: is defined as a continuous repetition of a particular arrangement of the same element. Patterns are thus subtypes of the basic element. Normally equivalent to the panel. However, it is sometimes convenient to define the pattern as two panels and the interlocking border.

Design: Is defined as the totality of the artistic expression usually limited by frames. It is a single panel (no borders break the panel) or if all panels contain the same pattern, the design is termed simple; a composite design consists of multiple patterns on the same tube, usually, but not always in separate panels. The design has limited utility because of the rarity of complete tubes and the emphasis on artistic uniqueness which seems to be demonstrated. It is the exception rather than the rule to find even paired tubes sharing exact patterns, and quite frequently the frames and borders will vary on the same tube.

BENNYHOFF BIRD BONE TUBE TYPOLOGY:

NAMES OF ELEMENTS: The elements form the primary types, and are designated vertically by descriptive names and symbolically by Roman numbers:

1. *Simple Diamond*

a. This element consists of small parallelograms, usually crosshatched, occasionally hatched. The patterns consist of varying arrangements of the diamonds formed by single diagonal lines running in opposite directions. The number of diamonds varies with the size of the tube. There are from two to seven columns around the tube, with three being the most common number. Each column has from three to thirty-eight diamonds.

2. *The Bar*
   a. This element consists of narrow, equidistant, parallel bars arranged diagonally in panels along the tube. Each panel contains two to four, in once instance five, of these bars around the tube; three is characteristic. The patterns are defined by the number of panels, direction of the diagonal bars, type of hatching and types of borders.
3. *The Ladder*
   a. The Ladder element consists of parallel equidistant diagonal bars which spiral the tube in one direction, with variant parallel bars, here considered as diagonal borders, superimposed on the bars and running in the opposite direction: the respective directions are reversed if paneling occurs. The majority of specimens is fragmentary and often lack horizontal borders, and so are included under panels defined by symbolically (). Two panels, or a single panel that covers the complete tube, again defines the pattern type.
4. *The Zigzag*
   a. This element consists of unbroken parallel zigzags, unhatched, hatched or crosshatched, arranged vertically or horizontally. The pattern contains any number of zigzags.
5. *The Chevron*
   a. This element consists of an acute angle formed by a parallel side band. The pattern consists of various arrangements of these chevrons, placed equidistant from each other.
6. *The Open Diamond*
   a. The space enclosed by the crisscross bars is filled with 1 to 5 hatched diamonds. They are termed OPEN to distinguish them from the simple diamond. The diagnostic traits are taken to be the open diamonds, and type of hatching.
7. *The Band*
   a. The panel-element is a wide crosshatched parallel band encircling the tube horizontally. The pattern consists of single or multiple bands; if multiple bands exists two panels are used to define the type since the cross hatching does not vary, but the panel borders do. No use of the crosshatched border associated with some other pattern included.

DIRECTION OF ELEMENTS. The direction of certain elements is often a major source of variation. This is denoted symbolically by Arabic numerals. In dealing with diagonals it is always assumed that the direction proceeds from top, either moving RIGHT or LEFT.

1. —
2. /
3. \
4. |

FILLER: Another variable is the type of filler. Cross-hatching is so characteristic of the compact elements (Band, Compact Diamond, Triangle) that it is always implied without a symbolical designation. The occasional hatching which occurs in these types is symbolized by a (-). The open elements are quite variable as regards type of filler and the number of lines utilized. The symbols used are the capital Arabic letters beginning with Z and proceeding backwards. [the numbers indicate the number of lines]

Z(2) Plain - 2 parallel lines ||

Z(3) Plain - 3 parallel lines |||

Y(2) Hatch that intersects 2 parallel lines |-|

X(3) Hatch that intersects 3 parallel lines |-|-|

W(4) Hatch that intersects 4 parallel lines |-|-|-|

W(5) Hatch that intersects 5 parallel lines |-|-|-|-|

W(6) Hatch that intersects 6 parallel lines |-|-|-|-|-|

V cross hatching

FRAMES AND HORIZONTAL BORDERS: Are designated by lower case Arabic letters. For simple lines the letters start with a (no border frame) and proceed forward; crosshatched borders (distinguished from the BAND element by their function and narrowness) begin with z and work backwards with each additional band; hatched borders begin with m and work backwards with each additional band.

a - No-border frame [b, c, d, etc.]

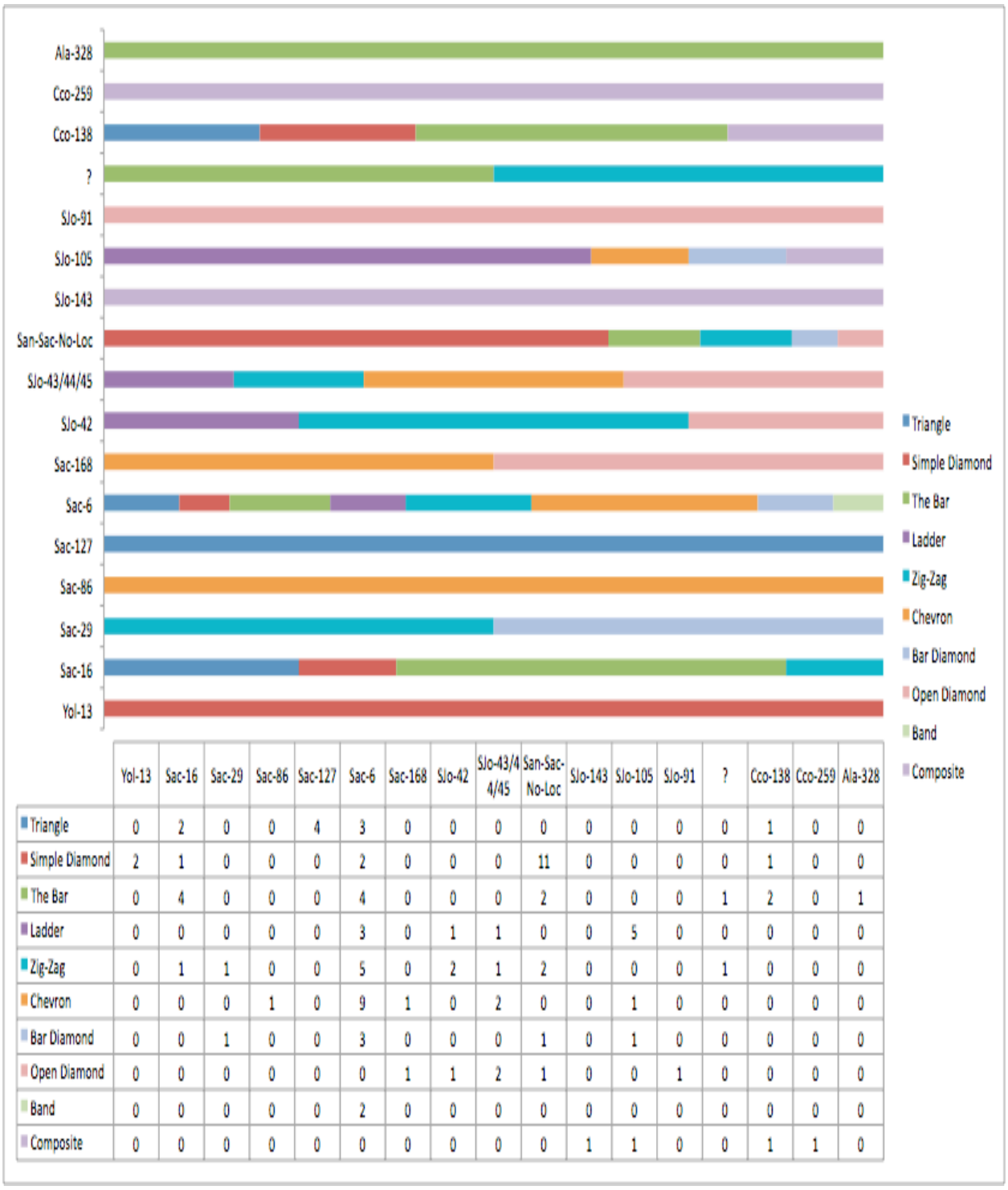
z - Crosshatched frames [z, y, etc.]

m - Hatched frames [m, etc.]

ANALYSIS OF BENNYHOFF'S BIRD BONE TUBE TYPOLOGY

There are two comments necessary before I present my reanalysis of Bennyhoff's work. First, in my view Bennyhoff over-describes the possible variation within each element (Simple Diamond, Bar, etc.). I have not included them above (although I do use them to rework his analysis), but Bennyhoff adds several subcategories to each of his *elements* in effect making them function as if it were a unique type rather than a component of his larger framework. While this is not surprising given the complexity and variation of the markings on the tubes, it does confuse the analysis and, as he points out, makes using the typology impractical. Second, in my study of the Hearst Museum collections, I found several pairs of tubes with the same design and that appeared to be made by the same maker, a point Bennyhoff never mentioned in his notes.

I now turn to my analysis of Bennyhoff's bird bone tube typology. After reconstructing his typology I categorized individual objects according to his *element* names by matching catalogue numbers in his notes with the objects. I then plotted the data with a stacked histogram in Excel. Because of the small sample size (n= 98), organizing the data by count was ineffective for showing the relationship of elements across sites. To overcome this, I plotted the data for each element as a proportion of a total sample from each site. This distributed the data equally across each and made it easier to interpret patterns that might exist. The results are shown below (Figure 6.7) and are organized along a south to north axis by site.



	Yol-13	Sac-16	Sac-29	Sac-86	Sac-127	Sac-6	Sac-168	SJo-42	SJo-43/44/45	San-Sac-No-Loc	SJo-143	SJo-105	SJo-91	?	Cco-138	Cco-259	Ala-328
Triangle	0	2	0	0	4	3	0	0	0	0	0	0	0	0	1	0	0
Simple Diamond	2	1	0	0	0	2	0	0	0	11	0	0	0	0	1	0	0
The Bar	0	4	0	0	0	4	0	0	0	2	0	0	0	1	2	0	1
Ladder	0	0	0	0	0	3	0	1	1	0	0	5	0	0	0	0	0
Zig-Zag	0	1	1	0	0	5	0	2	1	2	0	0	0	1	0	0	0
Chevron	0	0	0	1	0	9	1	0	2	0	0	1	0	0	0	0	0
Bar Diamond	0	0	1	0	0	3	0	0	0	1	0	1	0	0	0	0	0
Open Diamond	0	0	0	0	0	0	1	1	2	1	0	0	1	0	0	0	0
Band	0	0	0	0	0	2	0	0	0	0	0	0	0	0	0	0	0
Composite	0	0	0	0	0	0	0	0	0	0	1	1	0	0	1	1	0

Figure 6.7 Types of engraved bird bone ear tubes represented by site.

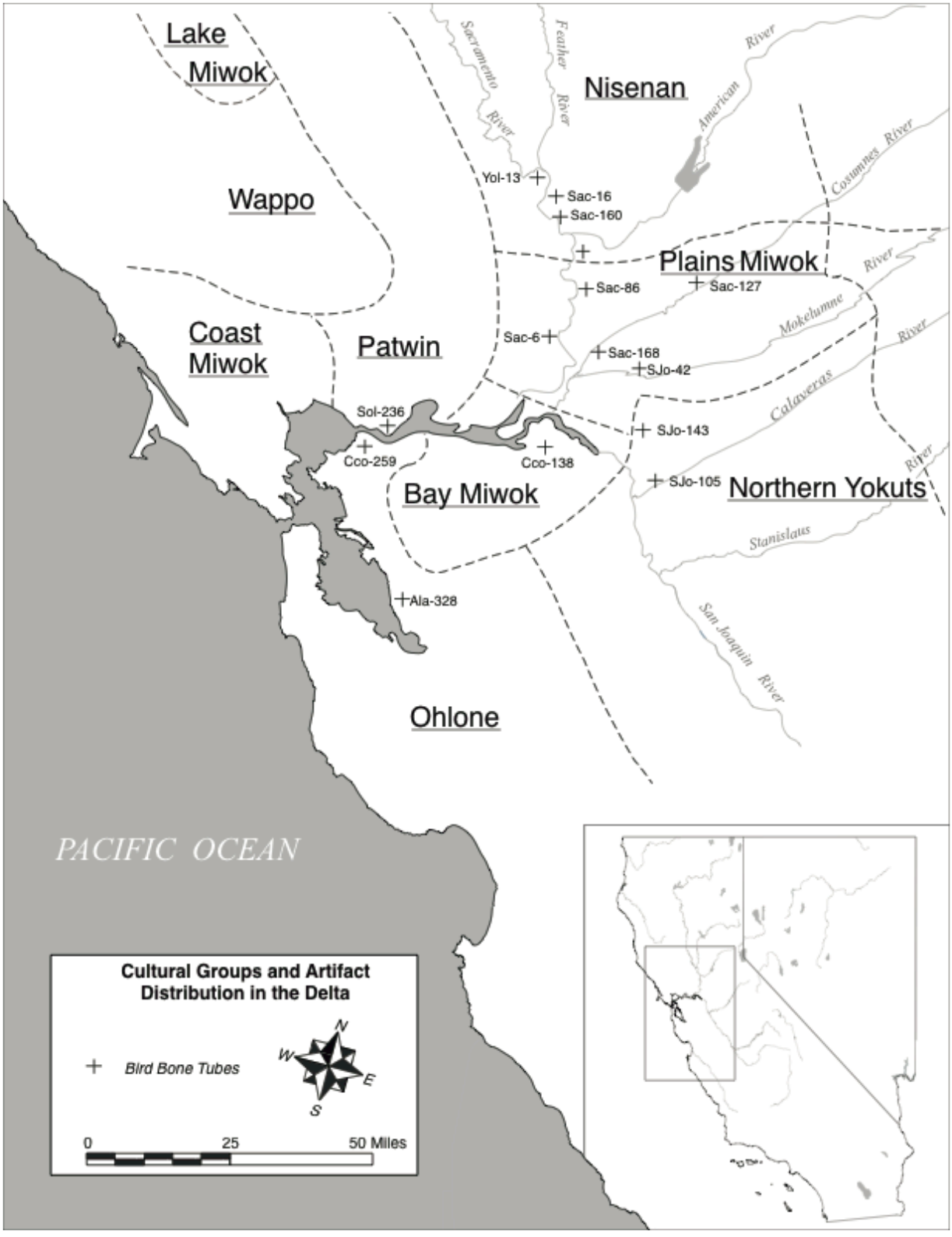


Figure 6.8 Distribution of sites in the Delta from which bird bone tubes were sampled.

Bennyhoff's typology focuses on the chronological and geographic distributions of combinations of design elements engraved into bird bone. As such, the active engagement of engraved bird bone tubes in the social world was a nonessential characteristic for elucidating social and cultural change over time. Rather, extracting discrete elements comprising patterns and then organizing those patterns along the axes of time and geography situated engraved bird bone tubes *in toto* as a proxy for culture and cultural difference. To put it another way, once patterns were recognized then certain social phenomena (ethnic groups for example) would be apparent (Chilton 1999; Conkey 1990; Stark 1999). Extracting unique design elements that have been engraved into bird bone then combining those elements into patterns without regard for the relationship between, for example the elements, patterns, actual bone, *chaîne opératoire*, and relationship of the object to the body; in other words the totality of the engraved bird bone tube ear tube, creates a fundamental misrepresentation of the object. This process diverts attention from social context by detaching specific attributes from the item then using that analysis, which is divorced from the totality, to reflect on the item as a proxy for something it may or may not have represented, in this case cultural difference and group boundaries. This process may work well for particle theory, DNA analysis, or fractal geometry where the 'part' being analyzed can be said to be equal to the whole, but with the analysis and interpretation of items such as bird bone tubes, the whole is often greater than the sum of its parts. This is not to say that it is impossible to find meaning by studying the small elements of objects and designs. Indeed, Elizabeth Chilton (1999) demonstrates that it may be less about discrete elements and more about the relationships between particular elements that furnish us with the ability to describe and interpret meaningful social and cultural phenomena.

INTERPRETATIONS

At the beginning of this chapter I questioned whether the differences in design elements engraved into bird bones and worn as ear decorations functioned as markers of ethnic group boundaries My reconstruction of Bennyhoff's typology and resulting analysis of the collection of ear tubes at the Hear Museum demonstrates greater variability in the sample population than was previously suggested by Bennyhoff himself. I also pointed to a potential gap between Bennyhoff's notes and the presentation of his findings: nothing in his notes illustrates the clearly defined differences that are found in the tables attributed to him. These findings raise several questions.

First, how does this relate to the five boundary models I proposed in chapter one? To review, these models are:

1. *Fixed*: Expect to see circumscribed patterns of distribution.
2. *Clinal*: Expect to see a gradient of in the distribution of objects.
3. *Temporally Fixed*: Expect to see circumscribed boundaries that shift according to chronological divisions.
4. *Temporally Clinal*: Similar to above, but changes should be gradual. This might be impossible to see.
5. *Punctuated*: Expect to see some types of objects circumscribed and others less constrained.

Given the data I present here, the engraved bird bone tubes do not fit into any of these models. While this outcome might be seen as problematic because it has the potential to forestall future attempts at extracting ethnic difference from stylistic elements engraved into bird bone, I interpret it as an opportunity to ask the following: why is it that bird bone tubes do not illustrate the clear or even clinal boundaries we might expect (and hope) they would? If the design elements and patterns engraved into bird bone do not indicate group boundaries in a way that is easily recognizable, might they do so in more subtle ways through relationships of particular elements (Chilton 1999; Stark 1999)? Similarly, if the ear tubes are not proxy's for ethnic boundaries, are they or can they be a proxy something else? Alternatively, and in the worst case, is their meaning is not fully understood?

Importantly, these questions point to limitations and future avenues for study. The most obvious limitation of this study is the sample size. Ninety-eight engraved tubes were taken from 16 sites distributed across an extensive geographic region. Because of this, there are a low total number of objects represented at any given site, and in some cases, only a single object is representative. This can be interpreted in one of two ways. It could be argued that the small sample size offsets my critique by creating a demand to increase the sample size by expanding the study to other institutions. The next most likely place to find collections of engraved bird bone tubes is the Anthropology Museum at Sacramento State. Although I attempted to include these objects in my study I was denied access because there were ongoing NAGPRA claims related to the collections. While a valid argument does exist for expanding the sample size, having seen many of the tubes informally at Sacramento State, and given their estimated number to be about 40, I am not positive whether increasing the overall sample by that amount will be significant. Furthermore, the engraved bird bone tubes I analyzed using the Bennhoff typology were the same tubes upon which the notes and Bennhoff findings were based. If the areal distinctions posited by Bennyhoff were viable, they should have been demonstrated by my reconstruction.

One potential area for future study of engraved bird bone tubes is to explore the "the makers hand" problem. That is to pay close attention to both the *chaîne* opératoire and more subjective characteristics of the engravings such as fineness or roughness of the lines and the relationship between these two variables. In some cases, based on a subjective standard, it seems apparent that a single artisan may have made some of the pairs tubes found at the sites sampled. Although I cannot yet substantiate this claim, it is an exciting hypothesis that opens the door to further research on these objects.

It is possible that the inability of this typology, or any typology for that matter, to tease out inherent differences between engraved patterns is limited only by the elements upon which its creator focuses. In other words, if Bennyhoff emphasized different elements or combinations of elements, different patterns might have emerged. This type of shift worked well for both Stark (1999) and Chilton (1999). For Stark, shifting her analysis of Kalinga ceramics in the Philippines from style to technological style demonstrated that subtleties in manufacturing techniques could be used to distinguish between different local communities (this is similar to a communities of practice approach posited by Lave and Wenger [1991] and employed by Roddick [2009]). Similarly, Chilton (1999) found that by shifting her analysis to the non-decorative aspects of Woodland Period pottery in New England she was able to distinguish between Algonquian and Iroquoian ceramic production processes and thus demonstrate that the application of a single typology in these different social contexts is inappropriate and unproductive.

While I find these studies methodological and theoretically compelling, vibrant, and exemplar for demonstrating how reorienting a typological framework by paying attention to more subtle indicators of style can lead to insights about identity they also highlight a potential problem, although they do not fall into it themselves. That problem is that as archaeologists we can sort archaeological data endlessly until a pattern emerges. Or, as Wobst puts it, "a strategy of 'sorting until shared form is discovered' is bound to succeed even if the archaeological recorded consisted of nothing but random numbers" (Wobst 1999:127). The ability and tendency to do this in archaeology results in what he denotes as *islands of similitude*. These islands are homogenous representations of the archaeological record (e.g., focusing solely on elements or singular patterns found on bird bone tubes) that do not highlight or address the how, why or in what ways material objects interact with and create the social world. These islands posit material objects, what Wobst calls 'interferences,' as dead forms ultimately silent as to their social meaning and existing in archaeological utopias where individuals and groups of individuals are bound in space and time by their symbols (Wobst 1999:120, 127-128).

Given my findings and this theoretical discussion of style, I argue that it is more appropriate to approach engraved bird bone ear tubes from the perspective of the wearer than the object. What I have demonstrated is that the characteristics of the engravings cannot be 'typed.' Rather, they are open ended precisely because both their form and function allows for individual variation. As Wobst (1999:128) rightly notes, "style is inseparably about society *and* individuals." Any distribution in stylistic elements is the result of individuals entering "form into their social field in ways they thought appropriate or satisfying (Wobst 1999:121, 123). In this sense, the social milieu or context into which the objects are entered is more important, it is there presence rather than their ability to mark the large scale (boundaries between cultural groups) or even more discrete strata within a social system (wealthy women) that give them social weight and meaning. Thus, it is not the distinct elements and groups of elements disconnected from a whole or even the rules for their manufacture that we might parse by eye or typology that are of interest. Perhaps the lesson to be garnered from this study is that it is the combination of the labor to create, thought process and ceremony, the "material packages" (e.g., shell beads, baked clay, obsidian, and engraved bird bone tubes), and their simultaneous integration into social worlds where different combinations have salience in specific contexts, and for specific people, even for those who do not possess or interact with that particular package. This attests to, and reinforces an understanding of, the complex and multifaceted ways in which social life is carried out. This is in stark contrast to the simplistic modes and models of cultural action that bind social units and groups in robotic ways—the same models that allow pots to be proxy's for people.

Chapter 7
Reflections

INTRODUCTION

In the previous chapter I briefly discussed a water color painting by Louis Choris. I argued that the image depicted Cholovone men from the area near Bantas/Stockton, California (Kroeber 1908:377) hunting in the San Francisco Bay area. My goal was to discuss the possible location for the Plains Miwok/Northern Yokuts "cultural boundary" in formal anthropological and archaeological terms and continue the theme of permeable cultural boundaries. That discussion raised certain questions: if the Yokuts men depicted in the painting were hunting along the eastern shores of the San Francisco Bay, what kinds of relationships existed between the Ohlone and Northern Valley Yokuts people to allow such an occurrence? What does it say, if anything, about borders - social, cultural, economic, or others? What can be inferred from this about how Native people are identified? Traditionally archaeology has focused on the patterned distribution of objects as proxies for cultural boundaries. Yet, the Choris watercolor and the ethnohistorical account of Pomo men traveling into Miwok territory to collect clam shell beads presented in Chapter 2, for example, call into question three common miscalculations. First, is mistaken correlation between objects and "Culture;" second, is the conflation between Culture and ethnicity, and third is the misrepresentation of cultural boundaries as geo-political boundaries. Indeed, the evidence I presented throughout this dissertation demonstrates that traditional archaeological markers for "Cultural" boundaries are not always adequate representations of "Culture." Archeological evidence delimits constitutive elements of Culture, but not the thing in its entirety - this is especially true when "Culture" is understood as the dispositions, orientations, and toolkits, of individuals and groups (e.g., Swidler 1986). At the heart of this dissertation is a cautionary tale about the need to pay close attention to the myriad meanings of the patterns observed in the archaeological record, not just the patterns themselves. This is true generally, but especially so in the context of NAGPRA.

In Chapter 2, I discussed the some of theoretical and empirical hurdles presented by task of culturally affiliating burial gifts collected from borderlands in the California Delta for the purpose of possible repatriation under NAGPRA. In Chapters 3 through 6, I analyzed objects from several museum collections curated by the Phoebe A. Hearst Museum of Anthropology. Most of the objects in these collections were foundational to the formulation of the culture area concept as well as contemporary archaeological interpretations of the location and nature of tribal ancestral territorial boundaries. My comprehensive analysis created one of the largest shell bead data sets, explored previously un-acknowledged objects, and re-interpreted objects unique to the California Delta. Independently, each data chapter provides the results and interpretations regarding each artifact class or type. Where possible, I tried to summarize and expand upon existing scholarship. Taken together, the data chapters demonstrate that recognizing and mapping certain behavioral patterns does not, and cannot, always lead to definite conclusions about the location of cultural boundaries. A Cultural boundary is not singular in the same way as a geo-political boundary. Rather, cultural boundaries are multiple and overlapping, and dependent upon context, history, and circumstance. At the most fundamental level of analysis, this dissertation demonstrates that if any inference about culture is to be made, careful attention needs to be paid to the specific type of boundary being observed - economic, social, ceremonial, or some other - and the relationship of that boundary to ways of being and knowing.

In this conclusion I briefly review the theoretical and empirical puzzles underpinning my research. I then review the results, both individually and collectively, of my analysis of shell beads, chipped stone obsidian objects, basket-impressed baked clay, and engraved bird bone ear tubes. Lastly, I return to the discussion of borders/boundaries in the context of NAGPRA. I argue that an appropriate way to understand the fluidity of border interactions that is demonstrated by the data presented in this dissertation is through a concept of *relations*. Along the way, I hope to point to important debates about power, knowledge production, and indigenous human rights with which the broader field of archaeology is now engaged. Failure to address these topics within archaeology results in the continued use of boundary models that are not only epistemologically and empirically inadequate, but reproduce power inequalities between the state, academy, and indigenous populations within socio-legal systems.

TENSIONS

THEORETICAL

As I discussed in Chapter 2, contemporary archaeological framings of the cultural and ethnic identities of the people living in the Delta Region of the Central Valley of California during the Late and Historic periods are rooted in culture-historic taxonomies and chronologies developed in the 1920s 1930s and 1940s. While advancements are continually made (e.g., Jones and Klar 2008), at base, many modern studies perpetuate the almost 100 year old goal of mapping "cultural" traits via observed stylistic patterns in the archaeological record. Moreover, in attempting to meet this goal, the idea that there is a one-to-one correlation between culture-historic taxa and ethnic groups is perpetuated. Linking group identity with taxonomic representations of objects that homogenize variation for the purpose of organization overlooks the important social roles objects fulfill and has the effect of defining groups internally uniform and unchanging (Jones 1997:24). This is problematic for several reasons, not the least of which is that, as Hart and Brumbach point out, the continued use of these ideas acts as an intellectual straightjacket, restricting the questions asked about the past and thus hindering the development of North American archaeology and reifying stereotypes about Native identities (Hart and Brumbach 2003:737 qtd. in Beisaw 2010:245).

In Chapter 2, I also pointed out that many of these issues extend beyond the discipline of archaeology. Importantly, I highlighted how and why specifically the culture-area concept finds traction outside the discipline, here I cited to the Kennewick Man case. Specifically, I discussed cultural affiliation as the legal manifestation of the culture-area concept. To review, NAGPRA defines cultural affiliation to mean that, "there is a relationship of shared group identity which can reasonably be traced historically or prehistorically between members of a present-day Indian tribe or Native Hawaiian organization and an identifiable earlier group" (43 C.F.R 10.2(e)). The regulations allow for several types of information to be considered to establish affiliation: historic, folkloric, geographic, biological, and oral traditions for example, practically speaking however, the discipline archaeology most often provides the expert opinions and final decisions about the final disposition of those four categories of objects. In this sense, NAGPRA is a highly formalized discussion using archaeology as a primary source of evidence to answer the question, "is this more mine [Museum] than yours [Tribe]?"

Additionally, I noted that NAGPRA's reliance on archaeological principals is not surprising for two reasons: 1) part of archaeology's popular claim to fame has been its ability to

give cognizable contemporary names and identities to objects from the past; 2) the very definition of *cultural affiliation* begs the application of culture-historical models and the direct historic approach (Steward 1942) with its assumption of cultural linearity from past to present (Beisaw 2010:244). Unfortunately, this reliance enlivens traditional archaeological models of ethnicity and identity which conflict with contemporary understandings. The continued and almost blind reliance on culture-historically informed modes of interpretation within the discipline and within NAGPRA is philosophically and empirically unreasonable.

Independent of law, the issues raised by the continued reliance on culture-historical methods and theories and cultural-taxonomies are well known to archaeologists and the discipline strives to break free from this straightjacket. Indeed much of archaeological narrative I presented is at odds with current research. This research highlights the fact that Culture and Ethnicity cannot be envisioned as homeostatic. Rather, they need to be understood as actively created, manipulated, and situational.

EMPIRICAL

The empirical problem used as a case study to highlight the theoretical tension outlined in Chapter 2 is the cultural boundary separating the Plains Miwok and the Northern Yokuts tribes in the California Delta.

Despite more than a century of determined efforts to locate and understand the nature of the Plain Miwok/Northern Yokuts boundary it is still archaeologically and anthropologically disputed. In 1926, Alfred Kroeber wrote, "in the northern part of the Yokuts areas the maps is, however, blank except for a few names of groups of uncertain situation and doubtful affinities" (Kroeber 1926:474). Approximately fifty years later, in 1977, James Bennyhoff wrote of the same area, "their [Plains Miwok] territorial boundaries have been the subject of more disagreement than those of any other group in California" and that, "…the data available on Plains Miwok society and culture are grossly deficient and unbalanced in coverage, though not as wanting for the Northern Yokuts…" (Bennyhoff 1977:1, 10). More recently, Jones and Klar noted, "...since Morratto's [1984] summary of Central Valley archaeology, our basic understanding of culture history in the [Delta] has progressed very little, and we continue to lack well-grounded chronologies for large segments of the valley (Jones and Klar 2007:147). As I pointed out in Chapter 1, a great deal of this uncertainty hinges on the fact that diagnostic artifacts representing particular archaeological districts were found "out of place" in other districts and that cultural taxonomies for these districts (thought to correspond to cultural areas) were not formalized with certainty.

Introduced in Chapter 2, and expanded on in subsequent chapters, was a discussion of ethnographic, linguistic, historical, and archaeological uncertainty regarding the location of the Plains Miwok/Northern Yokuts boundary. Linguistically, for example, Kroeber (1906:659), Barrett (1908:344-352), and Merriam (1907) argued vehemently about the specific location of the Plains Miwok/Northern Yokuts boundary (e.g., Images 6.1 and 6.2 below) while Kroeber's (1925) interpretation (Image 6.3 below) became, and remains, the accepted location of tribal cultural territories in California.

A glance at the Smithsonian's *Handbook of North American Indians*, a primary source of information for culturally affiliating museum collections, gives the impression that cultural boundaries, in the Delta for example, were clearly defined. As such, these dividing lines function as if they were pre-European geopolitical boundaries. This is perhaps especially true in the NAGPRA process when museums are reticent or tribes reluctant to affiliate a collection to one

group or another for fear of becoming embroiled in contemporary tribal politics and setting precedent for at minimum, future repatriation requests, and at most land claims. As I have tried to point out in both the introductory and data chapters, the boundaries anthropologists map(ed) are not political in the modern sense - they are not similar to the concrete fences along the U.S. Mexico border or a check point station between Israel and Palestine. Rather, they are the partial remains of past social behavior that may, but not always, be indicative unique cultural (that is social tool kits, dispositions, and moral communities) traits.[1]

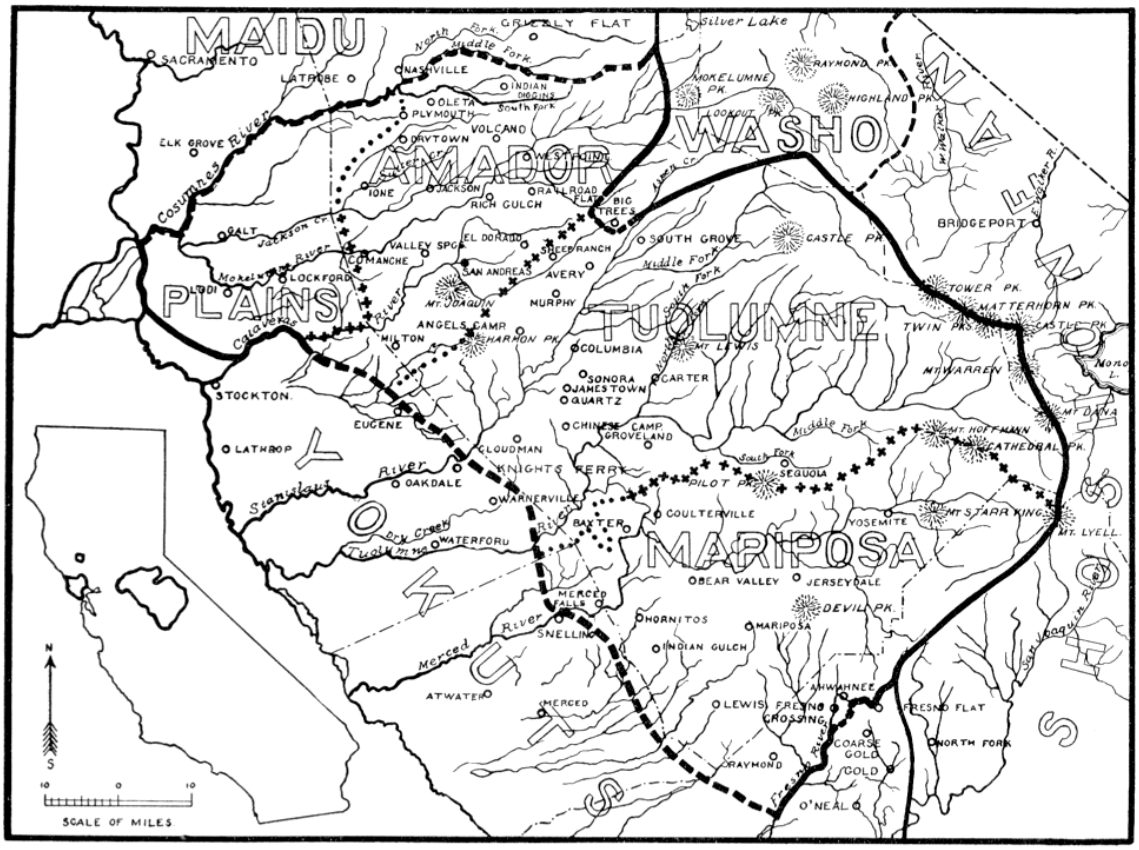


Figure 7.1 Illustrating Merriam's (1907) interpretation of Mewan (Miwok) cultural boundaries based on linguistic and ethnographic evidence.

[1] The notion that the patterns observed and circumscribed by anthropologists (be they linguistic, archaeological or some other) give the impression of geopolitical boundaries is exemplified by the chronological progression of Figures 7.1- 7.3. The borders of each map become increasingly certain and for intents and purposes, infallible.

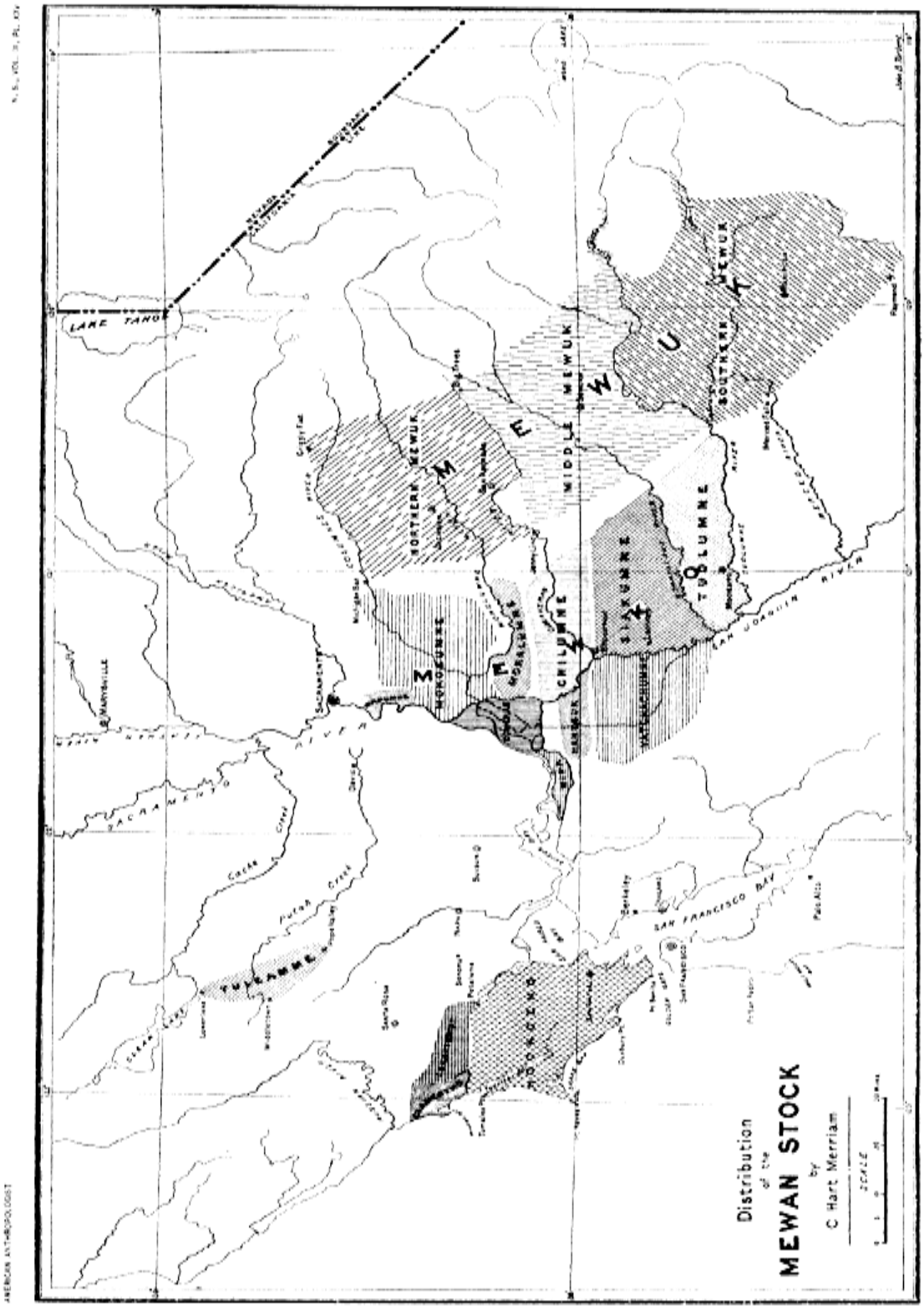


Figure 7.2 Illustrating Barrett's (1908:348, 349) proposed location of Miwok and Yokuts cultural boundaries from the San Joaquin River east to the foothills of the Sierra Mountains (with an emphasis on the Miwok).

Figure 7.3 Illustrating Kroeber's (1925) map of Native Californian tribal areas. This map remains an authoritative source.

EVIDENCE

The theoretical and empirical puzzles introduced in Chapter 1 are emblematic of broader discussion and issues in the discipline. Thus, the Plain Miwok/Northern Yokuts boundary provided an excellent staring point for not only understanding life in the Delta, but also contributing to a larger theoretical discussion. To begin, I did not start with the premise that there was or was not a boundary. Instead, I asked if a boundary or multiple boundaries existed what might it/they look like? To do this, I reanalyzed collections from 32 archaeological sites located in the Delta Region of the Central Valley dating from the Late to the Historic Periods. Examining such a large number of sites was essential to addressing the question: to whom can Ca-SJo-42 be culturally affiliated for the purposes of NAGPRA? In my analysis of these 32 sites, I focused on two types of artifacts that appear frequently in the archaeological literature—shell beads and obsidian—and two types of artifacts that appear infrequently in the literature, or not at all; basked-impressed baked clay and engraved bird bone ear tubes. In this next section I will review my findings for each of category.

SHELL BEADS. In chapter two I presented an extensive summary of shell bead studies in California. I also presented the results of my analysis of shell beads from five different sites located along a north-south transect across the Delta. To date, this is the one of the largest samples of shell beads in California archaeology (n= 204,170 total beads). Shell beads, in particular those made from *Olivella* and clam shells, are ubiquitous in the archaeological, are found in a variety of contexts throughout California and the Great Basin, show consistent variation in stylistic changes through time, and are frequently referenced in early ethnographies of California. These characteristics make them important artifacts for explaining and interpreting daily practices in pre-European Native California. In particular, there are five main areas of archaeological inquiry into shell beads: 1) building and refining archaeological typologies and chronologies; 2) studies of systems of conveyance; 3) explorations of social complexity; 4) provenience or source analysis; and 5) studies of craft specialization. While there are five avenues of shell bead research, I focused on just two, chronology and typology, and trade, and used the Bennyhoff and Hughes' (1987) shell bead typology as the basis of my analysis.

Because of the limitations associated with working with museum collections, I divided the shell beads into three different categories or *contexts*; burial, non-burial, and a combination of the two I called 'all.' Within these three contexts I conducted two types of statistical analysis - a description of the relative proportions of *Olivella* and clam shell beads found at each sites as well as cluster analysis. According to the descriptive statistics, in spite of differences in sample size between sites as well as changes in context, each site and context demonstrated the same trend. That trend was an inversely proportional relationship of *Olivella* to clam shell beads across the Delta. In other words, moving from north to south for example, the proportion of clam shell beads decreased while the proportion of *Olivella* shell beads increased. In each of the three contexts I found that the trend lines intersected at or about the location of Ca-SJo-42. I was careful to point out that this did not indicate the location of a boundary, but rather it was simply the point of intersection for two trend lines.

The second type of analysis I conducted on the shell beads was a simple cluster analysis. I did this to test whether or not there was any correlation between different classes and types of shell beads and group. Cluster analysis was an appropriate analytical tool to use to address this question because it explores the relationships between multiple variables, does not assume normality within a sample - making it appropriate for this data set where there provenience

information is limited. Cluster analysis can also create groupings that, in comparison with other data, give insight into the preferences for particular bead types. Cluster analysis also allows for an expansion of interpretations beyond those associated with the Bennyhoff and Hughes (1987) typology.

The results of the cluster analysis pointed to exciting future possibilities, but no clear answers. Several clusters did exist, the San Joaquin sites, despite their positions on either side of the Plains Miwok/ Northern Yokuts cultural boundary, clustered together according to bead type, whereas the Sacramento and Yolo County sites clustered. Although the meaning(s) of those clusters is not clear, they do provide an opportunity to ask questions about individual preference and even notions of aesthetics. The cluster analysis I conducted presents an interesting pattern.

My analysis showed that the Delta was the place where two shell bead economies met. In this sense, it was not surprising to find that clam shells for example, were limited to northern California and decreased in proportion to *Olivella* beads the further away sites were from the manufacturing location. Although my analysis demonstrated economic interactions it also supported ethnographic and oral traditions that spoke to the relationships that existed between individuals as well as groups. In this sense, shell beads were not just 'money' but rather personal adornment, objects of ceremony, and grave gifts. This highlighted the necessity of paying close attention to context and not just broad patterns.

OBSIDIAN. In chapter three I presented the results of my analysis of obsidian chipped stone objects. Because obsidian has unique and consistent characteristics, it is possible to use a variety of analytical techniques to locate the geological source of objects found in the archaeological record and better understand how and possibly in what manner they found their final destination.

I focused my attention on utilitarian and non-utilitarian objects. The category 'utilitarian object' consisted of projectile points while the category 'non-utilitarian' object represented Stockton Curves (presently, this study is the largest and, to my knowledge, only substantial XRF analysis of Stockton Curves). To find the geological sources for these objects I conducted XRF analysis to obtain the elemental "fingerprint" for each object. XRF analysis is an effective, low cost, and nondestructive method that allowed for inferences about trade networks and social relationships to be made.

I anticipated three outcomes from the XRF analysis. First, that my findings would extend the obsidian source distribution pattern uncovered by Jackson (1986) for the north-central coastal region into the heart of the Delta. Second, the concordance between Jackson's findings and mine would provide insight about the social and political relationships underpinning the obsidian trade and would speak to the permeability or impermeability of the Plains Miwok/Northern Yokuts cultural boundary. Lastly, I thought there might be multiple obsidian sources used to make Stockton curves and that each source would correlate to a particular tribelet or at least geographic area.

My findings did not completely match my expectations. While XRF analysis of utilitarian objects showed a continuation of Jackon's findings as expected, my hypothesis about Stockton Curves was not supported. I was unsuccessful at finding a correlation between obsidian source and tribelet.

Nevertheless, after comparing the XRF data with ethnographic evidence I found that there was a bidirectional flow of obsidian and ideas. This findings was based on the fact that, from ethnographic data it seemed as though the Bear Dance ceremony, the dance for which Stockton Curves were used, originated somewhere around Stockton, CA. but spread north

through the Delta (this is potentially why Stockton Curves were found at sites north of Stockton, CA. but in fewer numbers). Furthermore, based on the my findings associated with utilitarian objects, and in support of Jackon's (1986) findings, raw materials flowed in the opposite direction.

The combination of ethnographic and XRF evidence did not demonstrate clear cut economic boundaries but it did present a more complicated understanding of the Plains Miwok/Northern Yokuts border interactions. I found that in the context of NAGPRA and the affiliation process, my findings challenged traditional methods of using artifacts, such as Stockton curves, as diagnostic of a particular cultural group or economic boundary that might lead to decisions about social or cultural group affiliation. I also raised several broader questions; what *are* culturally meaningful markers of ethnicity, an emblem of participation in a social event or clan membership? Economic interactions? What if the material markers of certain practices, traditionally considered emblematic of one group were shared? If individuals crossed borders to participate in dances, ceremonies or to regularly visit kin, how do we distinguish what is culturally meaningful? The evidence derived from obsidian analysis indicated there was movement of people, things, and ideas and that clear distinctions between cultural groups based on obsidian data alone are impossible to draw.

BAKED CLAY. Baskets are of the utmost importance for Native people in California. For example, they are used for food collection, cooking, personal adornment, and ceremony. In particular, using baskets instead of ceramic vessels for cooking presents obvious challenges. Generally, to cook food in baskets stones are made hot in a fire then placed in the basket until whatever is in the basket is cooked or heated. The Delta however presents an additional obstacle to basket cooking. It is a region largely devoid of stones. To over come this difficulty Native Californian people took advantage of the abundant alluvial clay and shaped it into round or ovular forms and used them as a replacement for geologic stones - most often referred to as clay cooking stones or cooking balls.

Clay cooking stones are not as mundane as they seem at first gloss. Some of the clay cooking stones found in the Delta retain the impressions of baskets. While it is unclear how or why the impressions were made, they can provide important information about pre-European social worlds in the Delta. These hardened clay objects preserve essential information about the baskets that made them. They fill an important gap between the vast amount of ethnographic information connecting baskets and basket making traditions to specific individuals and tribal groups and the lack of basketry remains from most archaeological contexts.

By applying basket analysis techniques to basket impressed baked clay, I accessed information that is often thought to be lost to time. I sampled clay cooking stones from 13 different sites in the Delta. After examining thousands of pieces of baked clay collected from these sites, I found 562 pieces that retained impressions of baskets. Of those 562 pieces, 294 had impressions clear enough to analyze.

My analysis built on the work of Larry Dawson and with the help of his student, Ralph Shanks, I was able to decipher many of the complexities of basket analysis and apply them to the basket impressions preserved in clay.

I made three key findings. First, the large proportion of impressions of twined as opposed to coiled baskets suggests that twined baskets were used most often to make impressions in clay. As Pryor (1987) and others pointed out, twined baskets were "workhorse," everyday, utility baskets whereas baskets made by coiling tended to be used for important, special, or ceremonial

purposes. It makes sense then that I found a more twined basket impressions in baked clay cooking balls than impressions of coiled baskets. Second, the direction of weft twist, also known as 'work direction,' for impressions made by twined baskets does not vary enough to draw solid conclusions concerning the ethnic identity of the makers. Of the 196 examples of impressions made by twined baskets, 180 are worked down-and-to-the-right (DR), the remaining 16 are examples are worked up-and-to-the-right (UR). The work of Dawson and Shanks suggested Miwok weavers wove in both directions and Yokuts weavers preferred to twin up-and-to-the-right (see Table 5.2). The dominance of (DR) weft twists, a direction not usually found on ethnographic Yokuts baskets might suggest that the twined baskets used to impress the baked clay I sampled were made by Miwok weavers. Yet the presence of even a small number (16 or 8%) of all twined baskets is enough to make me leery of such a conclusion. Given that the sampled sites are located along the Bennyhoff Plains Miwok/Northern Yokuts boundary it is possible that Yokuts weavers made a few of these baskets. In Chapter 5 I coupled this finding with the fact that impressions of coiled baskets provide clear distinctions within certain categories (i.e. work direction, stitch type), but not between them. One the one hand, the dominant direction of work for impressions made by coiled baskets was to the left (98.5%). This suggests Miwok manufacture. On the other hand, impressions made from coiled baskets are dominated by non-interlocking stitches (92%), a characteristic of Yokuts weavers.

My analysis demonstrated that crucial information from archaeological contexts about baskets does exist and perhaps most importantly it demonstrates avenues for future research, future research especially in the context of feasting. Admittedly, there were some limitations: first, my findings were limited by sample size; second, I was constrained by sampling strategy of the original excavators of these sites. Despite my findings regarding cultural and ethnic boundaries providing less than definitive evidence, I was pointed to an important future questions about social and cultural behaviors. Why are there so few pieces of baked clay impressed by coiled basketry? In what contexts are these forms found? Is there any concordance between the shape of the baked clay and the type of impression found on it? To answer these questions however, a greater degree of contextual information about the basked impressed baked clay is needed; information that in large part is lacking for the objects I examined. Despite these limitations, my analysis helps to elucidate the complex social and cultural relationships taking place between individuals and groups living and interacting with one another in the Delta.

ENGRAVED BIRD BONE EAR TUBES. Like basket impressed baked clay, engraved bird bone tubes received almost no attention from archaeologists. As I discussed in Chapter 6, and according to ethnographic and historic travel narratives these personal items were often made in matching pairs by men and worn by wealthy Native Californian women until at least the middle part of the 20th Century. Most are made from the wing or leg bones of cranes, condors, eagles or other large birds. There are fleeting ethnographic and historic references from 19th Century travel narratives and paintings as well as interviews conducted by early 20th century UC Berkeley ethnographers. In fact, H.H. Bancroft commented in 1874, "ear ornaments are very much in vogue; a favorite variety being a long round piece of carved bone or weed, sometimes with beads attached..." (Bancroft 1874:368). Despite some ethnographic and historic attention, there have been no substantive archaeological studies. It is important to acknowledge that because of a dearth of contextual information relating to engraved bird bone tubes found by

archaeologists, this interpretation of their use in pre-European contexts is tenuous without further investigation into oral histories and traditions.

Despite the limitations associated with these objects Bennyhoff suggested that differences in the patterns of engravings found on the bones correlated with different archaeological districts and therefore different cultural groups. However, prior to this dissertation, no substantive discussion of how Bennyhoff arrived at these conclusions existed. Thus, I sought to test his proposition that different styles engraved on ear tubes could stand for different tribal groups. Thanks to Richard Hughes, I was given access to Bennyhoff's original notes pertaining to his analysis of engraved bird bone ear tubes. From these notes I reconstructed his typology and applied it to 98 ear tubes, excavated from 14 sites located in the Delta and North Central California. The sample presented in Chapter 6 represents every ear tube I could locate in the Hearst Museum dating from the Late to the Historic Period from the Delta and North Central California.

My analysis showed no agreement between style and group. While the variability could be the result of a small sample size distributed over a large geographic area, the objects I analyzed were the same as those analyzed by Bennyhoff. Using Bennyhoff's notes, I was able to match the catalogue numbers of the objects he typed with the engraved tubes I located in the Hearst collections. By doing this, I found there was only nominal difference in the total number of object analyzed between his sample and mine. I found most of this difference was attributable to different chronological controls. Where he took samples from Phase I and II of the Late and the Historic Periods, I limited my study to Phase II of the Late Period and the Historic Period.

Given that I was able to reapply his typology to the same objects he used to create it, I should have obtained the same results. This lack of agreement points to two key findings. First, as much as I hoped Bennyhoff's typology would illustrate clear distinctions and function as a tool for differentiating between groups living in the Delta region, it did not. Instead, this failure reiterated the practical importance of reanalyzing existing collections and retesting previous conclusions using the same methods and not just asking new questions or applying new methods to old collections. My analysis also demonstrated the correlation between style and identity is much more complex than any existing chart shows. The interpretation I posited at the end of Chapter 6 was that ear tubes were active "participants" in socially recognized and recognizable status hierarchies and that the engravings, instead of marking cultural boundaries indicated what was suitable for objects with similar social meanings and performances. In other words, the very presence of engravings, and not the patterns, were indicators of the objects meaning and its social job. Engravings were varied and open ended precisely because what ever the use of engraved tube was, it permitted individual variation on the theme. As Wobst pointed out, "style is inseparably about society *and* individuals." Any distribution in stylistic elements is the result of individuals entering "form into their social field in ways they thought appropriate or satisfying (Wobst 1999:121, 123). In this sense, the social milieu into which these objects entered is more important than their unique engraving pattering. It is their presence in, rather than their ability to mark large scale or even more discrete strata, that give them social weight and meaning.

BOUNDARY INTERPRETATIONS

When I embarked on this project, I hoped that after intensive investigation my task of culturally affiliating CA-SJo-42 would be straightforward. It has been anything but straightforward. The results of each data chapter present information about a different aspect of the social lives of

people living in the Delta in the roughly 500 years preceding the arrival of Europeans. At the beginning I presented five boundary models:

1. *Fixed*: This model is based on a traditional framing of cultural boundaries; the territory of an ethnic group as a homogeneous unit. In this case I would expect to see clear differences in stylistic variation of the artifacts. The boundaries should be 'hard'.
2. *Clinal*: The second model is one where I would expect to see a gradient of in the distribution of objects, a moving boundary. The territories for different groups should overlap like a venn diagram and in these overlapping areas there should be a great deal of mixing of artifacts. In this case, everything outside this grey area would suggest, like the first two models, hard boundaries. In this case, cultural permeability only exists in border areas. Boundaries may shift based on context.
3. *Temporally Fixed*: In this model, artifacts distribution patterns should be fairly distinct during each chronological period, but move between periods.
4. *Temporally Clinal*: This fourth type of boundary is similar to the temporally fixed boundary, but changes should be observable within each chronological period. Given my data this type of boundary might be impossible to observe.
5. *Punctuated*: The final model is the most flexible and random. I expected to see some types of objects circumscribed and others less constrained. Boundaries are more permeable than in any of the other models and there is a greater movement of objects. The overlap in material evidence could be similar to the overlap hypothesized in model two, but in this case, it should penetrate deeper into what is usually thought of as the 'cultural core.'

The combined findings from each of the data chapters do not neatly fit any one of the proposed models. This is not surprising since the boundary models are largely based upon the distributions of a single type of object, not a combination. Instead, what appears is a polythetic border in the sense that whatever borders were observed were of different magnitudes and directions; while shell beads demonstrated one pattern, basket impressed baked clay for example, showed a slightly different one, even though the study areas for both objects were largely the same (Figure 7.4).

In other words the Plains Miwok/Northern Yokuts boundary appears to be made up of multiple and overlapping constituent elements of each group or community.[2] Moreover, the degree to which those items individually and collectively reflect the nature and degree of the relationship existing between these two groups is entirely dependent upon the degree to which the social contexts in which these objects both acted and were acted upon can be understood.

[2] It is important to note that my definition of polythethic community differs from Clarke's (1968: 37-38) definition in a few key ways. First, I apply the idea to border areas where as Clarke conceived of it as a way to understand groups. Second, my use is not rigidly tied to a hierarchal organization and counting of attributes to define groupings. Rather it is based on the idea that any given group is comprised of multiple, overlapping, contingent, and sometimes different components and that some components may be more apparent - or even dominant - in any given situation.

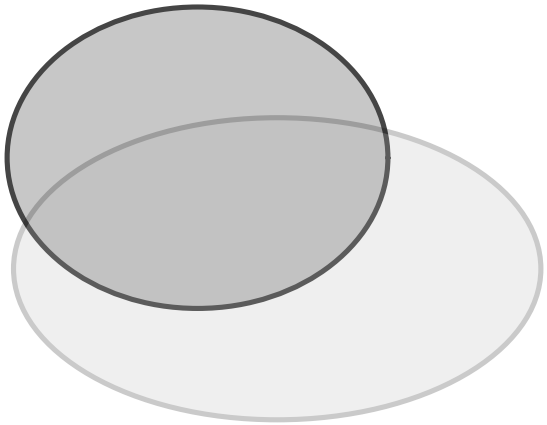


Figure 7.4 Illustration expressing the relationship of multiple and overlapping social spheres resulting from the analysis of archaeological objects. The point is a simple one: that when compared, the types of information gathered from archaeological data do not demonstrate neatly bounded social and cultural units, but rather different spheres of interaction occurring at different time and with different intensities.

NOT BACK TO THE DRAWING BOARD, BUT REDRAWING THE BOARD

At the broadest level, I attempted to provide empirical evidence demonstrating the complex and multifaceted social lives of the pre-European Delta. Specifically, my goal was to provide a clearer picture of different social interactions taking place across the Plains Miwok/Northern Yokuts boundary that might lead to conclusions about cultural boundaries for the purposes of cultural affiliation within the context of NAGPRA. Still, there are three lingering questions presented from the beginning of the dissertation. First, is it possible to tease out cultural/ethnic differences from the archaeological record? Second, how might interactions across borders be remodeled? Finally, what is the relationship between archaeological interpretations, legal contexts, and power?

Taken together, the data I created demonstrates that existing archaeological models pigeon-hole groups into neatly defined categories defined by just a few diagnostic artifacts and thus fail to account for variability. Despite these limitations it is still possible to tease cultural differences but only if closer attention is paid to the specific contexts from which objects were recovered and the analytic error of confusing culture with a "C" and culture with a "c", as outlined in chapter one, does not occur. Doing so however requires applying a more reasonable definition of "ulture to the process of cultural affiliation, a definition that understands culture as a set of dispositions, toolkits and orientations for solving problems and creating moral understanding about the world in which people live. For example, early in the first decade of the 20th century, Kroeber wrote,

> "It is the site of the village and not any social organization that gives the name to a group of people. At the same time there is no direct relation between the village and the dialect, as each dialect usually comprises a number of separate villages. Among the Yokuts it is the tribe or body of people, and not the locality or territory occupied by them, that gives them their name, and at the same time the dialect and tribe are coincident" (Kroeber 1906:662).

If correct, the above quote exemplifies a Cultural difference that is a way of understanding and interacting with the world. It speaks of how individuals related to places within a social and physical landscape, and importantly, how they understood their relationship to that landscape. While Miwok people understood themselves in relationship to specific villages or locations, Yokuts people referred to themselves as members of a larger group. This is an archaeologically unobservable "Cultural" difference, like a handshake or the protocols followed by different groups when conducting the same ceremony. This recognition does not negate the existence of boundaries nor does it make the claim that archaeologists cannot "see" Cultural differences peeking through the archaeological record. Rather, it reifies their existence, but in more careful ways. When asked about the Plains Miwok/Northern Yokuts cultural boundary, I now reply, which one? By asking this question I attempt to not allow cultural differences to be mistaken for political ones, as in a culture-historical framework. I argue my findings engender new sets of questions to be asked of the objects used to understand past social boundaries. For example, were these objects were used in ceremonials, feasts, trade, and/or for strengthening kinship relationships? Which particular boundaries are being highlighted, gender, class, status, economic religious, combinations of them? All of them? What do these things tell us about people's dispositions and orientations? When we answer these questions, we better answer questions of cultural affiliation.

Lastly, I turn to the question of politics power and knowledge. The evidence I presented in this dissertation reinforces the well-known critique that culture-historical models of group identity are inadequate. Despite this, NAGPRA draws on these models to carry out the process it calls *cultural affiliation.* This is problematic for two reasons; first it ignores advances made in the social sciences regarding culture and border dynamics and inter-relationships. Second, relying on these past models replicates power inequalities between the state, the academy and indigenous communities. Take for example the following excerpt from Alfred Kroeber, written in response to his Indian Claims Commission testimony, and published in 1955.

> It was White contact, pressure, edicts, or administration that converted most American Indian nations or nationalities into 'tribes,' that is to say, "tribal status." It was we Caucasians who again and again rolled a number of related obscure bands or minute villages into the larger package of a 'tribe,'...It was infinitely more convenient and practicable for us to deal with representatives of one large group than with those of ten, twenty, or thirty tiny and shifting ones whose very names and precise habitat often were not known...Generally we treated the nationality - "tribes" as if they were sovereign state-tribes...(Kroeber 1955:304).

Kroeber described the situation accurately. His summary points directly to the symbolic and practical power archaeologists historically claimed as a justification for the existence of the discipline and continue to claim, in part, because of NAGPRA. This is the power to define and to name. As I pointed out earlier, it is to archaeological evidence and archaeologists that NAGPRA practitioners most often turn for final determinations of cultural affiliation. To borrow from Bourdieu, this "…is a power of constructing reality, and one which tends to establish a *gnoseological* order: [or] the immediate meaning of the world," it is "…a power of constituting the given through utterances, of making people see and believe, of confirming or transforming the vision of the world, and thereby, action on the world and thus the world itself. This is almost

magical power which enables one to obtain the equivalent of what is obtained through force…" (Bourdieu 1994:166, 170). Understanding this opens the broader field of archaeology to new questions as well as new politically engaged roles.

In this setting tribal voices, interpretations, and sources of information become secondary or at worst are ignored. Moreover, there exists a complex interplay between past and present. While archaeologists search for past social realities the most common ways in which this is done reinforce contemporary definition of what a "tribe" is—a unique sovereign with a special government-to-government relationship with the United States. Traditional models represent culture areas as if they were the same thing as geopolitical territories. They confuse patterns of social behavior for cultural action/interaction and unintentionally represent changes in those observed patterns as if they indicated past political territories. If nothing else, I have shown this is not the case and that more care needs to be given to creating and understanding what exactly is, and is meant by, a cultural boundary. It is axiomatic to say that existing common approaches to cultural affiliation are inadequate within the discipline. This is recognition is not enough; it is cheap lip-service while power inequalities between the state, academy, and indigenous populations remain.

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