
ON MYTHS, MYTHMAKERS AND POST MODERN SCIENCE: A COMMENT ON JEANNE ARNOLD'S DISMISSAL OF A PREHISTORIC POLYNESIAN CONTACT EVENT IN SOUTHERN CALIFORNIA

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Ku-ku ka ehū o ke kai i na auwaa liliī.

"A fleet of small canoes will dash up the spray".¹

In a recent lead article in *American Antiquity*, Jeanne Arnold (2007) challenged our assertion (Jones & Klar 2005; Klar & Jones 2005) that prehistoric Polynesians made contact with Native societies in southern California. While we responded to some of Arnold's charges and an earlier critique by Anderson (2006) in *American Antiquity* (Jones & Klar 2006, 2008), we feel that it is also important to carry on this dialog with the Pacific specialists reached by the *Rapa Nui Journal*. Here we review some of the points made previously and develop several others in response to Arnold's rejection of a Polynesian contact event. In our original papers we offered a combination of archaeological and linguistic evidence in support of prehistoric diffusion between Polynesia and the Chumash and Gabrielino of southern California. Specifically, we argued that it was from this contact that Native Californians learned the technique for sewn-plank boat construction (along with linguistic referents) and the crafting of a particular style of two-piece fish hook from Polynesians. Such notions of transoceanic diffusion do not fit well within either the modernist (ecological) or post modernist paradigms that influence archaeological thinking in the United States (see discussion by Hegemon 2003), and we anticipated an onslaught of challenges from the North American archaeological community. Certainly a response from one or more southern California specialists was expected since the Chumash/Gabrielino ethnographic territory where we believe Polynesian contact took place is one of the most heavily researched areas in western North America and the Chumash are one of California's most highly acclaimed Native societies. Jeanne Arnold's (2007) paper, "Credit Where Credit is Due: The History of the Chumash Oceangoing Plank Canoes", thus came as no surprise since Arnold is one of the leading scholars in the Chumash area and is deeply invested in models of "in situ" innovation (see Arnold 1992). However, in re-asserting the case for autochthonous development of the sewn-plank canoe in southern California, Arnold completely dismisses certain lines of evidence (e.g., linguistics) in favor of extreme advocacy for one particular Native society (the Chumash) over others, and asserts that her own narrow views of Chumash prehistory constitute an unassailable ethnic "history". In defense of our original proposal, we consider here issues of

Native advocacy, linguistics, oral history, canoe size, genetics, chronology, and the implications of new findings from South America that clearly establish prehistoric Polynesian contact with the New World.

NATIVE ADVOCACY AND ETHNIC MYTH-MAKING

More than anything else, Arnold (2007) has essentially argued that to suggest Polynesians made contact among the Chumash and introduced the technique of sewn-plank boat construction constitutes an ethnocentric, if not racist, insult in that it implies that the Chumash were not capable of inventing this complex technology on their own. Ethnocentric thinking of this type harkens back to views on the origins of the Moundbuilder cultures from the 18th and 19th centuries, when the mounds and artifacts within them were thought to be too elaborate to be the work of American Indians. Any American archaeologist or linguist trained in the 20th century is well acquainted with the racist sentiments that underlie this line of thinking, and likewise is more than aware of the intellectual and technological achievements and capabilities of indigenous people both in North America and in the Pacific. Indeed, as we stated in the original paper (Jones & Klar 2005:478), one of us (Jones in Jones & Hildebrandt 1995) had long been comfortable with the position that the Chumash invented the sewn-plank canoe (Chumashan *tomolo*) in order to fish and hunt more effectively in the waters of the Santa Barbara Channel. The reason that we changed our view to one in which sewn-plank boat construction is seen to be a product of contact with Polynesia is simply the linguistics; while Chumash potential to invent new technologies and improve their maritime adaptation may well have been unlimited, they could not independently invent both Polynesian-sounding words and a Polynesian style of boat construction. In her haste to portray us as ethnically insensitive and promote her own views of the regional prehistory, Arnold disregards certain aspects of our argument and dismisses the linguistics entirely, yet our case is very much based on a combination of linguistic and material evidence. It was for this reason that we followed our 2005 *American Antiquity* paper with a more detailed treatment of the linguistic analysis (Klar & Jones 2005). Here we review

some of the salient aspects of that analysis in rebuttal to specific issues raised by Arnold.

ARNOLD'S LINGUISTIC OBJECTIONS

Arnold feels she can dismiss the linguistics simply and quickly by asking the rhetorical question: "It is odd that just one word (*tomolo*) was borrowed if Hawaiians arrived and stayed among the Chumash, as Jones and Klar suggest. Would we not expect other borrowed words associated with Hawaiian boat parts, leaders, clothing, status markers, or weapons, all of which would have been salient for the Chumash?" (Arnold 2007:203), to which she clearly expects an affirmative reply from typical readers. But from a linguistic perspective, the answer is precisely the opposite. If we found words for all those items, we would expect also that we could at least reasonably posit the diffusion of those material items themselves from Central Eastern or Eastern Polynesia, and we could expect corroborating archaeological and ethnographic evidence. But for now we have no evidence that such items were conveyed. We can say only that the linguistic data point to the Chumash adopting one specific manufacturing technique, plank-sewing, which gave them the ability to immediately make better use of one of their "scarce" and "valuable" (Arnold's terms) resources. We are not proposing that the Chumash and Gabrielino borrowed other "boat parts" or "leaders, clothing, status markers, or weapons" from visiting Polynesians, only plank-sewing and a new style of compound bone fishhook.

In Klar and Jones (2005), we posit the following sequence for Chumashan adoption of *tomolo* as the single signifier of "sewn-plank canoe". The other Chumashan words for varieties of boats are *'axipeneš* "dug-out canoe" (literally "worked piece of wood") and *tomol 'ištapan* "tule boat" (literally "*tomol* made of tules"). In 1878, Alphonse Pinart recorded a Purisimeño² form *šuašuaš* "boat" (Heizer 1952:45). This form occurs in no other known attestation of any Chumashan dialect (except in a placename *swaxil* — *q.v.*, Klar & Jones 2005:395), but there is little doubt that we can reconstitute the stem of this form as **šwax* ~ *swax*, a simple, unanalyzable, canonically Chumashan form, and that it formed part of the pre-*tomolo* boat lexicon in Chumashan, most likely being the generic word for any kind of boat. We propose that when the Chumash borrowed **tumuRaa'au* > **tomolo'o*, it signified not the canoe itself, but the material (wood) from which the canoe planks were hewn, and that it became part of a compound **šwax 'itomolo'o* ("*šwax* made of *tomolo'o*"; *i.e.*, "plank canoe"). Eventually, in a common type of metonymic process, the modifier (**tomolo'o*) became the word for the sewn-plank canoe itself. Later, perhaps because of the prestige of the sewn-plank boat, the old word (**šwax*) was lost almost everywhere in Chumashan except among the Purisimeño, for whom the sewn-plank boat was essentially a "foreign" object. Prior to the advent of plank-sewing in Chumashan society, a tule balsa would have been **šwax 'ištapan*, but the new, high prestige word *tomolo* or *tomol* (depending on dialect

development) replaced **šwax* here as well, becoming *tomol 'ištapan* ("*tomol* made of tules") (see Klar & Jones 2005:397).

But this is not the whole story, and not even the most important problem with Arnold's objections. For whatever reason, Arnold seems to have forgotten that we isolated not one, but three words which we argue are of Central Eastern Polynesian origin. We find only one (*tomolo*) in Chumashan languages, but in Gabrielino, the language of the only other North American group to sew planks, there are in fact two words. One (*ti'at*) is the word for a sewn-plank boat; the other (*tarayna* or *taraynxa*) is the word for "boat" in general. We derive *ti'at* from a Central Eastern Polynesian (and proto-Polynesian) base **tia* "to sew", and *tarayna* / *tarynxa* from a base **talai* "to adze, hew". *Ti'at* is thus "sewn object" and *tarayna/taraynxa* is "hewn object". In both cases, these are bases closely associated in antiquity as well as in modern times with the lexicon of Polynesian canoe technology. Considering both the Gabrielino and Chumashan forms, this is three items in two languages, in each case relating to a specific aspect of canoe construction. This is the kind of list that one might realistically expect in the situation. As Nicolay astutely pointed out in this journal:

The diffusion of words is even more complicated than the transfer of technologies.... It is not simply a question of two groups of people sitting down with their corresponding tourist phrasebooks and deliberately selecting the optimal word for a new idea. In fact, it is anthropologically naïve to envision the Chumash conducting a Tarzan-and-Jane type language session with their hypothetical Polynesian visitors trying to acquire various terms in each others' tongues. The Chumash had watercraft, and there is evidence that they had them at least since the early Holocene; it is the Polynesian technology that would have caught their attention. It makes perfect sense that they would only have retained a word for something they did not already have. (Nicolay 2007:65)

No matter how one reads the evidence, the Gabrielino forms must be part of the discussion. Our hypothesis suggests that the Chumash and Gabrielino encountered Polynesians at the same time and learned the plank-sewing technology together, not that the Gabrielino later learned it from the Chumash. If the Brotherhood of the Canoe guild was such a prestige-based institution with tightly restricted membership, and its secrets passed down from generation to generation, how likely is it that the Chumash elites who controlled this information would have willingly given it to another tribe? However, if this somehow happened, and the Gabrielino "received" the words *ti'at* and *tarayna* from the Chumash, we have no evidence to suggest that these words ever existed in the Chumashan languages. Here is the only bit of pure speculation we are willing to indulge in: the actual physical meeting point between Polynesians and native Southern Californians may have been at Malibu, at the boundary of Gabrielino and Chumashan territory, an eminently suitable place for boats to land; and that three ethnic groups were present for the duration of the contact. What happened subsequently is an interesting story in its own right, but is not the subject of our hypothesis.

In another justification for ignoring the linguistic evidence, Arnold states, "Lastly, the linguistic evidence — that

the word *tomolo* could have derived from Polynesian roots — is difficult to categorically accept or reject, and I leave that to linguists”. (Arnold 2007:203-204). To address this item, we would point out that at every stage throughout the process of research, writing, peer review, and publication of both Jones and Klar (2005) and Klar and Jones (2005), we consulted with, and were advised, critiqued, and reviewed by professional linguists who are specialists in Chumashan, Uto-Aztecan, and Polynesian languages. Those with whom we consulted are named in Klar and Jones (2005); one of them teaches at Arnold’s own university. Those linguists subjected our work to intense scrutiny, asked hard questions, gave valuable critique, and have all ultimately found the data to be genuine, the arguments to be methodologically sound, and our interpretation of its significance to be the most parsimonious explanation for the presence of anomalous lexical items in Chumashan and Gabrielino. The linguists *have* decided; what is difficult to understand is why Arnold thinks she can so neatly sever the linguistic evidence from the rest of the argument and still have a complete grasp of the situation.

Finally, Arnold attempts to attribute the linguistic borrowings to the post-contact era:

If acceptable, still no grounds have yet been provided, as I understand it, to contend that it was *borrowed at a specific time* [italics original]. A brief episode of contact responsible for the borrowed term could have occurred any time before observers began to record the Chumash [sic] languages — theoretically as late as the 1700s. In the absence of historical linguistic evidence providing time depth, this word’s presence among post contact Chumash [sic] speakers does not have specific chronological significance and tells us nothing of note about the origins of the *tomol*. (Arnold 2007:204)

First, to reiterate, competent linguists have found our hypothesis “acceptable”. Second, we are not talking about “the origins of the *tomol*”, only a specific technique associated with its construction, and the origin of the lexical item itself. Arnold’s other point is the lack of a *specific* date, but her complete dismissal of historical linguistic methodology to illuminate time depth is wholly unjustified. The Southern (Island and Central) Chumash forms show considerable diversity, and the four dialects of Central Chumash developed distinctive forms of their own from an original Southern Chumashan proto-form **tomolo’o* (the product of the “Chumashization” of **tumu-Raa’au*). This fact alone argues for significant time depth. There is enough diversity in those forms to be able to say with certainty that the borrowing did not take place in historical times, or even within the preceding half millennium. There would simply have been too little time for those forms to diverge as widely as they did, especially considering the close geographical proximity of each dialect’s speakers, a situation that would have if anything hindered diversity, not promoted it.

Furthermore, if Arnold has trouble accepting that the Chumash may have borrowed *tomolo* contemporaneous with the adoption (or development) of plank-sewing, why would she be willing to accept that the Chumash might, at a much

later date during the historical period, have simply replaced whatever perfectly good Chumashan word they had been using with a new word from a Polynesian language? Nicolay’s comment, quoted above, is relevant here as well. However, assuming for the sake of argument that such a replacement did happen, the word would almost certainly have come from modern Tahitian, Maori, or Marquesan, for it is those Polynesian dialects which retain the proto-Polynesian **/t/* (as in **tumu-Raa’au*). Standard Hawaiian has shifted **/t/* to **/k/* (hence *kumulaa’au* in Hawaiian); only in conservative and ceremonial usages is the **/t/* retained. A case would have to be made for either a dialectally conservative Hawaiian, or a speaker of one of the other Central Eastern Polynesian languages, to have been the origin of the *tomolo* form; otherwise, it would be **komolo* (and variants), or perhaps even something like **kumula’o*, depending upon how active vowel harmony restrictions were at the time of a more recent borrowing.

MYTHS AND ORAL TRADITION

Beyond the strictly linguistic evidence, Arnold also asks why native traditional lore offers no support for Polynesian contact. “Also puzzling”, she says, “is an absence of Chumash oral narrative about foreigners teaching ancestors how to make boats (as are found frequently on the Northwest Coast, for example) or visitors who appeared in large, impressive boats. Recorded Chumash narratives about *tomol* making clearly focus on elders and other local agents of invention and teaching” (Arnold 2007:202). In support of this statement she cites Blackburn’s (1975) *December’s Child: A Book of Chumash Oral Narratives*, a compilation (from disparate parts of John P. Harrington’s Chumashan notes) and reconstruction of traditional Chumashan narratives. This volume contains only four stories which touch at all on the details of canoe-building, and only one deals with canoe origins, though hardly in a way which would support Arnold’s position.³ In a narrative which Harrington (Blackburn) labels “Astrology”, Fernando Librado relates that:

The month of October was called *hesiq’momoy quvwue sulupiauset*, the “month of sulupiauset.” All canoemen were ordered not to go out at this time. Sulupiauset was the great-grandfather of Fernando’s uncle. When the first canoe was made, he was the first to enter it. There had been canoes before, but the ends had been round. He was the first to make one with ends such as they have now, and with rods for the inside of the ribs. He taught people the use of the canoe, how to sail out to sea and when. (Blackburn 1975:102)

The impossibility of this referring literally to the ultimate origins of the sewn-plank canoe scarcely needs to be emphasized. However, in a mythic sense, it does not reveal *who* made the first canoe, only that an ancestor of Fernando’s was “the first to enter it”. Subsequently, that ancestor is said to have introduced design changes to the canoe, and then to pass along his maritime knowledge.

Remarkably, Arnold fails to consult Hudson, *et al.*’s (1978) compendium of all of Harrington’s notes on the sewn-

plank canoe, *Tomol: Chumash Watercraft as Described in the Ethnographic Notes of John P. Harrington*. Here one finds 19 brief stories, told by Fernando Librado to John P. Harrington, about canoes in general and about specific individuals' involvement with various aspects of canoe culture. Indeed, most of these tales "focus on elders and other local agents of invention and teaching" (Hudson, *et al.* 1978:143-167). The first story in the *Tomol* corpus is the only narrative whose subject has unambiguously ancient origins, and it is from these few lines that Arnold attempts to spin out her own myth concerning the details of how the Chumash invented and tested the plank canoe technology over many generations. The story begins:

The first man in this world said that all the world is a canoe, for we are all one, and that which we finish now is a canoe. When the first canoe was finished, the first man who made it called the others to pay close attention to his canoemaking. Later this maker and his contemporaries died. The next generation remembered how the first man had made a canoe, so they too made one. There was always a little difference in their work, so their canoe was a little different from the first one. This generation died and another followed. They always did as the first man in making their canoes, and so it continued. (Hudson, *et al.* 1978:143)

As is common in traditional material from around the world, there is likely considerable distortion of what may be the historical truth behind the story as told here. For one thing, the "first canoe" springs fully-formed from the skills of its maker. But as the story stands, there is no indication of the ethnic affiliation of "the first man" who made a canoe. He exists in mythic time. The story merely says that someone made a "first [plank] canoe", others paid "close attention", and the skill was passed down from generation to generation, with each generation introducing small differences into their boats. As an artifact of historical memory, this could apply as well to an *in situ* development of plank-sewing as to a contact event with foreigners from whom the specific techniques for making plank (as opposed to dugout or tule balsa) canoes were learned. Development of the canoe *in toto* in this case is a figure which represents the acquisition of the technology by which plank canoes were distinguished from all other canoes.

The story continues:

Many men arrived here from their own lands [emphasis added], and they saw also how a canoe was built and paid attention to it. Several years later they made their own canoes, continuing to make changes in size and form (Hudson, *et al.* 1978:143).

Contra Arnold, the Chumash origin myth for the plank canoe indeed includes mention of others arriving from elsewhere. This passage could well represent a genuine ancient memory of having been visited by foreigners, but with the inversion of instructor and instructed. It is also possible that *they* in "they saw also how a canoe was built..." refers to the Chumash, not to the "men [who] arrived here from their own lands". At the least, the pronoun reference is ambiguous. However, given the subsequent cultural developments that the perfection of the sewn-plank canoe initiated among the Chumash,

the importance of the canoe owners having been the originators of this important knowledge is all too clear. It justifies an elite group in their control of "scarce" and "valuable" resources. Blackburn, following Fischer's (1963) ideas on anthropological approaches to understanding folk tales, suggests that "[t]he presence of distortion or fantasy is an indication of cultural stress or concern". (Blackburn 1975:xvii). We can envision no period of greater "cultural stress or concern" than the duration of the evolution from a simple hunter-fisher-gatherer society to one of complex chieftainships with unequal distribution of resources and social status. Those in charge would gain great authority from being able to claim credit for the invention of the very item which ensures their superior status. Providing such traditional authority is a time-honored responsibility of storytellers.

Further, Arnold considers only the oral history of the one society, the Chumash, yet many Polynesian societies have oral histories that allude to eastward voyages where land was encountered (*e.g.*, Dunis 2005; Handy 1930). Hawaiian oral history includes at least one unmistakable reference to a successful pre-contact, round-trip voyage to the New World. This account comes from the writings of Samuel Manaialani Kamakau, a well-respected native Hawaiian scholar of the 19th century, who collected and published research on the history and traditions of his people. According to the Bishop Museum's web page (<http://www.bishopmuseum.org/press/authors.html>), Kamakau's writings were originally published in a series in weekly Hawaiian language newspapers between 1866 and 1871. The following account was originally published in *Ka Nupepa Ku'oko'a* on August 12, 1865: "PUPUHULU-ANA was the pioneer voyager to Kahiki (foreign land), the land of America. Olo-lo-i-me-hani was its name, and this was the reason for the journey...". Here we are provided an account that not only describes a voyage that resulted in a contact event, but the name of the individual who accomplished the feat. There is also an interesting similarity in the meaning of the Hawaiian name for America and the meaning of the Chumash settlement of Malibu⁴. At the very least, we suggest that if oral history can be considered a legitimate source of information on the issue of contact, then Chumashan and Hawaiian oral narratives are mutually consistent in indicating that such an event took place. Furthermore, in recognizing the oral traditions of both societies, we denigrate neither of them.

CHRONOLOGICAL ISSUES

Arnold also raises the issue of chronology, which is somewhat complicated because it involves chronological sequences from both the Pacific and the North American mainland. Not surprisingly, questions of chronology have already been raised in an earlier challenge to our work from a Pacific specialist (see Anderson 2006; Jones & Klar 2006). Ultimately there are two phenomena whose dates are critical: the arrival of humans in Central Eastern and Eastern Polynesia (including Hawai'i), and the appearance of sewn-plank boats and a Polynesian-style composite bone fishhook in the Santa Barbara Channel.

Arnold asserts that the sewn-plank canoe appeared in the Santa Barbara Channel at 500 CE and that the settlement of eastern Polynesia is now accepted to date no earlier than *c.* cal 800 CE. In point of fact, the chronological sequences that define the timing of these events are neither perfect nor universally agreed upon, particularly in the Pacific where the chronology of human settlement has recently become highly contentious (see Flenley & Butler 2007; Hunt & Lipo 2006, 2007; Kirch 2007:11).

For California, we relied on our 2005 paper on Chester King's cultural sequence for the Santa Barbara Channel which was originally completed in 1982, and was later revised and published (King 1990), as well as Gamble's (2002) thorough summary of data on the antiquity of sewn-plank canoes. We also referred to proxy evidence from a study of pelagic fish remains by Arnold and one of her students (Arnold & Bernard 2005; Bernard 2001, 2004). King's cultural sequence was constructed on the basis of grave lot seriation, and in it, he assigns artifacts, including trifacial stone canoe (*tomolo*) drills, and shell and bone fishhooks, to phases. This cultural chronology represents an exceptionally important piece of scholarly research that provides the critical temporal framework for Santa Barbara Channel prehistory. However, anyone relying on the King sequence knows that it is not securely anchored in absolute time. King had few radiocarbon dates at his disposal in the late 1970s, many were not directly associated with graves, and the procedures employed to compensate for isotope fractionation and calibration of shell dates have never been clear. Some of these issues were resolved in the 1990 publication, but many southern California archaeologists have still sought to improve King's relative sequence by directly dating diagnostic artifact types via accelerator mass spectrometry (AMS). Most of this work has been done by Erlandson and his colleagues (*e.g.*, Erlandson, *et al.* 2005; Rick, *et al.* 2002; Vellanoweth 2001; see also Gibson and Koerper 2000) although Arnold herself has also seen fit to revise King's sequence as well (see Munns & Arnold 2002:131). Elsewhere in California (*e.g.*, the San Francisco Bay area) such efforts have resulted in significant refinement to chronological sequences with age ranges of phases and diagnostic artifacts adjusted by one or several centuries (see Groza 2002; Hughes & Milliken 2007). Comparable chronological precision has yet to be achieved in the Santa Barbara Channel, however, and the age ranges of most phases and diagnostic artifacts must be considered approximations with \pm factors of at least 100-200 years. Recognizing the reality of this situation in our 2005 paper, we ascribed the Polynesian contact event to a chronological window between cal 400 and 800 CE which encompassed most of the phases during which artifacts associated with the *tomolo* (*e.g.*, stone drill and the two-piece, Polynesian style fishhook) seem to have appeared.

Arnold's contention that the Chumash *tomolo* was invented in cal 500 CE is another example of her broader attempt to posit her own narrow interpretations as historic facts. In support of her position, she dismisses direct chronological evidence in favor of remote and poorly-dated proxies.

Specifically, she dismisses the oldest direct date from a drilled canoe plank (cal 625-700 CE; two sigma range) based on the "old wood" problem rather than acknowledge that use of a 100-year old redwood log to make the plank would mean that the date represents a range of cal 725-800 CE. This date remains, in fact, the best piece of evidence for the antiquity of the *tomolo* in the Santa Barbara Channel, and it falls well within our original window. Arnold insists instead that the remains of pelagic fish provide a more accurate index for the appearance of sewn-plank boats in the Santa Barbara Channel than a directly dated piece of a sewn-plank canoe (Arnold & Bernard 2005; Bernard 2004). Pinpointing the precise time when pelagic fishing became important in the Channel is far from straight-forward, however. The remains of pelagic fish are extremely uncommon in midden deposits in the Santa Barbara Channel, and dating their relative frequency is a highly imprecise. In Bernard's original study, pelagic fish remains appear in minute amounts sometime during the middle of the first millennium CE and increase incrementally into the 1300s (Bernard 2001). The dating in this study, however, is highly suspect since entire collections were assigned to unrealistically precise 100-year intervals, and the numbers of fish remains used to project the increase through time were extremely low. In her own discussion of the chronology, Arnold acknowledges the approximate dating in the study when she states: "the data demonstrate that albacore, yellowfin, and bluefin tuna began to be acquired in the 500-700 CE interval, and swordfish began to appear after 700 CE or so, although it was not until the 1300-1400s CE that swordfish became much more common" (Arnold 2007:201). In a revised version of the original study, Bernard (2004:31) aggregates data in more realistic 500-year increments which show an increase during the cal 500-1000 CE period. Nothing in these studies points to cal 500 CE as the date for an ostensive adaptive transformation. Indeed, one could easily argue that the key date was either 700 or 1300 CE. In the past, Arnold has argued with equal assertiveness that 1150 CE was the time of major socio-political and cultural change in the Santa Barbara Channel (Arnold 1992).

Finally, Arnold points to new studies in the Pacific that suggest that human settlement of remote Polynesia was later than previously thought making it too late to facilitate contact with North America during our chronological window. Indeed, a controversial study from Easter Island (Hunt & Lipo 2006) proposes that initial human settlement occurred only *c.* cal 1200 CE, nearly a millennium later than previously thought. For Hawaii, Kirch (2007:11) has revised his date for the earliest human appearance to "about" 800 CE, from his previous estimate of 500 CE (Kirch 2000:231). Kirch's use of the qualifier seems to reflect tacit recognition that these new shorter chronologies remain controversial and approximate. In an earlier and rigorous reassessment of the Hawaiian radiocarbon dates, Spriggs and Anderson (1993) rejected dozens of dates but accepted one with a two sigma range of cal 610-790 CE and a host of others with very wide two sigma ranges (*e.g.*, cal 110-1160 CE and cal 230-1010 CE). The cal 610-790 CE date

seems to establish the minimal window for the initial settlement of Hawaii, and it fits comfortably within our time range for contact in California of cal 400-800 CE.

One final note on the chronological issue is that we must also consider the possibility that California was encountered not by Polynesian voyagers who came from Hawaii, but rather during the voyages that led to the discovery of Hawaii, probably originating in the Marquesas. Using Irwin's (1992) model for the exploration and settlement of the Pacific, which remains the most viable and widely accepted, it must be assumed that discovery of the Hawaiian archipelago was preceded by countless unsuccessful voyages into the northeastern Pacific. While a voyage that resulted in a landing in southern California would have been long and complicated, the sweet potato's distribution in the Pacific is probably the result of equally arduous journeys. East Polynesian canoe voyages of 4,000 km have been documented recently, attesting to the skills of long-distance navigators (Collerson & Weisler 2007).

CANOE SIZE

Arnold finds the discrepancy in size between Gilbert Islands canoe (illustrated in Jones & Klar 2005) and a typical Chumash *tomolo* unsettling. A typical *tomolo*, she says, was "6 to 7 m in length, holding a 2-ton cargo or up to 12 passengers". (Arnold 2007:197) She notes that the Gilbert Islands sailing canoe was "17-23 m" in length. Here again, she has missed the point of our hypothesis, *i.e.*, that the Chumash borrowed only a technology from Polynesia, plank sewing. Her notion that the Chumash "mimicked the form of an Oceanic composite canoe" (Arnold 2007:196) is an outright misstatement of our position. Plank sewing *as a technique* could be, and was, used to make vessels of any size desired. We would draw Arnold's attention to "the oldest authentic Polynesian canoe in existence and a good example of "sewn" seams", as depicted in Dodd (1972:120). "It is 12-1/2 feet long", according to the accompanying description, and was brought back from the Tuamotus to England by Captain Wallis of the *Dolphin* in 1767. It resides now in the British Museum.

GENETICS

Finally, Arnold says that "the absence of genetic evidence for Polynesians also begs for explanation" (Arnold 2007:203). In reality, the absence of that information begs not for explanation, but for someone to undertake the collection and analysis necessary to find it, if it is there. For the Chumash, only mitochondrial DNA studies that trace the ancestral female contribution to contemporary lineages have been completed up until now (see Johnson & Lorenz 2006). Since the long-distance exploratory voyaging was done exclusively by men (there being taboos on women's association with voyaging canoes), Y-chromosome (male) lineages must be examined for evidence of prehistoric contact. However, it should not be assumed in any case that "absence of evidence is evidence of absence" in this instance. A Polynesian Y-chromosome

signature among the Chumash or Gabrielino implies both sexual contact resulting in male offspring and a surviving male lineage which passes on that marker. The first of these conditions would be subject to taboos against sexual intercourse practiced by Polynesians during the canoe-building process. The second condition is entirely at the whim of the tragic history of disease and genocide which reduced the Chumash population so rapidly and dramatically after European contact. If researchers find that a male Polynesian lineage existed and still survives among the Chumash and Gabrielino, it would corroborate our archaeological and linguistic case, but the latter does not depend on the genetics for validation.

FINDINGS FROM SOUTH AMERICA

Finally, we cannot argue for Polynesian contact *contra* Arnold without alluding to new finds from South America where a case for contact in what is now Chile was made at least as early as 1877 on the basis of similarities in circular shell fishhook styles and sewn-plank boat construction techniques (Lang 1877). Later, the chicken (Carter 1971) and Polynesian style basalt adzes (*toki*) were added to the list of items that were argued to be the result of borrowings from Polynesians (Ramírez 1990). The sweet potato of course, has an equally long history of debate as a likely product of contact (see Ballard, *et al.* 2005; Yen 1974), but sweet potatoes were not grown in the area of Chile where sewn-plank boats, circular shell hooks, and chickens existed. Like the case in California, the material evidence for Polynesian contact in Chile was never embraced by the scientific mainstream and there have been lingering doubts about the sweet potato as evidence for transoceanic diffusion, despite supporting linguistic evidence⁵, and the recovery of pre-contact sweet potato remains from archaeological contexts in the Pacific (Hather & Kirch 1991).

All this has changed dramatically with DNA and radiocarbon findings from chicken bones reported by Storey, *et al.* (2007) that demonstrate that chickens were brought to Chile from Polynesia before European contact. An even more recent analysis of mtDNA from modern chickens that attempts to discredit the Storey, *et al.*, research and invalidate the pre-contact radiocarbon dating (Gongora, *et al.* 2008) fails to disprove a Polynesian origin for the Chilean specimens and in so doing bolsters the original study (Storey, *et al.* 2008). The chicken bone findings indicates unequivocally that Polynesians made contact in southernmost South America, and further that the material similarities (sewn-plank boat construction, basalt adzes, and possibly circular shell fishhooks) previously argued to be evidence of contact were indeed just that. To this complex we can also add the same two-piece Polynesian style bone fishhook (see Bennett & Bird 1949:27E) that we see in California and an additional lexical referent to sewn-plank watercraft. There is also evidence of the harpoon that originated in South America but is found in numerous island groups of East Polynesia (Weisler & Green 2001:426). On the Swedish Magellanic Expedition of 1908, Carl

Skottsberg recorded the Alacalufan form *kiā-lu* for the “West Patagonian canoe”.⁶ (Cooper 1917:18) which is uncannily similar to the Hawaiian *kialoa* “long, light, and swift canoe”.⁷ (Pukui & Elbert 1986:146). The watercraft referred to here is the Alacalufan version of the *dalca* (pronounced [θalka] in Mapudungan⁸), the sewn-plank boat described by Heizer (1941, 1966) and others, and built by the indigenous group (the Chono) whose territory was immediately north of the Alacaluf.⁹ While no possible Polynesian etymology is immediately apparent for *dalca*, the resemblance of *kiā-lu* and *kialoa* cannot be dismissed lightly. As in the Chumash/Gabrielino situation, we have here in adjoining traditional territories separate linguistic groups constructing boats using the distinctive plank-sewing technique, and at least one of them seems to have a lexical referent of Polynesian origin.

Given that chicken DNA establishes that contact occurred in Chile, the principle of parsimony demands that sewn-plank boat construction, a linguistic referent to those boats, Polynesian style basalt adzes (*toki*) and two-piece bone fishhooks must also be recognized as elements of a complex that diffused from Polynesia to the mainland of southernmost South America before the arrival of Europeans. The sweet potato case, which includes linguistics and overwhelming material evidence, must also be recognized as equally undeniable in indicating a separate contact event in northern South America. Recent linguistic studies suggest this contact probably occurred in Ecuador (Scaglione 2005). With two areas of unequivocal contact in South America, how can the occurrence of the same technologies (sewn-plank boat construction and two piece bone fishhooks), and linguistic referents in two southern California languages represent anything other than diffusion via direct cultural contact?

DISCUSSION

In an attempt to discredit our hypothesis for a Polynesian contact event in southern California, Jeanne Arnold (2007) proposes a model based on the cascade theory of invention in which indigenous development of the sewn-plank canoe is viewed as having a monumental rippling effect throughout southern California over the millennium following its invention. The affects of the appearance of sewn-plank boat construction would have been the same regardless of whether the idea was developed independently or borrowed from others, but, as in South America, linguistics and material similarities tell us it was borrowed. Arnold ignores these similarities and the linguistics and presents as “facts” approximate and still-in-progress cultural chronologies to assert an alternative ethnic history in which plank sewing is seen as an independent Chumash invention.

Linguistics is an exceptionally important source of information on the past, and the prehistory of neither California nor the rest of the Pacific can be understood without the insight it provides. In California, the linguistic map tells us that the state was witness to a long series of migrations and population replacements, whereas in the Pacific, the strong similarity

among widespread Polynesian languages indicates a relatively rapid colonization by people who shared essentially the same cultural background. Understanding the unwritten past is such an extremely challenging undertaking that no information from any possible source (e.g., archaeology, oral tradition, linguistics, DNA) should be overlooked. Furthermore, collaboration among and between specialists in all of these areas is absolutely mandatory if we are ever to produce anything approaching true histories, rather than new myths.

NOTES

- ¹ “This saying refers to wrath over trifles”. (Green 1923:63) Laura S. Green collected 76 *Olelo noeau* “wise sayings” from Mrs. Mary Pukui.
- ² There is no evidence that the Purisimeño ever built sewn-plank canoes; their knowledge of them must have come from contact with coastal groups.
- ³ The other three stories are “Six’usus and Sumiwowo (I)”, in which “Thunder (whose name was Six’usus) ... shaped sand in the form of the boards of a canoe, with milkweed fiber and all, and then recited an incantation and turned it into a real canoe” (Blackburn 1975:106); “Coyote Visits the Sky”, in which Coyote is described in passing as “busily shaping boards for a canoe” (Blackburn 1975:195); and “Coyote and Qaq” (*qaq* “raven”), in which Coyote’s skill as a canoe builder is a plot device in this tale of his quest to get into Qaq’s bed. After a typical series of Coyote misadventures and misdeeds, the storyteller relates that, “...after he had climbed into the bed ... Coyote said, ‘I’m going to get into the canoe!’”
 “‘Go ahead’, said Qaq, ‘I’m ready for you!’
 “And so Coyote had intercourse with her, and now he had a home, now he was master of the house”. [Blackburn 1975:208]
 Though it may be an obvious metaphor in any maritime culture, it is at least entertaining to note that, in Hawaiian, *wa’a* “canoe” also means “trench, furrow, receptacle” and, figuratively “a woman”. (Pukui & Elbert 1986:375)
- ⁴ *Olo-lo-i-me-hani* means approximately “place where the waves make noise”. See the entries in Pukui and Elbert (1986:285, 286, 245) for *olo* “rub back and forth, grate, saw”; “to resound, sound long”, *oloolo* “reduplication of *olo*”, *olomehani* “dumping ground, refuse or rubbish dump [Rare]”, and *mehani* “smooth, curved”. Compare the etymology of Malibu: Chumashan *humaliwo* “place where [the surf] sounds loudly”. (Applegate 1975:29)
- ⁵ A recent, authoritative statement on the state of linguistic evidence for prehistoric contact is that of Adelaar and Muysken (2004) who say, “Although there have been many proponents of [trans-Pacific genetic] connections ... no valid arguments were brought forward to support them. The search for them, however, has shown at the least [emphasis added] two lexical items shared by Polynesian languages and languages in South America. One of them is the name of a plant domesticated in the New World, the sweet potato (*Ipomoea batatas*), Easter Island *kumara*, Hawaiian *’uala*, which is found as *k’umar* or *k’umara* in Quechua and Aymara. The second word is *toki*, Easter Island “stone axe”, Mapuche [= Mapudungan] “stone axe”, “military chief (the holder of the axe)”; compare also Yurumanguí *totoki* “axe” (Jijón y Caamaño 1945). Although the former case constitutes near proof of incidental contact between inhabitants of the Andean region and the South Pacific, the latter is not nearly as convincing but certainly deserves attention. Apparently, there were sporadic contacts that led to an occasional exchange of words, not to migrations of entire populations that could have brought along their languages” (Adelaar & Muysken 2004:41).
- ⁶ According to Cooper (1917:5) “The western canoe-using Fuegians are the Alacaluf. The name is “variously spelled” by different investigators. Cooper gives the following variants: Alacaluf, Alakaluf, Alacalouf, Alaculuf, Alaculoof, Alucáluf, Alukoeluf, Alooculoof, Alookooloop, Alukulup, Alokolup, Alikoolup, Alikaluf, Alikoolif. *Kawesqar* is the usual designation for the language today (see Adelaar & Muysken 2004).

⁷ This compound is formed of two widespread Polynesian bases, *tia “sew; stake, post”, and *loa “long” (Biggs & Clark 1994).

⁸ Mapudungan is the usual name of the language of the Mapuche people, although the two terms are often used interchangeably to denote the language. In this paper, we use Mapudungan for the language, and Mapuche for the people. To confound things further, the term Araucanian (as with the chicken) is another term for these people and their language. The post-contact nomenclature of native South America is even more various than that of California.

⁹ Heizer’s work seems to indicate the Chono as the builders of the *dalca*, but it is unfortunate that virtually nothing has survived of that language; *dalca* itself is a Mapudungan word. According to Adelaar and Muysken, the Chono were “[c]anoe nomads [who] had adopted a few Araucanian elements...: sporadic gardening (e.g., potatoes) and herding, the polished stone ax and the plank boat (*dalca*)”. Further, their language “was certainly distinct from Mapuche and Tehuelche, and more probably than not also from Kawesqar” (Adelaar & Muysken 2004:552). The evidence suggests that sewn-plank canoes were built throughout the western islands of Tierra del Fuego, but whether the Chono and even more southerly Alacaluf (Kawesqar) borrowed from the Mapuche is open to question, given the linguistic evidence.

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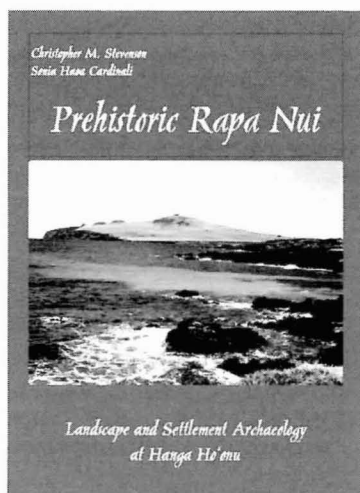
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PREHISTORIC RAPA NUI. LANDSCAPE AND SETTLEMENT ARCHAEOLOGY AT HANGA HO'ONU



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