COMMENTS

ON LINGUISTICS AND CASCADING INVENTIONS: A COMMENT ON ARNOLD'S DISMISSAL OF A POLYNESIAN CONTACT EVENT IN SOUTHERN CALIFORNIA

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In her recent article, "Credit Where Credit is Due: The History of the Chumash Oceangoing Plank Canoes," Jeanne Arnold questions our 2005 paper in which we suggested that a prehistoric contact event with Polynesians resulted in conveyance of the sewn-plank boat construction technique and a particular style of compound bone fishhook to the Chumash and Gabrielino of southern California. We agree with many of Arnold's views about the cascading effects of sewn-plank boat construction on Native societies of southern California, but question her dismissal of certain aspects of the empirical record, particularly the linguistics, in portraying this invention as strictly autochthonous. Here we recast aspects of the linguistic evidence that Arnold overlooks, provide evidence from oral history which she says is lacking, and discuss chronological issues that are much less straightforward than she suggests. We also mention implications of recent findings from South America. Finally, we submit that we have not discredited the Chumash or any other Native society in developing this hypothesis.

En su artículo reciente, "el Crédito Donde Acredita es Debido: La Historia del Chumash Canoas del mar de Tablón," Jeanne Arnold pregunta nuestro 2005 papel en el que sugerimos que un acontecimiento prehistórico de contacto con polinesios tenían como resultado transporte de la técnica de la construcción del barco de cosido-tablón y un estilo particular de anzuelo compuesto de hueso al Chumash y Gabrielino del sur de California. Concordamos con muchas de las vistas de Arnold acerca del cayendo en cascada los efectos de la construcción del barco de cosido-tablón en sociedades Nativas del sur de California, pero pregunta su despido de ciertos aspectos del registro empírico, especialmente la lingüística, a representar esta invención como estrictamente autóctono. Aquí refundimos los aspectos de la evidencia lingüística que Arnold deja pasar, proporcione la evidencia de la historia oral que ella dice falta, y discute los asuntos cronológicos que son mucho menos sincero que ella sugiere. Nosotros también mencionamos las implicaciones de conclusiones recientes de Sudamérica. Por último, nosotros nos sometemos que nosotros no hemos desacreditado el Chumash ni cualquier otra sociedad Nativa a desarrollar esta hipótesis.

In her recent article Jeanne Arnold (2007) has forcefully reasserted the prevailing wisdom that the sewn-plank canoe was a strictly autochthonous innovation in southern California that had cascading effects on the subsistence, exchange, and political organization of the Native Chumash. Arnold's paper is primarily a response to our proposal from two years earlier (Jones and Klar 2005; Klar and Jones 2005) that sewn-plank boat construction was introduced to the Chumash and Gabrielino via contact with Polynesian seafarers. Arnold clearly and succinctly summarizes the logical, empirical evidence for *in situ* development of the *tomolo* by the Chumash that has accumulated from decades of archaeological and ethnographic research on the mainland and islands of southern California. While the case for independent invention of the *tomolo* may seem to be so strong that few would question it, Arnold's argument is compelling only because she ignores the linguistic evidence that indicates a contact event between the Native societies of southern California and Polynesia. Here we challenge Arnold's interpretations under the premise that linguistic evidence is an essential component of the prehistoric record that cannot simply be dismissed. Indeed, our understanding of the prehistory of both western North America and the Pacific is intimately tied to insights

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gained from linguistic studies which go far beyond this one specific case. While we agree strongly with many of Arnold's assessments of events that transpired during the first millennium A.D. in the Santa Barbara Channel and their effects on historical trajectories thereafter, we feel that the linguistic record indicates that the ultimate cause of these developments was external contact. In light of the linguistic evidence we further submit that we have not discredited the Chumash or other Native societies of southern California; we have neither stated nor implied that they were incapable of developing this craft (and other related technologies) on their own. Rather, we presented what we believe is the most parsimonious explanation for all of the available empirical evidence, parts of which we review here in rebuttal to issues raised by Arnold. Specifically, we recast aspects of the linguistic evidence that Arnold overlooks, provide evidence from oral history that she says is lacking, and discuss chronological issues that are much less straightforward than she suggests. We conclude with reference to recent findings from South America that indicate almost unequivocally that Polynesians made landfall in the New World.

Arnold's Linguistic Objections

Before addressing Arnold's explicit linguistic objections, we note her implicit disapproval of our use of *tomolo* rather than *tomol*. From a comparative point of view, *tomolo* is the least-marked form among Chumashan dialects that preserve the form. The common use of *tomol* (without the final vowel) in scholarly discourse dates, as far as we can tell, only from the 1978 publication of Hudson et al., *Tomol: Chumash Watercraft as Described in the Ethnographic Notes of John P. Harrington*. Harrington obtained canoe information primarily from speakers of mainland Chumashan dialects, for whom *tomol* or *to'mol* was the local form of the word. Fernando Librado, Harrington's main consultant on sewn-plank canoes, knew some Island Chumash, but his everyday languages were Ventureño and Spanish. Early recordings of Island Chumash make it clear that *tomolo* (with the final vowel) was the original Island form. In his last and fullest article on New World plank-sewing, Robert Heizer (1966) used *tomolo* consistently. The Chumash Islanders continued to call their boat *tomolo* into modern times, as did the Purisimeño (with regular devoicing of the final vowel). The dropping of the final vowel was a late innovation among speakers of the southernmost of the Central Chumash dialects. As a pan-Southern Chumashan form, *tomolo* is the preferred technical appellation.

As for her explicit criticisms, 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). She clearly expects an affirmative reply from typical readers. But from a linguistic perspective, the answer is precisely the opposite. We have not suggested that Hawaiians or any other Polynesians stayed with the Chumash for any extended period of time. We have no idea of the duration of the contact, but 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,* or 'dug-out canoe' (literally 'worked piece of word') and *tomol 'ištapan,* or 'tule boat' (literally '*tomol* made of tules'). In 1878, Alphonse Pinart recorded a Purisimeño[1] form *šuašuax* 'boat' (Heizer 1952:45). This form is attested in no other Chumashan dialect (except in an island placename [*swaxïl* 'boat place'], q.v. Klar and Jones

2005:395), but there is little doubt that we can reconstitute the stem of this reduplicated form as **šwax-* or **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.[2] We propose that when the Chumash borrowed **tumuRaa'au* > **tomolo'o* from Central Eastern Polynesian[3] 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.[4] 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 and Jones 2005:397).

But this is not the most important problem with Arnold's objections. For whatever reason, she seems to have forgotten that we isolated not one but three words that we argue are of Central Eastern Polynesian origin. We find only the 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/taryanxa* from a base **talai* 'to adze, hew.' Thus, *ti'at* is '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, these are 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 points out:

> 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; this is perhaps the most important omission in Arnold's interpretation of our ideas. 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 somehow later learned it from the Chumash. If the latter were the case, we would expect to find some canonically correct forms of *tiat* and *tarayn(x)a* in Chumashan dialects, but we do not. The Chumash had (and have) their own words for the objects denominated by *t'iat* and *tarayn(x)a* in Gabrielino.

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, 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 acknowledged in Klar and Jones (2005) in which all of the details of the linguistic analysis are presented. That paper was peer-reviewed by specialists whose identities we do not know, but who (along with the known reviewers) 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 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 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 about the origin of a specific technique associated with its construction, and the origin of the lexical item itself. 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 **tumuRaa'au*). This fact argues for significant time depth, indicating that contact took place in prehistory, but it does not provide a specific date.

Myths and Oral History

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 presenting our argument in the 2005 papers we felt it would be unwise to refer to myths or oral history since the limitations of such sources could potentially detract from the hard evidence. In response to Arnold's challenge, however, we are more than willing to describe some of the many oral historical accounts from Polynesia and southern California that suggest inter-cultural contact.

In support of her statement above, Arnold refers to 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 collection includes only two stories that touch at all on the details of canoe-building, and neither deals with canoe origins (Blackburn 1975:195, 209–211). 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 sewn-plank 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 must be from these few lines that Arnold concludes that there is no reference to outsiders in Chumash lore on the origin of the *tomolo*. This 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. Development of the canoe *in toto* in this case is a figure that represents the acquisition of the technology by which plank canoes were distinguished from all other canoes.

But 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].

Although overlooked by Arnold, this story clearly shows that the Chumash origin myth for the plank canoe includes mention of others arriving from elsewhere. The distortion here concerns the obvious questions of who taught whom the technique and where the outsiders came from. 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 the pronoun "they" in "they saw also how a canoe was built..." refers to the Chumash themselves, not to the "men [who] arrived here from their own lands." At the very least, the pronoun reference is ambiguous. However, given the subsequent cascade of cultural developments that Arnold argues followed from perfection of the sewn-plank canoe construction method, 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 over a relatively short time period 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.[5] In the end, Chumash oral tradition is ambiguous, but it certainly is not devoid of possible references to outside contact.

Arnold also considered the oral history of only one southern California society, the Chumash. Polynesian oral histories, however, include a number of references to eastward voyages that encountered land including at least one unmistakable Native Hawaiian reference to a successful pre-contact, round-trip voyage to the New World. This account comes from the writings of Samuel Manaiakalani Kamakau, a well-respected native Hawaiian scholar of the nineteenth century, who collected and published research on Hawaiian history and traditions. According to the Bishop Museum's web page (http://www.bishopmuseum.org/press/authors.html), Kamakau's writings were originally published serially 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: "PUPU-HULUANA 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, and there are many other Polynesian myths that also allude to voyages to the New World (Dunis 2005). 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 suggesting that such events took place. Furthermore, in recognizing the oral traditions of both societies we denigrate neither of them.[6]

Chronological Issues

Arnold also challenges the Polynesian contact event on chronological grounds. Questions of chronology have already been raised in an earlier comment on our work from a Pacific specialist (see Anderson 2006; Jones and Klar 2006). Ultimately there are two phenomena whose dates are critical: the arrival of humans in Central Eastern and Eastern Polynesia, including Hawaii, and the appearance of sewn-plank boats and a Polynesian-style composite bone fishhook in the Santa Barbara Channel. Arnold feels that she can pinpoint the appearance of the sewn-plank canoe in the Santa Barbara Channel at cal A.D. 500, which is earlier than the most recent dating of the settlement of Eastern Polynesia, ca. cal A.D. 800. In point of fact, the chronological sequences that define the timing of these events in these two areas are neither perfect nor universally agreed upon, particularly in the Pacific where the chronology of human settlement has recently become highly contentious (see Flenley and Butler 2007; Hunt and Lipo 2006, 2007; Kirch 2007:11).

For California in our 2005 paper, we relied 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 colleagues (Arnold and 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 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 (see Munns and 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 and 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 ± 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 A.D. 400 and 800 that encompassed most of the phases during which artifacts associated with the *tomolo* (e.g. the stone drill and the two-piece, Polynesian style fishhook) seem to have appeared.

Arnold supports her view that the Chumash *tomolo* was invented in cal A.D. 500 by dismissing entirely the oldest direct date from a drilled canoe plank (cal A.D. 625–700; two sigma range) because of the "old wood" problem rather than acknowledging that use of a 100-year old redwood log to make the plank would equate to a date of cal A.D. 725–800. Because it is derived directly from a *tomolo* this date remains the best piece of evidence for the antiquity of sewn-plank craft 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 construction in the Santa Barbara Channel (Arnold and Bernard 2005; Bernard 2004). Pinpointing the precise time when pelagic fishing became important is far from straightforward, however. The remains of pelagic fish are uncommon in midden deposits in the Santa Barbara Channel, and their dating is highly imprecise. In Bernard's original study, pelagic fish remains appear in minute amounts sometime during the middle of the first millennium A.D. and increase incrementally into the 1300s (Bernard 2001). In that study, however, entire col-

lections 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 discussion of this chronology, Arnold acknowledges this imprecision stating, "the data demonstrate that albacore, yellowfin, and bluefin tuna began to be acquired in the A.D. 500–700 interval, and swordfish began to appear after A.D. 700 or so, although it was not until the A.D. 1300–1400s that swordfish became much more common" (Arnold 2007:201). In a revised version of the original study, Bernard (2004:31) aggregated data in more realistic 500-year increments which show an increase sometime during the cal A.D. 500–1000 period (Figure 1). Nothing in these studies points to cal A.D. 500 as the exact date for an adaptive transformation, rather the Santa Barbara Channel data suggest this happened sometime between cal A.D. 500 and 1000.

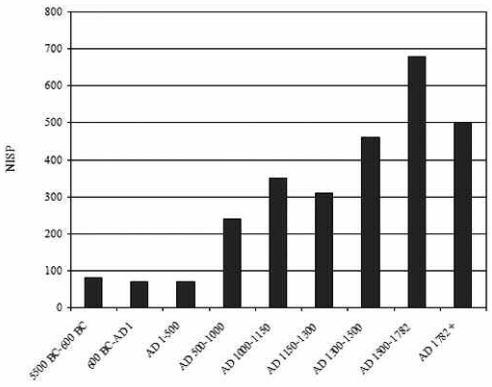

Figure 1. Maximum possible NISP of tomol-acquired species per period based on low resolution data (from Bernard 2004:32) showing that such species increased sometime between cal A.D. 500 and 1000.

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 new study from Easter Island proposes that initial human settlement occurred only ca. cal A.D. 1200, nearly a millennium later than previously thought. For Hawaii, Kirch (2007:11) has revised his date for the earliest human appearance to "about" A.D. 800, from his previous estimate of A.D. 500 (Kirch 2000:231). Kirch's use of the qualifier reflects 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 A.D. 610–790 and a host of others with very wide two sigma ranges (e.g., cal A.D. 110–1160 and cal A.D. 230–1010). The cal A.D. 610–790 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 A.D. 400–800.

New 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 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 (Ramirez 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 evidence,[7] and the recovery of pre-contact sweet potato remains from archaeological contexts in the Pacific (Hather and Kirch 1991). To this complex we can also add the same two-piece Polynesian style bone fishhook (see Bennett and Bird 1949:27E) that we see in California. It is also worth noting that recent attempts to argue for a natural introduction of the sweet potato into Polynesia from South America (e.g., Montenegro et al. 2008) ignore linguistic evidence in much the same way that Arnold does for California.

Of course, the case for contact in South America has been bolstered significantly by recent DNA findings (Storey et al. 2007) that demonstrate a Polynesian origin for chicken bones recovered from

Chile. AMS radiocarbon dating shows that the chicken arrived there before European contact. This study 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. An even more recent analysis of mtDNA from modern chickens (Gongora et al. 2008) does not disprove a Polynesian origin for the pre-contact Chilean specimens.

Given that chicken DNA establishes that contact occurred in Chile, the principle of parsimony demands that we recognize sewn-plank boat construction, Polynesian style basalt adzes (*toki*), and two-piece bone fishhooks as other 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 (Scaglion 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 challenge our hypothesis for a Polynesian contact event in southern California, Jeanne Arnold (2007) outlines an interpretive model based on the cascade theory of invention in which development of the sewn-plank canoe is viewed as having a rippling affect on Native subsistence and political systems during the millennium following its invention. We certainly agree with many of Arnold's views about the impacts of sewn-plank boat construction on Native societies of southern California, but are disturbed that she ignores certain aspects of the empirical record (e.g., linguistics, oral history, and striking material similarities) in order to portray this invention as a strictly autochthonous one. The effects of the development 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 indicate that it was borrowed. Furthermore, in acknowledging a prehistoric contact event we feel that we have not discredited any Native society. In our own conversations with Chumash and Tongva (Gabrielino) descendants, as well as with Native Hawaiians, few if any have expressed outrage at our proposal and several have told us that a prehistoric connection with Polynesia is something they have always known happened.

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Notes

1. There is no evidence that the Purisimeño ever built sewn-plank canoes during the historic period; their knowledge of them must have come from contact with coastal groups or as a cultural memory from a time when they were located closer to the coast.

2. The preservation of this evidence indicates just how serendipitous is the survival of any given linguistic form; languages are subject to processes and forces analogous to those which condition the survival of archaeological artifacts. The /s/ and /š/ variants are of no consequence here; their alternation is a common occurrence between various Chumashan dialects in the historic period, and almost certainly prehistorically as well.

3. The Polynesian bases we cite are taken from POLLEX (Biggs and Clark 1994), the standard reference on the proto-Polynesian lexicon. Central Eastern Polynesian was ancestral to the modern attested Polynesian languages Hawaiian, Marquesan, Tahitian, and Maori (including Rarotongan). In Jones and Klar (2005) and Klar and Jones (2005), we give the complete data set from which we derive CEP **tumuRaa'au* > proto-Southern Chumashan **tomolo'o*.

4. See Klar and Jones (2005:391–392) for examples of this type of metonymy; such semantic shifts are common in the languages of the world.

5. To our knowledge, no comparable corpus of Gabrielino tales has survived. Again, this is the serendipity of preservation.

6. There is ample scope for more work on the folk narratives of both the Chumash and the Polynesians. Hawaiian *oloolo i mehani* is composed of *oloolo* 'saw back and forth' and *mehani* 'smooth, curved' (Pukui and Elbert 1986:286, 245). In a Central Eastern Polynesian context, the cognates of *oloolo* mean 'rub, polish, saw' (i.e., action using a back and forth motion) (Tregear 1891:294). (Note that Hawaiian *oloolo* is Maori *orooro*.) This suggests the characteristics of an ideal place for landing boats—a place where the back and forth motion of waves has smoothed the beach—which is a logical way to describe a first landfall in an unknown territory.

7. 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,' [Mapudungun] '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 and Muysken 2004:41).

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