

ON OPEN MINDS AND MISSED MARKS: A RESPONSE TO ATHOLL ANDERSON

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While we appreciate Atholl Anderson's willingness to consider transoceanic diffusion as a viable possibility, he misrepresents parts of our argument and ignores others, particularly the linguistics that suggest that the Chumash and Gabrieliño borrowed the technique of sewn-plank construction and words related to that technique—not the word for boat or the specific design of a boat. The composite bone fishhook that appears in the Santa Barbara Channel ca. A.D. 700–900 matches simpler Hawaiian variants yet shows a significant stylistic departure from earlier southern California types. A chronological window of A.D. 400–800 for Polynesian contact is still consistent with realistic estimates for both the timing of the appearance of the sewn-plank boat technology in southern California and the initial settlement of Hawaii.

Mientras reconocemos la disposición de Atholl Anderson para considerar la difusión transoceánica como una posibilidad viable, él tergiversa partes de nuestro argumento e ignora otras, especialmente la lingüística, que sugiere que el Chumash y el Gabrieliño tomaron la técnica de construcción de tablón-cosido y palabras relacionadas a esa técnica, no la palabra para barco ni el diseño específico de un barco. El anzuelo compuesto de hueso que aparece en el Canal de Santa Barbara entre A.D. 700 y 900 es igual a variantes hawaianas más sencillas, aunque muestra un inicio estilístico significativo procedente de tipos locales. Una ventana cronológica entre A.D. 400 y 800 para el contacto polinesio es todavía coherente con estimaciones prácticas para el periodo de aparición de la tecnología del barco de tablón-cosido en California meridional, y en el asentamiento inicial de Hawaii.

We appreciate Atholl Anderson's challenges to our Polynesia–southern California hypothesis, especially his willingness to consider the topic of transoceanic contact with an open mind. As a specialist in Oceanic and southeastern Asian prehistory, Anderson seems more receptive to the possibility of long-distance oceanic contact than most Americanists. Written responses to our proposal from Oceanic scholars have been generally positive (e.g., Lee 2005; Nicolay 2005) while Americanists have been essentially dismissive (see Glassow et al. 2007), and seem determined to continue to assume *a priori* that any and all cultural developments in the New World can be readily explained as *in situ* adaptive responses to one or another ecological stimulus. This trend continues despite a number of remarkable cultural similarities documented between various societies on the western Pacific Rim and those

of the New World including, for example, Olmec and Chinese mortuary and other iconography (Meggers 2005¹; Needham and Lu 1974) and South American and Melanesian blowguns (Jett 1970). Few Americanist scholars are probably aware that Julian Steward, the father of cultural ecology, long ago acknowledged that South America had a number of precontact technologies and/or stylistic traits that are almost impossible to explain as anything other than artifacts of transoceanic contact (Steward and Faron 1959:277). Steward felt that such contacts were ultimately insignificant in the overall schema of South American prehistory, but he was open-minded enough to acknowledge the likelihood that they occurred. Of course, beginning with Heyerdahl, overly obsessed transoceanic theorists have demonstrated a continuing inability to sort out the compelling from the fanciful and have thus rendered any new transoceanic theories or evi-

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dence mute to the scientific mainstream. In our attempt to demonstrate prehistoric cultural contact between Polynesia and southern California, we revived a proposal that was first made over a century ago (e.g., Lang 1877) and that was taken very seriously by a number of highly respected California scholars during the first half of the twentieth century (e.g., Dixon 1934; Kroeber 1939; Olson 1930; Walker 1951). In the hope of avoiding both the pitfalls of the earlier cases and the questionable scholarship of some of the more recent transoceanic arguments, we assembled a combination of linguistic and material evidence, of which the former had not been previously recognized.

While Atholl Anderson is open to the idea of transoceanic diffusion in general, he feels that we missed the mark in attributing developments in the Chumash area to Polynesian seafarers rather than to voyagers from someplace else (e.g., southeast Asia), and he offers a number of linguistic, material, and chronological counters to our proposal. We appreciate both his open-mindedness and the fact that the issues he raises are so simple to rebut.

Similarity of Items

Anderson feels that the absence of the pan-Polynesian word for canoe, *waka*, from Chumashan languages represents a significant deficiency in the linguistic argument. He further suggests that eastern Polynesian sewn-plank boats have much less in common with the Chumash *tomolo* than they do sewn-plank craft from the east Asian mainland. Our response to both of these suggestions is simply that the Chumash and Gabrieliño borrowed the technique of sewn-plank construction and words related to that technique, not the word for boat or the specific design of a boat. The Chumash already had some form of reasonably effective watercraft prior to the development of the *tomolo* and they also had a perfectly good word for canoe (for additional details see Klar and Jones [2005]).

Archaeological evidence indicates that the Chumash or their predecessors were using boats as early as 11,000–12,000 calendar years ago to reach San Miguel (Erlandson et al. 1996) and Santa Rosa Islands (Johnson et al. 2002) off the southern California coast. To our knowledge, no one believes that these early craft were sewn-plank boats but

instead were probably dugouts, tule balsas, or some type of composite craft other than a *tomolo* (Cassidy et al. 2004). Contact with Polynesians provided the Chumash with a technique for constructing a more seaworthy vessel and for making more efficient use of a limited wood source (redwood drift logs). The Chumash and Gabrieliño borrowed from Polynesians not the word for boat but words referring to the new technique for creating wooden planks and sewing them together: **tomolo* 'of' from **tumu raa 'au* meaning source (of) wood, *tia* from **tia* ('to sew') and */tarayna/* from **talai/ + */na/* ('to carve, hew'). The linguistics, in our opinion, clearly indicate the technological complex that was borrowed and further point not to the east Asian mainland but to Polynesia as the source of both the technology and the words.

Anderson also suggests that any craft seaworthy enough to reach the North American mainland must have been of either multi-hull or outrigger construction, and that it would have included sails. He questions why the Chumash would have rejected one or all of these other technological improvements in the course of adopting other Polynesian innovations. As we stated in our original article, and cannot repeat strongly enough, the Chumash were exposed to sails as early as 1542 when the first Spanish seafarers landed on the Channel Islands. Unlike other indigenous groups of the New World who immediately added sails to their craft as soon as they were exposed to the innovation, the Chumash never embraced this technology despite repeated appearances of European sailing craft through the 1500s and early 1600s. It seems more than reasonable to assume that whatever factors led them to not emulate this innovation during the protohistoric period (e.g., lack of technological ability to copy the highly advanced double-hulled craft or absence of a raw material suitable for sails), were also in effect during the prehistoric era.

Anderson further questions the similarity between Chumash and Polynesian two-piece bone fishhooks, arguing that the former are quite simple compared to elaborate and highly stylized specimens from eastern Polynesia. While there is some truth to his statement, the Hawaiian record (Hiroa 1957:331) also shows clearly that composite hook styles ranged from the baroque to the relatively

simple, and that the latter approximate the type that appeared relatively suddenly in southern California. What we found additionally compelling was a switch to this style in the Santa Barbara Channel after 6,000–7,000 years of unchanged production of even simpler compound hooks made with straight, pointed bone pieces. The more elaborate Hawaiian-like forms are ascribed to King's Phase M5, dating ca. cal A.D. 900–1150, although there is no composite hook type at all ascribed to the immediately preceding phase, M4 (cal A.D. 700–900). The new hook style appeared only in the Santa Barbara Channel area and is not apparent among outlying Chumashan-speaking groups, which is consistent with the Channel being the point of cultural contact. Moreover, the function of these hooks was the same in both Oceania and southern California where they were actually used more as lures than as hooks, and were towed behind canoes in pursuit of large pelagic species (Anell 1955:152; Salls 1988:134). If the Chumash had no contact with Polynesians, this similarity in function has to be attributed to independent invention and convergent technological evolution. Anderson, like many American archaeologists, is apparently comfortable with the latter explanation, suggesting that the independent invention of the *tomolo* would have created new fishing opportunities that stimulated reshaping of fishhooks. The likelihood of this same style being created for the same purpose is absurdly low.

Chronological Questions

As proposed in the original article, we believe that Polynesians had direct cultural contact with Native groups of southern California sometime between cal A.D. 400 and 800. This window reflects a realistic assessment of the imprecision and uncertainties in both the California and Polynesia chronologies. A number of important developments are apparent in both areas within this approximate time span, but there is no firm basis for ascribing a more precise date to any of them—Anderson's assertions to the contrary. The most important issue, of course, is the timing of the appearance of sewn-plank boats in the Santa Barbara Channel, but evidence for assigning a precise date to this important event is neither abundant nor conclusive. The old-

est direct date from an unequivocal drilled redwood plank is cal. A.D. 625–700 (2 sigma probability) (Gamble 2002:308). While this assay is important as a direct date, it is also somewhat questionable because of the likelihood that it reflects influence of the "old wood effect," which would make it older than the age of the plank's construction for use in a boat. In lieu of additional direct dates, a number of proxies are used by Santa Barbara Channel archaeologists, none without problems. Most common is the trifacial stone canoe drill that first appears in King's (1990) Phase M3 (cal A.D. 400–700) (Munns and Arnold 2002:131). Gamble (2002:308) reported one considerably older date (ca. cal 2500 B.C.) from a possible trifacial drill, but expressed serious doubts about whether the artifact was in fact used for drilling wooden planks, and ultimately concluded that the oldest canoe drills with clear evidence of wood polish date to King's Phase M3. Likewise, asphaltum plugs associated with caulking and decoration of sewn-plank canoes appear no earlier than Phase M3 and more likely during M4 (King 1990). It should not be overlooked that with the exception of the questionable older drill, none of these proxy artifacts have been dated by direct association, but instead were all assigned to phases based on the chronological assessment of whole cemeteries or portions of cemeteries. While these age assessments can be considered generally accurate, their precision should not be overestimated either, given the limited number of dates used to define the phases and the uncertain procedures for calibration.

The other important proxy consists of the remains of swordfish, a species sacred to the historic Chumash that was pursued pelagically with sewn-plank canoes. A swordfish cape, recovered as a burial association from SBA-71 (Davenport et al. 1993:265), produced an uncalibrated date of 2040 ± 90 B.P. (Beta-5320) that was originally thought to contradict our minimal age estimate of cal A.D. 400 (see Edgar 2005). Recalibration of this date, however, brings it up to ca. cal A.D. 600, well within our ca. A.D. 400–800 window. The frequency of swordfish bones in middens has also been proposed as a marker of *tomolo* use, and the only systematic study of the remains of the taxon (Bernard 2001) shows a few bones appearing ca. cal A.D. 500, and a slow incremental increase during the second half

of the first millennium A.D. Arnold and Bernard (2005) subsequently asserted that the appearance of first swordfish bones provides a precise date for the initial appearance of the *tomolo* at cal A.D. 500. Remains of the species are never abundant, however, and the point at which their presence indicates exploitation via the *tomolo* is conjectural at best. As noted above, the Hawaiian-style compound hook associated with exploitation of pelagic species like swordfish is ascribed a minimum but somewhat uncertain initial date of cal A.D. 900. Overall, we see a complex of the sewn-plank boat technology, a style of compound hook, possibly carved wooden bowls, linguistic referents for some of these technologies, and related adaptive changes diffusing into California sometime between A.D. 400 and 800. There are no data that refute this window nor are there any that allow for a more precise chronological estimate although there is little besides Arnold and Bernard's poorly substantiated assertion that supports the first half of this period.

Anderson suggests that a date of cal A.D. 400–600 is too early for contact between eastern Polynesia and California based on increasing evidence for colonization of eastern Polynesia later than previously thought. This "consensus" is based on suites of recently obtained radiocarbon determinations that call into question the accuracy of earlier dates. Hunt and Lipo (2006), for example, feel they have clearly established the date of initial settlement of Rapa Nui at cal A.D. 1250–850 years later than the previously accepted date. While the process of dismissing nearly all dates acquired by researchers prior to the 1990s under the pretense of improved analytical rigor seems somewhat suspect to us, we are willing to accept revised estimates for the antiquity of human settlement of Hawaii. Among the few dates accepted by Spriggs and Anderson (1993) for the Hawaiian Islands is one from Maui (Beta-30860) 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 chronology of the initial settlement of Hawaii, and it fits comfortably within the cal A.D. 400–800 period of cultural change in southern California. Of course, in point of fact there is no consensus on the proposed short chronology for eastern Polynesia, and the southern California sequence is in drastic

need of confirmation with additional radiocarbon determinations.

Mechanisms of Transport

In addition to his advocacy for the short eastern Polynesian chronology, Anderson has recently been attempting to overturn what he refers to as the neo-traditional paradigm for the colonization of Polynesia that has been forged over the last two decades by Geoff Irwin and Ben Finney, among others. This model attributes discovery of the remote outposts of the Pacific to a program of intentional, into-the-wind, exploratory seafaring. In contrast, Anderson argues that Polynesian craft were not capable of sailing upwind to the degree that the intentional exploration model suggests, which has implications for their ability to reach the North American mainland. Based on his detailed analysis of the earliest historical accounts of Polynesian sailing craft, Anderson (2001) argues that Irwin and Finney overestimate the sailing capabilities of Polynesians. Irwin, Finney, and others suggest that the Polynesian sailing craft of the contact era were the end result of several centuries of devolution in boat design and sailing capabilities and that the craft used for the initial discovery and settlement of Hawaii and other remote outposts were superior to the contact-era vessels. Anderson counters that Polynesian sailing and watercraft design capabilities did not devolve, but actually continued to progress during the course of the early historic era. Projecting that trajectory back through time, he suggests that contact-era craft were the end-product of a long sequence of evolutionary progress and that the earliest craft could not sail upwind at all.

While Anderson's criticisms of the *Hokule'a* have some legitimacy in that the vessel combines all of the very best Polynesian innovations with some modern add-ons, his insistence that Pacific anthropologists essentially return to the Andrew Sharp (1957) model of Polynesian colonization by accident is fraught with problems and unlikely to attract many converts. The fact that Polynesians improved their vessels during the course of the historic era after exposure to European sailing vessels is a logical outcome of contact, but it has no bearing on the course of prehistoric developments. Devolution is a perfect explanation for the Hawaiian archaeological record that shows evidence for

return voyages to the Marquesas for several centuries after initial colonization and cessation of such voyages several centuries later. Once a viable population was established on the islands, the need for return voyages disappeared along with the technology used to facilitate them. The idea that Hawaiians or Marquesans did not have the technological or sailing capability to complete a voyage to the North American mainland between A.D. 400 and 800 is almost laughable given the size of the target and the fact that the distance from Hawaii to the Marquesas (3200 km) is basically the same as Hawaii to California (3360 km). In terms of winds and currents, both voyages would have been complicated, requiring at least one change of tack, yet we know that the passage from the Marquesas to Hawaii was made repeatedly.

Discussion

We find Anderson's willingness, on the one hand, to accept the possibility of trans-Pacific contact and, on the other, to insist that contact originated from Japan or the Asian mainland puzzling. He makes his case by presenting a very narrow interpretation of eastern Polynesian cultural chronology and by dismissing our linguistic evidence entirely. His statement that, "it is seldom difficult to find some words that have similar meanings between almost any two languages" has no applicability to the linguistic evidence that we've marshaled in support of a North American contact event. Additional details of the linguistics are available in Klar and Jones (2005), but no professional linguist (Polynesian or Native American specialist) to whom we've submitted our work for critique, or to whom it has been submitted for peer review, has suggested that the three forms (in two languages) we propose might be coincidentally similar. Our forms are not even remotely of the same nature as those that more speculative practitioners of the "mass comparison" method such as Mary Ritchie Key and Joseph Greenberg (but few other linguists) would accept as compelling. That being the case, the consensus is that the data we present demand an explanation, and all have been willing to admit that our hypothesis is a methodologically sound exploration of such. We see little in Anderson's claim that we've missed the mark, and continue to feel that the combination of linguistic and material evidence points

to a contact event sometime between A.D. 400 and 800.

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Note

1. In our reference to theories put forth by Betty Meggers in our original article we made a significant error when we suggested that the Valdivias Phase in coastal Ecuador dates to 500 B.C. The correct date is 3000 B.C. (Meggers et al. 1965:149–150).

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