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Comment on "Puvunga and Point Conception . . ." by Matthew A. Boxt and L. Mark Raab

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THIS article by Drs. Matthew A. Boxt and L. Mark Raab is presented as a comparative study of traditionalism. Their general discussion of the process repeats what has been said before by others, including the references they cite. What needs review and comment is the reliability of their two case studies which justify the article. I will leave a review of their Point Conception case to others.

It seems clear from how this article evolved that the basic subject is Boxt and Raab's views of the Puvunga issues. I find that their analysis is too flawed and superficial to be used in a comparative study and is misleading as a presentation of the issues. Therefore, my main purpose is to correct some errors and misrepresentations of data and to point out that they omitted important information that is contrary to their views. The rest of this comment is a summary of how the article evolved and the situation on campus which may account for errors.

Since 1993, Boxt and Raab have been expressing opinions about whether Puvunga, an ethnohistoric village, conforms to their conception of the nature of villages and also about the relevance of

archaeological data. They have challenged the evidence of its historic and religious significance and even its location.

Boxt and Raab began to express their opinions shortly after they started doing archaeological work on campus under contracts with California State University, Long Beach (CSULB). Some reports on their fieldwork have been prepared under contract but have not yet been released. However, in addition to the present article, two earlier documents became available. One is the immediate predecessor to this—an unpublished paper with a related theme (Boxt and Raab 1997). The other (Raab 1993) is a statement which was the source of data used by attorneys in public hearings and in litigation on behalf of the university. There are numerous errors and misinterpretations in the three documents. It is necessary to comment on the two earlier ones because Boxt and Raab include viewpoints in the present article which they discussed in more detail there.

Boxt and Raab make several errors regarding the campus location (pp. 46-47).¹ They say that the CSULB campus is "east" of the Rancho Los Alamitos Historic Ranch and Gardens. In fact, it is on the west side as their map shows. Their simple error lends support to their critics. Boxt and Raab are alone among scholars of reputation, as far as I know, in refusing to acknowledge Boscana's simple and widely recognized error in writing northeast instead of northwest in locating Puvunga. Boxt and Raab (pp. 53-54) use this in trying to cast doubt on the reliability of both Boscana and Harrington.

Only one source was cited by Boxt and Raab to support their view that Boscana did not make an error and that Puvunga, or another Puvunga, was located elsewhere. They cite "an account by [Kurtis] Lobo (1977), a Juaneño Indian descendant, [which] follows Boscana's description of the location of Puvunga, placing it in the Lake Elsinore region [to the northeast of San Juan Capistrano]" (p. 54). They do not further identify Lobo or his source of information. However, their phrase "follows Boscana's description" does leave the reader

with the impression that Lobo supports their view of Boscana as testimony derived independently from Juaneño tradition. However, that appears not to be the case.

Boxt and Raab quoted Lobo in an endnote to their unpublished paper (1997:37), claiming that Lobo "corroborates Boscana's original identification, placing Puvunga in the vicinity of Lake Elsinore." What they write in this article is paraphrased from that endnote, except that here they omit the Lobo quotation and do not mention the most essential information about it. That is, in the quoted passage, Lobo summarized part of the creation myth from Boscana's Chapter III and ended by mentioning Puvunga, " 'which Boscana describes as being about eight leagues northeast of the township of San Juan Capistrano.' " In that quoted passage, Lobo does not mention Lake Elsinore (nor, of course, did Boscana); rather, Boxt and Raab introduced it. Lobo was not independently "following" or "corroborating" Boscana, he was just citing Boscana as his source. Boxt and Raab do not say that Lobo disputed Harrington's correction or that he was even aware of it. It should be kept in mind that the 1933 edition of Boscana with Harrington's notes was a rare limited edition (Boscana 1933; Harrington 1933); the Malki Museum Press reproduction appeared in 1978, a year after Lobo's account. However, Lobo could have used a widely circulated paperback edition that was published in 1970 and included Boscana's error with no annotations (Robinson 1970:10).

Therefore, the information that they had included in their unpublished paper (Boxt and Raab 1997: 37), shows that Lobo's statement is not corroboration by an independent source of Juaneño traditional knowledge. Thus, I believe there is still no credible reason to doubt the evidence for the location of Puvunga on the Los Alamitos ranch or that Boscana made the same kind of careless error that Boxt and Raab themselves made in their present article.

Boxt and Raab make a number of avoidable errors in their discussion of the human burial on campus. First, the skeleton was not "near" site CA-

LAN-235, it was well within it as documented in the site survey sheet and map that Boxt and Raab had reviewed. There is no reason for Boxt and Raab to think the burial was "single, [and] apparently isolated" since there was no further excavation in that area of the site. The workmen found a complete skeleton, not "parts" of one. They collected only the bones that were dislodged from the trench sidewall. Those were later reburied in another location on campus at Native American request, and the rest are still *in situ*. Boxt and Raab have no cause to express doubt that the skeleton was that of a Native American because the "report did not draw such a conclusion" (p. 48). The so-called "report" was merely an inventory of the collected remains that was attached to the site survey form. The records showed that the burial was examined *in situ* by two experienced archaeologists and a physical anthropologist who were well aware of the relevant osteological characteristics and observed the midden context.

Boxt and Raab are vague in this article, but the basis of their doubts is in their earlier reports. Thus, in a sworn declaration on behalf of the university in a lawsuit, Raab (1993) stated, "I have determined that Dr. Dixon formed his opinion that the skeletal remains disinterred from LAN-235 in 1972 were Native American solely on the basis of tooth wear." Raab said he "determined" this from his "careful review of a memorandum summarizing an interview" with me on August 20, 1993, by "an archaeologist" working on campus with Boxt. In fact, there was no formal interview. I remember only brief, casual conversations with students who were working with Boxt when I visited his field project. For Raab to have drawn such an absurd conclusion from somebody's notes about how I formed my "opinion" without verifying it with me is interesting.

Raising additional doubt about the Native American identification, Raab (1993) went on to claim that ranch workers in the Mexican period "consumed a diet which contributed to rapid tooth wear, similar to tooth wear experienced by Native

Americans." Perhaps Raab can cite studies that demonstrate similar patterns of extreme tooth wear in the two populations.

In the declaration, Raab (1993) also said that, "Further, shovel shaped incisor teeth, indicative of Native American ancestry, were *not* noted in the Report" which was attached to the site survey form. Surely, it is unwise to assume that the trait was absent simply because it was not mentioned in the inventory. In fact, shovel-shaped incisors are noted in the coroner's report that was prepared by forensic osteologist, Dr. Judy Myers Suchey, in April 1979. Copies of the report were available to Raab from the administration, our department office, or from me, as well as from the county coroner's office. He was informed of the report and its contents in 1993.

Raab (1993) also claimed that "first-hand reports of Gabrielino Indian burial practices recorded by Father Boscana, and confirmed by archaeological evidence, indicate that the Gabrielino's [*sic*] cremated the dead"; and because the bones showed no sign of burning, "the condition of the skeleton weighs against Dr. Dixon's conclusion that the remains were that of a Native American." In fact, Boscana said both interment and cremation were practiced, and Harrington noted interment as far south as the Santa Ana River (Harrington 1933: 196-197; cf. references in McCawley 1996:157). Descriptions of burials without cremation attributable to the Gabrielino are common in archaeological reports and are part of Gabrielino tradition. (Some of the other evidence and references are summarized by McCawley [1996:157-158].)

Regardless of the burial's date, it does support the integrity of deposits in CA-LAN-235. The dates on midden shell that Boxt and Raab discuss (pp. 56-57) still need to be evaluated in terms of such factors as sampling strategies and their contexts. In any case, some kinds of ethnographic data and interpretations are not testable with archaeological evidence. There must be clear understanding of whether the radiocarbon dates, as well as other kinds of site analyses, are appropriate to the test

questions and of whether the test questions are appropriate to the research issues.

Boxt and Raab have misrepresented statements taken from their published sources. For example, contrary to their claim in this and their previous paper (Boxt and Raab 1997:1, 18, 22), Kroeber (1959) was not casting doubt on the location of Puvunga but rather was discussing the historicity of Wiyot (cf. Milliken et al. 1997:32-33). Also, their quotation from Meighan (p. 58) was taken out of the context where it had an entirely different purpose.

Boxt and Raab say that "Harrington assumed that Puvunga was a single, discrete location" and that he "never identified locations on the present-day campus as Puvunga or even hinted at such a possibility" (p. 55). However, they do not mention that Harrington only described what he saw around the ranch house; that he did not do a regional survey, and that he had no reason to speculate about village (*ranchería*) boundaries. Boxt and Raab create what they call a "Greater Puvunga model" (p. 55) that they claim was devised by me as "an entirely new archaeological twist on the Puvunga tradition"; instead, it reflects the ethnohistoric evidence for the nature of villages and the sociopolitical organization of the Gabrielino, as well as such related groups as the Juaneño and Luiseño (e.g., Altschul 1994; Earle and O'Neil 1994; McCawley 1996).

Boxt and Raab do not clearly distinguish "sites," LAN- numbers, and "villages." Villages (*rancherías*) are social units comprised of people whose most recoverable archaeological remains are the material evidence of domestic and community activities that vary in kind and intensity throughout their village territory. A village core may be most easily recognized in archaeological deposits by material remains that represent habitation areas of the elite class, sometimes including ceremonial spaces or cemeteries. The material evidence for the common folk and other activities may be labeled with such standard but imprecise archaeological terms as camps, work stations, or middens. Village com-

ponents need not be contiguous deposits. Their distribution, appearance, and contents are affected by such variable factors as seasonality, population size, activities, and time.

Boxt and Raab's use of a passage from Zahniser's (1974) report is misleading (p. 56). They quote his comments about his very limited excavation at the Rancho (LAN-306) and his not finding evidence to identify a Puvunga village (i.e., core) (Zahniser 1974:33). They omit the paragraph's concluding sentence: "The generalization I have offered above is my best guess, and not much more than that." Zahniser (1974:34-35) followed this with a discussion of what he found, in which he said, "The thorough disturbance of the deposits is quite clear in the distribution of historic materials throughout all areas and all levels of the site" (of his excavations); he then mentioned the causes of the mixing and scarcity of artifacts. Zahniser had changed the project's objectives and disregarded instructions, including those of the Rancho director, by choosing to excavate by levels through redeposited midden that had been moved onto a graded surface. Boxt and Raab's quotation from Zahniser's report is therefore irrelevant to their purpose. They did not use an excellent field investigation and literature study of LAN-306 by Milliken et al. (1997).

Boxt and Raab say their purpose is not to take a position on whether the burial on campus is Native American or on where Puvunga is located (pp. 48-49), but only to show that there are contradictions and ambiguities from which their colleagues and activist groups chose particular interpretations to foster their causes. However, they themselves have contributed to the confusion. Nothing in anthropology or history or life in general is certain, but when their errors and omissions are taken into account, I think that Boxt and Raab can be seen to have exaggerated their claims about ambiguities in the evidence, conflicts among the sources, and other people's biases.

In addition, there are minor errors in their quotations from published sources, omission of important

references, incomplete reference citations, and the like. At this point, I will not further discuss Boxt and Raab's other errors, in part for lack of space in this short comment, but also because it seems more important here to describe the history of their papers and the situation from which they evolved.

Much of the Puvunga portion of this article is recast from their paper, "National Register Site or Oversight: The Authenticity of the Bellflower Parcel, A Cautionary Tale for Contemporary Archaeology" (Boxt and Raab 1997). The present article cannot be understood without reference to that paper, which makes the space limitation of the *Journal* for comments even more of a problem. The paper is still unpublished but copies are available. Boxt and Raab gave a copy to the library of the Rancho Los Alamitos Historic Ranch and Gardens for research purposes. The letter of transmittal from Boxt (dated August 12, 1997) authorized it to be cited as "Pacific Coast Archaeological Society Quarterly (in press)." The theme of that paper is partly about the standards and criteria for entering sites on the National Register of Historic Places (NRHP), which "groups" take as proof of objective historical reality. Boxt and Raab mistakenly assumed that acceptance on the NRHP is what inspired contemporary Native Americans to claim the sites as sacred in their belief system. Boxt and Raab (1997) are concerned that "the current tendency to regard ethnographic data as more 'real' than archaeological findings is not only unjustified but leads to the creation of popular myths rather than demonstrably accurate understandings of the past." The point is not new, and in any case their analysis of Puvunga ethnohistoric sources, archaeological data, and recent events is too incomplete to qualify as a case study. Nevertheless, certain campus administrators are still using Boxt and Raab's views to support their quarter-century-old hope of removing the campus Puvunga sites from the NRHP.

For more than 25 years, campus administrators seem to have acted under the mistaken belief that archaeological sites prevent development and have

wished that both the sites and those concerned about their conservation would go away. In 1973, when I first brought the administration's attention to the California Environmental Quality Act in relation to archaeological resources, the first reactions from administrators were denial that there were any remains and the claim that CEQA did not apply to the university. When we finally overcame that, the administrators still did not have interest in the resources, but they did understand the need to have their paperwork done to show compliance with regulations.

As I experienced administrators' reactions over the years, I believe they evolved three main ideas (Dixon 1977). The earliest was to dig up sites by having our faculty use them for teaching archaeological field techniques to students. We all refused on ethical grounds and the obvious conflict of interest (the years of pressure on our department chairs and on our faculty archaeologists is too long a story to tell here). Then, the idea was to have the sites dug as legal salvage "mitigation" by contract archaeologists. Some of this has been done, and I am not aware of any serious efforts to incorporate resource protection into the initial project design processes.

The administrators also undervalued the significance of sites for research by claiming they are no longer "intact" because of early agriculture and later grading and landscaping. Another approach has been to challenge the importance of sites for the community, including Native Americans. One argument was that the archaeological tests of the midden deposits did not turn up what the administrators consider to be the material evidence for a village; a second argument was that Puvunga was not anywhere on this campus or even in this region because the ethnohistoric and historical information was wrong or not "scientifically" proved. When I and others would submit critical reviews of the administrators' claims and the contract reports they chose to rely on, the administrators would summarily dismiss contrary professional input as mere "opinions." As long as legal conditions appeared to be

met, administrators had no interest in critics, no matter how thoroughly facts were documented and carefully interpreted. There were some peer reviewers who apparently met on campus several times, but I have no information about what they were told or what documents they were given. Ordinarily, one should expect a university to encourage and give respectful consideration to the contributions of individuals and organizations who have relevant knowledge or are affected by administrative decisions. However, in this case, I believe that administrators have felt it expedient to ignore, discredit, and isolate potential critics, leading to error, resource losses, and even drifting into costly litigation as a result.

When Boxt and Raab began contract work on campus, this is the situation they met. I believe that the administrators' views are what first focused their interest on the themes in their present article and its predecessors (Raab 1993; Boxt and Raab 1997). I also think there is reason to believe that the administrators interfered by imposing or implying certain constraints on their work so that Boxt and Raab did not consult with knowledgeable people. The following is the situation as I know it.

I made a number of attempts to contact Raab. In December 1993, I sent him a detailed commentary on his statements about campus archaeology in his August 1993 sworn declaration that the university's attorneys had used in a legal proceeding (Raab 1993). He did not respond. Most of the same errors in the declaration persist in their 1997 and present papers.

I talked with Boxt and visited his first excavations on campus several times in 1993. He was very pleasant and informative. I offered my full cooperation and copies of all background documents. Soon after, his manner changed, and he did not respond to my request for a meeting. On December 23, 1993, I wrote again to both Boxt and Raab, asking why they had broken off contact. Raab did not reply, but in a letter dated January 13, 1994, Boxt wrote:

... [it was] with the best interests of all concerned. As you know, in this politically charged atmosphere, normal interpersonal relations are very often compromised and strained. I offer this as explanation and not excuse. Please keep this in mind for any problems of this sort as they may arise in the future.

I responded to express my concern about professional matters. There have been no further communications, but I think it was courteous of Boxt to have replied with frankness about the situation.

This account has to be personal because I can only document my own experience, but it is my understanding that Boxt and Raab also avoided other university faculty and Native Americans who were out of favor with the administrators. If Boxt and Raab received instructions or perceived a constraint, it would put them in a difficult position. Whatever the explanation may be, the irony is that the errors in this and their other papers could easily have been avoided through normal interaction with colleagues and other concerned people despite the "politically charged atmosphere."

This all bears directly on Boxt and Raab's concluding paragraph. Regarding the influence of anthropological research on Puvunga issues, they exaggerate its importance to Native Americans. Contrary to their statement, it is certainly not a "fact" that "widely held understandings of Puvunga are almost entirely a product of anthropological scholarship" (p. 63); the "understandings" preceded Harrington's and other anthropologists' information and interpretations. They also assumed that in recent years Native Americans have relied on archaeologists' interpretations of the local sites and on their NRHP status for "authentication." Native Americans and others have cited some of what Boxt and Raab call the "anthropological component of ethnogenesis" (p. 45), but it had only a small, nonessential role in the growth of local "traditionalism" on campus and elsewhere in the region. Native Americans and others in the community gained the strong support of anthropologists who agreed they were not being given a fair hearing. Due to incomplete research and analysis, Boxt and Raab underestimate

both the knowledge and the social, political, and economic processes that were already underway among local Native Americans independently of anthropologists.

The problem with this article is Boxt and Raab's analysis of the Puvunga case, not their review of the well-known general issues concerning the uses or misuses of anthropological interpretations in ethnogenesis. Boxt and Raab's caution to their colleagues in their final sentence might better have ended by recommending "... more thorough analysis than we were able to do here."

In their rebuttal, Boxt and Raab will have the last word because the *Journal* does not provide for replies to rebuttals. However, as responsible scholars, I think Boxt and Raab may now recognize the problems that arose from working under awkward circumstances. With regard to the campus sites and their views on Puvunga, I hope they now may understand that they have presented errors of fact and interpretations that warrant reconsideration.

Boxt and Raab are competent and experienced researchers. I readily acknowledge that they have had a particularly difficult dilemma to resolve. I hope they regret by now that they did not consult individuals who are sources of information that they knew to exist. I believe they must have become aware by now that other sources of published and unpublished information were available to them which they have not yet used. And I hope that they also will want to revise misinterpretations of some of the published and unpublished sources that they did use.

I hope the *Journal* will invite publication of an adequate follow-up to Boxt and Raab's article in the future in order that knowledgeable people can analyze the kinds of issues that they brought up about local archaeology and ethnohistory in order to prepare the level of response that is not possible in a brief comment. However, Boxt and Raab could use their rebuttal now to make that unnecessary.

I hope their response will be to disaffirm this and their previous paper (Boxt and Raab 1997) as

incomplete for reasons beyond their control, and then perhaps express their intention to consider preparing an independent study of the Puvunga issues by taking advantage of all the information that is available to them.²

NOTES

1. The campus is not in the "Los Altos community" of Long Beach; the Los Altos area is north of campus. The Rancho Los Alamitos Historic Ranch and Gardens is not a "park," it is a historic site with house, barns, and formal gardens.

2. If further comment is needed, I will place it on my website (<http://www.csulb.edu/~kdixon>).

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Comments on "Puvunga and Point Conception: A Comparative Study of Southern California Indian Traditionalism," by Matthew A. Boxt and L. Mark Raab

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THE provocative article by Matthew A. Boxt and L. Mark Raab is guaranteed to engender controversy because it raises questions about sacred sites, native identity politics, and the competence of archaeological research in an increasingly complex and emotionally charged world. Their discussion of the sacred village of Puvunga and its identification with specific archaeological sites, in combination with the recent paper by Haley and Wilcoxon (1997) on the emergence of the Western Gate as a sacred place among Chumash Traditionalists, provides much food for thought. Recognizing the complicated and impassioned nature of events that underlie both the Puvunga and Western Gate episodes, I applaud them for raising issues about the participation of anthropologists and archaeologists in the genesis of California Indian Traditionalism. I believe it is very timely to begin an open and frank discussion about the practice of California archaeology today, its

broader social and political implications, and its relationship to native peoples.

In commenting on the Boxt and Raab article, the primary point I raise here concerns the competence of contemporary archaeological research. I think it is naive to think that we can practice a totally objective archaeology that is divorced from the social concerns, political pressures, and funding constraints of today. Archaeological research is conducted for a variety of reasons and for divergent clients and funding agencies. Collaboration with involved stakeholders, especially native peoples who have a vested interest in the archaeological record, will continue to increase. I have no problem with archaeologists working closely with native groups to identify sacred sites or places, to assist them in becoming federally recognized, to develop strong and legitimate claims for the repatriation of culturally affiliated skeletal remains, associated funerary objects, and sacred objects, or to help them negotiate or promote their native identities to the broader public. My problem is with poor, sloppy, and/or inexcusable archaeological research.

A very significant point raised by Boxt and Raab is that archaeological research cannot be conducted in a hasty, arbitrary, or uncritical manner given the ultimate implications it may have for contributing to the politics of development/open space, for "authenticating" ethnic identities and histories, and for generating public perceptions of the past. Their article highlights a problem that is becoming increasingly common in studies of the past undertaken by archaeologists. In this day of postprocessual archaeology, little emphasis is placed on the development or refinement of formal methodologies that can be employed to construct interpretations of the past. While multiple "stories" are celebrated, very little attention is actually devoted to generating alternative interpretations and to evaluating competing scenarios in a critical manner. Not all interpretations of the past are equally valid and, as exemplified by the Puvunga case, archaeological research should involve the rigorous assessment of viable alternatives.