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

KENT G. LIGHTFOOT
Dept. of Anthropology, Archaeological Research Facility, Univ. of California, Berkeley, CA 94720-3710.

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.
The search for Puvunga underscores current problems that characterize studies of the past employing multiple lines of evidence. Although archaeological remains, written sources, and ethnographic data were considered in the Puvunga case, it is clear that the ethnographic findings of John P. Harrington were privileged over all other sources of information, including Fray Gerónimo Boscana’s original written account on the Chinchechinich rituals and beliefs. Boxt and Raab discuss the implications that result from uncritically accepting ethnographic information over other types of data. In the remainder of my comments, I follow on their discussion and focus on the problematic relationship between archaeology and ethnography as a case for why we need to revisit our methodological approaches for constructing interpretations of the past.

From the outset, it must be stressed that Harrington’s field work in the Puvunga case involved the transcription of oral histories and oral traditions as told by native elders, and not participant observer ethnographic accounts of a functioning native community. This kind of ethnography, known as “memory culture” or “remembered ethnography,” presents its own unique strengths and challenges to the study of the past. Oral histories usually refer to stories told by individuals that concern events that happened in their lifetime, while oral tradition is defined by narratives that have been transmitted by word of mouth beyond the present generation (see Vansina 1985:12-13, 27-30). There is an extensive scholarship on the use of oral histories and oral traditions in anthropology, folklore, and history, and it is commonly recognized that these sources can provide a unique perspective on history by providing an “insider’s view” to the past, a window for contemplating the worldviews, myths, ideological constructs, social relationships, and daily practices of past peoples (e.g., Dundes 1980; Erdoes and Ortíz 1984; Vansina 1985; Lummis 1992; Williamson and Farrer 1992; Finnegan 1996).

Oral traditions have long been employed in archaeological research (e.g., the fieldwork of Fewkes, Cushing, and Bandelier in the Southwest during the late 1800s), although with the rise of “processual” archaeology in the 1960s and 1970s, there was a tendency to downplay their usefulness in the study of prehistory. However, this is rapidly changing as archaeologists increasingly collaborate with native groups, participate in the NAGPRA process, and recognize the important insights that can arise from oral narratives passed by word of mouth from one generation to another. I think oral narratives have taken on new meaning and relevance as native groups play increasingly more powerful roles in the politics of archaeology. I do not think the Puvunga or the Western Gate examples are isolated cases, but rather represent a growing trend in which native oral traditions are taking precedence over archaeology and other sources for interpreting the past. What is clearly needed now is a more balanced perspective that does not privilege one data source over another. Recognition of the importance of native oral traditions should not diminish the potentially significant contributions that ethnographic reports, ethnohistorical accounts, historical linguistics, biological interpretations, and archaeological data can play in studies of the past. Each data set is characterized by its own distinctive strengths and weaknesses in constructing interpretations of the past.

The many challenges of using oral traditions to reconstruct the past are outlined in detail elsewhere (see especially Vansina 1985). However, the current consensus among scholars who study oral traditions is that they cannot be taken at face value, even though this can create considerable tension between specialists and stakeholders who endow their ancestors’ words with politically and personally entrenched values (Finnegan 1996: 888-889). In reality, oral traditions are often representations of the past as told in the present, and as contemporary remembrances they are subject to a variety of processes (memory loss, social and political reorientations, revaluations of the past) that can modify, update, and restructure oral accounts over time (Vansina 1985:xii, 186-196). Furthermore, most societies are characterized by diverse types of oral
narratives (myths, legends, folk tales, proverbs, songs, prayers, genealogies, etc.) that may or may not be linked with "real" events of the past. Vansina (1985:159-160, 196) cautioned that oral traditions should be treated only as hypotheses for reconstructing the past until they have been fully checked by independent sources of data.

The field work on "remembered ethnography" as undertaken by Harrington and many other California anthropologists represents a complex relationship to the past. Interviews with native elders involved at least two "authors"—the elder recounting the story and the researcher transcribing it (in addition to the many other "authors" who passed the stories down from one generation to the next). Multiple problems can result in the process of transcribing the story to English. For example, was the story really understood by the researcher? Did the story lose some of its meaning in the translation from the oral native language to written English? In recent years, researchers have emphasized the importance of performance and storytelling in understanding the meanings of oral narratives, and that understanding the social context in which a story is told is very critical (Bauman 1986:1-12; Cruikshank 1990:14-16; Finnegan 1996:890-891). Ethnographic fieldwork in California in the early part of this century tended not to record this kind of contextual data that is so critical for generating interpretations today.

My candid discussion of oral traditions is not meant to dissuade archaeologists from employing these data in their work. Rather, I am stressing the critical use of oral tradition in a holistic anthropological approach that employs multiple lines of evidence in the construction of interpretations. Hypotheses generated from oral traditions can be evaluated using archaeological information, historical accounts, and other sources of data, with the convergence of different evidential lines supporting, refuting, or modifying proposed interpretations. Farris (1989) demonstrated the utility of this approach in his analysis of two Kashaya Pomo stories told by Herman James and Essie Parrish in 1958 that were originally translated and transcribed by Robert Os-}

walt. Farris was able to demonstrate that the stories pertained to the passing of a large company of Hudson's Bay trappers near Fort Ross in 1833 by carefully triangulating the oral narratives with written accounts of the Hudson's Bay expedition in northern California.

In the Puvunga village case, it is important to employ a similar kind of rigorous methodology to evaluate the hypothesis presented by Harrington. Harrington's hypothesis appears to be based primarily on two informants' (Kewen and Acú) accounts of where Puvunga was located. Since the interviews with Harrington took place in the early part of this century, and the rise of the Chinigichinich rituals and beliefs date back to at least the late 1700s, the information provided by the informants appears to be oral tradition passed down from previous generations. Vansina (1985:147-160) cautioned that two different versions of the same story should not be viewed as independent sources when the storytellers hail from the same community and "memory pool." Since Kewen and Acú were both associated with the Mission San Juan Capistrano community, the stories certainly reflect where this native community believed Puvunga was located in the early decades of this century. But the convergence of their stories does not constitute adequate proof alone, since Kewen's and Acú's oral accounts provided Harrington with his hypothesis in the first place. This situation presents a perfect opportunity to triangulate their perspectives of the past with available written accounts (such as Boscana's) and the archaeological record. For example, does CA-LAN-306 in the Rancho Los Alamitos Historic Ranch and Gardens represent the remains of a large village community dating to the late 1700s?

In conclusion, I think the Boxt and Raab article points out methodological weaknesses that characterize archaeology today, especially in the use of multiple lines of evidence. The critical issue for me is not why the archaeological research is undertaken in the first place. I think working with native groups on sacred sites or demonstrating their long-
term occupation of a place is a very valid enterprise. My concern is whether a rigorous methodology is employed in the research process—one that considers multiple lines of evidence and evaluates alternative interpretations in a critical manner. In the long run, no one is served by poor research methods and weakly argued interpretations. The true test of any interpretation is whether other scholars, native peoples, and interested lay people support the conclusions based on the arguments and evidence presented. This is one reason why I think peer-reviewed research is so important today. Of course, interpretations are always subject to change with the addition of new information. Ultimately, I think the jury on Puvunga is still out.

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

EUGENE E. RUYLE
Dept. of Anthropology, California State Univ., Long Beach, 1250 Bellflower Blvd., Long Beach, CA 90840-1003.

The central point of Boxt and Raab’s article lies in their assertion that “widely held understandings of Puvunga are almost entirely a product of anthropological scholarship. This fact is rarely acknowledged” (p. 63). They claim that the “exact location of this community and its archaeological remains were unknown until J. P. Harrington announced his discovery to the academic world 60 years ago: Puvunga had been located” (p. 51). This is nonsense.

BOX AND RAAB IGNORE INDIAN ORAL TRADITIONS

First of all, to the extent that Harrington “discovered” Puvunga, he did so by talking to Indians. Boxt and Raab give no indication that they did the same. Boxt and Raab do thank their Indian moni-
tors "for their interest in our work, as well as for graciously sharing with us their perspectives regarding Puvunga and Gabrieleno identity" (p. 63), as if to imply that they actually discussed their views with living Indians. This is problematic.

I spoke to Anthony Morales, Chief of the Gabrieleno/Tongva Tribal Council, who was "deeply disturbed" by the article and the implication that he or other members of the council were consulted in any way about Boxt and Raab's views on Puvunga and Gabrieleno identity. After reading the article, he described it as a "bunch of hogwash" and "demeaning" to Indians. He further stated that he was never interviewed by either Boxt or Raab on his perspectives regarding Puvunga and Gabrieleno identity. As far as Morales knows, neither Boxt nor Raab ever presented the views expressed in this article to the Tribal Council. Morales (personal communication, 1999) expressed his view that this article is "just a continuation of the Native American holocaust in modern times, based on the European mentality of trying to make the Native American extinct."

Boxt and Raab fail to examine other sources which might indicate that Indian knowledge of Puvunga is derived from their own oral traditions. They do not cite the ethnohistorical research commissioned by California State University, Long Beach (CSULB) on Puvunga (Altschul 1994). They do not consider the oral testimony given to California's Native American Heritage Commission in public meetings on June 18 and December 3, 1993. (The American Anthropological Association has supported the Heritage Commission on Puvunga; see Overbey [1994].) They do not consider the sworn testimony provided by over a dozen Indian plaintiffs in the Puvunga lawsuit. (The case has twice been appealed to the California Supreme Court, which supported the Indians, not the University; see Ruyle [1995a]; Skomal [1997].)

If Boxt and Raab had discussed their views with living Indians, they may have heard what I heard from Chumash friends. In discussing Haley and Wilcoxon's (1997) article on Point Conception, I was told that the Indian elders decided to talk to Harrington so that their traditions would be written down and available when future generations of Indians needed them. The Indians, in other words, were using Harrington to ensure that their traditions would not be lost.

**BOXT AND RAAB IGNORE LONG BEACH'S EARLIEST HistorIAN**

The fact of the matter is that there has never been any mystery about the location of Puvunga. It was common knowledge in Long Beach decades before Harrington arrived. Before she died in 1918, Long Beach's first historian, Jane Elizabeth Harnett, wrote that

[The Indians of San Juan Capistrano, according to Father Boscana, "emigrated from a place called Sejat, distant northeast from the mission seven or eight leagues, and in the midst of a valley, now known by the name of El Rancho Los Nietos." "Originally," he says, "the inhabitants were numerous, but the success and influence of a holy conquest gradually eradicated their attachment of Sejat and it finally became subject to the spiritual as well as temporal administration of the ecclesiastical mission." Now it is very evident that in describing the location of Sejat Father Boscana has made a slight mistake, for, if it were on Rancho Los Nietos, it must have been northwest of San Juan Capistrano and not northeast, as he declares. This point is rather important because he describes another Indian village, Pubuna, about which center all the legends of the San Juan Indians concerning the origin of their god, Quaquar, as being also "distant from San Juan Capistrano, northeast about 8 leagues." Now, Hugo Reid, writing 22 years later, tells us that at that time, there still existed on Rancho Los Alamitos (one of the subdivisions of Rancho Los Nietos), an Indian rancheria named Pubugna.

In view, first, of Father Boscana's manifest error in stating the direction of Sejat; second, of the fact that Rancho Los Nietos was distant about eight leagues northwest from San Juan; and, last, of the great similarity of the names Pubugna and Pubuna, it seems reasonable to believe that the two (Reid's Pubugna and Boscana's Pubuna) are identical, in which case we may claim for this vicinity the most interesting of all the rancherías between the mountains and the sea. Whether this ranchería can be identified with the undoubted traces of an Indian
village at the head of Alamitos Bay, is, of course, quite another matter. At present, we have no means of knowing whether this village was one which had existed during the Spanish period, or whether it was one of those many more ancient sites which seem to have been abandoned many years, perhaps centuries, before the Spanish came. The finding of pestles of a type not used by Indians of the later days would seem to point to the latter conclusion. So far, too, nothing of undoubted later origin, such as bits of cloth, beads or iron, which would go to show that the site was occupied in Spanish days, has been discovered. The whole of Rancho Los Alamitos has proved so rich in Indian relics that some other location on the ranch may easily have been the site of Pueblo. Then too, by the time at which Reid wrote, the Indians had adopted the Spanish custom of giving a single name to a whole tract of land on which there might be several villages. Pueblo, a name which undoubtedly designated originally a single rancheria, was, in Reid’s time, used to denote all the Indian settlements on the Alamitos Ranch [Case 1927:26-27].

So, we have Reid in the 1850s (Heizer 1968), Harnett sometime before 1918 (see Case 1927), Krooher (1925); Harrington (1933), Dixon (1973), and Scientific Resource Surveys (1980)—all pointing to the vicinity around the CSULB campus and the present Rancho Los Alamitos as the location of Pueblo. This view is found not only in archaeological sources (Krooher 1925; Bean and Vane 1978), but also in standard histories of Long Beach and the surrounding area (Quinn 1973; Meyer and Kalayjian 1983; Queenam 1986; DeAtley 1988), histories of Los Alamitos (Robinson 1966; Salzer 1974), general books on the Gabrieleno (Johnston 1962), as well as the recent authoritative work by McCawley (1996).

Clearly, anthropologists have played a role in the preservation of Indian traditions, and quite appropriately so. To discuss this role without also examining Indian oral traditions and the writings of historians does a disservice to anthropology.

**BOXT AND RAAB’S ERROR IN THE LOCATION OF CSULB CAMPUS**

Harnett (see Case 1927) corrected Boscana’s “manifest error” in the location of Pueblo (it is northwest of San Juan Capistrano, not northeast as Boscana wrote) decades before Harrington offered what Boxt and Raab describe (p. 53) as his “something convoluted refutation of Boscana’s directions.” It is amusing that Boxt and Raab make an error similar to Boscana’s in their discussion of the CSULB campus. They write with misleading precision that “The 319-acre (127.6-ha.) campus of CSULB is situated less than one-half mile (0.81 km.) east of the park [Rancho Los Alamitos Historic Ranch and Gardens-ER] in the Los Altos community of the city of Long Beach, Los Angeles County, about 20 miles (32.6 km.) southeast of downtown Los Angeles” (pp. 46-47).

A glance at Boxt and Raab’s Figure 2 will show that the CSULB campus is located to the west, not east, of Rancho Los Alamitos. Further, a call to any realtor would confirm that the campus is not in Los Altos but just south of it. (The real estate map of Long Beach shows the campus to be in College Park. To the extent that there is a boundary, it runs along Atherton Street on the northern edge of campus.)

So here we have two Ph.D.s in anthropology, presumably with graduate training in map reading and site location, who are unable to give the proper location of the university that employed them. It is not surprising that they are confused about Pueblo.

**BOXT AND RAAB MISREPRESENT HISTORY OF THE PUVUNGA STRUGGLE**

Boxt and Raab claim that “Over a span of the last 25 years, then, a host of events materially affected sites CA-LAN-234 and -235. What is remarkable in retrospect is that there is no record that these activities were met with resistance or complaint from Indians, anthropologists, or the public” (p. 49). Apparently Boxt and Raab are relying on what they were told by campus officials, for they fail to cite any of the published material on the Puvunga struggle in anthropological sources (Overbey 1994; Ruyle 1994a; Ruyle 1995b; Skomal 1997).
They fail to cite any of the over 150 news articles on the subject. They apparently have not viewed the hour-long video "Sacred Lands, White Man's Laws" on the Puvunga struggle (Dodds and Gray 1994). They have not interviewed any of the Indian plaintiffs in the Puvunga lawsuit. They have not discussed this article with me or Keith Dixon or, as far as I know, anyone else critical of campus development plans. They have not requested copies of the papers on the Puvunga struggle that I have given at professional meetings (Ruyile 1994b, 1995b, 1995c, 1995d, 1996a, 1996b, 1999).

The movement to save Puvunga goes back to the 1970s, when Indian students at CSULB successfully mobilized for the reburial of the Indian remains that had been found on campus in 1972. The land was placed on the National Register of Historic Places in 1974. The university accepted this and modified its development plans accordingly. The president's residence that had been planned for construction on CA-LAN-234 was deleted in 1976, and student housing that had been planned for construction on CA-LAN-235 was deleted in 1978. Public records in the CSU Chancellor's Office clearly show that these changes were made because the sites were "registered in the National Register of Historic Places" and the "purpose of registration is to protect sites" (Board of Trustees 1976, 1978).

The Indian remains uncovered in 1972 were reburied in 1979, and the sign identifying the area as Puvunga was also put up at that time. The students were promised that the entire area would be preserved because it was on the National Register. Campus officials have denied that any such promise was made, but their own files support the Indians on this issue. Thus, this was not undeveloped land that happened to be on the National Register of Historic Places; it was undeveloped because it was on the National Register and because Indians had fought to preserve it.

For a dozen years, Indians were able to visit the site in peace for prayer and meditation. They did not object to the land being used for organic gardening, nor did they object to its use as a summer day camp for youngsters (including my two sons). Neither of these uses conflicted with the spirituality of the land cherished by Native Americans.

The situation changed when campus officials announced their plans to build a strip mall on the site. In December 1992, campus officials filed a false and misleading negative declaration stating that "No cultural resources are known to exist on site." This evoked a storm of protests from Indians, state officials, anthropologists, gardeners, students, and residents.

In response, campus officials brought in the contract archaeologist they had employed earlier. In a stormy confrontation with campus officials and this archaeologist on February 28, 1993, a group of about 40 Indians (including veterans of the earlier struggle at Puvunga as well as at Point Conception) demanded that Puvunga be preserved. They further insisted that the archaeologist be removed because of her "long past history of destroying sacred land that all Native Americans cherish." Thus, it was only after campus officials decided to build a strip mall on CA-LAN-235 and were frustrated in their attempt to conceal its National Register status that they hired Boxt and Raab for a "cultural review."

**BOXT AND RAAB MISREPRESENT THEIR WORK AT CSULB**

Here again, Boxt and Raab misrepresent the situation, claiming that "CSULB became the focal point of protests and legal challenges by Indians, anthropologists, and others after university officials announced a feasibility study to develop portions of campus terrain" (p. 45). The purpose of the "cultural review" by Boxt and Raab was clearly stated by CSULB Interim President Karl Anatol:

Unconvinced that Puvunga really lies under the Gardens, Anatol hopes results of the archaeological survey will allow CSULB to continue with plans to develop 22 acres of land. ... If Puvunga is indeed discovered under the site, Anatol said, "obviously the university will have to change its plans—not outright, not totally. There are ways to deal with the recognition of sites that still allow agencies and concerns to carry on with their projects" [Cox 1993:1].
As archaeologist Chester King, a veteran of the Point Conception struggle, observed, "They're trying to use archaeology as a smoke screen to destroy the site. I think everybody knows that and is seeing that, and the University is still trying the same game. I'm tired of it." (Dodds and Gray 1994).

In stating that "Owing to heightened concerns about CA-LAN-235, this site was not included in these studies" (p. 50), Boxt and Raab imply that Boxt's archaeological digs on campus did not include the National Register site out of some kind of sensitivity to Indian concerns. In fact, CA-LAN-235 was not included in Boxt's studies because another archaeologist was hired to do the job. Although Interim President Anatol had promised that the cultural review would be conducted with "the greatest possible respect and sensitivity to the interests of Native Americans," campus officials hired another firm for a massive backhoe excavation of Puvunga: "A grid will... be placed over the entire parcel, and 20-m long trenches will be excavated with a backhoe 20-m apart" (Altschul 1993:20).

While these plans were being made, Indians were holding a round-the-clock vigil at Puvunga to protect the site, and the Native American Heritage Commission wrote to the university recommending "complete avoidance of the site as the appropriate and only acceptable mitigation measure." (See further discussion in my comments on Haley and Wilcoxon's [1997] paper on Point Conception [Ruyle 1998]). Boxt and Raab do cite Boxt's reports on his archaeological digs on campus, but this is problematic as these reports are not available for independent review. Campus officials have repeatedly refused to release these documents, citing "verbal instructions" from university lawyers. Further, although Boxt and Raab base their ideas on the Haley and Wilcoxon (1997) article, they fail to mention the storm of criticism which that article evoked from the Chumash (Burns 1997) or the anthropological community (see the various comments in Current Anthropology 39[4], 1998).

It is not clear why Boxt and Raab fail to discuss a very significant site on the CSULB campus excavated by Boxt in the spring of 1993. As indicated above, Boxt's report on this site is not available, but a reporter for the campus newspaper gave me a one-page typewritten statement written by Boxt. This stated in part:

Excavations produced hundreds of shell beads, dozens of pieces of earthenware pottery, deer bone tools used to make projectile points, projectile points, cores that generated chipped stone tools, lithic waste flakes, and hundred of pounds of shell debris suggest that small bands of people utilized this site at certain seasons over a period of three or four hundred years. Radiocarbon dates confirm this assertion. Dates of T.S.N. 2 range from 1500 A.D. to 1850 A.D. The presence of steatite indicates trade with either Santa Catalina Island or Sierra Pelona near Palmdale. The presence of manos, mortars, and pestles indicates that the site's inhabitants processed seeds, acorns, and possibly small animals (rodents, etc.) for food [Boxt 1993].

During the 1993 excavation, a tooth from a human infant was found, but not reported to the coroner's office as required by state law. It was only repatriated after the Native American Heritage Commission notified the coroner's office of the find.

Following Boxt and Raab's convoluted argument ("the older an archaeological site is, the less likely it is to be connected with the Chingchinich tradition"), the site would seem to be a candidate for the historic Puvunga. A detailed discussion of Boxt's findings here would certainly be appropriate. Why is this site not discussed?

**BOXT AND RAAB MISREPRESENT INDIAN BURIAL**

Boxt and Raab assert that the identification of the Indian remains discovered at CA-LAN-235 in 1972 is based solely on "a two-page report" at UCLA. This is what Raab claimed in his court deposition in the fall of 1993. Raab's error was pointed out by Keith Dixon at the December 1993 meeting of the Native American Heritage Commission in Long Beach. In fact, the definitive identification was made in 1979 by Judy Suchey, the forensic anthropologist employed by the Los Angeles County Coroner, and reported in a letter to campus
President Steve Horn. This was covered in the Puvunga video (Dodds and Gray 1994) and I also quoted Suchey’s letter in my response to Haley and Wilcoxon (1997) (Ruyle “1998”). Yet Buxt and Raab continue to insinuate that there is some question about the identification of the remains.

**CONCLUDING REMARKS**

The article by Buxt and Raab is a shoddy and irresponsible piece of work. It is unclear how an article so lacking in substance could be accepted by an otherwise respectable journal. Unfortunately, space and time preclude a complete examination of the errors, misrepresentations, and half-truths of the Buxt and Raab article. I hope that serious students of the Puvunga struggle will consult the Puvunga web site (access through my own web site: www.csulb.edu/~ruyle), where fuller criticism of the article will be posted by the time it is in print.

This paper should never have been accepted for publication. The authors clearly lack the training in social and cultural anthropology which might qualify them for a study of “Indian Traditionalism.” They do not employ such traditional anthropological methods as participant observation, collection of life histories, or structured interviews. Worse, they ignore completely what Malinowski described as the “final goal, of which the Ethnographer should never lose sight. This goal is, briefly, to grasp the native’s point of view, his relation to life, to realise his vision of his world” (Malinowski 1922:25, italics in original). The native point of view on Puvunga has been well expressed by the Director of the CSULB American Indians Studies Program.

...the site in question is sacred not because it may contain artifacts or remains of ancient Gabrielines (simply removal and reburial of bones will not lessen the significance of the land), it is sacred to the Gabrieline because this campus sits on the land on which a new religion was born in the person of Chungichnish. This religion became the center of the tribes’ existence. Even though no one else can remember the sacredness of this land, the Gabrieline are bound up in that history and are therefore called upon to remind us all that we are walking on land that remembers another people, the Gabrieline of Puvunga and another god, Chungichnish [Jacobs 1993:3].

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Reply

L. MARK RAAB and MATTHEW A. BOXT
Department of Anthropology, California State University, Northridge, 18111 Nordhoff St., Northridge, CA 91330-8244.

EUGENE Ruyle and Keith Dixon, long-time advocates of Puvunga-related issues and members of the Anthropology Department at California State University, Long Beach (CSULB), are active stakeholders in the current debate. Since Ruyle and Dixon also offer the most critical reviews of our article, we turn our attention to their comments first. Although Ruyle and Dixon emphasize somewhat different issues, they reflect quite similar lines of commentary. The reader will notice, for instance that Ruyle and Dixon essentially ignore the major points made by our article. Instead, both commentators adopt a strategy favored by trial lawyers: if you cannot refute your opponent’s arguments directly, distract the jury with confusing side issues and character assassination. These diversionary tactics are designed to destroy the credibility of our article on three grounds: it leaves out vital information; we are guilty of sloppy scholarship; and we are “hired guns,” somehow enticed by the administration of CSULB to propagate ideas inimical to Native Americans and historic preservation. Let us look at these charges more closely.

SINS OF OMISSION?

Ruyle and Dixon say we omitted important issues and information from our article. Ruyle, for example, criticizes us for not reporting in detail the perspectives of contemporary Indians, and for not adequately discussing the “Puvunga struggle.” Both Ruyle and Dixon maintain that we failed to provide an accurate history of archaeological investigations on the CSULB campus, including what they say are misinterpretations of a Native American burial discovered a number of years ago. Curiously, Dixon focuses most of his comments on legal documents, personal correspondence, and an unpublished manuscript. This tactic complicates assessing our article published here, since the documents to which Dixon refers are not before the readers of this volume. On the other hand, this approach affords plenty of opportunity for creating confusion and introducing ad hominem judgments.

Each of the points outlined by Ruyle and Dixon is interesting in its own right. There is no question that Indians hold important perspectives on Puvunga, views to which they and others are deeply committed. The CSULB campus has an extensive and interesting history of archaeological investigation. We see nothing to keep Ruyle and/or Dixon from airing their perspectives on these topics in print. But our article in this volume is not primarily