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COMMENT

A Zoologist's Perspective on Presenting Archaeological Faunal Data

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One notable contribution of archaeological faunal analysis is the prehistoric information obtained from excavations that contribute to our knowledge of the range and natural history of organisms (Wolverton and Lyman 2012a:1–22). These findings often precede "modern" regional surveys and document the human-induced extinctions that have considerable relevance today (e.g., Kolbert 2014; Leakey and Lewin 1995). As examples from the vertebrates, Broughton (2000) identified eleven fish species from the early Holocene Homestead Cave in the Utah desert that contributed to our understanding of prehistoric fish biogeography; Butler and Delacorte (2004) documented the prehistoric presence of two minnows (Cyprinidae) and a sucker (*Catostomus fumeiventris*) in the Owens River, California; and Holdaway and Jacomb (2000) documented the rapid extinction of eleven species of endemic moas in New Zealand at the hands of Polynesian settlers around 700 years ago, a general phenomenon of bird extinctions addressed by Steadman (1995, 2006) throughout the tropical Pacific. Numerous articles edited by Turvey (2009) and Wolverton and Lyman (2012b) are dependent on zooarchaeological data to document global Holocene extinctions and extirpations. In California, I was able to document the presence of coho salmon (*Oncorhynchus kistuch*) remains among archaeological materials in Santa Cruz and Monterey counties (Adams et al. 2007; Gobalet 2012). These data helped uphold the listing of coho salmon as an endangered, and therefore protected, species.

Do these scholars and other zooarchaeologists generate accurate data? Several persuasive faunal analysts have recommended guidelines for enhancing the quality of zooarchaeological data and presentation (e.g., Butler and Lyman 1996; Driver 1992, 2011; Gobalet 2001, 2005; Lyman 2002; Wolverton 2013), but if the limited number of citations of these papers as reported by Wolverton (2013:384) is accurate, it appears that few archaeologists are ascribing to these guidelines. As a zoologist with 40 years of experience in the identification of fish remains from archaeological and paleontological sites, I continue to be perplexed by the apparent uncritical acceptance of faunal data as accurate. Following are examples of recent papers in the *Journal of California and Great Basin Anthropology* (*JCGBA*) that have accuracy issues that I hope we can learn from in order to enhance the credibility of faunal and floral analyses.

The recent two-issue compilation of papers published by the *JCGBA* [2015:35 (1 & 2)] on the Native American fisheries of California and the Great Basin is an admirable and positive step toward updating our knowledge of the fisheries that were important to prehistoric groups in the area. These papers provide insights regarding the aquatic world of California before the arrival of peoples from the Old World. I commend Editor Bill Hildebrandt and his staff for their initiative in making this happen.

In this essay, I strive to address some issues that hopefully will lead to a more credible presentation of biological data by archaeologists. Many of these issues have been raised previously (Driver 1992, 2011; Glassow and Joslin 2012:220–221; Gobalet 2001, 2005; Lyman 2002; Turnbull et al. 2015:83; Wolverton 2013), but these concerns are worthy of repetition because problems persist. The following points and examples are from recent volumes of the *JCGBA*, supplemented by other sources:

(1) The failure to question the sources and reliability of faunal data appearing in archaeological reports not subjected to peer review by qualified individuals can undermine the credibility of a paper. The faunal data may not be accurate.

(2) Archaeological reporting on faunal and ecological topics needs to be up-to-date in terms of the current literature in the appropriate discipline of biology, including taxonomy.

(3) Publications that do not permit the reader to know the individual responsible for faunal identifications

or the comparative materials used in making the determinations make it problematic for the reader to evaluate the accuracy.

(4) From the perspective of a biologist, inappropriate use of biological terminology can discredit a paper.

DISCUSSION

Noncritical Citation of Gray Literature

Numerous authors, including myself, have concerns regarding the citation of Cultural Resource Management (CRM) and similar reports that have not been subjected to anonymous peer review. These reports are difficult to access, are not widely distributed or abstracted, and—consequently—may not be credible (Collette 1990; Germano and Bury 1994; Gobalet 2001; Wilbur 1992). Most archaeological and biological literature from nearly a century ago would be categorized today as "gray literature" and in the words of Bill Hildebrand (personal communication August 16, 2016) "our discipline would be emasculated without access to these data." Nonetheless, the faunal data in these reports are often seemingly repeated without their credibility being questioned. When reviewing a faunal-based archaeological paper, initially one should ask—who made the identifications and what comparative materials were used for the determinations? Why isn't this practice universal? Having completed the fish faunal identifications from hundreds of archaeological excavations, I have *never* had my findings questioned and am concerned about the data of others. Why isn't there reciprocal uncertainty? Carl Sagan's (1996:28) observation that "One of the great commandments of science is, mistrust arguments from authority," should be heeded.

Tushingham and Christiansen (2015:Table 1) rely heavily on gray literature sources, including several for which James Quinn was the faunal analyst. I recently identified the fish remains from CA-NAP-39, which were provided by Far Western Anthropological Research Group, Inc. of Davis, Cal. A portion of the sample had been previously evaluated by Quinn, who was then on the staff of the Anthropological Studies Center, Sonoma State University. While evaluating the same materials, Quinn identified 14 fewer species and 42% fewer specimens to at least the family level from among 1,600 specimens. He failed to identify remains of Sacramento perch (*Archoplites interruptus*), the most abundant species among archaeological remains in the Central Valley (Hash et al. 2015), or the once common thicktail chub (*Gila crassicauda*). One should question who is correct (Gobalet 2001), but the multitude of problems with Quinn's work on CA-NAP-39 are even more frightening because Leidy (2007:85), a fisheries biologist, cited Quinn's (2002) data. This is an extreme example of the pitfalls of citing unreviewed and unpublished data.

Tushingham and Christiansen (2015:Table 1) also cite Julie A. Ricks as the analyst in a gray literature source. Ricks reports 28 Pacific herring (*Clupea pallasii*) elements, to the exclusion of Pacific sardine (*Sardinops sagax*), the other possible member of the family Clupeidae. I question the accuracy of numerous identifications as Pacific herring, as I have in the past (Gobalet and Jones 1995:815). Driver (2011:44) wisely reports the "tendency for the more experienced *faunal analysts* (author's addition) to be less willing to differentiate between closely related species." I suspect that Ricks may have limited experience, while another analyst, Justin Hopt—also referenced by Tushingham and Christiansen (2015) from a gray literature source—is more experienced and better trained because he reported 389 elements as Clupeidae, the appropriate taxon for most herring and sardine bones. Gobalet et al. (2004:805, Figs. 6 and 7) consider only the penultimate and the first three vertebrae to be diagnostic for distinguishing Pacific sardine from Pacific herring.

Using Current Taxonomy

To my knowledge, the only member of the Gadidae (cods) as currently defined by Page et al. (2013:93) to be found among the prehistoric archaeological materials of California are Pacific tomcod (*Microgadus proximus*). Another gadid would be a first for the region. The other five possible regional gadids suggest a deep-water fishery, an important point for understanding the Native American fishery. If an older usage of Gadidae was used following Hart (1973), Pacific hake (*Merluccius productus*) would be included in the family. Pacific hake are found in small numbers at archaeological sites along the Pacific Coast and islands of California (Gobalet et al. 2004; Turnbull et al. 2015). The three elements that Tushingham and Christiansen (2015:204) attribute to a member of the cod family (Gadidae) underscore the importance of using

current taxonomy, because they may have found Pacific hake or deep-water members of the family.

Tushingham and Christiansen (2015:204-205) also list both Atherinidae and Atherinopsidae as present. This suggests either the use of an outdated taxonomy for North American silversides (Atherinidae; Hubbs et al. 1979:15) or the presence of an introduced species, because Atherinidae currently is the family of the Old World silversides and Atherinopsidae is used for New World silversides (Nelson et al. 2004:215). Since there was no reference to an introduced species, I suspect the use of an older taxonomy. Tushingham and Christiansen's use of Sebastidae (2015:206 and Figure 6) is correct, but it further illustrates the need to define the standard used for nomenclature. Though Eschmeyer et al. (2016) designate Sebastidae as the family that includes *Sebastes*—the 61 species of rockfishes from the marine waters of California—Love et al. (2002:8) and Page et al. (2013:116) use Scorpaenidae. Bill Eschmeyer and Milton Love are both luminary ichthyologists with expertise with rockfish taxonomy, which demonstrates the differences of opinion that make any taxonomy tentative and confusing, especially to a non-specialist.

In an exemplary summary of the Great Basin Native American fishery, Delacorte (2015) references Sigler and Sigler (1987), though not exclusively, for the names of the fishes. Ichthyologists regularly change the Latin binomials and other taxonomic designations of fishes as new information is published. In the case of Delacorte (2015:30, Table 1), several of the members of the family Cyprinidae are now in different genera, and some killifishes are assigned to an entirely different family. With the utilization of molecular techniques like those of Schönhuth et al. (2014) for the genus *Gila*, we can expect major revisions to many fish taxonomies in the future. Such revisions can be significant to archaeological studies, as the following example illustrates.

Only a specialist on fishes would note Bernard's (2015:59–60) reference to a "chub (*Gila* sp.)" from the southern San Joaquin Valley of California. The only *Gila* from the region is *G. crassicauda*, thicktail chub (Moyle 2002), and the designation should reflect that. The geographically closest member of the genus is *G. orcuttii* (Arroyo chub), native to the coastal streams south of Point Conception. Tom Wake (personal communication 2016) used *Siphateles bicolor* (Tui chub; formerly *Gila bicolor*) as a proxy for the extinct *G. crassicauda*, for which there is limited available comparative material. As Driver (1992, 2011) points out, comparative collections never contain all the required comparative specimens, and when certain organisms are lacking, faunal specialists assume similarity because of close taxonomic association. Consequently, the incorrect genus (*Siphateles* and not *Gila*) was used for comparison in the Bernard study because of a taxonomic change.

These taxonomic changes are not unique to animals. Moe (2016:11–30) lists 65 generic or familial realignments and twelve pages of name changes for the flowering plants of Kern County, California since 1995. Note that is a single county during only 25 years!

Using Current Natural History Literature

Familiarity with and citation of current biological literature minimizes ambiguity in reports and strengthens the arguments presented.

Reddy et al. (2015:239, right column) state that "a maritime subsistence regimen should feature offshore species" and use California sheephead (*Semicossyphus pulcher*), kelpfish (Clinidae), and soupfin sharks (*Galeorhinus galeus*, named tope by Page et al. 2013) as examples of offshore species. No available biological literature is cited in support. California sheephead are residents of kelp beds and the rocky inshore from the sub-tidal to 55 meters (Hamilton and Caselle 2014; Hamilton et al. 2011; Love 2011:439). Soupfin sharks can be found in continental shelf waters, but also close inshore, including in shallow bays (Ebert 2003:136), and the giant kelpfish (Clinidae: *Heterostichus rostratus*) are most abundant from about 25 meters in depth to the shallow sub-tidal (Love 2011:480). Giant kelpfish reside in Morro Bay, a shallow and inshore habitat (Fierstine et al. 1973). Jones et al. (2016:132, Table 5) documented them in the archeological record of sites on Morro Bay. According to these sources, California sheephead, soupfin shark, and clinids are *not* "offshore species." From my perspective as a zoologist, the absence of any appropriate literature undermines the credibility of the entire paper.

Noting the Faunal Analyst and the Collection used for Identifications

Reddy et al. (2015:Table 5 and p. 244) report giant sea bass (*Stereolepis gigas*) from mainland archaeological

sites in California for the first time, yet do not mention the significance of finding this endangered species. Their massive size alone is noteworthy since a giant sea bass can reach 253 kg. (House et al. 2016). The authors unfortunately do not cite the individual responsible for the identification nor what comparative materials were used. The only indication regarding who completed the identifications is in the acknowledgements section, where Timothy Carpenter, Thomas Wake, and Kenneth Gobalet are thanked for the analysis of the fish remains. Since neither Tom Wake (personal communication 2016) nor I identified the giant sea bass, it is assumed that the analyst was Timothy Carpenter. Because this information was excluded from the paper, the interested reader would have to contact Carpenter to ascertain which comparative collection he used as the basis for his identifications.

Tushingham et al. (2016), in an otherwise informative report on excavations at CA-HUM-321, do not indicate who is responsible for the identifications of the seeds, fishes, mammals, birds, or invertebrates, nor the collections used in the determinations. Based on prior papers, I suspect that Justin Hopt completed the fish identifications, but the reader should not have to guess. I am suspicious of the fish identifications because Tushingham et al. (2016) do not distinguish herrings from anchovies. It is my experience that the vertebrae of clupeids (Pacific herring and Pacific sardine) are distinguishable from those of northern anchovy (*Engraulis mordax*). Though they cite a general source, they imply that two introduced species might be among the remains, the American shad (*Alosa sapidissima*) and threadfin shad (*Dorosma petenense*). If these are present, they suggest contamination of the site with relatively recently introduced species, a point not noted in the paper. A more thorough knowledge of the local organisms would have eliminated this ambiguity.

When I contacted Michelle Stevens to inquire about the source of the fish data referred to in Stevens and Zelazo (2015:175-182), she referred me to her coauthor, who did not respond to emails. Again, the individual responsible for making identifications is not cited and the data are left to be taken on faith, something I am not willing to do. To me, the absence of this information weakens the validity of the fish identifications in this chapter.

Alphabetical Listing of Organisms in Tables

The alphabetical listing of fishes in data tables reduces the information that can be simply accessible to the reader. Among the examples of alphabetical or haphazard listing are Colten (2001:201-206), Fitch (1972), Gamble (2008:27), Gusick et al. (2015:226-227), O'Connor et al. (2011:1120), Rick and Erlandson (2012:116), and Tushingham and Christiansen (2015). Alphabetization is a simple way to locate individual fishes and is not inherently wrong, but it separates related fishes, an issue avoided when the listing is taxonomic, phylogenetic, by habitat, or by technique of capture. To this biologist, this sends the message that the authors know little about the organisms that serve as the basis of their work.

Reporting Novel Findings

Gusick et al. (2015:225 and Tables 4 and 5) are the first to identify Pacific electric ray (*Torpedo californica*) in the archaeological record of California, and referencing the diagnostic elements used in the determination would be a helpful inclusion for the reader to determine the accuracy of the identification. With only a single member of this family Torpedinidae known from the area, it is confusing to see both Pacific electric ray and Torpedinidae listed in the Tables. As mentioned above, Reddy et al. (2015) are the first to find remains of giant sea bass somewhere other than the islands off California (including Cedros Island in Baja California by Turnbull et al. 2015:75), and this notable point is not acknowledged in the paper—which to me is an oversight.

Appropriate Use of Biological Terminology

Imprecise use of biological terminology in some archaeological papers can create confusion. The use of "taxa" and "taxon" is particularly problematic (Gobalet 2005:645). Due to my zoological background, I am bothered by the imprecision with which these terms are often used. I doubt that I am going to have any impact by mentioning this, but from my perspective the traditional usage of taxa/taxon has evolved in directions that perplex me. Perhaps the ultimate authority for nomenclature in zoology—the *International Code of Zoological Nomenclature* (*ICZN* 1999)—defines taxon (pl. taxa) as "A taxonomic unit, whether named or not: i.e., a population, or group of organisms which are usually inferred to be phylogenetically related and which have characters in common which differentiate

(q.v.) the unit (e.g., a geographic population, a genus, a family, an order) from other such units. A taxon encompasses all included taxa of lower rank (q.v.) and individual organisms." *The Compact Oxford English Dictionary* (1991:2016) is more succinct with its definition of taxon: "A taxonomic group, as a genus or species." Note that taxon is *not* defined as a synonym for "species" or "organism." An anonymous reviewer of this manuscript defended the usage of taxa/taxon with the definition as used by zooarchaeologists: "A biological unit to which they have been able to assign a skeleton element." Where is this definition formalized? I acknowledge that language changes, but with my background as a zoologist and my self-taught specialization in fish zooarchaeology, I find usages other than those traditionally defined above indicators of limited understanding of the classification schemes employed by organismic biologists.

Archaeologists often refer to "**small taxa**." This can be interpreted as a species whose members are generally diminutive in size, or as a genus with few species, a single species, a subspecies, or a population. The same issue arises with the use of "**lower ranked taxa**." Is the intention to indicate that a genus is a lower rank than a family or order? Replacing "taxa" with the common name of the organism in many of these instances eliminates the ambiguity. The before-mentioned reviewer defensively states that "Ranking a single taxon, or group of organisms as lower or higher ranked, in parlance of foraging theory is not ambiguous. Scholars who use this term are not concerned with biological nomenclature or taxonomic hierarchy, but in human use of resources. The context in which these terms are used is clear; there is no ambiguity." Once again I cannot reconcile this reviewer's usages with the formal biological definitions I presented above. I just do not see a population of hunter/gatherers switching from one taxon of organisms to another. They switch from one organism to another. I doubt that they sit around and ponder, "We are running out of prey in the order Anseriformes, let's now forage for members of the subclass Actinopterygii." As one with biological rather than archaeological training, my position is that if I see the terms taxa/taxon in a paper used in ways outside of the traditional taxonomic definitions above, I am prone to discredit the paper along with its faunal data.

Gusick et al. (2015:228) could clarify their usage where they suggest, "The largest identified fish **taxa** present in the assemblage, Triakidae and Squalidae, show a trend that is decidedly different from the smaller fish mentioned above." To me, the zoologist, a "large taxon" is one like Actinopterygii that contains well over 30,000 species divided into numerous more exclusive categories (smaller taxa). I suspect that Gusick et al. (2015) intended to indicate that the largest individual fishes in the assemblage belonged to the Triakidae and Squalidae.

As an example of traditional taxonomic designation, if we consider a single fish, the thicktail chub, it is categorized within the following taxa; *Gila crassicauda*, *Gila* sp., Cyprinidae, Cyprinoidea, Cypriniformes, Ostariophysi, Euteleostei, Teleostei, Halecostomi, Neopterygii, Actinopterygii, Osteichthyes, Pisces, Gnathostomata, Vertebrata, Chordata, Deuterostomia, Bilateria, Eumetaza, Animalia, and Eukarya (combining Erwin and Valentine 2013:75; Hickman et al. 2011; Nelson 1984:inside cover). Note that I have listed 21 taxa for a single fish. From my background as a biologist, the intended meaning of many authors employing the terms taxon or taxa could be clarified by the use of more specific terminology.

RECOMMENDATIONS

(1) **Cite appropriate biological literature** to support the ideas presented. If the primary content of an article focuses on fishes and fishing, it should contain appropriate recent biological literature, not another archaeological report with biological content. Any study that includes faunal and floral data should develop a context for the animals and plants that live in the region through the citation of pertinent biological sources. Eschmeyer et al. 2016 (for global fishes) or Page et al. (2013 for North American fishes) are appropriate sources for the current taxonomy and common names of fishes.

(2) **Collaborate with those having expertise** in the disciplines studied. No individual can possibly master the broad spectrum of expertise required today to interpret findings from an archaeological excavation. Consequently, reliance on those with the appropriate specialization is essential.

(3) **Reviewers** of manuscripts submitted for publication should include individuals with expertise in the

appropriate field. Journal editors should seriously consider specialized biologists as reviewers when appropriate. I suspect that the concerns I express here result from archaeologists with limited biological background reviewing the manuscripts of authors who also have limited experience in biology. Reviewers can certainly excuse themselves if their expertise is not appropriate to the topics addressed in the manuscripts being reviewed.

(4) In a perfect world, I suggest avoiding citing reports that have not been subjected to anonymous peer review. A more realistic recommendation is that authors **question the credibility** of sources before citing them and obviously reject citations when appropriate. If faunal data are a significant component of a gray literature source, it is imperative to designate the individual responsible for the identifications, the comparative materials used for the determinations, and clarify in the publication the tenuous nature of the source.

(5) Authors are accountable for indicating the **individual responsible** for identifications and the **comparative collections** used. **As a reviewer or journal editor**, insist upon the same. This should be standard for every report with a faunal (or floral) component. Because there is so little quality control in faunal analysis, "the only criterion for the validity of identifications is the reputation and experience of the analyst" (Driver 2011:41). Many others share these concerns (e.g., Driver 1982, 1992, 2011; Gobalet 2001; Lyman 2002, 2010; Reitz and Wing 1999; Wake 2004; Wolverton 2013).

(6) For those interested in furthering their understanding of the zoological usage of the terms **taxa/taxon** and **taxonomy**, the following references are recommended: Broughton and Miller (2016:1-4); Driver (1992:38); Lagler et al. (1962:423–428); and Reitz and Wing (1999:32–42).

(7) Anyone reporting faunal or floral information should follow the suggestions of Driver (2011), Gobalet (2001), and Wolverton (2013) when doing so. An exemplary recent publication that can serve as a model for faunal reporting is Dombrosky et al. (2016).

A PROPOSAL

Any individual wishing to test his/her skills at fish faunal analysis can be "blind tested." I am willing to provide specimens of bones (vertebrae mostly) from geographically appropriate fishes to any faunal analyst wishing to participate. This can be done anonymously by providing the individual participating with the answers so that only he/she is aware of the results. Individuals can thus evaluate their own skills without others being any the wiser. This sort of rigorous assessment and improvement in identifying bones should be commonplace. As an illustration, Andrews et al. (2003) utilized a blind test to evaluate the accuracy of using the growth rings of sectioned otoliths to determine the season of capture of fishes.

FINAL COMMENT

Considering the number of prior papers recommending aspects of what I repeat here and the persistence of the issues, I suspect that my comments will not be taken seriously. I acknowledge that individually, the concerns I raise for the most part will not affect the conclusions of the authors of the papers that I have singled out on an uncritical archaeological community; however, by collectively addressing these problems, the credibility of archaeological reporting can be enhanced and made **more compelling to biologists**. The responsibility for issues like those addressed in this paper lies with authors, reviewers, and editors of journals. Had qualified individuals reviewed the papers illustrated, the issues raised here could have been avoided. As a biologist with a specialty in fishes, the comments I make involve examples with which I am familiar. The suggestions recommended here are appropriate to any faunal reporting.

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