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Journal of California and Great Basin Anthropology

Title

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Permalink

<https://escholarship.org/uc/item/9gf9581t>

Journal

Journal of California and Great Basin Anthropology, 37(1)

ISSN

0191-3557

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Publication Date

2017

Peer reviewed

Animal Bones and Archaeology: A Reply to Gobalet

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We read with interest Ken Gobalet's comments entitled "A Zoologist's Perspective on Presenting Archaeological Faunal Data." We are always appreciative when veteran scholars take an interest in our work—Gobalet has over four decades of experience and has made enormous contributions to the field of fish-bone analysis—and we appreciate the spotlight on our research and the opportunity to publish a reply.

Gobalet drafted his comments in response to the 2016 publication of two special issues of the *Journal of California and Great Basin Anthropology* (Vol. 35:1 & 2), which feature a collection of papers on Native American fisheries in the region. While Gobalet praises the overall effort, he outlines some problems with the reporting of ichthyofaunal data in archaeology, and posits that if these are addressed "the credibility of archaeological reporting can be enhanced and be *more compelling to biologists*."

We write this as the authors of and contributors to two of the papers cited in Gobalet's critique (Tushingham and Christiansen 2015; Tushingham et al. 2016). Tushingham is the first author of both papers, and Hopt is a coauthor of Tushingham et al. 2016, and the zooarchaeologist who identified the fish bone in Tushingham et al. (2016) and studies summarized in Tushingham and Christiansen (2015). While we are by no means the only authors and analysts critiqued in Gobalet's paper, we are the most frequently mentioned. Those papers—and this response—are written from our perspective as archaeologists. Although we are interested in biological data, our focus is on human behavior, and we employ fish-bone data because we are interested in understanding how people interact with fish over the long-term historical record.

As Gobalet notes, several of the points he makes in his critique have been expressed before (e.g., Driver 1992;

Gobalet 2001; Lyman 2002), but have, unfortunately, not been consistently applied by archaeological faunal analysts, and Gobalet should be acknowledged for rehashing several of these points. However, other comments by Gobalet are less useful and deserve a response. Specifically, some of Gobalet's critiques are confusing or cast unwarranted doubt on the validity of several aspects of our work (Tushingham and Christianson 2015; Tushingham et al. 2016) and that of others as well. Gobalet critiques the use of grey literature and suggests some rules of engagement for reporting archaeological faunal data, but does so in an inconsistent manner that is often unfair to junior colleagues. We suggest that there are more constructive and realistic solutions to the problem of dealing with analytical variation and error. We also argue that much faunal data is really best treated as ordinal data, and that major patterns are really what we can have confidence in, rather than the level of detail Gobalet suggests he would interpret (Grayson 1984; Lyman 2008; Wolverton et al. 2014). And—in terms of our work—Gobalet's criticisms do not undermine our archaeological conclusions about northwestern California and southwestern Oregon fisheries in the slightest: small mass-harvested fish (especially smelt) are important at coastal sites in Humboldt Bay and Del Norte County, small to medium intertidal fish and surfperch are important at others, there are fewer salmon than expected, and fine fraction sampling is important everywhere (Tushingham and Christiansen 2015).

REPORTING AND TAXONOMY

We wholeheartedly agree that archaeologists need to be more careful in the details of fish bone reporting. One of the strongest arguments that Gobalet makes is how faunal analysts need to stay up to date on current taxonomy, as it allows for a clearer understanding of exactly which species occur within assemblages. This can be an issue when reporting older data—for example, Tushingham and Christiansen (2015) synthesized data on over 18,000 fish bones from 15 studies dating back to the 1960s. While we updated the taxonomy in most places, Gobalet was correct in pointing out that we listed both Atherinidae and Atherinopsidae in our paper. This mistake occurred because we failed to revise the taxonomy used in an earlier study, specifically Waechter

1990 (Appendix 3). A simple check of current taxonomy would have cleared up the issue.

A more confusing critique was leveled by Gobalet for the use of the family designation Gadidae in Tushingham and Christiansen (2015). Gobalet notes that a more detailed identification (to the genus or species level) would have allowed us to possibly identify a deep-water fishery, as some gadids in the region spent portions of their life in the lower regions of the ocean. While he correctly supports trying to achieve species level of identifications, it really has no bearing on our paper or the conclusions contained within it. Making an argument of a deep-water fishery based on a maximum of only six specimens would have been unwise. Instead, it is at most a missed opportunity for more nuance in our conclusions. Also confusing is why Gobalet implies that we were including Pacific hake (Merlucciidae) within our identifications as “Gadidae.” Pacific Hake is no longer considered a member of the Gadidae family, and his implication that we made a mistake in this instance is unfounded.

Another highlight of Gobalet’s article is his call to make explicit who the faunal analyst was and what was included within the comparative collection utilized to make identifications. These recommendations have been included in several previous articles detailing how to better report archaeological faunal analysis (Driver 1992, Gobalet 2001, Lyman 2002).

Interestingly, the Tushingham and Christiansen (2015) paper is not given credit for being explicit in this regard—note that in Table 1, all analysts who identified fish bone in the 15 studies we report are listed in column nine, including Gobalet. Instead, Gobalet goes out of his way to scold another paper published later in a different journal (Tushingham et al. 2016), in which the fish-bone analyst (Hopt) was a co-author but was not specifically identified as the analyst.

Other reporting recommendations include outlining the specific “universe” of possible fauna, specifically outlining how identifications were made, and including more detailed lists of elements that were identified. All of these recommendations are pertinent and should become part of standard zooarchaeological practice. Of course, page and word limits in journal articles become an issue when trying to detail how identifications of each specific (or even groups of specific) bones were identified to one species/element instead of to another species/element.

However, we suggest that this could be largely alleviated by the increasingly common practice of placing such “supplemental” information on the internet rather than within the article itself. In this regard, a benefit of the grey literature (see below), is that there is often more room for full reporting than in journal publications.

ISSUES WITH VALIDITY

The issues with taxonomy and reporting outlined above are relatively straightforward and simple to deal with, but Gobalet also calls into question, directly and indirectly, the validity of specific fish-bone identifications and studies. Gobalet’s solutions to improving the validity of fish-bone studies include exclusion or vetting of non-peer reviewed studies, and blind-testing identifications, and (although not a long term solution) he offers to blind-test fish bones that archaeologists send him. We understand Gobalet’s general call for better science, but take issue with certain arguments, outlined below, and emphasize that his criticisms are irrelevant to our archaeological conclusions.

PRECISION AND ACCURACY

We are concerned with the lack of care in how certain critiques are framed, which can, however unintentionally, cast sweeping doubt on the work of others. For example, Gobalet presents a specific critique of our work that is troubling in its implications:

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.

With this statement, Gobalet casts doubt on all of our fish identifications because he feels that he personally could have taken a different approach to identifying the Clupeiformes. That Gobalet is a more experienced faunal analyst than Hopt is true, and perhaps he could have separated some of the anchovies from the reported sample, but characterizing all of our fish identifications as “suspicious” because of this difference in methodology is unnecessary and unwarranted. The order of Clupeiformes was not separated out to the more detailed family designations because of analyst uncertainty related to

the lack of clear diagnostics on the bones in question. Reporting these remains to a higher taxonomic level is warranted when family/genus/species cannot be confidently established by the analyst (Reitz and Wing 2008: 164–166). Indeed, we know Gobalet is aware of this phenomenon, because he quotes Driver's (1992:44) statement that there is "a tendency for the more experienced to be less willing to differentiate between closely related species."

Decisions about "closely related species" are affected largely by the analyst's experience and the materials available—and this is influenced by both the taphonomic state of the archaeological fish bone being analyzed and the composition of the comparative materials. When uncertainty arises, a more conservative approach is warranted and should not lead to calls for suspicion on the part of more experienced analysts.

While Gobalet critiques us in this instance, he inconsistently praises us for not distinguishing between herring and sardine earlier in his comments. Of course, the reasoning behind both of these decisions is the same, analyst uncertainty, making both Gobalet's critique and its severity (calling into question all of our fish identifications) even more confusing.

Another way of looking at this is that the *precision* of faunal IDs improves as analysts gain confidence and experience. As stated above, we expect that analysts will be able to identify more bones to the species level over the years as they work with more bones and are exposed to more archaeological assemblages. However, being less precise does not equate with lower *accuracy*—being less precise does not necessarily make faunal IDs less valid, and in fact it is the case that sometimes purposely being less precise is the only way to ensure accuracy.

We see accuracy as being the analyst's primary responsibility, even if this is at the expense of some precision in fine-grained taxonomic distinctions. The long and short of it is that the analyst has to do the best he or she can in the real world, and prudence is merited for a variety of reasons: due to a lack of clear diagnostic characteristics in the bones being analyzed, analyst inexperience, or to the unavailability of appropriate comparative collections. It is simply naïve and unrealistic to assume all faunal materials will be well-preserved, that analysts are equally experienced with all taxa, and that the comparative collections they have access to include all of the taxa present in the archaeological collection.

As noted above, the inability to distinguish some bones to more detail is, at most, a missed opportunity for more nuance in our discussions and conclusions. By being more conservative we are ensuring that we are not putting out bad data, and thus not advocating what would be unwarranted discussions and interpretations to an audience that may not know better (we avoid making Type 1 errors this way). It is also critical to point out that many questions in archaeology can be addressed at a macro scale of analysis. Put another way, major patterns are most critical to our arguments.

DEALING WITH VARIATION

In archaeology, while we should always strive to be accurate, a great deal of precision is not always possible or necessary. In fact, compared to fields like biology, archaeologists are much more accustomed to the idea that there will be some variation in precision. In any field that relies on analysis by human beings, there will inevitably be differences in analyst identifications. Analyst variation is true not only for fish bone (Gobalet 2001), but has also been documented in studies of mammalian bone (e.g., Lyman and VanPool 2009; Marean et al. 2001); lithic microwear (Bamforth 1988); lithics and ceramics (Fish 1978); lithic tools (Beck and Jones 1989); shellfish (Bell 2009); and many other artifact categories. And in archaeology there of course are more potential sources of variation when dealing with archaeological fish bone than in biology due to (1) the differential selection and discard of faunal elements in the behavioral context, and (2) widely varying differential preservation in post-depositional archaeological contexts.

However, there are a number of ways of dealing with and minimizing such variation, including the innovative use of computer scanning and GIS (Betts et al. 2011; Niven et al. 2009); DNA species identification (Cannon and Yang 2006; Grier et al. 2013; Moss et al. 2014; Moss et al. 2006); treating faunal data at the ordinal or nominal scale (Grayson 1984; Lyman 2008; Wolverson et al. 2014); development of quality assurance practices (Nims and Butler 2017); obtaining a robust comparative collection; and accepting less precise, but accurate, data. Large data sets can also mitigate some of these issues. As Gobalet and Jones state in their own synthesis of fish bone from 51 sites in Central California, "The

value of these data increases when they are regionally considered” (Gobalet and Jones 1995:813). The intent of the Tushingham and Christensen paper was similar: to investigate broad regional trends by synthesizing a large number of studies. Indeed, as Gobalet himself apparently agrees, the overall conclusions of the papers he critiques remain intact despite the problems he points out. As for our work, Gobalet’s criticisms are far from devastating—small mass-harvested fish are important at many sites, intertidal fish and surf perch at other sites, there are fewer salmon than expected, and fine-mesh sieving is critical to understanding overall fish use patterns (Tushingham and Christiansen 2015).

GREY LITERATURE AND PEER-REVIEW IN ARCHAEOLOGY: A REALISTIC VIEW

We also take issue with Gobalet’s sweeping judgement of grey literature: “In a perfect world, I suggest avoiding citing reports that have not been subjected to anonymous peer review.” While Gobalet states that “these reports are difficult to access, are not widely distributed or abstracted, and, consequently, may not be credible,” *accessibility does not equate with credibility*, and such an argument is even less relevant in this digital age. Indeed, while there are problems with reports that have not been refereed, problems can also slip through peer reviewed articles. Although we disagree with Gobalet’s position on this, his own critique of refereed papers in *JCGBA* would prove this to be true!

Peer review can be really good at catching errors in higher level theoretical arguments, and for evaluating the research design, methods, logic, and the like, but it is less adept at vetting problems with specific analyses. In terms of fish-bone analysis, or any type of materials analysis for that matter, while reviewers might be able to catch inconsistencies or problems with precision in reporting, unless they are given the collection along with the paper to review, it would be exceedingly difficult for them to evaluate specific faunal identifications. And this is as true for faunal remains as it is for potsherds, lithic tools, or any of many categories of artifacts that archaeologists identify and analyze. This would leave the reviewer with another form of evaluation: having outside knowledge of an analyst’s skills (but see below) and/or the quality of the comparative collections.

In our view, if 90% of archaeology is done in CRM contexts (Sebastian 2010:7)—which are largely reported in the grey literature—we run the risk of overlooking a great deal of important information. Indeed, how practical would it be for archaeologists to ignore data generated by the \$1 billion annual CRM industry—a sum that is spent annually by public agencies and private sector companies (Altschul and Patterson 2010:297)? If we run down that road, wouldn’t agencies and other entities that fund these projects be less apt to fund specialized analyses? After all, it would be hard to justify their need if the studies were to languish uncited and thus have little impact. Rather than throwing the baby out with the bathwater, a realistic suggestion might be that—following some of Gobalet’s recommendations—CRM reports should provide a detailed accounting of the methods used, including the name of the analyst and the reference collection used to make the identifications. Yes, reliability is less easy to judge in the grey literature, but this shouldn’t mean we should avoid it, but rather work to ensure that the grey literature meets our standards.

We agree that it is important to be careful with data produced by others, but find Gobalet’s suggestions for moving forward unrealistic and inconsistent with his own approaches to research and publication. In any case, we understand Gobalet’s larger point, that we need to be careful with our sources. But we point out that the rules he suggests are unrealistic for our field and inconsistently applied. For example, Gobalet criticized Tushingham and Christiansen’s (2015) use of grey literature, which includes analyses by James Quinn. Gobalet notes that he recently reanalyzed fish remains from CA-NAP-39 that were “previously evaluated by Quinn who was then on the staff of the Anthropological Studies Center, Sonoma State University.” Gobalet found many problems with Quinn’s work: Quinn “identified 14 fewer species and 42% fewer specimens to at least a family 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*).”

From our reading of this passage, Quinn was less precise than Gobalet, but not necessarily less accurate. However, it is impossible for us to check this because Gobalet does not cite the CRM report that includes his

reanalysis. Indeed, Tushingham and Christensen did not have Gobalet's report when they wrote their 2015 paper; and besides, following Gobalet's criteria, we should not look at the report because it is *reported in the grey literature*. While we do not doubt that Gobalet's recent reanalysis is an improvement, we find this exceedingly ironic. Criticizing the Tushingham and Christiansen (2015) article for its citation of grey literature also seems incongruous coming from the first author of another major synthesis of fish bone in California, which relies heavily on "obscure, poorly circulated [grey] literature" (Gobalet and Jones 1995:813), including reports by Gobalet.

In any case, as highlighted by Gobalet, peer review does not always guarantee that we will avoid issues in reporting. In Gobalet's final paragraph, he states: "Had qualified individuals reviewed the papers illustrated, the issues raised here could have been avoided." This seems odd, as the peer review process of the *Journal of California and Great Basin Anthropology* involves blind review. So we ask, if reviewers are anonymous, how does he know that the reviewers of the papers he critiques are unqualified?

PROBLEMS WITH ANALYST "REPUTATION" IN DETERMINING VALIDITY

We also have general issues with a "reputation" being relied upon in peer review—especially when we already know that the process might be biased for or against particular individuals for a variety of reasons. First, if a reviewer insisted on basing a review at least partly on the reputation of an analyst, this would preclude them from refereeing papers submitted to any journal that engaged in double blind review. Second, and most critically, if the overall reputation of a researcher can be sullied as easily as discussed above, this should be taken very seriously.

Quoting Driver (1992:41), Gobalet notes: "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.'" This is indeed true, and this is why we take issue with Gobalet's blithe undermining of our work and that of another scholar, by stating that he *suspects* that one analyst (Julie Ricks) "may have limited experience" because she identified 28 Pacific herring bones (Gobalet this issue), which

are difficult (but, as he indicates, not impossible) to distinguish from sardines. Is he *really* prepared to cast doubt on all of Ricks's analyses because of this? And while we appreciate Gobalet's magnanimous judgment of Hopt's work, calling him "more experienced and better trained," is he really going to base this on the fact that Hopt was less precise in this instance? After all, as pointed out previously, he later criticizes our work, indicating that he is "suspicious" of fish-bone identifications by Hopt in Tushingham et al. (2016). To be plain—he may very well be correct about some particulars, but why not point out *potential* issues with specific details rather than casting aspersions on the entire work of another scholar? We take issue with the ethics of such sweeping and casually-stated judgments—they cast doubt on the overall accuracy and reputation of an analyst, and while perhaps not intended, ultimately can impact a young professional's career along with the scholar's chances of publishing and obtaining grants and contracts in the future. We find this particularly troubling, given that Gobalet does not have any firsthand knowledge of the collections analyzed by Ricks or Hopt, and—as we have already pointed out—Gobalet is inconsistent with his call for conservatism. Even worse, this emphasis on "reputation" really discourages people starting out in the field from going into fish-bone analysis. It is difficult enough to get people interested in such specialized and extremely challenging work, without their having to be afraid of being criticized in this manner by established experts.

CONCLUSIONS

We offer this reply with admiration for Gobalet's prodigious contributions to fish-bone analysis, as well as for his call to make fish-bone reporting "more compelling to biologists," but as archaeologists focused on understanding human behavior and the historical ecology of fisheries, our approach is more realistic for our field. We do agree with Gobalet's general call for improved reporting, the updating of current taxonomy, and the need to critically assess faunal data.

However, dismissing the grey literature is not a viable solution to analyst variation and possible identification errors. Indeed, even the peer review process is subject to human error and does not necessarily vet

problems with specific faunal analyses. And while we agree that we should promote critical inquiry, we outline several problems inherent with relying on an analyst's "reputation" to mitigate these problems. We argue that there are more constructive solutions to dealing with the analytical variation that is inherent in our discipline.

We believe that compiling large data sets is one of the most powerful means of mitigating these issues, and if we only focus on peer reviewed studies, such efforts are unnecessarily hobbled. The intent of the Tushingham and Christiansen (2015) paper was precisely to compile a large amount of macro-scale regional data for the purpose of better understanding human-fish dynamics over a long-term historical interval, and while we agree that we can improve our reporting in the future, we stand by the conclusions of our archaeological study.

ACKNOWLEDGEMENTS

We thank Colin Grier, Bill Lipe, Rick Minor, and several other colleagues for their insightful comments and review of earlier versions of this paper. Opinions and errors, however, are our own.

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