Applied zooarchaeology: the relevance of faunal analysis to wildlife management

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Abstract

Zooarchaeology has the opportunity to expand its analytical horizons into the little explored realm of modern wildlife management by applying the knowledge it gains from its unique perspective of prehistory. Ways in which animal populations threatened with extinction might be protected in perpetuity, identifying which forms or taxa should be reintroduced to which areas to recreate natural biotas, identifying which forms or taxa are exotic and should be removed from an area to create a natural biota, and helping to define the boundaries of biological preserves meant to preserve biota in perpetuity are all subjects to which knowledge gained through zooarchaeological research might be applied. The potential benefits include better informed wildlife management decisions, fewer extinctions, less loss of biological diversity and increased job opportunities for zooarchaeologists.

Keywords

Biological reserves; exotic species; extinctions; native species; reintroductions; wildlife management.

Introduction

Zooarchaeologists have, in recent years, displayed a remarkably increasing sophistication in analytical techniques and are thus providing an ever-growing set of insights into past human behaviours. Much of this sophistication has come from greater attention to taphonomic issues (e.g. Fisher 1995; Gifford-Gonzalez 1991; Lyman 1994b) and from more detailed ethnoarchaeological studies focused on faunal remains (e.g. Hudson 1993). Yet the questions we ask of the zooarchaeological record, and the problems we seek to solve with faunal data, have remained essentially constant virtually since the inception of zooarchaeological analysis (Robison 1978). The determination of past subsistence systems and paleoecological conditions remains at the forefront of modern zooarchaeology (e.g., Brewer 1992), although chronological concerns have also benefited from zooarchaeological research throughout the history of archaeology as a discipline. From Edouard Larret’s mid-nineteenth-century division of the French Palaeolithic into several mammal ages to
modern biostratigraphic analyses such as Cooke’s (1976) study of suid remains prompting redating of volcanic tuffs in Plio-Pleistocene hominid sites of Africa, zooarchaeology has contributed much more to our understanding of the past than just our recreations of ‘the interaction sphere that included people and the fauna with which they were in contact’ (Olsen and Olsen 1981: 193).

In this paper, I argue that zooarchaeologists are in a unique position to move beyond the important anthropological and archaeological issues they seem to prefer and to broaden the scope of their inquiries to include issues that are today, and will be in the future, important to humanity. Specifically, I outline here what I perceive to be a little explored avenue of zooarchaeological research – an avenue that could provide not only new sources of funding for zooarchaeologists but new sources of jobs for them as well.

Archaeology is generally taught as a part of anthropology in North America. In North America anthropological research seeks not only to increase our understanding of, but also to better, the human condition. From the anthropology of the homeless, AIDS (acquired immune deficiency syndrome) and domestic violence, to the anthropology of criminal behaviour – wherein archaeological forensics or taphonomic research plays a major role – anthropology can and does make a major contribution to making the world what we hope will be a better place. Archaeology in general, and zooarchaeology in particular, with the exception of forensic research, may appear to have little to offer in this respect. But because so many modern potential ecological problems facing humanity concern our impact on biological phenomena – destruction of the rain forest, decreasing harvests of marine fishes and the like – zooarchaeology, as well as paleoethnobotany, clearly has a major role to play in the resolution of those problems.

In the following, I describe several instances of the significance of zooarchaeological (and paleontological) data for addressing and helping to resolve particular kinds of modern problems faced by those concerned with wildlife management. Many nations have one or more forms of national park and/or game preserve. Many of these spatial units are managed in such a manner as to protect in perpetuity the biota included within their boundaries. Sometimes attempts are made to enhance or modify the biotas of these parks and preserves so that they reflect what are variously referred to as pristine ecosystems, wilderness or native (pre-industrial) or natural biotas. Further, many nations have various legal controls on hunting and trapping of animal species and legal controls over which species can be harvested and which are to be unmolested. Managing wildlife sometimes involves supplementing resident populations with individual organisms transplanted from elsewhere or attempting to re-establish species in areas where native populations have become extirpated. With its unique perspective on the time depth of ecosystems and biotas, zooarchaeological research can be applied to ensure that wise wildlife management decisions are made.

The kinds of wildlife management problems that zooarchaeological research might help resolve are many. They are, however, often very specific to a particular geographical place and/or species. The examples that follow are those with which I am most familiar, and thus centre on North America. Examples from other continents and regions surely exist. None the less, I have attempted to select examples that illustrate a broad diversity of wildlife management concerns and how particular kinds of zooarchaeological research might help resolve them.
Populations threatened with extirpation

One of the major issues facing wildlife managers working in national parks and wildlife preserves involves ensuring the wildlife species under their care survive in perpetuity. Thus, wildlife managers attempt to identify measures that can be taken to ensure that extirpation of species resident on park or preserve lands does not occur. One variable presently being debated in North American wildlife management that weighs heavily on this issue concerns what is known as SLOSS. Simply put, should a park or preserve consist of Single Large Or Several Small pieces of landscape (see, for example, Quinn and Harrison [1988] and references therein)? Thus far, the debate has employed data on the modern distribution of taxa and has typically inferred that temporal processes have caused these spatial patterns. Zooarchaeological data brought to bear on this debate can make the temporal inference empirical.

Grayson (1991) reviewed the biogeographic history of small mammals in the Great Basin physiographic province of western North America and then used zooarchaeological and paleontological data to test the implications of a model first proposed by biogeographer J. H. Brown (1971, 1978). In simplest terms, Brown’s model suggests that the modern distribution of subalpine and alpine small mammals (e.g. pika [Ochotona princeps], golden mantled ground squirrel [Spermophilus lateralis], ermine [Mustela erminea]) on Great Basin mountaintops was the result of Pleistocene colonization of those mountains followed by Holocene extinction of intermediate lowland populations and by differential extinction of various mountaintop populations. Grayson (1991: 372) found that Brown’s model ‘has impressive paleontological support’ and I note that much of that support derives from zooarchaeological research.

Grayson (1991: 373) noted that confirmation of Brown’s model with the fossil record ‘has clear implications for the management of high altitude environments in the Great Basin’. He was referring to the fact that many populations of small mammals in this area are today isolated on mountaintops. Basically following the theory of island biogeography (MacArthur and Wilson 1967) as modified over the years (e.g. Brown 1986), Grayson’s study underscored – using zooarchaeological data – the fact that such small, genetically isolated populations of species tend to be more prone to extinction than larger and/or non-isolated populations (see also Korn 1994). Larger populations provide sufficient genetic variation to ensure survival of a species, and larger areas allow migration between population nodes because corridors between them are shorter. Either isolation of those nodes from one another, or restriction of those nodes to small geographic areas – such as mountaintops – or both, increases the probability that one or more of those nodes will cease to exist. Thus the biogeographic histories of alpine small mammals of the Great Basin indicate that neither alternative of SLOSS is workable here. We cannot enlarge the insular alpine and subalpine habitats found on Great Basin mountaintops; boundaries of those habitat patches are climatically dictated. And these habitat patches are not only small, they are also multiple in number. Ensuring that these patches are not artificially disrupted in the future would seem to be the only way to guarantee survival of their included small mammals. Migration corridors between the habitat patches cannot be artificially constructed or maintained. If such corridors could be built, where should they
be located? The fossil record might help us determine where these corridors existed in the past, as can be illustrated with another example.

The pygmy rabbit (*Brachylagus idahoensis*) is a diminutive leporid with a modern range restricted to two areas of the western United States. One area is relatively large and encompasses portions of four states: northern Nevada, western Utah, southern Idaho and southeastern Oregon. The other historically documented area where this species occurs is the central portion of eastern Washington state. Populations in the two areas are presently isolated from one another. The wildlife management concern here is that the eastern Washington population has, over the past forty years, shrunk to an alarmingly small size, prompting the Washington Wildlife Commission to list this population as ‘threatened’ and to suggest that it be listed as ‘endangered’ (McAllister and Allen 1993), and to develop a management plan with the aim of ensuring survival of the population (McAllister and Allen 1994). The zooarchaeological record of this species provides insight to why this population is threatened with extirpation and how it might be preserved.

The zooarchaeological record for pygmy rabbits indicates this species probably occupied a wider range in central Washington during the Holocene than it presently does. Remains of this species have now been recovered from eleven archaeological and two paleontological sites in the area (updated from Lyman 1991). Some of these are extralimital records; that is, some remains have been recovered from geographic locations where pygmy rabbits have not been historically documented to occur. In brief, the fossil (botanical and faunal) record suggests that when big sagebrush (*Artemisia tridentata*) was more widespread in central Washington during the middle Holocene climatic interval known as the Altithermal – pygmy rabbits were also more widespread. When the Altithermal ended about 4,500 years ago the range of sagebrush shrank, and so too did the range of pygmy rabbits. Then, during the late nineteenth and early twentieth centuries, land was cleared of sagebrush for agricultural purposes. This prompted a second diminution of sagebrush and pygmy rabbit range.

Ecological studies of pygmy rabbits indicate that this leporid is dependent on big sagebrush for food and for shelter from predation (Green and Flinders 1980a, 1980b). The zooarchaeological record, in conjunction with paleobotanical data such as that from pollen records, confirms this, as both species appear to have responded to climatic change in like manner. Washington Department of Wildlife biologists have made use of both modern and zooarchaeological data in their planning efforts (McAllister and Allen 1993, 1994). The fossil record suggests that the prehistoric source of Washington’s pygmy rabbits resides in the area of the larger extant population to the south. Late Pleistocene fossils of pygmy rabbits recovered from a paleontological site between the two areas (Rensberger and Barnosky 1993) tend to confirm this suggestion. While it would probably be impractical to develop a migration corridor between the two populations today, it would also be impossible given the climatic history of the area. The probable corridor between the two today is characterized by vegetation habitats that are not conducive to the survival of pygmy rabbits. The zooarchaeological record does indicate, however, that the extant population might be supplemented by individuals transplanted from southeastern Oregon and that the maintenance of habitats dominated by big sagebrush is critical to the survival of the species.
When managing small isolated populations, we may not have many options other than to preserve those populations as they presently exist. In some cases, management efforts have failed and populations have been extirpated. When this occurs, one management alternative is to transplant individuals from other populations to the vacated range in an attempt to re-establish a population. Zooarchaeological research can also play an important role in these situations.

Re-establishment of extirpated populations

Eighteenth- and nineteenth-century commercial exploitation of sea otters (*Enhydra lutris*) along the western coast of North America ultimately led to the extirpation of this species from much of its range (Kenyon 1969; Ogden 1993; Roest 1973). Attempts to re-establish populations along the middle latitudes of that coast took place in the 1970s and thus far have met with less than perfect success (Jameson et al. 1982, 1986; MacAskill 1987). The typically expressed reason for the limited success of these efforts is that ecosystems to which sea otters have been transplanted have been modified too extensively by modern human industry to support them. I wonder if, however, part of the reason might reside in a factor that only zooarchaeological research can measure.

Sea otter skulls collected from much of the once occupied eastern Pacific range of sea otters display various character gradients from the Aleutian Islands to southern California. Some of these characters have been used to argue for or against the existence of multiple subspecies of sea otter (Davis and Lidicker 1975; Roest 1973, 1979; Scheffer and Wilke 1950; Wilson et al. 1991). Final resolution of this debate will be critical to future wildlife management activities, first because national legislation in the United States concerns species, subspecies and populations (O’Brien and Mayr 1991) and also because of conflict between one population (a possibly distinct subspecies) of sea otters and the modern fisheries industry (Wendell et al. 1986). The significant aspect to all of this is the transplanting of sea otters. Legally, only forms (ecotypes or subspecies) native to an area should be introduced there during efforts to re-establish populations. This also makes biological sense as it is likely that those native forms unique to an area are the product of millennia of evolution.

The International Union for the Conservation of Nature and Natural Resources – IUCN (recently renamed the World Conservation Union) – has drafted guidelines for such reintroductions. These read, in part,

An assessment should be made of the taxonomic status of individuals to be re-introduced. They should be of the same taxonomic unit (and ideally closely related genetically) as those which were extirpated. An investigation of historical information about the loss and fate of individuals from the re-introduction area, as well as molecular genetic studies, should be undertaken in case of doubt. . . . Release stock ideally should be closely-related genetically to the original native stock.

(Anonymous 1992: 2-3)

Clearly, a search of not only historic but also prehistoric information may be necessary. The rapidly developing study of ancient DNA extracted from prehistoric skeletal tissues
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(e.g. Richards et al. 1993, 1995, and references therein) seems to have great potential for contending with IUCN guidelines. The technique is applicable to curated as well as newly acquired archaeological specimens. It has, however, not yet been applied to Oregon coast sea otter remains.

Given the lack of genetic material from eighteenth- and early nineteenth-century sea otters on the Oregon coast, the problem becomes one of establishing the age of the character gradients displayed by eastern Pacific rim sea otters (see Wilson et al. 1991 for a recent summary) and determining if those gradients have shifted spatially through time. If those gradients have a prehistoric distribution in time and/or space similar to that documented using historic specimens, a potential problem is raised. If a particular form—represented by a particular character state—of sea otter was present prehistorically in one location within the range, and another form was present at another location, then transplanting individuals from the former to the latter may be doomed from the start. In such a case, failure to re-establish a population could result, on the one hand, because the transplanted form is not well adapted to its new environment; the extirpated form, on the other hand, is adapted to the environment of that location, hence its difference in form.

Several years ago I examined prehistoric sea otter remains recovered from archaeological sites on the coast of the state of Oregon—the middle latitudes of the historically documented range of sea otters (Lyman 1988b). The size of sea otter teeth recovered from those sites are more or less intermediate to those from Alaska (large) and those from southern California (small), but they also vary somewhat from this simple pattern. Prehistoric Oregon sea otters match modern Alaskan sea otters in some characters; in others, they match modern California sea otters. Precise details are unnecessary to the point I wish to make here, which is: Were the sea otters captured in Alaska and translocated to the Oregon coast in the 1970s of the proper phenotype (or genotype)? Zooarchaeological evidence suggests they were not. If this is a correct interpretation, then perhaps those transplanted sea otters were doomed from the moment they were captured. Whether or not my suggestion proves to be true or false, and it is unclear which way the cards might fall given that we still have much to learn about this species (VanBlaricom and Estes 1988), the significance of zooarchaeological data should be clear.

Not only can zooarchaeological data inform us as to which form of a species might be the most appropriate one to transplant to an area, such data have the potential to indicate the potential success of such transplant efforts. Even if the appropriate form to transplant is clear, this does not guarantee that the introduced form will survive in its new home. This is precisely the argument offered by biologists working with sea otters on the Oregon coast. The data substantiating their argument can be supplemented with zooarchaeological data. An example of this is provided by the California condor (Gymnogyps californianus) of western North America, a species presently near extinction (Emslie 1987). Zooarchaeological and paleontological data suggest this species enjoyed a wider range during the late Pleistocene, but that range shrank considerably with the onset of the postglacial Holocene epoch (Emslie 1987; Simons 1983). Those data also suggest that this species was a carrion feeder, and that the terminal Pleistocene extinction of many genera of large mammals probably contributed to the decline in frequency and range of the California condor (Emslie 1987). Finally, paleoecological data indicate attempts to reintroduce this species to portions of its prehistoric range would probably fail because of major differences
between late Holocene floral and faunal communities and late Pleistocene communities (Emslie 1987).

In the absence of historic data, or even with it (see below), zooarchaeological information may be critical to ensuring that the correct forms of animals are transplanted. The wrong forms may be ill suited to their new habitats, or they may be well adapted and proliferate in their new homes. Of course, proliferation of artificially introduced species may, on the one hand, be beneficial and what is desired. On the other hand, as I think many of us are aware, it may not be desired. And the latter brings us to another kind of use that can be made of zooarchaeological data when wildlife management is the issue.

**Identifying exotic animals**

Recalling the IUCN’s guidelines for reintroductions, if a particular form of organism (ecotype, subspecies or species) is exotic to a park or wildlife preserve, it may be necessary to remove it from the area in order to preserve the natural status of the ecosystem and the biota of the area. Thus, we must have a solid definition of *exotic species*. The United States National Park Service (NPS) defines an exotic species as one ‘that occurs in a given place as a result of direct or indirect, deliberate or accidental actions by humans (not including deliberate reintroductions)’ (Hester 1991: 127). In contrast, *native species* ‘are those which presently occur, or once did occur prior to some human influence, in a given place, area, or region as the result of ecological processes that operate and have operated without significant direct or indirect, deliberate or accidental alteration by humans’ (NPS documents cited in Lyman 1988a: 21; see also Wagner et al. 1995). Clearly, then, if wildlife managers are to manage native ecosystems – variably referred to as pristine ecosystems or wilderness – by exclusion or removal of exotic species and reintroduction of artificially extirpated native species, then they must have that baseline list of native species. In the United States, that list is derived from the earliest historical documents for the area included within a national park (e.g. Houston and Schreiner 1995; Leopold et al. 1963; see also Hoerr 1993). The ethnocentrism of such a procedure is, as might be imagined, unavoidable from a practical standpoint. More importantly, historical documents are sometimes incomplete and are sometimes simply inaccurate. Those written documents can, of course, be supplemented with another kind of document – the zooarchaeological record. Here, I mention two examples of such supplementation.

**Bison** (*Bison bison*) were artificially introduced to a portion of the state of Alaska that was to subsequently become Wrangell-St. Elias National Park and Preserve (Peek et al. 1987). By definition, these introduced bison should be considered exotic and are so considered by the NPS (Houston and Schreiner 1995). However, zooarchaeological evidence indicates that bison were present in the area until at least 500 years ago or slightly later (reviewed in Peek et al. 1987). Historic records suggest bison were not present when the first white men visited the area in the middle and late nineteenth century, but non-fossilized bison skulls were collected from the area early in the twentieth century (Peek et al. 1987). Given the paucity of late Holocene zooarchaeological data in the area, and the unknown nature of the relevant fossilization processes, it is unclear if bison were present in Alaska between about 450 years ago and the early twentieth century (McDonald 1981).
The NPS considers the bison of Wrangell-St. Elias National Park to be exotic due to the lack of evidence that bison were present when the first white explorers passed through the area (Houston and Schreiner 1995). As well, NPS biologists note that the introduced form of bison was of an incorrect – non-native – genetic stock (a distinct subspecies or ecotype), given current beliefs about bison taxonomy. For this reason alone, perhaps the extant bison of Wrangell-St. Elias should be removed. However, the zooarchaeological record is so poorly known that one cannot conclusively demonstrate that ‘native’ bison were locally extinct by the beginning of the twentieth century. This in turn raises the possibility that extant bison are hybrids of native and introduced genetic stocks. Whether or not this possibility is in fact reality is irrelevant. What is critical here is that only more zooarchaeological and paleontological research will establish, first, the timing of local extirpation of native Alaskan bison populations and, second, if all Alaskan populations were extinct when transplanting occurred in the middle of the twentieth century. Once the facts are determined, we will have strong bases for making a decision regarding the ultimate fate of the bison presently living in Wrangell-St. Elias National Park and Preserve.

I am much more familiar – and intimately involved – with a virtually identical situation that exists in western Washington state. Here, mountain goats (Oreamnos americanus) were introduced in the 1920s to the Olympic Mountains. A decade later, in 1938, these mountains were designated Olympic National Park. The present position of the NPS is that the mountain goats are exotic, and plans are, in mid-1995, under way to eradicate them. The plan is controversial (Anunsen and Anunsen 1993; Scheffer 1993), as is the evidence mustered to support it (compare Houston et al. [1994] with Lyman [1994a, 1995a, 1995b]). This is not the place to review that evidence in detail (see references cited). It is relevant, however, to note the significance of zooarchaeological evidence for final resolution of the controversy.

The historical evidence concerning the pre-1920s presence of mountain goats in the Olympic Mountains is ambiguous. Some documents indicate goats were absent, while others indicate goats were present (Lyman 1994a, 1995b). In fact, the only way to demonstrate that mountain goats were present, or absent, from the Olympic Mountains prior to their 1920s introduction requires paleontological and/or zooarchaeological data. At present, there are no paleontological data of any kind for the area, and the zooarchaeological record is woefully inadequate and thus cannot serve as a basis for a management decision (Lyman 1995a). Park biologists note that the purpose of national parks in the United States is not ‘to attempt to recreate late-Pleistocene or early Holocene biotas. [The NPS] is primarily concerned with historic, post-Columbian species distributions’ (Houston and Schreiner 1995: 207). The facts remain, however; the zooarchaeological record is poorly known and the historic record is internally contradictory.

From the perspective of dynamic biogeographic histories provided by zooarchaeology, one thing is abundantly clear: the definitions of native and exotic species used by the NPS to implement its policies are contradictory. Exotic species occur in a given place as a result of actions by humans, whereas native species are those which presently occur, or once did occur prior to some human influence, in a place as the result of natural ecological processes. The italicized phrase is where the contradiction resides, because it indicates that if, for example, mountain goats occurred at any time in the past in the Olympic Mountains
or bison in Wrangell-St. Elias – then by definition the extant individuals represent a native species. But the introduced species are also, by definition, exotic; having been transplanted from northwestern Canada and southeastern Alaska, Olympic National Park’s mountain goats are of a different genetic stock, and thus are a form distinct from mountain goats that may have been in the Olympic Mountains 200 years ago. Recognizing this contradiction some years ago, I argued that if the prehistoric remains of mountain goats should be found in the Olympic Mountains, then the NPS ‘should rethink policy issues’ (Lyman 1988a: 22). Some NPS biologists have only recently come to the same conclusion (Houston and Schreiner 1995). And given the discussions above of appropriate forms (ecotypes, subspecies, species) and the benefit of hindsight, I would argue that the policies and definitions of key terms should also be reworded to reflect the reality of wildlife management practice (Anonymous 1992; O’Brien and Mayr 1991). Thus, given their perspective of time depth, zooarchaeologists not only can contribute significant paleobiological information to wildlife management concerns, but perhaps they can also help write workable policies (see Wagner et al. 1995 for other examples). Paleobiological information provided by zooarchaeological research may also help wildlife managers plan for the future, as the final example demonstrates.

**Biological preserves and planning for the future**

Thus far the examples of zooarchaeology applied to wildlife management concerns have focused on the use of information on prehistoric faunal dynamics to inform management decisions. That same kind of information can also be used to make broad predictions about the future effects of certain kinds of decisions. For instance, as alluded to in the first example presented above, the spatial boundaries of parks and wildlife preserves may have significant implications for the long-term survival of populations found within those boundaries. In an important contribution to the role of planning for the future on the basis of the past, Graham (1988) notes that the boundaries of many parks and preserves are defined based on modern climatic and environmental conditions. However, those conditions, as we all know, change, and the probability of significant change increases as the length of time considered increases. Graham underscores the fact that much planning to preserve terrestrial biological diversity operates under the assumption that biotas or communities of organisms tend to respond to environmental change as intact units. The paleobiological record indicates, however, that ‘individual species respond to environmental changes by migrating in different directions, at different rates, during different times’ (Graham 1988: 392). Such individualistic responses to environmental change mean that the biological preserves of today are artifacts of the time when they were identified and created. Thus, Graham (1988: 393) argues that ‘constructing future reserves as habitat islands without avenues of migration [corridors] would probably result in [extirpation of bounded populations of managed species].’

Graham (1988) thus suggests that, as in the example of the Great Basin’s mountaintop mammals, migration corridors are critical to organism survival and that preserves must be located on the landscape in such a manner as to be conducive to inter-areal migration. I suggest, following my earlier discussion, that knowing where those corridors were located
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in the past surely will help in locating future preserves. And, as Graham and Graham (1994) indicate, detailed knowledge of the historical and ecological biogeography of a taxon could prove critical to reconstructing appropriate habitats for reintroduced populations of the taxon. Such knowledge can often come only from the fossil record.

Extinction – of populations, of species, of lineages – is a natural fact of life (e.g. Raup 1984). But ignorance resulted in the loss of the passenger pigeon (Ectopistes migratorius) from North America, the Great Auk (Pinguinus impennis) from the North Atlantic and the dodo (Raphus cucullatus) from Mauritius in the Indian Ocean. Our often stumbling attempts at wildlife management may, due to ignorance, ultimately lead to other such losses (e.g. Graham 1988; Hess 1993; Kay 1994). The international move to maintain biological diversity – not just the taxonomic richness of biotic communities, but the particular structure of those communities – seems to be in the correct direction if we are to maintain some quality of life and wilderness. Understanding the loss of diversity often demands zooarchaeological and related data (e.g. Culotta 1995; Steadman 1995), and will help us avoid future inadvertent and unintentional losses. Diamond’s (1984) Rosetta Stone of using knowledge of modern extinctions to understand prehistoric extinctions could, and perhaps should, be turned on its head.

Discussion

Nearly eighty years ago, archaeologist W. J. Wintemberg (1919) noted how zooarchaeological research could contribute to zoological knowledge and, indirectly, wildlife management. His comments were repeated and elaborated thirty years later by biologist R. M. Gilmore (1949). Their remarks have largely gone unheeded, perhaps because, like Legge (1978: 129, 130), many of us see our job as ‘aiming to contribute to the sum of the archaeological knowledge’ and that ‘bone data are archaeological data’. I agree with Legge, but also with Wintemberg and Gilmore: we should be more than just consumers of wildlife data (cf. Will 1982).

The potential topics under the broad umbrella of wildlife management to which zooarchaeological research might contribute are numerous. I have touched on only a few of the more readily identifiable ones above. There are, to be sure, others of some significance. For example, the modern fisheries industry of the eastern Pacific Ocean has been disrupted several times in the recent past by major changes in atmospheric circulation patterns causing alteration of ocean currents. These events – variously referred to as El Niño or Southern Oscillation events – appear to have some periodicity (Rasmusson 1985), but the historic record is of limited utility in plotting the precise nature of that periodicity. These events clearly influence much of the oceanic biota (e.g. Arnold and Tissot 1993; Trillmich and Limberger 1985). Given sufficient temporal resolution, the zooarchaeological record thus may eventually provide the time depth necessary to clarify the periodicity and magnitude of El Niño events and thus allow more accurate predictions of future events.

Zooarchaeological research has also shown that humans have long had a hand in modifying ecosystems. Based on his extensive research on the prehistoric extinctions of Pacific Island bird populations and species, Steadman (1995: 1130) concludes that humans
may find ‘it difficult to manage scarce resources in a sustainable way’. That humans contributed to these extinctions seems clear (e.g. Weisler and Gargett 1993), and thus Steadman’s conclusion has the proper ominous tone. But zooarchaeological research can also perhaps help us out of such situations. The wapiti (Cervus elaphus) of Yellowstone National Park in the United States are an example. Here, nearly 60,000 wapiti are managed and to a significant degree protected from human predation. However, historic and zooarchaeological research indicates that this population is a function of modern wildlife management; historic records virtually without exception fail to mention wapiti in the area, and zooarchaeological data indicate this large ungulate was very rare there during the last several thousand years (Kay 1994). Clearly, if the Yellowstone ecosystem is to be managed and maintained as a pristine wilderness in perpetuity (a laudable but in reality unattainable goal; see Wager et al. 1995), things must change before the ecosystem is irreparably altered. A virtually identical situation exists in Rocky Mountain National Park some 550 km southwest of Yellowstone (Hess 1993) where, again, zooarchaeological and other data from prehistoric times might help resolve debates.

The few examples of how zooarchaeological research can be applied to modern problems I have outlined are rather specific to a place and/or to a taxon. I do not perceive that to be a weakness of an ‘applied zooarchaeology’ but rather as an intellectual challenge. Broadening the scope of zooarchaeology in the manner in which I have discussed will not be easy, but it will be beneficial to our future, not only from the perspective of contributing to ensuring the preservation of biological diversity for future generations, but also, as I conclude below, from the perspective of our discipline, which might otherwise become increasingly perceived as the pursuit of esoteric knowledge of little practical use.

One might argue that the discussion here could be expanded by suggesting that the detection of animal residues such as blood and hair (e.g. Loy 1993) could also provide information on the taxonomic identity of species native to the locale where the tools were collected (e.g. Hyland et al. 1990). I doubt, however, that such a study would convince wildlife managers or all zooarchaeologists that a species was native given that stone tools were often transported significant distances by their makers (e.g. Meltzer 1995). I believe multiple instances of such residues, from multiple sites, would be necessary to build a strong argument. A partial skeleton or selected isolated bones and teeth of the species in question, recovered from a single site, would probably be much more convincing (Lyman 1994b).

Conclusions

I have attempted here to describe the structure of an ‘applied zooarchaeology’. This is not a plea to legitimize what some of us do for fun and/or a living. Rather, it is an attempt to broaden the scope of zooarchaeological research. I am all for research for the sake of research if there is a problem in need of solving or a question in need of an answer. But I am also concerned with the future of zooarchaeology. Too many of our new doctoral students are finding that the job market is closed, and too many of us are finding it increasingly difficult to secure funding for research. Perhaps a developed applied zooarchaeology will
provide a new job market and new sources of funding. It seems self-evident to me that if a national park, for example, has a staff archaeologist who deals with the archaeological resources on park lands, and has a staff biologist who deals with biological resources on park lands, then a marriage of the two would benefit both. However, given the increasing specialization of both archaeology and biology, a viable marriage may not be possible. The obvious alternative is a park staff zooarchaeologist who has a deep knowledge and understanding of both disciplines and has been trained to integrate the two.

New sources of funding may also become available if we can convince the wildlife management community that, because its decisions and policies are typically aimed at the future, knowing something of the past can result in wise decisions. That knowledge will, of course, come with a price tag. The cost we may encounter is that members of the wildlife management community – while they may well have a solid knowledge of the time depth of evolutionary processes – often tend to view biotas as static, ever enduring entities. That is, biotas have no past or at least their history is insignificant because they are being managed for the future. While we certainly cannot predict exactly when the next ice age or glacial period will begin, we can, I think, argue convincingly that climates and environments will change and, knowing this, we can use the prehistoric record to test our predictions about how certain kinds of changes may affect biotas of the future. And confirmed – or even rejected or falsified – predictions would be the strong selling point. Federal land-managing agencies whose charge includes wildlife management might well pay a zooarchaeologist to help them make wise management decisions. The other selling point is that a less desirable kind of price – ecosystem destruction and loss of biodiversity – might otherwise have to be paid.

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References


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