1

Applied Zooarchaeology, Because It Matters

R. Lee Lyman and Kenneth P. Cannon

> A knowledge of local archeology and history should be a part of the ecologist's equipment.
>
> —G. M. Day, "The Indian as an Ecological Factor in the Northeastern Forest" (1953:343)

At the beginning of the 21st century humanity faces a dilemma of its own creation. Global change wrought by a continuously growing and ever more resource-hungry human population is the most obvious symptom. The dilemma comprises the fact that the world's ecology is being anthropogenically altered, and it is unclear whether humans will be able to survive the alteration (Palumbi 2001; Vitousek et al. 1997). If we survive, what will our "quality of life" be like? Will there still be wild places to visit? Will people still be able to "get back to nature" on a weekend camping trip? Is the loss of biodiversity inevitable, and will such a loss be deleterious to ecosystem structure and function, as well as to humanity? These are pressing and significant questions, and this book is about one of the seldom-noted ways we can go about building answers to some of them. It grew from a discipline known as zooarchaeology—the study of animal remains recovered from archaeological excavations. We use the term *paleozoology* to refer to zooarchaeological and paleontological data.

During the early and middle decades of the 20th century Americanist ecologists and biologists explicitly noted the value of zooarchaeological data for addressing various wildlife-management concerns and conservation issues (Gilmore 1949; Wintemberg 1919). Over the next several decades virtually no one pursued this potential source of data with the explicit intention of addressing such concerns. In the middle of the 20th century a few biologists used zooarchaeological data to measure the influence of First American peoples on wildlife populations (Elder 1965;

Simenstad et al. 1978). At the same time, at least one paleobiologist suggested that Late Quaternary fossils should be consulted in the interests of conservation biology (Martin 1970), but most zooarchaeologists did not address what might be gained from applying their (or paleontological) data to biological conservation issues. Their silence with respect to the latter largely remained in the 1970s and 1980s, even though more zooarchaeologists turned from strictly archaeological and anthropological concerns to questions of prehistoric ecology and biogeography (Graham 1985; Grayson 1976, 1977, 1981; Gustafson 1968; Lyman 1983, 1986; Lyman and Livingston 1983; Parmalee et al. 1980, 1982). This turn spawned a new set of questions at the same time that the apparent crisis presented by an anthropogenically altered global ecology was becoming clear.

By the middle 1980s it had become obvious that paleozoological data, whether derived from archaeological or paleontological contexts, are valuable for the information they represent with respect to ecosystems and how those ecosystems have changed over time (Graham 1988, 1992; Grayson 1987; Livingston 1987; Lyman 1988a, 1988b; Parmalee and Klippel 1984), and these new research avenues came to be more frequently pursued. At the same time, ecologists began to ask paleozoologists to contribute what they knew to overviews of various taxa (Graham and Graham 1994) and of faunal-management problems (Graham 1992). A few years later, paleozoologists began to write explicitly about what their data indicate regarding prehistoric anthropogenic effects on ecosystems (Steadman 1995), and they began to argue with conservation biologists and wildlife managers about the significance of data on prehistoric conditions for making modern wildlife-management decisions (Houston and Schreiner 1995; Lyman 1994a; Scheffer 1993). Sometimes zooarchaeological data have suggested that prehistoric humans were an incredibly significant agent of ecological change in the past (Boesch et al. 2001; Grayson 2001; Kay 1994; Peterson et al. 2001), although this is clearly an empirical matter that must be evaluated on a case-by-case basis (see, e.g., the debate between Lyman [1988b, 1989, 1995c, 2003b] and Hildebrandt and Jones [1992, 2002; Jones and Hildebrandt 1995] and examples in Sarkar 1999).

Perhaps the most significant outgrowth of the new questions asked of faunal remains regarding the nature of prehistoric ecosystems has emerged in the last decade. During that time several zooarchaeologists offered explicit commentary about the value of their data to ecological management decisions, particularly those pertaining to faunas (Amorosi et al. 1996; Barker 1996; Grayson 2001; Livingston 1999; Lyman 1994a, 1996, 1998). It was with this recognition firmly in mind that we developed a plan to produce a book of case studies in which specific zooarchaeological data are brought to bear on particular ecosystem management concerns and conservation issues. Because the concerns and issues are as disparate as the faunal taxa involved, the book could not be authored by a single individual or even by several individuals. Rather, the requirements demanded that multiple authors—each with specific knowledge not only of a particular set of zooarchaeological data but also of a particular wildlife-management or conservation biology issue—be asked to contribute. Many of the following chapters were originally solicited for presentation at the 67th Annual Meeting of the Society for American Archaeology in Denver (March 2002). Additional essays were solicited to increase geographic, taxonomic, and topical coverage.

Authors were instructed to identify a management issue of concern, to describe and analyze relevant zooarchaeological data, and to offer recommendations as to possible resolutions of the management or conservation issue. Authors were to specify how prehistoric data might make for better-informed decisions regarding faunal ecosystem maintenance or restoration. Simply stating in an essay that zooarchaeological data are relevant to a conservation problem was an insufficient warrant for its inclusion here. Similarly, simply documenting that the structure of a prehistoric ecosystem was different from a modern one was insufficient. Some of the following chapters specify a pressing and particularistic management or conservation problem. The authors of these chapters react to those problems by outlining potential management efforts that zooarchaeological data suggest will produce the desired solution. Other chapters are more proactive in the sense that a pressing critical problem is not identified but, rather, one or more management concerns that do not yet require immediate attention are described and possible ways to resolve those concerns are derived from relevant zooarchaeological data. We believe that such reactive and proactive uses, respectively, of paleozoology's unique data are equally pertinent to modern conservation biology. And we contend that it would be a sad state of affairs indeed were all management efforts simply reactions to immediate crises.

We are archaeologists by training, and we have found this chapter equally exciting and challenging to write. This is so because we have had to walk a fine line between offending and patronizing either or both conservation biologists and zooarchaeologists. Despite our efforts, we will probably do a bit of both with respect to both sets of professionals. This is not a book of case studies aimed at archaeologists, although we think that they and other paleoecologists will find some things of interest. Our introduction, for example, is written largely with paleoecologists rather than environmental managers and conservationists in mind. The latter may therefore find it sophomoric or pedantic, but we offer it in the hope that it will provide insight to what at least some paleoecologists think about how paleoecological data can be applied in modern management settings. It can also serve as a primer for paleozoologists who are unfamiliar with the basics of conservation biology and wildlife management. This is a book about past ecosystems, particularly the faunal aspects of them, and how knowledge of those ecosystems is of value to those who contend with global change. The chapters are in no topical order; all of them are equally important and significant, so they are presented in the order of the authors' alphabetized names.

This book is not a set of case studies that can be categorized within the field known among anthropologists, geographers, and environmental historians as historical ecology (Balée 1998; Crumley 1994b). That field focuses on the fact that humans, throughout their history on the planet, have not just adapted to the earth's various environments but, rather, there has been a dialectic, a constant interaction between human cultures as adaptive systems and nature (Crumley 1994a). Each influences and responds to the other. Anthropologists have long recognized that this interaction has been ongoing virtually since our early hominid ancestors used culture (tools and learned behaviors) as a nongenetic means of adaptation (e.g., Heizer 1955), but until recently the basic notion has been that humanity's influence on the environment was minimal until, say, the last several thousand years; we now know that this is simply false (Redman 1999). Culture, no matter how primitive or sophisticated, is modified in response to environmental change, and environments are in turn modified to one degree or another by human-wielded culture. The literature on this topic is increasingly large, but to briefly summarize the underlying epistemology, the historical ecology viewpoint has made explicit the fact that modern environments are historical phenomena; they are a function of historical and evolutionary events and the order in which they have occurred (e.g., Russell 2003; Winterhalder 1994). Applied zooarchaeology explicitly adopts this epistemology and similarly explicitly acknowledges the anthropogenic effects of humans on environments, particularly the faunal portion of the environment. But applied zooarchaeology also goes beyond documenting the history of the dialectic between humans and the environments in which they live and attempts to use that historical knowledge to assist in ensuring the future of both humanity and the environment (Lyman 1996).

All studies included here are based on data from North America because that is the area where we work; we are familiar with the biology and geography, and we know many potential contributors working on the continent. Despite this geographic focus, we believe that we have compiled a number of exemplary contributions to what has been termed "applied zooarchaeology" (Lyman 1996). We agree completely with the implications of the title of Virginia Butler and Michael Delacorte's contribution (chap. 2) but have altered it a bit for the title to this introductory chapter. Not only *might* zooarchaeology matter in arenas other than archaeology, it simply *does* matter in many cases; and this constitutes the short description of applied zooarchaeology as well as the goal of this book—to show that zooarchaeological research *matters*. Later in this chapter we outline what exactly applied zooarchaeology entails by providing an overview of some of the kinds of issues that can be addressed under this rubric. First, however, it is necessary to consider several conceptual issues that are critical to the discussions found in all of the chapters. Some of the following will seem simplistic and superficial to resource managers, but paleoecologists and zooarchaeologists are not always familiar with the nuances of conservation biology. What follows is our take on some of those nuances.

CONCEPTUAL ISSUES

Various concepts must be clearly and explicitly defined if we are to determine what ecological restoration and ecosystem health comprise (Anderson and Dugger 1998; Falk 1990; Higg 1997; Huff 1997; Rees 2001; Scherer 1994; Smith et al. 1993; Stanturf et al. 1998; T. Young 2000). Conservation biologists do not always agree on basic ecological concepts within their own discipline (Angermeier and Karr 1994; Hall et al. 1997; Sarkar 1999). Two fundamental problems therefore attend various key concepts that underpin conservation biology and wildlife management, concepts such as sustainability, conservation, preservation, biodiversity, and integrity (of ecosystems). First, the concepts attending conservation are variously value laden (Callicott and Mumford 1997; Jepson and Canney 2003; Lélé and Norgaard 1996; Ludwig et al. 2001). The source of the value can reside in ecological or biological theory, the economics of resource exploitation, a personal or policy-dictated vision of "nature" or "natural," or some combination of these or other ecological, social, political, and economic variables (Bennett 1994; Doak and Mills 1994; Hull et al. 2003; Lawton 1997; Meffe and Viederman 1995). A recent book refers to these aspects of conservation biology as "political ecology," defined as those instances when ecological data are selected "to support preordained philosophical values or political agendas" (Kay and Simmons 2002a:xiv–xv).

The second problem is intimately related to the first and comprises the fact that there are no generally agreed-on definitions for many key conservation concepts precisely because virtually any definition carries with it some implied value (Angermeier 1992; Callicott et al. 1999; Hull et al. 2003; Margules and Pressey 2000; Noss 1983, 1990). An excellent example of the context-specific nature of the value concept resides in changes in the management goals of national parks in the United States. These changes have tracked shifting sociopolitical climates as well as modifications in ecological theory and resulted in alterations to various policies and management activities over the nearly 100 years the National Park Service has been in existence (McClelland 1998; Sellars 1997; Wagner et al. 1995; Zube 1996). For example, the bison herd of Yellowstone National Park was rescued from near extinction when the park was created in the late 19th century. Management initially involved keeping the local native herd separate from another herd made up of bison procured from private herds. These two herds were eventually allowed to interbreed, and as the total population grew, culling became a common practice. In response to the Leopold Report (Leopold et al. 1963)—one of the founding documents of modern wildlife management—Yellowstone managers shifted policies dramatically and adopted a noninterventionist approach to natural resource management. Park managers and wildlife personnel now rely on natural processes to effect change and control the sizes of bison and other wildlife populations (Keiter 1997; Schullery et al. 1998). This "natural regulation" of wildlife populations has been referred to as the "great experiment" by some Park Service personnel, and over the past decade it has come under critical scrutiny (Wagner et al. 1995).

It is not our intent in this book to resolve slippery and often contentious terminological and conceptual issues, let alone policy issues. The reality of conservation biology simply is that multiple factions have varied interests in the outcome of management efforts. Hunters will want a population with maximum harvest potential—that is, large—and a high proportion of trophy animals. Farmers may want small populations that cause minimal damage to crops and fences and which do not compete for open grazing range with livestock. Avocational naturalists and wildlife photographers may want many animals to observe and photograph closeup in nonurban settings. City dwellers will likely not want wild animals in their backyards or on the highway to the office. These and a plethora of other conflicting values, not to mention the shortages of funding and personnel, make conservation biology a challenge, and we do not envy those who have chosen it as a way to make a living.

Given the geographic focus of this book on North America, there is one concept that warrants detailed consideration. Discussions of what exactly a "natural" or "pristine" ecosystem—sometimes termed "wilderness"—comprises have occupied much space in various publications (Anderson 1991; Angermeier 2000; Callicott 1995; Dobb 1992; Foreman 1995; Guthrie 1971; Hoerr 1993; Hunter 1996; Landres et al. 2001; Lowenthal 1964; Maser 1990; Noss 1995b; Povilitis 2002; Scott 2002; Sloan 2002; Truett 1996; Wagner et al. 1995). Wildlife biologists in particular have grappled with conceptions of pristine/natural (Houston and Schreiner 1995; Meine 1999; see also the essays introduced by Flores and Bolen 1995). Although it was recognized much earlier (Day 1953), over the past two decades or so an increasing number of geographers, historians, and biologists have acknowledged that there is no post-Pleistocene (< 10,000-year-old) ecosystem or landscape in North (or South) America that is natural or pristine in the sense of simultaneously being both immediately pre- or post-Columbian and unmodified by human activities (Bonnicksen 1989; Denevan 1992; Gómez-Pompa and Kaus 1992; Rolston 2001; Schullery 2001; Shrader-Frechette and McCoy 1995; Sprugel 1991; Vale 1998; Wright 1974; see also the chapters in Kay and Simmons 2002b).

The initial goal is to identify an ecosystem that management and conservation efforts then seek to re-create and maintain. It is thus critical to recognize that as time passes, ecosystems change for myriad reasons other than (as well as in addition to) human or anthropogenic influences (Botkin 2001; Dickinson 1995; Lawton 1997; Sprugel 1991; Todd and Elmore 1997). Most of our perceptions of ecosystems come from post-Columbian observations, and much of what has been perceived during the last 500 years is a result of climatic history (Hewitt 2000). Three significant changes in North American ecosystems are known to be the direct result of Euro-American colonization of the continent. One change resulted from the fur trade that decimated some populations of beaver (*Castor canadensis* [Johnson and Chance 1974]), sea otter (*Enhydra lutris* [Ogden 1933]), and other fur-bearing taxa in the late 18th and early 19th centuries. In this case, of course, Euro-Americans often had the help—sometimes willing, sometimes coerced—of First American peoples. The horse *(Equus caballus)* was introduced to North America in the 17th and 18th centuries (Haines 1938a, 1938b); this resulted in significant changes not only to First American cultures but also to ecosystems, as horses competed with native ungulates for forage. The third change involved the introduction of European diseases that abruptly decimated populations of First Americans (for introductory discussions, see Black 1992; Meltzer 1992; Thornton 1997); this altered (human) predator–prey relationships, and in some cases prey populations increased, apparently as a result of decreased human predation (Butler 2000b).

People have been present in North America for more than 10,000 years. Nevertheless, awareness of Euro-American influences on the landscape prompts some to suggest that a pre-Columbian or near-Columbian contact-era ecosystem is desirable (Anderson 1996; Bonnicksen and Stone 1985; Egan and Howell 2001; Jordan 1999; Sprugel 1991). To be accurate, this means that either one must assume that there were no pre-Columbian anthropogenic influences on ecosystems—a notion we find patently absurd—or one must replicate First American influences on ecosystems (Parsons et al. 1986). The latter begs the question of which influences: the ones 10,000 years ago, 5,000 years ago, 1,000 years ago, or 500 years ago? The latter also means that we must be able to sort out anthropogenic influences from climatically driven ones among the various paleoecological records available, for climates have fluctuated considerably during the last 10,000 years or what is known as the Holocene, or Recent, epoch (Bartlein et al. 1998).

One might argue that the ecosystems dating prior to human colonization of the Americas are the ones to use as ecological baselines (Flannery 2001), but this not only ignores the significant differences between terminal Pleistocene environments and modern environments irrespective of human influences; it is also naive with respect to the major social and political changes that would have to attend the adoption of such baseline conditions (Willers 2002). Despite such difficulties, some paleoecologists continue to make this argument (Burney et al. 2002; Martin and Burney 1999). Recognizing such difficulties, some characterize First American peoples as wise, ecologically aware resource users who had minimal, if any, influence on ecosystems (Anderson 1996; Vale 1999).

The notion of an ecologically wise First American colonist—often referred to as an ecologically noble savage—has roots in the 17th and 18th centuries during the period of exploration, discovery, and colonization of new and exotic lands (for discussions of this concept in modern anthropology, see Headland 1997 and Krech 1999; for a detailed history of the concept, see Ellingson 2001). A number of scholars, including natural historians who formulated the bases of what were to become various scientific disciplines, were "attracted by visions of utopian landscapes peopled by noble savages" who, in the view of the natural historians, managed the landscapes and ecosystems they occupied in nondegrading ways (Grove 1992:45). That perception rested on ecological naïveté and the use of anthropogenically modified European landscapes as comparative baselines. At the time, the ecologically noble savage was an icon for European colonists searching for new, untapped (relative to what was observed in Europe) resources in what they took to be recently discovered lands (Bowden 1992; Dods 2002; Wilson 1992). The notion of an ecologically noble savage served as a basis for early conservation efforts on the part of natural historians (Grove 1992; Krech 1999; Yousef 2001). The notion still permeates aspects of modern conservation biology (Buege 1996; Grande 1999; Redford 1990, 1991), but today it is often tinted with various political agendas (Krech 1999; see also various comments in Headland 1997)—a form of value context. An example will help make clear, from a paleoecological perspective, the difficulty of adopting the alternative that First American peoples were ecologically wise.

Ethno-ecologist Kat Anderson's (1996:158) model of how humans in North America altered the trajectory of "ecosystem change" is shown in Figure 1.1. That her focus is on humans as the catalyst for change is evidenced by the lack of "disturbance" over some portion of the Late Pleistocene prior to when humans first arrived in the Americas and a low level of "disturbance" between the time of initial colonization by First Americans and the first Euro-American colonists. Her model fails to explicitly define "disturbance" (Sprugel 1991), though the figure clearly indicates that only humans "disturb" ecosystems and thus are, by implication, unnatural. Others (Guthrie 1971; Murray 1996) indicate that some natural processes such as avalanches, fires, and the behaviors of various nonhuman organisms "disturb" ecosystems: "Traditionally, disturbances have been viewed as uncommon, irregular events that cause abrupt structural changes in natural communities and move them away from static, near equilibrium conditions" (Sousa 1984:355). It has become clear, however, that ecological equilibriums or steady states are analytical constructs rather than reality (see below).

Anderson's figure also lacks any sense of scale on the vertical axis that is meant to measure the magnitude of disruption, and perhaps that is why it conflates "ecosystem change" and "disturbance." Ecosystem change can be climatically (or simply evolutionarily) driven; disturbance could be any nonclimatic process that disrupts or alters the trajectory of change, such as the immigration of a new organism, including but not limited to humans. Anderson's model implies that First American peoples caused

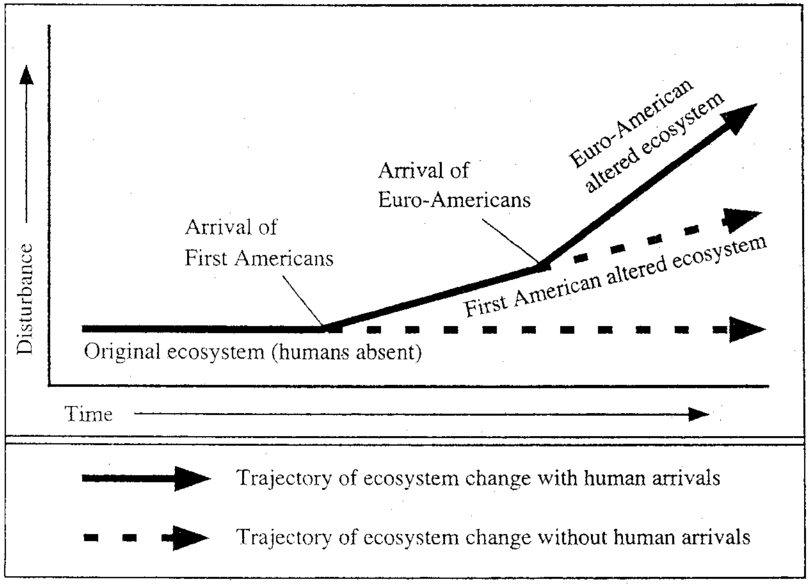

Figure 1.1. Model of how humans in North America altered the trajectory of ecosystem change (redrawn from Anderson 1996).

some degree of disturbance significantly less (or at a slower rate) than that caused by Euro-Americans and that the latter exacerbated the disturbance initiated by the former. Yet Anderson argues that First American peoples "had and continue to have a highly participatory relationship with nature [by] judiciously harvesting, crafting, and using products from nature" (1996:156). Although this may be true in some places at some times among some peoples, zooarchaeological and historical evidence suggests that it is not universally true (Grayson 2001; Low 1996; Martin and Szuter 1999a, 1999b). In fact, ascertaining the degree of influence of First American peoples on ecosystems is presently an extremely controversial issue in both North America (Krech 1999) and South America (Mann 2002). It has been controversial in North America throughout the 20th century (Adler 1969; Callicott 1989; Diamond 1986; Grieder 1970; Heizer 1955; Macleod 1936; Martin 1981; Mitchell 1978; Presnall 1943; Speck 1938; Wilson 1949).

Given the nature of their subject matter, archaeologists have long known that humans throughout time have influenced past ecosystems (Grayson 2001; Stahl 1996), but they have seldom explored this in research or scholastic contexts other than ones of interest to themselves. Many ecologists, too, are not so naive as to believe that humans did not influence ecosystems until the Industrial Age began (Day 1953; Hamel and Buckner 1998; Hunter 1996). What has happened ecologically in the past several decades, however, has made it abundantly clear that humans can be incredibly significant agents of ecosystem modification and change (Palumbi 2001; Vitousek et al. 1997), even with primitive or nonindustrial technology (Krech 1999; Low 1996; Mann 2002). This simple fact has prompted the emergence of numerous paradigms and disciplinary fields—ecosystem restoration, ecosystem health, conservation biology, restoration ecology, and the like—all of which are to some degree crisis oriented.

Ecologists, conservation biologists, and resource planners and managers think in terms of long-term human influences on ecosystems, but they look to the future and seldom into the past (Glick et al. 2001; Joyce and Hansen 2001; Westbrooks 2001). Archaeologists have been taught to think in terms of diachronic processes and long temporal spans, and they study members of the family Hominidae and their interactions with other animals. But archaeologists look into the past and seldom to the future. The pathway to useful synergy is indicated by the blinders of each.

The structure (composition) and function (processes) of an ecosystem can be conceived at various spatial and temporal scales. Given the spatiotemporal limits of what a single biologist can observe in his or her lifetime, it is not surprising that concepts such as the "balance of nature" tend to be modeled as if an ecosystem is in dynamic equilibrium, which allows concepts and analytical methods to be synchronically focused. Changes in ecosystem structure and function that occur with changes in season constitute a sort of stable, cyclical change that can be monitored by one individual. Ecosystem change driven by short-term chaotic events such as a wildfire can also be studied by a single individual. Longer-term directional changes such as shifts in timberline prompted by climatic change might also be monitored by one observer if the rate of change is sufficiently rapid or good historical data for a long time span are available. But change that occurs over long time spans, say, several hundred years, cannot be observed by a single person. As Chris Darwent and John Darwent (chap. 4) demonstrate, long-term data sets such as those represented by zooarchaeological data provide a unique perspective on colonization, extinction, and recolonization that might indicate whether or not we should be concerned about local extirpation events.

Landres (1992) emphasizes that we may only know if a perceived change is directional, chaotic, or cyclical if it is placed in a truly long-term set of observations. He also implies that we may be able to distinguish changes driven by nonhuman catalysts within a specified ecosystem from those driven by anthropogenic causes with data sets spanning long temporal durations. In both cases, paleoecological data would provide the requisite time spans. If archaeological data on prehistoric human behaviors are available for the same time span, then we can determine if any directional, chaotic, or cyclical changes have anthropogenic causes.

Some zoologists concerned with resource management and exploitation have pointed out that choosing wise management policies often depends on long-term experimental data that are variously unavailable or unobtainable (Ludwig et al. 1993). The historic record provides time depth that is to various degrees limited, but the prehistoric record potentially has limitless time depth. The historic record may be incomplete; it may be biased from the view of the author of the historic document; it may be unsystematic; it typically is nonreplicable. The prehistoric record also has potential problems, such as being incomplete or biased with respect to some analytical question (Lyman 1994c), and the temporal resolution of microscale ecological processes may be poor, though macroscale, long-term ecological processes typically are apparent. Further, although it is a historic record, the prehistoric record is often replicable in a very important sense. If one collection of bones and teeth does not answer your questions, then another collection from a similar spatial or temporal context might answer them, given the vagaries of formation and preservation (or taphonomy) of the zooarchaeological record (Lyman 1994c). And often one can find multiple cases of ecological events such as the extirpation of a local population. It is in fact easy to conceive of the prehistoric record as a suite of various sorts of empirical data comprising the results of multiple experiments; it therefore often provides precisely the sorts of data biologists have bemoaned as lacking.

Modifying the model in Figure 1.1 to account for what an archaeologist brings to the table results in the model in Figure 1.2. The latter is quite general owing to the fact that the scales of the two axes will vary considerably depending on the particular spatiotemporal context where it is applied. Note that we have labeled the vertical axis "change," that change can be in either direction, and that we do not mean to imply any particular kind of mechanism of change. Our model underscores two facts. First, a "natural" or "pristine" ecosystem unaltered by human hands is difficult to determine because change is incessant over time, irrespective of the presence of humans. Second, to determine what a "natural" ecosystem (as usually defined) comprised requires that we study the Pleistocene, a time when North (and South) American ecology was quite different from what we are familiar with as a result of (climatically driven) change in ecosystems (FAUNMAP Working Group 1996). We thus suggest that whatever reference ecosystem is called on by restorationists and conservationists, the spatiotemporal context of that ecosystem must be explicit. If that context can be identified in the archaeological record, then zooarchaeologists can bring the data they generate to bear on issues of conservation biology and restoration ecology.

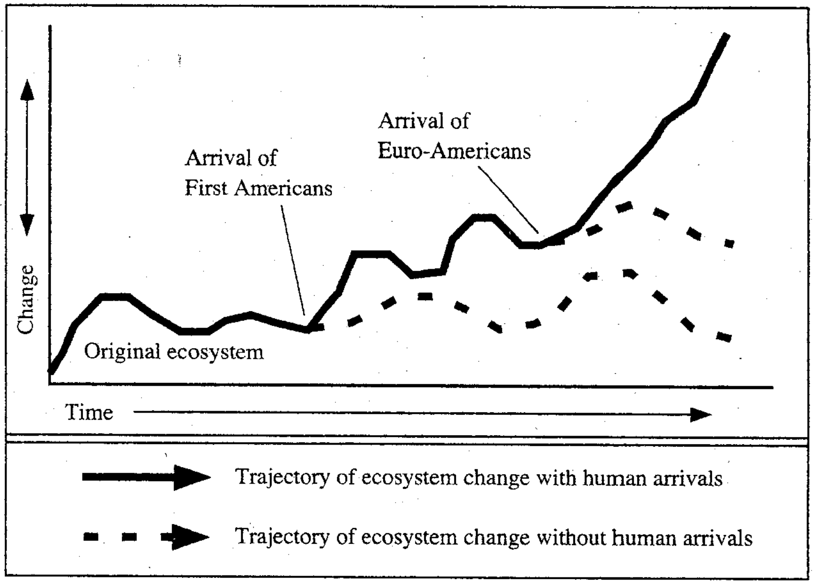

Figure 1.2. Revised model of how humans in North America altered the trajectory of ecosystem change (compare with Figure 1.1). The influence of climatic fluctuation on ecosystem change has been added.

As archaeologists we find that the words *natural, pristine,* and *wilderness* are false when defined roughly as "uninfluenced by humans," as they typically are. Instead of these terms, we suggest using the term *historic landscape* (Egan and Howell 2001; Jordan 1999) or *historic ecosystem* for the baseline that one seeks to maintain or restore. These terms are sensitive to spatiotemporal variation in ecosystems if they are accompanied by the identification of the spatiotemporal coordinates of the landscape or ecosystem specified. Given that any specified historic landscape will have existed not at a single point in time and space but, instead, over some period of time and some portion of space (Landres 1992), restoration ecologists note that the "historical range of variability" within the specified spatiotemporal coordinates must be determined (Egan and Howell 2001). Zooarchaeological material can provide just such data, and although the baseline for restorationists and conservationists may be something of a moving target, we find this far superior to the false concept of a pristine or natural ecosystem uninfluenced by humans.

WILDLIFE MANAGEMENT AND CONSERVATION BIOLOGY

About 30 years ago wildlife management could be defined as "the process by which closely related needs of wild animals and of people are evaluated, reconciled, and met" (Scheffer 1976:51). At about the time that this definition was penned, what some characterize as a "new" discipline called "conservation biology" began to emerge (Noss 1999). An early definition, though certainly not the first, is provided by one of the originators of the field, Michael Soulé: "Conservation biology [involves] the application of science to conservation problems [and] addresses the biology of species, communities, and ecosystems that are perturbed, either directly or indirectly, by human activities or other agents. Its goal is to provide principles and tools for preserving biological diversity. [It is a] mission- or crisis-oriented discipline" (1985:727). Discussion and debate over differences between traditional wildlife management and the allegedly new conservation biology appeared shortly after this definition was published (Edwards 1989; Gavin 1989; Jensen and Krausman 1993; Teer 1988; Wagner 1989). What made conservation biology *new*? What ecological and biological variables had wildlife management *not* been considering all along (Knight 1996; Shafer 2001; Temple et al. 1988; J. Young 2000)?

Major differences between the two that we derived from various articles published in major journals of each paradigm are given in Table 1.1 (see also Bunnell and Dupuis 1995; Jensen and Krausman 1993; Noss 1995a). Prior to the middle 1970s many wildlife managers and ecologists attempted to be "objective" in their scientific endeavors and concomitantly to not advocate solutions to "biotic impoverishment" (Noss 1999:114–115). Increasing awareness of potential ecological crises in the middle of the 20th century resulted in the passage of various environmental laws and policies in the United States, but these speak of the "human environment"—what is used and, particularly, exploited and modified—rather than the maintenance of some historic landscape or ecosystem, "native" species, and biodiversity. It is the latter topics that have become the central focus of conservation biology, along with an additional concern for the preservation of ecological and evolutionary processes (Angermeier and Karr 1994; Crandall et al. 2000; Moritz 2002; Murray 1996; Smith et al. 1993; Srivastrava 2002).

TABLE 1.1. Fundamental Differences between Traditional Wildlife Management and Conservation Biology.

Basis for Comparison	Wildlife Management	Conservation Biology
Central goal	Manipulations of population size	Maintenance of biological diversity
Basis for paradigm	Mostly empirical	Mostly theoretical
Taxonomic focus	Higher vertebrates, especially game species	All taxa

Note: Modified from Aplet et al. 1992.

Whether or not conservation biologists *should* be advocates for particular management policies is still hotly debated (see the articles introduced by Noss 1996, Rykiel 2001, and Wagner 1996). But the initial friction between traditional wildlife managers and the Young-Turk conservation biologists has waned considerably because of the recognition of common ground, including not only shared biological and ecological questions but also how to have the most effect on policy decisions (Beissinger 1990; Meefe and Viederman 1995; Temple 1992). Our point is simple. Because the focus of this book is on zooarchaeological data, the term *wildlife management* is sometimes used; but do not be misled—the contributors are concerned with modern issues of conservation biology.

Twenty years ago wildlife management was characterized as a science of "muddling through" (Bailey 1982) and as comprising "scientific experimentation" (McNab 1983) because complete knowledge with respect to the short- and long-term outcomes of a particular management activity was not available. The same characterization applies to conservation biology. Yet decisions must be made, and management and conservation activities must take place, else one is following "a path to inaction" (Wagner et al. 1995:175) and is susceptible to the "paralysis of analysis" (Hutchins 1995:1326) in which no action is undertaken because of inadequate knowledge. Such is neither management nor conservation. And although it is recognized that complete knowledge will never be available, it has become increasingly clear that *more* knowledge is better than less when it comes to making decisions and taking action (Ludwig et al. 2001).

This book is about a particular kind of knowledge—knowledge extractable only from zooarchaeological data, although a growing number of paleontologists are also recognizing how their data might be brought to bear in an applied arena (Barnosky et al. 2003; Burnham 2001; Chure 2002; Flessa 2002; Sepkoski 1997). It is the shared position of the contributors to this book that zooarchaeological data can often help answer questions of management and conservation. Thus, less muddling through will result, although it is also clear that we often have insufficient data to answer the questions we as archaeologists seek to answer and, thus, our data may also be inadequate to answer questions a wildlife manager or conservation biologist might ask. But we can sometimes evaluate empirically in either analytical context whether we have the requisite data or not (e.g., Lyman 1995a), and this can be a contribution in and of itself, as Lyman points out in chapter 8.

APPLIED ZOOARCHAEOLOGY

Zooarchaeologists have long borrowed ecological and behavioral data from the wildlife sciences to help interpret the shells, teeth, and broken bones they study. Seldom have they given back information of use to wildlife scientists. This book reflects the fact that the data zooarchaeologists generate may be quite relevant to various wildlife-management concerns. The kinds of conservation problems that zooarchaeological research might help resolve are typically very specific to a particular geographical place and a particular species or set of ecologically or taxonomically related species. The size of the geographic place can range from an area the size of several counties within a state to one the size of several states. Some of the kinds of problems that might be addressed with zooarchaeological data are noted in the next several paragraphs. The chapters making up the remainder of this book present additional, more fully developed examples.

One way to ensure that populations of organisms isolated by habitat patchiness survive is to "defragment" the patches by constructing habitat corridors among them (Kaiser 2001a). Zooarchaeological data can help determine where corridors once existed and, thus, where new corridors should perhaps be constructed, and such data may also indicate if isolation is the result of natural processes or anthropogenic ones and, thus, how to reverse fragmentation processes. The *cause* of isolation may, depending on the applicable policy, have a bearing on a management decision. Small, genetically isolated populations of species tend to be more prone to extinction than large or nonisolated ones (Korn 1994). Large populations provide sufficient genetic variation to ensure the survival of a species, and large areas more readily allow migration between population nodes because corridors between them tend to be short relative to those between small

areas. The isolation of population nodes from one another, or the restriction of those nodes to small geographic areas or both, increases the probability that one or more of those nodes will cease to exist. Some mountaintops in the western United States, for example, contain small isolated populations of alpine mammals (Brown 1971, 1978; see also Brown 1986; MacArthur and Wilson 1967) that zooarchaeological research indicates today cannot be reached by habitat corridors (Grayson 1987). Related examples are provided by Susan Hughes (chap. 7) and Paul Sanders and Mark Miller (chap. 9). Hughes uses zooarchaeological data to show that the modern migration patterns between seasonal ranges followed by bighorn sheep *(Ovis canadensis)* were established about 5,000 years ago and that Euro-American land use has resulted in alteration of those patterns and ranges. Similarly, Sanders and Miller demonstrate that a modern migration corridor used by pronghorn antelope *(Antilocapra americana)* to move between seasonal ranges seems to have been used in like manner 5,000 years ago.

Ensuring that isolated habitat patches (or migration corridors) are not artificially disrupted in the future may be the only way to guarantee the survival of their included populations of small mammals. In such cases, knowledge of the historic and prehistoric causes of habitat patch diminution and destruction would seem to be valuable to management decisions (Beever 2002; Beever et al. 2003). An example will help make this clear. 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 is the central portion of eastern Washington State. Populations in the two areas are presently isolated from one another. Over the past 40 years the eastern Washington population has shrunk to an alarmingly small size, prompting the Washington Wildlife Commission (a) to list this population as "threatened" and to suggest that it be listed as "endangered" (McAllister and Allen 1993) and (b) to develop a management plan with the aim of ensuring the survival of the population (McAllister 1995). The zooarchaeological record for pygmy rabbits indicates that this species occupied a wider range in central Washington during the Holocene than it presently does. Remains of this species have been recovered from 17 archaeological and two paleontological sites in the area (Lyman 1991, 2004b). Some remains have been recovered from extralimital geographic locations where pygmy rabbits have not been historically documented. The botanical and faunal records suggest 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 4,000 years ago the range of sagebrush shrank, and so too did the range of pygmy rabbits. Then, during the late 19th and early 20th centuries, land was cleared of sagebrush for agricultural purposes. This produced 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 (Gabler et al. 2001; Green and Flinders 1980a, 1980b). The zooarchaeological and paleobotanical records indicate that both species responded to climatic change in like manners. 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. Today it would be impractical to develop a migration corridor between the two populations, and it would also be impossible given the climatic and land-use histories 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, and much of that area is now under cultivation and irrigation. The zooarchaeological record indicates, however, that the extant population might be supplemented by individuals transplanted from southeastern Oregon because the latter seem to be the genetic source of the former. This supposition could be tested by study of the DNA preserved in the prehistoric bones. The prehistoric record also indicates that the maintenance of habitats dominated by big sagebrush is critical to the survival of the species, and the historic record indicates that the maintenance of pertinent habitats must involve changes in human land-use practices.

When local populations have been extirpated, one management alternative is to transplant individuals from other populations to the vacated range in an attempt to reestablish a population. Zooarchaeological research can play several roles in these situations. As Judy Harpole (chap. 6) notes, such research can indicate locations where the species of interest once existed and may be most capable of surviving today, and it might also indicate locations where the transplanted species may not survive given historic modifications to habitats (see also Emslie 1987; D. Gordon 1994; Owen-Smith 1989). Morphometric and biogeographic analysis of zooarchaeological remains may suggest which extant population should be sampled for individuals to transplant (Lyman 1988b). Most conservation biologists agree that taxa should not be transplanted to areas "outside" of their "historical range" (D. Gordon 1994:33), and biogeographic analysis of zooarchaeological remains can help establish a taxon's historical distribution. The International Union for the Conservation of Nature and Natural Resources (IUCN)—renamed the World Conservation Union—has drafted guidelines for reintroductions. These read, in part, as follows:

> An assessment should be made of the taxonomic status of individuals to be reintroduced. 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 reintroduction 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. (IUCN Reintroduction Specialist Group 1992:2–3)

Burney et al. (2002) refer to artificial efforts to rebuild diminishing biodiversity and "jump-start" ecological processes by transplanting organisms into areas where their conspecifics or congeners are now extinct as "evolution's second chance." What they mean by *evolution* is the creation of "independent evolutionary track[s] into the future, where there would have been few (if any) otherwise" (Burney et al. 2002:15), but of course this begs again the question of how close genetically is close enough when it comes to, say, restarting evolutionary lineages that have not existed for 10,000 years with a related genus from another continent. Ignoring this slippery (and value-laden) issue, it is clear that a search of not only historic but also prehistoric information may be necessary to determine which extant population would be the most appropriate genetically as a source of transplantable animals. The study of ancient DNA extracted from prehistoric skeletal tissues (Richards et al. 1993; Richards et al. 1995) seems to have great potential for contending with IUCN and similar guidelines, as demonstrated by Michael Etnier (chap. 5). Fortunately, the technique is applicable to curated as well as newly acquired zooarchaeological specimens (Pääbo 1993), and thus new archaeological excavations need not take place so long as appropriate collections are curated and accessible.

Biogeographic evidence in the form of prehistoric remains of a taxon in locations where that taxon no longer occurs can help establish which taxa are recolonizing once-occupied areas. Such a determination is critically important in light of the recently growing interest in "invasion biology" (Vermeij 1996) and "invasive" species, a major threat to indigenous taxa (Wilcove et al. 1998). Invading species are those that come to occupy areas outside their historically documented range as a direct or indirect result of anthropogenic processes. The importance of identifying invasive species resides in two arenas: the invasion process homogenizes community and global ecology, evolution, and biodiversity; and the invading taxa often threaten the survival of native species (Lodge 1993; Mooney and Cleland 2001). Dave Schmitt (chap. 10) presents zooarchaeological data that show how the invasion of a plant species to many areas of the western United States has resulted in the modification of small mammal communities. Many species have invaded new habitats and geographic locations as a (often unintentional) result of human activities. The black rat *(Rattus rattus)*, Norway rat *(R. norvegicus)*, and house mouse *(Mus musculus)* are all native to the Old World but were able to colonize the Americas along with European humans. There are numerous more recent examples, and all of these taxa are generally referred to as "exotic," "nonnative," or "alien."

If a particular taxon (ecotype, subspecies, or species) is exotic to an area meant to be characteristic of a historic landscape, then it may be necessary to remove that taxon from the area in order to re-create the historic ecosystem and biota. Thus, we must have a solid definition of exotic taxa above and beyond the notion that they include recent invasive species. The U.S. 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; see also NPS 1978). *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 alterations by humans" (NPS 1978). These definitions take a relatively synchronic perspective. From the temporally deep perspective of dynamic biogeographic history, the definitions of native and exotic species are contradictory. Exotic species occur in a given place as a result of actions by humans, whereas native species are those that presently occur or *once did occur* in a place as the sole result of natural ecological processes. The emphasized phrase is where the contradiction resides, for it indicates that if a taxon *ever* occurred at some time in the past in an area, then *by definition* that taxon represents a native species, irrespective of human intervention at a later date. Recognizing this contradiction some years ago, Lyman argued that the NPS "should rethink policy issues" (1988a:22). Some NPS biologists later came to the same conclusion (Houston and Schreiner 1995). The definition of an exotic species as "one whose comparatively short

historical residency stems directly or indirectly from human actions" (Povilitis 2002:72) is a step in the right direction, but it begs the question of how little time *short* comprises. Zooarchaeologists can perhaps help clarify and resolve such issues.

If a historic landscape or ecosystem is to be re-created by exclusion of exotic species or reintroduction of artificially extirpated native species, then we must have a baseline list of the original native species. In the United States, such a list is typically derived from the earliest historical documents for the area included within a piece of landscape such as a national park (Houston and Schreiner 1995; Leopold et al. 1963). That people were present in North America more than 10,000 years before the historical period may be acknowledged when compiling a list of "native" species, but it is typically ignored (Houston and Schreiner 1995). The ethnocentricity of such a procedure is unavoidable from a practical standpoint. More important, historical documents are sometimes incomplete and at other times inaccurate. Such documents can be supplemented and tested with zooarchaeological data, as Butler and Delacorte (chap. 2) and Thomas Whyte (chap. 11) demonstrate.

Bison *(Bison bison)* were artificially introduced to a portion of the State of Alaska that was subsequently to become Wrangell–St. Elias National Park and Preserve (Peek et al. 1987). Historic records suggest that bison were not present when the first white men visited the area in the middle and late 19th century. A review of the zooarchaeological and paleontological record indicates that bison were present in Alaska between about 450 years ago and the early 20th century (Stephenson et al. 2001). By definition, the bison introduced to Wrangell–St. Elias National Park should be considered exotic, and the NPS considers them to be so because of the lack of evidence that bison were present when the first white explorers passed through the area (Houston and Schreiner 1995). NPS biologists also note that the introduced form of bison was of a nonnative genetic stock (a distinct subspecies or ecotype), given current beliefs about bison taxonomy, so they argue that the extant bison of Wrangell–St. Elias should be removed. However, because it cannot be demonstrated that native bison were locally extinct prior to the transplanting event, it is possible that extant bison are hybrids of native and introduced genetic stocks. Perhaps DNA testing of extant bison and of prehistoric bison remains would clarify this. In the event that it does not, only more zooarchaeological and paleontological research will establish the timing of the local extirpation of native Alaskan bison populations and whether or not all Alaskan populations were extinct when transplanting occurred in the middle of the 20th century. Once the facts are determined, we will have strong bases for making a decision regarding the ultimate fate of the bison presently extant in Wrangell–St. Elias National Park and Preserve.

The preceding is but one example of many similar situations (Laundré 1991; Lyman 1998; Schullery and Whittlesey 2001; Varley and Varley 1996). All of these underscore several facts. First, the quality and quantity of zooarchaeological research—the sampling effort—may be insufficient to be reliable (Lyman 1995b). No one suspected, for example, that mountain goats *(Oreamnos americanus)* were once present on Vancouver Island off the western coast of British Columbia, but a recent report of paleontological remains of this species recovered from a high-altitude cave on the island proves otherwise (Nagorsen and Keddie 2000). Similarly, as Etnier (chap. 5) notes, until zooarchaeological materials were studied, no one suspected that Guadalupe fur seals *(Arctocephalus townsendi)* used to be found along the Pacific coast of Washington State and seem to have had breeding colonies there during the late prehistoric period. Taphonomic problems—those concerning biased or poor preservation of faunal remains—are also critically important in applied zooarchaeology, and zooarchaeologists can help biologists and ecologists interpret the zooarchaeological data used to inform management decisions (Cannon 2001; Etnier 2002b; see also Church 1997; Sisk and Noon 1995).

Finally, paleozoologist Russell Graham (1988) notes that the boundaries of many parks and preserves are defined on the basis of modern climatic and environmental conditions. Those conditions change over time, however, and the probability of significant change increases as the length of time increases (Landres 1992). Graham argues that much conservation planning 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; see also Hunter et al. 1988). Such taxonomically individualistic responses to environmental change imply that the biological preserves of today are artifacts of the time when they were identified and created. Zooarchaeological research has confirmed this implication time and time again. That research can also provide indications of what might happen to those preserves should climates change (Graham 1992). And as Ken Cannon and Molly Cannon (chap. 3) demonstrate with respect to the world's first national park, zooarchaeological data may prove to be invaluable to the restoration and maintenance of parks in some chosen conditions.

DISCUSSION

Archaeologists are beginning to argue that they must make their discipline relevant to modern concerns, but they are not always clear about why they should do so other than to note that archaeology provides a time depth to anthropogenically created ecologies (van der Leeuw and Redman 2002). We have outlined some much more explicit reasons in this chapter, and they are echoed throughout this book. The manner in which zooarchaeological research can be used in modern biological conservation is specific to a place or to a taxon. This makes "applied zooarchaeology" intellectually challenging. Broadening the scope of zooarchaeology to conservation and management applications will, we believe, be beneficial to our future, not only from the perspective of helping to ensure the preservation of biological diversity for future generations but also from the perspective of paleozoological studies in general, which might otherwise be increasingly perceived as the pursuit of esoteric knowledge of little practical use. Thus, applied zooarchaeology *matters* not only to zooarchaeologists but to all of humanity as well.

From a disciplinary-selfish perspective, we believe that a well-developed applied zooarchaeology will provide a new job market and new sources of funding. Both may become available if we convince wildlife managers and conservation biologists that because their policies are typically aimed at the future, knowing something of the past can result in better-informed decisions. Although we cannot predict exactly when the next ice age or glacial period will begin, we can 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 conservation might well pay a zooarchaeologist to help them make wise management decisions. The other selling point is less selfish and comprises the fact that a less desirable kind of price—ecosystem destruction and the loss of biodiversity—might otherwise have to be paid.

Some of the necessary convincing has already transpired. Many of those practicing landscape restoration, for example, are well aware of the value of data concerning historic and prehistoric ecosystems. They use such data "to determine what needs to be restored, why it was lost, and how best to make it live again" (Egan and Howell 2001:1); pertinent data regarding the past reveal "reference conditions" that serve as baselines toward which restoration efforts may be aimed (Egan and Howell 2001:2). Whether or not restoration activities actually attain such baseline conditions depends on a host of social, political, and other value-laden contextual variables. Yet the important point is that those reference conditions must somehow be established. One of the messages of the chapters in this book is that it is through paleozoological research that many of them can be determined.

In the preceding we have said that "many" restoration ecologists are aware of the value of historic and prehistoric data pertaining to ecosystems. In fact, ecologists of various sorts have during the past decade begun to pay much more attention to the deep histories of ecosystems, occasionally calling on zooarchaeological data to increase their knowledge of the history of particular variables (Jackson et al. 2001; Ludwig et al. 2001; Schullery and Whittlesey 2001). Such cases have yet, however, to become commonplace. For example, every one of a recently published set of 13 articles on ecosystem-recovery planning (introduced by Kareiva 2002) fails to mention paleoecological data. The case studies in the following chapters demonstrate the value of zooarchaeological data in particular and paleoecological data in general to conservation biology. This book also makes clear that the opinion recently expressed by conservation biologist Reed Noss is easily characterized as prejudicial. In his introduction to an edited collection on the restoration of large mammals, Noss (2001:13) indicates that only those North American taxa impacted by "Europeans" should be restored. Those impacted or driven to extinction by First American peoples, apparently, should not be the subject of restoration efforts. As we noted earlier, Euro-Americans did not work alone in impacting populations of furbearers across the continent. Given this, and in light of increasing archaeological evidence of the prehistoric impact of First American peoples on animal populations, biologists should find it increasingly difficult to maintain Noss's position without charges of racial and cultural discrimination.

We believe that no one can afford to ignore any potentially relevant data when it comes to planning and working toward an ecological future that is not only pleasant but also safe for humanity and ecosystemically wise. The following studies demonstrate the value of one kind of what we take to be *very* relevant data.

2

Doing Zooarchaeology as if It Mattered: Use of Faunal Data to Address Current Issues in Fish Conservation Biology in Owens Valley, California

VIRGINIA L. BUTLER AND MICHAEL G. DELACORTE

Ecologists are increasingly incorporating concepts such as "legacy" into their explanations of current ecosystems (Harding et al. 1998). This approach acknowledges that understanding the structure and function of extant ecosystems (or predicting future responses to climate change) requires knowledge of historical forces that have been operating for decades, centuries, or longer (Foster 2000; Moorhead et al. 1999). Indeed, recognition of the need for such long-term historical records is demonstrated by the level of National Science Foundation funding for the Long-Term Ecological Research (LTER) network (Kaiser 2001b; LTER Network 2001). Over 1,100 researchers funded by the LTER carry out research on 24 designated sites that have been studied from a few years to several decades (Kaiser 2001b). These studies cover a range of topics with the overall goal of "investigating ecological processes over long temporal and broad spatial scales" (LTER Network 2001). This goal is precisely that of zooarchaeology. Yet, to our knowledge, zooarchaeological expertise and data have not been incorporated into the LTER network. Our point is simply that ecological sciences seeking to understand the long-term properties of ecosystems have direct access to such information through zooarchaeology.

Zooarchaeology needs to collaborate with wildlife sciences because of the increasing speed with which habitats and biotas are being lost in the face of human population growth and habitat destruction (Minckley and Deacon 1991; Vitousek et al. 1997). In response to legislation such as the

Zooarchaeology and Conservation Biology

EDITED BY

R. LEE LYMAN AND

KENNETH P. CANNON

THE UNIVERSITY OF UTAH PRESS

Salt Lake City

09 08 07 06 05 04
5 4 3 2 1

Index prepared by Andrew L. Christenson

The Defiance House Man colophon is a registered trademark of The University of Utah Press. It is based upon a four-foot-tall, Ancient Puebloan pictograph (late PIII) near Glen Canyon, Utah.

Library of Congress Cataloging-in-Publication Data

Zooarchaeology and conservation biology / edited by R. Lee Lyman and
Kenneth P. Cannon.
p. cm.
Includes bibliographical references and index.
ISBN 0-87480-801-4 (alk. paper)
1. Animal remains (Archaeology)—North America. 2. Conservation biology—North America. 3. North America—Antiquities.
I. Lyman, R. Lee. II. Cannon, Kenneth P.
CC79.5.A5Z65 2004
560'.45'097—dc22 2004010719

Contents