Zooarchaeology, put simply, is the study of animal remains from archaeological sites (Reitz and Wing 1999:1). In the United States the term is used interchangeably with terms such as faunal analysis, archaeozoology, and osteoarchaeology (Baker, Shaffer, and Steele 1997:298). The research goals of zooarchaeologists can be divided into three broad camps: those primarily biological in nature (e.g., paleoenvironmental studies, paleozoogeography), those primarily anthropological in nature (e.g., studies of human mobility, diet, butchery, hunting patterns, exchange systems), and those focused on methods (e.g., quantification, identification, field methods for recovery).

Experimental zooarchaeology can be thought of as the derivation and use of experimental data to interpret the zooarchaeological record. As an interdisciplinary field, zooarchaeology can be informed by experimental work from a variety of perspectives, such as zoology, geochemistry, paleontology, forensic anthropology, and ethnoarchaeology. Because of this breadth of goals, perspectives, and disciplinary affiliations, any discussion of experimental zooarchaeology in general requires a statement of limitations. For the purposes of this chapter, we limit ourselves primarily to the archaeological literature, with a focus on laboratory or actualistic experiments. Although they are typically considered part of zooarchaeology, we largely
avoid reference to experiments on bone artifacts, which are discussed elsewhere in this volume (Bement, this volume). We also exclude reference to experimental elemental and molecular-level investigations of animal remains, such as trace element, stable isotope, and protein studies.

The chapter’s principal aim is to discuss fruitful avenues for, and limitations of, experimental zooarchaeology, theoretical considerations, and experimental design for student use in developing experiments. At this general level of discussion, no attempt is made here at a critique or comprehensive review of past experimental zooarchaeology work. The reference section provides a selection of relevant example experiments. This chapter begins with a discussion of the range of experimental zooarchaeology, then provides a number of working interpretative hypotheses susceptible to experimental work, discusses applicability and experimental design, and concludes with retrospect and prospects.

Experimental zooarchaeology can be conceived to include a range of approaches, from tightly controlled laboratory experiments to more realistic field experiments, or “natural experiments.” Controlled experiments are ones in which an attempt is made to hold a number of variables constant so that one or a few variables can be studied. Most are laboratory studies (e.g., Coard 1999; Egeland 2003; Lubinski 1996; McCutcheon 1992; Richter 1986; Shaffer 1992a; Shipman, Foster, and Schoeninger 1984; Smoke and Stahl 2004; Von Endt and Ortner 1984; Willis, Eren, and Rick 2008). For example, Patrick Lubinski (1996) placed various fish bones in aqueous solution and measured weight loss over time to model natural tendencies toward differential skeletal part survivorship.

Controlled experiments grade into somewhat more realistic but less controlled “natural experiments,” sometimes called actualistic studies. These are commonly contemporary field experiments or observations (e.g., Behrensmeyer 1978; Bennett 1999; Binford and Bertram 1977; Brain 1981; Haynes 1988; Hockett 1995; Jones 1986; Lubinski and O’Brien 2001; Lyman 1989; Marean et al. 1992; Payne and Munson 1985; Stiner et al. 1995; Thomas 1969; Whyte 1991). For example, Sebastian Payne and Patrick Munson (1985) fed carcasses of squirrels, rabbits, and goats to dogs, then collected and examined the fecal remains to evaluate bone survival under such conditions. Taken to an even more realistic level are studies that involve direct observation of faunal remains in natural settings, with data collection similar to typical controlled experiments. For example, Lubinski and Christopher O’Brien (2001) collected the mandibles from a 1991 cliff fall of 150 pronghorn to examine the distribution of seasonality and age estimates from this known event. This study established a comparative analog against which archaeological age and seasonality distributions could be compared.

Experiments of all stripes in zooarchaeology have tended to focus on formation processes (Schiffer 1987), particularly taphonomic processes. Taphonomy is a term borrowed from paleontology (Efremov 1940) and is generally considered to focus on how faunal remains were collected and modified to form what was recov-
rer by archaeologists and paleontologists (Shipman 2001) or, put another way, to focus “on the postmortem, pre- and post-burial histories of faunal remains” (Lyman 1994:3). The objective is typically to derive archaeological expectations for given processes that might have operated in the past. Clearly, assumptions of methodological uniformitarianism or actualism (Gould 1965; Simpson 1970) are called for in such work. That is, natural laws and processes are assumed to be invariant in time and space, and past results may be attributed to processes currently in operation (Lyman 1994:47). Processes observed in the modern experiment are assumed to be comparable to the same processes operative in the past, so modern results can be used as strong analogies for the archaeological record.

For the purposes of experimental zooarchaeology, the most broadly applicable work, providing the strongest analogies, revolves around the most basic and unchanging processes—such as the way a bone breaks or deteriorates given specific conditional parameters (see also Domínguez-Rodrigo 2008). Works such as these can be very useful, but archaeologists are often interested in more complex and unpredictable processes involving human behavior. Experiments in these latter areas may not be as broadly applicable, but they can provide insightful potential explanations for the archaeological record. One class of work is not better than the other, but the ways in which each should be used are different. The key concern is a careful consideration of the applicability of experimental results to a particular archaeological problem (see also Domínguez-Rodrigo 2008).

Closely allied with experimental zooarchaeology are ethnoarchaeological studies of faunal remains. These are normally very realistic regarding human actions on faunal remains, but they often suffer a lack of control. As such, it is unclear as to what archaeological situations the findings can be compared. Nonetheless, ethnoarchaeological work in zooarchaeology has made significant contributions to the field, as the continual reference to Lewis Binford's (1978) *Nunamiut Ethnoarchaeology* alone attests. In that seminal work, Binford described a number of effects of human butchery, consumption, and storage on caribou bone assemblages. Other, more recent works have also produced significant findings for interpretation of archaeological faunas (e.g., Bartram and Marean 1999; Bunn, Bartram, and Kroll 1988; Gifford-Gonzales 1989; Hudson 1993; Lupo and O'Connell 2002; O'Connell, Hawkes, and Blurton Jones 1990; Schmitt and Lupo 2008).

### ZOOARCHAEOLOGICAL EXPERIMENTS AND INTERPRETATION

Robust interpretation of zooarchaeological data depends on a careful consideration of as many plausible explanations as feasible. The use of multiple working hypotheses (Chamberlin 1965 [1890]) and multiple independent lines of evidence will lead to the most reliable interpretations, and experimental zooarchaeology can provide important analogs for selecting the most compelling or parsimonious hypotheses. The examples in this section should illustrate both a consideration of multiple
working hypotheses and fruitful avenues for zooarchaeological experimentation.

Suppose deer bones are more common than rabbit bones in an archaeofaunal assemblage. How could this be explained? The typical interpretation that might follow would be that deer were not just more commonly utilized but also that the animals’ larger size would have provided more resources than rabbits; so we see an economy with a focus on deer hunting. In fact, it could have been that there was an equal focus on procurement and that deer just happened to be more readily available as a result of hunting technology or habitat limitations. Or food taboos may have been at work in which most people ate deer but a few of the site occupants were not permitted to eat deer and thus focused their animal hunting on the ubiquitous rabbits in the immediate area. There are, indeed, many possible explanations—some resulting from human behavior, some from subsequent natural transformations and site formation processes, and some from some current methods of archaeological recovery and analysis. Modern experiments may provide important information for the evaluation of competing hypotheses, such as differential processing, preservation, recovery, and identification of the remains. Other hypotheses, such as differential deposition of the deer and rabbit remains for ritual, labor organization, or other reasons, lend themselves less readily to experimental modeling.

One alternative explanation is differential processing. There may have been practices in which the ways the animals were processed have biased our ability to recover each equally. For example, while a variety of bones may have been processed for marrow, smaller taxa such as rabbits and rodents may have had many of their bones completely ground into pulp (along with muscle and other tissues) and been consumed in their entirety, with the exception of a few larger bones that may have been discarded during processing (e.g., Fowler 1989:29; Michelsen 1967). It could be that rabbits were the primary source of animal food, but differences in processing practices resulted in a reversed representation of remains in the archaeological record; in such cases, an interpretation could be made that was actually the opposite of what was correct. Expectations for different sorts of processing, and bone modification by humans in general, can be derived from zooarchaeology experiments, such as in butchery (e.g., Bar-Oz and Munro 2007; Braun, Pobiner, and Thompson 2008; Church and Lyman 2003; Domínguez-Rodrigo 1997; Egeland 2003; Greenfield 1999; Jones 1980; Lupo and O’Connell 2002; Outram 2002; Pickering and Egeland 2006; Pobiner and Braun 2005; Sadek-Kooros 1975; Seetah 2008; Toth and Woods 1989; West and Louys 2006), hunting technology (e.g., Frison 1989; Letourneaux and Pétillon 2008; Smith, Brickley, and Leach 2007; see also Whittaker, this volume), burning (e.g., Bennett 1999; Buikstra and Swegle 1989; Hanson and Cain 2007; McCutcheon 1992; Nicholson 1995; Shipman, Foster, and Schoeninger 1984; Stiner et al. 1995), and human consumption (e.g., Butler and Schroeder 1998; Jones 1986; Nicholson 1992).

Another possible explanation is differential preservation. In a given soil environment, it may be that the more robust deer elements stand a much greater chance
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of surviving in the immediate matrix than do the more fragile rabbit elements. Thus, the greater number of deer remains may reflect more about the ongoing site formation processes than about actual cultural practices. Experimental zooarchaeology can be a useful avenue for generating expectations for natural tendencies of preservation and bone modification under different environmental conditions, such as ravaging by various non-human predators (e.g., Binford and Bertram 1977; Faith, Marean, and Behrensmeyer 2007; Hockett 1996; Marean et al. 1992; Moran and O’Connor 1991; Munson and Garniewicz 2003; Payne and Munson 1985; Schmitt and Juell 1994; Thornton and Fee 2001) and physical or chemical decay in the soil (e.g., Fiorello 1989; Lubinski 1996; Nicholson 1996; Olsen and Shipman 1988; Von Endt and Ortner 1984). R. Lee Lyman (1994) and Christiane Denys (2002) discuss these and other taphonomic factors and provide many useful references to zooarchaeological experiments.

Recovery methods also come into consideration here. Screen size obviously plays a potential role, and expectations for recovery of skeletal elements of deer and rabbits (and other taxa) with different mesh sizes can be derived from experimental data. For this example, experiments indicate that screen mesh sizes down to ¼” will recover the vast majority of deer elements, but they miss a fairly large number of rabbit elements (Shaffer 1992a). Screen size experiments can more broadly address issues of bias, abundance, and diversity in taxonomic, element, and animal size distributions (e.g., Nagaoka 2005; Shaffer 1992a; Shaffer and Sanchez 1994; Vale and Gargett 2002; Zohar and Belmaker 2005). Other potentially fruitful avenues for experimental zooarchaeology research here include examination of the role of flotation, wet versus dry screening, and the use of deflocculents.

Another possibility is differential identification. Larger taxa such as deer may be much more readily identified than smaller taxa. While numerous osteological guides can be found for larger taxa, some of which are comparative guides in which similar taxa are compared and contrasted by element, such guides for smaller taxa are largely lacking. It may be possible to distinguish a deer metapodial or sesamoid from that of a pronghorn antelope, but a similar distinction between rock squirrel and prairie dog has not been described in the literature. On encountering such small elements, assuming they were recognized as bone, there is little chance they would have been identified to a specific taxon. There may be a role for experimental zooarchaeology here as well, for example, in experiments with identification (e.g., Gobalet 2001; Sykes and Symmons 2007). More generally, experimental zooarchaeology can examine the role of zooarchaeological methods and their impact on interpretation, such as through blind tests and computer modeling (e.g., Blumenschine, Marean, and Capaldo 1996; Rogers 2000).

**APPLICABILITY OF EXPERIMENTAL DATA**

One could discuss a number of concerns about experimental zooarchaeology, but
one important set relates to the applicability of the experimental data to archaeological cases. Experimental results may be widely applicable or applicable only to a narrow range of similar cases. The applicability should be considered both in experimental design and in the use of experimental data for archaeological interpretation. As the potential issues addressed in an experiment become more uncertain (i.e., as the number of uncontrolled variables increases), the issue of equifinality is more problematic. By equifinality we mean that a given outcome can be the result of multiple causes (see also Lyman 2004; Rogers 2000). For example, determining if differential representation of recovered fauna is the result of a bias in recovery methods can be more readily tested than can ascertaining changes in animal exploitation (cultural factors) or taphonomic factors over time (site formation factors). This is the case because recovery bias is a simple problem to address compared to the countless potential reasons a people might change their food choices or the myriad factors that may influence the faunal assemblage between the initial deposition and subsequent recovery. Bias in recovery is a research-induced methodological impact on the assemblage that does not occur until the time of recovery. As such, it is a bias that can be controlled much more so than can controlling for hunting decisions a prehistoric people made or site formation influences since the time of deposition. The following example describing two different screen size experiments should illustrate this point.

A classic screening experiment was completed by David Hurst Thomas (1969), who used nested screens on an archaeological faunal sample to determine exactly what was missed by successive larger sizes of screens within three individual Great Basin sites. While this was an excellent actualistic test, it was site-specific. The amounts of recovery or loss could not be directly applied as corrections at other sites because of differing cultural and taphonomic factors. In addition, the experiment was limited to a small area of the site, and application of the results even to other areas of the site could be justified only with the assumption that the remaining site was homogeneous with the units tested.

By contrast, Brian Shaffer (1992a) and Shaffer and Julia Sanchez (1994) conducted controlled screening of various mammals (from a modern comparative research collection) using complete or nearly complete skeletons with little or no fragmentation. While it was widely known prior to these experiments that larger screen sizes recover less than smaller screen sizes do, just what would be recovered or lost by each screen size under ideal circumstances of complete and unfragmented animal skeletons was not known. For the taxa tested, the experiments showed which elements could be expected to be recovered by ¼” or ⅛” mesh screens if they were complete. This experiment was directly applied to recognizing full carcass internment (either by cultural or natural burial) versus dietary remains by Shaffer (1991, 1992b), who used the results to address whether gopher remains from a Mimbres site in southwestern New Mexico were actually noncultural interlopers into the assemblage or were food remains. The argument was that gophers naturally occur-
ring in the area would die in their burrows and leave behind complete skeletons with little or no fragmentation. However, gopher parts expected to be recovered given the screen size used were not recovered at this site. In addition, comparison with ethnographic accounts of utilization of similarly sized animals revealed that humans would have discarded many of the bones recovered before processing or consumption. For both reasons, the gophers were interpreted as human food remains rather than as natural, died-in-hole specimens.

**DESIGNING EXPERIMENTS**

Probably the biggest issue in developing an experimental design is ensuring that the results will be useful for archaeological interpretation. This requires a consideration of how to make the experiment as robust as possible and of how it should be appropriately applied (see previous section). Reasonable linking arguments between the experimental results and comparable archaeological situations are clearly keys to success. Many issues of concern in designing experiments for zooarchaeology are common to all scientific experimentation, such as stating assumptions clearly, attempting to control for as many variables as possible, considering the limitations of one’s instruments, using control samples, evaluating results with proper statistics, repeatability, and similar factors. A good starting point is a consideration of the overall objectives, including what variables will be examined and which will be held “constant.” For example, Lubinski (1996) found a pattern of fish-head elements predominating over trunk and tail elements at an archaeological site and designed an experiment to help determine if this pattern was likely to occur “naturally” because of anatomical differences in skeletal parts. This is a good example in part because of its familiarity to the author but also because it has both strong points and weaknesses useful for illustration in the discussion that follows.

In addition to the issues discussed previously, another concern common to experimental zooarchaeology is representativeness—to what extent are the experimental data representative of the phenomena being investigated? There are ways to evaluate this directly, such as by resampling one’s own experimental data (see also Mooney and Duvall 1993), but in general larger samples are more likely to be representative. Samples in zooarchaeological experiments are often small, which can lead one to question the wider applicability of the results. In experimental design, it is probably best to limit the number of “situations” modeled and provide large samples for those few cases rather than use a wide variety of cases, each with a small sample size. For example, if the objective is to determine natural patterns of decay or survivorship in skeletal part distributions of fish subject to burial (Lubinski 1996), there are many possible situations to model, including different species, burial durations, and burial environments. Although the most widely applicable findings will result from a large array of species and modeled burial environments, each with a large number of samples or runs, it is generally impractical to carry out such work.
Instead, one may choose to experiment with one species of fish and only two model burial environments in order to have larger samples. Although this may limit applicability to similar fish species and burial settings, at least there will be robust samples from which to derive basic insight into the patterns.

Use of control samples is another consideration. While not always practical, their use can measurably strengthen an experiment. For example, such controls would have been helpful in Lubinski’s (1996) fish bone experiment. Bones had been soaked in aqueous solutions (one acidic and one alkaline), dried and weighed at regular intervals, and the weight loss curves used to model bone decay over time. The data showed a significant difference between bone loss in acidic compared with alkaline solution, but use of a control set in pH-neutral solution would have allowed the researcher to note whether the alkaline solution produced results any different than those obtained with neutral water.

Some experiments require thought about the relationship between rates and magnitudes of the phenomena modeled. This is most obvious where an experiment is meant to inform us about trends that might take hundreds or thousands of years to develop in the archaeological record. Clearly, some linking argument will have to be made to relate findings of a short-term experiment with long-term archaeological phenomena. For example, Lubinski’s (1996) experiment placing bones in aqueous solution used somewhat extreme pH values to argue that the trends observed with “very” acidic (large-magnitude) short-term (fast-rate) experiments were likely in the same directions as those obtained with “moderately” acidic (small-magnitude) long-term (slow-rate) archaeological situations. This was an obvious concession to the problem of time, as it would have been impractical to wait several hundred years for experimental results. An alternative approach is to model time by use of heating or perhaps boiling to speed up weathering (Roberts et al. 2002).

One use sometimes made of experimental zooarchaeology data, particularly in taphonomic studies, is the “correction factor.” For example, if an experiment showed that salmon bones were destroyed at twice the rate of deer bones, an analyst might wish to “correct” a site’s bone count by multiplying the salmon bone count by a factor of two. Although it sounds reasonable and could provide interesting comparative information, this is dangerous for several reasons. First, it is unknown how similar the taphonomic history of the experimental case is to the archaeological case and thus how similar the mathematical relationship might be. Second, the sample in the experiment is unlikely to be sufficiently robust to have a high degree of confidence in a particular numerical relationship. That is, the trends and directions might be robust (e.g., deer bones survive better than salmon bones), but one should be conservative with the experimental numbers. Finally, if used beyond a simple comparison and actually used to “correct” the data for further interpretations, the corrected numbers to which other samples would be compared would not be actual data but instead an extrapolation of the data with no known level of accuracy. In the case of the application of screen size correction factors (e.g., James...
this extrapolation precludes the possibility that recovered fauna might differ between recovery contexts (Cannon 1999).

An additional consideration in zooarchaeological experimentation is not so much design as reporting. In reporting an experiment, it is important to be thorough and unambiguous in describing the materials and methods used so that others can replicate the experiment for verification. Objectives, assumptions, and limitations should be clearly and honestly enumerated, as should any mishaps in execution of the experiment. The more comprehensive the reporting, the more broadly useful the experiment will be.

**RETROSPECT AND PROSPECT**

We believe experimental zooarchaeology has and will continue to supply important analogs for archaeological interpretation, but only when well conceived and cautiously used. All of experimental archaeology—including experimental zooarchaeology—has made progress, in part as a result of numerous carefully designed and executed experiments. While significant progress has been made, it still pays to consider Ruth Tringham’s comments on experimental archaeology published in 1977, if only to ensure careful design and application: “I shall argue that experiments in archaeology have for the most part been justifiably ignored because of (1) their lack of [a] strong theoretical base and a resulting lack of general applicability in testing archaeological hypotheses (this is also true of many ethnoarchaeological observations), and (2) their lack of rigor and attention to scientific experimental procedure in design, execution, recording, and analysis” (Tringham 1977:171).

Much progress has been made with regard to Tringham’s second point, but there is room for improvement. Recent comments by Denys regarding experiments in taphonomy highlight some of these areas: “The experimental work remains timely and is generally not replicated. The samples used for the experiments are in most cases too small to be statistically significant, and the protocols are not always detailed in the published papers” (Denys 2002:480).

We believe all of these problems can be resolved. In addition to clearer statements on experimental protocols, there needs to be continual testing of prior results as well as expansion of variables and processes considered. These are necessary to better use these data for interpreting archaeological assemblages that result from multi-agent, complex processes that operated either simultaneously or in unknown combinations and sequences in the past. Although the best samples and experimental replication might most easily be generated by large research programs with several collaborators working intensively on an issue over time, small experiments can still contribute a great deal. Indeed, both authors have published results of small experiments undertaken as students for class projects (Lubinski 1996; Shaffer 1992a). With a concerted effort toward well-conceived and reported experiments at all scales, experimental zooarchaeology can move beyond providing “cautionary
tales” to providing more robust analogs for interpreting the past.

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