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[CHAPTER ONE]

Introduction

Erik J. Marsh and Jeffrey R. Ferguson



SOURCES OF ANALOGIES

Much of what archaeologists understand about variation in material culture and its behavioral correlates is derived from studies that create analogies with past behavior using modern material procurement, manufacture, use, reuse, and discard (Mathieu 2002; Stone and Planel 1999). These analogies generally describe two divergent methodologies that share a theoretical base: ethnoarchaeology and experimental archaeology. This volume focuses on experimental archaeology, “the fabrication of materials, behaviors, or both in order to observe one or more processes involved in the production, use, discard, deterioration, or recovery of material culture” (Skibo 1992a:18). This methodology offers a high degree of control of variables and explores specific research questions not usually accessible in ethnoarchaeological studies.

While ethnoarchaeological research can observe the production of material culture in a wider context, it can also be prone to the erroneous assumption that “all technological knowledge is explicit and can be elicited from any practitioner of the technology” (Schiffer and Skibo 1987:596). Informants in ethnoarchaeological projects may not want or be able to clarify the production and use of their own materials, whereas experimental archaeology can directly address questions such as

how different tempers affect thermal performance characteristics of ceramic vessels (Beck, Harry, this volume; Schiffer and Skibo 1987). Ethnoarchaeological research has great utility for examining material culture in its social context, but experimental archaeology is preferred for isolating the effects and relationships of small sets of related variables (such as how stone flake length relates to platform thickness and angle).

The common theoretical foundation of ethnoarchaeology and experimental archaeology allows researchers to tack between complementary analogies generated by the distinct methodologies (Skibo 1992a). While this volume focuses on experimental archaeology, its authors relate their projects to inferences from ethnoarchaeology; their results provide hypotheses to be tested in less controlled ethnoarchaeological settings, where the effects of unanticipated or untested variables and factors can be observed. Future research stands to profit from continued close integration of these two methodologies in comprehensive research programs.

EXPERIMENTAL ARCHAEOLOGY

Izumi Shimada (2005:608) has lamented that little has changed in the decades since Ruth Tringham (1978:171) argued 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 . . . and (2) their lack of rigor and attention to scientific experimental procedure in design, execution, recording, and analysis.” While experimental archaeology may not always “furnish a foundation for explaining technological variation and change” (Schiffer et al. 1994:198), it is having increasingly greater influence on archaeological inference, as is its close cousin, ethnoarchaeology (Skibo 1992b; Stark 2003). This trend has developed largely through the efforts of Michael Schiffer, James Skibo, and colleagues (1994), who have implemented integrated experimental programs based soundly in scientific methods and directed expressly at archaeological issues.

The chapters in this volume build on the foundation established by Schiffer and colleagues (1994) to contribute more directly to archaeological inference through controlled experimentation. To accomplish this, experiments are theoretically contextualized and conducted with rigorous attention to research design and procedure. These foundational principles allow the modern analogies generated by experimental archaeology to clarify past behaviors and practices. The projects presented here are “nested within families of related principles” (Schiffer et al. 1994:198) as well as within suites of related experiments, as part of long-term, multifaceted experimental research programs. Such programs explore diverse behavioral patterns and their relationships in the complex, long-term processes of site formation, moving research well beyond Tringham’s (1978) criticism.

Future research in experimental archaeology may be able to take cues from recent developments in ceramic ethnoarchaeology. While the chapters in this

volume generally follow the “behavioral archaeology” approach developed in the 1980s by Schiffer and colleagues (1994), recent research in ceramic ethnoarchaeology shows a much broader range of theoretical approaches (Stark 2003:199) that may foreshadow new avenues for experimental archaeology. Another similarity may lie in the regional focus of many Americanist ceramic ethnoarchaeologists, who often overlook research from other areas of the world or research not published in English, leading to the fragmentation of ceramic ethnoarchaeology (Stark 2003:215). Within experimental archaeology, it may be prudent to engage current theoretical tensions emerging in ceramic ethnoarchaeology and better integrate research from around the world.

RESEARCH DESIGN

The chapters in this volume follow coherent and consistent research designs and procedures, focused on the goal of contributing to inference, such as developing material expectations for archaeological data (Lubinski and Shaffer, this volume). The authors place their experiments in a theoretical context, making comparisons either to similar experiments conducted by the same research program or to other well-documented experiments. Experiments establish themselves as relevant to archaeological inference through research designs that explicitly address existing theories based on previous research.

Philip Carr and Andrew Bradbury (this volume) suggest that research design can be improved by heeding lessons from other sciences, such as A. Franklin’s (1981) characterization of “good” experiments. For Franklin, three types of good experiments—crucial, corroborative, and new phenomena—relate data to theory. Crucial experiments support an existing explanation or theory over an alternate explanation. Corroborative experiments furnish support for the basic idea of a single theory. New phenomena experiments produce results not expected based on existing explanations or theories, which may lead to the development of new theories. Karen Harry (this volume) follows a similar theoretical approach, arguing that experiments begin with a hypothesis or question that can be made more or less plausible by examining its consequences. Hence, effective research design makes explicit relationships with existing theories, and well-documented results can be directly integrated into subsequent experiments (Kingery 1982).

While archaeologists routinely work with small sample sizes from non-repeatable excavations, experimental archaeology provides a unique opportunity to corroborate conclusions through multiple trials of repeatable experiments. This methodology is fundamental to any scientific experiment relevant to theory (Carr and Bradbury, Harry, Lubinski and Shaffer, this volume) and can provide data otherwise unavailable to archaeologists. In the excavation of archaeological sites, “[n]o matter how much we dig, we work with impossibly small, unrepresentative samples of data” (Lekson 2008:13). However, data from controlled experiments

can be completely recovered and resampled to better assess how well small samples represent a larger population (e.g., Lubinski and Shaffer, this volume). Experiments can test for equifinality, where multiple causes lead to the same effect—a persistent issue in interpretations of archaeological data (Carr and Bradbury, Lubinski and Shaffer, this volume). For example, the damaged edge of a lithic artifact may be the result of diverse combinations of human and non-human influences, which can be sorted out through experimentation (Bamforth, this volume). Multiple trials of controlled experiments that explore alternate causes with a common result can directly contribute to reducing ambiguity in the interpretation of archaeological data.

These advantages of experimental archaeology result from adherence to the scientific method and, as the authors in this volume emphasize, from multiple repetitions of the same experiment that explore alternate possibilities. To address this strategy, the authors outline step-by-step methods specific to their materials that will guide future experiments. This degree of standardization is uncommon in traditional archaeological research, but it is essential to experimental archaeology.

DIVERSE RESEARCH SETTINGS: GREATER AND LESSER CONTROL OF VARIABLES

Experimental archaeology offers the choice to control some of the manifold variables involved in the use of materials in the past. To clarify the complex interactions of these variables, experimental archaeology has developed sophisticated modern analogies (Mathieu 2002; Stone and Planel 1999). Experimenters can decide how carefully to control variables and tack among highly controlled lab settings, more “natural” field settings (Lubinski and Shaffer, this volume), and, in some cases, ethnoarchaeological observations. These complementary methodologies share a common theoretical base (Schiffer 1987; Skibo 1992a, 1992b; Stark 2003; Tringham 1978), although with different research foci and potential. Many effective research programs integrate lab, “natural,” and ethnoarchaeological observations, an approach that has the most potential to provide data that directly impact archaeological inference (e.g., Skibo 1992b).

The research presented in this volume focuses primarily on controlled laboratory experiments, but it also relies on related experiments and research from field and ethnoarchaeological settings. Controlled laboratory experiments are characterized by their replicability and tight control of very few variables, usually in a laboratory setting. Field experiments relax control of variables to more closely replicate possible prehistoric situations, thereby becoming less repeatable and more subject to equifinality (Lubinski and Shaffer, this volume). Different settings are appropriate for different research questions, and the most effective projects use a variety of approaches (Harry, Jolie and McBrinn, this volume).

Greater Control of Variables

Highly controlled experiments examine a narrow range of variables in laboratory settings under replicable conditions and suggest relatively “low-level” principles that may seem far removed from archaeological inference (Skibo 1992a, 1992b). Such experiments often focus on the physical and mechanical properties of materials humans use to make tools, most commonly flaked stone (e.g., Dibble and Pelcin 1995; Pelcin 1997; Speth 1975). These low-level experiments often describe universal properties of common materials and apply to the same archaeological material regardless of cultural setting. Greater control of variables also allows for double-blind tests, which have direct bearing on the interpretation of use-wear on archaeological artifacts (Bamforth, this volume). These experiments isolate few variables, so they must be related to other experiments and archaeological observations to have wider relevance.

While highly controlled experiments can be conducted in laboratory settings, projects aimed at learning how real people used real tools to accomplish real tasks in the past should “replicate realistic use contexts to the extent possible” (Bamforth, this volume; see also Harry, this volume), taking into consideration the necessary control of variables appropriate for the specific research question. In use-wear analysis, Douglas Bamforth (this volume) recommends incorporating both field settings for making and using tools and laboratory settings for analyzing use-wear, similar to Harry’s (this volume) approach in selecting and gathering clays to make and test clay tiles in a laboratory.

In another example described by Harry (this volume), controlled laboratory experiments suggested that ceramic vessels with interior surface treatments boiled water more efficiently than those without untreated interiors (Schiffer 1990; Skibo 1992b). However, the same experiment repeated in more “natural” field settings showed that cooking food in vessels without interior surface treatments quickly clogged their pores. After food had been cooked in the vessels, they became as efficient at boiling water as vessels with interior surface treatments (Pierce 1999). Hence, multiple related experiments in different settings have the best potential for understanding past and present cooking practices.

In almost all experiments, the most difficult variable for which to control is the human user. If archaeologists conduct an experiment, they can more closely control the process to fit their ends. However, experimenters are often a poor proxy for those they seek to understand—people who were likely from a different time and culture, who were more skilled at making and using their material culture than are those conducting the experiment, and who have very different goals (Adams, this volume). To address this issue, Raymond Mauldin’s (1993) experiment with ground stone was conducted by a Bolivian woman who was more familiar with, and more expert in, the use of the tools (Adams, this volume). Flintknapping experiments have consistently documented significant differences between amateur and

expert knappers (Bamforth and Finlay 2008), and similar differences are evident in atlatl throwers' distance and accuracy. John Whittaker (this volume) uses data from recreational atlatl competitions as a general guide to the distance and accuracy prehistoric experts may have achieved with the weapon. These examples show how understanding the range of variation in selected human-related variables may help produce a stronger analogy with past behavior, especially when combined with more controlled laboratory analysis.

Lesser Control of Variables

Some ambitious experimental research programs have relaxed control of variables to observe large-scale aggregate effects. Such programs are difficult to repeat but are well suited for generating hypotheses and conducting related controlled experiments. Research conducted since 1972 at Butser Farm, England, has developed a setting analogous to an Iron Age farm. Within this setting, individual controlled experiments have been conducted using agriculture techniques and technology, such as crop rotation, types of manuring, use of livestock to work the fields, soil types, and arable weeds; other experiments have focused on grain storage, construction of buildings, earthworks, metallurgy, and kiln technology (Reynolds 1999a). A similar project at Lejre, Denmark, founded in 1967, builds on earlier Dutch experiments (see Steensberg 1979) and includes experiments focused on tools and construction techniques (Rasmussen and Grønnow 1999). Axel Steensberg's experiments in swidden agriculture using imitations of Neolithic tools, conducted in the 1950s, also incorporated large numbers of variables. As in other projects with many uncontrolled variables, these experiments reached few conclusions but did generate hypotheses and ideas to be tested in more controlled settings (Steensberg 1979).

Complex experiments with minimal control over variables are prone to an array of complications. In a long-term project similar to Butser Farm, the Pamunkey Project developed a rough analogy to a Middle Woodland period (ca. AD 1000) settlement in the southeastern United States. This scientifically conceived project produced and analyzed over 700 tools made of various materials, but it ended prematurely as a result of logistical problems, including disputes with the landowner (Callahan 1976). In a much smaller project, Nick Barton and C. Bergman (1982) attempted to compare the spatial dispersal of ancient and experimental lithic scatters in a sand matrix. However, unexpected bioturbation quickly "destroyed" the lithic scatters.

COMPREHENSIVE RESEARCH PROGRAMS

The authors in this volume unanimously call for comprehensive research programs, in contrast to isolated experiments—referred to variously as archaeological, explor-

atory, orientational, or imaginative experiments (Amick, Mauldin, and Binford 1989; Ascher 1961; Malina 1983; Schiffer et al. 1994). The relatively low cost and ease of conducting “weekend” experiments (Schiffer et al. 1994) make it common for such experiments to exist in isolation, lacking archaeologically relevant research questions, coherent research designs, and appropriate methods. In many cases the results of such experiments are not fully reported and cannot be integrated into the body of knowledge generated by experimental archaeology. The authors here reiterate the need for consistent and appropriate research methods, fundamental to the goal of contributing to archaeological inference.

In an effort to gain a more thorough understanding of ancient tools, their users, and their makers, effective research programs may include more perishable materials, such as the wood and fibrous hafting of projectile point weapons (Bamforth, Whittaker, this volume). While most experiments focus on materials recovered in archaeological sites, perishable items were both meaningful and functional in the lives of those who produced and used them. Compared with less perishable items such as stone tools, perishable artifacts constitute the vast majority of items in artifact inventories—especially those of hunter-gatherers—but they have received relatively little attention (Jolie and McBrinn, this volume). Given the generally poor preservation of such items, the cohesive program advocated by Edward Jolie and Maxine McBrinn (this volume) is especially necessary for a clearer understanding of these materials. The development of research programs that include perishable materials may be one of the few ways to approximate relationships between durable and perishable items. Such relationships may otherwise remain obscure given a research and preservation bias toward stone, as in the case of the Lignic period in Southeast Asia, when crude stone tools were used to make sophisticated wood tools (Ingersoll, Yellen, and MacDonald 1977). The atlatl is a synthesis of perishable and nonperishable materials, but archaeologists usually recover only a portion of this composite tool. Through trial and error and controlled tests, Whittaker (this volume) makes a step toward understanding how people made and used atlatls in the past.

In addition, comprehensive experimental research programs must address taphonomy, especially studies that aim to replicate artifacts as found by archaeologists (Carr and Bradbury, this volume). Further, taphonomy is a crucial aspect of comparative use-wear studies (Bamforth, this volume). More than other material classes, animal bones are especially affected by taphonomic processes (Lyman 1994); hence, they constitute a critical aspect of zooarchaeological experiments (Lubinski and Shaffer, this volume). In the case of bone tools or faunal technology, taphonomic processes may remove many details such as use-wear and cut marks (Bement, this volume), also an issue with stone tools (Bamforth, this volume). An advantage of experimental archaeology is that taphonomic variables can be controlled. While taphonomy is not the focus of this volume, it is indispensable to a full understanding of site formation processes (Schiffer 1987).

Experimental archaeology is especially appropriate for testing noncultural variables that do not vary by region, such as the physics of stone fracture, the chemical and mechanical processes of seed crushing (Adams, this volume), or the ways bones fracture (Bement, this volume). However, most factors in the formation of archaeological assemblages vary spatially, temporally, and culturally. Repeating experiments in other regions using alternate materials can help define the extent to which results are locally specific and suggest possible sources of variability (Jolie and McBrinn, this volume). For example, field-setting experiments may be limited to include only materials that were likely available in the time period and region under study. Repeating experiments in different regions using regionally specific materials is a simple way of increasing an experiment's relevance to regionally focused archaeologists (Carr and Bradbury, this volume).

Repetitions of regional experiments can suggest the degree to which results can be generalized to other regions. Larger experimental programs are well suited to careful documentation of long-term site formation processes, a particularly difficult variable to replicate or observe. For example, Anna Behrensmeyer's (1978) case study of bone weathering in Africa was based on data from a "natural" setting and showed how physical and chemical agents break down bone. While this study has been applied worldwide, regional taphonomic studies in Argentina emphasize differences with the patterns seen in Behrensmeyer's African study, based on bones from local animals exposed to local soils and weathering patterns (Belardi and Rindel 2008; Gutiérrez 2001). To clarify human decomposition in northern European bogs, Heather Gill-Robinson (2002) used fetal pigs to derive archaeological expectations for decaying human tissue in bog environments. At Overton Down, farm objects were placed in depositional contexts to be excavated after 2, 4, 8, 16, and 32 years, and so on (Bell et al. 1996). After placing leather, bone, pottery, and standardized plastic, experimenters were able to track the movement and degradation of different artifacts in an actively cultivated field. At Butser Farm, over 90 percent of plastic pieces remained within 2 m of their starting point, discrediting the notion that agriculture and cultivation widely disperse material culture (Reynolds 1999b; Schiffer 1987:129–131).

Experimental programs that integrate different materials, regions, experimental settings, and alternative hypotheses have the greatest potential to contribute to an understanding of archaeological data and argue for their larger role in archaeology (e.g., Skibo 1992a; see examples in Shimada 2005). For example, controlled experiments have shown that taphonomic processes vary greatly for different parts of a bone (Todd 1987), results that have contributed directly to the interpretation of human-animal relationships during the Formative period in central Argentina (Izeta 2007). Another Argentine project combined data from controlled bone fracturing experiments with ethnohistorical reports of local butchering preferences to describe the influences of butchering practices and taphonomic history on archaeological bone assemblages (Miotti 1998). In the United States, Schiffer and Skibo's

(1987) experimental studies examine the role of ceramic temper in the shift from Archaic to Woodland ceramic technologies in the eastern United States. In the Near East, trial and error suggested the best tool shape for harvesting different types of locally available grains. Laboratory microwear analysis of these tools developed clear material expectations for the analysis of lithic tools from archaeological sites (Anderson 1999).

CONCLUDING THOUGHTS

Compared with the logistical challenges and difficulties posed by excavation and ethnography, experimental archaeology is often more accessible to professional, student, and avocational researchers. This allows for research at low cost, with minimal or no travel, that does not disturb or destroy archaeological materials. However, projects that are easily carried out often exist in isolation, lack proper relevance to questions of interest to archaeologists, employ research designs and methods that undermine the results, and fail to fully report methods, if the results are reported at all. This book offers examples of experiments carried out in the context of relevant theory, with the goal of achieving broader archaeological or anthropological relevance.

Researchers typically turn to experimental research to help them understand or test hypotheses developed during the study of archaeological materials. For example, to explain a temporal shift from side to corner notching in an archaeological assemblage of projectile points, a researcher might hypothesize a specific functional difference related to breakage. The researcher in this case already has the question in hand and may then develop an experiment to quantify the fracture resistance of the two notching forms. The chapters here are designed to help researchers at this stage of the investigation. How does one design a proper experiment? What variables need to be controlled and tested? To what extent can modern materials substitute for those used in the past? What common mistakes can be avoided? The answers are found in the chapters that follow.

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