Experimental Archeology[1]

ROBERT ASCHER
Cornell University

INTRODUCTION

THE term experiment appears in a number of archeological contexts. Generally it is used in connection with either field or analytic methods. In both categories it most often means a trial; a test undertaken for the purpose of evaluating a new method. The appendix title, "Experimental Techniques," in Atkinson's *Field Archaeology*, for example, refers to new field methods which were not commonly in use in 1946, the date of publication, but which were undergoing trial. Willey's (1953:1) study of settlement patterns in the Viru Valley, Peru, termed an experiment, involves testing both a field and analytic method and Rouse's (1939:7, 9) *Prehistory in Haiti*, also called an experiment, is a test of a proposed analytic method.

Something analogous to the thought or imaginative experiment (Benjamin 1936:257) is often used preparatory to field work. Thus Thompson's (1954) excavation of a Roman aqueduct is guided by entertaining and manipulating mental images of the course the aqueduct reasonably could have taken. Less commonly, archeologists perform comparative experiments (Cox 1958:4) in field methods. In "Observations on the Efficiency of Shovel Archaeology," for example, Meighan (1950) tries two field methods with the end of evaluating their efficiency relative to each other. Experiments have also played a role in evaluating whether the shape of certain objects resulted from human or natural agencies. Experiments in this category are most often associated with the enigmatic eoliths of the Old World (for example, Barnes 1939), but similar experiments relevant to New World problems have also been performed (for example, Harner 1956).

Another category of experiments entails operations in which matter is shaped, or matter is shaped and used, in a manner simulative of the past. These experiments, which I call imitative experiments, differ significantly from all the above. The aim of imitative experiments is testing beliefs about past cultural behavior. If archeology is taken to be the study of past cultural behavior, the imitative experiment is the keystone of experimental archeology. The present study is concerned solely with the imitative experiment.

The importance of the imitative experiment is limited to the kinds of problems in which they can be executed. The fact that these problems, dealing mostly with subsistence and technology, cover a relatively narrow range in the total cultural spectrum, does not diminish their utility, for the bulk of archeological data consists of evidence relevant to these areas. The importance of the imitative experiment, therefore, is best judged in terms of its contribution to the solution of those kinds of problems for which there are archeological data and which, because the data exist, are most often handled by the archeologist. The importance of the imitative experiment might be demonstrated by example.

Legitimate archeological evidence for the practice of agriculture consists principally of either tools believed to have been used in agriculture or botanical remains of domesticated plants. Due to differential preservation, the latter seldom survive, the former are often preserved. In 1934 Garrod reported the recovery of "sickles" from a cave in Mount Carmel. The find excited interest because the "sickles," if they could be interpreted as agricultural tools, would provide evidence for one of the earliest introductions of agriculture in the Near East. The question arose as to whether the luster on the flint edges of the "sickles" was the result of cutting wood, bone, or grass. If it could be demonstrated that the type of luster evident on the "sickles" could be the result of cutting grass, and could not be the result of cutting bone or wood, then the practice of agriculture could be inferred. Previous to Garrod's report, Spurrell (1892), Vayson (1919), and Curwen (1930) performed experiments with parts or replicas of "sickles" or "sickle-like" objects. In these experiments various materials were cut in order to test the belief that different materials would produce different kinds of luster and, hence, the use to which the tool was put could be inferred from the luster. The results of these experiments were inconclusive. After Garrod's report, Curwen (1935) refined the experiments by more closely simulating the amount and kind of wear to which "sickles" would be subject. The results of this imitative experiment indicated that bone, wood, and grass produce distinguishable differences in luster. The type of luster produced by grass was similar to the luster on the sickles from Mount Carmel.

The performance of imitative experiments is not a recent phenomenon in archeological research. Fashions in every discipline change: imitative experiments, once in vogue, are now seldom performed by professional archeologists. At the turn of the last century, which saw archeology "come of age" (Daniel 1952:122), imitative experiments were being performed with regularity. During this era imitative experiments were being performed by, for example, Nilsson (1868) in Denmark, Pfeiffer (1912) in Germany, Evans (1897) in England, Lartet in France (Lubbock 1878:561), Heierli in Switzerland (McGuire 1894:724), and by Cushing (1894), Sellers (1886), and McGuire (1891; 1892; 1893; 1894) in the United States.

In spite of the fact that numerous leading prehistorians (Childe 1956:171; Clark 1953:353; Leakey 1953:30; Movius 1953:165) have dwelt on the potential value of the imitative experiment, this potential has never been realized. The imitative experiment has failed to receive general acceptance because the evaluation of the procedures and results of such experiments are ambiguous. This ambiguity can be traced in part to the fact that the locus of the imitative experiment, and the theory and logic involved in executing imitative experiments, are unclear.

The intent of the present study is not historical, nor is it intended to be a review of imitative experiments already performed. Although rooted in the past, the purpose of the present work is future directed: it is hoped that the study of the nature of the imitative experiment will provide a sound basis for future experimentation.

This study is the first formal examination of any of the tests which are used by the archeologist to transform a belief about what happened in the past into an inference. In future studies I will consider other aspects of the logic of archeological interpretation.

THE LOCUS

The imitative experiment can be used by the archeologist to transform a belief about what happened in the past into an inference. The execution of an imitative experiment involves simulating in the present time that which is believed to have happened in the past in order to test the reasonableness of that belief. Its utility is that of a testing mechanism; its locus is within the inferential process. A formal consideration of the inferential process, and an example of it as it applies to archeology, will serve to identify the locus of the imitative experiment with precision.

According to Thompson (1958), the inferential process can be viewed as a four-step sequence: 1) the recognition of the indicative aspects of the data; 2) the formulation of an indicated conclusion; 3) the introduction of probative data; and 4) the formulation of a probable inference. The sequence is illustrated in the following example in which Thompson (1958:61) demonstrates the formulation of an inference about the probable use of the category "small ceramic bowl" from a Mesoamerican collection. A group of bowls have already been separated from larger bowls on the basis of the idea that the size distinction is relevant. It is important to note that, according to Thompson (1958: 148), the probative data brought to bear on the indicated conclusion (working hypothesis) serves to test the original basis of the typological classification "small bowl," as well as the use of the small bowls. Thompson (1958:148) notes that although the working hypothesis has no alternative in the first stages of the inferential sequence, other hypotheses may develop with the introduction of tests.

Indicative data: Small bowl.

Indicated conclusion: The small bowl was used as a food dish.

Probative data: These vessels have a very small size and capacity.

They have an open and unrestricted orifice.

The form is almost identical to the half gourd which is extensively used for eating and drinking throughout tropical America.

Similar vessels are used for eating and drinking in other parts of contemporary Mesoamerica.

Probable inference: The small bowl was used as a food dish.

Consider the probative data. In testing the indicated conclusion or working hypothesis, three orders of evidence are introduced: 1) formal properties—

size and capacity of the vessels are viewed as limiting the range of its possible uses; 2) analogy—similar bowls are used as food dishes in a contemporary situation in the same geographic area; 3) skeuomorphism—the bowl resembles similar nonceramic objects which are used as food dishes. Generally, testing a hypothesis in archeology is a matter of analogy either to a specific situation which either is, or is believed to be, a direct continuance of an archeological situation, or by analogy to a more general situation which, although not directly analogous, is relevant. Other testing mechanisms, such as skeuomorphism and limitations imposed by formal properties, as illustrated in the above example, can also be used.

At least one other testing mechanism, not utilized above, can be of value. An experiment can be performed with one representative small bowl in the category small bowl to test whether the items in question could have been used in the manner suggested by the indicated conclusion. If Thompson had done this, he would have performed an imitative experiment.

Consider the following example in which an imitative experiment was performed. The example has been organized in the procedure outlined by Thompson, although originally it was not presented in this manner. The example is primarily meant to illustrate the locus of the imitative experiment within the inferential process.

(1) At Wupatki National Monument, Schroeder (1944:329–30) recovered a reconstructable jar embedded in an upright position in the ground. The vessel (dated A.D. 1075–1275) was situated 72 cm. below and 4 cm. in front of the ledge of a sandstone outcrop. Two natural depressions on the surface of the outcrop converged to a point above the mouth of the jar. A slab house and two cave shelters were within 20 meters of the jar.

(2) On the basis of the spatial arrangement and the desert environment at Wupatki, Schroeder reasoned that the purpose of the vessel was the collection of water.

(3) The fragmented jar was recovered and a container was substituted in its place. During a rainfall of .23 inches no water ran "out of the two depressions above the vessel on the outcrop." Hence, no water collected in the container.

(4) Schroeder interpreted the results of the experiment as follows: "a greater amount of rainfall is necessary (estimated .50 inch) before this method of collection would function. Rainfall of such magnitude occurs only in the late summer in this area and this would imply a seasonal method."

The testing of a belief about what is proposed to have happened in the past is the crucial step in the sequence which leads from the recognition of relevance to inference. This is so because an inference is only as convincing as the positive results of the tests which can be brought to bear on the hypothesis. The failure of a hypothesis to pass examination can lead to seeking alternative hypotheses, or can, as in the above example, lead to an important modification of the hypothesis.

FIVE CASES

Published accounts of imitative experiments range from the simple statements that experiments were performed (Fichardt 1957:53) to detailed reports (Steensberg 1943). Some experiments have been elaborate, taking years to complete (Steggerda 1941), others less involved have been executed in a few hours (Quimby 1949). Many of the most often cited experiments have been executed by nonprofessional archeologists (Cabrol and Coutier 1932; Mewhinney 1957; Pond 1930).

From this literature, five cases have been selected for presentation. The cases provide material for the examination of the theory and logic of the imitative experiment. The principal criterion of selection of the cases is the degree to which they mirror the theory and logic, never explicitly stated, which provides the conceptual frame within which the experiments are performed. Although an attempt has been made to represent several archeological problems and to detail the results of the experiments, these specific aspects of particular experiments are secondary in the present study. Complex experiments, for example the voyage of the Kon-Tiki (Heyerdahl 1950) and Israel's current experiments in prehistoric agriculture in the Negev (Evenari, Shanan, Tadmor, and Aharoni 1961), are not included in the cases because of the difficulties of concise description. However, the principles involved in more complex imitative experiments are no different than those of the comparatively simpler experiments detailed below.

Case 1. Cave painting media (Johnson 1957:98–101).

The people or peoples who painted animals and humans on cave walls in South Africa are unknown. The time that the paintings were executed is equally uncertain.

Using as models the cave paintings in the Clanwilliam area, South Africa, Johnson attempted to redraw the figures, varying the medium with each successive attempt.

First, the chemical composition, natural sources (animal, plant, and mineral), and present availability of pigment in the area of the caves were studied. Three sources of pigment, red ochre, bone, and lead oxide were chosen. Each is capable of producing a color present in the paintings. Of the three, only lead oxide does not occur in the immediate area.

The natural sources (plant and animal) and present availability of media in the cave area were then considered. As a result of this analysis, and after consideration of the suggestions of previous literature, eight media, wax resin, marrow fat, mutton fat, hyrax urine, plant juices, bile, honey, and tempera were selected. All are now available at the site.

Each of the media was then independently combined with the pigment and the eight resultant paints were applied to the cave wall using a quill and a bristle brush. Three principal criteria were used to evaluate the success of each medium: 1) the ability of the author to reproduce the fine lines found in the painting; 2) the amount of the medium needed to reproduce a figure; and 3) the retentive ability of the paint on the cave wall. Two media failed in all three criteria, three failed in two criteria, and two failed on a single criterion. Only tempera was successful in all aspects. The author evaluates the result of the experiment with the statement: "I find it difficult not to be dogmatic about its (tempera) being *the* method." The author notes that since an ostrich and a fat-tailed sheep are depicted in cave paintings in the Clanwilliam area, a source of tempera (ostrich eggs) and a source of mutton fat was available at the time the paintings were executed.

Case 2. The manufacture of "charmstones" (Treganza and Valdivia 1955:19–29).

The designation "charmstone" is used in the terminology of California archeology with reference to ground stone plummet-shaped objects which vary in length from 2 to 10 inches. Although the label implies the use of the object for magical ends, tests of that implication by analogy to specific ethnographic situations are inconclusive. The following investigation is concerned with their manufacture.

In an investigation reported by Treganza and Valdivia charmstones were made by Valdivia. Valdivia's technique of manufacture was by pecking and grinding; that is, the stone was worked in the following sequence: percussion fracturing, pecking, crumbling, and abrading.

The purpose of the investigation was "not simply to demonstrate that it was possible to recreate forms, but to experiment with various stone materials and tools known to have been utilized by Indians in the manufacture of pecked and ground artifacts." Specifically, the authors desired to learn "how various rock materials responded to different shaping tools, to understand perfection and latitude in the use of tools, the time factor involved in various stages of manufacture, and the casualty rate in production."

A total of eight tools were used in manufacturing the charmstones. The choice of some was based on tools recovered in association with artifacts in different stages of completion; for others, which were made by Valdivia, the criterion is not given. Presumably examples of them, with the exception of the bow-drill, can be found in archeological specimens recovered in California.

Two aspects of the materials from which the charmstones were to be manufactured were considered before the materials were selected: 1) the type of stone (e.g., steatite); and 2) the size and shape of the stone. The choice of type of stone was based on an examination of a series of authentic charmstones. Size and shape "approximated . . . the desired form of charmstones . . . on the assumption that the makers of the charmstones would . . . also have been interested in reducing the labor of pecking and grinding." Lithic materials used were sandstone of varying compactness and hardness and steatite.

The first three attempts at manufacturing charmstones failed. Two of the failures were due to heavy blows and one to "faulty material" from which the charmstone was to be made. Seven subsequent attempts were successful. The data for each of the seven successful specimens include: 1) the type of stone and its properties (compactness, hardness, etc.); 2) the tools used in manufacturing; 3) the technique of manufacture; 4) the time for pecking (percussion fracturing, pecking, crumbling); 5) the time for grinding and smoothing; and, 6) the total time. Drilling time is included in the data on two specimens in which a hole was drilled.

Blackwood's (1950) observations on the manufacture of ground stone objects by the Kukukuku of New Guinea are then quoted at length. It is noted that since Valdivia's work was conceived independently of Blackwood's report, the parallels between Valdivia's work and the work of the Kukukuku are of interest. McGuire's (1891) experiments in making ground stone objects are also introduced by means of extensive quotation. The choice of quotations leaves the reader with the impression that there are certain similarities between the quoted observational and experimental data and Valdivia's work.

Case 3. The notched scapula and ribs (Morris and Burgh 1954:61–63).

The scapulae or ribs of large mammals, showing considerable wear along a jagged or notched edge or edges, have been recovered from archeological sites in North America. Tools meeting this description occur in widely diverse ecological and cultural areas and at different time levels.

Huscher and Huscher (1943:37–38) recovered such tools in excavations at Mesa County, Colorado. Temporally, the tools may be Basketmaker. The jagged edge, according to the authors, originates in the manufacturing process. That is, the bone is deliberately broken in order to secure a working edge. After considering possible uses of the tools, which had been suggested in earlier literature (e.g., arrow-shaft smoothers, seed beaters), the authors infer that they were used as hide-scrapers. Their decision is based on the hunting emphasis at the sites they investigated, on analogy to "similar" tools which occur in Northwest Coast archeological sites, and on analogy to "similar" tools which are used for hide scraping by contemporary peoples of the Northwest Coast.

Morris and Burgh (1954) recovered notched scapulae and ribs from Basketmaker sites near Durango, Colorado. Taking issue with Huscher and Huscher, they contend that the tools were used in a different manner. Their experiment can be divided into two parts.

In the first part of the experiment a deer hide was stretched over a pole and first an unnotched deer rib, and then a notched deer rib, were used to remove tallow and hair from the hide. As a result of the scraping process, the unnotched deer rib was worn uniformly and no notches formed. The notched deer rib was less effective and the prongs between the notches broke. The authors interpret the results of the experiment as follows: "This suggestion (that the tool could have been used as a hide-scraper) we have disproved, at least to our own satisfaction."

The second part of the experiment proceeded as follows:

> . . . production of cordage was one of the major industries of any Basket Maker community and the principal source of fiber was leaves of the yucca plant. Therefore, we determined to ascertain if the notched tools would be serviceable in separating the fiber from the leaves. The yucca available was of the narrow-leafed species native around Boulder, Colorado. . . .

> The first step of the procedure that we eventually adopted was to strip off the edges of the leaf, which are more bark than fiber. . . . Next, the leaf . . . was slowly drawn across a peeled pole about 12 cm. in diameter and hammered with a smooth cobblestone until the bark was well macerated and the fibers considerably beaten apart. A rib from the North Shelter . . . was used to remove the pulp. . . . in less time than was anticipated, the strand of fibers was freed from the coarser bark and pulp. After about two hours of use, here and there the edge of the rib began to break through into cancellous tissue. Presumably the nicks so produced would have deepened into notches had we seen fit to continue the experiment indefinitely.
>
> For further cleaning of the fiber, we gathered the increments provided by individual leaves into hanks around 2 cm. in diameter, immersed them, and repeated the scraping process. Both a straight edged and a deeply notched rib were tried on the hanks. Each was effective, but the one with the notches more so than the smooth. . . .
>
> Having demonstrated that they are wholly effective for the purpose, we do not hesitate to identify notched scapulas and ribs as the tools with which the Durango people prepared yucca fiber for the making of cordage.

Morris and Burgh find "confirmation" of their interpretation of the results of the second part of the experiment in yucca fibers and sap found imbedded in a notched deer rib recovered at the site. They also cite as an "interesting parallel" an ethnographic example (Pima) in which a deer's scapula is used to separate pulp from fiber of a maguey plant.

Case 4. The arrow-shaft straightener (Cosner 1951:147–48).

A tool of common occurrence, especially in the later archeological periods of the western United States, is the arrow-shaft straightener. The distinguishing characteristics of the tool are the relative hardness of the stone from which it is made and a groove or number of parallel grooves which run the total length (usually from 2 to 6 inches) of the stone. It has been distinguished from a similarly grooved tool, the arrow-shaft smoother, on the basis of the hardness of stone; the smoother is generally fashioned from sandstone and presumably served as an abrader. The presumed use of the straightener, as its name implies, was straightening wood in order to make it usable for arrow shafts. Ethnographic analogies support the distinction between straightener and smoother.

Cosner (1951) had straightened wood to be used as arrow shafts without using a tool and had observed Pima Indians straightening arrow weed to be used as arrow shafts, also without the aid of tools. He was also familiar with reed cane shafts recovered with a child burial in the Tonto cliff dwellings at Tonto National Monument, Arizona. "Almost convinced . . . " that the tools designated as " . . . 'arrow shaft tools' actually had some entirely different function," he undertook a series of four tests.

The materials used in the tests consisted of: 1) a shale, 3-grooved, 2-inch, arrow-shaft-straightener recovered from a Pinto Creek site; 2) samples of arrow weed; 3) samples of reed cane; 4) water; and 5) a mesquite coal fire.

The first and second of the four tests were performed with arrow weed, the latter two with reed cane.

Tests 1 and 2 (arrow weed):

1. Four samples were successfully straightened into shafts by heating over mesquite coals and bending by hand. No tool was used.

2. The arrow-shaft straightener was heated and an attempt was made to straighten one sample with the aid of the heated tool. Presumably, the sample was passed through the grooves. No further samples were used because: "The first one absorbed so much heat that the shaft had to be heated twice before it was affected much."

Tests 3 and 4 (reed cane):

3. Four samples were "dampened and warmed, and an attempt was made to straighten them by hand." No tool was used. "All four were failures."
4. The arrow-shaft straightener was heated and the reed cane samples were dipped into water. Four samples were successfully straightened by passing them through the grooves of the heated tool. Cosner notes that, in addition to successful straightening, the diameter of the samples could be reduced at any point and the joints of the reed could be made even with the body of the cane.

Cosner interprets the results of the four tests in the following manner:

That the implement in question is really an arrow straightener . . . seems plain to me now. . . . This stone not only proved to be a good way to form cane; it is the only way I know of.

I am of the firm opinion that the stone is used only for the straightening of cane shafts. For solid shafts such as arrow weed it would not be necessary nor indicated.

Case 5. Copper smelting in the Old World (Coghlan 1940:57–65).

There exists evidence which indicates that copper was one of the first of the metals altered by man in shaping objects for his own needs. In nature, copper occurs in a virgin state and in the form of sulphide and oxide ores. Virgin copper may be directly shaped into small objects such as awls and pins by cold-hammering, annealing and hammering, or by melting and casting. In order to utilize the copper in copper-bearing ores, the ores must first be smelted. The knowledge of smelting was important not only because it released a potential source of copper but because the knowledge is necessary to produce copper alloys, such as bronze.

The knowledge that copper can be obtained from ores was probably obtained accidentally. The most widely accepted view of what that accident was, is that copper was discovered in the remains of a campfire into which some copper-bearing ore had been introduced unintentionally. Those who hold this view suggest that the first metallurgic hearths were campfires or simple "hole-in-the-ground" furnaces. Experiments by Coghlan bear directly on the problem of the origin of copper smelting in the Old World.

Coghlan constructed a "hole-in-the-ground" furnace. It consisted of a one foot circular hole surrounded by a stone wall three feet in diameter. A charcoal cone was constructed in the furnace and two layers of small pieces of malachite, separated from each other by charcoal, were imbedded in the cone. The charcoal was ignited and after it became hot more charcoal was added and the fire was permitted to burn for several hours. The experiment was performed on a windy day to permit as much natural draught as possible. The result was negative, i.e., no "useful metal" was obtained. The process was repeated with cuprite and results were again negative. Both malachite and cuprite are oxide ores. Sulphide ores were not considered necessary to experiment with as they "generally occur at a much greater depth than the oxidized ores, and so would not have been the first ores used by early man; also technically they are much more difficult to deal with." The experiments failed, according to Coghlan, because the "necessary reducing atmosphere" could not be obtained. Coghlan concludes:

> Since the failure of the experiments indicated that the camp-fire, or "hole-in-the-ground" fire was very unlikely to have been the first metallurgical hearth, the only suitable remaining source of heat would seem to have been the pottery kiln or furnace. As the use of pottery certainly predated the discovery of smelted copper by a considerable time, it is quite possible that the closed furnace, or possibly some form of reverberatory furnace, may have been in use. . . . If a copper carbonate ore such as malachite were introduced into a pottery kiln, the thermal conditions would be favorable for the reduction of the ore to take place; to test this, the following simple experiment was carried out.

Coghlan placed a "small lump" of malachite on a ceramic dish and inverted a ceramic pot over the dish. The arrangement was based on early pottery kilns which consisted of a dome of brick or burnt clay around which a fire was built. The "miniature kiln" containing the malachite was placed on hot charcoal and ashes and then imbedded in a cone of burning charcoal. After several hours the "kiln" was removed and its contents were examined. A spongy copper was produced. The experiment was repeated, but instead of using a malachite lump, the malachite was first "ground to a small size." The result was a compact copper loaf. Coghlan then states:

> The conclusion to be drawn from these experiments would seem to be that if a piece of malachite, or ground malachite, were left accidently in the baking chamber of a divided, or reverberatory pottery kiln, it would become reduced, and since the baking chamber would not contain any fuel, the resulting copper would be easily noticed.
>
> The question whether malachite could have gotten into a pottery kiln of course depends on whether malachite was ever used for the decoration or painting of early pottery.

Coghlan argues that there are two possible ways that malachite could have been used in pottery decorations: 1) as a slip or paint; and 2) as a constituent in a glaze. With regard to the first possibility Coghlan performed an experiment, the details of which are not given, which indicates that malachite, when used as a slip or paint, yields a "fine" black surface. An analysis of early Egyptian glazes shows 18.5 percent copper oxide. Since there is evidence that glazing was introduced into Egypt in the First Dynasty from an outside source, glazing would be earlier than the First Dynasty. Coghlan concludes:

> To sum up, it seems to the writer that the accidental reduction of a piece of copper ore, most likely the ore malachite, which led to the discovery of the knowledge of how to smelt copper from its ores, was probably made in a pottery kiln . . . .

GENERAL PROPOSITIONS

The archeologist, in performing an imitative experiment, seeks to test the reasonableness of such statements as "the medium used in these paintings was plant juices" (Case 1). Singular statements of this type, generally referred to as ideographic propositions, are thought to be the aim of historical, as distinguished from theoretical, endeavors (Nagel 1952:161–62). An examination of the above statement will show that propositions of a general character are also involved.

The statement "the medium used in these paintings was plant juices" is meant to describe the medium which may have been used by any member of a community and not by any specific individual within that community. Thus, the statement, while being special to a context (particular spatial area and time span), is not unique within that context. (Many individuals may have used plant juice as a medium on many different paintings.) The statement is, therefore, an implicit description of cultural behavior, where culture is taken to mean " . . . capabilities and habits acquired by man as a member of society" (Tylor 1871:1). The statement can be accepted as a legitimate proposition only if one is prepared to assume that: 1) cultural behavior is " . . . something more than an endless series of haphazard items . . . " (Kroeber 1948: 336); and 2) cultural behavior (e.g., the use of plant juices) can be inferred from its material results (i.e., in paintings).

It would be a mistake to believe that, because the archeologist performing an imitative experiment is primarily concerned with cultural behavior which is localized in space-time and in a culture, he does not implicitly employ general propositions. These propositions, generally considered within the domain of theory, are sometimes drawn from other fields. In archeology general propositions function, as they do in history (Danto 1956:20; Gardiner 1952: 45), as broad working hypotheses.

The first of two general propositions which serve in the performance of imitative experiments is drawn from general anthropology: "*All cultural behavior is patterned* (italics mine; Sapir 1927:118). The proposition is not novel: in fact, it is difficult to imagine how any effective inquiry could begin without the supposition that some kind of order exists in the subject matter of inquiry.

The particular kind of order subsumed under the pattern concept, key in the above proposition, has often been obscured by its ubiquity. The term has been used to umbrella a multitude of phenomena, at different levels of generality, investigated in diverse ways. For example, the phenomena embraced under the concept are as varied as society (Wissler 1923), plow agriculture (Kroeber 1948:313), a cultural personality syndrome (Benedict 1950), polite and impolite breathing (Sapir 1927:117), and taking or not taking a second mate (Kluckhohn 1941:119). Attempts at ordering the concept of pattern have sometimes compounded ambiguity by coining overlapping subsidiary concepts. Thus, Linton's "real patterns" (1945:45), Sapir's "action patterns" (1934:411), and Kluckhohn's "behavior patterns" (1941:117) refer essentially to the same kind of observed activity, on the same level of generality, investigated in similar ways. In spite of these and other difficulties (see, for example, Weakland 1951; Cohen 1948), there appears to be general agreement with regard to the ideas implied in the pattern concept.

In general, the kind of order which the term "pattern" connotes is that of direction, tendency, and slope. Compared with "law," a pattern is relatively less fixed and determined: a pattern is a regularity rather than a rule. A pattern consists of more than one element arranged in some systematic manner, such that one element in the pattern presupposes the other. This arrangement, or grouping of elements in a more or less regular fashion which tends to persist as a unit, is the distinctive feature of a pattern. The emphasis of those concerned with patterns is in the structuring of the elements, the recurrence of the pattern as a unit, and the relative strength of the interlocking elements which compose the pattern.

In the context of a single culture, a pattern has been conceived of as a model or guide—"something which someone follows in making something" (Radcliffe-Brown 1952:14). This notion of pattern is exemplified best in dress where, as Kroeber (1948:331) points out, "the first association of many women to 'pattern' is likely to be that of a paper model from which dresses are cut and shaped." That kind of order which applies to the pattern concept in general applies with equal force to the pattern thought of as a model.

In the present study, the pattern concept is limited in meaning to a model or a guide. Further, a pattern is portrayed as a dynamic concept; in this sense it becomes a procedure; that is, interlocking steps in a plan for getting something done. Thus, the following ethnographic description of the manufacture of a pounder on the island of Ua Huka in the Marquesan Islands is taken as a model for making a pounder.

> (1) An irregular piece of stone about three times the size of the finished implement is roughed out with heavy blows of the ax until it becomes a cone. . . . (2) This cone is then worked down by light blows on all sides until the general form of the finished implement is produced. (3) This is then smoothed by light, even blows with the corner of the ax until all the contours are perfect. (4) The neck and body of the implement are then shaved with the edge of the ax until the scratches left by the last process are obliterated. The head and base are left rough. . . . (5) The body of the implement is sometimes rubbed with a mixture of coconut charcoal and oil which gives it a shiny black finish contrasting pleasantly with the grey of the head (Linton 1923:337–38; insertion of numbers mine).

The ethnographic description of the manufacture of the pounder is a generalization grounded on the observation of on-going actions in the present. A man in a given day, for example, may make two items, a pounder and an "X." His actions with regard to pounder-making are separated from his actions with regard to making "X," coupled with the actions of others with regard to pounder-making, and generalized with the result that a model of pounder-making is detailed. The generalization is subject to test by further observation. Since some men sometimes polish pounders and some men sometimes do not (see step 5 above), the model is a description of a regularity.

In order to formulate a dynamic pattern, analogous with the ethnographic one, the archeologist would have to construct a sequence of manufacture. This construction would necessarily be based on the observation of actions as materialized or congealed into static forms. Thus, parallel with the above example, the archeologist might observe all the recoverable objects on Ua Huka, classify the objects into separate groups on the basis of features which are believed to be diagnostic, select out a group of bell-shaped objects (pounders) which he believed were manufactured in the same way, select out another group of objects (axes) which he believed could have been used in the manufacture of the bell-shaped objects, and, finally, construct and test a pattern of manufacture.

Although the forms in the archeological parallel are only two, bell-shaped objects and cutting tools, and the problem posed is manufacture, patterns may include other kinds of forms and problems. Consider, by way of example, two of the imitative experiments presented in the previous sections. The position of the jar relative to the outcropping, in the example in the second section, is a spatial arrangement relevant to the pattern of use, where the forms include a jar and a natural formation (outcropping). The case involving the scapulae and ribs (Case 3) is a problem which simultaneously involves use and manufacture. The sequence in this pattern, as constructed by Morris and Burgh, includes: 1) stripping the edges off a yucca leaf; 2) drawing the leaf across a peeled pole while hammering with a cobblestone; 3) scraping the leaf with a rib to remove the fibre from the pulp; 4) immersing the fibre; and 5) repeated scraping of the fibre with a rib. The sequence produced a notched rib, but a notched or an unnotched rib may be used in producing the fibre. Hence, the item, which might be a product of use in one instance, may become usable as a tool in another instance. The forms in this pattern include a yucca leaf, a peeled pole, a cobblestone, a fluid (kind not given), and a rib.

Although the particular forms and problems which constitute the subject matter of imitative experiments are varied, all have as a prerequisite the classification of artifacts into groups. In Case 2, for example, grooved, oblong stones had been divided into two groups on the basis of hardness of stone. The division was believed to be indicative of use: soft, grooved, oblong stones serving to smooth arrow shafts; hard, grooved, oblong stones serving to straighten them. In the experiment, Cosner might have tested the hypothesis that each class was used for a different purpose. However, he centered his attention on the belief that the class of harder objects had been used to straighten arrow shafts. The pattern he constructed in the experiment to test this belief included forms other than artifacts; for example, he used arrow weed and reed cane. The particular hard, grooved, oblong artifacts which he did use were real objects but, presumably, they were truly representative of the class of objects under examination. The same notions apply to other experiments: the designations "charmstone," "notched rib," "usable lump of copper," and "paintings" are tag names for classes or types of objects or representations, and the particular artifacts used in experiments are taken to be typical of those classes.

Approaches to formulating or grouping artifacts into classes have been a major theme in archeological literature for the past 30 years. Discussion has pivoted about the dual interest of archeologists in reconstructing cultural contexts and in establishing spatial-temporal relations. When interest is centered primarily on reconstructing cultural contexts, the objects recovered by the archeologist tend to be treated as "expressions of ideas and behavior of the people who made them" (Phillips, Ford, and Griffin 1951:61). When interest is concentrated primarily on spatial-temporal relations, the objects become instruments or markers used in the establishment of these relations. As Phillips, Ford, and Griffin have suggested: "one's approach to the problem of classification will depend largely on which of them [cultural contexts or spatial-temporal relations] is being served."

Those who have approached classification primarily as a means to aid in the reconstruction of cultural contexts, regardless of technique (Krieger 1944: 278; Rouse 1939:18; Spaulding 1954:392), apparently accept the following general proposition, in one form or another: Artifacts produced from the same scheme, or used according to the same scheme, exhibit similarities which permit their division into groups which reflect those schemes. Imitative experimenters work with classes developed on the basis of this proposition even if they do not always develop those classes themselves. This proposition, therefore, constitutes an implicit theoretical and methodological base for the execution of imitative experiments. The reasoning behind this proposition may be demonstrated by reference to the ethnographic example of pounder-manufacture given above.

Any individual on Ua Haku Island, familiar with the pattern for making a pounder, presumably would produce one which would look like, or be similar to, a pounder produced by another individual. Incomplete transmission of the pattern from generation to generation or within a generation, variation in manual dexterity, availability of proper materials, and other factors would result in observable variation in completed pounders. Nevertheless, the pounders produced according to the pattern for pounder-making would, presumably, look more similar to each other than to other objects produced in accordance with other patterns. Unquestionably, various factors, as indicated, would result in a range of physical properties of pounders: certainly no two would be identical.

If the above is true, then the archeologist should be able to separate bell-shaped objects from other objects and construct a class "bell-shaped object." Further, he should be able to choose one or a few type-specimens, where a type-specimen is defined as "*a real object designated to represent a class and serving as the basis for scientific description*" (Osgood 1942:23), or in the present context, serving as a basis for experiment. It need hardly be pointed out that any classification is an hypothesis. Classification is an "experimental activity of trial and error . . . there is no test for what is like and what is unlike except an empirical one" (Bronowski 1959:58). Taylor (1948:123), in particular, is insistent on this point: "There is no automatic, axiomatic assurance that the forms, types, and classes established today by the archaeologist are coextensive with any separable entities that existed in the minds or life ways of bygone peoples . . . any such correspondence is a matter for explicit hypothesis and testing, not implicit assumption."

Each imitative experiment is an attempt to test a belief about cultural behavior, relying implicitly on the first proposition: all cultural behavior is patterned. The statement of the hypothesis describing a particular pattern involves artifact classes and has implicit within it the second proposition: artifacts produced from the same scheme, or used according to the scheme, exhibit similarities which permit their division into groups which reflect those schemes. Taken together, the two propositions form the implicit broad working hypothesis of the imitative experiment.

THE LOGIC

At first glance the imitative experiment appears so unlike experiments in other disciplines that it becomes suspect. For example, the kind of order (pattern) with which imitative experimenters are concerned is cultural, not natural; hence it is not like an experiment in the natural sciences. The fact that the patterns existed in the remote past suggests that the imitative experiment is unlike experiments in the social sciences. If experimentation were defined by the order which is involved or the temporal locus of that order, it would be concluded that the imitative experiment is not really an experiment but is something else.

Experimentation is really a kind of activity which is common in daily experience as Conant (1950:7) has recently pointed out. An experiment is performed when a person tries to unlock a door with a key which he has not previously tried. In doing the experiment, the person may be thought of as saying to himself "If I turn the key, the lock will spring." In this case, according to Conant, "common-sense assumptions and practical experience determine the nature of the experiment. . . . " In more sophisticated experimentation "a series of connecting links usually relates some deductions of a broad working hypothesis with the final limited working hypothesis involved in the specific experiment."

In the present context, the broad working hypotheses are the general propositions detailed under theory. The limited working hypotheses are the ideographic statements made possible by the general propositions. The experiment is the testing of these ideographic statements. It is important to note that the general propositions are not under scrutiny in the experiment: the tenability of these propositions can be examined only in a contemporary ethnographic situation.

The essential ingredients in testing a limited working hypothesis, according to Peirce, consist of the following:

First, of course, an experimenter of flesh and blood. Secondly a verifiable hypothesis. . . . The third indispensable ingredient is a sincere doubt in the experimenter's mind as to the truth of the hypothesis. Passing over several ingredients on which we need not dwell, the purpose, the plan, and the resolve, we come to the act of choice by which the experimenter singles out certain identifiable objects to be operated upon. The next is the external (or quasi-external) ACT by which he modifies those objects. Next comes the subsequent ***reaction*** of the world upon the experimenter in a perception; and finally his recognition of the teaching of the experiment (Peirce 1934:424).

Peirce goes on to say that "the unity and essence of the experiment lies in its purpose and plan, the ingredients passed over in enumeration," but he does not elaborate on either. The purpose of the imitative experiment has already been detailed, the plan has not. An experimental plan is here conceived as the principles of reasoning or logic which facilitate the testing of a limited working hypothesis. The ingredients mentioned by Peirce are interwoven within that logic.

Consider the formal structure in the example given by Conant. The statement, "if I turn the key, the lock will spring," can be generalized to read: "If I do A, I will get B." Compare this with ethnographic pounder-making on Ua Haku presented above. In the first stage in the pattern, an irregular piece of stone is roughed into a cone with blows from an ax. The pounder-maker knows that if he applies the ax to a piece of stone in a certain manner he will produce a cone. Learning the pattern is, in effect, learning how to get certain results by performing certain operations. In the first stage he may be thought of as knowing "if I apply the ax to a piece of stone in a certain manner, I will produce a cone." The formal structure in this statement can be generalized in the identical manner as the statement in Conant's example: "If I do A, I will get B."

Conant's example and the first stage in pounder-making appear to differ in that the former is complete and the latter is a first stage in a five-stage pattern. The operations which the man at the door performs, however, can be viewed as a sequence. The placing of the key in the lock, for example, is a step which precedes the turning of the key. The operations of the man at the door can be viewed as a set A′, where A′ represents all the steps in the sequence relevant to the problem of opening the door. The five-stage pattern in pounder-making similarly forms a set A′, where A′ represents all of the related stages relevant to the producing of a pounder. Thus, the structure of both statements retains formal identity; "If I do A′, I will get B."

The example of opening the door and pounder-making is evidently different in other respects. The man at the door is testing the key in order to see if it works: the resident of Ua Huka knows that following the pattern will result in a pounder. Testing if the key will open the locked door is an experiment; making a pounder is not. It is the parallel formal structure of both statements, however, which supplies rationale for imitative experiments.

The manner in which the archeologist would handle a collection which included bell-shaped objects (pounders) from Ua Huka has already been detailed. Let it be assumed that the archeologist has grouped bell-shaped objects into a single category on the basis of what appears to be a common scheme of manufacture. His limited working hypothesis might be: roughing, shaping, smoothing, shaving, and polishing (or not polishing) produced the bell-shaped objects. The five steps in the proposed sequence can be grouped into a set A′. If the archeologist chose to test his hypothesis by experiment, he would have to convert his limited working hypothesis into a verifiable hypothesis of the form "If I do A′, I will get B." This is clearly verifiable for, in performing the operations, the archeologist either will or will not manufacture a pounder. Just as clearly, it is an experiment: the archeologist does not know if his converted hypothesis will pass the tests.

Examination of the cases of imitative experiments shows that initial limited working hypotheses are not always as complete as "roughing, shaping, smoothing, shaving, and polishing (or not polishing) produced the bell-shaped objects." An experiment will often begin with a statement of the form "notched ribs and scapulae were used to extract fibre from pulp," and the development of a sequence of use and/or manufacture will progress as the experiment progresses.

The progressive development of an experiment is limited and directed by a number of guides. If it were not for these guides, scant confidence could be placed in an inference based on an experiment, for, it could be argued, the experimenter could achieve B by one means or another. There are three guides which both limit and direct an experiment.

The first guide is evident in the choice of objective material. The objective material is here defined as the subject for study in the experiment. The objective material must be known to have been available, or could have been available, in the aboriginal setting. In Case 3, for example, the objective material is a notched rib from the site and fresh ribs. In Case 1, the objective material, wax resin, marrow fat, mutton fat, hyrax urine, plant juices, bile, honey, and tempera, were available in the site area. (For further examples of the choice of objective material with this criterion, see Nero 1957:303; Osgood 1942:33; Outwater 1957:261; Rau 1869:394; Smith 1953; Smith and Watson 1951:18.)

The second guide involves the choice of the effective material. The effective material is that material which is used to produce a change in the objective material or is changed through the use of the objective material. The effective material must be, or simulate, a means available to aboriginal people, or be in accord with a material which is known to have been or could have been available in an aboriginal setting. In Case 2, the means of producing a change in the steatite and sandstone (objective material) are tools (effective material) from closely affiliated archeological sites. In Case 4, the objective material (arrow-shaft straightener) produces a change in the effective materials (reed cane and wood). (For further examples of the choice of effective materials with this criterion, see Farnsworth and Wisely 1958:165; Griffin and Angell 1935:1; Iversen 1956:37; Kent 1957:464; McEwen 1946:111; Voce 1951:113.)

The third guide is the physical characteristics of the objective and effective materials. Unlike the first two guides, the third is not self-imposed; it is prescribed in the materials. An experimenter must, for example, work within the bounds of the hardness of steatite (Case 2) and the fibre-pulp binding in yucca leaves (Case 3).

The experimenter, forced to work within the limits of what is given in nature and what was, or could have been, available in an aboriginal setting, is also directed in his operations by those limits. In Case 5, for example, the smelting temperature (physical characteristic) of copper oxide ores (objective material) directs the experimenter to some means available to aboriginal peoples to effect smelting. The hole in the ground furnace (first effective material) cannot produce that temperature and is rejected. The model kiln (second effective material) can produce the temperature and is simulative of kilns known to have been used. (For further similar examples, see Cosner 1956; Ellis 1940; Fowler 1946; Hawley 1953; Knowles 1944.)

At any step in the progressive development of an experiment, the experimenter might attempt to work with one or more experimental materials or attempt a number of operations with the same or different materials and find that either: (1) only one operation is possible; or (2) more than one operation is possible. A set is composed of a sequence of steps; if one or more of the steps is different it defines a different set. Thus, if the experimenter could achieve his end (B) via only a single sequence he could observe: of the alternatives A', A'' · · · , $A^{(n)}$, if I do A' and only A', I will get B. If, on the other hand, he found that he could achieve B via more than one sequence he could observe: of the alternatives A', A'', · · · , $A^{(n)}$, if I do A' or A'', I will get B.

Converting the limited working hypothesis from a statement of what was believed to have happened in the past into a form that can be tested in the present establishes a verifiable hypothesis. The choice of experimental materials to operate upon and with, and the subsequent operations lead to an observation. The interpretation of this observation may be as follows:

If the experimenter observes that he can achieve B by one and only one set, he can convert his observation into an inference. The success of the experimenter in achieving B does not imply that B was achieved in the past necessarily in the same way as did the experimenter. The experimenter's success does mean that B could have been achieved in the manner indicated by the limited working hypothesis.

If the experimenter observes that he can achieve B by more than one set, he can still formulate an inference. The fact that the experimenter could achieve B in more than one way does not negate this interpretation because there is nothing necessary about the kind of order (pattern) with which the experimenter is concerned. Further, he is not attempting to discover how a people did achieve an effect, he is testing whether or not they could have achieved an effect in the manner indicated by the limited working hypothesis. The development of two equipossible sets, each of which could result in B, is parallel to searching the literature for ethnographic analogies and finding that two or more analogies can serve to transform an hypothesis into an inference.

The experimenter, finally, may find that by none of the alternative sets tried can he achieve B, and, hence, cannot formulate an inference. In this case he would re-examine his original classification, restate his limited working hypothesis, and begin the experiment once again. If he failed in successive attempts, he could formulate no inference on the basis of test by experiment.

The process of performing an imitative experiment may be summarized as follows:

1) Converting the limited working hypothesis into a verifiable form.
2) Selecting the experimental materials.

3) Operating with the objective and effective materials.
4) Observing the results of the experiment.
5) Interpreting the results of an experiment in an inference.

Confidence in an inference based upon an experiment can be increased in three ways: (1) by choosing experimental materals which were, or could have been, available in an aboriginal setting; (2) by finding corroborative evidence; and (3) by performing as many alternative sets as is feasible. Restricting experimental materials by the criterion of the aboriginal setting increases confidence by limiting the operations which the experimenter can perform to those which were possible for the aborigines. The finding of corroborative evidence suggests that the operations which the experimenter performed might have been performed in the past. Thus, the recovery of a notched rib imbedded with pulp and sap (Case 3), presumably after the experiment was completed, suggests that the experimenter and the aborigine used notched ribs in a similar way. By executing a number of alternative sets, the experimenter may eliminate some and find others which are equally possible. This process increases confidence in the same manner as a library search for suitable ethnographic analogies would: some analogies, believed to be relevant at first, are found wanting; others, previously or not previously considered, are found to be applicable. In the context of the imitative experiment, the number of alternative sets which the experimenter performed is probably the only measure of what Peirce calls "sincere doubt in the experimenter's mind of the truth of the hypothesis."

The recognition of possible relationships between objective and effective material and the awareness of the physical characteristics of those materials depends on the knowledge and capabilities of the experimenter. The execution of the operations depends upon the experimenter's skill. Knowledge, capabilities, and skill are subjective in the sense that all individuals do not possess them in equal quantities or proportions. It cannot be denied that these elements play a role in the performance of imitative experiments. On the other hand, these elements are present in any endeavor that involves the recognition of order among facts and the testing of that order. Clearly, the same subjective elements are involved in experimentation in any of the social and natural sciences (Cohen and Nagel 1934:245–72; Goode and Hatt 1952:76–80).

Criticisms of the imitative experiment have focused on the contention that the results of a particular experiment are not conclusive. When such criticisms are made, the critics generally suggest that the item or items in question could have been manufactured or used in a different way. (See, for example, Moorehead's [1936] criticism of experiments with bone points performed by Tyzzer [1936] and Tyzzer's [1937] reply.) In formulating their arguments, authors may introduce the differential nature of archeological preservation, differential geographical distributions, observations made in a protohistoric or historic setting, and parallel but different experiments. (See, for example, Over's [1937] and Ray's [1937] critiques of Cox's [1936] experiments with scrapers.) In certain cases the arguments are particularly persuasive. (See Fenenga [1953] on experiments with projectile points executed by Browne [1938, 1940] and a later experiment performed by Evans [1957].)

No belief can be established with finality and no knowledge is based upon knowing all the facts. Neither analogy, the imitative experiment, or any other tool which the archeologist now has at his disposal, can be used conclusively to establish a belief about the past. The archeologist must work with what Pareto (1935:319) has called "facts in scant numbers." The challenge of archeology is in transforming hypotheses based upon scant data into legitimate inferences. It is hoped that the results of this study will encourage archeologists to use one of the few mechanisms which can secure such transformations with confidence and clarity.

NOTE

[1] The initial stimulation for doing this kind of study came from reading Raymond H. Thompson's (1958) work on inference in archeology. I am particularly indebted to Clement Meighan, my teacher, for his encouragement, suggestions, and comments. Others who in various important ways have contributed to its present form are Marcia Ascher, Ralph Beals, Joseph Birdsell, and Makato Kowta.

REFERENCES CITED

ATKINSON, R. J. C.
1946 Field archaeology. London, Methuen and Co., Ltd.
BARNES, A.S.
1939 The difference between natural and human flaking in prehistoric flint implements. American Anthropologist 41:99–112.
BENEDICT, R.
1946 Patterns of culture. Mentor Book ed. New York, The New American Library.
BENJAMIN, C. A.
1936 The logical structure of science. Psyche Monographs, No. 9.
BLACKWOOD, B.
1950 The technology of a modern Stone Age people in New Guinea. Pitt Rivers Museum, Occasional Papers on Technology 3.
BROWNE, J.
1938 The antiquity of the bow. American Antiquity 3:358.
1940 Projectile points. American Antiquity 5:209–13.
BRONOWSKI, J. B.
1959 The common sense of science. Modern Library paperback ed. New York, Random House.
CABROL, A. and L. COUTIER
1932 Contribution à l'étude de la taille de l'obsidienne au Mexique. Bulletin de la Societé Prehistorique Française 29:579–82.
CHILDE, V. G.
1956 Piecing together the past. New York, Frederick A. Praeger.
CHURCHMAN, C.W.
1948 Theory of experimental inference. New York, The Macmillan Co.
CLARK, G. D.
1953 Archaeological theories and interpretation: Old World. *In* Anthropology today, A. L. Kroeber ed. Chicago, University of Chicago Press.
COGHLAN, H.H.
1940 Prehistoric copper and some experiments in smelting. Transactions of the Newcomen Society 20:49–65.

1943 The evolution of the axe from prehistoric to Roman times. Journal of the Royal Anthropological Institute 73:27–55.

COHEN, A. K.
1948 On the place of "themes" and kindred concepts in social theory. American Anthropologist 50:436–43.

COHEN, M. R. and E. NAGEL
1934 An introduction to logic and scientific method. New York, Harcourt, Brace and Company.

CONANT, J. B.
1950 Foreword. *In* Case 3, the early development of the concepts of temperature and heat, Harvard case histories in experimental science, D. Roller. Cambridge, Harvard University Press.

COSNER, A. J.
1951 Arrowshaft straightening with a grooved stone. American Antiquity 17:147–48.
1956 The "stone scraper" and arrow "wrench." American Antiquity 21:300–01.

COX, D. R.
1958 Planning of experiments. New York, John Wiley and Sons, Inc.

COX, I. W.
1936 The Indian spoon. American Antiquity 1:317–20.

CURWEN, E. C.
1930 Prehistoric flint sickles. Antiquity 4:179–86.
1935 Agriculture and the flint sickle in Palestine. Antiquity 9:62–66.

CUSHING, F.H.
1894 Primitive copper working; an experimental study. American Anthropologist 7:93–117.

DANIEL, G. E.
1952 A hundred years of archaeology. 2d ed. London, Gerald Duckworth and Co., Ltd.

DANTO, A. C.
1956 On explanations in history. Philosophy of Science 23:15–30.

ELLIS, H. H.
1940 Flint-working techniques of the American Indian: an experimental study. Ohio State Museum Lithic Laboratory (mimeographed).

EVANS, J.
1897 The ancient bronze implements, weapons and ornaments of Great Britain. 2d ed. rev. London, Longmans, Green and Co.

EVANS, O. F.
1957 Probable use of stone projectile points. American Antiquity 23:82–84.

EVENARI, M., L. SHANAN, N. TADMOR, and Y. AHARONI
1961 Ancient agriculture in the Negev. Science 133:979–96.

FARNSWORTH, M. and H. WISELY
1958 Fifth century intentional red glaze. American Journal of Archaeology 62:165–73.

FENENGA, F.
1953 The weights of chipped stone points: a clue to their function. Southwestern Journal of Anthropology 9:309–23.

FICHARDT, J.
1957 Prehistoric cultural material from Wellington Estate, Settlers, Springbok Flats, Transvaal. The South African Archaeological Bulletin 12:50–61

FOWLER, W. S.
1946 Primitive woodworking in the Connecticut Valley. Bulletin of the Massachusetts Archaeological Society 7:73–75.

GARDINER, P.
1952 The nature of historical explanation. London, Oxford University Press.

GOODE, W. J. and P. K. HATT
1952 Methods in social research. New York, McGraw-Hill Book Co.

GRIFFIN, J. B. and C. W. ANGELL
1935 An experimental study of the technique of Indian pottery making. Michigan Academy of Science, Arts, and Letters, Papers 20:1–6.
HARNER, M.J.
1956 Thermo-facts vs. artifacts: an experimental study of the Malpais industry. University of California Archaeological Survey, Reports 33:39–43.
HAWLEY, F. G.
1953 The manufacture of copper bells found in Southwestern sites. Southwestern Journal of Anthropology 1:99–111.
HEYERDAHL, T.
1950 The Kon-Tiki expedition. London, George Allen and Unwin Ltd.
HUSCHER, B. H. and H. A. HUSCHER
1943 The hogan builders of Colorado. Southwestern Lore 9.
IVERSEN, J.
1956 Forest clearance in the Stone Age. Scientific American 194:36–41.
JOHNSON, T.
1957 An experiment with cave-painting media. The South African Archaeological Bulletin 47:98–101.
KENT, K.P.
1957 The cultivation and weaving of cotton in the prehistoric Southwestern United States. Transactions of the American Philosophical Society 47, Part 3.
KLUCKHOHN, C.
1941 Patterning as exemplified in Navaho culture. *In* Language, culture, and personality, essays in memory of Edward Sapir, Leslie Spier, *et al.* eds. Menasha, Sapir Memorial Publication Fund.
KNOWLES, F. H. S.
1944 The manufacture of a flint arrow-head by a quartzite hammer stone. Pitt Rivers Museum, Occasional Papers on Technology 1.
KRIEGER, A. D.
1944 The typological concept. American Antiquity 9:271–88.
KROEBER, A. L.
1948 Anthropology. 2d ed. rev. New York, Harcourt, Brace and Co.
LEAKEY, L. S. B.
1953 Adam's ancestors. London, Methuen and Co. Ltd.
LINTON, R.
1923 The material culture of the Marquesas Islands. Memoirs of the Bernice P. Bishop Museum 8, No. 5.
1945 The cultural background of personality. New York, Appleton-Century-Crofts, Inc.
LUBBOCK, J.
1878 Prehistoric times. New York, D. Appleton and Co.
MCEWEN, J. M.
1946 An experiment with primitive Maori carving tools. The Journal of the Polynesian Society 55:111–16.
MCGUIRE, J. D.
1891 The stone hammer and its various uses. American Anthropologist 4:301–12.
1892 Materials, apparatus, and processes of the aboriginal lapidary. American Anthropologist 5:165–76.
1893 On the evolution of the art of working in stone. American Anthropologist 6:307–19.
1894 A study of the primitive methods of drilling. U. S. National Museum Annual Report.
MEIGHAN, C. W.
1950 Observations on the efficiency of shovel archaeology. University of California Archaeological Survey, Reports 7:15–21.

MEWHINNEY, H.
1957 A manual for Neanderthals. Austin, University of Texas Press.
MOOREHEAD, W. K.
1936 Credit to previous investigators. American Antiquity 2:132–33.
MORRIS, E. H. and R. F. BURGH
1954 Basketmaker II sites near Durango, Colorado. Carnegie Institution of Washington, Publications 604.
MOVIUS, H. L., JR.
1953 Old World prehistory: Paleolithic. *In* Anthropology today, A. L. Kroeber ed. Chicago, University of Chicago Press.
NAGEL, E.
1952 Some issues in the logic of historical analysis. Scientific Monthly 74:162–69.
NERO, R. W.
1957 A "Graver" site in Wisconsin. American Antiquity 22:300–04.
NILSSON, S.
1868 The primitive inhabitants of Scandinavia. London, Longmans, Green, and Co.
OSGOOD, C.
1942 The Ciboney culture of Cayo Redondo, Cuba. Yale University Publications in Anthropology 25.
OUTWATER, J. O., JR.
1957 The pre-Columbian stonecutting techniques of the Mexican plateau. American Antiquity 22:256–64.
OVER, W. H.
1937 The use of the thumb-scraper. American Antiquity 2:208–09.
PARETO, V.
1935 The mind and society, vol. 1. New York, Harcourt, Brace and Company.
PEIRCE, C. S.
1934 Collected papers, vol. 5, C. Hartshorne and P. Weiss eds. Cambridge, Harvard University Press.
PFEIFFER, L.
1912 Die steinzeitliche technik. Jena, Verlag von Gustav Fischer.
PHILLIPS, P. J., J. A. FORD, and J. B. GRIFFIN
1951 Archaeological survey in the lower Mississippi alluvial valley 1940–1947. Peabody Museum of American Archaeology and Ethnology, Papers 25.
POND, A.
1930 Primitive methods of working stone based on experiments by H. L. Slavlen. Logan Museum, Beloit College, 2.
QUIMBY, G. I.
1949 A Hopewell tool for decorating pottery. American Antiquity 14:344.
RADCLIFFE-BROWN, A. R.
1952 Social anthropology: past and present. Man 52:13–14.
RAU, C.
1869 Drilling in stone without metal. Annual Report of the Smithsonian Institution 1868:392–400.
RAY, C. N.
1937 Probable use of flint end-scrapers. American Antiquity 2:303–06.
ROUSE, I.
1939 Prehistory in Haiti. Yale University Publications in Anthropology 21.
SAPIR, E.
1927 The unconscious patterning of behavior in society. *In* The unconscious: a symposium, E. S. Drummer, ed. New York, Alfred A. Knopf.
1934 The emergence of the concept of personality in a study of culture. Journal of Social Psychology 5:408–15.

SCHROEDER, A. H.
1944 A prehistoric method of collecting water. American Antiquity 9:329–30.
SELLERS, G. E.
1886 Observations on stone chipping. Annual Report of the Smithsonian Institution 1885, pt. 1:871–91.
SMITH, C. S.
1953 Digging up the Plains Indian's past. University of Kansas Alumni Magazine 52:4–5.
SMITH, H. G. and W. WATSON
1951 Experiments with raw materials utilized by the Florida Indians in ceramic construction. Florida Anthropologist 4:18–26.
SPAULDING, A.C.
1954 Reply to Ford. American Antiquity 19:391–93.
SPURRELL, F. C. J.
1892 Notes on early sickles. Archaeological Journal 49:53–69.
STEENSBERG, A.
1943 Ancient harvesting implements: a study in archaeology and human geography. Nationalmuseets Skrifter Arkoelogisk-Historisk Roekke 1.
STEGGERDA, M.
1941 Maya Indians of Yucatan. Carnegie Institution of Washington, Publications 531.
TAYLOR, W. W.
1948 A study of archaeology. American Anthropological Association Memoir 69.
THOMPSON, F. H.
1954 The Roman aqueduct at Lincoln. The Archaeological Journal 111:106–28.
THOMPSON, R. H.
1958 Modern Yucatecan Maya pottery making. Society for American Archaeology Memoir 15.
TREGANZA, A. E. and L. L. VALDIVIA
1955 The manufacture of pecked and ground stone artifacts: a controlled study. University of California Archaeological Survey, Reports 32:19–29.
TYLOR, E. B.
1871 Primitive culture, vol. 1. London, John Murray.
TYZZER, E. E.
1936 The "simple bone point" of the shell-heaps of the Northeastern Algonkian area and its probable significance. American Antiquity 1:261–79.
1937 Credit to Professor Moorehead. American Antiquity 2:299–300.
VAYSON, A.
1919 Faucille préhistorique de Solférino. L'Anthropologie 29:393–422.
VOCE, E.
1951 Bronze casting in ancient moulds. Pitt Rivers Museum, Occasional Papers on Technology 4.
WEAKLAND, J. H.
1951 Method in cultural anthropology. Philosophy of Science 18:55–69.
WILLEY, G. R.
1953 Prehistoric settlement patterns in the Viru Valley, Peru. Bulletin of the Bureau of American Ethnology 155.
WISSLER, C.
1923 Man and culture. New York, Thomas T. Crowell Co.