A History of Flint-Knapping Experimentation, 1838–1976

by L. Lewis Johnson

INTRODUCTION

In this paper I attempt to discuss all flint-knapping experiments which have been published, either by the knapper or by others, from the earliest available references through 1976. Although I will mention some observations of living aborigines and some experiments in use, my focus is experimental knapping. Thus, my references are assuredly not complete on topics such as ground stone tools, tool use, and aboriginal knapping. Another area I have not completely reviewed is the study of eoliths, purported pre-Paleolithic tools. MacCurdy’s 1905 summary of the eolith controversy up to that date contains a 154-item bibliography, and the dispute continued unabated until at least 1936. In keeping with my purpose, I have concentrated on those disputants who were experienced knappers and who performed experiments intended to test the artificial nature of these objects. A chronological review of studies published will be a major portion of this presentation: what was attempted, why it was attempted, what the results of the experiments were, and how they were used by the author. The review is largely divided into decades. In each section I will briefly review works published, indicate trends or major concerns, and review in detail those studies which strike me as particularly important.

REPORTS OF EXPERIMENTAL KNAPPERS

BEFORE 1879

They began with sharpening into the figure of axes those hard flints, those thunderstones, . . . which in reality are the first monuments of human art. [Buffon 1778:381]

The first scientist to use his own knapping experience to help explain prehistory was Sven Nilsson, who stated (1868:6):

When, more than forty years ago, I first began to collect, I found here and there stones which had evidently been fashioned by the hand of man for some special purpose, and which showed distinct traces of strokes or knocks against some other equally hard, but more brittle stone. Having from my earliest youth made a practice of chipping flint-stones, and giving them any shape which I desired, I was able to recognize in these stone hammers the instruments by means of which the flint weapons had in ancient times been made.

Nilsson’s experience was gained in chipping flints for his rifle; there is no evidence that he ever tried to duplicate ancient implements. However, his application of his understanding of chipping to ancient tools marks the beginning of the scientific study of knapping and adds to his reputation as one of the founders of prehistoric archaeology (Daniel 1975).

Sir John Evans, in addition to being one of the most prominent English scientists of his generation and one of the champions of Boucher de Perthes, was the first scientist to demonstrate percussion and pressure knapping publicly, before the International Congress of Prehistoric Archaeology in Norwich, England, in 1868 (Stevens 1870:84). In his publication in support of the finds of Boucher de Perthes and similar handaxe finds in England, Evans stated that he did not accept them until he had proved, through experiment, that it was possible to duplicate them using only stone tools. His experiments also led him to recognize the basically ambiguous nature of single flakes (Evans 1860:289–90):

As also flint flakes are produced most frequently by a single blow, it is at times difficult, among a mass of flints, to distinguish those flakes formed accidentally by natural causes, from those which have been made by the hand of man; an experienced eye will indeed arrive at an approximately correct judgment, but from the causes I have mentioned, mere flakes of flint, however analogous to what we know to have been made by human art, can never be accepted as conclusive evidence of the work of man, unless found in sufficient quantities, or under such circumstances as to prove design in their formation, by their number or position.
In a further publication, Evans (1866) presented an excellent
discussion of the preparation and chipping of a Pressigny blade
core.

Evans's major publication, from the point of view of the
knapper, is his monumental 1872 work, which proposes to “give
an account of the various forms of stone implements,
weapons, and ornaments of remote antiquity discovered in
Great Britain, their probable uses and methods of manufacture,
and also, in some instances, the circumstances of their discovery” (1897:2). His practical experience in knapping informs
his discussion of British antiquities, as do his visits to the
Brandon gunflint manufactories, where he observed and talked
with the knappers, and his reading of travelers' reports on con-
temporary primitive knappers.

Evans specifically deals with experimental knapping at two
points in his treatise. In chapter 2, after describing gunflint
knapping at Brandon, Evans discusses the particular problems
of primitive knappers (p. 20): “The main difficulties consist,
first, in making the blow fall exactly in the proper place; and,
secondly, in so proportioning its intensity that it shall simply
disloge a flake, without shattering it.” He shows that this
second problem is not imaginary or due only to his own in-
experience by describing some ancient flakes with crushed
striking platforms. He offers the following as his reason for
experimenting (p. 33): “I think that if, at the present time, we
are able to produce flint tools precisely similar to the ancient
’scrapers’ by the most simple way possible, and without the
aid of any metallic appliances, there is every probability that
identically the same means were employed of old.” In his
experiments, Evans used a quartzite hammerstone and an anvil
support, and the only artifact he could not duplicate was the
projectile point. However, he was using pressure flaking against
a hard anvil, and although he considered “pressing the flint to
be operated upon on some close fitting elastic body” (p. 39)
in order to pressure-flake more efficiently, he did not try using
his left hand as that body.

Evans begins chapter 12, devoted to flake tools, with a
description of the conical fracture of flint and the use of this
property in hitting close to the edge of the block to produce
flakes. He also defines the bulb of percussion, a term whose
origin he attributes to Falconer (1868). Once again, as in 1860,
he points out the difficulties in distinguishing single artificial
flakes from natural flakes (pp. 247-48):

where the bulb is on the principal face, and analogous depressions,
or portions of them, are visible on the several other faces, and
at the same end of a flake, all of them presenting the same character,
and in a definite arrangement, it is in the highest degree probable
that such a combination of blows must be the result of design, and
the features present are almost as good a warrant for the human
origin of the flake as would be the maker's name upon it. When,
however, several of such flakes are found together, each bearing
these marks of being the result of several successive blows all con-
ducing to form a symmetrical knife-like flake, it becomes a cer-
tainty that they have been the work of intelligent beings.

The immediacy of Evans's impact on British prehistorians
can be seen by examining the publications of Augustus Henry
Lane Fox Pitt-Rivers (e.g., 1869, 1906). In reading a paper
first published in 1868, one can see that Pitt-Rivers has a
vague idea of flint chipping but no experience: “From the very
first, a peculiar mode of fabrication appears to have been
adopted, which consisted of chipping off flakes from alternate
sides of the flint, and the facets thus left upon the flint produce
the wavelike edge which you will see in the side views of all
the implements here represented” (1906:104–5). By 1875, he
has learned to knap and can use his experience to explain the
large amount of lithic debris on Neolithic workshops: “If any-
one will attempt to make a flint celt, as I have done sometimes
(and Mr. Evans, from whom I learnt that art, has done fre-
duently), he will find that it is difficult to command the frac-
ture of flint with certainty; every now and then a large piece
will come off, or a flaw will be discovered which spoils the
symmetry of the tool, and it has to be thrown away” (1906:
34). Although he does not here refer specifically to the mode
of manufacture of a handaxe, I doubt that he would still have
considered their mode of fabrication “peculiar.”

Two other reports, Wyatt (in Stevens 1870) and Skertchly
(1879), discuss the Brandon knappers. Stevens's Flint Chips
is the first book-length study of stone tools and contains second-
hand reports from explorers, such as Catlin, and contemporary
knappers, such as Nilsson and Evans. Stevens's introduction (p.
578) to the paper by Mr. James Wyatt, F.G.S., of Bedford,
states that “Mr. Wyatt is himself an accomplished flint-worker,
even with no other hammer than a conveniently shaped stone
pebble. He has, moreover, donned the apron and worked
away steadily in the Brandon sheds. His paper, therefore,
has the great advantage of being based upon a practical knowl-
dge of the subject and is not founded on mere report.”
Wyatt's clear, although brief, report on Brandon was soon
superseded by that of another knapper, Sydney B. J. Skertchly.

Skertchly's (1879) memoir is a thorough discussion of
knapping at Brandon based both on his own experimentation
and on watching Brandon knappers at work. He theorizes, from
his observations and from looking at Neolithic remains in the
neighborhood, that the Brandon knappers are on a direct con-
tinuum with Neolithic ones. He, like Evans, is concerned with
the problem of distinguishing natural from artificial flakes (pp.
43–44):

Those whose studies have not led them to pay particular attention
to flint often express doubts as to the artificial nature of some of
the ruder implements, and, still more frequently, of flakes. . . . I
am perfectly convinced that it is as easy for an experienced ob-
server to discriminate between artificial and natural flint chips, as
it is for the ordinary observer to distinguish stone arrow-heads from
rifle-bullets. This is in consequence of the very peculiar manner in
which flint and some other rocks fracture. It requires a sharp re-
bounding blow delivered with a definite amount of force to detach
a flake. If the blow be dead, the stone is only bruised; if it be
bounding, but too slight, the bruising shows an incipient cone, but
no flake is struck; if, again, the blow be too heavy, the stone is
shattered. Moreover, the blow must be delivered in a certain direc-
tion. Now it is possible that all these conditions will be found
sometimes in nature . . . but it is clearly impossible that they shall
occur in the majority, or even in any considerable percentage, of
cases.

Other reports for this period include Steensrup and Lubbock's
(1867) discussion of the Pressigny flint tools and Greenwell's
(1870) on the Grimes Graves flint quarries, in which he in-
dicates that he found bruised pebbles which could have been
used for hammerstones, “for which purpose, as I can testify
from experience, they are well adapted” (p. 429). Schumacher
(1877) discussed manufacturing methods. Cushing (1879) de-
scribed for the Anthropological Society of Washington the
knapping methods he had seen American Indians use and how
he learned to knap and then gave a demonstration of point-
making. (Note that the first American demonstration occurred
ten years after the first reported British one.)

A major reason for the lack of American works is that there
was no difficulty understanding artifactual remains found in
America: they were made by the Indians. It was interesting to
see Cushing knap, but as he could also talk about the Indians
he had seen knapping it did not provide a great deal of new
information. On the Continent, on the other hand, there were
two burning questions during this period which almost de-
manded experienced knappers to answer: How were ancient
tools made without the use of metal? How, when one is study-
ing the drift implements (i.e., Paleolithic handaxes), can the
handiwork of man be distinguished from the chance products
of nature? The majority of papers published during this early
period were concerned with one or the other, or both, of these

CURRENT ANTHROPOLOGY
Johnson: Flint-knapping experimentation

In searching the refuse of bone-caves, where flakes, knappers, and tools for working bone, etc., certainly lay with the general rubblish, it would seem that little care has hitherto been taken to collect and re-arrange them; it can only have been haste or carelessness, on the excavators' part, that lost so good an opportunity of obtaining those details which help to trace the turns of thought and ingenuity in overcoming those difficulties which enable us to distinguish the minor points marking the progress of man's mind. Though so much has been lost much may still be done, and it is to be hoped that in future greater care will be taken. [Spurrell 1883:110]

This decade is particularly sparse in papers relating to knapping; only the Spurrell report just quoted and Seller's 1886 paper are of value. Other papers of the decade include another form of hammer, and procuring a similar piece of stone, I succeeded in the way a violin bow is held. It is evident that in general the lower surface of a long hammer passing lengthways over the edge of the block would merely strike the thin edge, and that unless an irregularity in the hammer happened to hit further back, a flake, properly speaking, would not be detached. In order, therefore, that the hammer should strike the right spot at once, the projecting edge was chipped or trimmed slightly, so as to remove the projection in the line of stroke, at the same time roughing the surface and enabling the hammer to get a grip: this being done the flake was successfully detached. That this method was actually employed may be distinctly shown by the fact that many of the flakes, when placed together, show the trimming above described passing continuously across the base of both of them: thus they were chipped in a preparatory way more or less, or not at all, as occasion required.

In arranging the flakes for the restoration, most of which had fallen to the ground at once, some could not be found, and this is explained by the fact that when imitating the use of the hammer above described, occasionally a flake flew to a great distance; one flew with a fearful whirr a distance of over 60 feet; doubtless this incident occurred to the old men. Notwithstanding that these flakes on their first exhibition were at once identified by a very high authority as having been "used," it is evident that the chipping at their bases is not the result of wear, but is merely a detail of manufacture.

Seller's paper is the first American one to study modes of knapping and to observe and experiment with knapping by natural forces. He begins with information on methods which he was given by Catlin, who drew on his experiences among the Indians. One of these methods is the long T-shaped chest crutch, used with the stone to be chipped in a depression in the ground between the knapper's feet or "secured between two pieces or strips of wood like the jaws of a vise, bound together by cords or thongs of rawhide" (Sellers 1885:874). Sellers proceeds to describe quarry workshops in the Saline River region and his own experiments in replication. Like Spurrell, Sellers points out that various contours and edge treatments on chipped flint implements are not of decorative or functional significance, but are by-products of the mode of manufacture of the implements. Unable to reproduce flat flaking on hoes using stone or metal hammers, Sellers invented his own support so that pressure flaking could be used. He is properly cautious about it (p. 882): "I do not present this as a mode that was practiced, but as a device that answers the purpose, and I judge to be within the capacity of the ancient flint-workers." He also illustrates Catlin's "flaking staff," which accords remarkably well with Jury's (1949:32-34) reconstruction from archaeological data of the mode of quarrying large blocks in an Ontario quarry.

Sellers discusses the identification of natural as opposed to human flaking and reaches the same conclusion as Sketchly and Evans: natural forces are quite capable of mimicking human knapping. He looks at water-broken pieces and pieces crushed under carriage wheels and performs his own experiments (p. 891):

I filled a metal cylinder with pebbles of various sizes and shapes, brought a pressure by a screw on them through a plunger; immediately a crepitating sound was heard, which as the pressure increased became simpler and louder, at times almost explosive, as the interstices became filled with broken fragments, producing side pressure and cross fractures. The sound became more confused and died away. On emptying the cylinder, the result was many representations of the rude implements found in the drift.

Unfortunately, Sellers does not illustrate the results of this experiment. Despite a good deal of valuable information, his article does not have the impact of Spurrell's because of its lack of clarity in the central sections and its rambling style.

Neither of the reports of this decade is really concerned with the questions of the preceding period. Both Sellers and Spurrell open new ground in looking at specific quarries and trying to reconstruct the methods used to produce specific
results. They thus anticipate by close to a century much of the modern emphasis in lithic experimentation.

1890–99

If I would study any old, lost art . . ., I must make myself the artisan of it—must, by examining its products, learn both to see and to feel as much as may be the conditions under which they were produced and the needs they supplied or satisfied; then, rigidly adhering to those conditions and constrained by their resources alone, as ignorantly and anxiously strive with my own hands to reproduce, not to imitate, these things as ever strove primitive man to produce them. [Cushing 1895:310]

This decade is the most prolific in reports of the entire period prior to the 1960s, exclusively because of the writing of Americans: only two reports of the 34 published are by non-Americans. These two are Man Before Metals (Joly 1894), reporting on Evans’s experiments, and Evans’s (1890) report on his opinion that Tertiary eoliths are not artifacts. The latter article is important in foreshadowing the future furor over eoliths but contains no information of value on knapping.

A number of the American reports are not highly pertinent to the topic at hand, being descriptive of other people’s experiments or of Indian methods. Thus, Mason’s articles (Mason et al. 1891; Mason 1894a, b) are all descriptive and synthetic, and Fowke’s (1891, 1892) excellent papers are reports on Indian methods culled from original sources. Wilson’s reports (1891, 1895, 1896, 1898, 1899) are similar, although the last does refer to experiments in use made by Wilson. Wilson’s experiments refuted Seller’s assertion that beveled points were not arrowheads meant to rotate in flight. This he did by hafting the points on unfeathered shafts and dropping them from the roof of the Smithsonian Institution; they rotated.

The most prolific writer of the decade is William Henry Holmes (1890; 1891; 1892a, b, c; 1893a, b; 1894; 1897). Despite a certain amount of repetition, his reports are of great importance, both intrinsically and in their influence upon American archaeology: “The net result of all the Early Man claims and the disproofs by Holmes, Hrdlička, and others was that by 1914 there was little time depth to American prehistory. The concatenation of circumstances which led to this situation had a major impact on the development of American archaeology” (Willey and Sabloff 1973:58–59).

Holmes learned to knap in order to duplicate and thus understand the blank-making process at argillite quarries in the Washington, D.C., area. An understanding of Holmes’s theory of stone tool manufacture, derived from archaeological and experimental experience, can be gained through a review of his 1894 article, “Natural History of Flaked Stone Implements.” One major reason his theory of lithic manufacture has not had the influence on later developments it should have is probably his unilineal, overbiological evolutionary bias. This is unfortunate since, separated from this extreme evolutionism, his derivation of manufacturing trajectories and interpretations of them bear a remarkable resemblance to those presented by such recent knappers as Bradley (1975), Collins (1975a), Gunn (1975), and Johnson (1977a), all of whom could have profited from a careful reading of Holmes before beginning their own work.

Holmes distinguishes three steps in the study of flaked stone implements: (1) their identification as implements and not preforms or naturefacts; (2) “their natural history, which embodies two distinct lines of development, one of the individual from its inception in the raw material through a series of steps of technical progress, to the finished result; the other of the species or group from a primal culture germ through countless generations of implements” (1894:121); and (3) their nature as historic records. He diagrams the study as shown in figure 1.

His concern with separating implements from preforms stems from his work in the Virginia Tidewater quarries and his battles with other scholars of the time over the existence of an “American Paleolithic” (an American “eolithic,” although of concern today, was of negligible concern throughout the 19th and early 20th centuries). His major criteria for the identification of implements are “(1) degree of elaboration, (2) indications of specialization, (3) signs of use, (4) manner of occurrence, and (5) association of other articles” (1895:123); all of these must point strongly toward implemental status before an artifact may be accepted as an implement. An important point, which has had to be restressed recently, is that “it is not unusual to see in the vicinity of flaking-shops

![Fig. 1. Holmes's diagram of the study of stone tools (Holmes 1894:122).](image-url)
specimens of flakes and rejects that have been flaked upon the brittle edges by passing feet until they present most deceptive appearances of elaboration and even of specialization" (1894:124).

Once an artifact is identified as an implement, it can be studied in various ways. First, its individual history and the history of the group of implements to which it belongs must be examined. The final form of a lithic artifact depends to a certain extent on the material of which it was made, and this also, must be studied. "The history of implements, both as individuals and as species, must be studied largely through the process employed in manufacture" (1894:126). Holmes goes into the manufacturing process in some detail, as his understanding of it is central to his argument. He distinguishes direct and indirect percussion and pressure flaking and says of the relationship between percussion and pressure (1894:127):

Each may be used unaccompanied by the other, the one making large or rude forms, the other shaping forms such as flakes already too delicate for percussive treatment. When both are employed on the same specimen percussive processes take the initiative, breaking up the stone and reducing the pieces, when large, to approximate shape, and to a degree of tenacity and a relation of surface such as to make the other methods readily operative. Smaller hammerstones are used as the forms become more delicate. The pressure processes are the elaborating and finishing processes, taking up the work when the ruder processes are compelled to leave off. The change from percussion to pressure does not necessarily take place at uniform stages of the work. With tough stone the hammer must go further than with brittle stone, as the pressure tool cannot so soon be made effective.

Holmes next discusses the process of manufacture of various tools, beginning with a cobbble from which one flake has been removed and ending with a pressure-flaked notched arrowhead. He arranges all of these in a progressive diagram (fig. 2) that illustrates both the steps in, and rejects from, the manufacture of implements of increasing complexity and the resemblance of finished crude pieces to early stages in the manufacture of more elaborate ones (pp. 128-29):

It will be seen that the course of procedure in the simplest shapes is repeated more or less closely in the earlier steps of the more elaborate shapes and that the divergence of specialization takes place toward the end of the process in each case. . . . Analogies between the rejects, especially in the earlier portions of the various lines, are thus very close. The finished implement in a given case, if not definitely specialized, may not be distinguishable from rejects of corresponding elaboration belonging to the more elaborate implements above.

Holmes also sees this sequence as reflecting the evolutionary progression of lithic technology, in which one moves from the lower left to the upper right of the diagram with time. Thus, not only do the rejects from a more elaborate artifact resemble completed simpler artifacts, but also they resemble completed ancestral artifacts. Therefore one must use all possible criteria in order to identify an artifact as an implement (p. 137):

The facts to be especially brought out are these: The conditions of art in stone are such that the simpler forms of flaked implements employed in cutting, picking, scraping, and striking are necessarily shaped by like processes, pass through like changes of form, and reach closely identical results, whether made by a people of low culture grade doing their best work, or by a people of high culture doing their rudest work. The early shapes will be repeated in the later shapes, and the refuse of rejection will, in the nature of things, up to the stage where specialization begins to take effect, be largely identical.

The problem with which Holmes grapples still besets knappers and analysts, and the one detailed attempt to refute Holmes's arguments directly, Bryan (1950), suffers from its author's lack of familiarity with the lithic literature and his consequent illustration of several quite clear naturefacts as implements.

I have concentrated on the 1894 article, rather than on those in which Holmes discusses his detailed reconstruction of methods, because of its theoretical importance. It is unfortunate that Holmes made up his mind about the nature of the knapping process by the beginning of the decade (1890) and, rather than continue to investigate it, reiterated his theory until the end of the decade (1897): pages 89-61 of 1897 are a verbatim restatement of pages 12-13 of 1890.

An interesting argument of the decade concerns the priority of chipped over ground stone tools. The major knapper writing on this topic was J. D. McGuire (1891, 1892, 1893, 1896), who, through his own experience, argued that ground stone tools are easier to make than chipped stone tools and, since they imitate natural forms, are likely to have temporal priority. This judgment was also based on an evaluation of the intelligence of the artificers (1896:227-28):

There are few persons who have attempted to work with primitive tools and there are probably even fewer who have tried to produce them by primitive methods. The writer has demonstrated how some of the neolithic or polished implements were made, and has experimented sufficiently in chipping different stones to be able to say in what manner much of this chipping was done, and to demonstrate that unusual expertise and skill were a requisite in order to shape many of the so-called paleoliths. With the average neolith little time was required to complete it and but little skill was needed, the tools once in which the work was done being very primitive. With the paleolith, on the other hand, but little time would be required to complete it, but the tools necessary were complex and much skill was required, not only intelligently to chip the stone, but yet more so to select a proper stone for chipping.

In his early articles, McGuire argued only for the priority of grinding over chipping in the Americas; by his 1896 article, he was asserting that even in the Old World grinding must have had precedence over chipping. McGuire's arguments were obviously counter to the stratigraphic evidence, particularly in Europe (Read 1894), but in America the prevailing stress on the recency of human occupation led to a general acceptance of his views.

Other active knappers were Mercer (1892, 1897) and Cushing (1895, 1896, 1897; see also Youmans 1895). Mercer (1897) reports attempting to duplicate argillite artifacts and having little success until he discovered from "neighboring curbstone cutters" that argillite has bedding planes which must be followed if the stone is to chip satisfactorily. With this knowledge, he proceeded to duplicate the implements to his own satisfaction. Cushing describes an Indian knapping method which he has been able to duplicate. In this method, flakes are removed from a block by percussion on an anvil support and then are given their preliminary retouching with a padded anvil or thigh for support. The final step before pressure flaking sounds rather overelaborate, but this may be only because of the difficulty of describing knapping (pp. 317-18):

In finally forming arrow-points from these trimmed blanks, the smallest of them only were chosen. The first care in fashioning one of these was to remove protuberant points from its edge and sides and to thin it down by means of a pitching tool of buck horn. This was effected in several ways, usually by clamping it in a folded pad of buckskin under the knee against a hammer stone or notched curbstone cutters" that argillite has bedding planes which must be followed if the stone is to chip satisfactorily. With this knowledge, he proceeded to duplicate the implements to his own satisfaction. Cushing describes an Indian knapping method which he has been able to duplicate. In this method, flakes are removed from a block by percussion on an anvil support and then are given their preliminary retouching with a padded anvil or thigh for support. The final step before pressure flaking sounds rather overelaborate, but this may be only because of the difficulty of describing knapping (pp. 317-18):

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Fig. 2. Holmes's chart of the process of manufacture of various stone tools, showing the similarities between finished crude tools and pre-forms for more completely retouched tools (Holmes 1894:128).
them off by a most dextrous motion, which I can exhibit, but not adequately describe or illustrate" (p. 318).

Accounts of tool making were also published by Hayes (1890), Murdoch (1890), and Snyder (1897). In 1895, John Wesley Powell summed up the information produced by the American knappers, Holmes, Cushing, and McGuire, making seven major points (pp. 6-7): (1) Stones are battered or chipped according to the material. (2) Indian tribes adapt their methods to their material. (3) Some tribes chip, some batter, and some do both. (4) Ancient materials are found at quarries and at villages, the type of material depending upon the locale. (5) Present Indians transport particularly valuable materials long distances from the quarries. (6) So did the ancient Indians. (7) American paleoliths are quarry debris.

Powell concludes (p. 7), "In view of these facts, abundantly demonstrated far and wide over the continent, many American archaeologists and geologists have reached the conclusion that the distinction between 'paleolithic' man and 'neolithic' man, as determined by the method of making the implements, is not valid for this continent."

The continued absence of reports on experimentation in Europe during this decade would seem to be due to the fact that the main problems there were considered solved to the satisfaction of the archaeologists of the time. In America, on the other hand, the acceptance of the European Paleolithic as genuine had led to all sorts of claims about the age of American bifaces. Thus, the earliest flowering of American experimentation was aimed at proving that American bifaces were not ancient, however much they resembled European handaxes. The refutation was accomplished primarily through detailed study of lithic quarries, duplication of the forms found there, and the discovery of these forms on the campsites of recent Indians. Although Holmes, in particular, went overboard in his rejection of age for the American Indians, not recognizing that to prove that some bifaces were recent did not prove that all were, he did produce valuable work on technology, particularly on the nature of the lithic reduction process.

1900–1909

Another point which, I think, is of some importance is that "eoliths" would have made such very poor tools or weapons for any conceivable purpose. Mr. Harrison seems to admit this, and suggests that they were "body-scrapers," used mainly for removing the hard skin from the feet of human beings. I find it very difficult to believe that any man with sufficient intelligence to shape flint by means of chipping or crushing should have gone on so long producing tools of such a low type and, one would imagine, so much inferior in every way to natural objects of bone, horn, or wood, which must have been easily procurable. [George Clinch, in Warren 1905:362]

Although prior articles had identified and described eoliths (MacCurdy 1905), S. Hazledine Warren's 1905 article is the first in which the arguments are supported by experiment. Warren is refuting the evidence of Rutot (e.g., 1902, 1904) and Sir Joseph Prestwich on eoliths found in Miocene and Pliocene deposits in England and on the Continent. His article is a good presentation, based on his knapping experience, his experiments in counterfeiting natural chipping, and his observations of natural forces which produce flakes. It is not totally convincing, even to a supporter, because he does not have enough evidence on naturally produced artifacts. Lack of clear-cut evidence of naturally produced eoliths allowed the controversy to rage on for another 30 years.

Other European publications in this decade are Bardon and Bouyssonie's 1905 discussion of percussion-chipped implements, in which they do not discuss their experimental knapping but make it clear that they do knock, and W. J. Knowles's 1903 discussion of his own work, that of a forger, and that of his son in the fabrication of Irish flint arrowheads and spearheads. Munro (1905) also discusses forgeries.

In America, the literature of this decade is primarily redundant, Holmes (1900) on the quarries again and Wilson (1902) continuing to throw beveled arrowheads off the Smithsonian as well as performing other experiments with them. Jenks (1900) discusses another breed of knappers, those who produce forgeries for profit, and indicates why they have had little impact on the study of primitive manufacturing techniques: at least in America, they tend to use metal knapping tools and often start with broken Indian artifacts rather than from scratch. The only valuable American papers of the decade are MacCurdy's two: one (1905), already mentioned, reviewing and evaluating the eolith question and the other (1900) an attempt "to describe the distinguishing features of obsidian fracture, to seek an explanation for the same, and to show that to them is due, in a measure at least, the excellence of obsidian as a material for knife and razor making" (p. 417). This is the first description by an archaeologist of the process of fracture, and as such is quite accurate.

It is clear that the impetus for experimentation in lithic technology has moved back across the Atlantic to Europe. In America it has been proved to the general satisfaction that all stone tools are recent and that a Paleolithic period does not exist. In Europe, on the other hand, the discovery of chipped flints in pre-Pleistocene deposits has reopened the search for criteria by which to recognize human handiwork and thereby created a renewed need for experimentation.

1910–19

If we knew nothing of the matter, and were shown a perfect crystal of a diamond and a rough piece of broken brick, I think we should imagine that there was more evidence of human design in the crystal with its perfect regularity of form and its polished facets, than there was in the brick. Where problems of human implements are concerned let us take warning lest we mistake the diamond for the brick. [Warren 1914:413]

Just over half of the articles published in this decade were written by British authors, and most of these were concerned with the eolith controversy. Because of his facility as a knapper and his experiments in natural fracture, Moir (1912, 1913, 1914, 1919) was the most persuasive advocate of the artifactual nature of the eoliths; for the same reasons, Warren (1913, 1914) was his major adversary. Most of the other people publishing (Engerrand 1912, 1913; Haward 1912, 1913; Abbott 1914; Lankester 1912; Schwartz 1914) suggested that a more thorough study of flint and of natural and artificial fracturing would be advisable before coming to hard-and-fast conclusions.

The four 1912 publications contain various points of view. Haward, although not as determined an advocate as Warren, points out two factors which tell against the eoliths: (1) if angular flint is fixed in any matrix, any other stone can roll over and chip it, and (2) those who wish eoliths to be artifacts pick a small number of stones out of a large, randomly assorted mass. Engerrand (see also 1913 and Graham 1962), a prior advocate of eoliths, presents a powerful critique of them based on his observation of their manufacture by Baja California rivers. Lankester points out that a regrettable lack of knowledge of flint fracture makes any conclusions doubtful (p. 331):

In order to interpret correctly the significance of fractured flints . . . very definite and accurate knowledge of flint, only to be arrived at by careful quantitative investigation, such as the skilled physicist and chemist can bring to bear, is necessary. Yet the entire
scientific world is in a remarkable state of ignorance with regard to flint.

He proceeds to outline an excellent program of investigation by which to understand flint—much of which still has not been carried out.

Moir begins his article (p. 173) with a thinly disguised dig at Warren (1905): “I would point out that the conclusions put forward in this paper are the result of practical experiments. Theoretical papers dealing with the manner in which flints might, under abnormal circumstances, get flaked by natural means, appear to me to be a complete waste of time to write and an annoyance to read.” Unfortunately, Moir’s experiments do not seem to come very close to duplicating the conditions found in nature: his percussion experiment consisted in putting a bunch of flints in a bag and shaking the bag; his pressure experiments included pressing flint chips vertically with or without sand on top and pressing two flint chips together in a press. His percussion experiment produced erratic chipping angles not found in human-chipped flints or in eoliths, and the pressure flakes bore no resemblance to human flaking in the first case and were bipolar in the second. From these experiments, Moir concludes that nature could not have produced the eoliths in his collection.

Warren (1913) argues that Moir’s experiments are both meaningless and ludicrous, a conclusion with which I am forced to agree. Haward (1913:347), on the other hand, tries to pour oil on troubled waters: The fullest credit must be given to our members, Mr. Reid Moir, F.B.S., and Mr. S. Hazeldine Warren, F.G.S. (and others also) for their efforts to solve this problem by experiments. This is obviously the right line to adopt, but, unfortunately, experiments (including my own, for I have been experimenting for years on similar lines) are only proof that (given suitable conditions) the sharp edges of split flint will chip under “Force,” whether this is applied by Nature or Man. Experiments can prove the difference between the results of the forces of “Percussion” and “Pressure”: but it is doubtful, in my opinion, if it can be proved by experiments whether Nature or Man has applied the force, as the same forces are used by both Nature and Man, the only difference being the intensity with which the force has been used and the way the force has been applied.

Moir’s 1913 and 1914 articles basically are descriptive of collections. In 1913 he avers that the eoliths from Kent are man-made, in 1914 that, since the early Chelles implements show large crude flaking and his pre-Chelles rostro-carinates show even larger cruder flaking, they are, therefore, also implements. (A rostro-carinate is “a chipped flint, shaped like the inverted end of a Canadian canoe, flat underneath with a keel above” [Burkitt 1937:635].)

The other three 1914 publications are a plea for a detailed study of flaking before coming to grandiose conclusions (Abbott), an outline for such a detailed study (Schwartz), and a report on “an experimental investigation of flint fracture and its application to problems of human implements” (Warren). Schwartz’s outline is exhaustive, and, as far as I know, the research suggested has never been carried out in its entirety. He divides the field of study into “agency of nature” and “agency of man.” Within the former there are five subdivisions: (1) the examination of flints in which there is no possibility of human flaking, (2) fortuitous percussion, (3) directed percussion, (4) fortuitous pressure, (5) directed pressure. The latter is divided into (6) unpolished neoliths and (7) paleoliths. Under (1), there are two subdivisions, flaked nodules and flakes, and the characteristics Schwartz (1914:452) thinks should be investigated with reference to the first will give an idea of the detail of his outline:

A. FLAKED NODULES. (1) weight; (2) frequency of occurrence; (3) number and dimensions of flakes (a) truncated, (b) untruncated; (4) ratio of area of flaked to unflaked surface; (5) “flaking diagrams” showing the disposition, sequence, and direction of the blows necessary to remove the flakes; (6) characteristics of secondary chipping and flaking diagram of same; (7) angle between bulb or conchoïd and striking platform or cortex; (8) relative frequency of occurrence, number, and dimensions of flakes (a) on natural protuberances of the nodule, (b) on normal surface of nodule; (9) ditto, ditto, on the ends and sides of cylindrical or elongated nodules; (10) disposition and size of areas showing battering, abrasion, or incipient cones of percussion relative to (a) natural protuberances, (b) normal surface of nodule, (c) centre of gravity of nodule; (11) relative frequency of occurrence of Eolithic or implemental forms and characteristics of same; (12) relative frequency of occurrence of thermal fission and characteristics of same; (13) prismatic or starch fracture; (14) classification of natural forms of flints based on shape; (15) constituents of the deposit and their mineral condition; (16) scratches on flints and their characteristics; (17) staining, patination, and other physical or chemical changes in flint.

Warren presents a detailed report on his experiments in replicating natural fracture which corrects half of the problems found in his 1905 report. Before presenting his results, he discusses problems with experimental reproduction, among them quantity of material and length of time, and general characteristics of chipping. In the latter section he is particularly concerned with planes in resistance to chipping found in flint nodules. He defines four angles, alpha, beta, gamma, and delta (fig. 3), from which a blow can come, ranked (in the order given) according to the ease with which a flake can be removed. Thus, 1.1 watts of energy remove flakes easily from the alpha direction and less easily from the beta direction. To remove flakes from the gamma direction, 4.65 watts are required, and even when the impact is raised to 29.4 watts in the delta direction no flakes are removed from the dorsal surface, although more flakes come off from the alpha direction (p. 417). These results lead Warren to promulgate a law (p. 418): “Under certain well-defined conditions governed by the form and quality of the flint operated upon, we find: That forces acting from a wide range of angle upon either side of a flint tend to produce chipping in one direction only, rather than in the various directions corresponding to the incidence of the blow.”

In looking at various natural means of flaking, Warren passes rapidly over thermal and prismatic fracture, since they are easily distinguished from human fracture. He pauses briefly on “the breaking of flint by tension or flexion at some point other than at which the force is applied” (p. 419), although it too is distinguishable, since it provides platforms with alpha and beta angles for the operation of fortuitous percussion and pressure.

Warren first tests percussion, performing two types of experiments: throwing hammerstones at the stone to be chipped and bowling the stone to be chipped along a beach. The former

![FIG. 3. Idealized plano-convex stone showing Warren's knapping angles](redrawn from Warren 1914:417).
It is true that palaeolithic implements are selected, as an associated group by themselves, from the river gravels in which they are found, and it may be argued that there is equal justification for selecting the eoliths. This leads us to the difficult question of deciding the difference between that which may safely be determined as different in kind, and that which is different only in degree, from the (admittedly) naturally broken stones among which it is found.

Unfortunately, Warren either did not know or did not think of the work of Frere and Pengelly in the sites of Hoxne and Brixham, where Palaeolithic artifacts had been found in undisputed deposits, not selected from water-laid gravels.

The methods used by Warren to pressure flake are (p. 425):

1. By pressing or stamping with the heel of one's boot.
2. By the sudden shock of falling iron weights.
3. By the similar shock of pieces of timber falling under the action of gravity.
4. As 3, but pressed down by muscular effort.
5. By pressure slowly applied in a screw press.
6. By the crushing action of cart wheels.
7. By the drag of a sled loaded under various weights; this gives a reversal of the relative positions of the rounded stone and the edge of the flint to be chipped.

The results of these seven methods are virtually indistinguishable, and they produce a group of forms which "repeat themselves indefinitely in the results obtained, and . . . are indistinguishable from the eolithic forms" (p. 427). With plano-convex or tabular flint, the most common form is the notch, which grades into a straight or slightly convex edge or into a double reversed notch.

In an appendix, Warren gives "technical details and tabulation of experimental results" with great detail and clarity. He concludes that, much as he would like to see revealed the remains of Tertiary man, the eoliths can be most economically explained as the results of natural processes.

The final word on eoliths for this decade is Moir's *Pre-Palaeolithic Man* (1919). Here Moir brings together all of his experimental work and observation in a grand synthesis. His experiments in reproducing natural fracturing still do not seem sufficient, and he has taken no notice of Warren's experiments. Unlike Warren, he seems to have no real understanding of the magnitude of natural geologic forces. He describes the objects with which he is concerned and details their mode of manufacture as replicated by him. This exposition proves nothing, since the fact that man can duplicate natural forms speaks for man's intelligence, not for his ancestors' having made them originally. Moir also discusses the evolution from his rostocarinate to Paleolithic handaxes and Mousterian prepared flakes. Finally, Moir discusses, as support for the artifactual nature of eoliths, their presence in association with Pittdown man.

Outside of the eolith controversy, the most important work of the decade was not directly archaeological. De Freminne's (1914) long and detailed study of the nature of flaking in natural materials was aimed at engineers studying stresses on man-made materials such as metals but is extremely valuable for archaeologists studying lithic fracture—particularly as De Freminne's major test material was glass. Another seemingly important study is Pfeiffer's 1912 essay *Die Steinzeitliche Technik*; unfortunately, my command of German is too weak for me to comment on its contents.

This decade also produced the first reported knapping by a Russian scholar (Gorodzow 1914). Gorodzow worked for a month and twelve days, after which he came to quite definite conclusions (pp. 229–31)—some overly definite, considering the paucity of his experience, others clearly repetitive of previous experiments: (1) Some varieties of flint work better than others, and some do not work at all. (2) Chipping flint requires an elongated percussor with a straight or slightly curved edge, not a spherical hammerstone (not necessarily so). (3) A great many naturally broken pieces of flint resemble artifacts, Palaeolithic and Neolithic cores, handaxes, and scrapers (true, but not new). (4) It is easy merely to retouch these natural flakes and produce functional tools (true). (5) The ease of doing so will give rise to the ambition to chip cobbles, but this requires a knowledge of the various forms of flint and knapping tools, which is difficult to acquire.

1 "Donc, isolément, un éolith ne peut avoir aucune valeur documentaire. Ce ne sont que les conditions annexes de gisement, d'objets industrielle indisputables, autres concomitants, etc., qui peuvent permettre de la classer, soit comme produit du travail de la nature ou au contraire comme résultat du travail d'un homme ou d'un hominien. Il s'agit là d'une constatation facile à faire si l'on dispose d'une grande série de ces pièces. Elle est, on peut m'en croire, très pénible à faire, mais . . . magis amica veritas. En effet, le fameux critère théorique de la taille intentionnelle n'existe pas."
(if then assumption weak). (6) One may conclude that this technique of chipping the edges of natural flakes was invented in earliest times and retained for a long time before the creation of cores was attempted. Following this stage it was possible to use larger pieces which, with a few blows, could be turned into Chellean or Acheulian handaxes (eolith argument). (7) Only after much practice was it possible to hit with enough accuracy that the flake removed had a predetermined shape (the Mousterian technique), and at this point it was easy to advance to the Solutrean level. It is unfortunate that Gorodzow had not read the works of such experimenters as Capitan (1917) and Bardon and Bouyssonie (1905) so that he could have taken their results as a basis for his own experiments. As it was, he progressed considerably, but his work repeated the labor of others and his original conclusions were unsupportable.

Apart from Bourlon's (1911) work on burins, the remaining reports are by Americans. Holmes (1919) basically says nothing new, although he does introduce some new observations on native knappers, particularly on Ishi. Other reports on Ishi's knapping are those of Pope (1913), Weston (1913), and "Robin Hood II" (1917); the latter two, however, are more concerned with Ishi's bow-and-arrow technique than with his knapping. Pope's description of Ishi's pressure technique sounds remarkably like Crabtree's method and suggests direct historical influence. Further American work is by Harvey (1913) and Smith (1911).

An amusing report is that by Eames (1915; see also Nagle 1914) on the chipping of flint by the use of fire and water. In his technique, he heats the flint and then drops water on it from the end of a feather. Eames states (p. 70), "While making my own experiments I frequently become much discouraged with the results obtained. Persistence won at last, however, and I succeeded in forming a very fair specimen of arrow-tip flaking." Eames gives no details on the method, nor does he illustrate the point. After trying this method and falling, Ellis makes the best possible comment on a similar report of aboriginal flaking (1940:55):

If Mr. Fraser's artist took several hours to fashion a projectile point using the methods detailed, one might better blame the assiminity on the Indian rather than Fraser's reporting. Anyone who would spend so much time making a single arrow-point when there are any number of methods which might have been used to better, or at least as good, advantage would be the exception to any rule. Thus it might be said that perhaps there were a few isolated peoples who did make their implements by heating stones and dropping water on them, but whoever or wherever they were, the use of such a technique certainly stamps them as lacking in ingenuity and ordinary "horse sense."

Ellis, unfortunately, does not examine the reports of Putnam (1883), in which genuine heat treatment by native knappers is described, and Coutier (1929), in which it is experimentally shown to be valuable.

The eolith-related reports of this decade provide a clear case of argument and counterargument, with the balance of evidence falling on the side of those who do not accept eoliths as artifacts. This is so particularly because Warren, as the major antieolith spokesman, has come closer to the series of tests of knapping recommended by nonpartisan observers than has Moir, although the experiments of neither are conclusive. The other reports of the decade are either repetitious of previous work or foreshadow future emphases.

1920–29

It would seem probable that prehistoric man, from the earliest times, utilized wood for making into implements and weapons, but it is only under exceptional geological conditions that any examples of this material would be preserved till the present day. The experiments I have carried out have demonstrated to me that very many of the ancient stone implements, referable to every phase of the Stone Age, which have been found, are admirably adapted for the shaping of wood and similar material, and I feel that these practically indestructible flint implements represent, as it were, merely the "insoluble residue" of prehistoric industries, of which the other and more friable artifacts have disintegrated and disappeared during the great periods of time that have elapsed since they were made. If this surmise is correct, then the flint implements of ancient man give us, perhaps, but a very small and imperfect picture of his state of civilization. [Moir 1926:656]

This decade produced few studies of importance to the history of lithic experimentation. In America brief reports appeared on the knapping techniques of the modern knappers Halvor Skavlem (Stewart 1923) and Joseph Barbieri (Harris 1926), and Harrington (1928) described how he astonished an old prospector by chipping a flint point for him. Pope's classic work on the bow and arrow appeared in 1923, but it is concerned more with use than with manufacture. Holmes (1928), in his last publication, outlined his reasons for rejecting the presence of Paleolithic man in America.

In England, the eolith debate continued, but no reports of great importance were published. Moir's 1926 work, quoted above, is a valuable description of woodworking and the manufacture of tools to do so, while his 1928 works are popular renditions of Moir (1919). Warren's only publication (1924) denies the status of implements to a particular set of eoliths. Haward's (1921) paper, a response to criticism of an earlier one, contains a valuable discussion of the factors influencing the fracture of flint and its reporting (pp. 448-49):

(a) The temperament of the Observer. The extent of his experience, and his views (the latter often pre-conceived).
(b) Physical conditions, such as temperature, moisture, etc.
(c) The hardness of both Hammer and struck Flint, and the weight of each relative to one another.
(d) The foot-pounds of energy put into the blow and the rate of impact. Whether the blow was struck by a heavy hammer moving slowly, or a light one moving fast.
(e) The exact spot struck relative to the mass, and the angle at which the blow was struck.
(f) The nature of the support on which the struck flint rests, whether resilient or not. Etc., etc.

In France, Schleicher (1927) published a report on French gunflint makers, from which one may note the difference between French and British knappers (cf. Skertchly 1879, Spurrell 1883). The most important report is that of Coutier (1929), who spent several months experimenting in the replication of Paleolithic implements in order to clarify details of their manufacture. In his experiments, he knapped sandstone and flint using stone and acacia wood for percussion and stone and bone for fine percussion and pressure. He concludes that for a Chellean axe there is no prior preparation; rather, the tool is knapped by banging it on an anvil, starting at the point and working towards the base. Acheulian axes are made similarly but with more control, and they evidence secondary retouch by hand-held percussion. In the Upper Acheulian, wood is used as a percussor, which is clear from the longer, narrower, and more feathered flakes produced. In the Mousterian, flakes are removed on an anvil from a previously prepared core, but chipping with wood as the knapping tool is also done to produce feathered flakes and blades.

In the Upper Paleolithic, cores are carefully prepared, and the wooden baton is used to take off long, straight percussion blades. Solutrean retouch is accomplished in two stages. The preliminary retouch is done by resting the preform vertically on one edge and chipping the other edge with the wood, giving directly perpendicular blows. Secondary pressure retouch is
accomplished as follows (and Coutier here adds a clear use for a "bâton de commandement") (p. 174, translation mine): 2

In my left hand I hold a piece of flattened wood perforated by an oval hole in the same way as those antler artifacts which place the "bâtons de commandement," which are found in the Upper Paleolithic. I place the piece to be retouched on top of the wood so that the edge to be retouched is over the hole, with the face to be retouched down. In my right hand I have a piece of bone (a toothbrush handle, on this occasion) and with this I press sharply and rapidly along the edge of the flint, successively pressing on neighboring points.

When he wants to pressure-flake more carefully, he mounts his point in a shaft which he can press with his shoulder while his hand corrects the aim. He also has discovered that flint works better if its quarry water is removed by slight heating, which he accomplishes by placing it in the corner of a slow oven (salamandre).

Coutier's work is another addition to the literature dealing with attempts to replicate the work of particular periods. Unfortunately, this cannot be called a "trend of research" because of the lack of continuity and seeming lack of communication among the scholars/knappers making these studies.

1930–39

It is necessary that the artisan know just the amount and quality of force that is required in working the different materials, and for successful accomplishment he must concentrate to the limit. In his hands, the material, the tools, and he himself become one, isolated from all surroundings and noises. In working he continually watches what he has done, studies for position for the next blow or pressure and has in his mind the finished product of his labor. (Barbieri 1937:101)

A careful study of the edges and points of these so-called tools (eoliths) will convince anyone who has used flint tools that an extremely small percentage could ever have cut or penetrated any solid substance more resistant than soft butter. (Pond 1930:132)

The two main topics of this decade were detailed studies of manufacture by experienced knappers and what should have been the termination of the eolith debate. There were also a number of brief reports on related topics, one of which (Blackman 1932) is bizarre and another (Heese 1933) unreadable. Blackman observed an artisan producing the pebbling on a window of a bank by placing glue on the glass and allowing it to dry, upon which potlids of glass scaled off with the glue. He therefore concluded (p. 39): Having heard the story about dropping water on the flint to chip it, the thought occurred that this water might have contained glue. The aborigine had plenty of glue. This Murphy specimen has every appearance of the glass in the bank, so I selected a number of flint silex, successivement sur des points 'voisins.'

The technique of chipping flint and similar materials is simply the pressure method of shaping an implement and may produce many typical conchoidal flakes, but at best there are always many splintery scars to tell the initiated that a hammer-stone and not a bone pressure tool was used \[1930:63\]. By "splintery scars" Pond seems to mean hinge or step fractures, and he makes no mention of bone or wood percussors.

In discussing Skavlem's work, Pond in several instances gives Skavlem's explanation of a process and then follows it with one based on mechanics. Among other topics, he discusses the reasons for the production of feathered, stepped, hinged, and run-off flakes. He continues by discussing how Skavlem has made various artifacts, including pecked and ground tools. In approaching his conclusion, he points out the importance of good stone to handsome artifacts (p. 101):

The most extensive study of manufacture is Pond's (1930) study of the knapping techniques of Halvor Skavlem. (A number of other reports on Skavlem's work appeared in the same year, among them Wisconsin Archaeologist 1930, El Palacio 1930, and Davis 1930a; also see Museum Service Bulletin 1935.) Pond states: "It is the purpose of this paper to explain the technique of Mr. Skavlem in such detail that those following the description can also make stone implements with the tools available to early man" (p. 14). Pond's account is excellent, and his general discussion of fracture mechanics is quite good, but he runs into some problems in generalizing from Skavlem's work to all knapping. Since Skavlem almost exclusively pressure-flaked waste-flake material obtained from Indian quarries and workshops, Pond tends to underestimate the degree of control possible in percussion flaking: "In the hands of a skilled workman the percussion technique may approach the pressure method of shaping an implement and may produce many typical conchoidal flakes, but at best there are always many splintery scars to tell the initiated that a hammer-stone and not a bone pressure tool was used" (1930:63). By "splintery scars" Pond seems to mean hinge or step fractures, and he makes no mention of bone or wood percussors.

Joseph A. Barbieri (Harris 1926, Davis 1930b). In a report requested by the Campbells on the manufacturing techniques of the implements found during their work at Lake Mohave (Barbieri 1937), he studies the artifacts, describes his replication of them, and then discusses the requisites for their manufacture. He does not make the mistake of assuming that he has indeed duplicated the aboriginal method (p. 100):

This flake was struck off with a round hammerstone weighing about one pound, the lump being held on the upper leg, protected by two folds of buckskin padding in addition to my clothing. The portion to be struck off must be placed downward and slightly supported by pressure against the leg to achieve a long flake of uniform thickness. The striking off blow is delivered at an angle of about 45 degrees, the point of impact being as far back from the edge of the lump as the desired thickness of flake to be produced.

Johnson: Flint-Knapping Experimentation

Industries in South Africa. It is difficult, however, to understand what he is trying to say. Apparently not a knapper, he describes Pfeiffer's (1912) method of retouching a "c.d.p." (presumably coup de poing, or handaxe) as follows (p. 39):

The tool to be retouched is held in the hand, with the undersurface touching the anvil or support (stone or bone) at the part that is to be retouched, at an angle such as is desired for the bevel. A tap with the finger or hammer (wood, bone, soft stone) above the point of contact in the edge—and away comes a flakelet on the obverse of the kind as desired, and at the spot desired, the worker being able to watch and perhaps to progress all the while, the effect of each step being exposed to his view.
The face against which the blow is directed must be free of protuberances or projecting edges which could misdirect the force of the blow, and it must be sufficiently flat to give the point of the hammerstone a firm "seat" at the point of impact. For this reason, striking platforms are sometimes necessary.

We cannot say whether the Lake Mohave tool-makers struck off their flakes in just this manner, but an examination of their handiwork leads me to believe that they did. In any event, they certainly knew from experience that a slightly yielding surface is almost a requisite for producing suitable flakes. Too soft a pad spoils the result, as does a too hard support, such as stone.

Barbieri discusses the raw materials and the modes of manufacture of the various implements found. He includes detailed technical information, as in the following discussion of blade making (p. 102):

With the hammerstone held in the right hand, a sharp blow is struck on the striking platform, half an inch to an inch above the base of the platform seat. At the same time the blow is entering straight into the material, the left hand gives the nodule a sharp turn to the right. A successful blow has removed a workable flake.

Twisting the nodule as it is struck, gives a more nearly flat flake. It modifies and lessens the bulb of percussion and also the S or Z curve at the lower end of the flake.

He describes how the artisan then proceeds to straighten the blade and prepare the edges for further retouch. Following percussion retouch, a few of the implements were pressure retouched, and Barbieri also outlines this process. His descriptions are all clear and accurate, except where he realizes they cannot be: in pressure flaking, "both the angle of the tool relative to the specimen, and the specimen relative to its support, enter into the problem of achieving good results, but this can hardly be put into writing" (p. 105).

Knowles and Barnes (1937; Barnes 1937) report having tried to make a gunflint following Sketchley's description of the process and, finding this impossible, visiting Brandon to observe the process for themselves. This allowed them not only to correct Sketchley's account, but also to discover how Tardenoisian burins (microburins) were made—by percussion on an anvil. Coutier and Cabrol (1932) are also concerned with discovering how an archaeological artifact was made, in this case a Mexican obsidian blade. They quote the reports of Torquemada, who stated that pressure flaking was used, and of Hernández, who averred percussion, but find that neither works. Experiments lead them to discover, however, that indirect percussion reproduces the blades excellently. Cabrol and Coutier (1931) also experiment with the use of wooden batons as percussors, discover that there are differences between blades taken off using wood and using stone, and conclude that our ancestors definitely used wooden batons.

Barnes was particularly active during this decade. In addition to his work with Knowles, he published a paper discussing his well-designed experiments in the use of stone tools (1932); he and Cheynier (1935) reported on measurements which should be taken on stone tools, and he and Kidder (1936), in a paper primarily devoted to a description of the technology employed at La Ferrassie, introduced these measurements to the English-speaking audience. Further reports are those of Harrington (1934), Howell (1939), and Over (1937).

The most important paper in the whole eolith literature is Wen Chung Pei's 1936 report. Pei worked with Breuil at Choukoutien (see Breuil 1931) and published a report on the quartz implements found with Pithecanthropus. It was being denied that they were artifacts, and Pei set out to discover a means of distinguishing natural from human flaking to prove that they were. His superb analysis should have eliminated any necessity to argue the relative merits of eoliths. Pei begins by pointing out that, whatever their origin, mechanical fractures of stone are fundamentally identical, and laboratory re-creations of natural chipping methods are insufficient because of such factors as time, the simultaneous or sequential action of multiple forces, and an incomplete understanding of the action of such forces as glaciation (p. 352). He goes on to consider all natural and human means of flaking.

He first discusses temperature, noting that thermal fractures are easily recognized but can confuse an inexperienced person and can present edges for subsequent pressure. He then turns to mechanical actions, which much more effectively counterfeit knapping. He first looks at those in the beds where the flint is formed, noting that tectonic motion of the beds often produces pseudo-blades, cores, and flakes and that this motion can be combined with other forces which will further modify the stones. He next examines mechanical actions in deposits such as flinty clays, flinty sands in crags, and alluvial deposits. Regarding pressure within the soil, Pei notes the steepness of angle of naturally pressure-flaked flints as opposed to the acute angle found in most humanly flaked flints. He then discusses a number of deposits in which the characteristics of naturally formed flake can be observed. As a second force within soils, Pei mentions the action of large roots in chipping stone. He gives examples of all these methods from natural deposits.

The next topic Pei discusses is that of deposits in motion: glaciers, solifluxion, running water, the sea (both beach deposits and underwater deposits), and the wind. Most interesting in this section is his noting that while stones chipped on beaches bear little resemblance to those made by man, those chipped underwater are much more similar because of the absorption of vibrations by the water (p. 390). The final natural actions Pei discusses are those which occur on the surface of the ground, caused by the passage of vehicles, people, or animals. Here he points out in particular the need to beware of pieces chipped by Pleistocene megafauuna.

In his third chapter, Pei discusses knapping, both experimental and ancient. He reports primarily on the experiments of Breuil and Coutier which he has observed and then compares their results with those of ancient man to come to conclusions about the modes used in prehistory. This is an excellent résumé of methods and of the characteristics of flakes produced by each mode—direct percussion with or without anvil, indirect percussion, and pressure.

The other articles of this decade are brief, although some are important. Steward (1938:337) reports that Great Basin Indians placed stones to be heat-treated under the ashes of the fire for five nights. Barnes (1939:111) gives a statistical rule for the determination of human versus natural flaking: "The flaked tools of an industry made, or supposed to be made, by percussion and measured in accordance with the directions set out in the Appendix may be considered to be of human origin if not more than 25 percent of the angles platform-scar are obtuse (90° and over)." This formula, of course, assumes that all chipped rocks from a particular deposit are collected, not just those that the collector thinks look like artifacts. Farmer (1939) points out a new source of confusion in determining human flaking: lightning can produce flakes with characteristics quite similar to human flaking. Finally, La-caille's (1939) discussion of Scottish artifacts, knapped on quartzite, is valuable in introducing the analysis of nonflint implements.

The '30s are important in producing a number of detailed studies of lithic technology. These studies are more ambitious than the majority of earlier studies in that particular industries are being studied in detail. Efficient knappers interested in using their talents for the solution of archaeological problems are found in France, England, and America. Strangely and unfortunately, except for Barnes, who publishes in journals of all three nations, the knappers in one country seem to have little contact with those in another.
The object of my study was to determine how chipped artifacts might have been made by primitive men, the time element involved, the mistakes and errors that led to broken and unfinished products, and, in general, to gain an impression of the art of the arrow maker. [Bixby 1945:345]

My objects in making these investigations have been to find out as much as possible about the way in which Palaeoliths were made, and, about the conditions under which the work was done. [Baden-Powell 1949:38]

Some of the publications of this decade, like those just quoted, entirely overlook the carefully built-up body of information on knapping to begin all over again from scratch: Plus ça change, plus c’est la même chose. Two works, however (Ellis 1940, Knowles 1944), are superb, and several others are notable.

Tindale and Noone (1941), in analyzing “an Australian aborigine’s hoard of knapped flint,” make use of both their own experimentation and careful observation of practicing aboriginal knappers. They show a good understanding of the factors important in flake removal (p. 121):

Our experience in experimental knapping, as also that gained by one of us of stone work done during his sojourns amongst the aboriginal tribes still using stone, has led us to believe that, provided the impact point on a nucleus of good material offers sufficient obstruction (a correctly delivered blow suitably placed being assumed), little else but a favourable range of angularity was required of the platform. The shape of the knapping tool at the spot where it comes into contact with the nucleus is apparently a more intimate influence on the nature of the fragment detached.

The careful selecting of the exact portion of the hammer that is to come into contact with the nucleus immediately before the blow is struck is a noticeable characteristic of present-day aboriginal knapping. There is also a freedom from working restrictions which is also revealed by the analyses given above in regard to bulbs and platforms, and this exposes the minor part actually played by the platform. The major factor in knapping technique is shown to be the contour and ridging of the face of the nucleus from which the desired fragment is detached—toogether with the position of the point of impact in regard to same, these being the main controlling factors of block, flake, or blade form.

Jury (1949) is primarily concerned with the description of lithic quarries and workshops which he discovered and excavated on the shores of Lake Ontario. After observing the position of two granite boulders and a pine stump, he arrives at a mode of original flint block reduction which not only works, but, as I have said, is listed by Catlin as a method used by the Indians. Jury also discusses the further process of knapping, and although his illustrations are useless, their captions indicate that he is versed in knapping.

Goodman (1944) attempts to apply materials analysis to the study of stone tool materials, examining the density, hardness, toughness, and resiliency of samples of flint, obsidian, chert, tuff, fossil wood, limestone, and quartzite. She defines her terms carefully as well as her reason for attempting the study (p. 416): “A study of technology is not complete without some knowledge of the properties of the material utilized, and also, if possible, an inventory and similar knowledge of those which were not utilized.” The results of her analysis of 35 samples are presented in tables and accompanied by discussion. She wanted to test the practical meaning of the differences she found by having an experienced knapper work the materials, but, unfortunately, her knapper was drafted. This study represents an attempt to move from theoretical descriptions of pure minerals to empirical descriptions of the stones actually used in the manufacture of artifacts. (Goodman includes an excellent photograph of a just-separated flint flake, obtained by using the sound of the blow to trip the camera.)

The three major articles in this decade will be discussed in chronological order. The first is H. Holmes Ellis’s “Flint-Working Techniques of the American Indians: An Experimental Study.” This should be required reading for anyone who wishes to understand flint knapping. Ellis’s aim was to review all that had been written of knapping, both experimental and aboriginal, and then to replicate all methods which had been used. In this he succeeds admirably. The only important references he overlooks are Coulter, Pei, and Spurrell, and the only method he does not discuss is glue—and that he knew about, as he references Blackman. Ellis’s procedure is to review the literature on each method of percussion and pressure and then discuss the attempts at the Lithic Laboratory of the Ohio State Museum to duplicate them. In each case the efficacy of the method is evaluated relative to that of other methods for achieving the same results. One method with which Ellis had no luck was heat treatment, but given the inaccuracy of most reports of the use of heat this is not surprising. The only attested aboriginal method not tested at the laboratory was the use of teeth (p. 52):

Whether or not this comes under the heading of rest or free-hand pressure is a mooted question with the odds favoring rest. If the bits of glass were thin enough, it is understandable that the edges might be shaped by biting. However, with thin edges but a thicker body, the pincer technique may have been employed, i.e., holding the piece of stone in the teeth as in a clamp, and breaking off the edges by a downward “impulsive twist.”

In view of the fact that no one on the staff of the Laboratory had the necessary dental constitution for such work, this is the only chipping means which was not attempted experimentally.

Ellis’s essay is beautifully complemented by Knowles’s The Manufacture of a Flint Arrow-head by Quartzite Hammer-stone, a detailed discussion of the manufacture of one artifact from core to final basal notching. Knowles’s description is excellent and cannot really be condensed. By following it carefully, I am sure that it would be possible for a neophyte to produce a passable point. A few examples of the clarity of Knowles’s prose will have to suffice (pp. 12–13):

Position of worker when flaking the “quarter” or core. Seated on a chair of ordinary height; body bent forwards; knees well apart; left elbow and forearm supported along the left thigh and knee; left hand holding the core clear of the knee and in front (i.e., in front of knee or body—according to ease of striking the core with the hammer-stone), tilting the platform of the core towards the hammer to the requisite slope; right elbow supported by right thigh (a stuffed leather pad can with advantage be used as a cushion between right elbow and thigh); right hand holding the quartzite hammer-stone (the hand holds the quartzite pebble about the middle and the forearm acts as the hammer handle, so that the end of the hammer-stone is brought down on the core in the same way that a blow would be struck by a hafted iron hammer-head).

When flaking the core three main difficulties are encountered:

1. The inclination to strike a blow which at its impact is pulled towards the striker, instead of going on firmly down with the flake. This sort of blow results in the flake breaking out part way down, leaving a lump on the core for the rest of the distance which is very hard to get rid of. One or two of such failures soon spoil a core. Perhaps this blow is produced by the fear of striking in too far on to the platform of the core.

2. A correct blow has a peculiar feel and sound. The striker feels as though the hammer-stone were resting for the fraction of a second actually on the core, and the weight of it going on down along the line of cleavage: the sound is a peculiar “splitting” noise that is difficult to describe but unmistakable to the experienced flaker.

Not only is Knowles’s prose clear, but so are his illustrations.

The next study of interest is Bordes’s (1947) attempt to do for European lithic technology what Ellis had done for American (though without having read Ellis). An unavoidable lack
of research, which Bordes admits in his preface, makes his article much less valid and valuable than it would be otherwise. Despite being repetitive of earlier studies published both in French and English, Bordes’s discussion of the various methods of percussion flaking is excellent and well illustrated. His method of pressure flaking would, it seems, tend to leave a prehistoric tribe blind, since the chips removed would fly into the knapper’s face: “The blade being held with the point turned towards the palm, a pressure is applied on the edge, from bottom to top, from right to left, and from the point of the blade towards its thick part ... (here, it is the upper face of the blade which is retouched, and not the bottom face as in percussion)” (1947:21, translation mine). Bordes concludes with a description of the methods used in various European industries.

1950–59

Because of the stuffiness and complacency of so many contemporary antiquarians, because of their laziness and lack of scientific method, the unfortunate Flint Jack was regarded as a dishonest forger. His unique ability as a craftsman, and his rare qualities as an artist, were appreciated by none but a handful of scientific men, and even they looked upon him more as a curiosity than as a positive asset to scientific research. Apparently oblivious to the importance of the study of technology, they made no accurate record of his techniques of flaking—which might have prevented some of the fantastic accounts of flaking techniques which have since appeared in archaeological text books; they neither noted such a simple but important fact as the proportion of waste flakes manufactured in the production of a single tool, nor did they concern themselves with the psychological aspect. [Blacking 1953:209]

Blacking’s appreciation of Flint Jack is one of two “ethnographic” reports of this decade. The second is Blackwood’s (1950) presentation of New Guinea lithic technology. Although not experimental, this report provides grist for the experimental mill (the “knapping” styles described are very similar to those elicited by White [1969] in the 1960s). Other peripheral reports include Ouwterwater’s (1957) description of making an Aztec war club with glass teeth in order to test dental, the possible from the impossible. This burin spall which has been made by removing a spall from the truncation: the spall must be concavity “to place rosin in” for a “better grip” while using it, a nuts-and-bolts approach to knapping, an amusing and informal guide which is generally accurate when he wishes it to be. Knowles’s introduction to the collection of stone tools at the Pitt-Rivers Museum is an excellent follow-up to Knowles (1944), giving precise directions on the manufacture of other bifacial tools and aimed at answering the questions “how far advanced in techniques was the stone-worker who struck off the flake, what tools did he use, and what was his range of methods?” (p. 71).

The only pertinent eolith paper of the decade is Clark’s (1958) recognition, based on experiment, that the full range of Kafuan “tools” can be made by natural causes. A related paper is Harner’s (1956) presentation of an experiment designed to test whether heating cobbles in an oven would replicate the artifacts of the Malpais industry. Some, but not all, were replicated, and Harner suggests further experimentation and careful examination of the purported occurrences by trained geologists.

De Visscher (1955) reports on the use of a lever to make large pressure flakes. Unfortunately, neither the text nor the illustrations make it clear exactly how his lever works. Bordes (1955), on the other hand, clearly describes the Levallois technique, indicating that free-hand percussion can be used to remove the Levallois flake from the core at least as efficiently as swinging the core against a stationary anvil.

A number of general introductions appeared during the 1950s. Watson (1950), in a guide to the implements in the British Museum of Natural History, gives a general introduction to knapping. The only change in this introduction in the third edition is a note that “it is now generally recognized that [striking off a true Levallois point] can be done only by raising the tortoise core in the hand and bringing it down on an anvil” (Watson and Sieveking 1968:55). Witthoft’s articles (1956–57) are essentially typological, although there is some mention of experiment in the first installment. Although he gives no references, so one cannot be sure, it appears that Witthoft is unaware of the results of much of the earlier experimentation (e.g., Ellis 1940, Knowles 1944) and bases his technological discussion on his own limited experience. Evolutionary in intent are Lacaille’s (1953) discussion of the evolution of the knife, which is informed by experiment and Bordaz’s (1959) discussion of technique and its advances, which is based on experimental work by Barnes, Knowles, Bordes, Coutier, and Leroi-Gourhan.

Four other general treatments are by experienced knappers: Leakey (1953, 1954), Mewhinney (1957; see also 1952, 1953, 1955, 1956), and Knowles (1953). Leakey’s discussions of Stone Age techniques are based on his 21 years of experimentation. His outline of method is marred only by the dogmatic assertion that methods which he has found to work in replicating particular artifacts were the methods used for the originals. Mewhinney’s A Manual for Neanderthals is a nuts-and-bolts approach to knapping, an amusing and informal guide which is generally accurate when he wishes it to be. Knowles’s introduction to the collection of stone tools at the Pitt-Rivers Museum is an excellent follow-up to Knowles (1944), giving precise directions on the manufacture of other bifacial tools and aimed at answering the questions “how far advanced in techniques was the stone-worker who struck off the flake, what tools did he use, and what was his range of methods?” (p. 71). The two other articles of the decade discuss demonstrations: a South African demonstration by Leakey (1950) is briefly reported, and Lacaille (1954) reports in detail on the methods of Coutier as revealed both in person and in a movie made of his work. It is unclear from these reports which of these two individuals, Coutier or Leakey, was the first experimental knapper filmed in action: Coutier was filmed in June, 1947 (Lacaille 1954:19); Leakey had “recently” made a film in 1949 (Leakey 1950:74). The earliest film of knappers may be Haward’s 1930s film of the Brandon knappers (M. Newcomer, personal communication).

The 1950s are mainly a period of stabilization. General textbooks on prehistory appear which contain clear discussions of knapping technology and set out what is known about tool making to date. The decade might be looked at as the lull before the storm; knapping was generally understood, and more specific studies, which have exploded in the past 15 years, had not yet reached print. Presagers of the future are Bordes (1955) on the Levallois technique and Neill (1952) on the manufacture of fluted points, which will be discussed below with the multitude of other treatments of this topic.

1960–76

But how can one best understand these simple chipped stones? First by eliminating as much as possible the fundamental causes of error, the most important of which is the misunderstanding of flint knapping, of the laws which it imposes, of its impossibilities, of its unforeseeable accidents. This indeed is the point of departure for all Typology: to know how to tell the intentional from the accidental, the possible from the impossible. This burin spall which took the base of the core with it was not made in order to have a concavity “to place rosin in” for a “better grip” while using it, but is the result of an accidental misplacement of the burin blow. This burin on an obtusely retouched truncation could not have been made by removing a spall from the truncation: the spall must

“La lame étant tenue la pointe tournée vers soi, une pression est appliquée sur le bord, de bas en haut, de droite à gauche, et de la pointe de la lame vers sa partie mousse; ... (ici, c’est la face de la lame, tournée vers le haut, qui est retouchée, et non plus celle tournée vers le bas, comme par percussion).”
Up to 1960, knapping experimentation had remained outside of the main line of archaeological investigation, even that concerned with stone tools. Since then, however, the number of knapping archaeologists and their published output have increased greatly (see, e.g., Hester and Heizer 1973), resulting in much-improved communication among knappers and between knappers and the rest of the archaeological community. I have so far examined 144 publications; for the 15 years since 1960 I have located 177, and I am sure that my coverage is less complete as I near the present. Not only has the number of publications increased substantially, but, to a great extent, so has their quality. This explosive growth of interest and competence in lithic technology would seem to be due to a fortunate concatenation of circumstances. General factors include a growth in the numbers of practicing archaeologists, which almost assures a greater number interested in stone tools, and a general increase in interest in technology among modern academics, fostered by the "conquest of space." A particular factor of great importance was the "discovery" of Don Crabtree by Earl Swanson of Idaho State University. Although Crabtree had been experimenting since the age of seven and had worked with Kroeber and Gifford at Berkeley and at The Ohio State Museum Lithic Laboratory (Biography 1970), he had never published, partially because of a lack of interest on the part of archaeologists and partially because of Crabtree's own humility. Swanson arranged the Les Eyzies Conference, the experimental program with Crabtree at the Idaho State University Museum, and the National Science Foundation Flintworking School, as well as becoming a close friend and general publicizer of Crabtree.

The Les Eyzies Conference (Jelinek 1965, Smith 1966) brought together the self-trained knappers François Bordes, Don Crabtree, and Jacques Tixier to knap and exchange notes in the presence of 14 essentially nonknapping archaeologists from America and France whose specialty was stone tools. In addition to demonstrating various techniques, Bordes, Crabtree, and Tixier examined collections brought by the other participants for the technological information they contained. Not only did this conference initiate a continuing and fruitful friendship and exchange of ideas between Bordes and Crabtree, but also it forcefully introduced a number of prominent archaeologists from all over America to the potentialities of technological analysis in the explication of the archaeological record.

The experimental program set up at Idaho State with National Science Foundation support was designed "(1) to place Don Crabtree's knowledge in print for examination by professionals and other interested persons; and (2) to study with a high-speed movie camera the character of Crabtree's flint working" (Swanson 1966). This program resulted in a large number of publications by Crabtree and the production of four general films on Crabtree's knapping in addition to the specialized footage. I would venture that it is also partially responsible for the emergence into print of a number of other competent "amateur" knappers, who realized that the "professional" community might finally be ready to appreciate their hard-won knowledge of lithic technology.

The initiation of the National Science Foundation Flintworking Workshop in the summer of 1969 allowed graduate students from all over the United States and beyond to spend a full month's apprenticeship under the direct tutelage of Don Crabtree and his assistants. The full value of these schools, which have run annually since 1969, in the development of American archaeology cannot yet be assessed, but, adding these individuals to those trained by Bordes, Tixier, Callahan, Solberger, and others, and to those that they have in turn trained, it would seem that the pool of technologically aware archaeologists has probably attained "critical mass" and that the "forgetting" of previously learned technological information, which has been noted too often in this review, will no longer occur (see Swanson 1975).

Additional assurance that technological studies of lithic implements based on experiment will remain alive is provided by the inception in 1972 of a new publication, the original concept of which is as "a vehicle for the dissemination of information on current projects dealing with all varieties of lithic artifacts. Thus, scholars may avoid some of the duplication of effort currently being carried out, and people with similar interests and proposals may combine efforts" (Knudson and Muto 1972:1). Initially the brain-child of Ruthann Knudson, the Newsletter of Lithic Technology was edited during its first two years by Knudson and Guy R. Muto. The reins then passed to Muto, soon joined by Susanna R. Katz. With the last issue of 1975, the newsletter moved from the Laboratory of Anthropology, Washington State University, to the Center for Archaeological Research at the University of Texas at San Antonio, where editor Susanna Katz is now assisted by Paul R. Katz and Joel D. Gunn. It has served its stated purpose well, and with the publication of volume 6 has become known as Lithic Technology.

Given all this ferment, what have been the results? Totally new lines of investigation, such as idiosyncratic variation, have opened, and traditional investigations have been pursued in greater depth. To begin with the latter, five ethnographic knapping traditions have been reported: an almost moribund New Guinea technique of bipolar flaking was reported by White (1968, 1969), and a similar technique was found being used by African herdsmen to produce knives, each of which was used once to geld sheep and then ritually disposed of (Kosambi 1967). Bordaz (1969) reported on Turkish threshing blades made by direct percussion, and Gallagher (1972), while studying an obsidian quarry workshop in Ethiopia, was surprised to note neighboring tribesmen stopping on their way through to make blades and shave. Finally, Runnels (1975, 1976), after noticing apparent use wear on the edges of glass bottle sherds picked up in Greece, discovered that local shepherds used the pieces of glass in woodworking. Runnels tested his interpretations by making bottle-sherd tools and using them—the manufacture being accomplished by breaking bottles.

Eoliths have received little attention. Three papers have dissected specific "industries": Mason (1965), while not experimenting himself, refers to earlier experimental work in laying to rest the African eoliths; Painter (1967) cleverly and convincingly reargues the quarry-debris vs. early-tools controversy with reference to the "Lively Complex"; while Jelinek, Bradley, and Huckell (1971) report that Bradley has produced "chapeaux gendarmes" flakes from a single blow on a core and that, therefore, their presence cannot be taken as clear evidence for the human manufacture of a flake. A paper

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intermediate between studies of eoliths and studies of use is Wesley's (1968): he concludes that edge damage on microflakes must be caused by use rather than natural causes after testing for natural damage by tumbling clean-edged flakes with water, sand, and gravel for five hours and holding the edges in a match flame or a butane torch (cf. Sellers 1885).

Like Wesley's report, many discussions of use experiments lack information on the mode of manufacture of the tools used. Keller (1966), Leroi-Gourhan and Brézillon (1966), Ahler (1971), Michels (1971), Goodyear (1973), and Hester, Gilbow, and Albee (1973) have none, while Crabtree and Davis (1965), Gould, Koster, and Sontz (1971), Goodyear (1974), Newcomer (1974), and Brose (1975) have very little. In an increasing number of such reports, however, the authors specify both how they made their tools and out of what materials (Epstein 1963, Elliott and Anderson 1974, Tringham et al. 1974, Patterson 1975, McManamon 1976), while Béggerly (1976) indicates the nature and size of the raw material, the mode of production, and the size, weight, and hardness of the hammerstone used. The latter study is a model of the careful reporting of variables. Keeley's (1974a, b) and Sheets's (1973) studies are especially critiques of microwear analysis which point out the importance of studying the pieces to be used immediately after manufacture, since striations, attritions, and small chips found after use may well be results of manufacture. Finally, Knudson (n.d.) presents a cautionary parable on "bovifacts."

General coverages of knapping range from poor to excellent. The bottom of the order is held by Hodges (1964, 1970). In addition to providing limited coverage of man's oldest and longest-lived technology, Hodges makes such statements as that Holmes (1919) "is the only general account of stone working before steel tools" (1964:232) and that "looking only at the handaxes, it is tempting to credit early man with very little intelligence and, to take an extreme view, assume that the making of implements became an acquired character much as in the making of nests for birds" (1970:23). Minimal coverages include Ross's (1970) brief, informal, and somewhat inaccurate report on the physical properties of flint and how to knap and Coles's (1973) discussion in Archaeology by Experiment.

Healy (1966) is a good primer on knapping by a responsible, self-trained amateur; Howell (1967) is general and not wholly accurate but does have a brief illustrated discussion of Bordes's knapping; and Pfeiffer (1969) has a good general review of the experiments of Crabtree, Bordes, and other recent knappers. Of Oakley (1972), in its sixth edition since 1949, one may say that revisions of the knapping section do not seem to have kept pace with the development of the field.

Sankalia (1964) is designed to introduce Indian students to lithic technology. In chapter 4, devoted to techniques of manufacture, Sankalia indicates that he and his colleagues have knapped but refers for his detailed descriptions to the standard references: Barnes, Bordes, Leakey. This is a good general description with an Indian focus. Crabtree's 1967 (a, b) papers are discussions of knappable stones and knapping tools from the point of view of a knapper. Reading a Crabtree article is much like having a pleasant conversation with Crabtree: the presentation tends to ramble, but a great deal of information is transmitted.

Bordes (1968) offers a general discussion of lithic technology to introduce his presentation of Paleolithic industries and comments on technological matters throughout. He notes that Coutier's assertion that Abbevillian handaxes were made by anvil percussion must be rejected, since he can easily replicate these handaxes using a hand-held hammer. He also comments:

We have numerous Levallois cores that have been cast aside before the detachment of the flake because of some unforeseeable defect in the flint which made this detachment impossible. On some of them, an attempt has been made all the same; for optimists have always existed! [Bordes 1968:137]

At about 2.50 metres—dated at 38,000 B.C.—near a human skull a single flake was found which reminded Paterson very strongly of the Middle Soan flakes in India. It is perhaps rash to venture to draw conclusions from a single flake! [Bordes 1968:212]

Crabtree and Gould (1970) report on Crabtree's 1970 demonstrations at the American Museum of Natural History and on the exhibit that accompanied them. They also argue for more and more careful experimental archaeology. The exhibit, photographs of which accompany the article, is excellent and should be examined by anyone contemplating mounting a similar exhibit or wanting an overview on lithic technology. Crabtree's (1972) guide is useful for those who understand knapping already and who need their memories jogged on his extremely important insights. Unfortunately, the text is too elliptical to be of value to neophytes, while the glossary is of uneven quality. Rather than being titled "An Introduction to Flintworking," the volume might better be titled "Notes of a Flintworker—Directed to his Colleagues," and as such it is invaluable.

Phagan (1976) is an extremely important work, not for the results of his analysis, which are somewhat weak, but for the attempt to indicate the technological meaning of each attribute used in his flake analysis system. Whether or not these interpretations are totally correct, the attempt to define the significance of attributes, rather than using as many as possible in the hope that they will have significance (e.g., Leach 1969, Lewis 1973), is crucial to the further development of technological analysis.

While not experimental, Rosenfeld (1965) is a good discussion of raw materials, presenting information important to experimentation. Hayashi (1968:129), on the other hand, explicitly refuses to experiment:

In carrying out such a techno-typological analysis, the author does not employ the so-called "experimental" approach. This is because the experimental approach can indicate at best only some way(s) in which artifacts could have been manufactured. Nevertheless, it does not necessarily indicate all other possible ways of manufacturing the same specimen. Without proper control of mechanical, petrological, as well as ethnographic knowledge, the so-called "experimental" approach may become a mere empirical exercise. Together with these theoretical difficulties, there are too many variables involved in the experimental approach for it to be employed for the present purpose. The same technique can produce different results in terms of flake scars and hence the appearance of the artifact, depending on the nature of the raw material or the flaking tool employed.

Krantz (1961) and Bordes (1971a) are both concerned with evolution. Krantz postulates that Abbevillian handaxes are crude because the thumb of the hominid who made them was too short for an effective precision grip and that only clumsy handaxes could be made using a power grip. He confirms his postulation through experiment. Bordes (1971a) is a fascinating philosophical exegesis of the relationship between human mental evolution and the evolution of technology.

For the 1975 meeting of the Society for American Archaeology, R. Knudson and A. Benfer organized a symposium entitled "New World Lithic Analysis: Regional Evaluations." From the point of view of the student of experimental lithic technology, the presentations range from excellent (Fenwick and Collins 1975, Knudson 1975) to useless (Dincauze 1976, Benfer 1975), with a number intermediate (Sheets 1977, Fagan 1975). The uselessness of those mentioned is not the fault of the authors; Dincauze, for example, reports (p. 33):

Replication experiments have barely begun in the Northeast, particularly in regard to the crystalline rocks of the Appalachian provinces. At the University of Maine, Orono, an ambitious program...
has been initiated with microcrystalline volcanics, the first such serious attempt, in order to refine criteria for explaining cultural change during the Late Archaic period (R. Bonnichsen, personal communication).

Results from Bonnichsen’s experiments should be valuable, as he apprenticed for a year to Crabtree and already has had experience in running a Lithic Technology Laboratory at the University of Alberta (Bonnichsen 1968a).

Mitchell (1971) is a heartening paper. After watching a knapping demonstration by Mark Newcomer, Mitchell revised his approach to the archaeological industry he was studying: “I do not wish to overstate the point, but it may well be that it has been all too easy for excavators (including myself) to claim as deliberately struck implements what may have been to the Mesolithic knapper no more than debitage produced in large quantities” (p. 280). Also related to demonstrations is Callahan’s (1975b) report on an informal lithic workshop which took place at J. B. Sollberger’s residence in Dallas. Particularly intriguing for archaeologists was the following (p. 4):

It was apparent from observing the knapping procedures that three distinct flintworking “traditions” were evident. We might label them the “Idaho,” “Texas,” and “Virginia” traditions. Those who studied under Don Crabtree in Idaho, J. B. Sollberger in Texas, and Errett Callahan in Virginia, reflected, to a greater or lesser degree, their teacher’s idiosyncrasies. As all three teachers were “self-taught” . . . differences were apparent in holding positions, padding devices and knapping technique. It was interesting to see that knappers in the “Idaho” tradition tended to swing their billets from the elbow with comparatively little movement of the wrist or shoulder while the “Dallas” tradition knappers tended to raise their arms and swing from the shoulder. . . . Knappers in the “Virginia” tradition, however, swung from the wrist with little comparative movement of the elbow or shoulder.

Callahan’s 1976 papers are related to his Living Archaeology projects. In 1976a he reports that it took about 281 pounds of flint and 28 hours to turn out student knappers with a 45% success rate at thinning bifaces. This is important practical information for those setting up a lithic technology training session, and it suggests that, aboriginally, most apprentice knapping probably took place at the quarries. Callahan’s (1976b) Pamunkey Project is the most ambitious attempt to date to set up a complete functioning replica of an aboriginal American campsite. His argument in doing so is that replicas of aboriginal artifacts made and used in a laboratory situation cannot inform nearly as well about the archaeological record as those made and used during the course of a subsistence project, in which situation their correct and efficient use spells the difference between subsisting and merely surviving. Although beset by a number of unanticipated and unavoidable difficulties, Callahan’s groups in the first two years, 1974 and 1975, produced information of value in understanding the archaeological record, and it would seem that further attempts in this direction might lead to results of major significance.

The cross-fertilization of engineering and archaeological approaches to fracture is indicated by Tsirk’s (n.d.) presentation at a joint meeting of the engineering and anthropology sections of the New York Academy of Sciences in the winter of 1973–74. Tsirk briefly introduced the terms and problems of the two
fields to one another and impressed all by his knapping abilities. Another indication of the growing connections between these two fields is the publication in the *International Journal of Fracture Mechanics* of a paper entitled “The Fracture Mechanics of Flintknapping and Allied Processes” (Fonseca, Eshelby, and Atkinson 1971). This is an elegant mathematical analysis of pressure and percussion flaking which determines that efficiency of removal falls with velocity in pressure flaking and rises with velocity in percussion flaking (p. 431).

With the great increase in number of archaeologist/knap-pers, the number of technological studies of archaeological industries has also grown considerably. In many of these, the authors do not mention particular experiments, but use their experimental experience in the interpretation of the archaeological specimens. Among these are Bordes (1961a), Dakaris, Higgs, and Hey (1964), Fitting, de Visscher, and Wahla (1966), Laughlin and Aigner (1966), Sharrock (1966), Clark (1967), Hammatt (1970), Katz (1971), Shafer (1971), Bradley (1972), Hester (1972a), Hester and Hill (1972), Knudson (1972, 1973), Johnson (1972, 1975, 1976, 1977a), Feustel (1973), Lewis (1975), Clark and Kleindienst (1974), Green (1975), Lavine-Lischka (1975), and Gunn (1977). The Clark and Kleindienst work is particularly valuable to lithic technologists for the brief discussion (p. 87) of the difficulty of working the tough stones found at the Kalamo Falls site in contrast with the chaledonic sedices (Rosenfeld 1965:123) and obsidians used by most experimenters. Their terminology appears to be an Anglicized version of Tixier’s (1963, 1967), with appropriate variations, which indicates a healthy terminological consistency among Africans.

Tixier’s doctoral thesis (1963) is a model of clear exposition of terms and of definitions based upon long, carefully evaluated knapping experience. Fortunately for those who do not read French, sections of it have been translated by Mark Newcomer and published in *Lithic Technology* (Tixier 1974). The passage quoted to begin this section reveals the clarity of Tixier’s thought. Tixier (1967) is another model analysis, although I am afraid that his suggestion that *piquant-triédre* be translated as “trihedral prickle” (p. 806) will not succeed. Would he accept “three-sided tip” as an alternative?

Excellent analyses of whole industries include Bordes’s (1961b) short discussion of Mousterian cultural traditions and development, included in which is a brief but clear outline of the Levallois technique and the following parenthetical comment: “When one undertakes to make a side scraper on a thick flake, the odds are good that he will make a Quina-like tool without trying to” (p. 805). Ranere (1975) is an important report on Panamanian lithic artifacts combining “making and using stone tools, [and] microscopic analysis of technological and functional attributes on both archaeological and replicated specimens” (p. 208) to understand the industry. Katz and Katz (1975) is a superb example of how, using technological information, a great deal of data may be squeezed from unpromising sites—surface sites containing flakes and little other archaeological material.

Sollberger (1968, 1969) and Bonnichsen (1968b) exemplify two quite different ways of proceeding in technological analysis. Sollberger, a collector turned amateur archaeologist (1968:95), approaches lithic replication from an almost aboriginal point of view. He thinks of things that need to be done in the maintenance of a hunting-and-gathering community, such as cutting branches with straight ends for arrows, hoe handles, etc., disjoining animals (1968), or making and notching arrow shafts (1969), and then looks at the archaeological lithic implements from his area and experiments with his own replicas thereof to discover which stone tools were used for which purposes. He arrives at novel and workable conclusions slightly faulted by the common habit of con-
Finally, Phagan and Vierra (1975) is a cautionary paper showing that summary percentages of artifact classes may not reflect even major differences in prehistoric cultural systems.

Manufacturing trajectories and their output have attracted much attention during this period. These include studies of artifact classes in general, of particular artifacts, and of specific techniques. Also included are notes of caution, such as Sollberger's (1970) plea for the recognition of rejects and preforms for what they are and their separation from finished points and Sheets's (1975a) note concerning the extremely rapid buildup of debris in an area where preforming is being carried out, resulting in the swamping of any other class of cultural remains. In this article, Sheets reassesses the date of use of the El Chayal obsidian quarry in Guatemala, moving it from the Preceramic to the Post-Classic.

The most frequently replicated artifact is the projectile point, and, aside from those on fluted points which will be discussed below, the experimental reports are of uniformly high quality. Bordes's (1961a) report on Levallois points has already been mentioned: he concludes that there are three ways to replicate a Levallois point, two involving hand-held percussion and the third being Coutier's anvil technique. Crabtree (1970) uses a wooden pressure tool to duplicate Palli Aike and Kimberley points and notes the similarity of the resultant scars to those on the archaeological specimens.

In concluding a general discussion of pressure flaking, he stresses (p. 148) that it is important to understand that the flint knapper is not merely copying the finished artifact. He is necessarily concerned with the sequence of steps that can be deduced from the succession of flake scars on the prehistoric tool or that can be reconstructed by examination of the flakes found at the archeological site where the object was recovered. The result of experimental work is usually a reduction of the number of ways in which the prehistoric object can be replicated. Most often, two or three solutions remain as suitable explanations of the techniques used by prehistoric man, and a number of other methods have been discarded entirely.

Callahan (1975e) exhaustively analyzes the reduction sequence followed in the manufacture of Volgu Laurel-Leaf Point 2 (creating an excellent recording system to aid in doing so) and then replicates the point, discovering that results most similar to the original can be achieved using soft-hammer percussion followed by minor pressure. He then analyzes other Solutrean points, finding a consistency in the reduction sequence which leads him to hypothesize an overall technical unity among them, with variations being due to idiosyncratic factors. Support for this hypothesis is provided by the analysis of a selection of Solutrean "look-alikes" whose various reduction strategies are recognizably distinct. He concludes, therefore, that the careful analysis of reduction strategies can reveal important cultural information.

Other artifacts replicated are Cahokia microliths (Morse [1974], total data) and beveled knives of the Central Plains (Sollberger 1971). Sollberger experiments with sharpening the edges of these knives by bifacial renewal (only three complete renewals possible per tool) and by beveling (ten complete renewals) and concludes that "the extra material waste required to resharpen bifacially will guarantee a minimum of 5 extra trips per knife to a quarry site. Thus, if it were 50 miles to a quarry, the bevelling of one knife would save a minimum of 500 miles of walking." This is another useful Sollberger study, progressing from archaeological observation through replication, use, resharpening, and explanation. Jelinek (1966) gets into the thorny area of distinguishing marks of manufacture from those of use, questioning whether "Blackwater end scraper retouch flakes" have battered dorsal platform surfaces because of prior use as scrapers or because of difficulties attendant upon their removal.

Crabtree and Swanson (1968) join Crabtree's prior experimentation with Swanson's prior archaeological observations to elucidate the nature of a problematic artifact. It had been observed that edge-ground cobbles were found on many North-western sites, with blades also being present on some, and Crabtree had percussion-flaked blades using the edges of cobbles. As the cobbles produced by Crabtree resembled some of the archaeological cobbles, it was concluded that those cobbles might have been used in percussion flaking. Further experimentation was suggested to resolve the uses of the residual cobbles and determine whether other uses might also replicate the cobbles used for percussion flaking.

Related to the understanding of specific techniques is Balout's (1967) clarification of the French-language distinction between tailier, débiter, and retoucher (p. 704): On taille un galet, un rognon, une plaquette de silex pour les transfor-mer en objets: un galet est taillé en "chopping tool," un rognon est taillé en biface. On débite un nucléus en éclats ou en lames. La langue française établit une nette distinction entre tailier et débiter un arbre. La retouche accommode la pierre taillée (par exemple la retouche secondaire, ou retaillle des bifaces), elle spécialise les pro-

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That is, *tailler* is to remove flakes to make a core tool, *détérier* is to remove flakes for use from a core, and *retoucher* is to retouch a biface after roughing it out or to retouch a flake. Mewhinney (1963) gives brief production notes on 2-inch-long pressure flakes, multiple *éraillures*, and exaggerated hinge fractures. In 1964, he challenges archaeologist/technologists to distinguish flakes he has made using a hammerstone, a steel ball-peen hammer, and a wooden billet. This challenge is accepted by Muto, who successfully classifies the majority of the flakes (1971b:Appendix 2). Brief reports on single techniques are Lenoir’s (1975a) elegant article pointing out that fractures à languette (Bordes 1970) are the result of misapplied pressure and Pond’s (1969) invention of a wooden anvil to hold a point while point pressure flaking. Pond’s invention is a simple solution to the problem of adding mass to a small piece in order to be able to exert sufficient pressure to flake it and is functionally identical to Phagan’s (n.d.b) subsequently but independently invented device (see below).

The bipolar technique of flake making—holding a core on an anvil and bashing it with a hammerstone—has received a fair amount of attention in this period. McPherron (1967) found bipolar cores at the Juntunen site and briefly describes his attempts to replicate them, and Honea (1965a) has effectively duplicated Texas pebble cores and flakes using the bipolar method. Kobayashi (1975) details his experimental work in using the bipolar technique; unfortunately, he does not compare his experimental results with any archaeological collection. Patterson and Sollberger (1976) aver that the bipolar technique is technologically unsound, the knapper having no possibility of control over the results. They suggest that most reported occurrences, though probably anvil-support techniques, are not genuinely bipolar (*some may also be pièces esquillées*). Although they may be correct, the New Guinea core-smashing technique does appear to be genuinely bipolar—*and* uncontrolled (White 1969). This article prompts a note of caution: we must be aware, as we develop a corps of expert modern knappers, that our “aborigine” may often have been interested in producing a rough-and-ready tool for a particular function and not a work of art (Callahan 1976b, Gallagher 1972).

Two related technical problems which have received a great deal of play during the last 15 years are those of manufacturing fluted points and blades. Initial studies concerned with discovering how these objects could be made at all were followed by studies aimed at identifying the means used in a particular archaeological situation.

The earliest report on experimentally manufacturing fluted points (Neill 1952) is also one of the more bizarre, and it is unfortunate that it is illustrated by drawings rather than photographs: points made by Neill’s method might resemble “Clovis” points, but I cannot see how they could resemble the Folsom points he is replicating. First one pounds a boulder into pieces, obtaining a few large, heavy flakes (Neill does not seem to have done this step). One of these, conveniently wedge-shaped, is placed in a vise (two sticks bound together and wedged apart; see illustration in Crabtree 1966), and steep short flakes are punched off of one edge and one face. After doing the same on the other edge of the same face, one is left with an edge-retouched artifact with a large mass of material remaining in the middle. The point is then placed in the vise held by the edges and with the point down, the base is chipped a little for purchase of the punch, and a flute is punched off, removing the unsightly and awkward ridge. The same series of actions is performed on the other side, the edges are touched up a bit, and the fluted point is complete. The method works—Neill made 150 points using it—but one would have to examine Neill’s points before accepting it as a possible aboriginal method.

Two articles on fluting appeared in 1961. The first, by Orville Peets, describes a simulated fluted point made by James Parsons of Lewis, Delaware (note that I am following Callahan [1976b] in using *simulation* in contrast to *replication* to indicate that fidelity to a particular archaeological example is not being attempted). Parsons prepared an essentially finished 3-inch-long obsidian point using a “small” oval hammerstone, carefully isolated a basal striking platform and, using the same stone, struck off a long flute, almost the full length of the point. The flute on the other side was similarly removed but was not as long. The flute scars bear the prominent ripple marks characteristic of hard-stone percussion on obsidian. Unfortunately, the thickness of the point is not indicated, although its overall proportions move it out of the Folsom class and into the “Clovis” size-range. The second article is a short but good discussion of unfinished fluted points and channel flakes. By experiment and examination, Cambron and Hulse (1961) conclude that fluting on both Cumberland and Clovis points was done by indirect percussion, that the points were finely finished before fluting, and that a medial ridge, formed by collateral flaking or primary side fluting, was necessary for a successful main flute. Their determination of indirect percussion is based on (1) the lack of suitable hammerstones at the sites, (2) crushing on the channel flake platforms, and (3) salient bulbs on the channel flakes. Since their observations indicate that soft-hammer percussion produces diffuse bulbs, they conclude that indirect percussion was used.

The approaches of two other amateurs to the understanding of fluting are both valid but diametrically opposed to one another. James A. Healy (1962, 1966) began to knap in 1957 and reports the progress he has made in order to help others learn. As he is still using nonaboriginal tools (for which he apologizes), his effort is a simulation of fluted-point manufacture. He uses direct percussion followed by pressure to shape the point blank and then places it in a wooden vise in order to press out the flute, using a 3-inch metal rod, 18 inches long, held in both hands as his pressure tool. Incidentally, Healy is concerned about the production of fakes by the unscrupulous for sale to the unsuspecting and marks all of his products with his initials and the date of manufacture.

Floyd Painter (1963, 1972), on the other hand, has studied in detail the remains from producing fluted points left at the Williamson workshop on Little Cattail Creek, Dinwiddie County, Virginia, in order to elucidate the mode of manufacture carried out there. He concludes that the fluting was done very early in the point-making process in order to thin the developing blank and was repeated at least once during manufacture. The process, as reconstructed by Painter, involved the selection of a large flake and its reduction by percussion to a bifacially flaked blank measuring 12–19 mm in thickness, 25–76 mm in width, and 88–100 mm in length. The basal end was then removed, using rest percussion, and one or two flutes were removed by indirect percussion. This whole process was repeated at least once and sometimes twice before the base and edges were pressure-flaked into final form and the basal portion of the point ground. The final point was at least 25 mm shorter than the original blank. Painter’s construction of this process is based on meticulous examination of thedebitage, and he does not mention having experimented. (Apparently J. Jeffrey Flenniken, after examination and replication, is reaching conclusions similar to Painter’s concerning the manufacture of the Lindenmeier Folsom points [Are Tsirk, personal communication].)

Crabtree (1966), the first reported attempt to *replicate* a Folsom point, in this case the Lindenmeier Folsom, is a model of replicative experimentation: (1) Carefully examine the archaeological specimen and its debitage and describe its...
sallent technological characteristics. (2) List all methods—in this case 11—used to try to replicate the specimen. (3) List the factors—here 35—which must be taken into account for a successful replication. (4) List the procedures followed and the results obtained in each of the experiments, noting to what degree the completed point and the associated debris resembled the archaeological specimens. (5) Indicate which method or methods replicate the specimen successfully. In the case of the Lindeneimer Folsom, Crabtree concludes that, of the methods attempted, (a) indirect percussion with rest, (b) pressure using a chest crutch, clamp, and rest, or (c) a combination of these two, applied to a finely finished point, successfully replicate the fluting. Phagan (n.d.), dissatisfied with the clumsiness of Crabtree’s vise (similar to that of most other experimenters discussed so far) and visualizing the problem as one of stabilizing such a small blank while pushing off such a large channel flake from its face, uses an atlatl hook as the vise: “It’s large enough to efficiently add the hand’s mass to the preform, as well as that of the ‘hand vise’ itself. The hook supports the tip, and the flute separates into the groove. Neat. And it works” (p. 6). At the time of writing the above, Phagan had made 100 points with the device and Crabtree was also experimenting with it.

Another responsible amateur, Nichols (1970), with 20 years of knapping experience, reports on the problem of “reverse hinge fractures” or “hanging through” in the fluting of points. Nichols found the problem of “reverse hinge fracture” to be one which did not decrease as he gained experience in fluting but remained relatively constant. He also notes that many “reverse hinge fractured” fragments are found on archaeological sites, indicating that the Indians also had difficulty in controlling the channel flake. He asks:

How much of a factor was this breakage pattern in abandoning flute points for types simpler to manufacture? Were the classical Lindenmeier Folsom points limited in their distribution, use, and length of time they were in vogue because of the high degree of breakage in manufacture by reverse hinge fracture? Due to their ultra-fine workmanship was there a higher degree of breakage by reverse hinge fracture in the classical Folsom than in the larger Clovis types? How much does the quality of stone have to do with hinge fractures?

These questions provide an excellent example of how controlled experimentation can feed back importantly into archaeological investigations, providing insights into prehistoric cultural processes which can then be tested using the archaeological record.

The first salvo in the blade debate was fired later and carried farther than that in the fluting controversy. In his critique of Semenov (1964), Bordes (1969a) details the methods by which blades may be produced: “simple percussion with elongated hammerstone, simple percussion with wooden hammer, with antler hammer, indirect percussion with a punch (certainly the most employed), and finally, in certain cases, pressure.” We have obtained blades by using each one of these techniques” (p. 11). For each method, Bordes indicates the general mode of procedure, the platform and core preparation necessary for success, and the normal characteristics of the resulting blades. He is careful to note that these characteristics are not absolute.

The next two reports are attempts to replicate particular techniques. Crabtree’s 1968 presentation of his experiments in replicating Mesoamerican pressure blades bears all the hallmarks of caution and detail one expects in a Crabtree experiment. He concludes that the method given by Torquemada, if corrected to account for Torquemada’s and the translator’s lack of familiarity with lithic technology, will serve to replicate Mesoamerican blades and blade cores. Bordes and Crabtree (1969; Bordes 1970) report on their joint replicative experiments, primarily of Clovis blades. Once again, the detail is such that the reader, if skilled, could duplicate their steps; if not skilled, could use the description as a learner’s manual; and, if not manually inclined, would still clearly understand the procedure. Bordes and Crabtree conclude that Clovis blades were made by indirect percussion with rest while Clovis blades could be replicated using hard- or pressure percussion without rest. Their presentation allows one to criticize Stoltman (1971), who, using Crabtree (1968) as his reference and considering only pressure and direct percussion as alternatives, interprets a blade cache from Minnesota as having been made by pressure. The blades’ characteristics (1971: fig. 1) clearly resemble those of blades produced by indirect percussion without rest.

A second Mesoamerican blade-making technology is discussed by Rovner (1974) in his excellent study of the Maya-pan industry. The Mayapan knappers did not remove the overhang from prior blade removal and therefore had to move their hook tip farther in on the striking platform, resulting in blades with larger platforms, relatively thick bulbs, and marked blade curvature.

Newcomer (1975) is concerned about an inadequacy in the terminology used in experimental knapping studies, which results in inaccurate or inappropriate application of experimental results to the archaeological record. He proposes that the terms method, mode, and technique be used to discuss experimental knapping (pp. 97–98): “Method refers to the stages used in making a stone artifact. . . . Mode . . . is . . . the kind of flaking used within the stage by stage framework implied by method, “hard hammer,” “soft hammer,” and “pressure.” Technique . . . refer[s] to the way in which force is applied to detach a flake, the way the piece is held during flaking, etc.” Newcomer asserts that while we can often distinguish the methods and modes used to produce particular artifacts, we can rarely if ever distinguish a single technique within a mode, but must be content with isolating a number of possible techniques. For example (p. 100):

Tixier has recently shown that using the blade method outlined by Bordes (1967) and Bordes and Crabtree (1969) the third stage, blade removal, may be accomplished by holding the core under the foot and detaching blades using a “punch” held longitudinally rather than nearly vertically on the core’s platform (Tixier 1972). My own experiments, also following the three stages, show that good replicas of Upper Paleolithic blades can be struck by direct percussion with an antler soft hammer, the core being held in the hand.

Three articles published in the next year explore the reliability of distinctions made between modes of knapping. Henry, Haynes, and Bradley (1976) analyze flakes removed by Bradley in making Clovis points using hard- and soft-hammer percussion and pressure modes. They report that (1) many soft-percussion flakes do not have the lipped platforms they are reputed to have, although very few hard-percussion flakes do; (2) it is not possible to distinguish between the two percussion modes on the basis of flake weight, but pressure flakes are lighter and show less variance around their mean; and (3) mean maximum thickness also does not distinguish between percussion modes, but pressure flakes are thinner. Sollberger and Patterson (1976) investigate the same modes, using blades produced by Sollberger, and conclude that there are no absolute differences and that those which exist might easily be masked by individual variation in knapping techniques. Chandler and Ware (1976), although able to distinguish between an original sample of percussion- and pressure-struck blades, point out that a sample of percussion blades carefully made to be thin is not successfully discriminated.

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The conclusion from all of these studies would seem to be that an expert knapper can counterfeit any knapping mode using any other, but a large sample of debitage, analyzed carefully, can be fairly reliably identified as to the mode used in producing it. The experimenters are trying to confound one another; the aborigines were undoubtedly trying to get tools made for specific purposes in the most efficient way possible and would therefore choose from their repertory of techniques that would most easily give them the desired result.

Two final topics began to receive detailed treatment in the 1970s. Muto (1971a, b) first reintroduced (see Holmes, various) the idea of the sequencing of the lithic-reduction process as a mode of analysis. He distinguished between blank, a general term denoting an early stage of lithic manufacture, preform, a piece far enough along in the process that the final product is evident, and product, the finished tool desired by the workman. Models of lithic-reduction sequences mushroomed at the IXth International Congress of Anthropological and Ethnological Sciences in 1973 (Bradley 1975, Collins 1975a, Gunn 1975, Johnson 1977a). Although Collins references Holmes (1890), none of these authors references Holmes (1894), in which the reduction sequences are very similar to the ones proposed. Bradley and Johnson are both concerned with locating and identifying nodes along manufacturing trajectories (Bradley 1975:figs. 1, 2; Johnson 1977a:chart 2); Gunn and Johnson examine the decision-making process on the part of the knappers (Gunn 1975:fig. 1; Johnson 1977 a: chart 1) and Collins and Johnson look at the nature of the materials which end up in archaeological context (Collins 1975:fig. 2; Johnson 1977a:chart 1). Bradley's sequence seems somewhat overdetailed, particularly as far as modes of fabrication are concerned. He applies it to imaginary situations, and I suspect that the archaeological reality is so much more complicated and ambiguous as to make the model inapplicable. Collins does test his model on archaeological collections, but he does not give the criteria by which he places particular objects in groups. The conclusions of Collins (1975b) seem to be better founded, and he is working on further refining analyses based on his model. Gunn's model, presented very briefly, is aimed at elucidating the relationship between the mental template held by a knapper and his output. He attempts to identify individual knappers from the marks left on their completed tools. Johnson's model is used to explain the manufacturing process involved in producing the remains in a particular quarry workshop and to generate hypotheses about patterns which might be found in differently used quarries.

The newest application of experimental flint knapping is to the study of individual variation. First taken up, to my knowledge, by Hammond (n.d.) in an attempt to evaluate, through observation of my knapping, the cognitive level necessary for manufacturing an Acheulian handaxe, and first published on by Gunn (1975), it has since been investigated by a number of lithic technologists (Gunn and Korth 1975, Hartman 1975, Johnson and Button 1975, Johnson 1977b). These studies have involved analyses of both archaeological and experimental specimens, and, although no definitive results have yet come forth, the results to date are intriguing, and research on idiosyncratic variation is continuing.

**SUMMARY AND CONCLUSION**

This review of lithic experimentation indicates that the major lines of research pursued by present knappers were well established by 1950. Some work done since then is repetitive, but many of the old problems have moved much closer to solution and new directions have been pursued.

The first problem which led modern investigators to the replication of stone tools was the recognition of man-made lithics, originally their very identification as objects which primitive man could manufacture and subsequently the distinguishing of man-made from naturally produced flakes. The first problem was easily and rapidly solved by the simple process of showing that it could be done; the second has had periods of active debate followed by times of quiescence. As I have said, I think that this debate should have been laid to rest by the publications of Warren and Pei. That it has not been seems to say more about the eternal optimism of archaeologists than about their intelligence.

Another trend in experimentation which had an early inception was the duplication of ancient implements in order to understand the manufacturing process followed in making the originals. This began quite generally with the attempts of Evans to make handaxes, but gradually led to the replication of particular effects and to detailed studies of particular industries which were given depth and accuracy by the application of insights gained through knapping. This trend is only now reaching its culmination with attempts to understand and identify the work of particular prehistoric knappers. This line was explicitly not pursued for most of the period studied: in the 1940s we find it asserted that "variation at the level of the individual is implicitly excluded from the considerations of the archaeologist, whose attention must be centered upon similarities and differences of a larger order" (Goodman 1944:415).

A final trend of research established during the pre-1950 period and followed by many present knappers is the attempt to understand the actual process of fracture of flinty materials. This trend has encompassed both subjective description and laboratory studies, from simple to elaborate. Both in the past and today, there is a mix of studies using natural materials and those using man-made glass or ceramic which have, in a simplified form, the qualities of stone tool material. An examination of the overall progress in understanding lithic fracture indicates that the process follows a repetitive course as it moves from the very general to the very particular. The initial question asked is "How is it made?" Once this question is answered to the general satisfaction of the investigator—by hitting it with a stone, by bashing it between two stones, by holding it in a vise, with a punch—the focus moves in to a particular industry and a detailed examination of how its particular implements are made. This careful examination leads to difficulties in exact replication, and it is discovered that more than one technique can lead to the same result: a handaxe may be made by hard or soft percussion, a bipolar core may be a core or a wedge, a fluted point may be fluted before or after it is retouched, a blade may be punched or pressed. This process of narrowing the focus and increasing the complexity has gone on throughout the experimental study of knapping and has led to our present realization that we will probably never know exactly how a particular archaeological specimen was made, but we can look at the stages by which it was produced in detail and present a limited number of possibilities for each.

A perplexing question is why this development has tended to move in uncertain fashion, two steps forward, one step back—why later experimenters have often shown a lack of cognizance of the results of their predecessors. The major reason may lie in the gap between the academically trained archaeologists who studied the past and the craftsmen who manufactured replicas of past implements: "Experimental interest and evidence has been generated in spurs with no extensive follow-up in between. It has centered on keen and observant craftsmen who have generally proven themselves to be masters of their art" (Muto 1971b:98). When academic archaeologists did learn the kinetic skills of knapping, their
sense of accomplishment tended to go to their heads and to blind them to the possibility that anyone could ever have learned such a marvelous thing before. This gap now seems to be closing, not only because many scholars have learned at least the rudiments of knapping, but also, and more importantly, because we are now seeing significant communication between craftsmen and scholars, which is resulting in great enrichment for scholars and, I hope, benefit to craftsmen as well.

Experimental flint knapping has come of age. Not only are all archaeologists aware that the practice exists and that it can aid in the interpretation of the archaeological record, but most who study lithic implements, even if they don't know themselves, understand at least the basic principles of lithic technology. For those of us actively involved in investigations which revolve around the use of knapping skills or information derived from knapping, the future seems to hold numerous possibilities for meaningful research. We feel that we are finally beginning to tap in depth the reservoir of information held in the remains of our oldest recorded technology.

Comments

by Jeffery A. Behm
Department of Anthropology, University of Wisconsin, Madison, Wlz. 53706, U.S.A. 30 XI 77

Johnson's article is a well-prepared and concise view of the development of experimental lithic technology. Its short, detailed, and critical discussions of the works that have contributed to this field are indeed welcome. It should become a basic introduction for both the flintworking and the nonflintworking anthropologist. Because of the scope of this work, not all topics are thoroughly covered. One such instance occurs in the discussion of heat treatment and the proliferation of reports on the subject. The author appears to accept Gregg and Grybush's (1976) thesis that unintentional thermal alteration may have occurred as the result of prehistoric quarrying activities. Postulating that fire may have been used in a number of situations to fracture the chert or other flintworking material into suitable pieces, they suggest that this material may have undergone thermal alteration and thus become indistinguishable from specimens heated at some later stage in the reduction sequence. Several objections come immediately to mind. Thermal alteration and its requirements have been extensively investigated by a number of experimenters (Crabtree and Butler 1964; Mandeville 1973; Purdy 1974, 1975; Purdy and Brooks 1971). In all cases, the application of direct and uneven heat such as results from the placing of rocks directly in a fire or the building of a fire against one side of the material produces extensive thermal stress on the material. This often results in crazing, potlidding, and other forms of destructive fracturing which will either hinder or prohibit any subsequent successful flintworking. For cherts to exhibit their characteristic change in luster and/or color, a carefully controlled, even heat must be applied. Therefore, the building of fires against the surface of a rock outcrop should not be expected to produce thermal alteration in the material. Instead, this form of heating would most likely destroy the material, removing it from any further consideration in a lithic industry.

My second comment concerns the discussion of bipolar reduction as a lithic industry. Sollberger and Patterson (1976) argue that the technique is extremely wasteful of material and virtually uncontrollable and as such would not have been employed by prehistoric workers. Johnson is correct in pointing out that this technique has been observed ethnographically (White 1969) and therefore cannot be dismissed. In a true bipolar technique, the material is supported on an anvil stone and struck on the opposite end with a heavy hammerstone (Minshall 1973), causing the material to fracture along compression stress trajectories (Faulkner 1972:57-61). This technique is indeed less controlled than the fine flintworking of an artisan, but the results are highly predictable. With one blow, the material is reduced to many long, flat fragments that can then be further worked into a number of tools by either percussion or pressure techniques. Instead of employing a high grade of nodular chert, which is well suited to the more typical forms of reduction, Sollberger should have used highly rounded cobbles, which require some form of this technique. In many areas where there are no nearby sources of suitable material, the utilization of well-rounded cobbles in stream beds and glacial deposits is likely. The presence of many pitted hammer and anvil stones along with the characteristic debris in, for example, central Wisconsin (Faulkner 1974; Overstreet 1976:95-107) and Arkansas (Goodyear 1974:61-65) should be considered evidence for the prehistoric use of this technique.

by Francois Bordes
Laboratoire de Geologie du Quaternaire et Prehistoire, Universite de Bordeaux I, 33405 Talence, France. 12 XII 77

Johnson's paper is an interesting one, not only for the history of science, but also because it points out how often communication among scientists has been lacking. For instance, when I wrote my 1947 paper I was aware of the experiments of Coutier in France and the work of A. Pond, but that was all. I had, however, made burins with Barnes and Noone, whom I met at Les Eyzies in 1935 or thereabouts, when I was a boy. Now, as a result of the 1964 lithic conference at Les Eyzies and the others that followed, either in France or the U.S.A. or elsewhere, it is unlikely that such a situation will occur again, because the importance of technological studies is understood by most archaeologists. However, no publication, no conference, no movie will ever replace the actual production, by the archaeologist himself, of the tools he is studying. One of the most striking features of the 1964 conference was that, while information flowed easily among Crabtree, Tixier, and myself, it was much more difficult to pass it on to the archaeologists who had never, or almost never, taken a hammerstone or an antler in their hands. I myself—and I know that Crabtree, Tixier, Bradley, Callahan, and others will agree—am absolutely unable to describe exactly what I am doing when I work a tool by pressure. Moreover, we do not do it in exactly the same way: the position of the hands, the knees, and the shoulder and the way the pressure is applied to the stone vary from one to the other. The same observation applies, more or less, to percussion work. This may lead to very interesting results, since most of the time I can tell whether a stone has been worked by Crabtree, Tixier, or myself. Our styles are different, but do not ask me to say what the differences are! I feel them more than I see them. I suspect that flint-knapping will have to become a necessary part of the training of any archaeologist interested in prehistory.

Some observations about the paper itself: What Coutier did was probably not heat-treating stone, but just warming it. This helps when the stone is too cold, but I wonder if the method described would remove the water. I have never seen on any of the flints worked by Coutier the characteristic sheen of heat treatment, but of course I have not seen all of them.

As for what the author says about the primitive method I used for pressure flaking in 1947, I agree up to a point. It is much less efficient than the methods developed by Crabtree, but it is feasible, and I made some arrowheads that way. As a paleolithic specialist in the Mousterian at the time, I was not much interested in pressure flaking. As for chips flying

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into my eyes, they never did; they went to my left, and not high enough to reach the eyes.

What Bradley produced from a single blow is not what we call the "chapeau de gendarme" type of butt, but what I called in my 1947 paper "en oiseau stylisé." The chapeau de gendarme is a butt which is concavo-convex seen from the side.

As for the "pièces à languelette," they are still very mysterious. I have made thousands of blades by direct or indirect percussion, but obtained this type of fracture only once. They are fairly common, however, in the Upper Perigordian of Corbiac. I doubt that they are the result of "misapplied pressure."

by DANIEL CAHEN
Musée royal de l'Afrique centrale, B-1980 Tervuren, Belgium. 22 xii 77

This article is essentially devoted to a historical study of flint-(and other raw materials) knapping experimentation. From this point of view, it is interesting to note that this approach, like many others generally considered to have been recently discovered or developed, in fact appeared at the same time as prehistoric archaeology. It was thus fitting to pay tribute to the foresight of several pioneers in that field. However, I feel that the author has not drawn sufficient conclusions from more than a century of experimentation or used the historical development to present new perspectives for future research in this field.

It seems important to note that most of the experimenters have knapped stone in an effort to authenticate prehistoric finds or to understand and explain prehistoric technology. That a number of experiments have been repeated independently several times may indicate, as the author thinks, poor circulation of scientific information (this would not be surprising). In my opinion, however, it mainly indicates the limitations of this kind of experimentation, restricted to imitation of prehistoric models. For a modern experimenter, the artifact represents a goal in itself; for the prehistoric craftsman, it was only a functional tool, a device to satisfy one or more needs. I do not claim that we already know everything about prehistoric technology, but, judging from some of Johnson's numerous quotations, I am afraid that stone-knapping experimentation tends to be an achievement in itself instead of an approach to the reality of a prehistoric way of life.

by DON E. CRABTREE
Laboratory of Anthropology, University of Idaho, Moscow, Idaho 83843, U.S.A. 30 xii 77

Johnson's paper is a most important contribution to the archaeological profession. It is well documented, an excellent successor to Hester and Heizer's (1973) lithic bibliography, and brings out a lot of inaccessible things that many of us in the profession for years had missed. My only wish is that the author had discussed more the relationship of flint-knapping experiments to the understanding of the worldwide distribution of technological traditions and their culturally diagnostic value. For example, in both the Colima-Manzanillo area of the western Mexican coast and in Belize there is evidence of a specialized tradition of perforating obsidian blades. In both areas, apparently, a small depression was drilled into one face of the blade; then a punch percussion tool was seated in the drill hole and a small cone was knocked out of the opposite (usually bulb) face. The resulting biconical hole may have a minimum diameter at its waist of only \( \frac{1}{2} \) mm. The artifacts in the two separated geographic areas look alike—we need to do some knapping experiments now to determine the whole system of their production. Once we have been made, we can look to the rest of the archaeological data (debitage; wood, shell, or bone tools; natural plant, animal, or mineral resources (e.g., strong spines) available in the prehistoric environment) to resolve whether or not the same technology obtains in both areas. By providing us with the basic information of possible or even probable production methods and techniques, experimental flint-knapping becomes an integral part of any study that ultimately looks to the definition and explanation of cultural variation among stone tool users in time and space.

by DENA F. DINCAUZE
Department of Anthropology, University of Massachusetts, Amherst, Mass. 01003, U.S.A. 15 xii 77

Johnson has prepared a useful and interesting review of a body of literature that is frequently neither. Teachers, and students new to lithic studies, will be indebted to her for this lucid summary and extensive bibliography. This article should, as the author hopes, play a major role in reducing the amount of duplication and redundancy in the literature of the future. Furthermore, it is my hope that it will mark the end of the era of anecdotal experimental reports and give impetus to more refined and more conclusive studies.

In this report which is generally so judicious, I regret the approbation giver. to the groundless old idea that alternately beveled weapon points were meant to impart spin to projectile shafts. Wilson's "experiment" in behalf of this concept was multivariate in the extreme. Neither his assumption (that the points were "arrowheads") nor his conclusion (that they were intended to cause rotation in flight) can be supported on the basis of his irrelevant test. The great majority of alternately beveled "spinner" points belong to the late Paleo-Indian and early Archaic periods; they tipped spears, lances, or darts, not arrows. In order to reach Wilson's conclusion, one would have to demonstrate that there is some utility in the rotation of such weapons and, moreover, that such weapons would in fact rotate when used in their normal mode. Spears obviously would not. There is only a remote possibility that lances or darts would rotate in their low-energy flights. I believe Sollberger's (1971) argument fits these cases as well as the case of the beveled knives of the Plains.

This review elucidates, as no other has, the strong tradition of empiricism in lithic replication studies and the attendant dearth of controlled scientific testing of hypotheses. It should be obvious to all readers that more control of the many variables in replication experiments is needed. Studies of use-wear have been moving in that direction (e.g., Keeley 1977). The separation of use-wear studies from knapping experiments in this article obscures this important trend in experimental lithic analysis. Without rigorous controls, experiments result in the dilemma met so often in past studies—one can demonstrate only a possible, not a probable, path to a desired or observed result. Johnson is sensitive to this dilemma but has not given it the emphasis I believe needed.

The inconclusiveness of many experiments in lithic studies forces the reader to ponder the ultimate utility of the recent spate of articles. Much of it deals to exhaustive lengths with very minor technical matters, while some of the most ambitious work (e.g., dissertations by Faulkner, Phagan, and Bradley) remains unpublished. Lithic replication has succeeded in de-mystifying a complex topic—stone tool production. It remains now for analysts to move into a phase of rigorous observation and control of variables if lithic analysis is to achieve its scientific aim, the elucidation of human behavior in the past.

by CONRAN A. HAY
C/O Department of Anthropology, Pennsylvania State University, University Park, Pa. 16802 U.S.A. 9 xii 77

The most commendable aspect of Johnson's review is undoubtedly its depth. Her coverage is thorough and her discussions are
These features alternate with strings of references which take presentation moves from eoliths to pressure flaking, to heat up entire paragraphs. To add to all this, the author never even and it is a not very useful piece of historiography at that. Its basis. One-line statements about the topics of individual authors are followed by copious and often unnecessary and meaningless quotations used to illustrate, among other things, writing styles. There are two outstanding aspects of this paper: its great bulk and detailed discussion of a literature that is central to the historical perspective; it also fails to inform nonspecialists of the subject not only fails to place modern lithic experimentation in economic differentiation, the extent of various forms of exchange, and the relationships of these components to other aspects of the social system are conceived as the ultimate object. American flint-knapping research, like American archaeology in general, is thus behavioral and systemic in orientation, rather than typological or technological. It is this characteristic that distinguishes it from much of what has gone before, and it is in this area that flint knapping can make its major contribution. A review that fails to emphasize this aspect of the subject not only fails to place modern lithic experimentation in historical perspective; it also fails to inform nonspecialists of flint knapping’s potential contribution to general anthropological goals.

Despite this weakness, Johnson has presented an interesting and detailed discussion of a literature that is central to the growing field of lithic technology. It should contribute to the further growth of this specialty and to the nonspecialist’s understanding of the techniques involved in the production of chipped stone tools. At this level, Johnson succeeds admirably in achieving her stated objectives.

by Thomas R. Hester
Center for Archaeological Research, University of Texas at San Antonio, San Antonio, Tex. 78285, U.S.A. 21 XII 77

Johnson’s paper is a very useful addition to the literature on lithic technology. When R. F. Heizer and I (Hester and Heizer 1973) published a bibliography of lithic experiments, we found that there was an extensive body of replicative data in widely scattered and obscure journals; Johnson is to be complimented for ferreting these out and presenting summaries for lithic specialists.

In the interest of making Johnson’s presentation even more complete, I would like to offer a few comments (and several more references) on certain major topics covered in the paper. First of all, I am extremely pleased to see the attention given to the research of J. B. Sollberger. Sollberger has worked as a lithic experimenter for many years, and his papers contain valuable data on stone tool making and stone tool use. His 1976 paper dealing with the bifacing of prismatic blades provides some solid experimental data on the technological rationale for diagonal flaking patterns.

In reviewing the heat-treatment literature, Johnson had apparently not seen Hester and Collins (1974), in which experimentation is used as a basis for testing the proposal that a distinctive projectile point series found in southern Texas was fashioned of thermally altered chert. And, while experiments are not the specific topic, the review of ethnographic accounts of thermal alteration by Hester (1972b) should provide some heat-treating examples that could be tested via experiment (in this same regard, see also Gould 1976 and Hester 1977).

While Johnson does not deal in any depth with the subject of experimental tool use, she does provide a series of references in which “the authors specify both how they made their tools and out of what materials”; another reference in which this is done, and which contains descriptions of minute-by-minute use of the experimental tools and conditions of the tools before and after experimental use, is the paper by Hester et al. (1976). In this brief paper, the experimental use of obsidian flakes in deer-cutting is described.

Johnson also notes the work of several amateur knappers. J. M. Morris, of Kerrville, Texas, is in this category, and his publication (1973) is a good, plainly written introduction to knapping experiments.

The discussion of the lithic reduction process could have been profitably expanded, but it is pleasing to see that the author has given W. H. Holmes proper credit for his early research in the realm of lithic-reduction models. I agree with her that the papers of M. B. Collins are probably the best contemporary studies of the variations in lithic reduction. An early version of his lithic-reduction concepts was applied by Hester (1975) in the analysis of site functions and the correlation of site function with settlement distribution in a study area in southern Texas.

Finally, it is to be hoped that similar syntheses of lithic studies can be published in the future by Johnson or other lithic specialists. Lithic technology continues to expand as a research area, and, as a result, there are numerous papers appearing each year. Such literature syntheses should be supplemented by specialized conferences and meetings (initiated by the international gathering at Les Eyzies; Jelinek 1965) similar to such

by Brian Hayden
Department of Archaeology, Simon Fraser University, Burnaby, B.C., Canada V5A 1S6. 30 XI 77

There are two outstanding aspects of this paper: its great bulk and its almost total lack of purpose or unifying theme. Its conclusions could have been presented in a short comment. Surely we can fill our journals and fill our own time more profitably with topics that deal more directly with the vast amount of basic research that still remains to be done in lithic studies as in almost all major branches of archaeology. Or should we look forward to future lengthy articles on the evolution of screening techniques and screen construction in archaeology?
recent events as the use-wear conference organized by Brian Hayden and the 1976 Belize Field Symposium on Maya Lithics organized by Norman Hammond and me (Hester and Hammond 1976), with the help of H. J. Shafer and Rick Wilk.

by PAUL R. KATZ
Center for Archaeological Research, University of Texas at San Antonio, San Antonio, Tex. 78285, U.S.A. 23 xii 77

I have a great deal of praise and admiration for this article, and two comments. First, the comments:

I read the article several times and found it fascinating. I suspect, however, that not everyone will care to do so, and some of the value of this exceptional resource will be lost. My one comment concerning the organization of the article would be the lack of easily recognized and more informative summaries for each period discussed. While detailed summaries would be repetitious to some extent, they would certainly benefit the reader who was not inclined to pursue the details of certain periods.

The other comment is more substantive. The purpose of the article is history, but there is a thread which runs throughout the entire course of lithic experimentation which is verbalized at the end of the article as a “perplexing question.” I do not think this point is made with sufficient force. The “question” concerns the irregular progress of lithic experimental and analytical studies and the sometimes unfortunate and amazing duplication of effort and replication of results and conclusions. One hundred years of this is enough! For instance, Johnson herself admits that she and several other scholars would have benefited from reading W. H. Holmes’ discussions of manufacturing processes; and there do indeed seem to be similarities among most attempts to devise a general model of the chipped stone manufacturing process and particularly among the models themselves. The weight of history, both analytical and experimental, suggests that we now have enough personal versions of the same general phenomenon and that it is about time to concentrate on explaining the differences in degree and kind between various culture-specific manufacturing processes. It’s time to look seriously at our models instead of admiring them.

Johnson observes an increasing communication between craftsmen and scholars. I would say that her own contribution has been to open avenues of communication between scholars and other scholars, past and present, and to have struck a blow I hope will be telling against unecessary reiteration. She has provided in one place a compendium of studies which should never be allowed to go to waste.

by RUTHANN KNUDSON
Laboratory of Anthropology, University of Idaho, Moscow, Idaho 83843, U.S.A. 12 xii 77

Oh, to have had this review available when studying for graduate exams those few years ago! Johnson is to be commended for having survived the obvious rigors of interlibrary loans and hours in the stacks in order to provide us with this compilation. There appear to be few gaps in the pre-1976 bibliography presented here.

This paper does, however, have one major “gap” in not more thoroughly discussing the general significance of experimental flint-knapping within historical and contemporary archaeology. This matter of relevance or significance is inserted at various points in Johnson’s narrative, but needs a fuller statement, particularly for the nonspecialist who attempts to appreciate these data. Except for some knappers in gunflint workshops at Brandon and other places, flaked stone tool manufacture is not part of our modern Western technical repertoire (or even of very many non-Western technologies). Yet stone tools form the major cultural residue and evidence of prehistory. Modern ethnographic studies of flaked stone technologies are limited, both because of the scarcity of societies in which knapping is still frequent and because of the simplicity of the contemporary lithic design systems. We do know that stone tool production is a reductive technology, highly constrained by both the physical nature of the stone being worked and the technical knowledge and skill of the human being working that material. We are also aware that patterns of individual or idiosyncratic design and of group or “cultural” expression are imposed on tool manufacture and use. Stone tools are sometimes the direct intermediaries between human beings and their physical environment, are sometimes items functioning more indirectly within systems of aesthetic or social values, and frequently serve in multifunctional ways over an artifact’s lifetime. As archaeologists we are trying to unravel insofar as possible all these aspects of lithic production and use systems as they relate to specific questions of human adaptation over time and space; this is certainly impossible without thoroughly understanding what is an almost extinct technology. Consequently, we need to reevaluate, of necessity by experimentation, systems of flaked stone tool manufacture and use so as to be able to distinguish, among the attributes of stone tools, the probable results of physical constraints, individual choice, and frequent yet variant patterns of human motor activity.

It is only by making this paradigm explicit that we can appreciate the need to understand the past and the present in lithic manufacturing experimentation. By assessing the state of the experimental art now, perhaps we can discontinue our reduplication of some basic investigations and get on with the construction of more complex models that better allow us to separate (or understand the probabilities of the occurrence of) physical and cultural causal factors in the creation of our archaeological data. Experimental flint-knapping in both Europe and the United States over the last generation has finally taught us that flake debitage is a valuable artifact data set, for analysis rather than discard, and there are many more valuable insights yet to be gained.

An additional point merits attention. As Johnson has pointed out, the roles of scholarly archaeologist and skilled knapper are usually not played by the same individual. Each takes a great amount of time, either in the library/lab or else in practicing one’s craft. It is the responsibility of the scholar to seek out, perhaps even nurture, the crafts-person in order to carry out well-designed and well-executed knapping experiments that provide valid data for the construction of archaeological models. This is as it should be—usually the knapper hasn’t the time or the facilities to do all the reading and writing (and money-raising) necessary for putting together problem and project, and I learned long ago that I couldn’t do these things and still knap two hours each day to retain and build my “hand.” For a greater part (leaving out the exceptional Tixiers, Callahans, Newcomers) it is nonscholars who flint-knap for the creative joy and satisfaction of their craft who have perhaps the most to tell us experimentally. If we ask the right questions, do not get too isolated within our developing lithic jargon, and respect their work as product as well as means to an end, we can probably reap tremendous rewards from the hundreds of “amateur” knappers who are spread across the United States and, I suspect, many other parts of the world. I know that in the American West there are people “making arrowheads” all around. Their knowledge and experience need to be sought out and supported.

Finally, I would like to make a plea for maintenance of high standards in both constructing and then reporting flint-knapping experiments. Communication of the rationale, design, and results (whether positive or negative) needs to be as complete and accurate as possible. Appropriate geological terminology (with citation of the reference used as basis for definition) should be used to describe the stone being worked, including its surface texture, weathering or bedding planes, special characteristics, and dimensions and weight. Both the knapping
Johnson has produced an impressive historical summary of experimentation with chipped stone. It should help to prevent the overlooking of early lithic experiments by contemporary and future archeologists. Johnson and others (e.g., Coe 1975:229–30) have pointed out this unfortunate duplication of past effort.

One important contribution of the article will be its usefulness as an introduction to past work. It is easy to recall the confusion I experienced when confronted with the task of assessing for myself some of the early experimental work after becoming interested in lithic technology and use. Also, Johnson’s work should make it somewhat easier for researchers to choose the past reports most relevant for contemporary research projects. However, the usefulness of the article is reduced by its organization and the patchiness of the critical review of the past work. The chronological organization makes it difficult to relate some of the earlier studies to the most recent ones. For example, some of the descriptive early experimentation and technological studies might be useful to modern analysts selecting technological attributes for quantitative studies. If the article had been organized in terms of topics or problems, contemporary uses for past studies and the relationship between past and present work might be clearer. Johnson treats the latter very generally in her summary and conclusion.

I perceive a disparity in the extent of critical review and discussion of the works cited. For example, many of the articles concerning the eolith controversy during the late 19th and early 20th century are described and critiqued in detail while other articles on different topics are only briefly commented upon. In some cases the articles might not need or deserve more than a brief treatment, but in others more detail would have been better. For example, the conclusions of a 1939 article by Barnes are simply summarized. Johnson includes a caveat that Barnes’s formula for distinguishing between cultural and natural origin of an assemblage assumes the collection of all (or a representative sample?) of the chipped stone from a deposit. This formula has been applied by at least one other archeologist (Bleed 1977) and may be applicable to other archeological assemblages, but I question its validity for all times, materials, and conditions. Johnson’s treatment implies that it has universal utility.

In her section in the 1960–76 period, Johnson asks rhetorically what the results of the increase in experimentation have been. She begins by identifying three traditional lines of investigation which have been continued: the description of ethnographic knapping traditions, the discrediting of eolithic “industries,” and the general description of knapping. At the end of the section she identifies two topics which began to receive detailed attention in the 1970s: the analysis of the chipped stone reduction process and studies of individual variation in knapping. In between she discusses other topics, including heat treatment, technological descriptions of whole industries, fluting, and fracture mechanics. Also discussed at various points are individual articles unrelated to these topics. Her coverage is extensive but, again, might be organized more coherently.
The paper by Johnson is a very useful contribution on the topic, but apart from the chronological compilation of the history of lithic experimentation I fail to see what contribution it makes to the understanding of lithic tool technology or even in terms of guidelines for future research. Even if we admit that the trends in this experimentation have not been organized because of the gap between academically trained archaeologists and the craftsmen who manufacture replicas of past implements, or that there is now a close relationship between craftsmen and scholars, what next? If the aim is to understand not only prehistoric tool technology but certain functional aspects, we have not even reached the stage of knowing which specific technique was used to make a particular tool. It therefore seems important to emphasize that these experiments require knowledge of the functional aspects based on an examination of the wear on the tools. Semenov's (1964) work is already known, and further progress has been made along these lines in discovering more or less the exact purpose or function. While the experimental—laboratory—approach may give us some indication of the stages of preparation, it will not tell us about or recreate the actual conditions of work, which depended upon several other factors. A combination of experiments in flint-knapping, tests of the efficiency of tools, and reliance upon ethnographic material will, however, play a vital role in the study of the functioning of ancient tools and the establishment of techniques of manufacture.

The European prehistorian might have expected more detailed treatment of the rediscovery of burin and microburin technology and of the replication of "pseudo-tools" or humanly struck flaks with natural retouch due to trampling, stream rolling, etc. As major constituents of various industries like the Tayacian, Alpine Mousterian, Clactonian, and Kharagan, these pseudo-tools present a problem which is a worthy successor to theolith question—which, incidentally, will not stay dead (Coles 1968). I was sorry, too, that there was no discussion of Dauvois's (1976) important work on drawing flaked stone tools, which makes extensive use of knapping experiments and highlights what I see as an interesting and controversial aspect of much American publication, in which photographs of stone tools are substituted for technical drawings. An accurate technical drawing shows, as no photograph can, the sequence of flaking on a piece, and it is precisely these sequences which are of interest to experimental knappers everywhere; I would have enjoyed hearing Johnson's views on this issue.

In conclusion, I would like to draw attention to a few papers not cited in Johnson's review, which concern either general aspects of knapping (Bordes 1971, Kragh 1952, Lenio 1975, Smith 1894, Tixier 1958–59, Waldorf 1975) or more specialised aspects of debitage or retouch (Barnes 1947, Cheynier and Barnes 1937, Hesse 1967, Lenio 1973, Newcomer 1976, and Hivernel-Guerre 1974).

Johnson's paper is an excellent account of flint-knapping experiments undertaken by Old and New World archaeologists during the period 1838–1976. Since the account is essentially of a descriptive nature, one is perhaps not justified in attempting a critique. However that may be, some new literature on this topic will have been published by the time Johnson's paper appears in print.

My experiments concerning the direction of the backing or striking, during the period 1838–1976. Since the account is essentially of a descriptive nature, one is perhaps not justified in attempting a critique. However that may be, some new literature on this topic will have been published by the time Johnson's paper appears in print.
surface or from both surfaces are clearly in a minority. My experiments show that backing from the ventral surface was preferred because of the following factors: In view of its flatness, the ventral surface of the blank provided a suitable platform for the pointed end of the retoucher (stone or bone) to rest upon. The latter could be held vertically and chips could be pushed off vertically across the entire thickness of the blade blank, thus producing the desired effect of steepness on the margin. The dorsal surface failed to provide a suitable platform because of the presence of one or more negative flake-scars. It was difficult to manipulate the retoucher in a vertical position, and the chips always cut obliquely into the thickness of the blank and not across it. Thus the margin could only be retouched in an oblique fashion and failed to acquire the effect of bluntness.

by PATRICIA PRICE-BEGGERLY

Johnson's article in CURRENT ANTHROPOLOGY is a sign of the blindness, of experimental flint-knappers who have difficulty seeing beyond the last decade (I admit to being as nearsighted as any). Johnson's discussion of many important early publications on flint-knapping experiments (surely she didn't find them all) should convince all researchers of the value in consulting the earlier literature. At any rate, she has convinced me. Her goal of discussing all published flint-knapping experiments from the earliest available through 1976 certainly seems to have been fulfilled.

Although it might be argued that value judgments give meaning and purpose to scientific research, I would suggest that less value judgment might have been exercised by Johnson regarding certain publications, for "valueless" experiments can often be instructive in illustrating pitfalls in experimentation and defects in method. I think this is particularly true in that her stated goal is not to address theoretical issues, but rather to review published works, indicate major concerns, and review in detail important studies.

The primary value I find in this article is the synopsis of lithic research. Now when designing experiments we no longer need repeat the labors and errors of others, for we have at our fingertips an account of previous work or at least an indication where similar research might be located. In addition, it is of value in reminding us of the need for continued communication between craftsmen and scholars as well as among all anthropologists if meaningful research is to be conducted.

by ANTHONY J. RANERE

Smithsonian Tropical Research Institute, Box 2072, Balboa, Canal Zone. 3 1 78

This article should help remedy the nearsightedness, if no longer blindness, of experimental flint-knappers who have difficulty seeing beyond the last decade (I admit to being as nearsighted as any). Johnson's discussion of many important early publications on flint-knapping experiments (surely she didn't find them all) should convince all researchers of the value in consulting the earlier literature. At any rate, she has convinced me.

I am less convinced that "experimental flint-knapping has come of age," even though I recognize that the appearance of Johnson's article in CURRENT ANTHROPOLOGY is a sign of the field's growing maturity. Experimental flint-knapping, like its sister field of microwear analysis, has been preoccupied with the development of analytical skills and techniques. As Johnson points out, we have gotten increasingly better at specifying how particular implements in particular industries are made. This includes, of course, recognizing the limits of the experimental approach, that is, knowing what we cannot as well as what we can say about aboriginal tool making. Nonetheless, this knowledge has not yet been widely applied in archaeological research. This is particularly true for nonknapping archaeologists, but can be said of a number of knapping archaeologists as well.

Now that we understand a great deal about the technology of knapping stone, it would seem appropriate to utilize this knowledge in solving problems of general archaeological interest.

Johnson seems encouraged by the fact that knappers are now talking to each other. Granting that this is a step in the right direction, I would like to see knappers go one step farther and begin talking to others.

by H. D. SANKALIA

Deccan College, Poona 411006, India. 6 1 77

This is an excellent review of the literature on the methods of flint knapping produced during the last 100 years. It seems to be fairly exhaustive and should help the teacher or student of prehistory to acquaint himself with the various methods tried in the past with a view to understanding how man during the Stone Age prepared a handaxe or a blade.

By way of comment I might say that while Bordes (1947, 1955, 1968, 1969a, b) is certainly right in saying that it was not necessary to follow any one method of flaking—any of the three methods might have been practised—still the question, I think, is whether early man practised all the methods at one time or learned them, as time went on, one after another. If we accept the latter alternative, then we can postulate some chronological development only by studying a collection of handaxes or blades. This point can be settled only when factory sites are excavated and total collections made and scientifically analyzed, for it seems certain that when the crude, anvil method of flaking was adopted, the majority of flakes would normally have wide angles and there would be deep flake-scars on the core.

I have no experience of "heat and water treatment" for removing small symmetrical flakes, but huge slabs of granite are said to have been removed by stonemasons in certain parts of India (Andhra, Karnataka, and Tamil Nadu) by this method. I wonder if it would not have helped a majority of readers, particularly students, if the author had given more detailed summaries of the principal flaking methods.

by PAYSON D. SHEETS

Department of Anthropology, University of Colorado, Boulder, Colo. 80302, U.S.A. 24 1 77

Johnson has performed a valuable task in assembling the most complete set of references to date on the topic of experimental lithic manufacture. Her article will help knappers avoid the chronic problem of isolation. As she shows well, knappers have often been ignorant of previous work, so they have tended to rediscover that which had been known and to repeat many interpretive errors, often imposing them on others through their publications.

If she errs in her history, it is in stressing chronicle at the expense of interpretation and theory. I would have liked to see a more complete development of topics and their theoretical implications. For example, she does allude briefly to one of the unresolved difficulties of replicative experimentation, the relationship between the experimental replication and the ancient...
manufacturing trajectory. Knappers too often assert that the method they used to replicate an implement was the method used aboriginally. There can be many ways of obtaining a given end product. One means of increasing the probability of a close fit between replication and ancient manufacture is the detailed analysis of debitage. Another is the analysis of the behavioral structure of a manufacturing industry (Sheets 1975b).

As Johnson mentions, Sellers (1985) used a chest crutch and a vise, but he made it clear that these were his inventions to immobilize the core. Later scholars, including Cabrol, Coutier, Barnes, Ricketson, Kidder, and Crabtree, investigated aspects of prismatic blade production. Many scholars, however, used Sellers or the ethnohistoric literature improperly, and the chest crutch and vise have come to be accepted as the means of production. It is true that they do replicate the aboriginal products, but alternative means of production have not been given the attention they deserve. One possibility is that prismatic blades were manufactured by pulling them off a core, the knapper in a seated position using a wooden pressure instrument. Scholars interested in these problems should consult the ethnohistoric literature, Fletcher (1970), and Feldman (1971).

To Johnson's excellent compilation of the literature I would like to add the following: Barnes (1947) on obsidian core-blade manufacture, J. Johnson (1976) on lithic manufacture and use at Palenque, Mexico, and Puleston (1969) on a lithic toolkit from Tikal, Guatemala.

I have three quibbles: (1) I disagree with her interpretation of Pond's "splintery scars," for I think they refer to the edges of bifaces crushed as the hammerstone drags across the edge immediately following flake detachment or, in some cases, to the negative scars of shattered percussion flakes which left well-developed radial fissures. (2) I believe she is incorrect in thinking Withthof was unaware of earlier experimentation, e.g., that of Ellis or Knowles. (3) In my reassessment of the El Chayal quarries and workshops, I argue that the proper date is the Late Classic and Postclassic. This was independently substantiated by Michels (1975) using obsidian hydration dating.

Johnson is to be commended for her accomplishment. Her article definitely fills a need, and it will be used frequently as a standard reference for those interested in lithic-replication studies.

Reply

by L. LEWIS JOHNSON

Poughkeepsie, N.Y., U.S.A. 25 t 78

The general reaction of the commentators, with the exception of Hayden, to my article is gratifying and convinces me that the time necessary to research and write it was well spent. I especially wish to thank Behm, Bordes, Crabtree, Hester, Katz, Newcomer, Paddayya, Price-Beggerly, Sankalia, and Sheets for their comments on those aspects of the review related to their specialized areas of knowledge. I am particularly grateful to Newcomer for his bibliographical additions. I apologize for the unpublished references, but in bringing the review up to date I felt that I could not ignore the information I had.

Cahen should be encouraged by Callahan's Living Archaeology projects, which address the very problem he sees by having the student knappers depend upon their tools for their subsistence. I am sorry that the irony I intended to convey in my discussion of Wilson's experiments did not come across to Dincauze; I certainly did not intend to hold Wilson up as a model experimenter! In my discussion of Barnes (1939), criticized by McManamon, I meant to assert the universal requirement of complete collection and assessment of chipped stone from a doubtfully cultural deposit in order to determine its cultural or noncultural nature. For example, the major fault in the conclusions reached by Ascher and Ascher (1965) lies in their preselection of stones, which are then found to have "cultural" flaking angles. Finally, concerning Sheets's quibbles: (1) After rereading Pond, I stick to my interpretation of "splintery scars." (2) If Witthof had read the work of earlier experimenters, he did not inform his readers of that fact. (3) Sorry.

Hay, Knudson, Malik, McManamon, and Sheets all lament omissions from my review, Malik desiring an extended discussion of experiments in tool use and the others wishing that my synthesis had been accompanied by more analysis of the relationship of experimental knapping to the greater concerns of archaeology and of anthropology as a whole. I cannot disagree, thank them for the brief discussions of what should be done, and look forward to future analyses now that we all know what has been accomplished in lithic experimentation over the past century.

To conclude, I note with pleasure the inception of a new newsletter, Flinthknapper's Exchange, edited by Errett Callahan and Jacqueline Nichols and published by The Catholic University of America. It should be especially valuable in encouraging the communication between knappers and academics that will promote the contributions experimental knapping can make to an archaeological anthropology.

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