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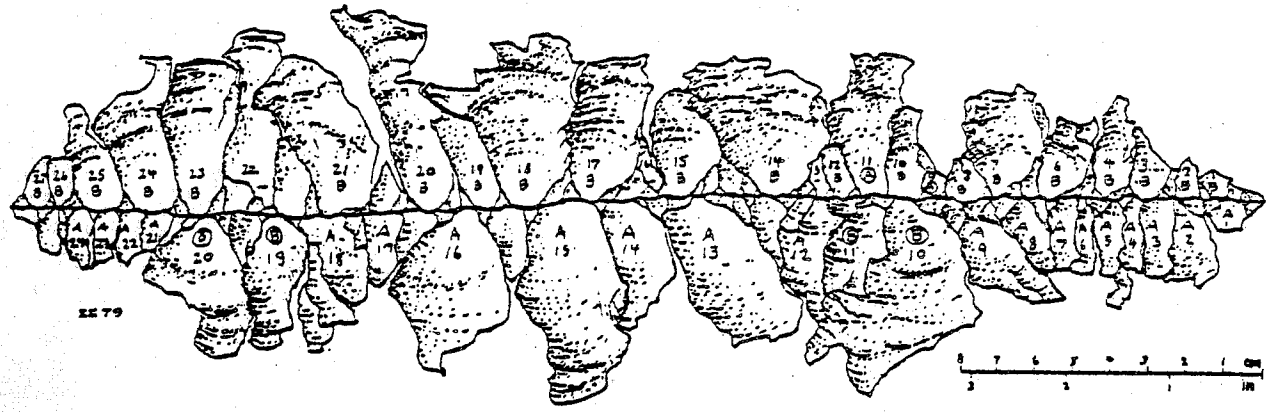

FLINTKNAPPERS' EXCHANGE

AN EXCHANGE MEDIUM OF, BY, AND FOR LITHIC TECHNOLOGISTS

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FE is published 3 times a year (Feb., May, Sept.) as an informal, non-academic, and non-statistical medium of exchange among flintknappers and lithicologists in all walks of life. Controversial issues will not be discouraged. Letters, comments, and other contributions on any aspect of lithic technology may be sent to either editor below.

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PUBLISHED BY THE GREAT BASIN FOUNDATION

EDITORIAL

We are kicking off our second year of publication with a facelift. We feel that our new cover portrays more accurately what we as flintknappers have to offer to the study of mankind's past than did our previous cover. That is, instead of flintknappers merely offering their services for rendering replications of beautifully made stone tools, we can offer a new way of looking at tools so as to lead to a greater understanding of the behavior that produced the tool, knowing that this behavior was in turn interrelated with a total utilization system of which the stone tool was but a part. (See Binford, this issue.)

For all our awareness of the importance of the cutting edge of a supposed cutting tool, there are aspects of this edge that have been totally ignored in the past by non-knappers. Flintknappers and experimentors who make and use stone tools are well aware of the ways in which edge attributes are manipulated by the flake scars deriving from that edge, of how flake scars on one face are interrelated with flake scars on the opposing face. Thus as we work, we are constantly examining both faces of the margin so as to allow us to produce a cutting edge that will accomplish its supposed job most effectively. The illustration on the cover is an attempt to portray what the knapper cognizes as he manipulates an edge. Thus, what we are saying is that the initiation of the flake scar is more important to the function of the tool in cutting than is the termination of the scar. The traditional illustration shows how flake scar terminations from opposing margins intermix in the center of any face but neglects showing the intermix of flake scar initiations. As for the cutting edge, that makes all the difference.

The cover illustration derives from a paper this editor gave at a lithic symposium, Recent Emphases in Lithic Technology, at the 40th Annual Meeting of the Society for American Archeology in 1975 in Dallas. The paper, entitled "Flake Removal Sequence and Cultural Inference," was an attempt at analyzing and replicating the Solutrean Laurel Leaves from The Volgu Cache in France. (This paper is now being revised for publication.) The famous Laurel Leaf #2, which Jacques Bordaz characterizes as the "height of craftsmanship among Paleolithic workers" (Tools of the Old and New Stone Age, p. 79), is shown in our cover drawing: both faces of one margin of one longitudinal half. The scale is just half natural size.

In conjunction with the above statement of what flintknappers have to contribute to science, please note our Crabtree interview, our Binford interview--both of whom direct themselves to this question--and to the following statement by Patterson, also in this issue: "The need for more widespread education in lithic technology is rather apparent, as many archeological reports

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CREDITS

Editorial assistance: Penelope Katson

The Illustrations: Cover: both faces of one margin of one longitudinal half of a (the well-known and magnificent) Solutrean laurel leaf #2 from Volgu, France. (In Callahan 1975:42 ("Flake Removal Sequence and Cultural Inference").) "Craftsman" illustrations are by Errett Callahan; "The Denver Series" illustrations are by Bob Patten; others are submitted by authors, as noted.

Addressing and Stapling: Students in the Henrico Developmental Center for the severely retarded/physically handicapped (Henrico Co., VA). Kathe Cohen, Director and Linda Abbey, Instructor.

continue to be published with poor or incomplete analyses of lithic collections." This is a matter which has bothered me for years. Much damage comes from supposedly up-to-date anthropology texts concerned with the origins of man, texts wherein illustrations depict prehistoric knappers in totally impossible holding positions flaking merrily away on a biface that we knappers all know would break at the first touch of the hammerstone. Then there's that one of the guy sitting on the ground with a core positioned upside down on an anvil and striking the bottom of the core with an antler punch at an angle that would only produce gravel. Then we are told the difference between "hard" and "soft" hammer percussion, with "hard hammers" being of stone and "soft" hammers being of antler, bone, or wood--as if there were no difference within the hardness range of stone or of antler. Knappers know that there are some hammerstone materials softer than most antler just as they know the range of difference between the hardest moose antler and the softest elk.

Then we are told, "Pressure flakes? Oh, they're the small ones. Percussion flakes are the large ones." Ditto for flake scars. Knappers well know that pressure scars can be massive just as percussion scars can be quite small and delicate. We know that all pressure scars don't have to be parallel just as all percussion scars don't have to be non-parallel.

Then there comes the matter of the "antler drift." What is an antler drift? I've been knapping for 23 years now, and though I have seen dozens of antler drifts illustrated in the archeological reports and have made up some as potential knapping tools, I have to this day to find anything I would use them for. Would the archeologist who so cleverly informed us as to their use please stand up and verify? Or is it the term rather than the tool which is archaic?

And why must bifaces continually be called "blades," when we all know that a blade is a specialized kind of unretouched or slightly retouched elongated flake and not a biface at all. I thought Crabtree straighened all this out in his glossary (1972). Yet the site reports come pouring forth with poorly drawn projectile points illustrated upside down, with bifaces being called "blades," having been flaked with "antler drifts" in holding positions that would only produce gravel, broken tools, or a lot of blood on the hands of the maker.

Yet isn't it a little bit our fault that we have let writers get away with this recycling of old myths? How many of us write in and complain to the publisher about the antiquated information relating to flintknapping found in their books? Sure, it is the duty of the writer to research this, to go to the knappers and get their OK or advice on this or that aspect. But the point is that flintknappers are not being utilized as a serious source of information in areas pertaining to our field, yet our field suffers accordingly. Should we not instead speak up and let it be known that we do have an area of expertise that we are willing to share? I suggest we all spread the word to all the archeologists we know so as to clear the air before more damage is done.

Flintknappers' Exchange is one means of pooling information in the field so that information may be shared. We are reaching a lot of flintknappers, true, but it will take more than this to reach the source of the education problem. Our facelift is but one attempt we will be making in order to reach a wider audience and thus to facilitate this education. Knap-ins and any reports which derive therefrom are another means of communication for both those inside and outside the field. Our effectiveness would be greatly increased, however, if we could have more EXCHANGE going on. Your editors do not want this to be a publication in which a few people have the podium. We like the regulars we do have but we would like more active exchange among our readers. Our readers are, after all, our primary writers. Perhaps we have incriminated readers by stating in the past that we would like

two months' leeway for papers and articles. I'd like to say now that all contributions are welcome at any time. We will try to squeeze them into the upcoming issue up until the day we do the final layout. This is usually a month before we can start mailing. But this is no reason to hesitate. Mail in your comment, criticism, letter, article, or whatever, whenever you wish and we'll use them in the next issue we can fit them. Spread the word on this, too.

In this issue we have the long-awaited, in-depth interview with Don Crabtree. There is no man who has done more for flintknappers than he. Almost single-handedly Crabtree has elevated the field from simply rock-busting to the respected field of lithic technology. He has shown the professional world that flintknapping must be taken seriously. And he is an absolute master of the craft, a living legend. His work is of such a high caliber that he has set standards of excellence that few of us will ever realize. Though he constantly belittles himself and pours praise on others, it is his pioneering breakthroughs into anthropological circles that have made the task of acceptance infinitely easier for the rest of us. Accordingly, we barred no holds in the interview, allowing Crabtree the opportunity to give the kind of interview he always wanted but couldn't because of limited audience interest.

The next issue of Lithic Technology will contain a clearinghouse of information of vital importance to all knappers. A survey now being conducted among its readers will specify where flintknapping raw material may be obtained, where one may attend a flintknapping field school, attend a course in chipped stone analysis, and find an annotated bibliography of lithic studies. We strongly encourage our readers to check into this most important publication. And don't forget, this editor runs an experimental field school course in which flintworking is strongly featured as part of the overall Pamunkey Project. Write for information before May 1.

PS. Please don't forget your subscription renewal or there won't be any next issue.

Errett Callahan
4 February 1979

LETTERS

I'd like to express my appreciation for the Flintknappers' Exchange. I find it a much more useful and practical source of information than other periodicals that deal with stone technology.

As one carrying out experimental research concerning the early stone age of Africa (as a member of the East Rudolf Research Project), I hope that there will be more feedback from Old World technicians as well as interest in the very early stone technologies of the world!

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U.C. Berkeley
Berkeley, CA 94720

A friend introduced me to your first issue, Jan. '78. It was like finding an unusual Indian relic which has been one of my favorite spare time activities since a young boy (42 now).

I have always been interested in producing stone tools, etc. and really got down to business about 5 years ago. However, I am rather a purist and did not seek out others who obviously know a great deal more than I.

All my work to date has been original as devised in my own mind. After seeing your first issue of Flintknappers' X, I see that I've missed much.

I would very much appreciate if you would assist me in locating data on the works of lithic technologists. Even though I have produced at least 2000

pieces, I know very little of tools utilized, platforming, type of stone and quality used and where located, etc.

My work is done by producing flakes through direct or indirect percussion, and then using bicycle spoke wrenches to apply flaking pressure. I file the spoke wrench notches to the widths and depths I require to apply correct pressure and leverage.

I can produce a rather attractive and functional point in a matter of 3 to 5 minutes.

However, in viewing some of Mr. Crabtree's beautiful work, I see I have a great deal to learn.

Thank you all for terrific find in Flintknapper's Exchange.

Jack Stewart Kelley
1786 Sherman Lane
Kennesaw, GA 30144

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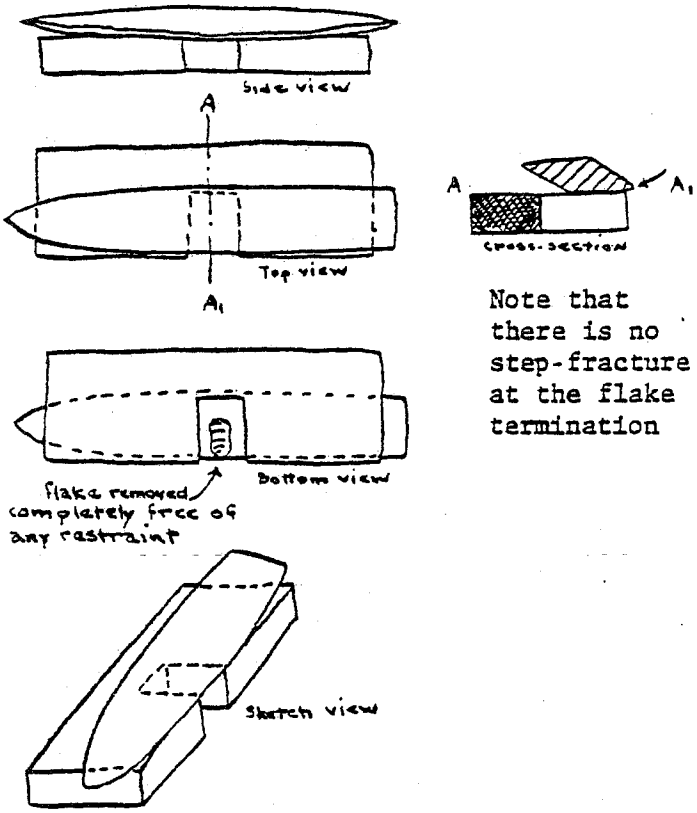
Slotted block support used by B. Bradley

Scale: ½ = 1 (approx.)



Note that there is no step-fracture at the flake termination

I feel that the strength of FE rests primarily in the letters, experimentation, and general communication. I feel that the Craftsman interviews have been the least interesting not so much the biographical info and knapping interests but all the questions on influence sound more like name dropping than true revelation of influences. A Crabtree's student elitism?

In reference to Hibben's comments on Brandon flint, I spent 3 years working Brandon flint in England and found that the surface pieces worked fine—in fact the best I found came out of soil units and not the chalk.

The illustration published in Vol. 1, no. 3 was inaccurate as to the slotted block support—please put these illustrations in your next issue.

Bruce A. Bradley
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I wish to congratulate you on the success of Flintknappers' Exchange. My personal favorite part of the newsletter is the interviews. All of them have been excellent. I do have some comments concerning a recurring theme of the interviews. There appears to be a lot of concern about the role of using metal (mainly copper) during replicative experiments. My own feelings on the subject are that copper allows extra follow through when compared with antler indentors. I note that the interviews are split over whether copper should not be considered as a viable alternative for most replicative studies. Mr. Flenniken suggests that there is archaeological verification for the use of copper indentors at a number of locations. I was under the impression that sound evidence for the aboriginal use of copper as flintworking tools is lacking. I would like to see some in-depth discussion of the matter. It is crucial that the issue be resolved if we are to

relate replicated materials to prehistoric materials. Some replicative studies in print do not identify the materials used as indentors. This makes interpretation of the results difficult.

I feel that there may indeed be valid replications made using copper or other metals. However, the use of metals in a replicative study requires that a number of assumptions about the possible situation extant in the past have been made. For example, a replication of the Brandon technique for making blades and gunflints using stone and antler indentors would be somewhat inaccurate. By the same token, a replication of fluted points using copper may be faulty. I look forward to some debate on the issue.

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The notice of the exchange in the AAA Newsletter aroused my interest. We would appreciate whatever you could send us regarding the exchange and further information about knappers in this area. We would like to have one or more flintknappers visit our campus for a demonstration. We could arrange this through out museum, the Cosumnes Community Anthropology Museum.

Thank you for whatever you may be able to offer us.

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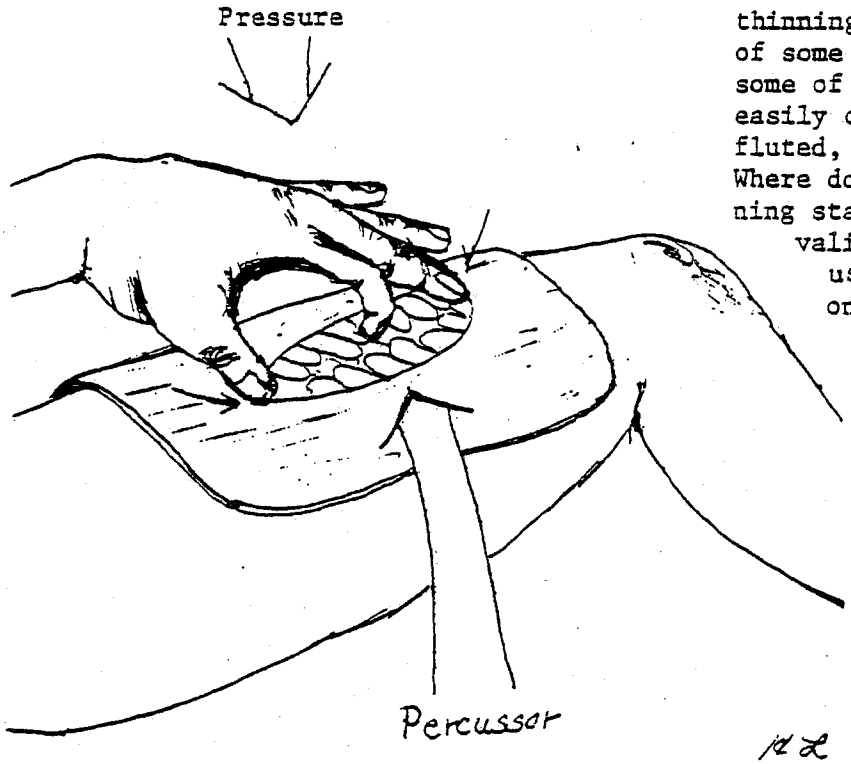
Eagerly read issue 1-3 of F.E. with excitement and interest. I was pleased to find that F.E. is rapidly moving toward a significant contribution to the understanding of common problems in lithic technology and flint-knapping.

I found Patterson's article on heat treating particularly useful in that it parallels some of my own conclusions about low heat parameters using outdoor hearths, charcoal, sand, and hot ashes. His findings have rekindled an old interest.

Flenniken caused me to do some soul-searching and hard thinking about his conceptual difference between evolutionary replication and flintknapping. I feel that he is correct in his assessment of the situation. As for his note about not annotating replicated artifacts and not being concerned about being able to differentiate between real artifacts and replicas, I would caution him about potential unintentional confusion on "out of context" artifacts as the result of experimental patination using such chemical processes as caustic soda and ultraviolet light—a technique that leached out the soda and lime from certain cherts and flints just as occurs in natural alkaline soils, especially from areas like Texas. Results are often very deceptive, though admittedly, I have not done a comparative analysis using a microscope. Besides, well executed work should be readily attributable to an individual knapper, especially if future idiosyncratic studies are potentially viable. I am especially impressed with Flenniken's high production yield and would like to see more data on such factors as failure rate, time to complete, template formulation techniques, strategies, task analysis, problems encountered, and his most difficult accomplishments and undertakings. His concept of replicating an entire aboriginal system is commendable.

I would like to second Solly's Tip Sheet No. 3, as it pertains to the prevention of a snap break while executing a Clovis flute by direct percussion. Direct end-to-end (lateral) pressure against an anvil (usually hand held against the inside portion of a leg) in concert with an appropriate perpendicular force will usually counter predictable

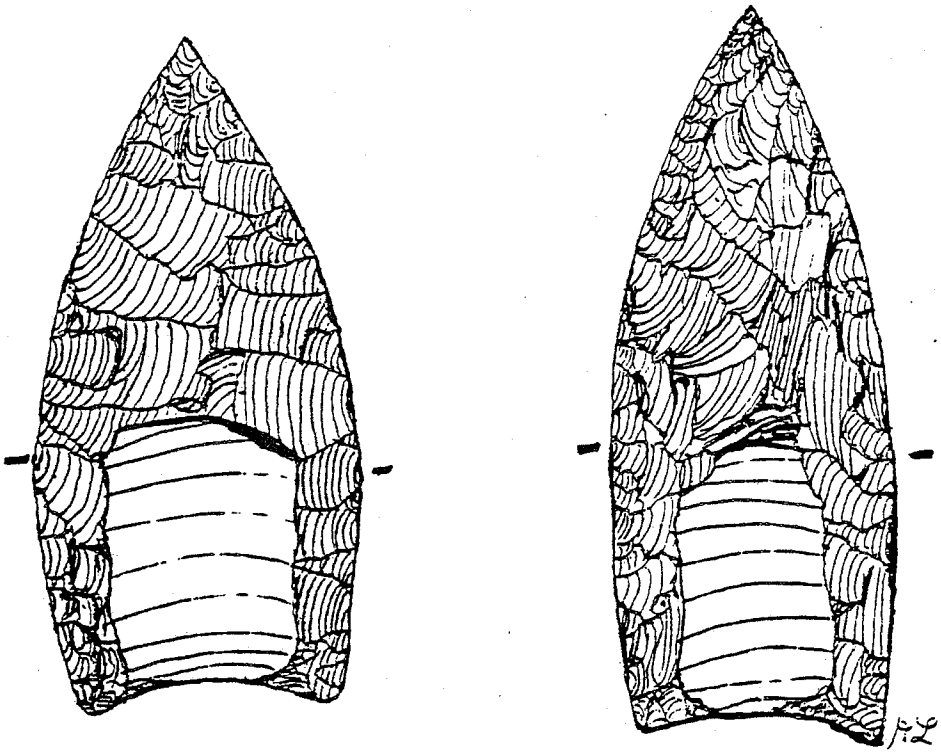
rebounding effects--which so often accounts for an undesirable snap break. I also find that the prevention of snap breaks (during the bifacing/performing stage) can best be accomplished by placing the middle finger and thumb on opposite ends of the biface and pressing down against the top side of my leg as shown in the following diagram:



This technique of bending the preform also seems to aid with flake detachment. I have had good success at replicating Clovis using only direct, soft hammer percussion, including obverse and reverse fluting. For me, this technique or method most closely approximates true Clovis fluting--as shown by random flake scars having hinged fractures and terminations, and by the resulting wide bodied flute as shown in the replicated example below. Note its similarity to the Clovis points found at Dent, Lehner, and Naco. The same method of fluting a preform on each side and then completing to final shape also works well for Folsom. To me, fluting is merely an exaggerated form of extended basal thinning--having resulted from the purposeful exploitation of a prepared platform. Later basal thinning of Clovis seemed to have reverted to multiple flake scars of minimal length, especially for Clovis points found on the east coast of the United States. I would like someone to provide me a clear definition or distinction between true fluting and basal thinning. If I compare flake scar patterns of some declared Clovis-like points with some of my own early Archaic, I can easily conclude that the Archaic is also fluted, for they are identical in pattern. Where does fluting stop and basal thinning start? I would also like to validate Solly's bent twig concept and use of the leveraged pressure. Not only does this technique remove long flakes, it also works very well for fluting.

Referring to my previous observation and comment about the orientation of oblique, parallel flake scars, I can accept the fact that some individuals have the ability to execute either direction by willful determination and as demonstrated by the rotation of their hand. My point is, however, that the aboriginal knapper would have formed convenient and logical habits for which he would not normally deviate. This means that most of his results (including flake scar orientation) would have been consistent until a new habit was formed as the result of some stimulus. It would have served no real purpose to oscillate between configurations, just as it would serve no practical purpose for me to write backwards or with opposite hands. Capability does not dictate habit. Holding the preform comfortably in the left hand and pushing off flakes with the right hand, I would expect resulting flake scars having an upper left/lower right orientation--and vice versa.

In reference to the discussion on Sandia points, I would like to see more evidence on the subject from other areas



besides central New Mexico. As a result of Judge's major survey project for the Central Rio Grande Valley and his not finding evidence of Sandia, I wonder if the type is not severely limited in distribution. I have, for example, seen similar types from the east (out of context) that were attributable to poor knapping, mistakes or as possible knives.

One item that seems to be missing in the various F.E. discussions, as well as other publications, is that of a common point of departure or baseline. Use of any imperfect language creates slight differences in understanding and perception as does individual background, experience, values, and other factors. In an effort to overcome this burden, I recommend F.E. begin a new section with a glossary of common terms of reference—similar to what Crabtree did in An Introduction to Flintworking. Responses requested.

Major Howard D. Land (Doug)
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A regional Knap-in is scheduled to be held April 7 in Casper, Wyoming in conjunction with the Wyoming Archeological Society. While this is primarily a planning session for future Knap-ins (do we want to give papers, have classes, just sit around and knap, what?) it is open to all subscribers. At least 3 of our "craftsmen" have indicated they will be present: Don Crabtree, Jeff Flenniken and J. B. Sollberger; and George Frison—who was kind enough to arrange the meeting—has hinted at a buffalo banquet, so it should be a good meeting.

Please, if you plan to come, get in touch with me at once or use the box on the renewal coupon.

Jackie Nichols

experimentation

ADDITIONAL COMMENTS FOR NOVICE KNAPPERS

Mike Johnson (FE1(3):14-15 , September 1978) has made some good remarks on my article, "Comments for Novice Knappers (FE 1(2):10-12, May 1978), and I would like to say something further on this. However, I would first like to comment that many of my ideas probably appear to be very elementary to experienced knappers. This is completely intentional. There is no need for self-taught novice knappers to "reinvent the wheel," when elementary principles can be made available. The problem is that few basic articles on practical flintknapping are available in the literature. I am in agreement with Johnson that experienced knappers should write more basic articles. It is important for all archeologists to have some practical experience in flintknapping, as background for the interpretation of archeological materials. Since flintknapping schools are generally not available, many people must depend on self-instruction. More basic articles should be made available in the literature to aid the novice. The need for more widespread education in lithic technology is rather apparent, as many archeological reports continue to be published with poor or incomplete analyses of lithic collections.

As Johnson notes, my original comments (May 1978 issue) on the angle of force application are an over-simplification, as a starting point for beginners. Larger flakes are removed by hitting more directly into the mass. This must be coordinated with striking platform angle, however, In my original correspondence with Sollberger, it took me some time to realize that biface "platform preparation" meant more than establishing a uniform edge and included good control of the striking platform angle. As force is applied more directly into the mass, to remove larger flakes, the striking platform should be beveled at a steeper angle. Force application will then continue to be fairly perpendicular to the striking platform surface. The only good way for a beginner to appreciate this is by direct experimentation. It is recommended that the novice simply try different combinations of platform angles and force application angles. Self-instruction can be successful only if a person takes time to study the effects of his actions.

The need for continuing practice to develop knapping skills must be emphasized. After four years of practice, Johnson still regards himself as a novice, and I feel about the same with my own efforts. Practice alone, however, can be rather pointless if one does not learn what each action means in obtaining flaking control.

L. W. Patterson
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COMMENTS ON HEAT TREATMENT

The entire world of heat treatment and flintknapping does not revolve around Texas flint as the last FE seems to suggest. Some of us jealous people out here do not have access to this seemly limitless Texas flint. At any rate, here at the Lithics Lab we have heat treated literally hundreds of different lithic materials found around the world with various results. There are problems, however, with "non-Texas" flints. Some lithic materials pot-lid or craze if not covered with a sand bath, others do not. So, sand or dirt baths are not important for some lithic materials but are for others. Novaculite, by the way, needs no sand bath.

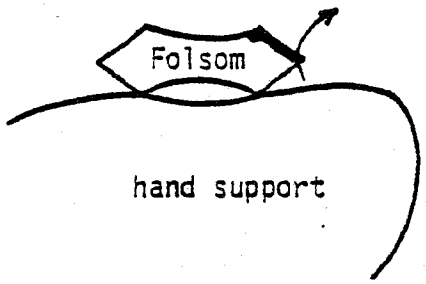
Also, time and temperature ratios are critical for successful alteration. Higher temperatures usually mean less time but this is not always the case. This also depends on whether or not a

sand bath is employed. We have not, of course, tried all possible "recipes" for heat treatment, but we have had successful heat treatment results on the same lithic materials with numerous recipes of differing times, temperatures, and ± sand baths. Also, important is the fact that all siliceous materials recrystallize (alpha to beta) at 573°C only, not before. This change causes irreparable damage to the material in times of flintknapping.

Color change arguments have been in the lithics literature for years. Color change in lithic materials is dependent upon internal ingredients of the specific lithic material as well as the "heating environment" or atmosphere. A reducing heating environment causes no color change, whereas an oxidizing heating environment does cause a color change if the material is susceptible to a color change. The "cookie sheet" method (Patterson FE 1(3):7) is an oxidizing environment and most heat treatment pits are reducing environments.

These comments are not meant to suggest that I know all the tricks of heat treatment because I certainly do not; they are, however, meant to suggest that there are numerous ways one can heat treat a specific raw material with the same favorable results. Aboriginally, I am sure this was the case.

I would also like to comment on Bradley's "grooved block technique" for Folsom retouch (FE 1(3): 5).



He may very well be correct that his techniques cause "clean" termination on Folsom points. There is, however, a much simpler technique. Prepare the margins for the removal of retouch flakes and "lift up" from the bottom, not push down into the palm. This terminates each flake just into the channel scar which causes the "neat" and centered appearance of the channel scar. No blocks are needed.

J. Jeffrey Flenniken
Director, Laboratory of Lithic Technology
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SOLLY'S TIP SHEET: HAND ANVILS

Hand Anvils (Fig. 1) may be the least mentioned and a most valuable asset to your lithic tool kit. Make it by percussion, from quartzite or other real tough stone. Polish-grind the larger acute-edged upper face around its full perimeter. It then becomes a lasting, multi-purpose tool.

Why do you need it? You need it for bifacing small thick tough chert flakes. Place a folded soft tanned deerskin on the upper surface as Fig. 1. Lay your light weight, tough blank on top so that the percussor force will clear the anvil. Four fingers are under the anvil for support, while the large thumb muscle immobilizes the preform blank—on top. The result will be longer flakes—less cupped—thinner bifaces with reduced breakage. The anvil reduces the normal blank rotation and deflection to give these good results because of its added weight and better grip afforded to the holding hand.

Your hand anvil face is used to grind down sharp-edged platforms to proper strength. Its ground, acute edge is used to form and isolate platforms. No spherical hammerstone will work as well in "tight" places. For hammerstone edge beveling using the flat-face edge, there is almost no overhang on the piece being flaked. Therefore, striking accuracy is improved. The ground edge, acute-

angled corners make a fine denticulating tool, or stem-forming percussion flaker for dart points. The hand anvil is a very versatile tool.

J. B. Sollberger
8515 Forest Hills Blvd.
Dallas, TX 75218

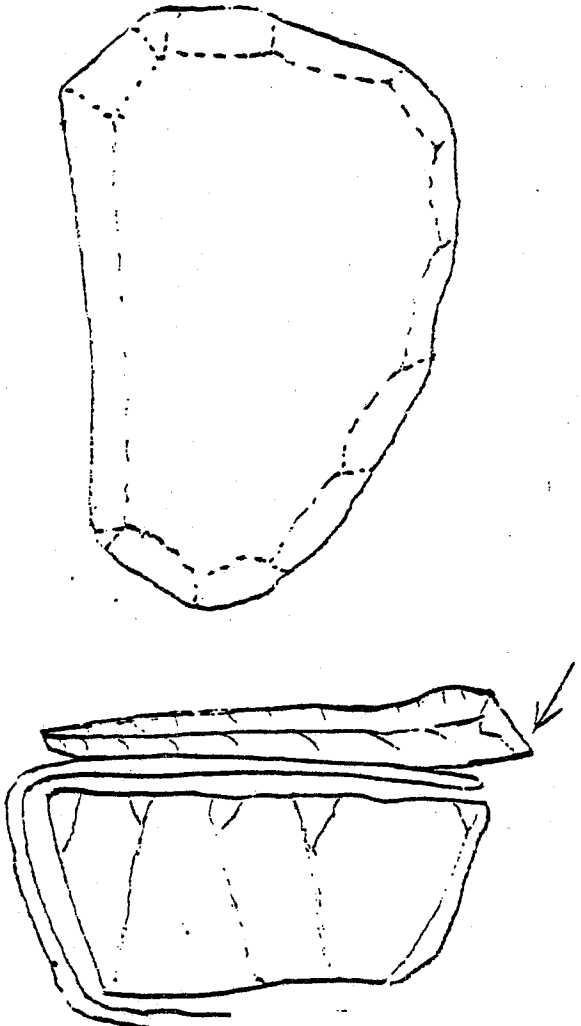

Figure 1. Hand Anvil, wt. 5 oz. Full size.

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MINIMUM EFFORT STRATEGIES IN LITHIC REDUCTION

Introduction

Many persons engaged in experimental flintknapping concentrate on obtaining extremely good control in biface thinning, starting with large preform sizes relative to final biface sizes. This produces good examples of the possibilities of fine modern craftsmanship. However, this type of experimentation may not be representative of many typical lithic reduction strategies employed by prehistoric Indians. There are, of course, many examples available of prehistoric projectile points and other bifaces with very good workmanship. While pride of workmanship can be an important factor, I submit that many Indians would have likely used some minimum effort strategy in lithic reduction, as the most straightforward approach to frequent manufacturing activities. In my own experimental work, I have used a number of minimum effort techniques and have seen many similar examples from actual archeological collections. This brief article presents several minimum effort techniques for lithic manufacturing. Most of my thoughts on these techniques are probably not new ideas and are known to many experienced flintknappers. Summaries of minimum effort strategies are not generally available in the literature, however,

Primary Quarrying Activities

One obvious factor to obtain minimum effort in manufacturing stone tools is the proper selection of raw materials, where a choice is available. It is well known that Indians traveled long distances and/or traded to obtain high quality siliceous minerals. Many aboriginal flint quarries show obvious signs of testing of individual pieces of raw material.

Another major factor in use of minimum effort is use of optimum flake sizes as the starting point for biface manufacture. Good control of flake sizes can be obtained by establishing proper core geometry and by selective use of hammerstone weights. For example, in quarrying ledge type flint in central Texas, I usually use a hammerstone weighing about 600 grams to produce flakes as tool blanks for the manufacture of small dart points, and use hammerstones weighing over 1,000 grams to obtain flakes for making large dart points and other large bifaces.

If raw material supply was not limited, Indians would many times produce large numbers of flakes and then select the most desirable specimens for final use in biface manufacture. There are many examples of this at quarry sites and knapping stations in central Texas. Large quantities of unretouched flakes can be observed scattered over large areas, where Indians were apparently very selective in which flakes were finally used.

Biface Manufacturing

The effort required to manufacture bifaces can be minimized by choice of starting flake size and geometry. Size, shape and thickness of flakes used as tool blanks are all obvious choices available to minimize the amount of reduction required to produce finished bifaces. Avoidance of large dorsal face ridges on flakes also minimizes a reduction problem.

The most apparent method of producing a biface with minimum effort is to select a flake with a uniform thickness, with a maximum thickness corresponding to the final thickness desired for the finished biface. Shaping and thinning of edges can then be done with a minimum number of flake removals required. No large flake removals covering the central portions of the flake faces are then necessary. Use of this strategy on archeological specimens can be observed when central portions of biface faces have few or no small flake scars.

It is difficult to produce flakes with completely uniform thicknesses. Often flakes are produced with the thickest part starting at the bulb of force on the proximal end. If this thick end is reduced to form the point of the projectile point, reduction efforts on the lateral edges and base of the point can be minimized due to the thinness of the distal end of the flake. On archeological specimens, use of this strategy can usually be observed by the maximum thickness of a point being nearer to the tip (say about one-third of the total point length from the tip). Also, there may be fewer flake scars on the basal end surfaces of the point.

When a flake is thicker on one lateral edge, this edge can be selectively reduced. A minimum effort will then be required for reduction of the other thinner lateral edge. This can sometimes be observed on archeological specimens by cross-sections of bifaces being asymmetrical, that is thickest toward one lateral edge. Another indication of this strategy being used is that there are more flake scars on surfaces near one lateral edge than the other. In fact, many specimens will have relatively few flake scars of large size along one lateral edge.

General Comments

Minimum effort techniques can be employed in any type of lithic manufacturing activity, and these techniques can sometimes be easily observed. For example, in east Texas, late prehistoric small arrow points are usually well made bifacial specimens. It is common, however, for well made unifacial arrow points of the same types to be present on the same archeological sites.

People who do not knap flint probably do not realize how little effort is required to produce a shaped, uniform edge on a unifacial tool. One technique is to roughly shape an edge by percussion or pressure, using a minimum of effort. Then a uniform edge is produced by raking the edge being prepared with a hammerstone or a flint flake. Fine retouch can be obtained in this manner with little effort. A similar technique has been mentioned for trimming geometrically shaped microliths in the European Mesolithic period, as follows: ". . . a fabricator was devised that would detach a number of minute retouch flakes simultaneously, so trimming whole edge in one movement." (J. Hawkes 1963, Prehistory History of Mankind, Vol. I, Part 1, Mentor Books, p. 223). I have made many fully functional arrow points using the above techniques, with only about two minutes per point required.

Possibly the ultimate in minimum effort involved in lithic manufacturing are the accounts of some Australian

aborigines. Here, rock is simply broken and whatever sharp flakes result are utilized, or sharp natural flakes are selected with no manufacturing efforts required (R. A. Gould 1969, Yiwara: Foragers of the Australian Desert, p. 82, Charles Scribner's Sons). In describing cultural behavior, the key point is not that minimum effort strategies are employed for any activity, as might be expected. Rather, the question should be asked about how frequently the same people employed the same strategy, which might then show a characteristic trait of the culture being described.

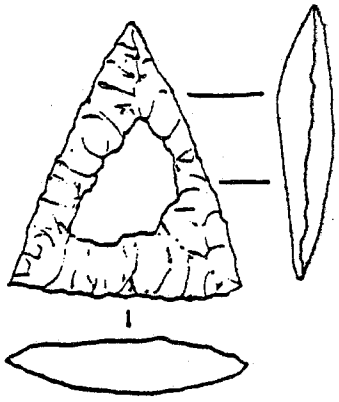

Figure 2. A triangular point with selective thinning of the thickest proximal end of a flake.

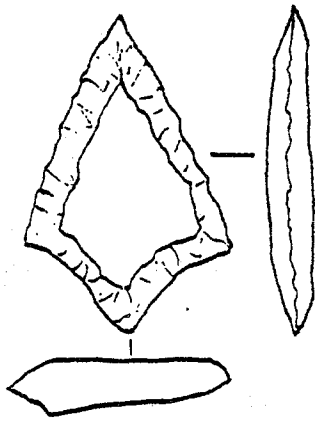

Figure 3. A contracting stem point, where the starting flake was selected for desired thickness and uniformity.

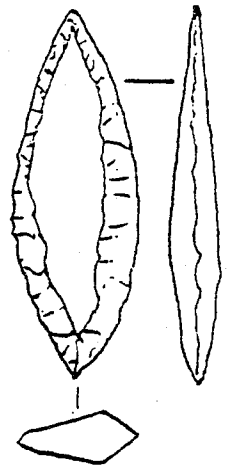

Figure 4. A leaf shaped point with selective thinning of the thickest lateral edge.

Summary

It is hoped that the above ideas will be of some use to people in the study of lithic manufacturing activities on archeological sites. It should also be noted that when any of the minimum effort strategies are used for lithic reduction, the overall debitage will tend to have smaller average flake sizes and a lower number of flakes, compared to starting with raw material pieces where much bifacial thinning is required to produce a finished item.

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EDITOR'S COMMENT:

Patterson does have a good point here: that reduction of thin flakes is an alternative to overall biface reduction. (This is the old flake vs. core tradition controversy of the Old World.) Flenniken emphasized the careful selection of starting spall in his Folsom paper in American Antiquity 1978, 43(3):474; such may apply to a greater or lesser degree to all biface industries. However, as to whether the practice of selecting the thinnest available raw material was a matter of degree or of kind, this must be left to the analyst of entire assemblages. Frankly,

I see no evidence of "thinning flake scars" on any of the projectile points above. According to my definition, to be dubbed a thinning scar, a flake must be removed so as to reduce the spine plane angle, not just the edge-angle. (See Tringham et al. in Journal of Field Archaeology 1974,1(1/2):171-195 for definition of these terms.) In other words, thinning flakes must contact prior flakes from the opposite margin: otherwise one is simply "edging."

Edging flakes, which are usually removed early in reduction or in the learning process, may very well be the terminal flakes if they produce a completed biface with edge-angles falling within the range of variation for the anticipated function or type. Nevertheless, I simply have not seen any aboriginal projectile point bifaces in my area with edge-angles as steep as those on Patterson's points: 75° to 90°. Edge-angles closer to 45° to 60° seem more typical of the aboriginal situation which I have observed. Steeply-angled, edged bifaces, however, do seem to be diagnostic of contemporary Mexican neo-artifacts, of non-authentic, commercial pseudo-artifacts, as well as of amateurish work from any period. It is possible, however, that Patterson has put his finger on a unique feature of some Texas artifacts.

Errett Callahan

PATTERSON'S REPLY TO COMMENT

1. I certainly would not argue with the technical distinction between "thinning" and "edging" flakes. In this sense, my article should have probably used a general term such as "reduction flakes," instead of "thinning flakes." However, when I talk about thinning a thick edge, I think most people can understand this terminology.

2. In respect to not seeing any "thinning flakes" on my examples, this is the whole idea. The effort of thinning is minimized by the several minimum effort strategies described.

3. There is nothing unique about seeing prehistoric example of minimum effort strategies on bifacial projectile points in east and central Texas. I have observed this also in examples from such widely separated places as Utah and Ohio.

4. I don't know exactly where Errett was measuring my points to get edge angles of 75 to 90 degress, but I do know that many portions of the edges of these points have much lower angles than this. There are, by the way, some common projectile points in Texas and Louisiana that characteristically have steeply beveled lateral edges.

L. W. Patterson

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DYNAMICS OF THE BI-POLAR TECHNIQUE

For the last nine months I have been breaking rocks using what Lewis Binford calls "the bi-polar flaking technique" (Binford 1972, pp. 346-72). Through experiencing the effects and results of this technique I have reason to believe it represents a tradition known to and used by the La Jollan and pre-La Jollan cultures of Southern California.

The essence of bi-polar flaking is this: A core is placed squarely upon an anvil (a large, stable, flattish boulder) and struck from above with a large hammerstone, or maul (H. Minshall, pers. comm., 1978). While a core is being held and steadied on its vertical axis with one hand, the maul is held by the other. The maul hand guides the maul as it is allowed to drop onto the core of its own weight and thus produces a resonating force of concussion that is delivered to the mass of the core. When the core is subjected to this type of treatment, it is being subjected to two equal (or nearly equal) forces simultaneously.

The context of the forces is this: The primary force is initiated by the "maul" and proceeds downward through the core and onto the anvil. At the same time there is generated a second force (or

rebound force) caused by the anvil's resistance to the primary force. This rebound force, being of similar intensity to the primary force, proceeds upward through the core. So in essence, the core is being compressed. The entire core is the focus of two forces which permeate its mass from top-to-bottom and from bottom-to-top.

Short of actually experimenting with this technique in your own backyard, one can only grasp the peculiar nature of bi-polar mechanics if it is compared to the mechanics of more commonly understood techniques. For this comparison I have chosen percussion and block-on-block techniques in the hope of arriving at a practical perspective.

Percussion and block-on-block techniques are defined in Bordes' The Old Stone Age. Percussion is the "direct striking of a core with a hammerstone or with a cylindrical (bone or wooden) hammer" (Bordes:1968, p. 246). Indirect percussion, using a punch, is a more refined technique allowing for increased precision but operates by the same principles as percussion. Block-on-block is also known as the anvil technique. "It is a method for removing flakes by swinging the core against the anvil" (Bordes:1968, p. 242).

Compared to what was said earlier about bi-polar physics, the physics of percussion and block-on-block techniques are rather straightforward. In both of the latter there is: 1) a "striking" implement and a "struck" implement; 2) a single focus of force is determined at the point where one object strikes the other. In percussion, the "striking" implement is a hammerstone or a hammerstone-punch combination, while the "struck" implement is the core that is being reduced and/or modified for some future end. The block-on-block technique uses a similar procedure but reverses the relationship (or roles) of the "striker" and the "struck". Here, it is the striking object that is being modified by the object being struck.

Though these two traditions give rise to different (and possibly overlapping) forms, it is important to recognize two basic similarities: 1) the focus of the force of the two masses occurs and/or radiates from one point on the core stone; and 2) the nature of the delivered force is uni-directional. On the other hand, the focus of force in the bi-polar technique is the entire core which is being compressed between two solid masses, and this condition generates a bi-directional force--one passing down the vertical length of the core, the other rebounding upward from the anvil's resistance.

A couple of the outstanding features of the effect on the core subjected to this process may illustrate some of the ramifications of the technology. Given a tabular cobble, blades may be produced that equal the vertical length of the core (Fig. 1). The thickness of the blades may vary from a thin sliver to a thick and hefty one. Or the core may split in two. The quality of the blades and the frequency of their occurrence is largely dependent upon the integrity of the material. Because of the radical nature of this bi-directional force, if the core has any incipient fracture lines in it, these are usually the parts of the core that are first affected, thus resulting in a high instance of "shatter" (chunks of useless rock). In my own experience I have found that fine-grained quartzite responds the best.

Another novel feature of this technique is that the rebound force has the power to remove flakes from the core's anvil end. This is a frequent occurrence of this technique. In fact, it is the rebound force which is primarily responsible for the flakes and blades being detached from an elongated core. The flakes knocked from the top (proximal end) of the core are usually "stepped" or "hinged". High-scale percentage analysis with pure grades of many rocks must be undertaken, however, before any precise ratio of this occurrence can be stated. Again, let me emphasize that if you are using weak or fractured material as a corestone, anything can happen--unless you can read the fractures and control the path of the forces.

Over the decades bi-polar technology has been generally regarded as a poorly

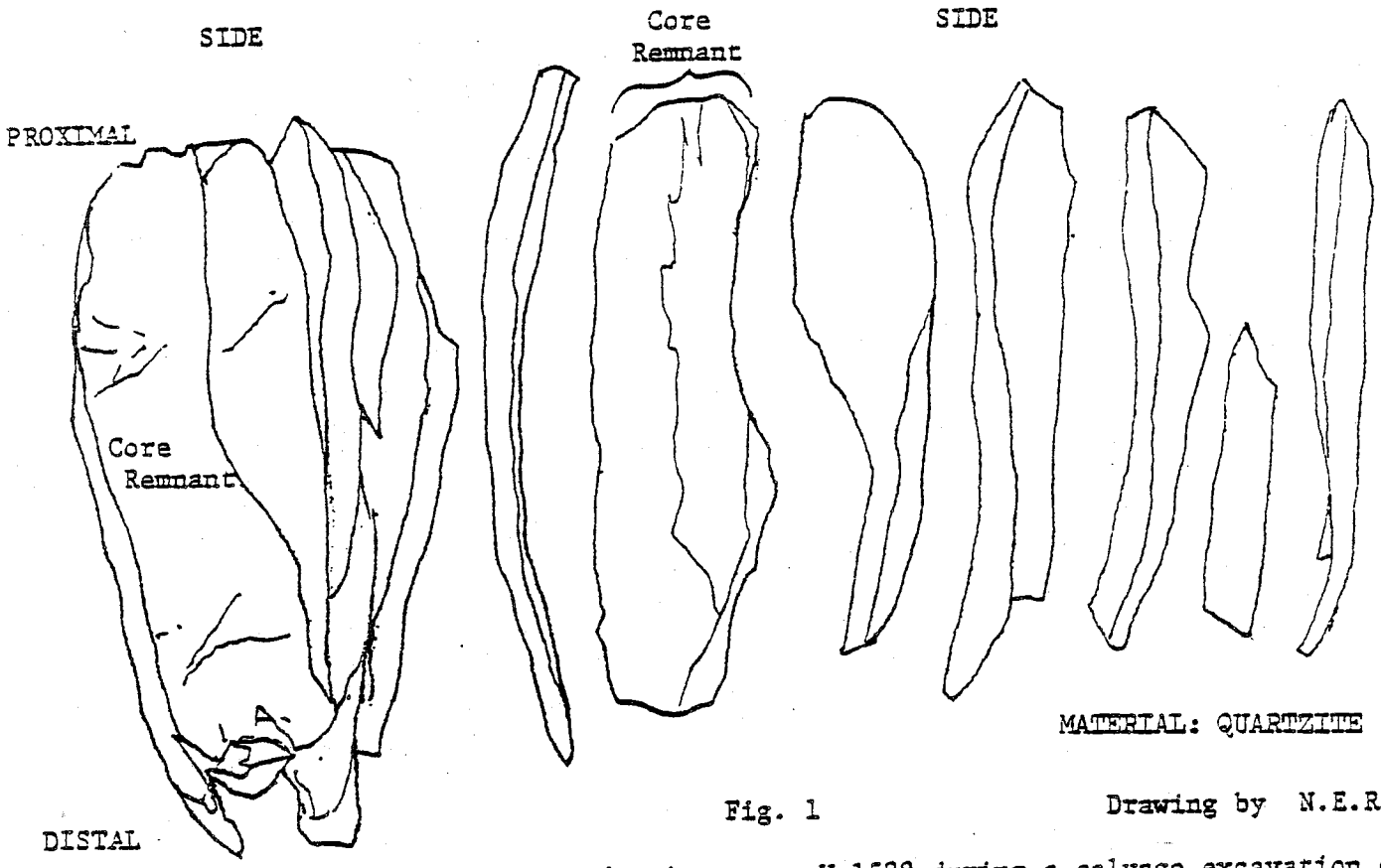

Fig. 1 Drawing by N.E.Reiner

controlled method of lithic production. In my own experience I have found that control or prediction of the effect of this process on the core is more difficult than percussion or pressure. The variables which dictate the results are of a different order by virtue of the rebound force and the fact that the entire cobble-core is the focus of force. In this technique some of the variables are the material of the core, the weight of the hammerstone in relation to the shape and size of the core, the intensity of resonance that is generated. The lines of least resistance along which cores fracture are multiplied to an unknown degree when the core is compressed as opposed to when a core is being percussed. The derived elements from a bi-polar core will have to be studied closely in order to clarify and determine the practical limits of this technique.

I have reason to believe that the La Jollans and pre-La Jollans were familiar with the bi-polar technique. Herbert Minshall sent an elongated flaked stone to China. A letter with the core was sent back by Dr. Chia lan poo. Dr. Chia identified it as a bi-polar core. In August 1978, I found an anvil stone on W-1588 during a salvage excavation of a La Jollan site in Del Mar, California. The anvil looked like some of the my specimens and was characterized by pitting on its flat surface. The last, and most important indication of the presence of bi-polar technology in this area was the discovery that "domed-scrapers" or "domed scraper-planes" can be easily manufactured by this process (Fig. 2).

Instead of an elongated core, the flat ventral surface of a round cobble that had been split [also effected by bi-polar] was placed on the anvil. This core is shaped like a dome. In order not to break the core in two, a smaller maul is used. When the dome-shaped core is struck, I find that at least 90% of the flakes originate around the anvil (distal) end of the core. I have carried out this process on cores made of quartzite, chalcedony, and a porphoritic "meta-volcanic" rock and all have responded in this way; that is, the flakes arose from the perimeter of the core's distal end and the bulb scars prove it. The variety of the types of flakes obtained in this manner vary from step-flakes to concavo-convex flakes to blades, depending on the shape of the

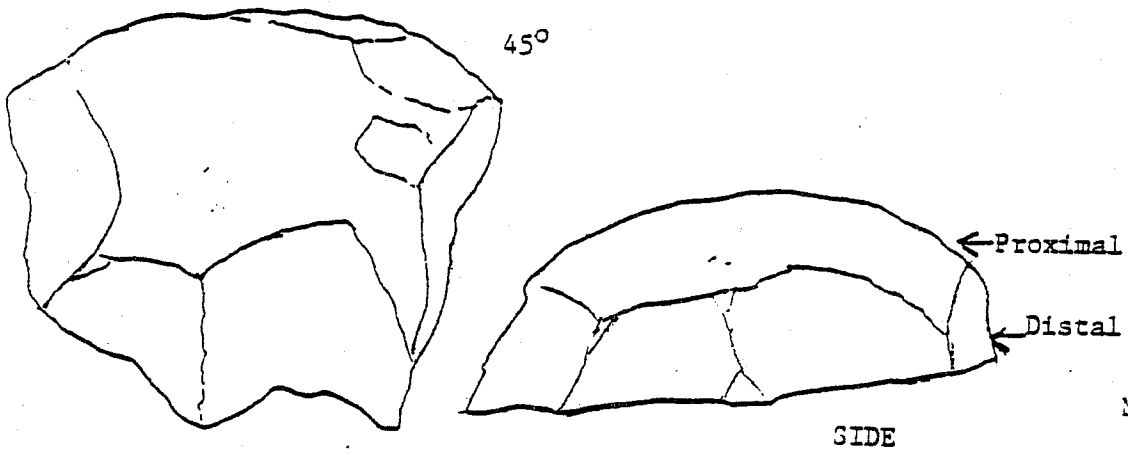

Fig. 2 Drawing by: N.E.Reiner

dome preform and its constantly altered shape.

It is necessary to make these items by the percussion technique and to compare these results with those of bi-polar, and then make a comparison with the prehistoric items. In that the striking platform of the dome undergoes much stress, battering marks are sometimes present on the core's proximal end, and may be the feature on these artifacts that separate the percussioned forms (if there are any) from the bi-polar.

Conclusion

The bi-polar flaking technology is a relatively new type of lithic technology to most New World archeologists. Archeologists here are just now becoming aware of its presence among the native American. What the implications of its presence mean still remains to be seen. One thing is certain, however, namely, the native Americans had knowledge of a technique that could easily break hard river cobbles into utilizable tools. In order to discern this technology in river bed quarry sites, we are going to have to increase our understanding of the forces that naturally break rocks in this context as well as the forces that accompany bi-polar techniques.

Though this technology has been studied in China and Russia, the ancient homelands of the New World populations, the political pressures on the academia of these countries is probably responsible for our ignorance concerning this type of technology. Another reason is that flintknapping experimentation is only now assuming the importance it deserves in figuring out the various ways and means of replicating prehistoric items foune in sites.

Because of the peculiar nature of the mechanics of bi-polar technology, I see no other recourse to understanding it other than by doing it oneself—to experience it. Just as one cannot grasp archeology as it is practiced in the field by reading a book about it, so it is with the understanding of the various lithic technologies. To attain some degree of empirical knowledge about the stones we dig up and catalogue, there is no substitute to actually replicating them oneself.

References:

Binford, Lewis R. and Quimby, George I. "Indian Sites and Chipped Stone Materials in the Northern Lake Michigan Area." In An Archeological Perspective. New York: Seminar Press, 1972. Pp. 346-72.

Bordes, François. The Old Stone Age. New York: McGraw-Hill, 1968.

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the Denver series

This article is one of a series of technological descriptions of projectile points available in facsimile from the Denver Museum of Natural History (Publications Department, City Park, Denver, Colorado 80206). The casts are widely available in universities and museums, and were chosen so that interested readers might study identical samples.

Bob Patten continues the series, describing #17 Browns Valley in terms of geometrical-technological-mechanical relations and suggests "hopefully from these descriptions an integrated methodology of artifact description will emerge which is capable of dealing in several aspects at once."

POINT #17 BROWNS VALLEY POINT
Browns Valley, Minnesota

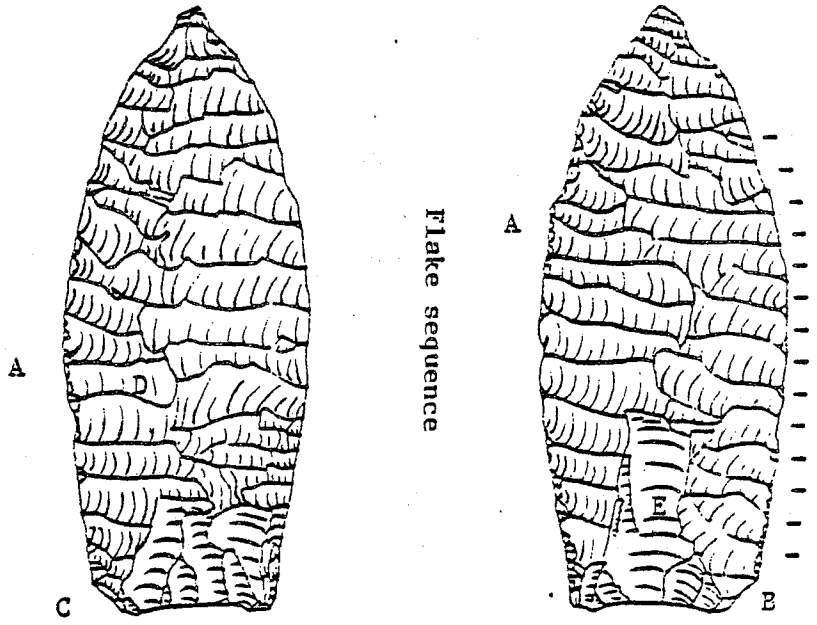
A = Continuous Platform

B = Flake Spacing

C = Start of Flake Sequence

D = Prior Stage Remnant

E = Fortuitous Flute



Flake Removal:

Flakes were removed by pressure from a continuous platform (A). A high degree of control is evidenced by the uniformity of undulations, lack of hinged terminations and very flat trajectories.

Dominant Mechanics:

Parallel arrises, wide separations at the edge, and even undulations are accumulative evidence of flake removal by maintaining an even force in the plane of trajectory for the full term of fracture. Slight deviations in the trajectory terminations reveal that the artifact had some limited freedom of movement during the fracture.

Organization:

Flake spacing (B) is remarkably even and is in part responsible for the dramatic ribbon flaking appearance of the point. The primary factor in making the chipping unique, however, is the mechanical adherence to a clockwise flake sequence.

A peculiar aspect is that the sequence started (C) at the tip on one face and at the base on the opposite face. It appears, from the remaining platform remnants, that one face was entirely finished before the edge was prepared for work on the remaining face.

Edge Treatment:

Extremely short beveling flakes were used to prepare a continuous platform completely around the edge before working the face. Between the fineness of preparation and the trimming of the flashing, little remains of the platforms on the model.

Prior Stage:

The sequence of flakes is broken at one site (D) where an island of platform preparation is also left. From this meager clue we can surmise a previous stage very similar to the final one with the possible exception that the flake scars were wider.

Retouch:

Only at the base is retouch obvious and then it is slight. Elsewhere, retouch and platform preparation are not distinguishable from each other. To get by with so little retouch, the form would have had to be well established at the prior stage.

Damage:

A break at one ear has such a contorted surface I suspect a flaw in the stone. Breakage at the tip was caused by force perpedicular to the face as if the tip was pressed between pebbles rather than the longitudinal impact expected in a use situation. Repair of a previously broken tip would have interrupted the sequencing so this can be considered a virgin form.

Hafting:

Hafting was facilitated by the thinning flakes directed toward the tip which left a concavity at the base. One large thinning flake created a short flute but the abrupt change in trajectory makes it appear fortuitous because of some flaw in the stone rather than by design. Edges expand in a straight line for the retouched portion of the base.

Width/Thickness Ratio:

The Width/Thickness ratio of 6.2 at greatest width leaves this point much more fragile than is usual for paleo-style projectiles.

Functional Aspects:

The fragility of this artifact may be more suggestive of a knife than projectile use. While narrower blades might be expected to provide better penetration, the pronounced flatness might be an offsetting factor resulting in a wide cutting edge which produced greater damage with relative ease.

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In the next issue:

More of the Don Crabtree interview. Don looks back on his career in lithics . . . reports on the knap-in in Casper, Wyoming . . . more on staging. . . .

problems / solutions

It is difficult to think of anyone who has posed more problems or offered more solutions in lithic analysis than Lewis R. Binford. He stands in much the same primary relationship to lithic analysis that Don Crabtree does to lithic replication.

For this, our first issue of our second year, we wanted to get the thoughts of both, so we interviewed Dr. Binford at the University of New Mexico where he is a professor of archeology. For our non-academic readers, Dr. Binford's articles are most easily available in a collection, An Archaeological Perspective (1972, Seminar Press, New York). To our academic readers, he needs no introduction.

The following is an interview with Dr. Lewis R. Binford at the University of New Mexico.

Jackie Nichols: You stopped doing lithics in 1968. Did you lost interest? How do you explain that?

Dr. Binford: Oh, I didn't lose interest, but at that time I could see no way of developing a methodology, in the sense that I could see no way of developing a sufficient body of knowledge which would permit the accurate interpretation of what we were seeing in lithic patterning at the assemblage level. Now, of course, that's what I was primarily interested in--inter-assemblage variability. The obvious routes of finding a group of people that you could look at ethnographically who were lithic technologists seemed to me to be impractical, i.e., there were some groups of people who were still around in which there was some knowledge of lithics but they were not using lithic technology. And there's a big difference because it's the organization of the technology that results in properties in the archaeological record, not some little knowledge as to how to make a projectile point. So I couldn't see any way of getting, or putting, myself in a good educational position to understand the organization of the lithic technology directly. I decided to try and develop, in a sense, a kind of Rosetta Stone approach. That is, if I can understand organizational variability in fauna or in site structure, i.e., how man organizes activities in space; if I can understand those things, then I could relate lithic variability to them and perhaps begin to understand something about lithic variability at a level that we haven't understood before.

So that was my reasoning in the late '60s. I gave up directly studying stone tools themselves in favor of looking at fauna, looking at hunting organization, and trying to find a system that I could look at in its totality so that I could understand something about settlement pattern, internal site structure, and variability in fauna. I think I've been successful in that; that is, I think I have now developed a method which we can use (the study of fauna itself), to tell us a lot about where we are in the adaptive system, i.e., whether we are in a hunting camp or whether we are in a base camp or whether we are in some kind of special purpose location. I can probably even tell you a lot about the system state conditions, that is, where they had enough to eat or whether they didn't, whether they were using secondary strategies or primary strategies or whatever. The thing that in a sense surprised me was this: OK, I've been successful I think in developing a methodology in the analysis of fauna I probably have even been successful (and I hedge a little bit here)--when it comes to site structure--what surprised me was that having learned what I did about those things, I now had a totally different understanding of technologies in general and it didn't matter whether it was lithic or not any more. I now had some general understanding of the organization of technology per se which I gained from these experiences which gave me a way of now coming back to lithics from a general perspective rather than trying to get a general persepctive from the lithics.

I hadn't thought that that would happen to me--that I would come away understanding as much as I did about technologies as such. So right now that's what I'm involved in doing: describing what I understand about the organizational properties of technologies and then going from those general kinds of statements back to the implications for variability in lithics reduction strategies--use strategies--collection strategies--discard strategies, etc. I think we may even come up with some operational theory of lithics independently of my fauna and site structural arguments.

J.N.: Could you describe the difference between the situational and the anticipated in technology?

L.B.: To begin an organizational statement about technology, the initial question to ask is whether or not the items are produced in anticipation of future usages, or are the items produced in order to accomplish immediate tasks? So that's the first kind of dichotomy. We can expect, I think, very different kinds of techniques, very different strategies of raw material usage, and very different patterns of wear and associations of design properties with use properties when items are situational. When items are used after the fact, their properties derive from the situation of use, in contrast to items which are used in the context of intended usage, that is, when they were produced in anticipation of future conditions and those conditions were, in fact, realized. Any modern system has some of both. I still think that Neanderthalers were all basically situational assemblers. This business of planning is a property of modern man exclusively as far as I can see from the standpoint of lithics and archeological remains in general. So when I'm talking now, I'm talking about the archeology of modern man in the Sophian sense. That is, no plans are ever perfect no matter how many plans we might make. We never succeed in anticipating everything, so that one big source of variability, and one big source of adaptation, is the degree that men plan for unplanned conditions. To what degree do men design a technology which gives them situational flexibility and what is the frequency with which that is realized? This conditions an enormous amount of variability in the archeological record quite directly. A good example, for instance, is many of the items which Frison has documented as being tools used in butchering bison. These are situationally produced. These tools were manufactured after the bison had been killed in order to accomplish a task. They were not trying to anticipate general conditions. They knew in great detail what the specific conditions were so they didn't have to build any design properties into the tools to meet a range of contingencies. They knew exactly what they needed and only needed to meet a narrow set of contingencies of tool use or design properties. Many of the assemblages that Frison has documented in large bison jumps are good examples of what I would call contingent technology, as opposed to gear that is produced as personal gear, or gear that is produced in anticipation of use in events not yet experienced. There will be differences in the archeological record between, say, an assemblage that Frison finds on a bison kill versus tools that might be used for essentially identical functions in a base camp. The tools are going to be designed differently; they're probably going to be made out of different raw material; they're probably going to have different use-life potential. There will be a lot of variability in the archeological record deriving from these kinds of organizational properties. You never get any staging in a production of situational gear. You're responding to a set of use demands that are already in existence, so you get your raw material, you make your tool, and you use it all in the same place without interruption. Whereas in the production of personal gear--gear that's curated in the sense that I use the term, that is produced in anticipation of future conditions--may be highly staged in the sense of the question that you ask [FE, vol. 1, nos. 2 and 3, Problems and Solutions]. We may procure it from the quarry in one season, we may transport partially

processed material into base camp, we may let it sit around for three months, working on it during off-work schedule time and moving an item from an introduced form of blades or flakes or cores into, let's say, a form of preform of some stage. Then I may carry it with me out to a hunting camp where I sit and finish it while I'm bored to death watching for animals, bringing it back as a finished item and storing it for use next winter. Now, under those conditions of production you're apt to get lots of staging in the sense that you are asked to question in the journal, and the degree of the staging is going to be to some extent a function of the preplanning. Also we should look at the degree that staging is designed to maximize flexibility. The longer I can maintain design flexibility, the more I can meet situational contingencies with that piece of raw material. I move it from the quarry as a big flake and I carry that thing around with me. It's a functional core at that point and I can remove flakes from it to butcher animals, I can remove flakes in order to produce five scrapers, or I may even if I need to, manufacture a projectile point or a spear blade or whatever from it, depending upon what my immediate gear needs are out here in the bush. The longer I can maintain its flexibility, the more my personal gear meets situational needs. So staging in the sense that you ask the question is going to be very, very important in industries in which you have a lot of planning.

In my experience that is a function of the stability of the environment. In a very, very stable environment you don't have to anticipate things. You know pretty much what you will need most of the time, so you can go to direct production strategy. Whereas if you are heavily dependent upon hunting, if you are in an unstable environment, then you're going to have a much more flexibly planned technology. I would expect staging in the sense that you ask the question to co-vary with the growing season primarily. The shorter the growing season, the more staging, and the longer the growing season the less staging, and more situational gear you get for a number of reasons. For instance, access. Simply having access to lithics sources is conditioned by snow cover so as the growing seasons get shorter, the more snow cover you get in winter, the more you've got to go to relying upon personal or transported gear to deal with situations that may arise in winter--the more flexible you've got to be, so the more staging you've got to have. They're the kinds of relationships that we're interested in and I think I'm now beginning to build some of them that are going to have meaning to lithic assemblages, whereas in '68 I couldn't see any of this. I couldn't see anything but lithic variability everywhere.

J.N.: What happens to concepts like style and types and the things we used to deal with?

L.B.: At one point in my training as an archeologist all variability seemed to be stylistic to me. As I learned more about technology and more about adaptation, less and less became stylistic. I'm now at the point at which I think we have to look at design alternatives among functional equivalents as the best clues to style. And we already knew this, because archeologists have been most successful when they have tried to work with style within categories like projectile points or within categories like ceramic containers. Now, sure, we can argue about functional variability--plates are different from cooking vessels--but most ceramicists know this, and when they're really dealing with style in a sophisticated way, they're talking about decorative motifs and stylistic elements that co-vary when we hold function constant. This is what any science does--it controls one thing and looks at other things to see how they vary. We have to be able to control functions in order to talk about style. We're just beginning to get to the point that we have any intuitive feeling at all about what kind of variability is responsive to functional considerations.

J.N.: Are you in any way saying that certain classes of artifacts are going to be better for figuring out functions?

L.B.: For instance, when you talk about the Frison Site, you're probably talking about the utilized flakes. Basically, I think, the less manufacturing investment that is put in them, the more immediate is the knowledge of the task to the producer, and, hence, the more specific the use is going to be for any given form. So, if you wanted to know what's going on at a site, you'd probably look at the least structured lithics first. If I was after function—and understanding function of the assemblage as a whole—I'd go right to the expedient stuff—to the situational gear first, because that will have had a shorter use-life history and therefore less complicated background noise than an item that has had a lot of investment and has been carried around and been used in different contexts.

J.N.: Would you comment on what you conceive of as parts and wholes—ethnographically speaking, that is, what you observe people doing. This is very hard for people to visualize who haven't any experience with present-day hunters and gatherers.

L.B.: The problem of the archeologist is summarized perhaps well as a problem of parts and wholes. That is, we look at a tool or a site or maybe a sample out of a site and the inevitable problem which we face is: how does what we see—which is part, inevitably, even if it's only part in the sense that it's part of what used to be (everything is not preserved)—how does this part relate to some whole that existed in the past and to what degree is the whole made up of the diversity of parts which we are unaware of. And that's the archeological problem. What the ethnographer sees is an internally differentiated system—always. There is no such thing as a system that doesn't have some internal differentiation and when you're dealing with hunters and gatherers particularly, their mobility in and of itself betrays systemic variations. That is, they cannot do everything in the same place as well as they can by paying the mobility cost and going somewhere else. So that the system is inevitably internally differentiated. When one sees such a system, one begins to appreciate all of the different dimensions along which differentiation can occur. I think I've already mentioned the business of the degree that technology is planned. We can have variation that derives from relative degree of planning of the technology. Then we can have variation that derives from what the technology is designed to accomplish, that is, the actual activities and tasks that people are differentially performing in different places and those two can be partially independent, which then means you've got combinations of permutations which are still more complex from the archeological standpoint. So that I think when one looks at a system, or has the opportunity as an ethnographer to see what a differentiated system looks like, what is driven home is that anybody working with archeological material is being unrealistic if he doesn't address what he sees as a problem of part and whole. And I think archeologists haven't. Archeologists have blithely assumed when they dig a site it's representative of a culture, or that any site is equal to any other site if it's got a big enough assemblage. Jim Ford used to say if you can get hundreds of potsherds off of it, it's as good as any other site. That kind of naiveté is staggering, I think, under modern conditions of understanding, about the way in which human adaptations are organized. But it's still very much with us. We have to make the point that systems are internally differentiated and they are also integrated along multiple dimensions. The result is incredible complexity in the archeological record.

I was up in Canada about a year ago and listened to a group of papers. I thought I was back in 1952 listening to the Midwest Archaeological Conference. These people were saying: "I went out into an area half the size of the U.S., put in two 5-foot test pits; here's the cultural sequence." This is just incredible! There's no way that that person has any appreciation for the problem of parts and wholes, of how what he sees relates to a holistic set of conditions.

He is making a set of naive assumptions which he'll have to eat.

J.N.: I want to have you reiterate something, and that is how classifying lithics as a separate subject has hurt the field. I thought that was an interesting proposition.

L.B.: Back to the business of parts and wholes. Archeologists don't have to worry about parts and wholes from the standpoint of what happened in the past. But we have to worry about it from the standpoint the social organization of archeology itself, and how we have to learn. Some of us by virtue of chance, or whatever, may learn a lot about lithics and so therefore we tend to emphasize the analysis of lithics maybe at the expense of site structure or something else. We tend to break up even at the descriptive level in a site report. We have a chapter on lithics, we have a chapter on fire cracked rock and parts; a chapter on houses, a chapter on pits, etc. We partition the archeological record into categories or observation which demand various kinds of different expertise. Those categories of observation are invented by us. They did not exist in those past systems. Lithics was not a category that made up the culture. Lithics was an element in larger activity syndromes. It was never a unit in and of itself in the past organization. Our analytical units of observation have to be reintegrated if they're going to be used as meaningful statements about the past. You can't have a chapter on lithics which will stand as a statement about the past. You have to try to understand lithics related to hearths, to houses, to hunting behavior, to whatever. We have to integrate these things. We have to begin to look for the whole and put what we've done analytically back together and most site reports don't do this. They ride with the chapter on lithics, and a chapter on houses, and a chapter on fauna, and never try to put them back together into a statement about what all those analytical categories are telling us about the past integrated organization.

J.N. It seems that something like use-tools are more integrated with the fauna than they are with, oh, say, ceramics--do you think this is the reason this kind of category has been neglected?

L.B.: Oh, I'm sure. And I think there's a whole category of things which archeologists rarely ever deal with and that is--I forget Wendell Oswald's horrible term--but he's right on--I think he calls them "nature facts"--these are things that are picked up and not modified greatly but are toally out of place. They are like tent weights, they're like anvils, or hammerstones that don't show a lot of evidence of use. Man makes use of an awful lot of things in his immediate environment without going through a big modification trip. This is even more important before evidence of children's play, for instance. I got on the kick of trying to document what the archeological record of child's play looked like and what it looks like is a bunch of stones, unmodified largely, but selected for color, for strange shape. Certainly they're not a random sample of stones, but they're not modified in any sense. Now you don't very often see a chapter in an archeological book about "unmodified stones on my site." But certainly they were important to much of the activity of past systems in many cases.

J.N.: Getting to modern flint workers--what kind of part did they play in this? Do you think their work has been useful?

L.B.: I think it has been useful but I don't think it's been used to the point of its potential. The reason being, I think, that most of the people who have encouraged or given direction to the post-1964 Crabtree-Bordes liaison and everything that's happened since in terms of replicative studies, etc. have been, I think, the wrong anthropologists. Don Crabtree, of course, never made any pretences of being an anthropologist because he was first an archeologist and then a lithics man and he knew what he

wanted to do it for. A lot of the people who have done it have seen it as a way of becoming an archeologist. You learn lithics and then you go out and you dig them up and then you look at them and say, "See, this guy was a good knapper and that was a bad knapper." That gets nowhere. So that I think there's been very little growth at the theoretical level or at the level of specifying contexts of relevance for various kinds of observations. That's why I think there's been very little contribution except at the personal level. Most of these knappers and people who've learned how to do it, love to do it. It's a great trip. Also it's a crowd getter! In other words, most archeologists even if they don't admit it, would secretly like to be able to do it. You see, they so admire these people who can sit down in a campground and in a sense create what was in the mind of the Maker, you see. But there's much more to understanding the past than that, and that hasn't been touched. These people with these skills haven't been moved beyond these performance kinds of accomplishments. But the rewards are so high. They acquire great prestige on the basis of their talent, like being an art critic.

J.N.: Well, what can be done?

L.B.: Well, I think anything that we see as a problem is usually our fault in the long run so what has to be done is that the whole field of lithic studies in archeology has to become more sophisticated. This is not a problem of flintknappers or lithics specialists. This is a problem of archeologists in general. It's a problem of how you use information and it simply has not been used very well. I hope, or I would like to say that maybe I can contribute to this change in the next 10 years. I don't know whether I'll be successful or not.

J.N.: What kinds of things could flintknappers do to help archeologists?

L.B.: Ok. Let's take something that has had some success. Most archeologists now think they can tell heat treated stones from non-heat treated stones. Here's a whole area that has had a lot of research by the lithics people. There's good research--much of it good experimental work, and we've learned a lot. Now, how do we tell the difference between heat-treating that's incidental to quarry processing and heat-treating that is done volitionally for modifying the property of the stone directly? I think those clues to those kinds of questions have to come from studying the product, that is, if there is an advantage to heat-treating stones for certain purposes, then in principle there should be some patterning in the archeological record relative to heat-treated versus non-heat-treated material. For instance, there's a fellow named Paul Brockingon. He studied lithics from the Kansas City Hopewell material. He spent a lot of time distinguishing between heat-treated and non-heat-treated material. He found some interesting things, I think. He found that a high percentage of projectile points and drills, as I recall, were heat-treated whereas other things in the assemblage were not. He also found in looking at utilized flakes, the frequncy with which utilization, or evidence of use, occurred on heat-treated flakes versus non-heat-treated flakes was much higher on heat-treated flakes (which make some sense in terms of cutting ability). Now, if heat-treating is a stage in the projection sequence, production of what type in the organizational sense that I talked about before? Certainly it's not going to be for situational gear. Is it going to be primarily in terms of other work schedules related to leisure time seasons (like winter in the temperate zone for males) or are we going to get stockpiling? Are we going to get differential scheduling because we're heat-treating? All kinds of systemic questions haven't been asked about heat-treating. This is the problem for the archeologists; the flintknappers cannot tell us this. Archeologists have to begin to use the knowledge the flintknappers are generating for us. In turn, there is a lot of expertise which flintknappers have--back to the question that you asked about the

staging. Say, you put this question to them: You have to make something, then we have to put it in a porcelain pan a..d rattle it around while we jump over a fence, and then you have to make something else out of it, at what stage are you going to put it in the porcelain pan? Now, you see, they would know where it belongs in the stage, they would have some intuitive notions of where its strength is apt to be greatest which would survive such a situation and then still allow them to do something afterwards. They're the kinds of things we need to start interjecting into the flintknappers. We need to start giving them problems that primitive man had to solve. Most of them are doing demonstrations, you see. Primitive man wasn't doing any demonstration! He was making tools; he was designing his technology. He was doing all kinds of things in order to cope with real situations that arose in his life on a day-to-day basis. The next step, with people with lots of skills, is to start giving them those kinds of problems to solve, and see what kind of design and staging properties come out of the solution to such problems. Nobody in the past produced lithic work to show to friends.

J.N.: Let me ask one other question. I've had a couple of people write in about how can we tell what's man-made from what isn't? Do we really have any variables that we're sure of?

L.B. There are some patterns, I think. But this is a statistical problem and so when you're dealing with a few examples, or a flake, or something like this, I think this is an almost insoluble question because the reliability with which we say something is a function of the redundancy in patterning in the things we're talking about. When you have something that's unique, there's no redundancy. I mean it's unique! So it must, I think, forever remain problematical. I don't mean that you have to become a statistician but I'm saying the problem is essentially a statistical problem in character because, yes, we know that there are certain patterns that are almost impossible to duplicate in nature, that is, the frequency with which flakes are removed from the same face and so forth and so on, and spacing, and all these kinds of things, but if you have a unique piece, you cannot ask those questions. You can only look at it in its uniqueness. So we may have learned an awful lot, but since it is basically a statistical problem, our knowledge doesn't increase the degree that we can take a single thing and make a very concrete statement of it. No.

J.N.: Is there anything you want to be asked?

L.B.: Not particularly. The only kind of comment I have is that lithics is personal fun for me. I started out being very interested in lithics--and when I started out, the only person that had done any kind of experimental work that was in the professional area was Johnny Wittoft and some of the people that were documented by Pond out of Wisconsin were the only sources to learn from. I went in to the lithics from a pattern recognition standpoint and did some of the staging kinds of stuff--we did the Eastport Quarry and later on the bipolar stuff. I got in from there to interassemblage variability concerns, and in the meantime met Bordes and had lots of exposure to lithics people--and good ones. And you would have thought that here after this kind of growth, I would have become more lithic. I didn't. I couldn't deal with the lithics because I knew I didn't know what was going on. That's when my lithic pessimism set in. And I went to the fauna. It's personally very great fun all of a sudden to be working with lithics again after 15 years or so. I think one of the things that lithics offers the archeologist is this combination: you can gain a personal skill and be proud of yourself secretly and have a little ego growth with lithics in a way that you can't do with statistics or computer science or some of the other things that archeologists have to learn.

MORE ON STAGING

Specific stages in stone tool manufacture are very important and most certainly were aboriginally. If a knapper sits down to manufacture one projectile point of a particular kind, distinct stages may or may not be separated mentally or physically. If that knapper, however, sits down to make 80 points of a particular kind, as I have done in the past three days, distinct stages are a reality due to the "assembly-line" technique. First, all 80 flakes were made; second, all 80 blanks were made; third, all 80 preforms were produced, and fourth, all 80 preforms were pressure flaked into finished products one at a time. I have found that considerable time and energy are saved by this assembly-line technique, and low time and energy expenditures are very important when considering techniques of manufacture. To me, stages are specific subsystems within any reduction system of stone tool production. I find similar situations in archeologically derived collections.

Jeff Flenniken

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In a previous letter, I gave you a few comments on "staging" in biface manufacture. I prefer to use the term "work sequence". Everyone uses work sequences, which can vary with the tasks performed, the materials involved, and personal abilities and preferences. For example, I started making bifaces by direct hard percussion and then learned to finish them with pressure flaking. I now find that a better work sequence is to shape some with hard percussion followed by pressure flaking, and then thin some more with hard percussion, and finish off with more pressure flaking. My main point here is that a large number of work sequences could be considered when examining debitage from actual archeological sites. Individual skill is also very important. For example, Sollberger can do work very similar to good billet flaking by use of a hammerstone, if he works carefully on platform preparations.

L. W. Patterson

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In your last issue [1(2):25] you asked for opinions on knapping in stages and its relevance to prehistoric knapping stragegies. While it may be that highly specialized prehistoric knappers such as the mastercraftsmen of the Solutrean anc Classic Maya who made extremely thin bifaces were cognizant of stages and used them as strategies, I think it is almost certain that prehistoric knappers such as those that made Abbevillian and early Acheulian handaxes did not use such concepts. As most Solutrean bifaces are really quite small and crude, it seems dubious as to whether they used such concepts either. Moreover, by using stage concepts in contemporary work, and calling Abbevillian, Acheulian, etc. bifaces typical of stage I or II, the idea is conveyed that what those prehistoric peoples (indeed all prehistoric peoples) were trying to do was really turn out a stage V biface, a top of the line product; but that because of lack of skill or whatever, they were only able to attain the first or second stage. What I am trying to say is that by using stage concepts, and as the concept has been used, all forms of bifaces are treated a stylistic variants of a basic [sic] functional tool, exhibiting teleological behavior. This conception is false, as I do not think there is any doubt that the very fine Solutrean bifaces were totally different functional artifacts from Abbevillian bifaces; and I do not think that there can be any doubt that Abbevillian bifaces were the forms sought by their prehistoric makers, nor that they performed their intended tasks in an adequate fashion. For teaching flintknapping, the system may work well, although it should be used cautiously; I am more dubious about its value for learning about emic approaches to flintknapping by the vast majority of prehistoric peoples.

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craftsman....

DON CRABTREE

Don Crabtree was born June 8, 1912, at Heyburn, Idaho. He currently resides in Kimberley, Idaho and has been a Research Associate connected with the Laboratory of Anthropology, Department of Sociology/Anthropology, University of Idaho, Moscow, ID, since 1976. He attended a semester at Long Beach College, California and in the late 1930s was supervisor of the vertebrate and invertebrate laboratory at the University of California at Berkeley. He also worked in the anthropology lab with Alfred Kroeber and E. W. Gifford at that time. Following a flintworking demonstration at a meeting of the American Association of Museums in Ohio in 1941, he was employed at the Ohio State Lithic Lab with H. Holmes Ellis and Henry Shetrone. He was also advisor in lithic studies at the University of Pennsylvania and the Smithsonian Museum. During World War II, Crabtree was coordinating engineer with Bethlehem Steel in California. Between 1952 and 1962 he was County Supervisor with the USDA in Twin Falls, ID.

In 1962 he opened the First Conference of Western Archeologists on Problems of Point Typology with a flintworking demonstration at the Idaho State Museum. This was his first major breakthrough into professional acceptance and was soon followed by his participation, with François Bordes and Jacques Tixier, at Les Eyzies Conference on Lithic Technology in France, where he received international fame. His 7 films and writings followed soon thereafter.

Between 1964 and 1975, Crabtree was Research Associate in Lithic Technology at the Idaho State Museum in Pocatello and was supported by NSF funds via Earl Swanson.

Don Crabtree is a member of 8 scientific organizations and has been the subject of innumerable articles. Among his most recent are full page features in the Twin Falls (ID) Times-News (27 March 1977 and 21 Feb. 1978) and a visual chapter in The New World, by Bray, Swanson, and Farrington (1975). He has published papers, mostly on replicative flintworking, in American Antiquity (1939, 1968), Current Anthropology (1969), Science (1970, 1968—with E. L. Davis), Curator (with Richard Gould, 1970), Tebiwa (1964, with Robert Butler,1966, 1967, 1968, 1972, 1973, 1974, and with François Bordes in 1969). His popular textbook, An Introduction to Flintworking (ISU Museum) came out in 1972. He also has chapters in Swanson's Lithic Technology: Making and Using Stone Tools (1975) and Raymond and others' Primitive Art and Technology (1975).

Besides a spot in NBC's film, The First Americans (1969), Crabtree is featured in seven 21 to 28 minute films: Shadow of Man (1968, ISU Museum), Alchemy of Time, Ancient Projectile Points, The Flintworker, The Hunter's Edge (all 1969 and available from U/C Extension Media Center, Burbank, CA 94720), and Blades and Pressure Flaking (1969, U. of California, Berkely. This film won best in anthro/arch at the 1970 American Film Festival).

In addition, Crabtree has lectured and given demonstrations around the world: Canada, The University of Bordeaux, the National Museum of Denmark, the British Museum, and universities throughout the United States. James Michener got Crabtree to edit three chapters of his best/seller Centennial. As François Bordes so aptly puts it, "If this Crabtree has lived 40,000 years ago, he could have taught ancient man a thing or two about toolmaking" (Times-News, 27 March 1977:C-10).

REPLICAS BY DON CRABTREE
The Dean of American Flintknappers

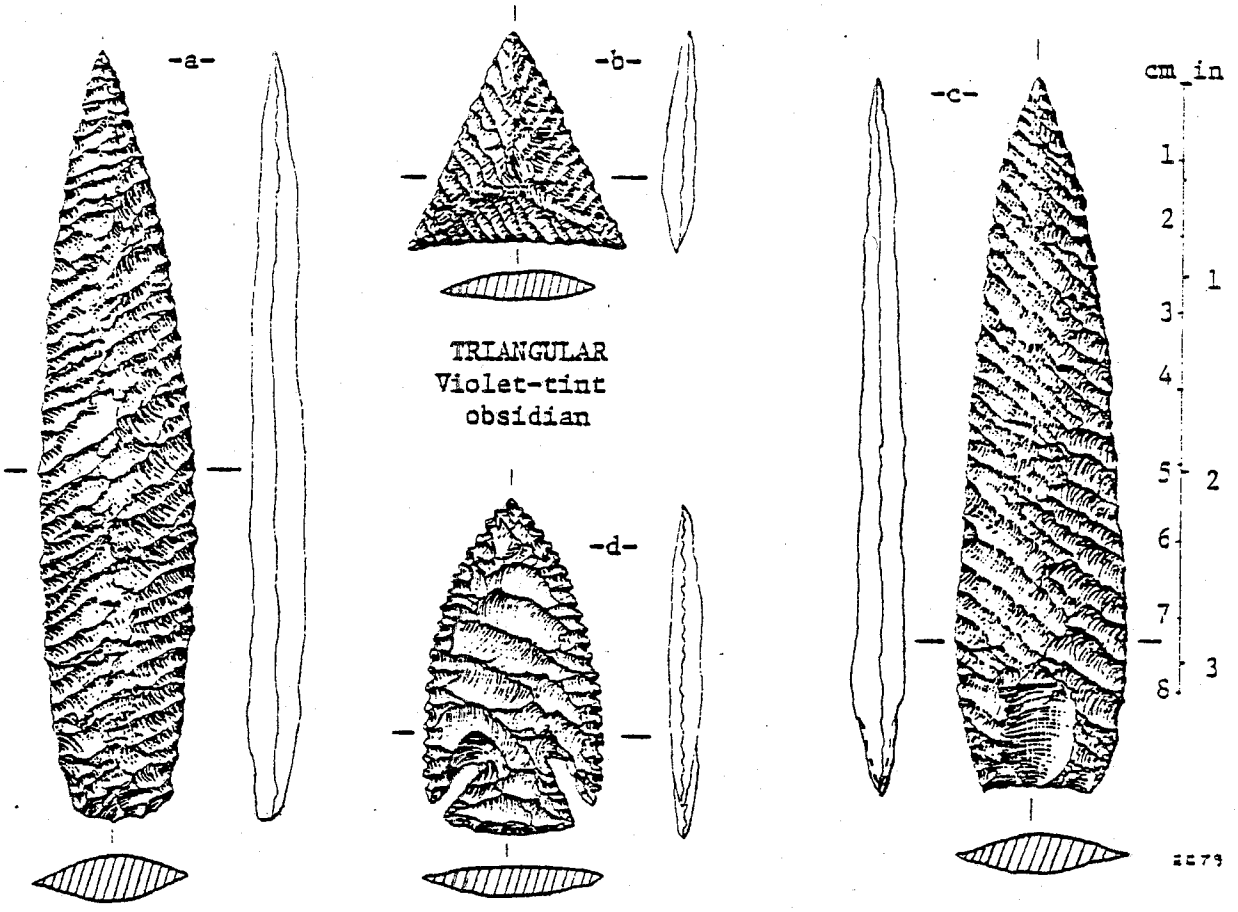


Fig. 1

A Personal View

Don Crabtree is a remarkable human being, a skilled and thoughtful craftsman and a scholar in the truest sense of the word, and I am privileged to know him as my teacher and friend. As the preceding brief outline of his career indicates, he is a self-directed person who has spent a lifetime asking questions and looking for new information. I believe him to be a better anthropologist than many who have advanced degrees in that field—his constant search for the patterns of human behavior that go with making and using stone tools, when coupled with his considerable expertise in flintknapping, has led to important contributions to world archaeology. Don has been nominated for an honorary degree from the University of Idaho, and in supporting that nomination François Bordes of the Université de Bordeaux recently wrote:

> Besides his theoretical work, which has been sadly underestimated for too long, he had a direct effect on the comprehension of the problems raised by archaeological finds, by teaching many archaeologists how to work stone. This

is easy to see in the numerous publications which stem from his work. One could say that in American lithic archaeology, there is a pre-Crabtree and a post-Crabtree period.

As Don comments in the following interview, he has spent over fifty years knapping obsidian, chert, basalt, and whatever else he could find that would work. He has reinvented heat treatment and use of obtuse tool angles, has by thousands of demonstrations shown literally millions of people across the world how to pressure flake, and still remains a hometown boy when talking to people in his community or classes. His wife Evenlyn was his constant companion, supporter, and assistant for over thirty years, and those of us who knew her warmth miss her. But thank goodness Don is a survivor, and we are grateful that we continue to share with him years of conversations about Folsom fluting, Belize blades, and an elusive obsidian source. And what a flintknapper!

Ruthann Knudson
Laboratory of Anthropology
University of Idaho

The following is an interview with Don Crabtree on December 28, 1978.

Errett Callahan: With all the modern surgical methods at your disposal, why in the world did you decide to have your surgery done with stone tools?

Don Crabtree: I think we've all experienced a certain amount of blood letting, made a contribution to the lithic blood bank. But with obsidian, with all of the cuts and the blood, there's hardly a scar. It heals up quickly, but it leaves little or no scars and they're hardly apparent. We used to cut up chicken fryers--you know, the uncut ones?

E.C.: Yeah.

D.C.: Just to practice on, we used an obsidian blade, and they're of course better than a paring knife with that ol' slick skin sliding back and forth. It just literally falls apart when you learn where the tendons are. Oh, one time I cut off the top of my little finger right up next to the finger nail--a blade caught it--and it was just hanging by a dead cuticle and so I stuck it back in place. But you could see the cartilage was still on. I thought, man, I've lost that joint, but I stuck it back on there with a band-aid and took a couple of broken matches and another band-aid on top of it and you know, in six days it had healed all except my finger was numb. I cut the nerve but you know at the end of a week the feeling was back in. It was as good as new. Oh, I've seen several pretty bad cuts a time or two, but no one has lost his eye.

Well, I don't know, those flakes, why they just zip right on through like nothing's there. I've knocked one right through my shoe and into my foot. You hardly know that you've been cut with it, though, it's so sharp.

E.C.: Does that apply just to obsidian?

D.C.: Obsidian <u>is</u> much sharper. And of course my contention is that heat treated flint is considerably sharper than the untreated. Now, I do have a scar on my left hand that I got with a jagged-edged flint. It's still there. But the glass is the sharper. I'll have to tell you about how this came about. I was working with Holmes Ellis and Shetron and he took me over to Purtell Industries which is right next to Columbus. They had the first electron microscope so when we went over that night we met a doctor who was an outstanding industrial research designer and was so enthusiastic about things

and particularly with all silicon materials. He developed this glass hat changes colors, you know, when you go out in the sun. He developed another one that had no molecular, I mean no thermal, coefficient--no contraction or expansion.

E.C.: That was in the late thirties?

D.C.: Yeah. This was right around 1940. He tested my heat treated siliceous materials and it appeared that the crystalin structure was reduced in size. Well, I lost track of Andy, and it wasn't until, oh, almost 30 years later when NBC put on that hour special. Do you remember "The First American"? Did you see that?

E.C.: Yes, I definitely remember that.

D.C.: And so anyhow he saw me there and we got back in touch again and he brought his family out with him to Sun Valley and we had just a great get-together again. He offered to do some electronmicrophotos of the blades and made comparisons of the edges. The platinum plus razor blade is the sharpest thing man has ever developed, far sharper than the old surgeon's scalpel. Those looked like exacto knives and they are not the sharpest things in the world. But even this platinum plus blade had a rounded edge at about 750 diameters. Still the obsidian blade is far sharper even at 10,000 diameters. That platinum plus may be like an aerial view of west Kansas, you know--pretty near flat. But the obsidian blade can be magnified so many thousand times more and still have been sharp. It fractures right to the last molecule of matter. There's no way you can hone a diamond or sapphire or something to ever get an edge like this. In fact, they use a glass blade for their dissections now, but if they get an eighth of an inch of cutting surface they think it's remarkable. Yet some of those cores approaching 14 inches in Guatemala would have given 20 inches of cutting edge on both surfaces of the lateral margin.

E.C.: Would you care to discuss the extent and nature of the surgery you had done with stone tools?

D.C.: Well, my doctor is a former top thoracic surgeon at the University of Oregon. After showing him some of the difference in sharpness of the tools, he was interested in giving them a try. The first surgery was when I had a rib removed and a lung section. The cut goes from right under the breast there, clear around back under the shoulder blade. So it's about an 18" cut. And you know I hardly have a scar. You have to get the light on it just right to see the sutures. And then I've had abdominal surgery four times, from my sternum down to the pelvis--one time was to remove a blood clot. Then I had bilateral femur arteries of woven dacron tubing put in. The last one was primarily arterial surgery but these were just the initial cuts; they didn't use it all the way through. And the tools are made, Errett, something like Tixier's Capsian blades from North Africa so that you can have that needlepoint and it doesn't have a right angle edge like a straight razor [Note Figure 2,b] . I just ground the lateral margin a little to dull the back side.

E.C.: Were they hafted?

D.C.: No, they were unhafted. And there was no problem with sterilization either. A fresh blade comes off sterile. And you can use it right then. Gene Titmus had his doctor use them on Karen, his daughter's arm day before yesterday. He says you kind of had to get used to them because they are a little different shape. The doctor is interested in using obsidian for eye surgery, too, when they put the eyeball together, so it doesn't hemorrhage and lose fluid. He's never tried it yet. He is going to try this on some animal eyes first. And there is another local doctor here who started to do cosmetic surgery and he thinks it has great possibilities because it's promotes such rapid healing. *What we do need to do is set up a foundation to promote flint surgery.* /Emphasis ours: the editors./

E.C.: Could you tell us about your very beginnings in flintknapping--where, when, and how?

D.C.: Errett, I suppose my first interest was when I received some arrowpoints for running errands for my mother to the neighbors. My neighbor had this box of arrowpoints--that's when they used to plow by hand--he'd pick them up and pocket them and then he'd dump them back of their old majestic range, you know, and so when I ran these errands--take a cup of sugar or something--she'd give me an arrowhead. This goes clear back, I dunno, to when I was about five years old. What was of interest to me is how they were able to work stone harder than steel with such perfection. So I got my first flakes when they gravelled the road. I picked up pieces of agate. We have no flint in this county. It's nearly all of volcanic origin. And so from there I took chips off and put some sort of a point on the end and invariably I thought they had to be notched, you know. So I'd screw around with that; eventually I'd get some little indentation in the side of it and I carried on from there. No one that I could find could remember the Indians doing it.

I have always been interested in western history and culture so it just developed from there as far as it would go. Sometimes I'd get a long flake and wonder why and ultimately I started doing so a little more frequently.

Six or 8 years later when I was working agate, I noticed that while mine had that satiny, sugary texture, the ancient artifacts looked like opals, you know—always bright and shiny. And yet that rock had to come from that area because there never was any of that other type around there. So I heated some in the oven, in the old Majestic range.

E.C.: What gave you that idea?

D.C.: Well, it was around the fire pit that stone had to be altered. It had to be changed, and heat was the only thing that I could think of. Of course, there was that rumor I think we heard of from the Boy Scout Manual where you drop water on the heated rock, and so of course we tried that with no success. But this was my first introduction to heating. I'd cover them with sand. They'd pop and crack a lot, you know, if they cooled down too fast. And so after many years I got control of this heat treatment a bit.

When I was at Columbus with Holmes Ellis, Dr. Shetrone didn't think anything of it but Holmes did see the difference, particularly with the Hopewell cores and the material that came from Flint Ridge, Ohio. The Harrison County flint doesn't alter too much. In fact, it works well just as it is. You don't have to do anything to it. But the Flint Ridge, Ohio, flint, why it enhances the colors as well and it works so much more easily and, of course, give much sharper blades.

Kim Ackerman, just in the last six months, studied Australian aborigines' heating flint. They have songs and dances and great preparations for heating their siliceous materials. It takes them four days to do it. I did it in less time in the film but by practically the same method of digging the pit. They would take the extremely hot coals out and put in sand over the earth and then flints on top and then cover that again and then build a fire on top of it. But they would wait for four days before they would dig it out. After 48 hours it's still hot; there's a chance of a fracture so they probably give it a little more time than needed.

E.C.: Was the fire burning for four days?

D.C.: No, they build a big fire and let it cool down. But they didn't uncover it for four days.

Bill Longacre, when he was working at Grasshopper, found a series of ovens and they had a little laminated sandstone in there and there was a lot of flint or siliceous material--petrified wood and agate and that sort of thing--in these ovens. I never did get a paper on them and I don't know that it was actually written up, but there are really few clues that we have regarding the aboriginal method of treating their flint. It's a pretty sophisticated thing because each flint takes a different

heat. They vary from a little over 400°F on up to, I think novaculite takes the greatest, so far, which is around 1100°F but there'a s lot of variation. Most of them break down, if they're fairly waxy to start with, at about 450°F. I find out now that even François Bordes checked it in Europe and he's gone back as far as Solutrean times. It's in Japan and I had photos of it happening there in India. It is also in the Philippines, the Arctic, South Africa--well practically every continent in the world. The Egyptians also did a lot of heat treating. In fact we seldom find natural carnelian. Yellow agate after heat treating will change to carnelian and get that dark reddish texture.

E.C.: What tools and techniques did you use for working flint at the beginning of your career?

D.C.: Well,I used an antler to start in with. Then I'd try all different types of points--I mean of different materials--copper and brass and aluminum and a number of other metals, but I find that copper more nearly approaches the antler. I think the edges would be a little sharper using antler. It takes quite a little extra grinding all the time to sharpen the antler, and for notching you've got to cut it pretty thin. It's pretty fragile for that but it works beautifully and does replicate those single crescent shaped flakes that come out on either side [Note Figure 1d and 2d]. I would think that they probably would use indirect percussion sometimes; some notching flakes are a little harder than you have strength in your hands to pop off. And then you see extremely fine flaking like on some on the retouch on the Folsom points: up to 52 flakes to an inch.

I use bone for pressure a lot--and that works very well. The rib of a walrus is something else. The hardest bone in the body is the rib, of any animal. It's that exterior surface that's extremely hard; but before I forget it, Kelly Murphy brought me in a cannon bone of a bison.

E.C.: Who did?

D.C.: Kelly Murphy. He's doing survey work for the University of Idaho through Ruthann [Knudson]. I always thought it was no good for percussion. Even a cannon bone of a horse splinters--it's soft and spongy. But if you had a chunk of the long bone of a mammal, especially some large foot bones, you might be able to do some percussion.

Another collector I just heard of yesterday has two bison cannon bones. One of them has a hole drilled into it to tie it on your belt. You should try this--it has a lot of good service and it's not a long billet. I'm kind of against the billet now where I used to use it a great deal--François uses a billet a lot--but the further distant you get from your work, the more it accentuates your errors with that long leverage. With this very short one, why I find that it is a most satisfactory tool. In fact, there was flint and siliceous rock embedded into this one. It had been used, no question about it. So we change our ideas as we go along. It helps to be kind of open-ended. *I thought that I could immediately tell the difference between percussion and pressure flaking but now I am becoming a skeptic whether I can or not.* We need a lot more experiments yet.

E.C.: Now will you tell us a little bit about what your aims and objectives in the beginning were and how they have changed in the intervening years?

D.C.: Well, I think it was much the same as it is now. I was encouraged to make quite presentable artifacts when I went to work for the University of California. The Museum of Anthropology was just right across from the Museum of Paleontology so I got to meet Dr. Kroeber and Dr. Gifford. Whenever I had a few minutes, I got to see a lot of Ishi's material. Instead of just using antler pressure tools, he used those made out of nails stuck in the end of a shaft of wood so he gave up the antler early. There was nothing ever mentioned about his percussion work other than his coating his face with mud to

keep the flakes from cutting him when he came up with a back hand. On the actual flint working of Ishi there was almost nothing written. They spent more time shooting arrows into liver and seeing what the penetration was, instead of on his beautiful work. He was a very skilled worker. It turned out that he held the flap of his quiver sometimes on the heel of his hand and did it in the same manner that I hold. Now of course I drop it down between the legs and I use my right knee and do much of it between my knees. That doesn't take any wrist pressure that way.

E.C.: At the beginning what were you trying to prove?

D.C.: Well, I was trying to prove that instead of basing typology on the morphology or the shape--and this was what was done--you couldn't convince them of anything else--they should pay attention to the surface treatment. *The surface treatment is the fingerprinting of the artistry and of the artisan that made it.*

In France, François Bordes showed me a certain strata where for three thousand years it looked like almost the same man made every one of these unifacial shouldered points. It was that consistent.

E.C.: Was it Solutrian or Mousterian?

D.C.: Well, I think this is Mousterian. Don't quote me on that. I have tried to keep away from a lot of this typology, but it was the unifacial shouldered point. I'm sure you're right--it is Mousterian. But then, like with some of these Eden points, there are a lot of tiny, almost insignificant technologies that can be of help for typing certain particular tools. Now you've got one back there in the Upper Mississippi Valley that has a V-notch at the base and instead of grinding the stem, they would remove a burin from both sides, which is distinctive. Then, in North Africa in the Capsian, they would take a type of burination on the tip of their projectile points and it would be needle sharp. They would probably grind it a bit so that the platform was removed with the flake so it produced an extremely sharp needle tip. There are all of these little idiosyncracies that could prove most valuable in typology. In fact, with the Meso-American cores, after writing that paper I find there's no such thing as Meso-American Core. It's like describing an apple as being representative of all apples. There are different kinds of core types and different types of termination preparation where the overhangs were removed. There are probably many technologies we don't know about in the preparation of the core prior to getting it ready for pressure flaking. All of these things can be traced in time and space and be pertinent to certain people. I think you can see influences and directions and movement and combinations and techniques and these things are tremendously important. This is where ultimately our computerization is going to come in so we can get a description of these things. Now it's like the perforation of a eraillure flake that occurs on the west coast of Mexico; the same thing happens in Belize, British Honduras, where they use this sort of punch to knock out those little micro cones from the far side [See his film "The Flintworker."] It's amazing because even a steel point is hardly strong enough to knock out the rest of the little cone after it's been drilled part way through. For another example, Harry Schaffer has written this up on sharpening the adz by removing an orange peel slice, and this is a curved flake that comes clear around the chip. You don't have to grind it; it leaves a razor sharp edge. In Egypt they sharpened these pressure blades by taking that same orange peel flake by pressure from the tip and sharpened it from right to left and left to right until it was entirely used up. They had a beautiful scalpel with that nice curve protecting a razor edge point.

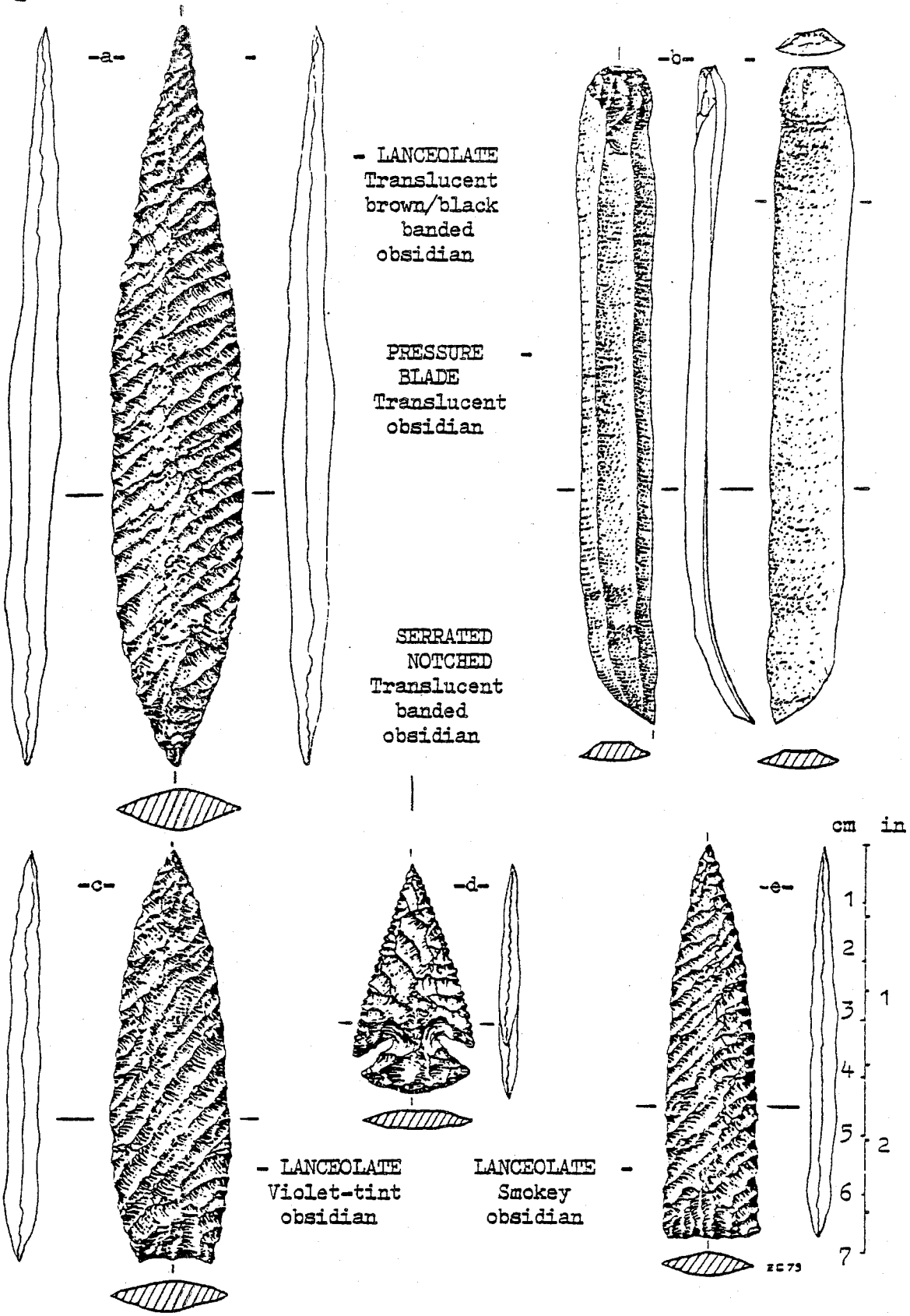
And then there is another thing which I don't understand. They were doing the same thing around the platform part of the core, removing curved portions all the way around the top of the core. You can see the blade starting along the sides. They would make a notch for the platform and peel this off and that would be their pointed end which could serve no purpose. It looks as though it was done by pressure. Does that appear someplace else. Why did it appear? There are so many unanswered questions still to go on and ultimately they will have to be resolved.

[*END OF PART I. In the next issue Don discusses technique--plus a Crabtree bibliography.*]

Flintknappers' Exchange 2(1):1979

REPLICAS BY DON CRABTREE

Figure 2



Don Crabtree sent these alternative "solutions" to my "problem": another term besides "Knap-in" for our get-togethers.

ALTERNATE "KNAP-IN" TERMS

KNAPPIN (over slept)
Somnambulist Stand Up
The Van Winkels
Cutups Convention
The Guitarless Rock Festival
Chipor and Chipee Conclave
Rock Knockers Revival
Fracture Fiesta
Crackers Commune
Bloodletting Ritual
Scarification Ceremony
Red Cross Participation Welcome
Fissure Frolic
Crack Analysis
Rock Rupture Revel
For Stoned Only
A Get with It Flakey Sit In
Chips and Dips Get Together
Formal Disport on Percussion and Pressure
Informal Bash on Hitten and Pushin
And finally a Knappin among friends can be an intellectual feast with no moral implications.

I don't know, Don. I think it's back to "Knap-in."

Jackie Nichols

* *

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☐ I plan to attend the regional knap-in in Casper, Wyoming on April 7, 1979
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