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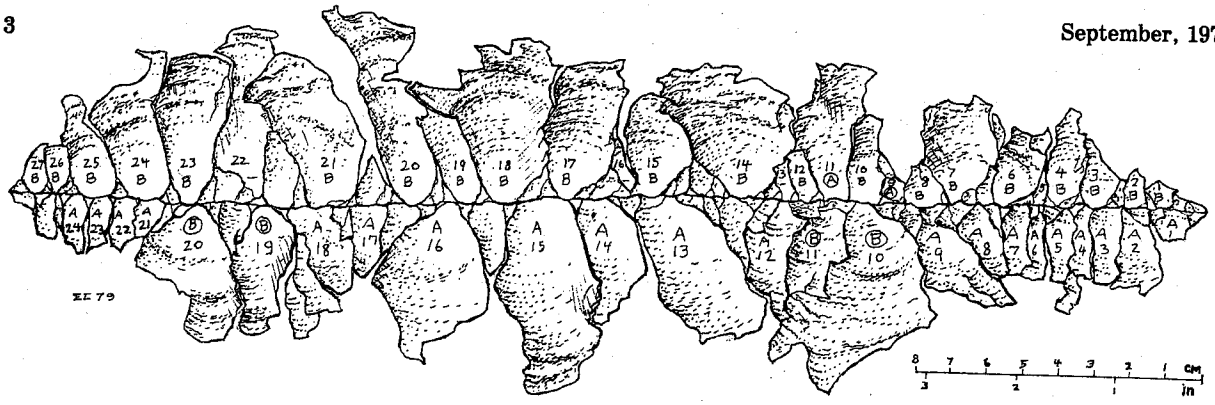

FLINTKNAPPERS' EXCHANGE

AN EXCHANGE MEDIUM OF, BY,
AND FOR LITHIC TECHNOLOGISTS

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Flintknappers' Exchange
1701 Catron S.E.
Albuquerque, N.M. 87123

FE is published three times a year (Feb., May, Sept.) as an informal 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 Editor Nichols.

- EDITORS -

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The Illustrations: Cover: both faces of one margin of one longitudinal half of a Solutrean laurel leaf #2 from Volgu, France. "Craftsman" illustrations are by Errett Callahan; "The Denver Series" illustrations are by Bob Patten; others are submitted by authors, as noted.

I'd like to thank everyone who wrote in to complain about the new format. First of all, the last issue was not shorter. We went to printing in order to expand--postage costs were killing us, and we could not add so much as a page. The last issue was about the usual length, but we will be expanding.

Secondly, we had to be able to use photographs. We intend to improve our photos as time goes on, and we encourage authors to submit photos plus negatives.

Finally, *FE* will continue to change. Next year Bob Patten will add to "The Denver Series" only periodically. He plans to try something new. The "Problems/Solutions" section will, with this issue, become a general research papers and replies section. Archeologists don't seem to have any specific problems. On the rare occasion we have someone confess to a problem, it will appear in the section with stars and exclamation points. I want to thank everyone who responded to my staging problem, and I will appreciate any responses to the "problem" I pose in this issue (and I will print the responses, of course), but I do not want this section to become a supra-editorial.

A last note to the reader who accused us of getting "glamorous." I have decided to take that as a compliment. Perhaps you never received one of my 10-staple specials, where I tried desperately to staple 36 pages doubled over and failed, and failed again, and again. . . . Those readers with rips and holes in their old issues doubtless appreciate the new format, and I retired my stapler with relief.

JACQUELINE NICHOLS

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NOTE:

Our publisher, the Great Basin Foundation, has asked us to announce its new publication CONTRACTS ABSTRACTS. As the name implies, this is a journal for contract archeologists. Like *F.E.*, it will be an exchange medium: there is so much contract work going on, what is everybody doing? Although most reports--running to an excessive number of pages in most cases--are available with a little persuasion from the various contracting agencies, there is a need for this information to be made available in the form of concise research reports, summaries or abstracts to other professional archeologists.

CONTRACTS ABSTRACTS will require certain categories of information to be included in all reports, and will publish ongoing summaries of this information in the form of regional maps. CONTRACT ABSTRACTS will (like *F.E.*) have a letters section, and include replies and exchanges. Crediting formats must be strictly observed.

Prospective authors should request a copy of CONTRACT ABSTRACTS guidelines from:

Penelope Katson, GBF Publications Managing Editor
4426 Constitution N.E.
Albuquerque, N.M. 87110

Those interested in subscribing should write to Penny at the same address.

In view of the increased activity of some of our commercial flintknappers (*FE* 2(2): 3 and this issue) I would like to put in a few cents' worth of personal opinion on their behalf. For too long the archeologist and academic flintworker has been given a one-sided perspective of the state of the art. Lucy Lewis Johnson in her "History of Flintknapping Experimentation, 1838-1976" (*Current Anthropology* 1978, 19(2): 337-372), makes it clear that flintknappers have been around for a long time. No sooner had it been discovered that prehistoric stone tools had been made by man and not by lightning than imitations started popping up. "Flint Jack", active in the 1860s, gave us all a bad reputation when he was found to be salting sites and passing off his handaxes as old. Archeologists have ever since tended to ignore the work of the commercial knapper.

With the publication of D.C. Waldorf's *The Art of Flint Knapping* (1979), we can no longer afford to ignore these knappers. Waldorf's book will be making an impact for years to come on the general public via sales through museum shops and similar outlets. Fortunately, in his latest revision, Waldorf has dropped his antagonism toward the archeologist which marred his 1976 edition. We now have a strong defender of professional attitudes within the commercial field. (And we have an excellent textbook for our courses and personal training. See p. 11 for review.)

There is another way of recognizing lithics than that which the academic knapper has presented. The study of variability has for too long left the commercial stone, as it were, unturned. Many commercial knappers have preferred it this way too, by the way. At this point we should distinguish between the commercial knapper such as Waldorf who sells his work in the open, signs his pieces, and does not pass them off as ancient--or Indian (unless he is Indian)--and the underground commercial knapper who does pass his work off as ancient. The latter usually works in non-authentic manner, does not sign his work, and chooses to remain anonymous. The former activity is perfectly legitimate. There is nothing wrong with craftsmen working in their chosen media, be it ceramics, basketry, or flint, as long as it is sold for what it is--contemporary craft.

It is my personal opinion that neither the legit nor the non-legit commercial knapper does archeology any harm. It is not difficult to distinguish the old from the new, despite "aging" techniques, nor is it difficult to distinguish one contemporary knapper's work from another. The harm that is done by passing off one's work as ancient (or allowing others to pass it off because it is not signed) is to the collector, not to the archeologist. There seems to be some justice in there somewhere.

In actual fact, the collection of modern works should help reduce pot hunting, not encourage it. I congratulate Waldorf for his courage in stepping forth, for sharing his cognition with us, for unconsciously becoming a model which other commercial knappers might follow. There is an unwritten brotherhood among flintknappers, a brotherhood that should be encouraged by such lowering of walls. Now let the academic flintknapper, without sticking his nose where it is not wanted, take a step toward lowering the walls even further.

Errett Callahan

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NOTES

That's Les Eyzies, not les Ezies, not Las Ezies, nor anything else!

Errett Callahan is teaching lithics (apologies to Bradley) at the Lejre Center in Denmark this semester. Send all papers etc. to Nichols for the time being.

LETTERS

"WHAT I DID LAST SUMMER"

This summer I worked with the Public Archaeology Survey Team on a pair of National Park Service planning grants. We investigated site distributions in the towns of Glastonbury and Coventry, Connecticut. Among the more interesting finds were numerous rock shelters and some isolated quarries. We also recovered a substantial amount of data which suggest a fairly complex burin technology in Connecticut. We are attempting to generate a model for prehistoric site location by correlating site placement with geophysical features. So far poison ivy appears to be the best indicator species for sites.

Terry A. Del Bene
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* * * * * * * *

I spent June-July working with George Frison on a continuation of excavations at the Agate Basin Site. We dug a sealed Folsom unit that contained large quantities of *in situ* point manufacture materials including a cut and shaped elk antler that we have concluded was used for fluting. Frison and I are currently working on a paper that will describe the antler, our proposed reconstruction of how it may have been used, and descriptions of the results of some experiences with using a modern copy--quite successful result. During the summer I helped butcher another bison (with Frison, Stanford, and Reher). Also participated in the elephant butchering in June, and learned quite a bit about points, foreshafts, and handaxes. Also, I have just completed consultation work on the Chaco project, where I assisted in the formulation of the analysis of the flaked stone artifacts from the monument. Have just completed the final copy editing of a monograph of the analysis of the flaked stone technology of the Hanson Site (manufacture and use), which I have co-authored with George Frison and is being published by the University of New Mexico Press.

Just as an added comment: Will all people writing about flaked stone please take the time to go to any English dictionary and look up the word Lithic? Please note that *it is not a noun* but an adjective! Therefore there are no such things as Lithics!

Bruce A. Bradley
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Oracle, Arizona 85623

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I spent the first half of my summer in the usual way by teaching the Flintknapping Fieldschool. This year's participants were as outstanding as ever and traveled to WSU from all parts of the world: Dr. Pat McCoy, Bishop Museum, Hawaii; Dr. Peter Storck, Royal Ontario Museum, Toronto; Dr. Lars Larsson, Lund University, Sweden; Carol Ebright, SUNY-Binghamton, New York; Ken Binkley, University of Mississippi; and Sue Lewenstein, Arizona State University. Miranda Warburton (WSU) was the field assistant.

The remainder of the summer I spent completing my Ph.D. dissertation and conducting field research with Alan Stanfill, for several consulting jobs concerning various aspects of lithics (aboriginal quarrying processes, lithic reduction systems, evaluation of "lithic scatters," etc.). That's about it for the Lithics Lab at WSU.

Jeff Flenniken
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* * * * * * * *

Received a post card saying that you (Jackie) will be abandoned until December. The scalawag was saying something about teaching a course in Denmark. Could it be a repeat of the old story about the grasshopper fiddling around while we ants stay home and do the work? Well, anyway, I just want to thank you for all the work you put in making *FE* a success. It's been a great thing for me--providing access to the published thoughts of others.

During last spring and early summer 1979, I worked on a project to replicate the adz as reported by Shafer--from Colha-Belize. This adz, which may be an axe, has a cutting edge formed by a single tranchetflow flake, what is known as an orange peel flake. Orange peel flakes have two faces, each of which has the curvature of a semi-flexed recurved bow. The junction of these two faces forms a line having the same recurve. These attributes were best attained by handholding the preform and striking with a small, hafted, medium hard hammerstone.

Ongoing work is the replicating of a South Texas tool which was first termed "Attwater Gauge" and later renamed "Guadalupe Tool," because its function is not certainly known. Like the Colha-Belize Adz, Guadalupe Tools have their bit formed by a single flake removal, but the flaking platform is located at the end of a dorsal face ridge. This tool has also been successfully replicated.

J. B. Sollberger
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Dallas, TX 75218

* * * * * * * *

Since our great get-together in Casper it seems the summer has gone by and as usual what you wanted to accomplish was only half realized.

Have been working on a few projects--trying to replicate the Dorset point which is a real challenge--maybe some day! Don Crabtree and I have been working on the "Eden" point and his success, I must say, has been far superior to mine. My holding technique doesn't seem to lend itself to this type of flaking. Have to practice more on the holding method used by the rest of you rock knockers.

The "Folsom" point--what a challenge! I have been working on this point for 20 years trying to bring my success ratio up to a reasonable level and every time I think I have it coming my way, old mother stone says, "you're not as smart as a lot of people I knew 10,000 years ago." Anyone selling time machines?

Have one more project I am working on. Jim Woods, the director of the museum at the College of Southern Idaho, found that there was a need for some type of teaching aid in our elementary schools on the subject of "The Indians of Idaho." He is preparing a traveling instructional display board showing as much as possible of the different Indian cultures of Idaho starting from Early Man up to the historic period. I was asked to replicate many of the stone tool types for each period that will be used in conjunction with the display. Haven't quite completed this project yet--had to do a little research and practice a bunch--particularly on the "Haskett" point.

Will be anxiously awaiting the next issue.

Gene L. Titmus
Route 3
Jerome, Idaho 83338

* * * * * * * *

So good to hear from you! This has been a very busy, exciting, and rewarding summer. I would suppose number one on the list was where I met you in Casper and our most interesting symposium as well as meeting the members of the Wyoming Archaeological Society and the discussions of flintworking with George Frison. I feel it was a very worthwhile experience and it gave you an opportunity to meet Gene Titmus and see his craftsmanship. So, now that you have a person rather than just a name, hopefully we can have more of these sessions. I feel everybody gained more knowledge of stoneworking through the exchanges of information as well as learning the sources of some of the beautiful and exotic materials from Wyoming.

In May I had a little stint at Pullman with Jeff Flenniken's students and again my thanks to Dr. Richard Daugherty for his co-operation to arrange a laboratory on primitive technology on a permanent basis. I feel this is a great step forward in the study of the prehistoric technologies.

After the completion of our sessions we went from Pullman to Vancouver, B.C., for the Society for American Archaeology meetings. This was a great opportunity for me to apologize for not answering letters for the past several years and to keep current with new approaches and activities.

A great experience in my lifetime took place on May 19th, at the graduation exercises in Moscow, Idaho, where I received a doctorate of science. This was mainly due to the endeavors of Dr. Ruthann Knudson and Dr. H. Marie Wormington. It was indeed a great tribute. I will take this opportunity to try to show my appreciation and thanks for the magnificent serigraph by Mazonowicz - "Standing Bison, Altamira Cave, Spain." I shall always cherish this, and it will bring fond memories of my many friends who have been so gracious.

In June, I attended Jeff Flenniken's flintknapping symposium out of Colfax, Washington, and it was a great experience for me. I was only there a week, unfortunately, but the participants were very knowledgeable and had so much information. It is certainly great to see the amount of enthusiasm generated at these field schools mainly due to Flenniken's ability and good humor. I think we are indeed fortunate to have such a laboratory set up as Flenniken has arranged. Each year it is improving and he now has most every facility necessary for the pursuit of technologies and equipment, as well as a laboratory of materials for study and comparison.

Another very exciting and rewarding event took place at the University of Lethbridge at Alberta, Canada. A third summer session was chaired by Dr. Terry Moore on lithic technology, and instead of having a six-week course it was concentrated into five days of intensive training by the use of audio-visual portrayal of various stoneworking techniques by using a series of films, slides, and closed-circuit television with two color monitors and one black and white for the demonstration. The color monitors were for eighteen participants. Practice sessions were held in the evening as well as during the day. Everyone had an opportunity to participate in the fracturing of brittle materials.

A fairly complete coverage was given on intercontinental technologies. A study was made of the Eichenberger casts showing different techniques in preparing the Paleoindian artifacts. To me, this was a great step forward for manual manipulative skills particularly of the cameraman, Landy Esau, who was most skilled in the use of the zoom television camera. He was able to point out many details by zooming in on the problems discussed. A more formal report of this sesion will be published by Dr. Terry More. His organization and facilities were all that could be desired. It was a really most enjoyable experience. In our five-day session the results were most gratifying and the enthusiasm of the participants couldn't have been better.

In the meantime, Gene Titmus and I have been working on numerous other techniques with our continued interest in stoneworking. Two problems that are now being resolved are the replication of the Eden style flaking with the flake terminating at the midline coming from both margins and both faces. Too, another experiment in progress is the removal of large flat plates of material for the reduction of cobbles and nodules as well as the preparation of polyhedral core tops and their rejuvenation. It is similar to the Setuchi technique as described by Tsegge Matsuzawa (personal communication). This was first discovered at Lethbridge to efficiently utilize our limited supply of material. It needs considerably more work before it can be described in detail.

I have been working with John Clark of the New World Archaeological Foundation working in Chiapas, Mexico. He has developed a new insight into producing prismatic blades from a polyhedral core. His experiments should be of considerable interest to those interested in blademaking. He, by the way, brought me a large sample of arrows from the Lacondone Indians from southern Yucatan that are still being tipped with stone flakes or blades. They are some of the last people in the world still using stone projectile points.

My compliments to you and Errett Callahan for a most informative and excellent publication. I am sure we will all look forward to receiving our next copies.

Don Crabtree
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Kimberly, Idaho 83341

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OTHER LETTERS

It is certainly admirable that someone is at last recognizing the art and skill of lithic technology. We self-taught "arrow head" makers have had a difficult time over the years.

J. F.Carter
Rt. 2 - Box 152
Monroeville, Ala. 36460

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I waited with bated breath for my first copy of *Flintknappers' Exchange* hoping it would enlighten me further in my flintknapping endeavors; however, it has failed to do so. . . . So far people seem to talk more about finished products or beat around the bush in a lot of areas without getting specific. Either I'm much better than the majority or the various people don't know how to expound on techniques or are reluctant to do so. I have yet to meet a better flintknapper than myself and yet I learn from the newcomers in this field. Let's get back to basics, advanced techniques and the arbitrary.

One individual who is a professional (?) flintknapper denounced the usage of slabs which irritated me. A good flintknapper can handle slabs or the old method and just for purism's sake, I don't believe or feel that we should waste the superb material. A rock broken by percussion can yield 2-5 good points but will make 10-15 if cut on a diamond saw. Many people do not have ready access to vast quantities of good material; therefore, people should exercise prudence in their criticism and material usage.

A Reader

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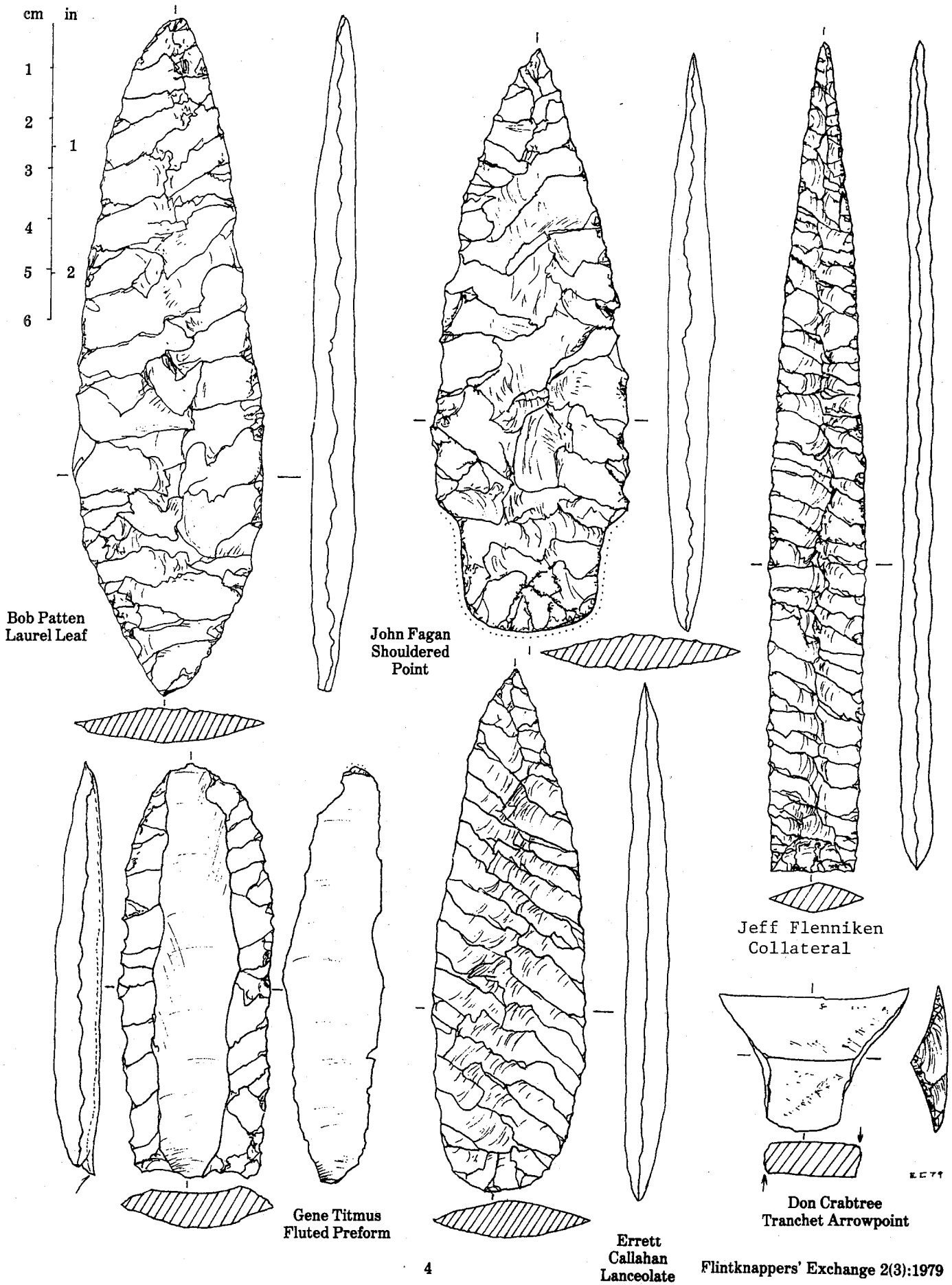
Figure 1. Replicas Made at Wyoming Knap-in.



KNAP-IN NOTES

This sampler of work done at the Wyoming Knap-in didn't quite make the last issue (Fig. 1). However, it gives me an excellent excuse to announce a knap-in for spring, 1980. This will not be the only knap-in to be held; we are delighted that they seem to be going on everywhere, and we appreciate the opportunity to announce them and report on their results. However, this editor can really only get actively involved in the planning of one per year. Even then, without wonderful hosts like the Wyoming Archeological Society, such events are impossible.

The hosts for this knap-in are the Little Lake Duck Club and the Great Basin Foundation. It will be held April 19-20, 1980 on the grounds of the Duck Club at Little Lake, California. All *FE* readers are invited, but most particularly those in the Far West. Southwestern readers might also like to consider this knap-in.

At the last knap-in, everyone agreed that some formal experimentation should be part of the next meeting. Therefore, will readers please send in ideas for experiments suitable to a knap-in? These may be formally or informally presented. The most interesting of these will be published, and a couple of free knap-in dinners will be awarded. The best experiment will also be performed at the knap-in.

Don't say I never learn from experience: this is--by anyone's standards--plenty of notice. Furthermore, the event will be two days this time. Little Lake is one of the most beautiful places I've ever worked, or, for that matter, ever been. Susan Schroeder, the GBF public relations director, has planned a great meal for Saturday night and has compiled the motel lists, maps, etc. The last I saw of her, she was designing Little Lake Knap-in t-shirts. It looks like a great get-together!

If you already know you're going to attend, send for information to:

Susan Schroeder, GBF Public Relations Director
1704 Catron Pl. S.E.
Albuquerque, N.M. 87123

Save the address if you're undecided, because we must *know* if you're attending this one.

JACKIE NICHOLS

experimentation

"TWO-MAN FLAKING TECHNIQUE"

In Errett Callahan's interview with Don Crabtree (*Flintknappers' Exchange*, May 1979) Don mentions his desire to further explore two man knapping techniques, concluding that "hopefully in the future we will get a few more of these done" (*F E* Vol. 2, No. 2: 10).

With this idea in mind, I would like to report on the two-man technique that I am currently working on. This technique isn't necessarily unique, but rather is one of those knapping techniques where you *put two knapping techniques together and come up with a third technique.*

The technique that I am using, and adding to, is hand held pressure flaking. To be more specific, I grip the biface in the left hand, *hold it down tightly against the thigh,* while the right hand uses the full weight of the body from the shoulder to bear down on the flaking tool. Now, to this I add a little more force by using a second person to deliver a *light tapping blow* to the end of the pressure flaker with a mallet, just at the instant that the pressure flake is pressed off. (Fig. 2) In other words, pressure flaking plus indirect percussion equals a third (two-man) technique that adds more force to pressure flaking than could normally be achieved.

PRESSURE + INDIRECT PERCUSSION = NEW HYBRID TECHNIQUE

In my actual "set-up", I used a copper tipped pressure flaker with a wooden handle which extends eight inches beyond my shoulder after being tucked under my right arm like a knight's lance. This gave my assistant, Russ Gloege, enough room to hit the end of the pole with a *sideways glancing blow* at the moment that I had loaded all the pressure I could onto the pressure flaker. Call signals were used (one, two, three,*hit)* in order to coordinate my "loading" of the pressure force with the delivery of the blow. Russ and I found that if he used a sideways blow, I would not only receive added force to my pressure flaking, but that the tip would jump sideways, away from the stone, flicking out the pressure flake as it was detached. "Flicking the flake out" produces a larger flake with less force in pressure flaking (as well as in certain types of percussion flaking) and is absolutely essential to our two-man technique.

Although Russ and I do not have enough hours of experience with this new two-man technique to know exactly what can be done with it, I would like to report on the success of our initial experimentation. First, I used soft hammer percussion to prepare a number of percussion preforms of obsidian, straightening the edges and sanding them to increase platform strength. Within a short time, we were coordinated enough to remove skimming flakes, which to my delight were *many times larger* than I could have made by pressure flaking. The flake scars we produced look like large percussion flake scars and are fairly smooth in texture, like soft hammer work (Fig. 2). Unlike soft hammer flake scars, there is usually a somewhat pronounced ball of percussion at the exact point where the flake was detached. The most significant fact about these flake scars is that, like "punch work", they can be placed exactly where one wants them and made to go in the direction that one desires. The exact length of the flakes, like in other techniques, is somewhat difficult to control. Hopefully when this two-man technique is fully developed it will allow lithic reduction with accurate positioning of flakes.

Since it was apparent that this new flaking technique could produce large and directed flake scars, I could not resist the temptation to attempt fluting. No vise-like clamp or wooden block for front end support (to avoid end shock) was necessary for fluting with our technique. I held the biface against my thigh, orienting the nipple on the base to line up with the pressure flaker, applying all of the force I could to the pressure tool, and then had Russ hit the end of the stick with that sideways glancing blow to detach the flake. Much to my delight, large flutes were detached, and we did not have any end shock breakage even though the front end was not supported. Apparently the light sideways tap from the mallet is not violent enough for end shock to be a problem.

One unexpected problem with the fluting did arise, however, and I would like to use this opportunity to seek advice from other knappers on what to do about it. Several of my flutes did not fully detach; i.e. the channel flute flake would break in two, leaving the forward portion attached but easily pried free afterwards. This produced a channel flake scar with a central ridge dividing it in half, rather as if the channel stopped halfway down and then started up again (Fig. 3). In another case (not pictured) the channel flake detached normally in one piece, but the channel flake still had to be pried free after fluting. I suspect that it may be related to the hand grip, because the channel develops at the point where my fingers are pressed across the face of the rock with the greatest force. Perhaps if a new hand grip were found whereby only the margins are gripped, the channel flake would fall free as desired. Perhaps there is a similarity between my fluting 'problem' and the Cumberland fluted point artifacts described by Don Crabtree (last issue.). He noted that the flutes terminate with a step-fracture just before reaching the tip. Don, do you think that possibly the manufacturers of those Cumberland points used a grasping technique similar to mine (i.e. focused intense pressure on the tip to 'stop' the flute?)

Criticisms, questions, and suggestions on the foregoing article are most welcome, as well as reports from anyone who

would like to experiment with this two-man technique. Are there any suggestions as to what I might use instead of a copper-tipped tool? That blow would put a great deal of force on an antler tip!

I would especially like to thank Janet Eidsness and Darcy Ike of San Diego for making the photographs included in this article, and Dan Griffin of San Bernardino for sharing the special coating process technique he developed for photographing lithic artifacts.

Rod Reiner
884 Bonsall
San Diego, Calif. 92114

Figure 2.

Arrows indicate flake scars produced by two-man technique.

Scale: 2/3 actual size
Material: Obsidian

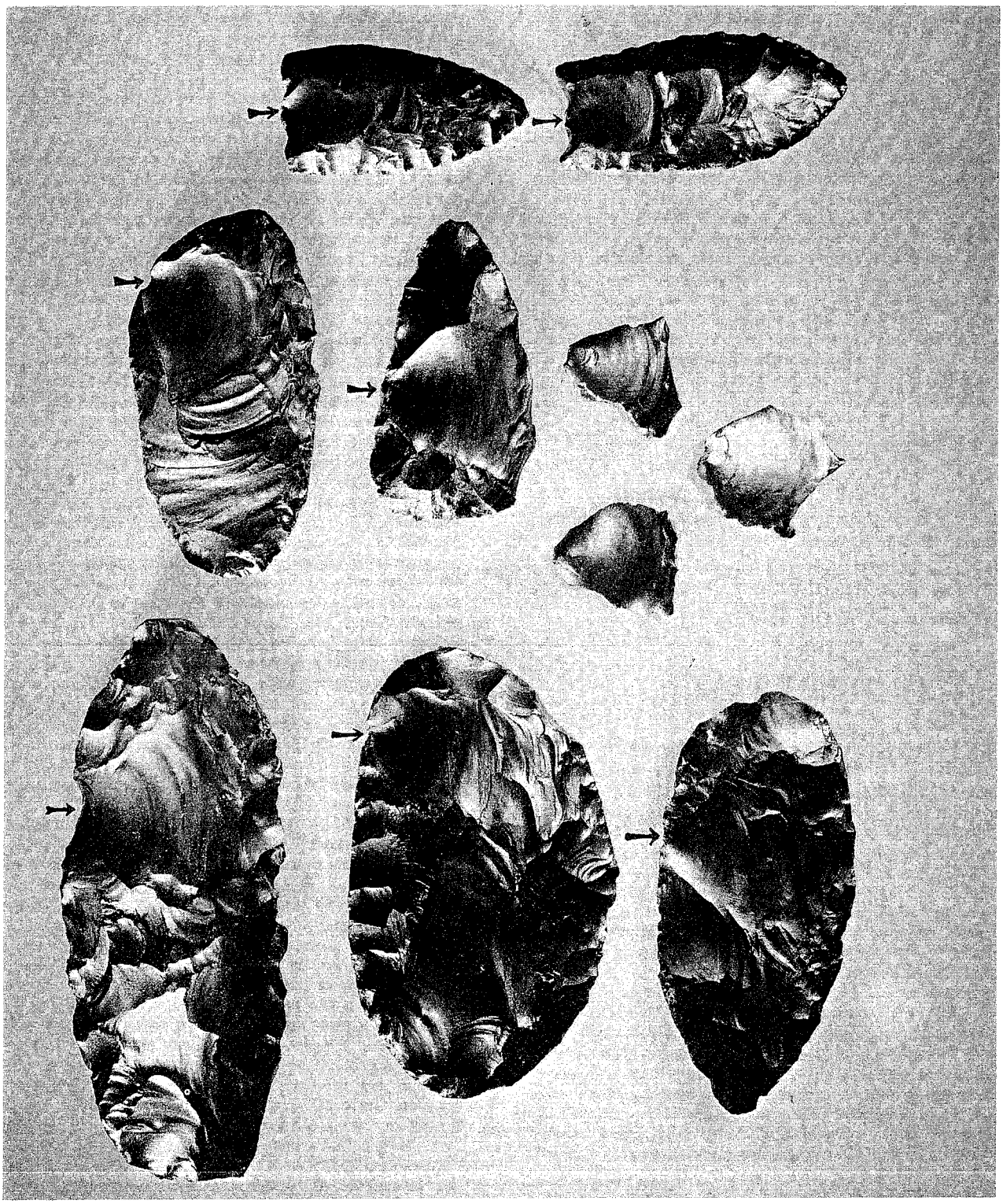


Figure 2.

Rod Reiner (seated) and Darcy Ike demonstrating two-man technique.

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COMMENTS ON ABRASION OF STRIKING PLATFORM EDGES

In considering the proper methods to use in making bifaces, the importance of striking platform preparation should be emphasized. This usually entails removal of small flakes from the core edge, by pressure and/or percussion, to obtain a uniform edge, having the desired striking platform angle. The striking platform angle is one of the key variables in controlling lengths of flakes removed. Uniform striking platform edges are important to allow uniform force application, to prevent premature small fractures and other uncontrolled fracture events. Feathered sharp edges and small protuberances are to be avoided. As a final step before force application to remove a large flake, the striking platform edge is usually ground with an abrading tool. I would like to comment on the effects of this last step, using abrasion.

A number of people seem to feel that grinding simply strengthens the striking platform edge, to allow full force application before fracture occurs. J.B. Sollberger has told me that as one effect he feels that grinding actually weakens the striking platform surface, analogous to use of a glasscutter to scratch the surface before controlled breaking. I think that the above ideas all have an element of truth, and should be combined. Grinding of a striking platform edge, and surface near the edge, certainly does make the edge more uniform. This in turn allows more uniform force application to the striking platform surface, while minimizing undesired localized fracturing and shatter. However, abrasion is an act of damaging a surface. There is little doubt that surface damage facilitates fracturing. I have personally observed in my experiments that ground edges fracture easier when using direct percussion. I conclude that grinding of striking platform edges, and surfaces near the edges, serves the dual function of creating more uniform edges to allow more controlled force application, *and* causing some surface damage to allow fracture with less force application. Surfaces roughened by grinding also aid in reducing slippage of the percussor on the surface being struck, which then permits more efficient application of force. J.B. Sollberger (personal communication) also feels that one of the main effects of grinding striking platform edges is to increase the area of flaking tool contact.

Research is continuing on the fracture properties of stone and ceramics. The American Society of Testing Materials (ASTM) has a subcommittee E24.07 on fracture toughness of brittle nonmetallic materials. In their recent report (S.W. Freiman, From Stone to Ceramics, Glass to Concrete. ASTM Standardization News, April 1979, pp. 25-28), the following statements show the importance of surface damage to the ease of fracture of nonmetalic brittle materials:

1. Because of their extremely brittle nature, the fracture toughness of glasses, ceramics, rocks and concrete is one to two orders of magnitude less than that of metals. These low values of plane-strain fracture toughness lead to a sensitivity to very small flaws (typically of 10 to 50 micrometres), which can occur at the surface due to machining or handling.
2. Because critical flaw sizes in brittle materials are so small, they can in many cases be the same order of magnitude as grain sizes.

Research on the fracture of brittle nonmetallic materials is still an ongoing, developing subject. As theory becomes more developed, it is likely that experimental flintknappers will find physical explanations for empirical procedures that they have been using for some time.

L.W. Patterson
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KNAPPING TIPS

If one observes a flame on the end of a match stick, one will note an orange outer part and a blue inner flame. The inner part suggests a flute, the other portion the point. It is my theory that fluting originated out of the worship and respect for fire. You will also notice "tangs" on the bottom of the flame.

Step fluting is the manner in which Cumberland points (except the one at U.N.M. which was achieved by clamp and anvil) were fluted. Points of any size can be fluted by this method. The Sandia and some Folsom were also fluted in the same manner. The Lindemeier was, however, pressure fluted, as is obvious upon observation.

Step fluting will terminate the flute at the tip in a step fracture which does not break the fragile tip. Take a tapping hammer and antler punch. Place the tip of a point to be fluted on the edge of a stone and place a piece of leather over it. Place the outside of the left foot (or inside of left foot for Cumberland flutes) firmly on top of the leather and tip. Immobilize preform with foot, seat punch and flute in an *upward* direction using Crabtree's platforming method.

The Notched Flaker

Take an antler and cut a notch in it ¼ inch wide and 1 inch long or so. Grasp the flakes in one hand and the preform in another. By sitting down on a low seat, the hands can be placed inside each thigh. With both legs and hands pushing toward one another a great amount of force can be generated. Much more pressure can be created than by using Don Crabtree's "shearing" method.

Place the tool against the edge allowing the stone to cut into the antler somewhat. This "grabs" the edge. Push hard and give the tool a slight upward twist when the desired amount of pressure is attained.

Flake length and flake direction are relatively easy to control. A reasonable flute can be pushed off by this method also. Since the tool rests on the preform, slippage is not a factor.

The pointed flaker can be used for some notching and retouch and can be held like a knife when using pressure with wood anvil. When using the twist flaker, be sure not to let the antler get underneath the edge but dig into it instead. The edge can be abraded slightly, marginally reduced or platformed before flaking. Around the tip it is usually advisable to use the pointed notching tool. (See the *Ancient Art of Making Arrowheads* by Leonard Haslag.)

A point can be finished up with the finish flaker. Grind an antler flat on one side. Isolate platforms. Hold flaker in one hand and preform in the other. Push down and press outward to the left. The tiny so-called retouch flakes on the Lindenmeier Folsom were made by this method. Otherwise it is impossible to get so many flakes to the inch. Fluting was accomplished after the so-called "retouch" flakes were made, not before. Also look how the flaking is circular *around* the Lindenmeier instead of just diagonal parallel flakes.

An Ishi stick can be hafted to the arm for much greater pressure.

The preferred knapping position with a billet is with left leg up on a couch or something and right leg on floor. Billet toward yourself with piece being knapped held vertical, not lying flat on the leg.

Inner tubes from old tires make good alternatives for leather padding.

This article is the combined work of:

Raouel Ibarra and **John Wellman**
Instructor, Primitive Stoneworking
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P.S. I am convinced Bruce Bradley's technique of using a bent twig in forming Eden points is absolutely correct. JW

* * * * * * * *

Just received and read *FE*, Vol. 2, No. 2. Craftsman Don Crabtree Interview brought out some interesting questions such as Alaric Faulkner used 800 lbs. force to do what Crabtree does with one-tenth that amount of force. Do it the easy way, Don says. So say I.

A Pressure Flaking Tool that Allows Independent Control Between the In, and Out, Forces.

According to Crabtree (*FE*, Vol. 2, No. 2) the "out" element of flaking force is necessary to reducing the required "in" primary force, in pressure flaking. We are saying that much energy and control is lost just to instigate the fracture. In practice we do not always judge the correct amount and force angle because all platforms do not have identical strength. Consequently, we often get a flake that is shorter or longer than was planned. The problem's solution then, is to separate the two forces and make each one independent of the other. Then, the knapper can apply a more nearly perfect primary force initially, and the necessary outward force for fracture initiation can be applied without compromising the intensity or direction of the primary force. The results then are more massive pressure flakes if you want them; parallel (to each other) horizontal or oblique flakes: Platform and/or flake either face alternately or serially with one holding position. That is, you can flake both faces from one lineal edge without changing the preform orientation (Fig. 4).

The tool drawn on Fig. 4. A, lets you use the superior copper whereas, B is made from deer antler. C, illustrates the special holding. I use a long 3" wide multi folded strip. The upper half of the preform has both faces exposed from one lineal edge. The preform parallel's is the thumb while the fingers immobilize the leather and preform. The backside of the holding hand is supported near your knee or between your knees. You see equal amount of both preform faces.

Place the short prong on the flaking platform, and the long prong,2 nearest to you on the face to be flaked. Apply the primary force for the flake scar desired, then rotate the tool as shown to release the fracture. If you wish to flake the other face, place the long prong 2 on that face and rotate the tool in that direction. This tool does a fine job for isolating platforms, beveling edges for serial platforming, and accurately replicates the fine, serrated edge beveling as seen on Dalton Type Points.

Will others please be in on the search that this tool may be a prehistoric artifact?

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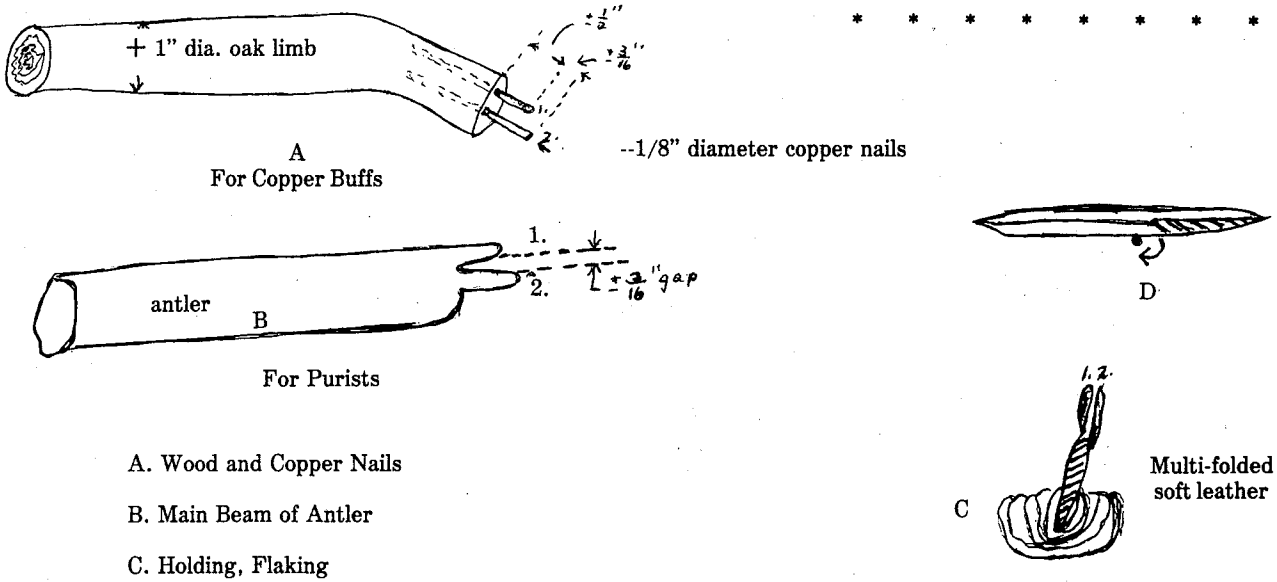
* * * * * * * * *



A. Wood and Copper Nails

B. Main Beam of Antler

C. Holding, Flaking

D. Prongs 1, 2, in Flaking Position

Figure 4. Pronged Pressure Flakers.

THREE GOOD BOOKS

[Book Reviews]

We are not introducing book reviews as a regular feature of *FE*. These publications simply happen to be of unusual interest to our readers. It may be taken as a given fact that we consider them to be of unusual merit as well.

Archaeological Studies of Mesoamerican Obsidian. Thomas R. Hester, ed. $9.95. 210 pp., illustrations, bibliography, paper covers. 1978. Order from Ballena Press, P.O. Box 1366, Socorro, New Mexico 87801.

The idea behind this splendid book, according to the preface, "was to reprint, and assemble in one place, a series of papers dealing with studies of obsidian technology in Ancient Mesoamerica" (p. iii).

The book is divided into five sections: Mines and Quarries, Working Obsidian: Ethnohistory and Replication, Technological Analysis of Obsidian Artifacts, Trace Element Analyses of Mesoamerican Obsidian, and a Bibliography of Mesoamerican Obsidian Studies.

Mesoamerican archeologists will welcome the two latter sections, and replicators will find the three papers on working obsidian useful. Included is the well-known experiment, "Pressure Blades and Total Cutting Edge," by Payson Sheets and Guy Muto, originally printed in *Science* in 1972.

Some *FE* readers will enjoy, as I did, the section on Mines and Quarries. These four papers are accounts of early visits to various mines and quarries, and observations by these travelers are accompanied by interested and interesting speculations.

Otto Stoll, visiting El Chayal, in 1886, says:

> I had always wondered where the ancient Indians had obtained the immense masses of obsidian that they needed for the production of their easily-breakable arrowheads and the swordlike weapons whose edges were set with rows of sharpened and cut obsidian pieces. I had now found one such place, where a vast quantity of obsidian lumps, the largest of which were almost a foot in diameter, lay easily accessible together. They were perhaps brought together in this little hollow during many rainy seasons by a creek that flowed here, or perhaps loose volcanic material spat up from the obsidian-producing layers of the earth had landed here.
>
> Even today, one not infrequently finds here and there the oblong stone cores from which were struck the thin obsidian plates from which, in turn, arrowheads were manufactured. These are prismatic solids 7 to 14 centimeters long on one end of which is a flat surface perpendicular to the length, while on the other end the rounded stump of a point runs out. The sides of the prisms are taken up by twelve or more somewhat concave longitudinal panels of varying width separated by edges running lengthwise. The manner and means of which the obsidian plates were derived from these stone prisms is easy to imitate experimentally. If one sets such a core on its stumpy point and places a sharp-edged object, such as an iron plate, on the upper end surface so that the plane of the object is parallel to the plane of one of the sides, it is possible to strike off a thin obsidian plate the same length as the core with only a few elastic strokes of a hammer (p. 2).

Adela Breton, visiting Magdalena in 1902, writes:

> Some three miles from Teuchitlan, on another spur of the ridge, the obsidian cropping out along the top, has been worked, and the heaps of rejects extend for a mile. Some of the flakes are covered with a thick white crust. Obsidian takes a long time to weather, and the lance-heads at Tulancingo are as fresh as if made yesterday, so that where the volcanic glass has materially weathered, a prolonged period must have elapsed.
>
> The town of Etzatlan, about 20 miles beyond Teuchitlan, is a station on the railway to San Marcos, and from it the Island in the Lake of Magdalena can be visited. This is in some respects the most remarkable of the obsidian workings which I have seen, as it appears to have been a manufactory of the many-sided objects hitherto, called cores. There are no pits, but lumps of obsidian occur on the surface, and these objects are strewn over the ground in quantities. In an hour or two my servant collected so many that I brought away thirty-one, and only left the others as too heavy to carry.
>
> Now, in not one of the other workings, among the very many thousands of pieces of all shapes which I have handled, was there one of these "cores." I have found them on the temple sites in other parts of Mexico, and at Teotihuacan and Mitla they have been numerous, but their marked absence from the extremely varied heaps of rejects I have mentioned (especially at Teuchitlan,) their presence in burial deposits, as at the mount at Guadalupe near Etzatlan, and this enormous quantity, apparently rejects, at Magdalena, seem to make a reconsideration of their name desirable. That they were originally developed from real cores is most probable, the Mexican mind being peculiarly ingenious in finding uses for things which other people would throw away (pp. 6-7).

W.H. Holmes, visiting Hidalgo, 1900:

> The industry must have been conducted for long periods, as extensive areas are covered with these deposits of pure black ringing flakes and fragments. One great heap which lies upon the mountain slope is over forty feet in vertical extent and many feet in depth, comprising perhaps 20,000 or 30,000 cubic feet of flakage. No headway could be made, however, for there was no earth to hold the flakes together, consequently the holes dug were immediately filled by the sliding, tinkling slivers of glass, every piece of which is as clean and incisive of edge as when struck off by the workmen hundreds of years ago (pp.11).

It is the section of technological analyses, however, which will be of most interest to our readers. Included are analyses of artifacts from Tres Zapotes, Papalhuapa, Mayapan, Villa Morelos and Chulchuapa. The latter analysis is Payon Sheets' "A Model of Mesoamerican Obsidian Technology Based on Preclassic Workshop Debris in El Salvador."

The photographs and drawings in this section (of obsidian blade cores primarily) are alone worth the price of the book. Our poor reproduction (figure 5,) should not be taken as representative. Monographs from Ballena Press are always visually outstanding and this is no exception.

The papers in this section, taken together, provide a full description of blade-core technology; insofar as it has been worked out to date. I had many questions answered (for example, on platform rejuvenation; see Irwin Rovner's article, p. 126; also Figure 6. Useful attribute lists are provided in two papers by editor Thomas R. Hester. The first list is the result of analyzing four macrocores:

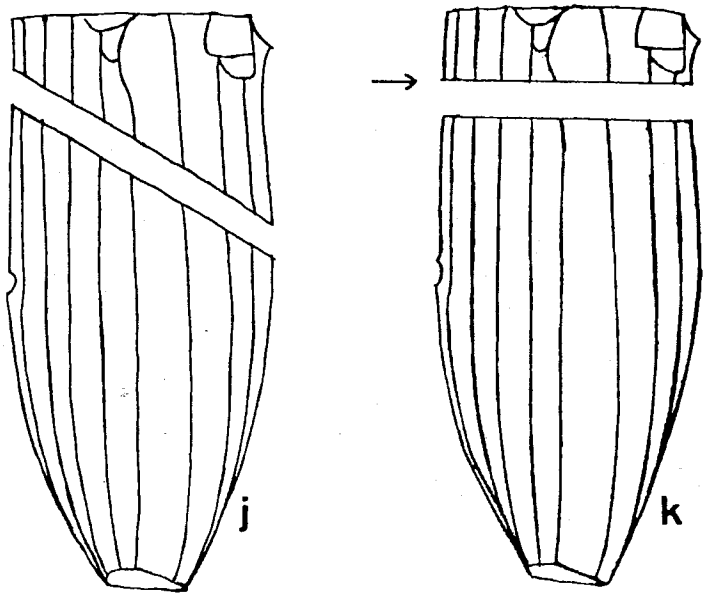
> With this brief review of our Mesoamerican macrocores, it is apparent that these pieces in their initial form share a number of characteristics: (1) they are pyramidal in shape; later in the blade removal process, their shape often changes to cylindrical, conical or bullet-shaped; (2) striking platforms are flat flake surfaces, modified only along the peripheries; the extensive platform alterations (such as truncation, facetting, grinding, and scratching) noted by Hester, Jack, and Heizer (1971) were apparently performed when the cores were greatly reduced in size (there is an area of scratching on the platform of the Papalhuapa specimen); (3) a number of large blades (eight to ten) had been detached; such pieces could

have served as blanks for biface manufacture, as they did at Papalhuapa; (4) the sides of the cores, just below the platform edge, were trimmed in order to remove overhang created by negative bulbs; these trimming activities were necessary throughout the life of the core; (5) the distal end of each shows crushing or battering (p. 103).

In a second paper on Villa Morelos, Hester's list includes platform angles (clustering at 92 -93) and mean number of blade scars of facets (13; range:7-19 facets).

Hester, introducing this section, notes there are archeological (specifically, provenience) problems with some collections analyzed (p. 36). However, *FE* readers will find the technological analyses stand on their own merits. Taken together, the papers, photographs, and drawings are an outstanding catalog of this technology.

JACKIE NICHOLS

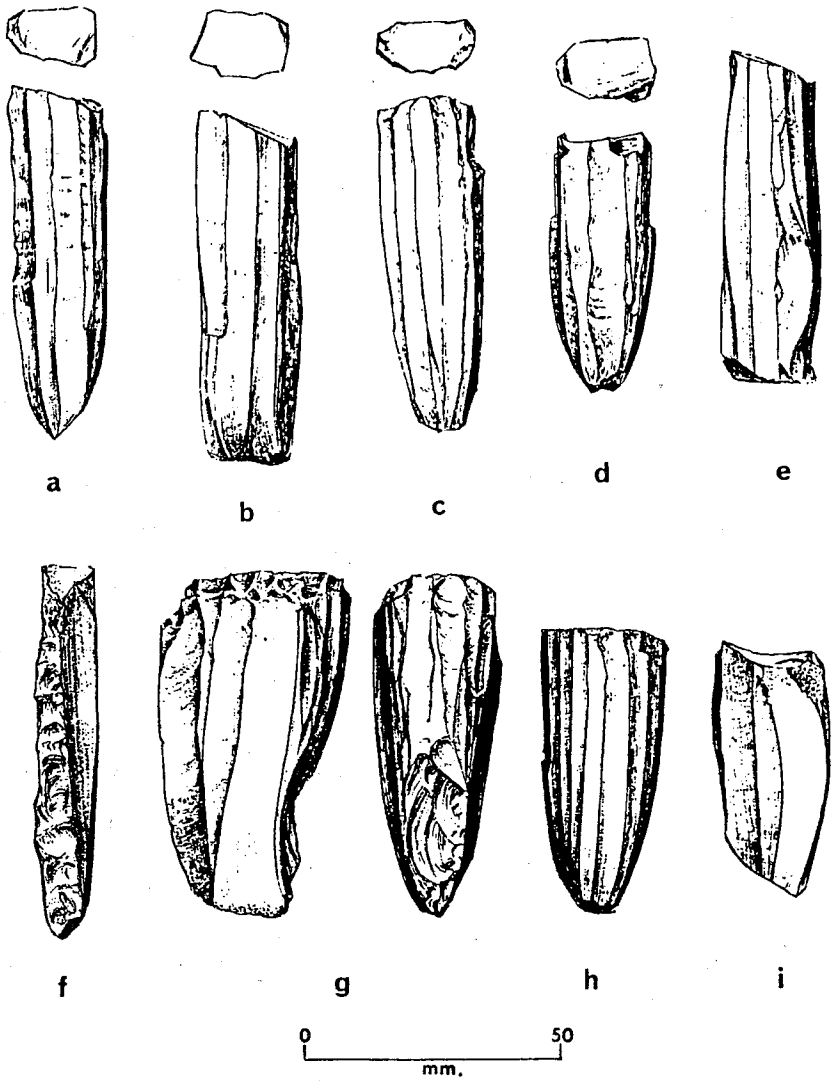


From Thomas R. Hester, ed., *Archaeological Studies in Mesoamerican Obsidian*, p. 86.

Figure 6. Methods of Core Truncation.

From Thomas R. Hester, ed., *Archaeological Studies in Mesoamerican Obsidian*, p.85.

Figure 5.
a-i, Proximally-truncated Cores; e Has Been Bi-trucated.



The Art of Flint Knapping. D. C. Waldorf. The Flint Shop. P.O. Box 702, Branson, Mo. 65616. 1979. 52 pp. illus. $5.00 (paperback, 8½" x 11").

Waldorf is a commercial flintknapper who has taken it upon himself to write a how-to booklet on flintknapping for the common man. It is copiously illustrated with quality black and white and color (front and back cover) photographs and drawings. The illustrations of stone tools, done by Valerie Waldorf, are of fairly high quality and apparently modeled after *FE* standards. There are eight chapters, an introduction, acknowledgment section, and short bibliography. Chapter titles include "Some General Questions on Flint Knapping," "Flint, Flint Sources, and Heat Treating," "Tools of the Trade," "Elementary Flint Knapping," "Billet Flaking," "Finishing," "Cores, Flakes, and Blades," and "Advanced Flint Knapping." This publication is a thorough revision of Waldorf's 1976 booklet of the same title. Subsidiary topics include stages of manufacture and the mechanics of fracture.

After years of reading academic, jargonized literature on lithic replication, I could not believe how refreshing it was to read about my favorite subject in plain, non-academic "folk" talk. I had thought for a while that there was only one way to talk about lithics. Waldorf knows his stuff too (now here *I* go.) He is a very competent knapper. Although his specialty is notched Archaic projectile points, he demonstrates familiarity with fluted points, Old World Paleolithic/Mesolithic technologies, and gunflint replication.

Waldorf is definitely not an amateur writing just another booklet on back yard "arrowhead" making. He works primarily in the traditional manner using antler billets for percussion and antler tines for at least some of his flaking. He does use copper for pressure and nail punches for notching, but he first mastered same with natural materials. He also does rather unconventional pressure flaking, using a table instead of hand-held support. But this is the beauty of those working outside of the Idaho mainstream: idiosyncracies in style can pop up which just might be relevant to the archeological record. Variability has no bounds.

The book is full of practical, personal, and sensible advice. Many of Waldorf's insights and realizations could prove to be universals. His questions and answers section is excellent in this regard. Though Waldorf makes no pretense at being an anthropologist, he is filled with anthropological insights. He is up-to-date enough to take advantage of contemporary thinking and standard terminology without falling into jargon. Though he tries to address himself to the beginner, he actually talks to the knapper of some experience. This apparent failure is a strong point; due to his conversational style, he is intelligible to readers of a wide range of experience. Neither the advanced nor the beginning knapper is cut out or talked down to.

Waldorf rightly criticizes the literature for ignoring holding positions; little on this subject has been attempted since Crabtree's *Tebiwa* papers. Waldorf illustrates his positions somewhat. We need a lot more of this in order to understand variability.

In essence, Waldorf is commercial and proud of it (see editorial). Yet his attitude toward archeology and collecting of prehistoric artifacts is professional and ethically sound. He hopes to reduce the destruction of archeological sites by encouraging the collection of modern, signed replicas, an attitude I applaud. Because of his background and this attitude, we have a whole new perspective on flintknapping available to the archeologist and lithicologist, a perspective we have ignored in the past, a perspective that, in part, has been around a lot longer than has responsible archeology. This is the first time a commercial knapper has stepped forth, revealed his "secrets," and offered his services in a responsible manner. (The book is also relevant to the "survivalist" and black powder buff).

The book has some drawbacks, but they are minor. These vary between a few typographical errors to the misspelling of flintknapping (should be one word), the illustration of some points "upside down," a poor photograph of the author, a musunderstanding of Mousterian/Levallois core technology, an over-emphasis on heat treating, a weak bibliography, and his haste in assigning techniques that work for him to be the sole past reality. Many knappers are guilty of the latter; thus the need for investigating variability before jumping to such conclusions.

The Art of Flint Knapping is the hottest book on the basics of working flint to come along since Ellis (1940). In the intervening 40 years there has not been a single writing that has accomplished this purpose so well. Other writings have but covered facets of the field but none have put it all together so succinctly. It is interesting that this was accomplished by a knapper working outside of the mainstream of contemporary lithic technology. Used in conjunction with Don Crabtree's *Introduction to Flintworking* (1972), and Holmes Ellis' *Flintworking Techniques of the American Indians*, Waldorf should become required reading for every course in flintworking. I strongly recommend it to every flintknapper, beginning or advanced, academic or non-academic. It is a training manual, a reference work, and itself a beautiful object. It should be made readily available in museum shops as well, for the mature attitudes it portrays could do more for educating the general public to the goals of the flintworker than do all of our professional journals combined.

ERRETT CALLAHAN

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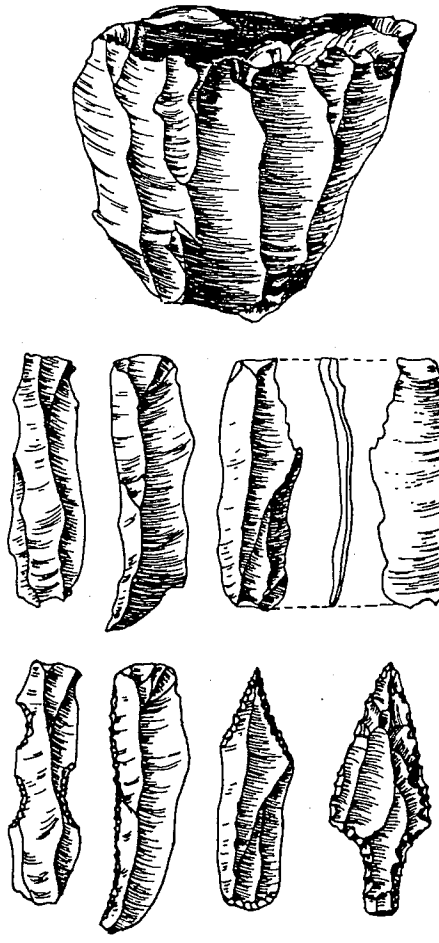
Figure 7. Solutrian Core.



A Solutrian core with blades and tools made from the blades. From left to right a shaft scraper or denticulate blade, a backed knife, combination borer-end scraper, and a Font Robert point.

From The Art of Flint Knapping by D. C. Waldorf, p. 33.

The Basics of Flintknapping in the Eastern Fluted Point Tradition: A Manual for Flintknappers and Analysts. Errett Callahan. Vol. 7, No. 1 (1979). Archaeology of Eastern North America. 170 pp., 75 pp. of drawings. $10. Order from ESAF Business Office, American Indian Archaeological Institute, Box 260, Washington, CT 06793.

The original title of this work was "Variability in the Early Stages of Manufacture of Virginia Fluted Points: An Experimental Study." It was Errett's master's thesis, and I read it twice in that version. I have read it only once since Louis Brennan has edited it for publication. Nothing vital has been lost, and Brennan has done a fine job editing. The text, however, is only a reference. It is the illustrations which make this book extraordinary, in my opinion. (Figure 8).

Errett performed some one thousand experiments, replicating early stages in the manufacture of Clovis points. He, for example, attempted to replicate and then illustrate "every conceivable means of obtaining blanks from which to fabricate Clovis points." In the age of sampling, the concept of an exhaustive set is unusual, but coupled with Errett's meticulous attention to detail, both in replication and illustration, the result can only be compared to some artisan's product of past centuries; perhaps a detailed warrior's shield in gold. I often think, how can he spend so much time on this or that? I've learned Errett works to achieve his own standards. He puts time and thought into things so that *he* feels proud of them. He is the one, for example, who feels badly about typographical errors in *FE*. He strives for perfection.

Of course, we are deep friends and no one would expect me to give his work a bad review. I will say that many will not find information on the Virginia archaeological sites to be of interest. It is, however, unobtrusively embedded in the referent information.

I want to try to make clear what the book is like and so I am going to give an example. This is from the original, so the text may differ slightly. It was Errett's goal that this work could be used as a catalog:

> that archeological specimens may be visually compared with the illustrated replicas in order to describe, analyze or classify those specimens. By perusing the illustrations until one finds the replica most like the specimen in question, and by using the table of contents and/or key codes, one may quickly flip to the corresponding discussion in the text for further elucidation. One may, I hope, thus quickly analyze and classify the problem or unit under consideration. It is hoped that the "catalog" is extensive enough to cover all basic forms of acceptable and rejectable material. Naturally, occasional aberrant specimens will occur which may not be found in these pages. But I hope these will be in the minority.

The book is divided into Errett's working conception of stages of manufacture familiar to *FE* readers. Let's take Figure 9 from "procurement" (Figure 1 in the book). First of all, illustrations of Errett's experimental replications are full scale. They are themselves packed with data.

> I have tried to render the illustrations with enough clarity to allow for the subsequent amassing of potential data concerning the nature of flake scars as correlated with type of percussor, material, lithic grade, etc. The illustrations were made at full-scale (on 8½" x 11" stock) in order that archeological units might be compared with these experimental type specimens without the credibility gap of having to deal with a reduced scale (except where impractical as with the large core materials). It is anticipated that researchers might use the illustrations and corresponding text of this monograph in such a manner that archeological units might be compared with the experimental replicas both visually and statistically.
>
> . . . . . .
>
> The key codes provide all pertinent information concerning the manufacturing process. One can tell at a glance the stage of manufacture of any given unit; whether or not it is acceptable or rejectable; whether the area of emphasis, that is, the problem or solution illustrated, is concerned with end-thinning or lateral-thinning; the type of problem encountered (if rejectable); the type of fabricator tool; and the number assigned to the individual unit. Key codes were made up for core types (Stage 0, procurement: page viii), blank or flake types (Stage 1, obtaining the blank: page ix), and for biface types (Stages 2-4: page x). The latter key applies, with additions, to all stages up through the finished points (Stage 9).

I am reprinting the original text to this illustration in full: Block Cores and Flakes (OAI and 1AII & IIIa)

From block cores struck with a hammerstone (Figure 1, a-d), one may obtain regular blade-flakes with unprepared or prepared platforms (*not shown in review*) Block cores may also be made to yield irregular flakes. Whether or not one obtains a bladeflake or a flake is determined simply by whether or not a ridge of vertical convexity is (successfully) followed during spalling. "Regular" blades follow one or more ridges or convexities and are typically long and parallel-sided (Crabtree 1972:42).

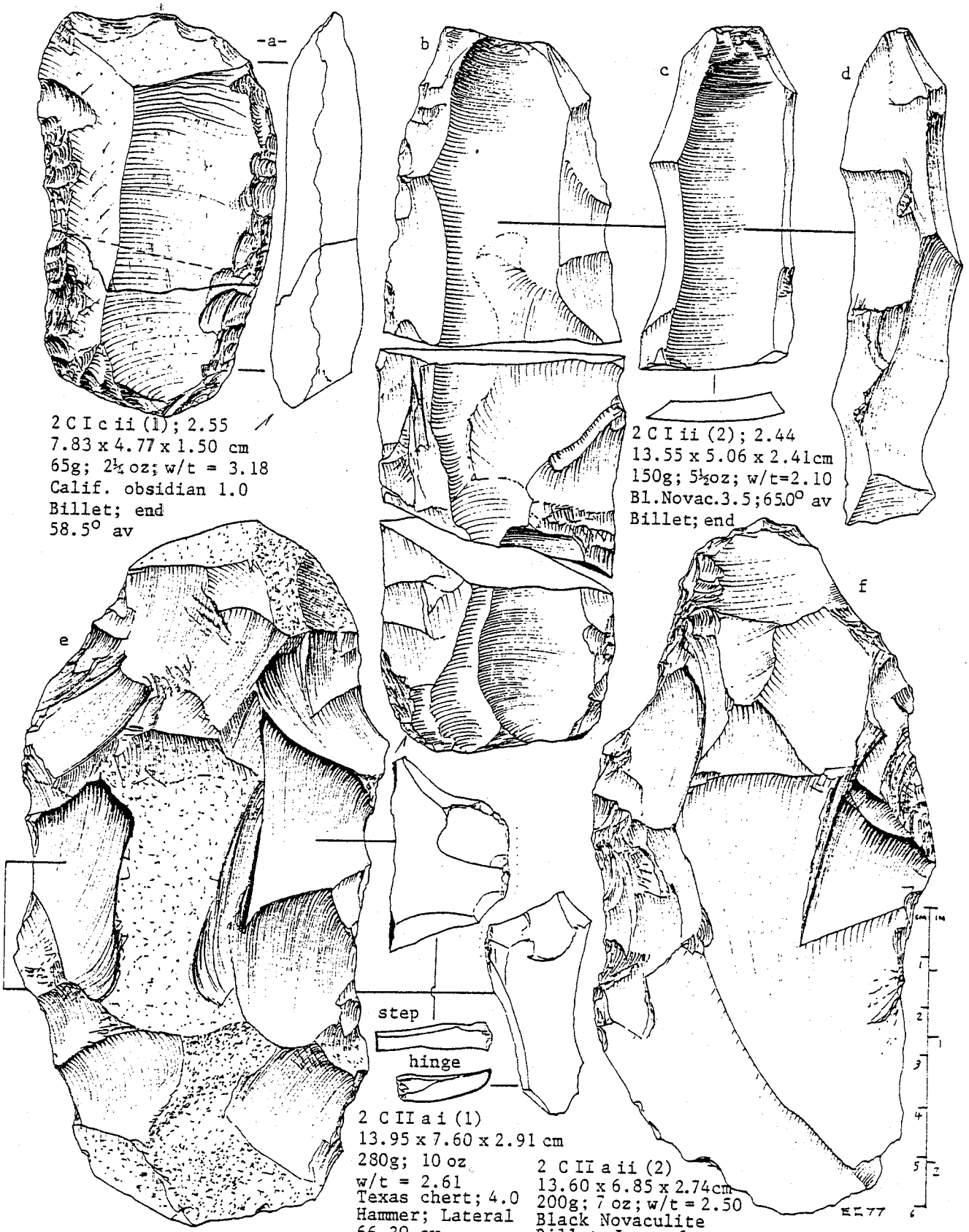
Blades with unprepared platforms do not have the overhang from the prior blade removed. Prepared ones do. A coarse abrading stone such as a granite or quartzite flake or split cobble or even the rough face of an antler billet, abraded from the top of the core downward and outward at a 45 degree (or greater) angle to the top serves to clip off or trim the overhang in most cases. Little actual abrading (grinding) is required. (Such lithic abraders, used primarily for biface platform preparation and possibly flake core overhang adjustment, as above, have been documented at Williamson as well as at Flint Run (Painter 1972 and Gardner 1974:6A). Unfortunately, at Williamson, it has been erroneously stated that such abraders were used primarily "to abrade the lateral edges of finished projectile points" and as "blade core abraders" (Painter 1972:16 & 18). In view of the archeological evidence, coupled with extensive experimental replication, these abraders, as well as the identical ones from Flint Run, seem to have been primarily used for platform beveling and edge grinding during the course of reduction of all stages of biface production. Their use for basal grinding of completed projectile points may have amounted to less than 1% of their use. Their use in spalling for biface blanks is optional).

I have found using an almost unwieldy, heavy oblong, unhafted hammer of medium-hard material such as sandy quartzite, sandstone, or eroded granite or greenstone and weighing between 5 and 6 pounds to be most suitable for removing massive blade-flakes. Such hammers have a short use-life because of their softness, but they do enable one to remove massive, straight spalls with little or no curvature and with diffused bulbs supposedly reminiscent of billet flaking. (Note Bordes 1969:11, Bordes and Crabtree 1969, and Figure 11a and 12a-c, this work).

In spalling, I rest the core atop a pad on the inside of the left thigh, tilt the platform quite far down (vertical) and strike at a very acute angle to the platform. Alternately, one may tilt the platform face upward and strike perpendicular to the platform (Bordes' "rectilinear downward translation" 1969:11. Cf. Bradley 1974:192, Figure 5.1,a).

Figue 8 Rejects: End-Thinning, Fracture; Lateral Thinning, Hinge and Step-Fracture

Stage 2



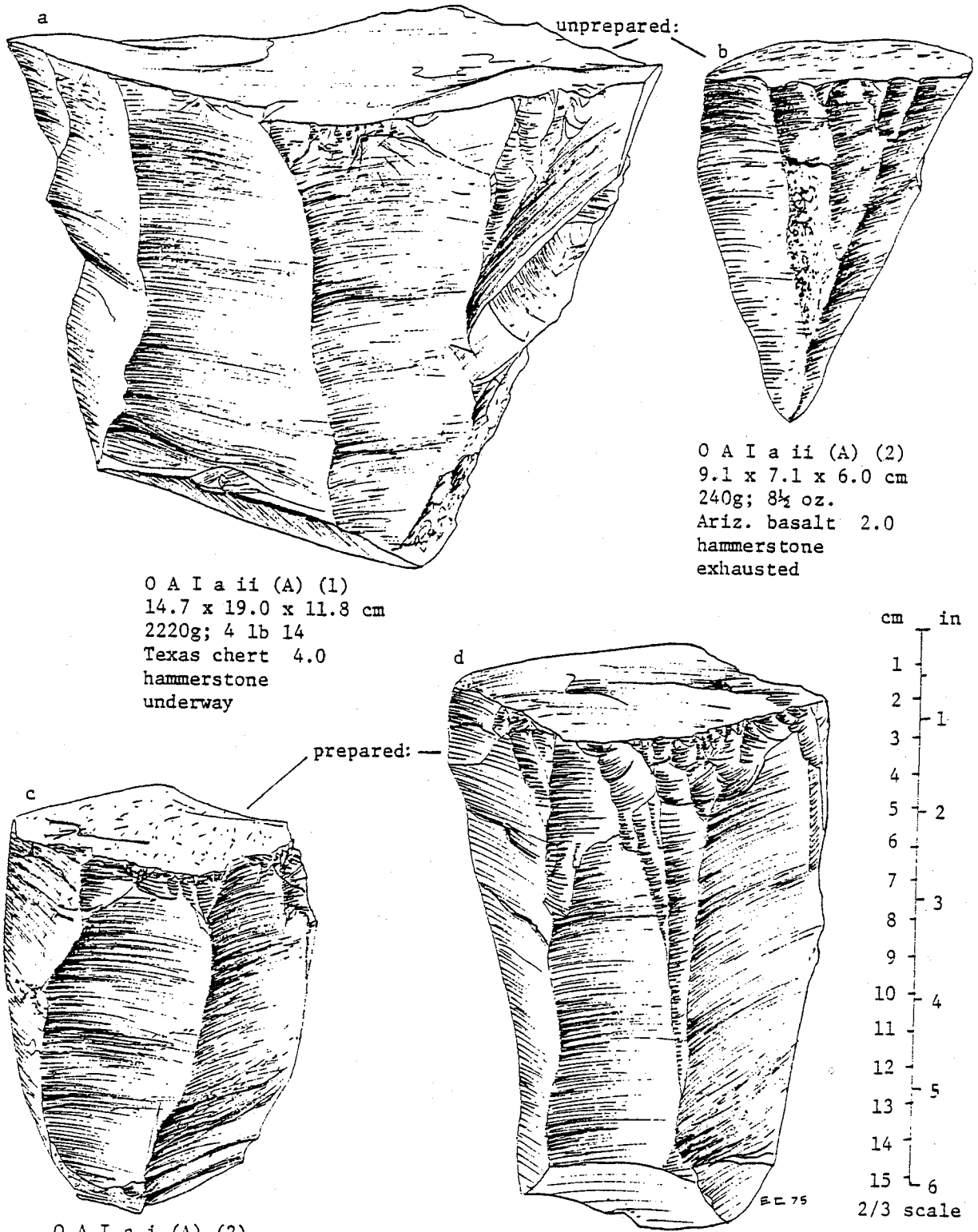
2 C I c ii (1); 2.55
7.83 x 4.77 x 1.50 cm
65g; 2½ oz; w/t = 3.18
Calif. obsidian 1.0
Billet; end
58.5° av

2 C I ii (2); 2.44
13.55 x 5.06 x 2.41cm
150g; 5½oz; w/t=2.10
Bl.Novac.3.5;65.0° av
Billet; end

2 C II a i (1)
13.95 x 7.60 x 2.91 cm
280g; 10 oz
w/t = 2.61
Texas chert; 4.0
Hammer; Lateral
66.3° av

2 C II a ii (2)
13.60 x 6.85 x 2.74cm
200g; 7 oz; w/t = 2.50
Black Novaculite
Billet; Lateral
57.3° av

Figure 9 Block Cores, Unprepared and Prepared Platforms — Procurement



0 A I a ii (A) (1)
14.7 x 19.0 x 11.8 cm
2220g; 4 lb 14
Texas chert 4.0
hammerstone
underway

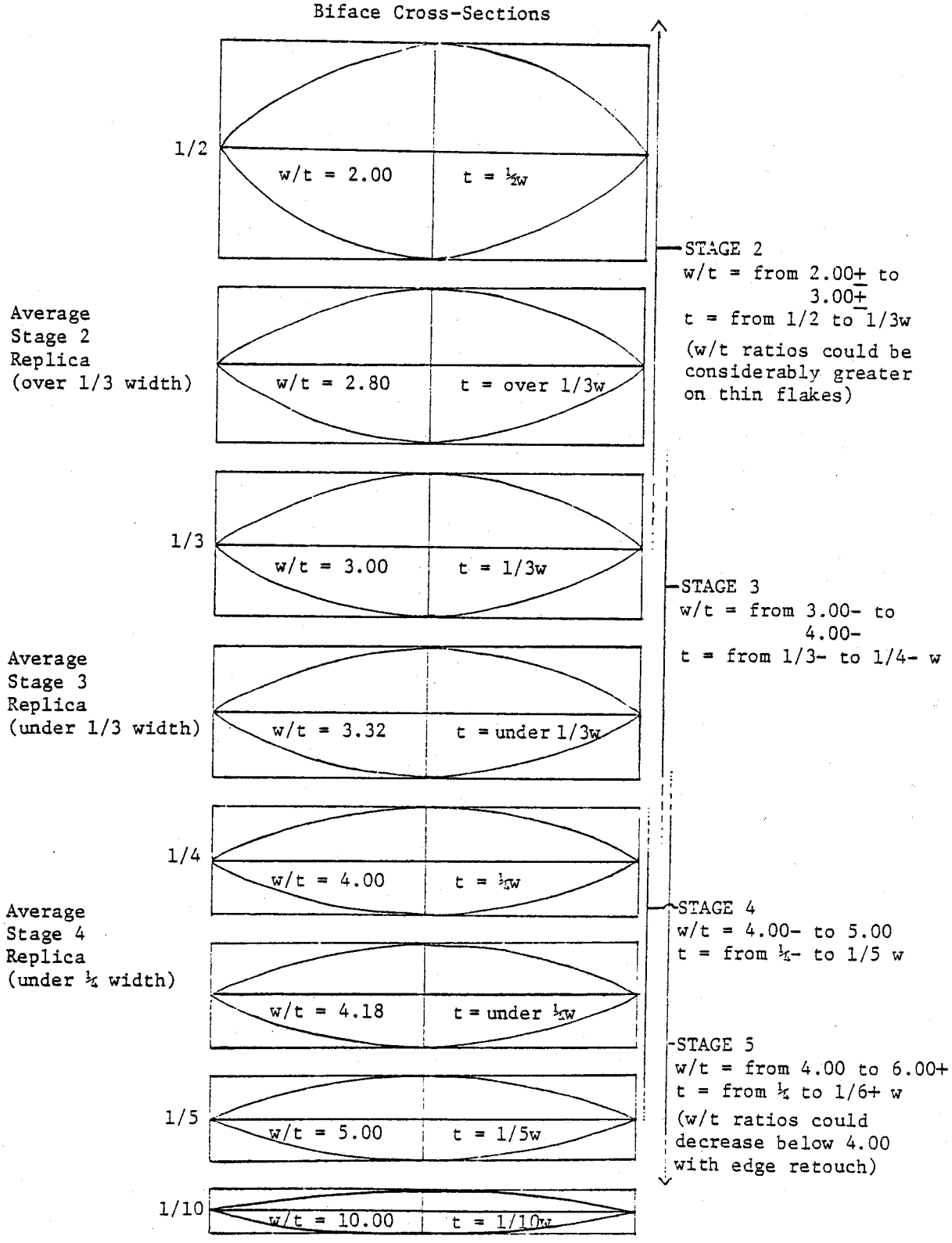
0 A I a ii (A) (2)
9.1 x 7.1 x 6.0 cm
240g; 8½ oz.
Ariz. basalt 2.0
hammerstone
exhausted

0 A I a i (A) (2)
10.2 x 9.5 x 7.1 cm
760g; 1 lb 11
white novaculite (Ark.) 3.5
hammerstone
exhausted

0 A I a i (A) (1)
15.2 x 12.4 x 8.2 cm
1730g; 3 lb 13
Texas chert 4.0
hammerstone
underway

Table 4

WIDTH/THICKNESS RATIOS AND STAGES OF MANUFACTURE OF EXPERIMENTAL REPLICAS



To use this scale visually, hold the biface in question endwise between this sheet and your eyes. Find the corresponding ratio by moving biface toward or away from eyes and match image to outline.

"Irregular" flakes are so-called because instead of following a ridge, they are removed from a flat, concave, or only slightly convex surface, creating either a roundish flake or, more commonly, one wider than long (Figures 13,14, c & d, 15, 16, b & c, 17, b, & 18). The distal end of the subsequent biface tends to be oriented at right angles to the point of impact of spall removal (Figure 66, b). Such irregular spalls with variable platforms may be removed during the normal course of spalling activity from a variety of cores: spheroid, biface hammer, biface billet, cobble, etc. (See Figures 2, 3, & 4,b). In other cases, as mentioned, a core worked down to a relatively small mass may be split in two, yielding one or two usable spalls (Figure 5,b). McCary notes the presence of large, irregular "flakes" of up to 10 cm (4 inches) in length at Williamson and feels that such may have been used for fluted point reduction (1975:57).

(Note: A heavy billet--as well as a hammerstone--may be used to remove suitably large flakes from block cores. This was not attempted during the course of these experiments due to material shortage, but such flakes have been subsequently removed with considerable success. This does need further investigation.)

The tables, too, are packed with new information, experimentally derived. (Table 4, as in the book).

The tables have also been used for data storage. In Tables 3 and 4, I indicated data collected as a result of obtaining length/width/thickness ratios of the replicas. To obtain these ratios, each replica was measured as to the above attributes, and calculations were made from these measurements. In addition, I measured work time and weight, by stages, for each replica (Tables 5 and 6). Records of work time and weight changes were kept for only a fraction of the total experiments because it was not until I was several years into the project that I realized the potential of doing so.

I think this gives the reader some idea of the richness and usefulness of this book. Fortunately, Errett is off in Denmark and will not see this review until he reads his final copy along with everyone else. His book will be reviewed elsewhere in a detached fashion, but why should I try for a phony detachment? This book is unique.

JACKIE NICHOLS

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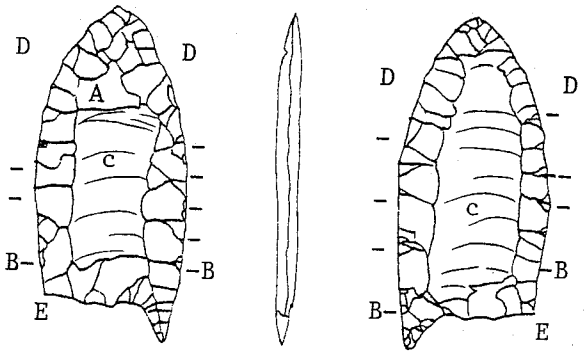
the Denver series

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 #5, Folsom Point from Folsom, New Mexico 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."

——#5, FOLSOM POINT FROM FOLSOM, NEW MEXICO——

PRIOR STAGE: A small remnant (A) beyond the end of the shortest flute shows that the flat face of a large flake was utilized as part of the pre-fluting preform. Unfortunately, a dip in this surface caused the flute to terminate before it reached the tip.



A = Prior Stage
B = Dominant Spacing
C = Channels
D = Flake Sequence
E = Broken Ear

Figure 10

FINAL STAGE: Evenly spaced flakes line the margins and are interesected by large channels of uniform width on each face. Ignoring retouch, marginal flakes are spaced at about 5mm (B).

DOMINANT MECHANICS: The wide, flat character of the major dressing flake scars indicate flaking with a broad tool tip. Even spacing and low undulations of rings in the channels (C) illustrate a uniform load applied with fine control. My experience is that many methods could provide these features, but pressure on a restrained preform is most consistent with the observed flutes.

ORGANIZATION: While spacing is reasonably uniform, the flakes seem to be non-serial except near the tip where the sequence is towards the tip (D). The flat, unmodified face was fluted first. Uniform channel width was made possible by consistent, uniform spacing on the preform.

RETOUCH: Retouch is minimal and does not reach the channel except from the end of the short flute forward to the tip. Just enough trimming was done to establish the shape and straighten the edge.

EDGE TREATMENT: The edge has been kept in a plane and is very uniform. Since retouch has been unusually light, the edge is more acute than usual.

HAFTING: Channeled faces separated by only 2mm both weaken the point and offer a chance to stabilize the artifact through hafting. Each flute has been additionally thinned by flat flakes to facilitate hafting.

W/T RATIO: The width/thickness ratio of 10:1 reflects an extremely flat and lightweight projectile point. Unless hafting compensated for the lack of strength, this artifact would be very vulnerable to breakage.

DAMAGE: One ear (E) is broken off level with the basal inset, apparently due to pressure perpendicular to the face.

FUNCTIONAL ASPECTS: Hafting probably would restrict damage to the extreme tip of the projectile, and then rejuvenation would be very simple with short flakes. Quantities of replacement points would be easy to transport and the potential for repair would also facilitate moving large distances from stone sources before replenishment became necessary.

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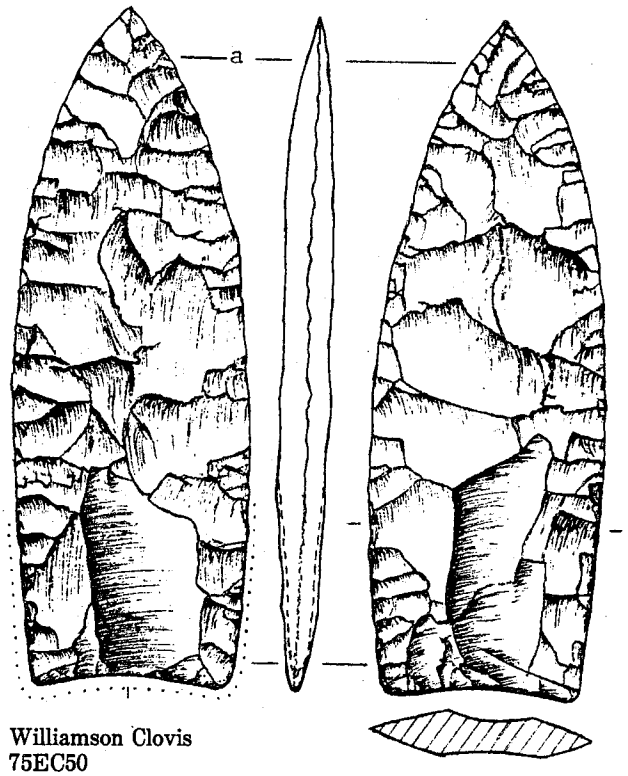
COMMENT ON PATTEN'S ANALYSIS OF THE CLOVIS FROM CLOVIS [FE2[2]:5-6]

The main disagreement I have with Patten is that I feel that the point was flaked by percussion not pressure. There is not a single flake on the point that could not have been done by careful direct percussion. I have replicated these flake scars many dozens of times. (See Figure 1) In fact, it was the point illustrated which led me to seek the control over percussion I needed in order to interpret the Williamson site Clovis points, many of which bear these same attributes. Direct freehand percussion with an antler-like billet is capable of extremely controlled and delicate touch and retouch.



Williamson Clovis
75EC50
heated Belton chert 3.0
9.40 x 3.40 x .60cm
w/t = 5.67

Figure 11
100% Percussion

On the other hand, the point could have been replicated by pressure--at least some of the scars. I doubt that the overshot scar "D/B" was a pressure scar though it seems a little too broad. Such scars are (by pressure) feasible only on a preform that is first flaked out by controlled, flat percussion--the same kind of scars that are evident on the final series.

I also feel sure that the flutes themselves were removed by percussion. In my thesis research, I uncovered over 50 seemingly similar but different means that I and others around the country today are using for the removal of Clovis-like flutes. In time, when we get all of these analyzed, we will be able to narrow down the ways in which the flutes under discussion may have been removed. In the meantime, I offer my opinion tentatively.

I see no evidence of "multiple fluting" on the obverse (left) side of this point. A small end-thinning flake was removed to the right of center (evident to the left of the unlabeled "flute-like spall" which was obviously removed during excavation, not manufacture). After the flute was removed, two flakes were removed on either side of the flute scar. They were *not* removed prior. These end-thinning flakes should not be termed flutes as they simply served to adjust the primary flute scar for receiving the hafting. The reverse face (right) does bear evidence of a prior fluting attempt. I feel that the second attempt was made, not because there was a "multiple fluting" tradition, but rather because, due to misjudgment of the flake scar path or of aim (which would not occur with pressure), the first flute was off-center to the left. The second flute apparently straightened things out enough to get by. The remaining end-thinning flakes serve as ridge adjustment flakes to prepare the base for hafting. As with all fluting, the function of the fluting was to prepare the base to receive the hafting mechanism, not to present visual symmetry for "art's" sake.

The base of this point, by the way, is quite sharp, making it difficult to tell which face was fluted first. As it appears to me, the reverse face (right) was fluted first, the obverse face (left) second, and at least some of the basal retouch then given to the first face. I am not entirely sure on this.

A cross-section and side view of this point--views that are critical to this kind of analysis--are missing in Patten's figures. These would have revealed that this point is rather thin for pressure flaking at this width (.73 cm on my cast). The width and length, by the way, are 3.50 and 11.82 cm, respectively. This gives it a W/T ratio of 4.79, not 4.6 as Patten indicates. Again, this could be due to the cast. The edge-angles, which can often be more revealing than W/T ratios, vary between 30 and 45 , along the cutting edge.

I have tried to illustrate elsewhere a wide range of variability of both percussion and pressure flake scars as they apply to the early stages of manufacture of Clovis-like fluted points (again, the thesis). I did this because there is an almost unbelievable amount of overlap between the two.

ERRETT CALLAHAN

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problems / solutions

Interpretations from Replication of the Bipolar Technique: A Cautionary Statement

It is important to note that while lithic replication has become recognized as an important element in the analysis and interpretation of archeological collections, there are other elements of equal or greater importance. The disagreement here with Hardaker's (1979a) bipolar study is not so much with his observations of the mechanics involved, as it is with his interpretations of the archeological record.

> 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 (Hardaker 1979a13).
> His view is further indicated in a second paper:
>
> This paper illustrates the necessity of becoming familiar with a common Asian stone technology--bipolar flaking--in relation to investigating and evaluating the natures of fractured stones found on possible pre-20,000-year-old sites in the New World (1979b).

The second quote assigns a tentative time period to his "pre-La Jollan cultures". What is referenced in the first quote by "pre-La Jollan" and "river bed quarry sites" (1979a:16) are those stream beds and alluvial deposits in the San Diego area that have been interpreted to contain artifacts from the Lower Paleolithic period of several hundred thousand years ago (Hardaker: personal communication). Previous interpretations concerning the Texas Street Site (Carter 1957) and other localities (Minshall 1975) have argued for the bipolar technique's great antiquity in Southern California.

The purpose here is not to dispute that artifacts occur in stream beds or alluvial deposits nor the possible presence of the bipolar technique in Southern California. It is, rather, to indicate those other elements besides repliction studies that are

necessary for the interpretation of lithic collections.

A basic element of such interpretation is the existing archeological literature, in this case, concerning the bipolar technique in the New World.

> It is one of the most common lithic reduction techniques I have seen represented in North American artifact assemblages (Knudson 1978:45).

Knudson (1978) goes on to list an impressive number of areas for North America in which evidence of this technique has been recovered from the archeological record. To his list can be added Panama (Ranere 1975). In numerous cases this technique is noted to occur relatively late in prehistory.

Another important element in interpretation is a consideration of the logic in relating replication studies to the archeological reality. The statement by Hardaker that "it represents a tradition" is an interpretation of the archeological record that does not logically follow from any results that can be produced by experimental replication. Further, Weir (1976) argues against the possibility of such a tradition in that bipolar flakes in some cases may be a result of a generalized use of anvil stones. Knudson has expressed the view that the bipolar technique "often is an accompaniment to more stylized and complicated technologies within a single cultural system (1978:45)."

An additional element directly concerns the replication techniques themselves in that some factors may not actually replicate the prehistoric situation. The conclusion that the bipolar technique is poorly controlled and supplies a marginal product has been indicated by Weir, who stated that his experiments used "ledge-type flint" and that "the conclusions would apply as well to pebble materials" (1976:41). The conclusion that this technique is crude at best was also reached by Binford and Quimby (1972). Hardaker (1979a) notes, correctly, that the form of the core plays an important role in the types of flakes produced. Replication experiments by this author support the contention that useful flake forms cn be repeatedly produced with reasonable efficacy. It is suggested that Weir reached the conclusions that he did simply because the wrong form of the raw material was used. Knudson tends to support this view:

> It tends to be used whenever the lithic resources are small cobbles to pebbles that have to be used efficiently (1978:45).

Another element to consider in the interpretation of any archeological collection is the archeology which produced that collection. Specimens specifically offered by Hardaker as prehistoric bipolar cores constitute a case in point. First, their origin from a river bed does not necessarily indicate an archeological context and certainly does not provide a datable context. Secondly, his specimens were water-worn to the point that specific flake scars, that he claimed represented bipolar flaking, could not be identified. Thirdly, his contention that there was battering associated with the "flake scars" was observed by this author to overlie the water smoothed surface of the cobble and appeared to be recent origin.

Finally, Hardaker's specimens appeared to be much too large when compared to those found elsewhere in the New World (Knudson 1978; Binford and Quimby 1972; Ranere 1975). The size of Asian Paleolithic cores is not known to me, although it may be suggested by Kobayashi (1975) that they were larger than those in the New World, but Kobayashi is not clear on this point.

In summary, the development of interpretations concerning prehistoric lithic collections must consider 1) the existing archeological literature concerning both the previous interpretations of such collections as well as previous replication studies, 2) the logic in developing those interpretations, 3) the factors that must be controlled during the replication study, and 4) the archeology that produced the collection under study.

Without a careful consideration of the above listed elements, no amount of lithic replication will aid in correctly interpreting archeological collections. In conclusion, replication of the bipolar technique cannot demonstrate the antiquity of man in Southern California, and it alone cannot identify the presence of the bipolar technique in Southern California.

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An Experimental Approach To The Study Of Early Stone Tools From Koobi Fora, Kenya: Preliminary Notes On Research Design

I am currently engaged in research which is designed to explain the nature of the forms of early stone artifacts from the Koobi Fora region of East Turkana (Rudolf), Kenya, and to throw light on their possible functions. Sites discovered here (about a dozen major excavated occurrences) span a time range from about 1.8 to 1.2 million years ago, making them (along with the Omo, Olduvai Gorge, Peninj, Melka Kunture, etc.) among the world's earliest known.

The assemblages from the earliest of the Koobi Fora sites (KBS, HAS, etc.) are contemporary with Olduvai Bed I and are not unlike the "Oldowan" ("chopper"-dominated) assemblages (Isaac, 1976). From the middle range of strata at Koobi Fora

come a set of assemblages dominated by core and flake "scrapers" plus "choppers", etc. which have been designated the "Karari Industry" within the Oldowan Industrial Complex (Harris and Isaac, 1976; Isaac and Harris, 1978). In the upper part of the sedimentary formation scattered artifacts of early "Acheulean" aspect have been found and one site consisting of large flakes often unifacially retouched into pointed "picks" or "handaxes" has been excavated (unpublished).

By approximately 1.5 million years, at least two hominid forms seem to be represented in the Koobi Fora record: a robust australopithecine, and an early form of Homo erectus (Walker and Leakey, 1978).

My research is being carried out as a part of the Koobi Fora Research Project, which is a project of the National Museums of Kenya and the International Louis Leakey Memorial Institute for African Prehistory (TILLMIAP) who have invited me to undertake the investigation of form and function. Other research of the Koobi Fora project by Berkeley students includes Kathryn Schick's experimental investigations into archaeological site formation and burial, Henry Bunn's experiments in non-hominid and hominid induced bone fracture patterns, and Ellen Kroll's spacial analysis of Koobi Fora sites.

The primary goals of my research are 1) to locate sources of the raw material, determine technological and functional qualities of the raw material, and reasons for hominid selection patterns; 2) to determine how the stone artifacts were manufactured (techniques, strategies, etc.) and what types of core/debitage relationships exist, and 3) to determine which of the stone artifacts were used, and how they were used.

Lines of evidence include: 1) careful analysis of the archaeological assemblages; 2) conjoining cores, flakes, and fragments back together again (already with good success), 3) geological field work to locate and analyze the fossil river gravels which appear to have been the main source of the raw material for a given site. This is being done with the help of sedimentary geologist Ian Findlater of TILLMIAP; 4) petrological identification of the raw material types to ascertain their original place of origin (being done by vulcanologist Ron Watkins of Birkbeck College, London; 5) replicative experiments to produce all artifact forms in as many different ways as possible, and study populations of cores and debitage created; 6) experimental usage of the relicated forms for such tasks as butchery, woodworking, digging, etc. to see what the functional capabilities of these forms are. The efficiency of the stone forms will also be compared with tools of wood, bone, shell, one's own teeth and hands, etc.; 8) observations of edge damage and microwear of experimental and excavated artifacts (with the help of Lawrence Keeley, University of Illinois, Chicago Circle) to investigate which artifacts were used and in what way.

Besides these lines of evidence, ethnographic and primatological studies will, in part, serve as possible models of hominid activities, and studies of the knapping of "naive" or novice toolmakers, with no knowledge of lithic studies, are being carried out.

In general, most of the Koobi Fora assemblages consist of lava cobbles that have been flaked, plus the flakes and fragments detached and sometimes also retouched: the cores are relatively simple forms ("choppers", "polyhedrons", "discoids", "core scrapers", etc.) It is very likely that many of these core forms are simply the by-products of the manufacture of sharp flakes, and their overall morphologies (and therefore their typological classes) to a large extent determined by the original shape of the cobble.

Based on examination of the cobbles, cores, flakes, and fragments from the Koobi Fora sites, and my preliminary replicative experiments (to be published), I suggest that direct percussion with lava hammerstones was a major flaking technique, through bipolar technique and throwing cobbles against an anvilstone were possibly also practiced.

About 95% of the raw material at these sites consist of dark lavas (especially basalts) that originate in the volcanic hills around the eastern margin of the sedimentary basin. Lava clasts are rounded by being carried into the basin by streams, and the archaeological sites are most prolific in the vicinity of these palaeo-stream courses (Isaac and Harris, 1978; Harris 1978). These lavas are coarser-grained and more tenaceous than obsidian, flint, or chert; greater force is required to detach flakes, and after a session of knapping, one's hands can be sore and swollen from the shock absorbed. (It would be interesting to examine fossil hand bones of early hominids in search of pathological conditions that could be induced by this type of toolmaking/using.)

The rest of the raw material types (about five percent of the artifacts in the assemblages) include ignimbrites (glassy welded tuffs), a range of cryptocrystalline silicas ("chalcedony", "chert", etc.), silicified breccias, silicifed tuffs, fossil wood, and quartz. With the exception of quartz, these materials are usually finer-grained than the lavas, flake more easily, and produce a sharper edge. However, in most fossil and modern river gravels in the area, these non-lava clasts are relatively rare. Presumably this accounts in part for their low numbers at the archaeological sites (Harris, 1978).

I am taking a critical look at what has previously been said about early stone artifacts. Some of the more dogmatic examples include: 1) typological classifications that presume that all flaked cores are "tools"; 2) all edge-damaged pieces are "utilized" by hominids; and 3) the assumption that these core types are predetermined target pieces of these hominids.

This holistic approach to the questions related to the forms and possible functions of early stone tools will hopefully increase our knowledge of the behavioral patterns of early Pleistocene hominids, and help to generate new and testable models of hominid adaptation based on the results of replicative and functional experiments.

The facilities for doing this research are being provided by the Koobi Fora Research Project, TILLMIAP, and the University of California at Berkeley. The archaeological program within the Koobi Fora Research Project is supported by grants from the National Science Foundation.

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Like so many of our contributions, this "problem" could well find a home in another section of *FE*. For example, I could have included it under "What I did last summer." Certainly the most important thing I did last summer was to spend some time at the San Bernardino County Museum acquiring data from the Calico collections. In August, Ruth Simpson accompanied several of us to Calico for another geologic assessment of the site, this time by Dr. Roy Shlemon. I had harbored some little hope the site was not nearly so old as previously claimed; however, Dr. Shlemon assured us we were still in the ballpark of 100,000 years BP. Had the site somehow proved to be younger, at least one could begin to raise some questions in a rational atmosphere. For this site stands enigmatically on the border of all we know about early man in America, and it must not be ignored.

The Calico site is located in the Calico Mountains on the Mojave Desert, up on an alluvial fan bordering Pleistocene Lake Manix. Ruth Simpson brought the site to the attention of the late Dr. Louis S.B. Leakey, and under their joint direction excavations went on there from 1964-1970. Reaction to the artifacts found and the date of their context can generously be described as less than enthusiatic. The site was "too old," the context of an alluvial fan was thought too difficult to assess, the artifacts were too borderline or unfamiliar.

I can't do anything about the date, it evidently must stand. Several of us are preparing a formal paper on the context (it could *not* have "created" the artifacts) for presentation elsewhere. However, I would like to discuss a few points about the artifacts, then pose a problem concerning one of them.

We have come a long way from considering only the most complex tools artifacts, and such things as utilized flakes and debitage irrelevant, however, we are still in the age of "arrowhead validation." More specifically, we still generally assess what we find in terms of bifacial reduction. In *FE*, we have been very concerned with the stages of bifacial reduction, but we are trying to open discussions of discontinuous sequences and other technologies--most recently we have given a lot of space to bipolar techniques. It is almost impossible, however, to see past the spectacular Clovis points--and how can you hope to see a "pre-projectile point stage" if you are looking in terms of projectile point technology.

Looking at the Calico artifacts with American-trained eyes is an insecure experience. There is a definite sensation of quicksand beneath--nothing to hang onto. I know that Africanists and European archeologists have found them familiar. This is not sufficient.

What can one observe (a more complete and balanced overview of the artifacts is available in Simpson, 1979)? There are, first of all, morphs one has come to associate with woodworking, deliberately created concave edges, obvious notches. This is not surprising. The region was once wooded, possibly as late as 11,500 BP (Van Devender 1979). However, these morphs in my past experience have been validated by their association with projectile points. Here they stand in the unfamiliar company of handax forms.

In spite of pieces with flaking on both sides, there is no bifacial reduction as we know it. There is some shaping, much edging, but the 'concept of thinning stone' does not exist in this collection. Repeatedly, there are familiarly worked (not simply battered) edges on--let's just say it--rocks. There are also many small and utilized tools burins and gravers-made on flakes and blades. (See Singer, 1979).

And then there is an exhausted jasper blade core, bullet shaped, 8-faceted, with a possibly truncated top and a crushed distal end. (Figure 12). It could be indistinguishably placed in the illustrations for a book reviewed elsewhere in this volume (see *Archaeological Studies of Mesoamerican Obsidian*, Hester) were it made of obsidian. It was found 210" below surface. An initial and rather frantic examination of the collection produced no associated jasper blades; however, no one has denied there has been mudflow movement downslope and such associations--if they existed--are probably lost.

Is this core a hopeless anomaly, like finding a 3-piece polyester suit? Is it another Calico enigma? Is a collection of woodworking morphs, hand axes and blade cores a useful model for pre-Clovis tool assemblages?

I pose the problem of the little Jasper core to California readers. *FE* has more readers in California than any other state. Make a trip to the San Bernardino County Museum and look in the main display case. Then send me a solution to the peculiar problem of this core's existence. Is it really what it appears to be? One note: beware of the starch fracture argument; be prepared to really argue, not simply cite it as a possibility--and, please, no Devil's Postpile arguments.

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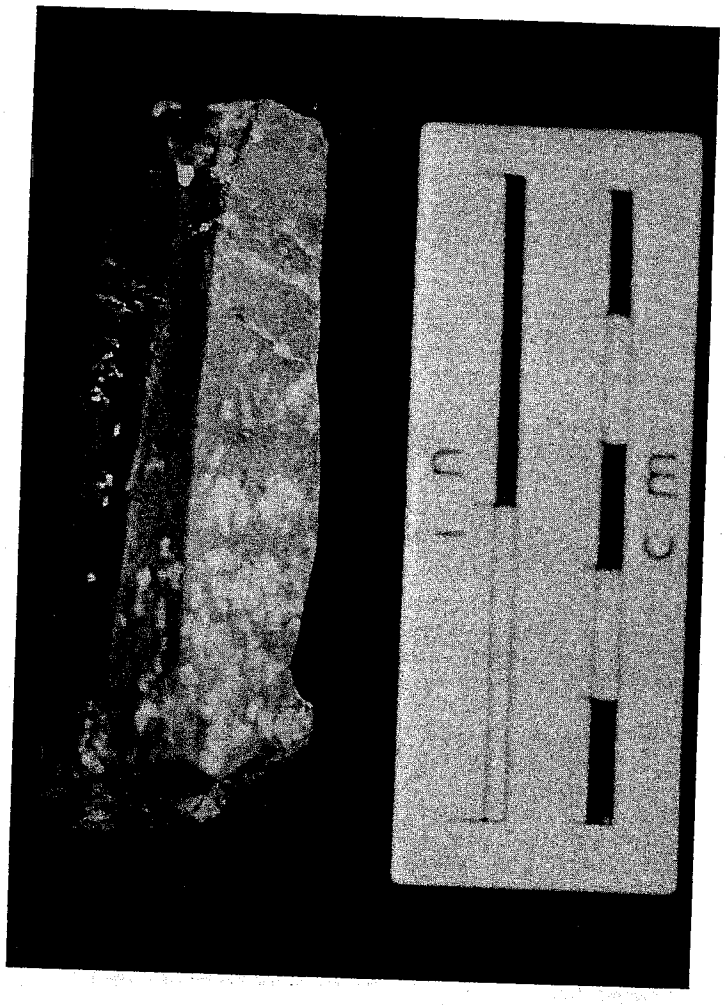
Figure 12. Jasper Core from Calico



Additional Comments On Bipolar Flaking

INTRODUCTION

Articles on bipolar flaking have been published periodically for a number of years. J.B. Sollberger and I (Sollberger and Patterson 1976, Patterson and Sollberger 1977) have published two articles expressing doubts on the general use of this technique. Our arguments center on the lack of fracture plane control when obtaining true bipolar fractures. We do not say that Indians never used a true bipolar fracture technique, but simply that there is no technological advantage for use of true bipolar flaking to warrant frequent use. For most lithic manufacturing purposes there would be a technological disadvantage for the use of bipolar flaking. Controlled flaking of flint can only be obtained by fractures initiated at a single point of force application.

After more study of comments by others on bipolar flaking, I would like to make some further comments on this subject. It is now apparent to me that there are real semantic difficulties involved with the term "bipolar". People seem to be classifying several techniques under this general term. This leads to continued confusion and illogical debates. Unfortunately, studies on bipolar flaking can contain any or all of the following lithic techniques:

1. simple use of a hard anvil
2. simple use of multiple opposed striking platforms
3. true bipolar fracture
4. use of a hard anvil with simultaneous flake detachments

This article will attempt to clarify some of the issues involved. Correct identification in archeological collections of the use of true bipolar flaking is also a major problem in my opinion.

SIMPLE USE OF HARD ANVILS

As previously noted (Sollberger and Patterson 1976:40), the use of a hard anvil can offer a mechanical advantage, by preventing deflection of the core during percussion, thus giving more efficient use of applied energy. However, the use of a firm support for a core does not necessarily produce bipolar fractures. In fact, to obtain controlled flaking, the core should be positioned on the anvil in a manner to avoid bipolar fractures. This can be done by allowing core overhang on the anvil (Crabtree 1972:10-11).

Simple use of hard anvils is sometimes equated with bipolar flaking. Childers (personal communication) and Carter (1978:21) now both state that the term "hard anvil technique" would be preferable to their previous uses of the term "bipolar" (Childers 1977). General roughing and damage can occur to the core at the anvil contact point during percussion, but should not be related to a true bipolar technique when flaking is being done with single impact point fractures. Several of Binford's (1972:figs. 4-7) illustrations labeled as bipolar cores could possibly simply show the use of a hard anvil without true bipolar fractures, but with some roughing of the core at anvil contact points.

MULTIPLE STRIKING PLATFORM USE

Many cores illustrated in the literature could show use of multiple striking platforms rather than a bipolar technique. It is common to find cores on archeological sites with opposed striking platforms, where the platforms have been used individually by core rotation. This possibility increases the difficulty of analysis to identify true bipolar technique cores.

TRUE BIPOLAR FRACTURES

A number of people limit the use of the term "bipolar flaking" to cases where true bipolar fractures occur (Crabtree 1972, Kobayashi 1975, Sollberger and Patterson 1976, Patterson and Sollberger 1977). Binford (1972:355) also seems to limit bipolar flaking to true bipolar fractures, which he describes as "external cleavage, resulting from the production of opposing cones of percussion". True bipolar fracture involves initiation of fracture at both the proximal end of the core, where force is applied, and at the distal end of the core resting on a hard anvil. The secondary fracture resulting from force rebound at the anvil joins the primary fracture plane. As previously noted (Sollberger and Patterson 1976), true bipolar fracture gives poor control of the resulting fracture plane, as well as undesirable results such as flake distal end damage and a higher percentage of broken flakes.

There are difficulties in identifying true bipolar fractures on archeological specimens. Most flakes produced by true bipolar fracture do not have a second bulb of force on the distal end. Cores from true bipolar fractures are not always distinctive either, except for crudeness. Many of the cores illustrated by Binford (1972:figs: 4-7) as bipolar have flake scars that could easily be the products of single impact point fractures. Goodyear (1974:fig. 21) illustrates cores as bipolar which the reader must take on faith, as few analytical criteria are given except for the use of multiple striking platforms. I also have doubts about the true bipolar fracture nature of some cores shown by many others, including Honea (1965) and Leaf (1979). This is especially true if the striking platform angle to the core face is very acute. In this case, the chances of obtaining true bipolar fractures directly through the core are small, due to the inherent limitations on directions of force that will produce primary fracture planes.

I have experimented with hard anvil bipolar flaking using small chert cores with acute angle striking platforms, similar to Binford's (1972:fig. 4) "ridge-area" cores. Even when trying to obtain true bipolar fractures, useful product flakes only resulted when random single impact point fractures occurred. True bipolar fracture debitage was mostly very crude.

Hardaker (1979:fig. 2) has illustrated the results of an experiment in bipolar fracture. He started by splitting a round cobble by bipolar fracture on a hard anvil. This is probably a good use of the bipolar technique, but is not really controlled flaking. Once the cobble was split, the flat ventral face was rested on a hard anvil and hard percussion was applied to the rounded dorsal surface, directly into the core to produce bipolar fractures. The results shown are just what would be expected, a series of jagged edges resulting from irregular flake scars, with no precisely controlled fracture planes. A much better "domed scraper" could have been made by simply turning the core over on the anvil. This would use the ventral surface as the striking platform, with some overhang of the core edge from the anvil to prevent bipolar fractures. Better control of fractures would allow uniform flake removals, with more uniform core edges obtained. It would probably not take primitive man too long to learn this, as Don Crabtree (personal communication) agreed in a recent discussion.

SIMULTANEOUS FLAKE DETACHMENT

One problem with many articles on bipolar flaking is that true bipolar fracture is not involved, but simply a detachment of separate flakes on the striking platform and anvil ends of the core. While this can be demonstrated experimentally, it should be given a separate name, such as "simultaneous flake detachment", to avoid confusion with technology involving true bipolar fracture. I also feel that simultaneous flake detachments would be difficult to identify on archeological specimens, compared to the more normal use of core rotation with multiple striking platforms using single impact point fractures.

In a recent conversation with Jeff Flenniken, it became apparent to me that we were not talking about the same thing when discussing bipolar flaking. Flenniken (personal communication) is involved in experiments which he labels as bipolar flaking, but which do not necessarily involve any true bipolar fractures. If I understand correctly, he uses cores with opposing acute angle edges. One edge is placed on a hard anvil and the opposite edge is used as a striking platform. Force is applied directly into the core edge serving as the striking platform. Flakes are simultaneously detached at the striking platform edge and the edge resting on the anvil.

I suspect that several current experimenters are making

"bipolar cores" where little or no true bipolar fracture is involved. A recent paper by Cable and Most (1979) may fit this case, judging by the core descriptions.

SUMMARY

Several different lithic techniques have been discussed here which I feel have all sometimes been classified as bipolar flaking. This has led to undue confusion, and should be corrected by clarification of terminology. In my opinion, only cases involving true bipolar fractures should be classified as bipolar flaking. Simple use of a hard anvil should not be included in bipolar flaking examples. Use of a hard anvil to obtain simultaneous flake detachments should also be given a separate classification, distinct from true bipolar fracture techniques. Archeological studies can not be understood and compared unless descriptive terminology is generally understood.

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* * * * * * * *

craftsman....

Don Crabtree

We conclude our interview with the master, Don Crabtree

E. One of the things that you've championed over the years is the information we can get from the flake scars on the stone tools. Could you elaborate on the kind of information you think we should be obtaining from the study of the flake scars themselves on the tools?

D. Oh, Errett, I think this is of course the whole fingerprinting thing. Of course, we only are working with the last series and of course the flakes are gone. If it has a sharp edge the platform went with the flake so we don't know what the platform was like, but there's sufficient evidence with flake scars and the overlapping that you have only the flake scar. You've taken off the other half when you take off the next one. And then you produce another ridge and you take off half of that. So your flake scars are not half as wide as your flakes or little bradelets if you want to put it that way. But with the detail and the *sequence* and the *direction* you took these off--tip to base, right or left, or however--they create an edge character. There's a point that's still basic--that is, how were they able to get that edge like a knife-edge. Some edges were as straight as a butcher knife, with every flake scar going clear to the center and terminating from both sides. Some of them are very thin--one of the Hohokan points I've seen was extremely thin but every bit of the surface had been flaked with long, narrow, flakes.

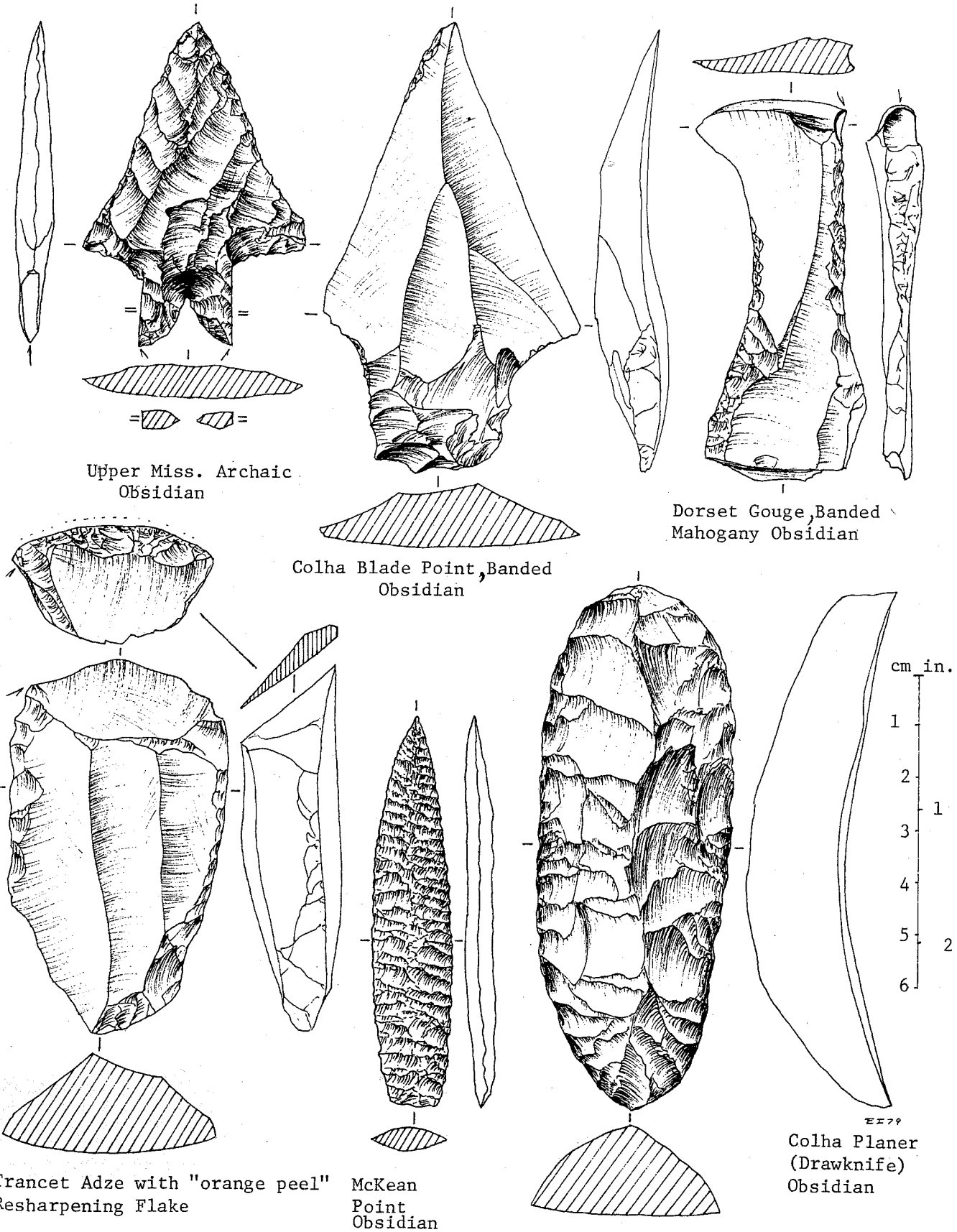
Once the tools are gone, all we have left are the flakes; but this is enough. There is enough flake evidence around that you could interpret what the tool was that they walked away with. They were taking a lot of time and patience to prepare each platform and then spacing it and flaking in different directions. So the flake scars as well as the flakes, I think, can furnish a great deal of information. I think the same thing can be true in following different technology, when they are interpreted and you have enough population of tools. When you have only one or two tools, you don't know what the continuity is. But ultimately we'll be getting more and more tools and these collections will start to fill in. It's going to be much easier to trace a technology where you can set up 8 or 10 tools with all the same technology even though there are some different areas and maybe even different periods of time. But I do not think that margins and edges like this have a great deal of importance.

I think Bruce Bradley said (about the Stockton points) that they examined those under the microscope and found this grinding. Well, it's common interest that they were doing grinding. In the recent stuff they didn't do much grinding. I still use it because I find it to my advantage, but aboriginally you don't find too much of that; still, where you do find flakes, you do have platform grinding.

E. How would you differentiate between replication and simulation?

D. A replica to me would be a true copy of the original while the other is only simulated, maybe just in form, without bothering with all the flaking technology and the rest of it, and it should

Figure 13. Replicas Made by Don Crabtree.



be similar without being a copy. I feel a true replica is done with a great deal of accuracy. However, there is this discrepancy, that even among aboriginal students there are no tools that are alike. There is much difference in fingerprints; there is no way that you can take a cast of one and do an overlay on another and have them match, because the scars are unique individual things.

E. Talking about flake scars a little more, do you see much difference, perhaps on a subtle level, between the flake scars made by experienced modern knappers on a given type of stone tool? And if so, could this tell us about different individuals in the past?

D. Seriously, one would have to study the different artists but I think that aboriginally they were able to eliminate a lot of the faults that you are going to find in some of the recent tools, particularly with the sawed blanks you see in some of these gem shows. They'll use some lever action and take big concoidal, wide, expanding flakes off to get the sawmarks off. That's number one. Once they've got those off, the edge almost looks like it's beveled--they're very short on the margin. They don't carry in towards the center. With the aborigines' points, generally, unless they're extremely thin, they are bi-convex or even plano-convex. But they have that curvature of the flake. A little short, stubby edge is not good for cutting. It just wasn't made aboriginally.

E. Talking about these non-academic commercial flintknappers further, what contributions do you think they could offer to flintknapping?

D. I think a great deal. The academic's time is so limited, preparing lessons and so on, for the next day's class, before going on field work. You're living in a hotel, an apartment or a house, you haven't any of the raw materials. It's hard to knap in the front room and still keep your wife. And you can't do it in the bathtub. Poor Are Tsirk, he was working in a bathroom with no windows in it in New York for a while. Some will persevere but generally they haven't time and they lose their courage and their coordination and that sort of thing. It's very difficult for the professional to find time to do these things. I suppose that if the board of regents saw them pounding on some rock, they might frown on it. So they just haven't the opportunity and the time to keep their coordination in tone.

Your non-academic man, however, may have arranged his time to where he can spend several hours a day on it. So as an advisor the non-academic knapper would probably be more qualified than some of the professionals who have so much more limited time. In order to do any testing, it takes so many hundred thousand flakes and the more you make of them, the more of these little old idiosyncracies and characteristics keep appearing. You wonder how this or that could be made and you must be able to replicate that same thing again. The non-academic knapper can quickly categorize a certain flake as being at a certain stage or development of an artifact while maybe the professional hasn't that ability. He just hasn't removed that many flakes.

E. Then getting onto another area, what are some ways in which you think the professional world might do more to sponsor the talents of the underground commercial knappers so as to give them a viable alternative to the underground market?

D. This is kind of hard. Some of these knappers have such great skill. For many long years they have studied collections so that they can keep in touch. Original artifacts can get lost in shipping, get damaged, and so on. If we had them make up actual replicas or study collections for all of the different institutions this would be a tremendous project. But it would be very difficult for one individual to spread himself out to all the different techniques. Certain ones might be skilled in European work or in different varieties of old world technology. Another might make a series of all varieties of Levallois flakes; others might specialize in certain core types. I think it would still end up with a man being a specialist even though he was a non-academic man. Of course, just making projectile points isn't the purpose. The purpose should be for them to confine themselves to making some replicas and working toward a replication rather than a simulation. Once they've achieved this, the replicas could be cast and sent to other institutions, if they pass muster, to compare to aboriginal material. I do think there's a future for commercial knappers and I feel there are other ways probably than what I mentioned.

E. In what subtle ways do you think that flake scars produced by copper and iron flakers are different from those produced by antler tines?

D. Some Eskimos used meteorite iron as a flaker, and of course, the Old Copper Culture passed on a lot of copper pressure flakers in the Upper Mississippi Valley. They found some in the mounds. But generally, I think that the amount of crushed edge is more apparent with the metal tool than with the antler or the bone tool.

E. How about the flake scars themselves?

D. Well, with the flakes scars I don't know whether I can tell. I think with the metal tool you'll get deeper bulbs as you have a finer point when you concentrate the force; while with a broader tool it will be considerably more diffuse. But Titmus wouldn't use anything but antler now, and some of his work with antler is probably more exquisite than what he has done with copper. Where you use the antler often, your pressure flakes will be lipped and this will leave a very sharp edge.

E. Don, how many individuals have studied under you and what kind of things have they gone on to do?

D. Oh, I've been very fortunate, Errett, in the people that I have met and contacted here, though I did have this coronary a number of years ago. Then I got going with the field school with Idaho State and from there on it has snowballed and I have no reason to stop yet. I've been paid a thousandfold for the friends like yourself and others that I have met. I owe them so much because I have absorbed from them all of these little spinoffs and things that never appear in books.

I was thinking the other day that there have been probably 14 of them that have received their doctorate--I don't know how many their master's. But I think that for the duration of the field school I tried to work with too big classes. You're kinda like a bee going from one flower to the next, you know, working with them and trying to hold them together all at once. Some are sitting facing you and others are backward and so on. Invariably I get a left-handed person and if I can put those behind me it'll come out right.

What one needs is closed circuit television. Sometimes you actually have to hold the other person's hand and even prepare a little margin or edge in pressure flaking and also in percussion. Everytime you strike, your conditions change. So you have to change with the conditions and redo your platform. But I'm getting off the subject about the quantities, Errett. I've given so many demonstrations to different universities, to groups and things like that--there may be 40 or so. Some of them have been with closed circuit television. So the exposure has been really quite great. Of course the formal demonstrations, why if it only lasts, say, an hour and a half or two hours, they don't get very much except a general idea that it rests on a rock. Movies help because they can actually zoom in over your shoulder to show what happened there. It's a very difficult thing to observe because you yourself are always working on the blind side--on the underside. You know from experience where those ridges are and by the feel how much pressure it will take and how you'll rotate your left hand in case you want to run clear across the surface. Or if you want to terminate at the midline, you use another pressure; but you can't see these pressures. So it's a very hard thing to demonstrate. However, a lot of people have been exposed. How many of these go on from there, I don't know.

E. What are your main complaints and suggestions as to the direction the field of lithic technology has and should take?

D. Well, I think the door is just barely open. It would seem, you know, with the work you and I have done, Errett, most things have been accomplished. But really we're only starting. It is often brought to my attention by a bunch of flakes, that the aboriginal thinking is so different from ours and we have to conform with their way of thinking. We're able to replicate a lot of flaking but some of these are works that exceed Michelangelo. If you will look at some of the Egyptian knives--some of those butcher knife looking arrangements--they didn't miss a flake. But I always find some little imperfection in mine. If you look at that margin, the serration, I don't know yet how they got inside enough to pop up a little cone from there. That made a most beautiful serrated edges, and they didn't miss once. It was almost like machinery. They were able to make a bracelet out of flint that has the four sides on it, rectangular in cross-section, out of one piece of flint. The scar looks like it passed all the way around and it doesn't slip over and take off the opposite edge. And you can come up with these marvels that are just impossible to believe, like in the National Museum of Mexico City there are two fighting warriors done in a piece of flint probably, oh, 12 inches long and I don't know how wide, but all of the details are flaked into that. There is also a single pressure blade probably 11 inches long with parallel scars running down both margins. Each one of them meets exactly in the center *without an error.* The artistry in some of these things is simply unbelievable. I don't know how they made a parallel sided polyhedral core that would appear to be like a pencil and maybe 10 inches long without talking the end off of the thing and keeping it from spreading. There's just one mystery after the other. We've never gone into the lapidary arts--the exploration of how they handle all their jade. By the same token all of this drilling--they would take an obsidian cup, a mug, and carve a monkey for the handle. With obsidian if anything contaminates your abrasive, it will cause scratching all over the surface. Everything has got to be done by all of the different stages in turn. Like the earplugs that they had; no one has ever replicated one. Somebody thinks they have the short cut how they were done, but take a quartz crystal skull, life size, and try to cut this out. They say they hadn't the wheel and axle but I've wondered what they had for polish and all the rest of it. I don't think it was just all time and patience. I think they knew all the shortcuts then. They had great skill in doing these things. Take one of these 38 in. obsidian swords, like they have in Southwest Museum. It would be some project to use the core technique. Someone should get that thing worked out. Then there was this core from India, scarcely a little over a centimeter in length. In preparing the platform for that they took off little bladelets that were an eighth of an inch across and full length. Then they used that as a platform for the top of the core. But how they held all of these things, I don't know. The amount of mechanization that went into those things must have been considerable. The moving of those Olmec heads that weighed 80,000 pounds across 70 miles of swamp and river was a marvel. Why, we haven't even started to go into the primitive technology. And I think we should also incorporate bone technology. Here locally we found some chips and bones that had polished edges that show they were using bone tools. This is a whole different set of technologies, of course--to be able to chip bones.

I am trying to think, but it's mind boggling, the things yet to do, the skills still to be accomplished. We'll know so much more about it when we are able to replicate them. Many of these we'll probably never be able to replicate. We may work on them for the next 20 or 30 years, you know, and still not accomplish what they were able to do. So I don't know, Errett, how many different things like this, how much change in thinking is needed. I feel we should leave a lot of this open-ended before finalizing that certain things had to be done this way or done that way. All things may point to its being done this way at this time, but it may change, you know. Complaints, I don't know; I haven't any complaints really.

I think cooperation is the main thing. There have been one or two that want to be macho about this. They don't seem to want to give their information to anybody else. It's like Cro-Magnon pulling the bear out of the cave by the tail. It's a kind of macho sort of thing with no gloves, no glasses, or anything else, to be able to make stone tools in front of somebody. But I do think that we should cooperate a lot more.

Now one of your readers that I was so pleased to see you get ahold of was Richard Warren. He is a man with great skill that certainly ought to be recorded. I had always hoped that he would have some detail on McCormick's work, too. I think a lot of these knappers. There's another friend of Richard Warren's--Jack Putnam--formerly of the Museum of Natural History in Denver. Here locally there is Jim Woods. He should be a subscriber. He's become very interested. There are a number of people here that I think are certainly potentially contributors. I think we should all get together and discuss things. Sometimes we can put two things together and come up with a third, by comparing ideas, just throwing different thoughts around. Why, there are a lot of things that can come out of this. You know, like a method of holding--what are the possibilities and the new innovations in order to secure cores and things where they could be released fast? In making blades you can spend half your time tightening up your clamps to hold the core. It breaks your back. If it loosens, tug and pull to change it immediately would save a lot of time. But I have broken a lot of metal carpenter vises that were not as good as a couple of poles spliced together. If I don't get enough pressure I can make the poles longer.

I'm very excited about what *Flintknappers' Exchange* is doing. I think you've made a great contribution, and it's not only all of the many friends that you've made--it's what we've learned. It's like in our field schools--they come from different areas and they have different materials with them and there's the interchange of information. That cross-pollenation is of such great value to all of us. And I've gained a great deal from the different pieces. Too bad we can't get more knappers to write down some of their sidelights and experiences.

Instead of stereotyping so much of our literature, I think sometimes a little humor and interesting sidelights would take a lot of dryness out of a lot of our archeological papers and could make it more exciting. I hate to see them chop out all of the little sidelights.

E. Well, Don, what other topics have we admitted that you would like to get on the record?

D. Errett, you've got to come out so we can get together here and pound some rocks. Things will come up while we're doing it, you know. After this interview, I'll probably think of a hundred and one different things we haven't mentioned, things that I'd like to see done. I'd like to see Alaric Faulkner continue on with his experiments using the hydraulics gauges and pressures and so on. I like to see Are Tsirk combining engineering with his work of fractured materials. I think this is a great thing. And Barbara Purdy carrying along with her heat treatments. There are so many, I just can't think of all of the different ones doing different things. This is all very important. And this field has barely opened up. I feel that there are probably a lot of these--I wouldn't call them amateurs but I'd say non-professionals--that could certainly assist the professionals in flake studies where the flakes could be recovered and so on.

Material studies haven't been explored a lot either. And we have such a variety of materials. We're doing a little on obsidian now for trace elements, but I think the other materials are going to play an important part. The better the material is the further it's going to travel from its source; I think that ultimately trade routes can be traced. The lousy material probably won't get too far from the site, you know.

Oh, one thing I would like to add is a lot of tools you know seem to be very crude, but how can you tell whether it's crude unless you try it and you're familiar with the material? The materials will look ideal and look beautiful but when you go to work 'em, they're just impossible. If it's perfectly lousy material, no matter how skilled you are, you can't get too much out of it.

E. Yeah, you can't make solutrian laurel leaf out of brick.

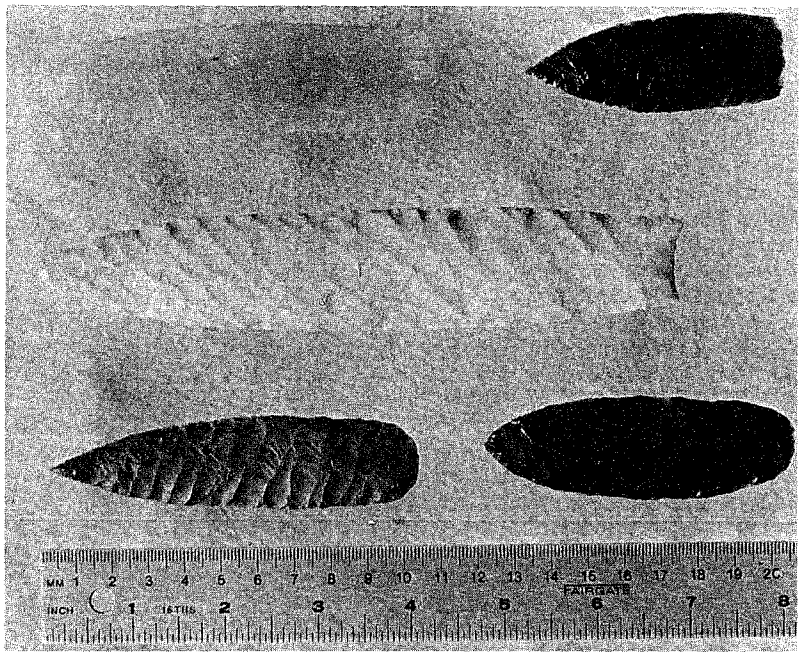
D. Yeah, that expresses it real well!

Figure 14. Some Replicas by Don Crabtree

Top left:
Lanceolate
Hafted Idaho Jasper

Center:
Lanceolate
Construction glass

Top Right:
Lanceolate
Heated Tube
Agate



Left Bottom:
Lanceolate
Peacock Ribbon
Sheen Obsidian

Bottom Right:
Chevron Lanceolate
Byrnes Obsidian

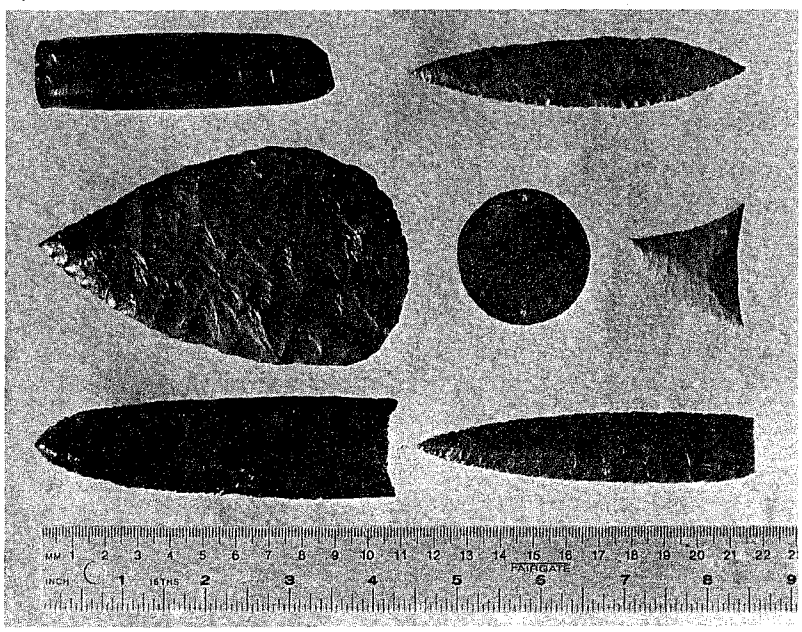
Figure 15 Some Replicas by Don Crabtree

Top Left:
Cumberland Fluted
(Hammer and Anvil)
Banded Obsidian

Center Top:
Pressure Biface
Mahogany Obsidian

Center:
Perforated
Erailleur Flake
Obsidian

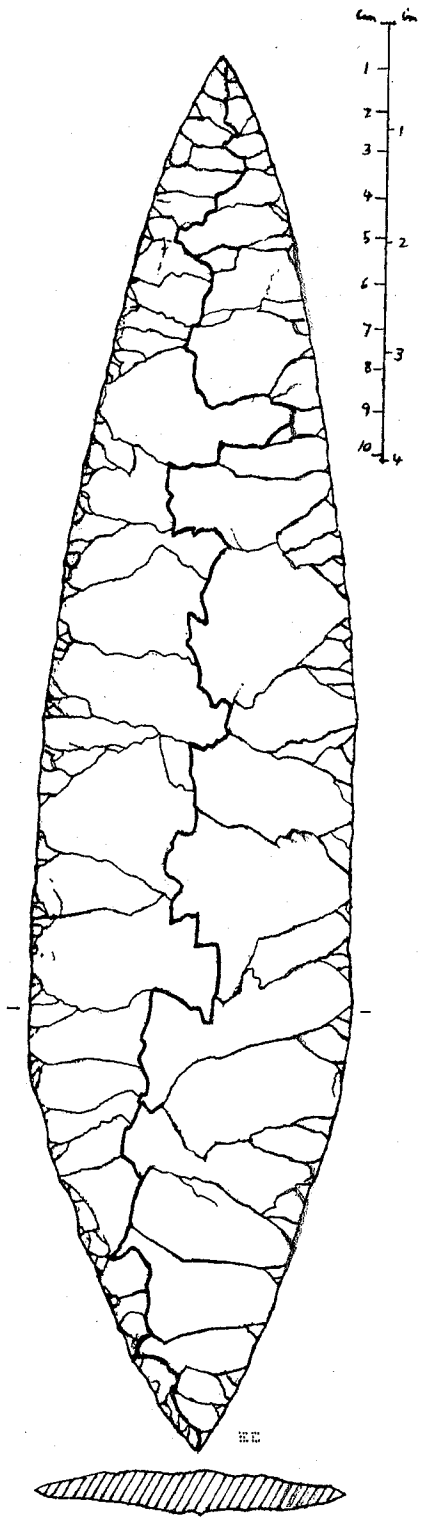
Top Right:
Polyhedral core
Guadalajara
Peacock obsidian



Bottom Left:
Collateral
"Thirdview"
Obsidian

Center Bottom:
Serrated Point
Harrison County
Indiana Flint

Bottom Right:
Lanceolate
Mahogany Obsidian



Editor's Note:

We thought readers might like to see the Frontal View of the Longitudinal half of a Solutrean laurel leaf #2 from Volgu, France, which appears on the *FE* cover.

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