

David G. Anderson

BULLETIN 9

SOUTHEASTERN ARCHAEOLOGICAL CONFERENCE

Proceedings of the
Twenty-Fifth
Southeastern Archaeological Conference

Edited by
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Morgantown, West Virginia

1969

EDITOR'S NOTE:

The Proceedings of the Twenty-Fifth Southeastern Archaeological Conference, held at the University of Tennessee's McClung Museum on November 8-9, 1968, are being presented in a slightly different format than previous proceedings. The chairman of the conference, Alfred K. Guthe, requested an informal type meeting with no "formal" papers being presented. It was hoped that everyone would participate in the discussions with their comments and questions. The topic for the meeting was "What can we learn from Lithic Technology?" This "informality" has been carried through in this Bulletin, with the meeting being reported in its entirety.

After the tapes were transcribed, copies were sent to the participants with a request not to rewrite their comments into a formal paper. Many of the questions and comments have not been altered. Several sections of the tape could not be transcribed because of background noise and this fact has been noted throughout the Bulletin.

Additional papers by Faulkner, Kraft, and Broyles, not presented at the meeting but applicable to the subject under discussion, are included in the Bulletin.

You Editor wishes to apologize for the lateness of this Bulletin, but it is sometimes difficult to get the participants to return the corrected copy of the transcribed tape. Hopefully, we can do better in the future.

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PROGRAM

THE TWENTY-FIFTH SOUTHEASTERN ARCHAEOLOGICAL CONFERENCE

SESSION I (Friday Morning) GENERAL SESSION

Chairman: Alfred K. Guthe, University of Tennessee

"Lithic Materials and Technology"

John Witthoft, University of Pennsylvania

SESSION II (Friday Afternoon) GENERAL SESSION

Chairman: Alfred K. Guthe, University of Tennessee

"Prehistoric Lithic Preferences: Aids to Site Analysis"

Don G. Wyckoff, Oklahoma State Archaeologist

"Some Comments on Hinge Fracture in Fluted Point Manufacture"

Richard A. Marshall, Mississippi State University

SESSION III (Saturday Morning) SUMMARY AND CONTRIBUTED PAPERS

"Summary of Friday Session"

Alfred K. Guthe, University of Tennessee

"Problems in Physical Anthropology"

George K. Neumann, Indiana University

SESSION IV (Saturday Afternoon) CONTRIBUTED PAPERS

"Sluicing System Used at the St. Albans Site, West Virginia" (MOVIE)

Bettye J. Broyles, West Virginia Geological Survey

The Twenty-Fifth Meeting of the
SOUTHEASTERN ARCHAEOLOGICAL CONFERENCE

Members that attended

Stanley Ahler	Anne Gatewood	Charles R. Nance
Betty Ann Anderson	Marjorie (Mrs. Robert) Gay	Robert S. Neitzel
Norman F. Barka	J.B. Graham	Robert W. Neuman
Joseph L. Benthall	J. Bennett Graham	George K. Neumann
G. Gerald Berry, Jr.	Joseph E. Granger	Jerry J. Nielsen
Thomas Birohet	Alfred K. Guthe	Carey B. Oakley, Jr.
Susan Bodie	David J. Hally	E.M. Parker
Ursula K. Botbye	Carole E. Hill	Daniel T. Penton
Oscar Brock	Marjory Honerkamp	David S. Phelps
Paul E. Brockington, Jr.	William Hony	James H. Polhemus
Samuel Owen Brooks III	John B. Huner	Karen Rogers
Bettye J. Broyles	Harold A. Huscher	Martha A. Rolingson
Adelaide K. Bullen	Brooks Hutto	Thomas M. Ryan
Ripley P. Bullen	Stephen Israel	Fred Schneider
Joseph R. Caldwell	J.R. Jackson	Kent A. Schneider
Jefferson Chapman	Edward B. Jelks	Frank T. Schnell
David W. Chase	Judy Jelks	Jack M. Schock
R. Burle Clay	Jay K. Johnson	Gail Schroeder
Kenneth W. Cole	Calvin Jones	Lanier A. Simmons
John D. Combs	John Keller	Edward M. Simmons
John M. Connaway	A.R. Kelly	Gerald P. Smith
John L. Cotter	Thomas H. Koehler	Edward Lee Spence
David L. DeJarnette	Edward B. Kurjack	Robert L. Stephenson
Chester B. DePratter	Bob Lefaye	Noel R. Stowe
Stephen Deutschle	Shelia Landreth	Donald Sutherland
John T. Dorwin	Samuel Lang, Jr.	Jeivis D. Swannack
Boyce Driskell	Barry Lewis	Robert M. Thorne
Charles H. Faulkner	Howard A. MacCord, Sr.	E. Bruce Trickey
Richard D. Faust	Ben C. McCary	Wyman W. Trotti
Jason Fenwick	Samuel O. McGahey	Kent Vickery
Leland G. Ferguson	Charles H. McNutt	Robert Wauchope
Paul M. Fink	D.M. Mackintosh	Ray Williams
George R. Fischer	Marnie Mandeville	John S. Witthoft
Vincent P. Foley	Marilyn Ann Manos	Frances Witthoft
Janet Ford	Richard A. Marshall	Don G. Wyckoff
David Frayer	William H. Marquardt	Richard A. Yarnell
Frank B. Fryman, Jr.	Gordon M. Medgette	Jean Yarnell
	Mrs. Gordon M. Medgette	

SOUTHEASTERN ARCHAEOLOGICAL CONFERENCE

SESSION I

Friday Morning, November 8, 1968

JOHN WITTHOFT:

By flint technology, I mean essentially the technological study of flint tools, as opposed to the typological study of them or the attempt to reproduce them, which is one part of a field called experimental archaeology. I have gradually been drawn deeper and deeper into this problem of the flint tools, in trying to get back of the typology and into more significant realms where we can deal better with the flint tools as factors in culture evolution, as factors in technological evolution, as factors in culture history, and as factors in human adaptation. I started out by getting myself imbedded in this bog by trying to deal with archaeological cross-ties, with trade pieces from one area into another; such as, for example, the so-called Copena points from central Tennessee that are found in sites of certain ages all the way across the north, from Illinois and Indiana to Delaware.

It seemed to me at the time that a type description for a flint tool had to be multi-factored after the manner of a pottery type description and that we could not deal just with morphology and know that we were making a cross-tie, but that we also had to come to grips with the specific stones that were used and the preference for specific kinds of stone as a culture factor involved in the type. We had to deal with what the tool was used for, how it was used, and what its history was when it was in use. This would be somewhat parallel to this business of a pottery type having to be defined not only on the basis of style, but also on the basis of form, temper, and paste. The more factors we can bring into the description of an artifact, the more ways we have of comparing it and contrasting it with other individual artifacts. I am not opposed to the traditional typology and traditional typological concepts, but to me, as to Jim Ford and to many other people, the type concept is a tool, not an end in itself, and I refuse to define or name a type until I have some reason for doing this, until I need such a definition or tag to fit a specific problem. It would not concern me in the least bit if somebody else working with the same group of material would set up completely contradictory types and use them in an entirely different manner. I should think both would be equally valid.

What I got drawn into was something that has been developing for a long time and going off into two widely different directions in the archaeology of Europe. Our typology, our typological concepts, are derived from French typology which started to become a powerful tool in the 1870's. French typologists dealt with artifact types as though they were fossils, and were using them in a way that a paleontologist uses fossils to plot his stratigraphic sequences. This is something that we shall forever continue to do. It is a powerful tool. French typology, through the years, has become more and more sophisticated in description, more and more refined, more and more stylized, more and more the compilation of tremendous catalogues of attributes and elaborate statistical

methods of dealing with them, until typological procedures, as they are being followed by some in France today, seem to have had a great deal of their cultural meaning squeezed out of them. When we look at modern French formal techniques for classifying burin types, we realize that we have gotten ahold of a modern cultural horror and that the French archaeologist has lost sight of what would be more natural and simple methods of classifying burin types. He is like the stamp collector who is hunting for dot, flaws, and breaks in a printing plate. One reason why I say that all the cultural meaning has been squeezed out of French typology is that this intense concentration on formal classification and the mathematical juggling of attributes so focuses one's attention on one kind of attribute that all sense of use and cultural significance seems to be lost. So we have the Binfords coming to deal with the Mousterian. From this point of view, and instead of studying the technology of the tools directly, they are doing all sorts of complex statistical things to try and put them together into tool use groups without ever looking at the tools themselves to see what evidence there is that they are parts of tool kits, that they may be complimentary groups. Binfords' papers on Mousterian, to me, are sort of a "bankruptcy" of the final stages of French typology. On the other hand, starting in the 1870's, the British student Stephens, in his excellent book Flint Chips, which was really a catalogue of a museum collection, tried to bring together into one book notes on what he thought ancient tool types were used for, accounts from the traveler's literature of stone tools that had been seen made and used in many parts of the world in his time. First hand observations, like the present day Australian ones, and comparisons between the ethnographic technologies, the industry of the gun flint shop, and the archaeological stuff of the past. Somehow or other, the title that he chose for this book (Flint Chips) has always seemed to me to be a stroke of genius, because he is the first man that looked at chips as seriously as he did tools. I do not know of much following Stephens in England, but in Germany Ludwig Pheiffer, in 1910, published his Technology of the Stone Age: Early Times and Classic Times, and Pheiffer is really the father of modern technological studies. He dealt with the classic tool types of France and Spain. He went to the men who made gun flints and learned from them different techniques of blade making, and studied the details of the flaking scars on the blades that they made and that he made. He took this study of a modern steel age technology back and showed its clear similarity to the flint chipping technology of Upper Paleolithic times. He went beyond this and studied to some slight extent wear marks on the edges of tools. He went back to the peasant crafts of Europe and the primitive industries of other parts of the world and tried to make meaningful comparisons in use between the tools of the traditional crafts, both European and exotic, and the tools from ancient times. He studied the whole tanner's industry, for example, in back-country parts of Europe, where he considered that it represented a survival from primitive times, even though the tools were now being made of materials other than stone, and tried to find equivalent tool kits in the prehistoric material. Pheiffer's book seems to have had almost no effect on anyone. You almost never see a reference to it, and it is an extremely rare book. I had to pay \$40.00 for it some years ago. The only citation I have seen to it is in the modern Russian literature. Apparently Pheiffer deeply influenced Russian students of the Upper Paleolithic and had little effect in Europe or America.

At the time when I had become deeply embedded in this whole broad set of problems, tool type by tool type, there appeared in about 1960, S.A. Seminov's book in Russian on Prehistoric Technology. When my friends in the Arctic Institute first called my attention to this new book in Russian, and Henry Michael, who reads Russian, scanned some of it for me and I looked at the illustrations, I suddenly realized that some of us in this country and some of us in the U.S.S.R.,

without any communication what-so-ever between us, and neither one having any knowledge of the existence of the other group, had been proceeding in a completely parallel manner to extend Pheiffer's and Stephen's techniques through microscopy, through the analysis of resharpening traces, through the analysis of breakage patterns, into something that reached far deeper into culture than did mere typology. Fortunately, in 1964, M.W. Thompson, a British student of the Upper Paleolithic, produced an excellent English translation of Seminov's book which is still in press, and which is one of the great landmarks in the history of the study of stone tools. I've talked to people who have visited Seminov's laboratory, and they are impressed with the enormous amount of work being done with these tool types in Russia today, and the laboratory facilities and the very large resources that are available to Seminov and his circle of students. We know of no place in Europe or in America where there is anything but a token program of the same sort of study going on. This is parallel to the great Russian advance in pollen studies, for example, as compared to the extremely inadequate programs in Palenology that have so far come into existence in North America. So, I keep trying to draw people into this type of approach to stone tools, and maybe I should say something about what the approaches are without getting into any great complexity of the methods involved. In all of this there is nothing esoteric, these types of studies are really the simplest things in the world, provided that one always reaches back to sound advice and sound guidance in the realms of petrography, chemistry, physics, and mechanics, and provided that one deals with proper samples.

This sampling problem is extremely severe. My friend John Guilday, the mamologist who is doing such a tremendous job on the Pleistocene, tells me that archaeology has come into a new awareness. He said we used to save all the stuff out of the sites that is worthless intellectually, like whole spear points, pot sherds, bannerstones, and etc., and we threw away everything that was of any value. He says now we are getting to the point where you can ignore your spear points, and your bannerstones, and your pot sherds, because these sites have dirt and they have garbage in them. From the garbage you can get a mammalian assemblage which gives you a picture of the surrounding environment, and from the dirt you can get radiocarbon dates, you can get a pollen sample which gives you a picture of the environment, you can get the vegetable details that pertain to the diet of the people, and he says you can keep your damn spear points, all he wants is the garbage and the dirt out of these sites. Something very similar prevails in the sampling of archaeological sites. It is in this area that the survey collections, and particularly the collectors of relics, have done us the most damage, because we can not obtain skewed and unskewed samples from the surface of most sites. When we go to the work of older excavators, we discover that the sample of the flint tool technologies available to us from excavated samples are in most cases worthless for our purposes. They have been selected, the debitage has not been saved, by and large all that we have are whole flint tools, or large fragments from them, particularly broken bases. In many cases, as with Moorehead's samples, we don't even have the broken pieces. They were discarded in the field.

In the study of technology of ancient times, the broken fragments are far more important than the whole specimens and in many cases the very tiny broken fragments are more important than the large pieces. The debitage often includes damage chips that have come off of artifacts in use, they include specific types of retouching and resharpening flakes which show us something about the tools themselves. The resharpening flakes in the debitage carry far more in the way of wear marks than do the edges of the whole tool which were continuously being resharpened. In the Paleo-Indian series, one has almost to

collect every little crumb of flint on a site, because the retouch flakes taken off of the beveled end-scrapers are of such a distinctive form even if they are 1 or 2 mm. in diameter and show us much about the highest re-edged so-called end scrapers that they come off of.

So, we have gotten to the point where we start with a group of things that look like a tool type, but we don't worry about whether it is a type or not, or try to define it as a type, but instead, we try to get certain kinds of samples that pertain to this thing that may be a type. We first try to collect a large pooled sample of obvious examples from all sources, not worrying too much about their provenience or what collections they come out of, or whether they are off the surface or not. This would be the way in which I pooled 3,000 surface collected examples of the Perkiomen spear point and studied them for re-touching and re-pointing patterns and for traces of edge wear and breakage pattern. This is also the way in which we pooled 1,200 arrow points from the Point Barrow area that pertained to post-contact times, to deal with the study of breakage patterns and re-pointing patterns on a type that was known to be an arrow point in the Ethnography and which existed in old specimens as a part of an arrow. This is not enough though. We must then go back to sites where this so-called type exists in abundance and make completely random samples. And by a completely random sample I mean absolutely everything. This often means staking off an area within a site that has been plowed and is in good condition for surface hunting and taking from it every fragment of stone of artifactual origin that there is on it, down to the smallest flake. One may have to do this repeatedly over large parts of a site in order to get the rest of the picture that has been warped by the selective collecting of Indian relics. Or, one may be able to go to a site that has some depth to it, where one can find in a stratigraphic segregation specimens of the so-called type that we are dealing with, and excavate a piece of this, taking everything out of it, and by everything I mean absolutely everything. This is far different from any of the older standards of excavation.

I once dug a Susquehannock site, Eschelman at Washington Boro, which was a very big and deep midden, with the tiny triangular arrow points of the last eastern archaeological stage. I was primarily excavating this for a pottery sample and a bone sample. The site was filled with fire broken stone and large river shells, Unio shells, in great quantities. I was dealing with tons and tons of midden in a short time period. I screened everything that I got, but it was impossible to complete this test excavation using anything smaller than one-quarter-inch screen because of the great quantity of fire rock and shell. When I got my sample back into the lab and began to study the triangular arrow points I began to intellectually kick myself, because I found that the bulk of my arrow points were not whole but that they were present as very irregular shatter fragments and I had only the shatter fragments that were too large to go through a quarter-inch screen. I had three of these triangular arrow points that were embedded in deer bones-- they were completely embedded. The ears of the arrow points were at the surface of the bone. We X-rayed them and found that these triangular arrow points within the deer bone were made up of shatter fragments, one was broken in seven places, one was broken in nine. I began to see a little bit of the mechanics of the Susquehannock system of archery.

With an excessively light arrow with a tiny point, and a bow with a strong and an extremely hard cast, used at close range, which thus had as its factor an excessively high velocity, low mass projectile. What I was seeing in the shatter fragments was the consequence of this high velocity, low mass state of the arrow. So I have very little interest in whole Madison points.

When I dug the Sheep Rock and we got nice separate stratigraphic units for Susquehannock and for three earlier stages in Late Woodland, we put all of this midden through very fine screening and we tried to get everything. Here, for the first time, we began to get all the little chunks that broke off of these Madison points, so-called, upon impact with bone. All the stuff that had been imbedded in the carcasses of the deer and that had fallen out in the boiling-up of the bone for the soup and thrown away on the floor of the camp with the dregs from the soup kettle. So, we begin to have a fundamental picture of the small triangular arrow point.

I must tell you that we have gone on to apply the same kind of sampling to the arrow points of other areas, particularly in the last two summers; to the arrow points of the Shoshone of Wyoming, where we have a tiny corner-notched arrow point type used with a comparable bow, but with some slight differences in breakage patterns than those of the small triangles of the east. And as a foil to this we have a very different kind of breakage pattern with the massive Point Barrow Eskimo arrow points, where we have a bow with a very heavy pull, but a logie, slow cast, an extremely high mass projectile, an arrow of large diameter with a heavy bone foreshaft and a heavy bone tip and no feathering, where the projectile, the arrow, is a high mass, low velocity projectile. We believe that the low mass, high velocity projectile of the eastern Woodlands delivers just as much energy as does the high mass, low velocity arrow of the Eskimo. Mass times velocity equals impact energy. But they behave very differently from one another, they evolved in different directions. The eastern Woodlands arrow is like a 22 Hornet, like one of the really high velocity 22's with the big-bottle cartridges. The arrow of the Point Barrow Eskimo is like a shot gun slug. So they kill in a different manner and the flint points break up in a different manner. Neither one of them breaks up in the manner that the point for the spear-thrower dart does, but that is a weapon, a projectile, that delivers less energy upon impact and in which there is a still higher ratio of mass to velocity. The same kind of analysis and reasoning can be applied in many other directions, especially among the so-called projectile point types, and I will restrict myself to them here rather than getting into unifacial tools and more conventional types of knives.

The Point Barrow Eskimo frequently broke an arrow tip in the field when he was hunting and he had a stump of the arrow point left in the arrow. He apparently did not have other arrow points with him, nor the tools or the flint with which to make one, and apparently he did not have the chipping tools with him. So, in this case, when he really needed the arrow, he would do something to it that we call truncation. He would take a pebble and he would percussion flake the arrow point to a tip. Instead of making a long needle-like tip like the original arrow it would take on this form, or sometimes even be chisel shaped with a different type of chipping than had been made on the original point. Now, we see comparable repointing as an emergency in the field all the time on Archaic and other projectile point types from the Southeast and the Northeast, and in any large sample of a type that we lay out. When we start to see a lot of truncation right away we say of course this is a spear point type. This is the way an emergency repointing job is done on a spear point. However, when we lay out many series of things that are called spear points we can not find specimens that show this type of truncation of emergency repointing, but instead their edges show many ranks of reworking. They continue to be shaped with an acute point, without the blunted tip, but they keep getting reduced in size as the edges are flaked away. Well, this is so obviously a characteristic of bifacial knife blades that it hardly needs

mentioning.

One after another, as we have subjected the so-called projectile point types of the east to this type of analysis, our projectile points disappear. For example, in the so-called projectile point types of Adena and Hopewell of the north, which includes such things as the Snyders Corner Notched points and etc., I, to this day, have never seen a flint projectile point. Every single one of them shows more or less evidence of use as a knife if it was in use long enough to take up some wear or to be resharpened. Furthermore, the standard description of these types is based upon a few grave lots of specimens that had scarcely been used. Most of the examples that pertain to these types are un-recognizable to anyone who has read the type description, but when we take huge pooled samples of corner-notched tools that resemble the Snyders type and, with thousands of them, start laying them out in graded orders, we discover that the great majority of Snyder Corner Notched points were used until they were mere stumps, had even lost the characteristics of the front end of the tang by rechipping and had been discarded, and we would never relate them to the same type as those from practically unused grave lots unless we had laid out continuously gradiated series.

The same thing is true of each of the Adena knife types. In the same way, as we lay these out, we discover that the Snyders Corner Notched point completely intergrades into the Adena type Robbins and the Adena types made of Flint Ridge stone (not the others) completely intergrade into one another. We are obviously dealing with some complex problem in culture continuum in flint working, as well as the continuum within a technology of a tool that is constantly being reduced as it is being re-edged. Now this leaves us without any projectile points in Adena and Hopewell, unless we can recognize some bone or antler ones, and I have not seen them yet. So it begins to look as though the Adena and Hopewell people were much like some of the Archaic-Early Desert people of the southwest and some of the Basket Maker groups who made all of their projectile points from wood. And this is not an un-common situation in ethnology. Copena in the southeast, which I have studied in a far more limited way, shows the same features. The so-called Copena points that I have studied all show evidence of knife use and knife retouching. And there is not a spear point among them. We can simply eliminate them from our projectile point typological schemes.

CALDWELL- Couldn't they have been used for both?

WITTHOFT- If they are used for both, and we do have such series, they show both types of repair. They also show two contrasting breakage patterns as well, and we do have series that were used for both. There are differences in the way a spear point breaks up on impact with bone, and a knife snaps when it is wrenched. On an individual specimen this is not a diagnostic feature. You need several thousand broken pieces to deal with the hinge break and to deal with the tiny landmarks on the breakage scar that permits you to sort into two groups: impact breakage and snap breakage. This is a further subtlety, whereas the truncated and edge reworking are quite obvious. The details of the fracture scar are a little more subtle and difficult, but not esoteric. I am not able to show them to people without samples in front of me. I have to be able to point to these minute differences in scar detail and curvature of the flake break, of the break.

BENTHALL- Have you done any work with the so-called pentagonal points?

WITTHOFT- I have dealt with the pentagonal forms only in the northeast, from Virginia to the north.

BENTHALL- For example, Coe's Pee Dee points.

WITTHOFT- Coe's Pee Dee points, to the extent that I know them, are arrow points. Now I must introduce one word of caution into this business. The Iroquois, the Owasco, the Shenks Ferry people, the Pee Dee people, have a rather restricted and even impoverished flint industry, in which a arrow point is the main factor and the other tools are the junkiest kind of poorly made by-facial and uni-facial scrapers. Not at all like the refined Archaic scrapers, but when we study large series of these Late Prehistoric triangles or pentagonals, we find that there is a small percentage of them that are typologically indistinct from the arrow points but were used for something else. Most often we get ones that have been put in a slot in the side of a large wooden handle, and have been used as knives, as cutting tools, and on one side of the triangular arrow point it is deeply worn and sometimes re-edged. In other cases we have specimens where the triangular point has been driven into the end of a handle and the hollow base or straight base of the triangular point has been used as a knife.

BENTHALL- Is there any evidence as to what the type of point would be?

WITTHOFT- No, they are identical. They are simply an arrow point that might have been taken off an arrow in the quiver and made into a different kind of tool.

BENTHALL- This is what I was wondering. If there was any relationship in the triangles that you describe from the Eskimo.

WITTHOFT- The Pee Dee points are not truncated. They were made that way and this is a local excentricity or a localized evolutionary development with the eastern triangular arrow point groupes. It is really more widespread than the Pee Dee. It spreads up into Virginia quite a ways, but I don't know what its cultural associations are there. Now, there are many earlier pentagonal forms that can sometimes be confused with the Pee Dee Pentagonal. There is a group of pentagonals that fall in Late Middle Woodland times, with the greatest concentration in eastern Pennsylvania, and they form a transition between the corner notched Jack's Reef point of Middle Woodland times and the triangle. They are evolving into the triangle, and they differ in minor but very significant details from the Pee Dee Pentagonals. There are some Archaic pentagonals which also are not truncated that differ in their flaking style and other things from the Pee Dee ones. The Pee Dee ones are rather a crisp unit to me, but again, with them as with the small triangular arrow points, you have a small percentage that show use as something else than an arrow point, but in their typology they are not different.

CALDWELL- The differences that you see in the use-marks of these specimens reflects their use. Can you reproduce these experimentally?

WITTHOFT- Sometimes I can, sometimes I can't. I am not terribly interested in experimental archaeology and what flint chipping I have done indicates that I have little competence at the hand skills. It would require a good many years of apprenticeship to these crafts, such as Crabtree has spent, in order to gain the hand skills that are not comparable but could approach those of the ancient people. I am too interested in other problems to spend all my

time in the training of my hands and muscles and eye to the very, very skilled performance that we see in these ancient flint tools. So, in what flint working I do, I try to do experimental stuff that is reduced to the simplest mechanical problem in any detail. Such as, what is the difference between a flake drawn by pressure and one drawn by various kinds of hammers, simply on a blank or on a piece of flint debitage, rather than any attempts to make a tool. In this I have been impressed with the difficulties of making judgements of this sort. Several different techniques can produce apparently identical results. The same technique used by two different people with a different training can produce quite distinct looking results. So, I am unwilling to guess as to how a tool was flaked and what tools were used in making it until there is a complete technological study plus a complete tool kit with bone preservation in which it may be possible to identify all of the chipping tools. Now, I think we can do this with Hopewell, for example, but I have not gone back to look again at all of the excavated samples that I have seen that include the things I think are chipping hammers of antler, pressure flaking tools and stone hammers, but from what I know of Hopewell in its bone and stone industry, there are at least four flint flaking techniques blended together within it, and there are many, much more severe, problems involved at this level. Here I think we often jump at conclusions, and say this is pressure flaked or this was done with a crutch technique, or this was done with an antler punch and a hammer, or this was done with a hammerstone, when, really, there is a much greater variety of techniques that might have been carried out and then so much lies in the great perfection of skill, the real art in the hand and eye of the man who made these ancient tools. I am impressed with the skill that some of the forgers show in their crass commercial manufacture of Clovis and Folsom points, and etc. I am also impressed with the extent to which they use tools, like the arbor press, that were never available to ancient man. I am impressed with the variety of different techniques that each one of them uses to produce the same result and every one of them is sure that he has discovered the flaking technique of Folsom man. I am also impressed with the shabbiness of their work as compared to that of the ancient people. I have learned a lot from the forgers I have known, but most of it is negative knowledge, in more than one sense.

BENTHALL- Is it possible that some of these earlier people could have had a number of techniques that could produce the same thing?

WITTHOFT- It is possible, I don't know. There are a few techniques that we think we understand, where we think we know the hall-mark of the technique if we can see it on the tool. One is Clactonian technique, which is important in practically all of the North American Indian cultures. Clactonian technique is a fact, but the technique can be carried out with several techniques and tools, and there are a number of ways of doing that. It is like what we can see from the work of the Australian natives in the Kimberly Mountains, where they practice several different kinds of pressure flaking.

VOICE- Question concerning the meaning of Witthoft's statements about the abyss of French typology (part of question was lost at beginning of tape).

WITTHOFT- Well, it starts by following Bordes' classificatory devices and attempts to refine them through attribute analysis typologically. Then it involves listing these types in their instance of occurrence at different sites and using statistical devices to show that a site includes certain of these types in abundance and few of others, and this other site includes certain other types in abundance but some in small numbers, and then comes the attempt to cite that this place is a place where they were primarily working skins, or

that this other site was primarily a hunting camp, and therefore the difference in the parts of the tool kit that are left behind. Now this is trying to get at functional things through typology rather than technological studies. And this, to me, is carrying the over-refinement of French typology to its extreme and I think the attempt is a failure. As much as I understand Binford's analysis of this, it is still unworkable crap. He has to bring other analytical factors besides typology into this. I should not overstate this objection. That this is the bankruptcy of French typology because French typology is an excessively powerful tool for stratigraphic work. It is as refined for one particular purpose as the use of index fossils by the paleontologist is refined for one specific purpose. And this is not all of paleontology or all archaeology: it is rather a narrow tool. At the same time nobody has ever taken Mousterian reclaims, for example, and studied them for their series of edge retouching, beveling, and constantly resharpened, and one tool type becomes another. No one has taken Mousterian points and laid them out in seriation according to their edge retouch and shown that these types are all within something for which the Mousterian point is the initial part of the series. Just the extent of Mousterian retouching itself is a very very important culture history document, as compared to the Lower Paleolithic. The whole implications in human evolution come from flints themselves.

I am being a little unfair, because in many ways I naturally admire typological attempts after the French manner. I have myself been deeply influenced by French typology, but I think it is so narrow a tool that takes over and blinds us to use-work, problems in archaeology, and makes it difficult for us to reach beyond and below typology into other culture questions. For example, in the Fallen French rock shelter, Le Micoque, apparently, obviously on a pre-Mousterian level, for the first time in the world as far as I know, you get massive beveled edge tools that show many stages of resharpening and re-edging. This is one of the great revolutions, in human evolution and in tool making, when you don't throw the tool away when its raw edge becomes dull, but you systematically, in a certain tradition, resharpen it and resharpen it. Obviously they are carrying it around for a long time. Then when you get into Mousterian stuff you have a great richness of re-edging, so that, and this re-working blurs any typological distinction that can be made valid in classical typology. Of course, when we get up into the later stages, like the Upper Paleolithic, or the North American Archaic or something of that sort, we have a great richness of re-edging and we have tools that are being used to the limit until there is nothing left of them. This is one of the great trends in evolution and has biological and cultural implications. Now this is the kind of thing that has been traditional for many thousands of years of human life.

COTTER- Perhaps we need a more extensive compilation of observations from Ethnography about the use of tools, first hand observations. These are very limited but there are some.

WITTHOFT- They are very limited and we have tried to use them and we have tried to extend them. For example, Belcher, in 1830, saw arrow points being made in Alaska by the Eskimo and described the process. It is a rather sophisticated type of pressure flaking. We can identify in the Ethnographic collections and in the archaeology the kind of tools that Belcher saw being used. We know what the chipping tools were. We know the type of types that were being made in his time, and we have them on Ethnographic arrows, and we have them from dated grave lots, so we segregated 1,200 Point Barrow area Eskimo points of this type. The Eskimo of today can't tell us anything about this, but the things can speak for themselves. We can see the tools, we can see the

end results, we can see how they were used, we can see the effects of a certain kind of use, we have the bows, we have the whole arrows, so we can do something with the mechanics. In the same way, the Australian stuff can be extended by taking McCarthy's observations in the field and carrying them to the Ethnographic collections that were made at a somewhat earlier period, and apply McCarthy's observations on work against the observations made by earlier students and the picture can be greatly enriched even when the Ethnographic material is scant; it can be extended. This Ethnographic allying field is one of the most important laboratories.

COTTER- There is another point I would like to just mention here for what it's worth and that concerns the time that the lithic tradition is supplanted by the use of metals. At that time you have a carry-over in usage that is sometimes not related to the new material. My point is this, at that particular time they have interesting adaptations of the lithic technique to the new material which is not perhaps being properly used but it is bearing the relics of the past trait; for example, there is the crooked knife of the north which is still in use. You can see how it is used and yet you can see the tradition of the crooked knife back in the lithic tradition.

WITTHOFT- It comes right across northern North America, and right across Siberia, and so on to northern Europe. There are two kinds of crooked knives. There are those of the Athapascan people of western Canada and Alaska which involve a very clever way of setting a beaver tooth into a handle to use as a carving tool. Now, on the other hand, there are the steel bladed crooked knives which are so much alike all around the Arctic and in the Bering Straits area. We have evidence for steel burins, steel crooked knives, and steel adze blades, having a continuous history back to the time of Christ at the Bering Strait. So that these stone arrow points that we study from the Bering Sea area and earlier arrow points of the type are in existence along side of a metal age technology in steel. Mason came to grips with this problem in the 1890's with his distributional study of the northern crooked blade forms. We are no closer to a solution than he was. How much transference there is to steel from the beaver-tooth knife, how much diffusion there is of actual steel tools in Pre-Columbian times. We know that steel was in Alaska as well as in Siberia for a long time before any recorded history. Here we get into another interesting business when we try to identify the mark of a specific tool in the carving marks on ivory and bone, which is another false set of technological questions.

(QUESTION FROM FLOOR CONCERNING STEEL COULD NOT BE TRANSCRIBED BECAUSE OF NOISE)

WITTHOFT- The Bering Strait metal edges of antiquity are steel, not soft pure furnace iron or natural iron. These specimens have been analyzed. They are not natural minerals, and they are not soft blast furnace iron.

(QUESTION CONCERNING ORIGINS)

WITTHOFT- According to the Chinese historians, the Sythians among peoples of Siberia, were making iron in early times, but had lost the art by the time of Genghis Khan. So that the source of this iron is some place in Asia, along the trade routes, up into the Bering Straits. The trade routes that the earliest Russian explorers followed up to Bering Straits were already in existence among the natives. Where is our one great example of New World-Old World contact? Right across Bering Straits. This is one which there is no question. It's steel made in Asia.

BULLEN- How can you tell a repointed (truncated) point from one made that way?

WITTHOFT- Working with the middle Archaic complexes in the Susquehanna Valley, a comparable situation. When you put together a very large series and work out with the measurements on them, they form two separate groups, which did not overlap. One as they had been made and one that had been repointed after break-age.

QUESTION- What is the history of resharpener of a flint knife?

WITTHOFT- Very often something started off as a great big knife, and you see this especially in the Ohio Valley Archaic, and it got resharpener and resharpener until eventually it was about this long and shaped like a drill, but it doesn't show any rotary wear, it shows knife wear. Many collectors would call it a drill. After awhile this drill blade part becomes dull and they snap that off and put a scraper bevel across the face of it. This pattern holds for at least five or six of the Ohio Valley Archaic bifacial types; the end product is a notched end scraper.

BENTHALL- Has anybody come up with a plausible explanation of beveled edges?

WITTHOFT- Oh, there are invariably knives. This is one of the patterns of knife re-edging in many cultures, and this is particularly characteristic of many of these Ohio Valley Archaic types. It is typical of the Dalton knives, it is typical of the bi-pointed knives, bi-facial knives of very late times, Mandan bi-pointed, Dismal River, etc. What happens is that the hafted tool is held in the hand in the most convenient way, probably by the handle, and re-edged. The direction of the bevel shows that, and not turned back this way. The beveled edge is just as good a knife edge.

BENTHALL- None of these would really be projectile points then?

WITTHOFT- Not the ones I have studied, because they show too much use mark, they show too much wear. Now you lay them out in series of progressive resharpener and they make sense. Incidentally, they show an incidence of about 4% of left-handedness.

QUESTION- Have you studied basal grinding?

WITTHOFT- This, I think, is a technique for dulling the basal edges so that it doesn't damage the hafting, the wrappings, the fingers, and it shows certain interesting chronological and use correlations. In the Paleo-Indian stuff it is on everything that was hafted; in most cases in the Archaic it is a characteristic on knife blades rather than projectile points. I wouldn't say all of these, but, quite generally, it is a feature of Archaic knife blades. It is characteristic of Adena knife blades, but not of Hopewell knife blades. In the transitional of the north, it is typical of both projectile points and knife blades. In the Savannah River I don't think it occurs.

VOICE- I have seen some in Savannah River.

WITTHOFT- It is rarely present after Early Woodland in the eastern woodlands.

BULLEN- We have both beveled and unbeveled in Florida.

WITTHOFT- They all start out as unbeveled tools. The beveled knives start out not having beveled edges. Of course, sometimes you may get bi-facial resharp-

ening. This beveling business is interesting. On the Eastern Plains these big double pointed knives, called scalping knives, are invariably beveled-edged-resharpened until they become diamond shaped stones and the tips begin to look like drills and they break up. On the Western Plains, as in our Wyoming samples, they are bi-facially resharpened and never become diamond-shaped. They are greatly reduced in size, but they continue to retain their form right to the end.

QUESTION- Are there no drills?

WITTHOFT- Many of the things we call drills are really the stages in the reduction of a knife, and the wear traces have to be studied to determine which use they had.

SMITH- Are there any Ethnographic examples of the use of projectile points as a knife?

WITTHOFT- The only sound Ethnographic examples that I know of is Bushman in South Africa. Krieger, in one of his papers, claims that the Eskimo of Bering Strait used the arrow as a butchering knife. As far as I know there is no such statement anywhere in the Eskimo literature. Nor do the Eskimo tool kits suggest any possibility of this.

QUESTION- Aren't they ever the same?

WITTHOFT- There are some Archaic series that show both, where the projectile point or spear point seems to have been used as a knife. We have some very puzzling examples from the Basket Maker sites, in which the dart foreshaft had been used as a knife handle and one of them has a flint at each end. It is a re-used foreshaft from the dart of a spear thrower which has been pressed into use as a knife haft. The knife blades are bi-facial and they show wear. All kinds of unexpected combinations might be expected.

JELKS- Do you consider Clovis points knives or projectile points?

WITTHOFT- I consider the Clovis points to be projectile points in terms of their breakage patterns and in terms of their truncation, and also because they are accompanied by other tool types which show knife use, which are typologically knives. Besides, with the later Plainview points in the Eden Valley and other sites, you get the Cody Knife as a companion of the Eden and Plainview points. You get the Dalton Knife with an accompanying Cumberland spear point. In Shoop Site complexes you get the long blade tool accompanying the projectile points as knives, and occasionally in early Paleo-Indian complexes you get almost any kind of a tool made from another kind. You get a broken fluted point that has been made into a side scraper or spoke shave, you get the tip broken off of a fluted point and it has been made into an end scraper. Improvisation, tool scraps, flint scraps, being used for tools and broken tools being made into other kinds of tools. In most cases where you can see the complex, it is easy to compare the projectile point to the knife.

(BULLEN EXHIBITS CERVICAL SPINE OF ELEPHANT -- LATE PLEISTOCENE FAUNA-- FOUND IN FLORIDA, ASKS WITTHOFT TO COMMENT ON THE CUT MARKS ON IT)

WITTHOFT- The cut marks appear to have been made with an axe or an adze. Certainly not a butchering mark. Look as though they had been made with a bi-facial tool, except that the hacking is too deep for anything but chopping.

MAC CORD- If this holds true-- that the tools were not made of the hafted projectile point and/or knife and later was whittled down into a smaller tool and finally ended up as a un-usable stump which was discarded. In our closed site or single component site you should find a whole range of this deterioration. I think that Betty's dig (St. Albans Site) and some of the cave digs where we have discrete strata, where we have whole samples, we should get this range. I don't know of any such instance, do you?

WITTHOFT- I don't know of any samples that are big enough, or well-enough studied.

GUTHE- This morning we have heard a little bit about adjusting our thinking in terms of looking for continuum rather than simply so many horizon markers. This afternoon we will hear papers on site analysis and how it is related to lithic samples, one on fluted point manufacture, and comments on the material from the St. Albans Site.

(END OF FRIDAY MORNING SESSION)

LITHIC PREFERENCES
 PRECERAMIC COMPONENTS
 BROKEN BOW RESERVOIR LOCALE
 MCCURTIN COUNTY, SOUTHEAST OKLAHOMA

LITHIC MATERIALS	CALLAHAM INTERMEDIATE ARC. FLAKES ARTIFACTS		LAMAS BRANCH LATE ARCHAIC FLK. ART.		E. JOHNSON EARLY & LATE ARC. FLAKES		BEAVER LATE ARCHAIC FLAKES	
		%		%		%		
BIGFORK CHERT	32	36%	33	7%	64	9%	66	5%
NOVACULITE	32		181		62		66	
QUARTZ	4		5		1	14	13	
QUARTZITE	3		16		3	3	8	
LITHIC TYPE I	11		10		2	2	1	
SANDSTONE			1		-1	3	2	
SILTSTONE							-1	
UNIDENTIFIED	10		25		2	4	1	2
FOREIGN	-1				-1			-1
SLATE					6	3		
SAMPLE TOTAL	8338		318		26837	575	5637	2869

SESSION II

Friday Afternoon, November 8, 1968

DON WYCKOFF:

All of the work that I have been concerned with has been reservoir salvage in the eastern part of Oklahoma, which ecologically is a part of the eastern Woodlands. Therefore, I do think what I have to say has some pertinency to this particular conference. The subject of my talk is Prehistoric Lithic Preferences: Aids to Site Analysis.

For the past six years we have been working in four or five reservoirs in eastern Oklahoma. Along with the usual problems of salvage archaeology, there has been the necessity to try to reconstruct as much as we can of the prehistory for each one of these reservoir locales. Most of our salvage operations have emphasized the artifacts: artifact associations, artifact sequences, and component recognition, doing what we can to reconstruct as much of the prehistory and early history of these reservoir locales before they are flooded.

One approach that we have been using is based on the tenent that analysis of both lithic tools and lithic trash provides useful data for component recognition. In this talk I wish to emphasize the analysis of the lithic trash.

Dr. Witthoft has touched upon some of the problems, or the potential, of analyzing lithic materials, all lithic materials recovered at the sites under study. This involves fine screening, to get the small flakes as well as the large, and the interpretive potential of the stone working technology(ies) represented at these sites.

Our work in Oklahoma has not been too involved with the flaking or tool technologies although these study areas are certainly something we should have been working on. We excavated the sites, used fine screens, kept all material that did not pass through the screens, and we analyzed this material, flakes and artifacts, but not in terms of the technology. Instead, we were looking for lithic preferences that would associate with different cultures and/or time periods. Occupations by different cultural traditions do, for Oklahoma, exhibit certain preferential usages of different lithic materials.

This particular approach, the concern for lithic preferences, may not be applicable to all regions. In some areas there occurs very limited quantities and diversification of the lithic materials which prehistoric man could have utilized. One area that comes to mind is east Texas where there is a lot of usage of petrified wood with apparently little fine grained cherts being available. Cultures using the petrified wood have lithic assemblages that are really quite limited in the forms, the varieties of tools which could be made from this material. Petrified wood is not the best kind of flaking material but these people apparently did get along with what they had.

The idea of preferential usage of lithic materials is not an end in itself to site analysis. This approach offers only supportive data in component and assemblage definition and must always be combined with the analysis of the entire assemblage that is recovered.

The actual analysis consists of first gathering all data possible on prehistoric quarries or on any of the geological strata exposed in the particular region. This works quite well for reservoir salvage work. In a reservoir area some 15 to 20 miles long, your archaeological survey can locate exposed strata or gravel beds of the various lithic materials available to the prehistoric flint knappers. In some instances, actual prehistoric quarries may occur although this is one type of site which most states have not really concentrated much study on.

I think Holmes is the person that has done the most to present data on prehistoric quarries, but this is largely survey data, locating where these places were. As far as I know, no one has gone to the quarries and mapped and excavated or analyzed the varieties of lithic materials which might occur. Also there has been little presentation on the actual mining and flint working technologies represented at lithic quarries. Certainly, knowledge about prehistoric quarries and the kinds of materials found therein are essential in establishing the types of lithic materials one uses in the analysis of flakes and tools from occupation sites.

Of course, in some areas prehistoric quarries may not occur, and it will be necessary to depend on geological data. In Oklahoma we have tried to work closely with the geologists in the Oklahoma Geological Survey, and these persons have been most helpful in identifying lithic materials, outcrops, and potentially interesting outcrops. They have run surveys on these outcrops, and we can find these outcrops in the reservoir areas, thus having a source, or potential source, for some of the lithic materials that show up on the prehistoric habitation sites.

The analysis of artifacts and debris from a habitation site is relatively simple. As you sort the flakes and artifacts it is necessary to identify and maintain counts with regards to the types of lithic material. Our Oklahoma salvage excavations have been controlled excavations using five foot squares and digging in natural or arbitrarily assigned levels. We have found that you can do with the lithic materials essentially what you do with artifacts. The analytical approaches are much the same. You can seriate, establish stratigraphic sequences, etc. These approaches provide meaningful, helpful data in understanding the nature of occupations at these habitation sites.

Examples of the approach:

I got interested in this analytical approach in 1962 through the suggestion of Dr. Robert Bell. At that time we had a site with no physical stratigraphy, no observable soil changes, although the site had four feet of occupation fill. The excavations recovered a lot of lithic trash and not too many artifacts. The analysis of the artifacts was done first, and this work generally indicated that there were two components, two stratified components, at the site. The point types were completely different formal types occurring in two different vertical zones. It helped to substantiate the presence of these two components when we did a lithic analysis of the flake debris; this analysis emphasized the kinds of lithic materials utilized by the prehistoric occupants. We found that in the upper two feet of this physically undifferentiated midden there occurred a preponderance of one kind of flint while in the lower two feet a completely different flint type was the predominant material utilized. In the same locale we excavated another site with a good cultural sequence represented in some twelve feet of fill, and the cultural interpretation for the preceding site fit quite nicely into the overall stratigraphy of the lithic material and lithic preferences that were recovered from the site with twelve feet of habitation refuse.

In a reservoir area in southeast Oklahoma we excavated an apparent single component Archaic site with point types that suggested an intermediate Archaic time position, perhaps around 3000-4000 B.C. Our analysis of the lithic materials found that 39% of the flakes were of Bigfork chert while 16% of the artifacts were Bigfork chert; 32% of the flakes were novaculite and 33% of the artifacts were of novaculite. Lithic Type I, which was a siltstone, constituted 11% of the flakes and 10% of the artifacts. Unidentified materials, which are largely foreign to that reservoir locale, were 10% of the flakes and 25% of the artifacts. Again, I reiterate that the unidentified material is probably foreign; I think coming out of east Texas or perhaps further north in eastern Oklahoma.

In contrast, we dug a single component, late Archaic site some seven miles distant from the intermediate Archaic site. At this late Archaic station we found 81% of the flakes were of novaculite while 64% of the artifacts were novaculite; this contrasts with 32% and 33% for the intermediate site. At the intermediate Archaic site there was a preponderance of Bigfork chert (39%) in the flakes but in the late Archaic site Bigfork chert was only 7% of the flakes; artifacts of Bigfork chert were fairly consistent for the two sites: 16% for the intermediate site and 13% for the later site. In the late Archaic site we found increasing usage of quartzite for a wider variety of tools (hammerstones, choppers, and a nice series of pear-shaped stemmed hoes). Such quartzite tools do not occur in the intermediate Archaic site although 16% of the artifacts, mainly projectile points, in this site are of quartzite.

One nice thing about working with the lithic materials and utilizing the flakes and artifacts is the quantification of data. For the intermediate Archaic site discussed above we had a total of 318 artifacts but we had 8,338 flakes. At the late Archaic site, there were 575 artifacts and 26,837 flakes. In a real sense, this analysis of the flake debitage bears resemblance to the quantified data obtained from your emphasis on pottery analysis. Again I say that analyses of the flakes and lithic preferences are very, very helpful and useful for studying the preceramic sites.

Flake and lithic preference analysis are also useful for those sites which do have pottery. We excavated several southeast Oklahoma sites with two or three feet of late prehistoric (Caddoan affiliated) midden fill. I do not remember the exact totals, but in one we recovered around 43,000 flakes and in the other around 42,000 flakes. We really missed our bet by not doing a technological analysis at the same time because we handled each and every one of these flakes while we were doing the lithic preference analysis.

Lithic preference analysis is a useful approach, and I think it bears consideration, particularly in areas where you have variations in lithic materials and when your cursory analysis indicates variations are occurring from site to site.

There are some cultural inferences which can be made of course. One of the main ones is, if you know your quarries and your outcroppings, you get excellent data on local contacts, movements, and/or trade. In eastern Oklahoma we have evidence of trade of lithic materials from quarries in the north-east corner of the State occurring clear down to the southeast corner. We have artifacts from the Arkansas River valley near the Arkansas-Oklahoma border that are made from flint coming from clear out in the Texas panhandle.

Another aspect we have observed through the analysis of the several sites in the various reservoir locales is that you get very good data on local

adaptation through time. In Archaic periods you tend to get an emphasis of using flinty gravels for stone tools. But by the late prehistoric occupations you tend to have a widespread usage of all sorts of lithic material from a wide variety of sources and source areas. In the Arkansas River valley, the Archaic and early Caddoan Occupations are using primarily chert cobbles that they are picking up out of streams, but by the time of Spiro you get evidence for widespread trade of lithic materials. In the subsequent Fulton aspect or late Caddoan, it is even more widespread. This trend towards utilization of more kinds of lithic materials and establishment of more trade contacts through time seems very consistent for the several reservoir locales we have studied.

MAC CORD- In Virginia we have been using this type of chip analysis for quite a few years. Even in sites where certain collectors have picked up all the visible relics, we can tell what cultures used to be there by collecting the chips.

WYCKOFF- This approach works real well because you can do surface seriation.

QUESTION- What if you have materials that are not on the same time level?

WYCKOFF- This is a problem. Some contexts do not allow use of the lithic preference approach or, for that matter, any other type of analytical approach. It helps to work in very limited regions. The reservoir area makes a nice geographical model within which to work.

QUESTION- Since different kinds of chert have different chipping properties, will shatter differently, in different numbers of places, that we find in series... the statistical statement as to the aging of the particular kind of material on a site is not necessarily meaningful in terms of the artifacts produced, and therefore it seems to me that the same results can be gotten by just examining the artifacts and not the flakes.

WYCKOFF- This is generally true but not always. In many instances we have found a close correspondence between the percentage frequencies for artifacts and flakes.

QUESTION- Then you would get the same results from just examining the artifacts?

WYCKOFF- Sometimes. However, on the intermediate Archaic site discussed earlier, there was a preponderance of Bigfork chert, yet novaculite artifacts outnumbered those of Bigfork. There are several possible explanations for this. The materials are quite comparable, and the same types of artifacts were being made out of both materials. The only conclusion that I could come to was that the people who were occupying this site were utilizing the Bigfork chert, which is local, but yet they had brought in novaculite of grades which do not occur locally. Once they left the site, they took a lot of Bigfork chert tools which they had made at the site. This explains the fewer artifacts than flakes. It is a potential explanation. I will not say it is the only explanation.

BULLEN- This idea worked very well, particularly in New England.

WYCKOFF- We found that in the 12 foot deep site, we got four or five variables. Just by the flake frequencies we were able to define four or five occupation levels by the maximum frequency of the flakes in the superimposed levels

that we excavated.

CALDWELL- You have some good responsible Indians in Oklahoma. I will not quarantee those of north Georgia. I once excavated a grave in north Georgia where, although we found no bones, we found some objects I interpreted as flint-knapping tools. In one corner of the grave was a pile of Archaic points. In the other corner of the grave were five small Mississippian isosceles points. One of these isosceles points was made out of a very curious kind of green flint which occurred in a fragment of an Archaic point. Apparently one of his lithic preferences, if you will, was to just go out into the village and pick up old points.

WYCKOFF- You will get all sorts of exceptions, but the approach still has some amount of validity.

WITTHOFT- My experience has been that this is a very powerful tool and it implies all kinds of sociological and cultural contact information. Especially when it can be carried to the study of quarry and workshop sites where a specific industry is strongly represented and then carried out over broad geographical regions. For example, the Copena Knife Complex, centered around Dover Flint, is now turning out to be one of the great cross-ties for the northeast, and up into the Ohio and Illinois river valleys. What is an insignificant tool in some Adena and Hopewell complexes, that the collectors and local people do not recognize, turns out to be a magnificent cross-tie in terms of both technology and lithic materials. Some of these things can be culled out on almost a half a continent basis.

WYCKOFF- For certain parts and time periods in Oklahoma we get evidence of very widespread contacts, flint materials from way up in Nebraska occurring down in Oklahoma on a late prehistoric time level. The Kay County flint, which is a very distinctive flint for Oklahoma, is very widespread down into Texas, all over. Some of these distributions may reflect population movements, the Wichita moving around were probably using Kay County flint.

STEPHENSON- I would like to comment on an incident from the State of Nevada. In north-central Nevada there are four parallel valleys in the Great Basin range province where the mountains all run north and south. They are separated by deep valleys, from west to east, Paradise Valley, Eden Valley, Kelly Creek, and Evans Creek. A survey was made of the valleys with some 180 or so known sites in the four valleys combined, rather evenly distributed between the four. In Paradise and Eden valleys on the west the whole range of stone tools occurred, with obsidian predominant, but chert, pink chalcedony, and a few other things present. In the two valleys on the east almost no obsidian tools or chippage were found, almost everything was white chert and pink chalcedony. A very clear cut dichotomy. We go to the B.A.E. Bulletin and the article by Julian Stewart and find that the mountain that separates these two pairs of valleys is his dividing line between the Shoshone and Paiute. All of the local people who lived there for a generation or two knew that this mountain range was the dividing line between the Shoshone and Paiute. Local tradition. All of these tools that we were finding were from types that were 500 to 2,000 or 3,000 years old. So you apparently had this dichotomy along the same line for long periods of time. A dichotomy that stretches up into historic time.

GUTHE- I wonder how much we are doing in the southeast along these lines, quarry identifications, distribution of lithic materials, etc. Perhaps Dick Marshall has a few thoughts on this. He does have something to say on tool technology.

DICK MARSHALL:

Regarding what Don Wyckoff was saying, the site that I have been working on in east Mississippi seems to bear out an awful lot of preference. I do not know the importance of this yet because it seems like every site that I have been on in east Mississippi has had the same red flint regardless of how old the site is or how new it is. There are several Archaic sites almost on the Alabama line to the east of the Tombigbee that I was on a year ago this spring. I never saw such a proliferation of flakes and almost every one of them was made out of a dark red jasper or something. Everything is seemingly made out of this red flint except along Lyons Bluff, where we picked up two Nodena points that were made of the typical yellowish-brown material that is found over in the Mississippi valley. I think that what Don was saying would bear some, if nothing more, casual checking. It could prove quite interesting.

What I would like to present is a comment on hinge fractures and fluted point manufacture. The paper I am about to read was written by George Nichols of Columbia, Missouri, and the comments and conclusions are entirely his own.

Anyone who has barked his knuckles, bruised his ribs, and produced blisters by trying his hand at flaking a stone projectile point begins to appreciate early man's problems, skill and masterful domination of flint chipping. This difficult craft is an art that prevailed millenniums before the development of more easily worked but durable materials. We may duplicate to some degree his tools, but since our objective in doing so is entirely different, we can never know his real problems, his exact methods, and his long-gone secrets beyond a certain point. For both amateur and professional archaeologists alike, the (ancient) technique holds undeniable fascination.

Here, then is a dissertation on a small facet of flint chipping. They are some observations made after more than fifteen years of studying the problem. Initially this experiment was a theoretical study of how to make a Folsom point. First there was an attempt to test the theory by trying to produce a point. As the experiment progressed, a greater interest developed in observing a certain manufacturing phenomena, the hinge fracture and reverse hinge fracture.

Before launching into this dissertation, however, acknowledgments are due. The writer conducted the experiment and engaged interested professionals: Dr. Frank H.H. Roberts, Jr., formerly Director, River Basins Surveys, Smithsonian Institution, and Raymond S. Baby, Curator of Anthropology, Ohio State Museum, Columbus, Ohio, advised in various capacities and provided data for comparison. Their help and encouragements are appreciated. The writer also acknowledges the help of Richard A. Marshall, Director, Museum of Anthropology, University of Missouri, who assisted in the writing of this paper and prepared all of the drawings.

Techniques and Materials Used

At the outset considerable trial and error was involved in the duplication of fluted points. Percussion techniques were tried without results. It was found that pressure techniques, using deer antler tools for both blade shaping and the fluting, was by far the most productive. The blade, after rough

shaping, was held for fluting securely in a wooden vice (Fig. 1), adequately padded with leather to diminish shock and unnecessary breakage. After fluting, the tip was shaped and the entire blade then finished.

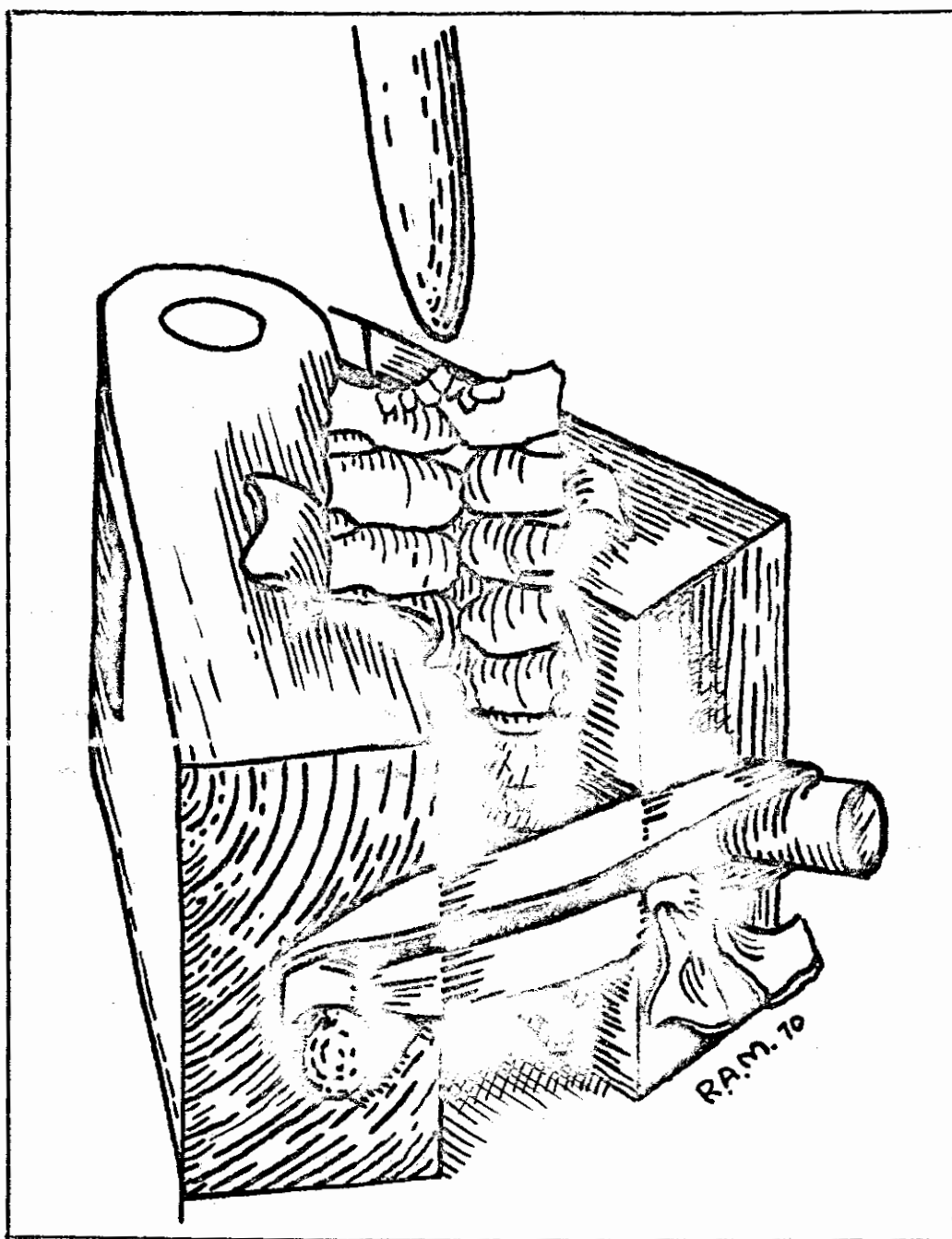


FIGURE 1- Wood vice used in fluting (designed by the author).

Obtaining adequate flint of suitable flaking quality and size posed an early problem. Construction glass was resorted to which proved to be an excellent flint substitute. The darker varieties were especially useful for studying fracture and stress lines that were difficult to detect in the lighter colored flints. This material compares favorably with good quality flint and chalcedony but admittedly works much easier than the average quality na-

tive material. Hinge fractures (the problem under discussion) were produced with flint and glass. This fracture can be produced with any type of material that lends itself to flaking, whether amorphous or crystalline in structure.

More than 100 fluted specimens were produced in this study and of this almost 30% of them were broken during fluting by reverse hinge fractures. This may seem a high percentage and is! Throughout the study, blade-shaping techniques improved considerably; however, the problem of reverse hinge fracture diminished unproportionately.

Not knowing where to go with this problem, Dr. Frank H.H. Roberts, Jr. was contacted. He provided much comparative information on the fluted points from the Lindenmeier Site, a Folsom hunter camp in Colorado. A comparison of the experimenters' experience with the data from the Lindenmeier Site led to the realization that reverse hinge fractures were an inseparable part of fluted point manufacture.

Although Folsom makers developed a high degree of skill, the correspondence with Dr. Roberts indicated that the Lindenmeier chipping debris suggested that reverse hinge fractures were a frequently occurring event at that site.

It appears that many physical forces are combined when flint and related materials are flaked. Flake scars, the result of chipping, tell us a great deal. When a large flake is removed from a perfectly flat, plain surface, unrestricted by flake scars or flaws in the material, the tendency is for the flake to fan out immediately from the apex point of pressure (striking platform) in almost a perfectly circular fashion (Fig. 2). Considerable energy is transferred from the pressure transmitter or flaker to this restricted area during detachment.

Energy producing the fracture disperses through the material, ideally, evenly and smoothly. Impurities or uneven densities set up a number of waves which are sometimes called shock rings, ridges, or strain waves. They are concentric, one following the other. The further the flake detachment travels the more violent the waves or ridges become until the flake is completely separated from the parent body. This is evident to some degree in any chipped stone artifact.

The flakes produced by pressure chipping are ideally (evaluation mine) ones which feather out and detach by a smooth fracture which has a very thin and sharp edge. This thin edge is often quite fragile and crumbles under slight pressure. When violent shock waves are generated during separation, the flake is often detached by a hinge fracture (Fig. 3) an irregular detachment, having, opposite the striking platform, a surface which is curved back on itself and which is very smooth. Though this is not an ideal separation, it is not to be considered an imperfect one. There is occasionally produced a secondary flake.

Although hinge fractures are produced in many types of flaking, let us deal here strictly with the hinge and reverse hinge fracture common to fluted point manufacture.

Illustration is simpler than verbal description; let us examine both a hinge fracture and a reverse hinge fracture produced on the same archaeological specimen (Figs. 4 a and b).

Figure 4 is a specimen that was originally intended to be a large Clovis-like point, perhaps four or more inches long. It was found on a high upper ridge

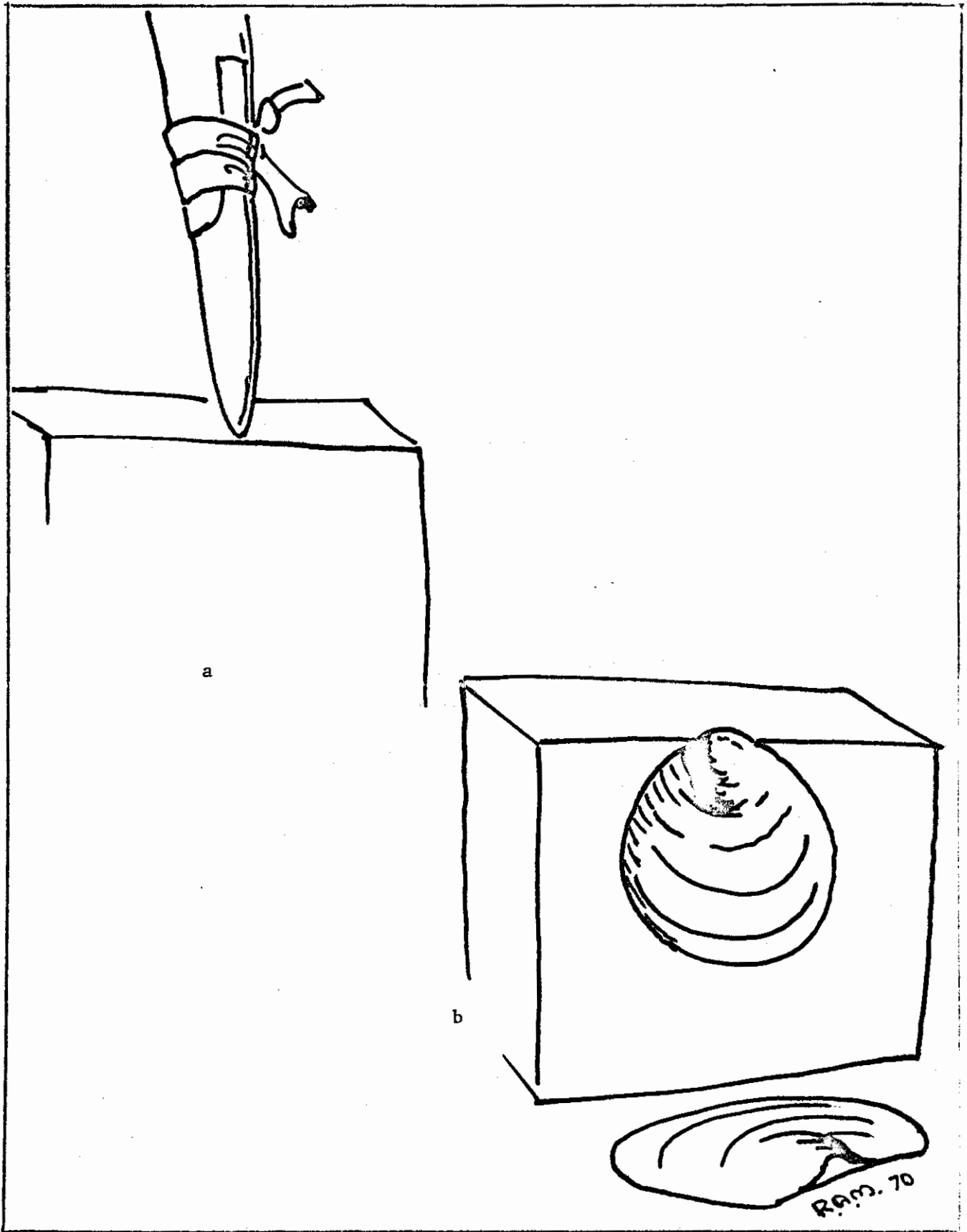


FIGURE 2- The pressure flake: a- flaker at pressure point (potential energy); b- concoidal flake removed (spent kinetic energy).

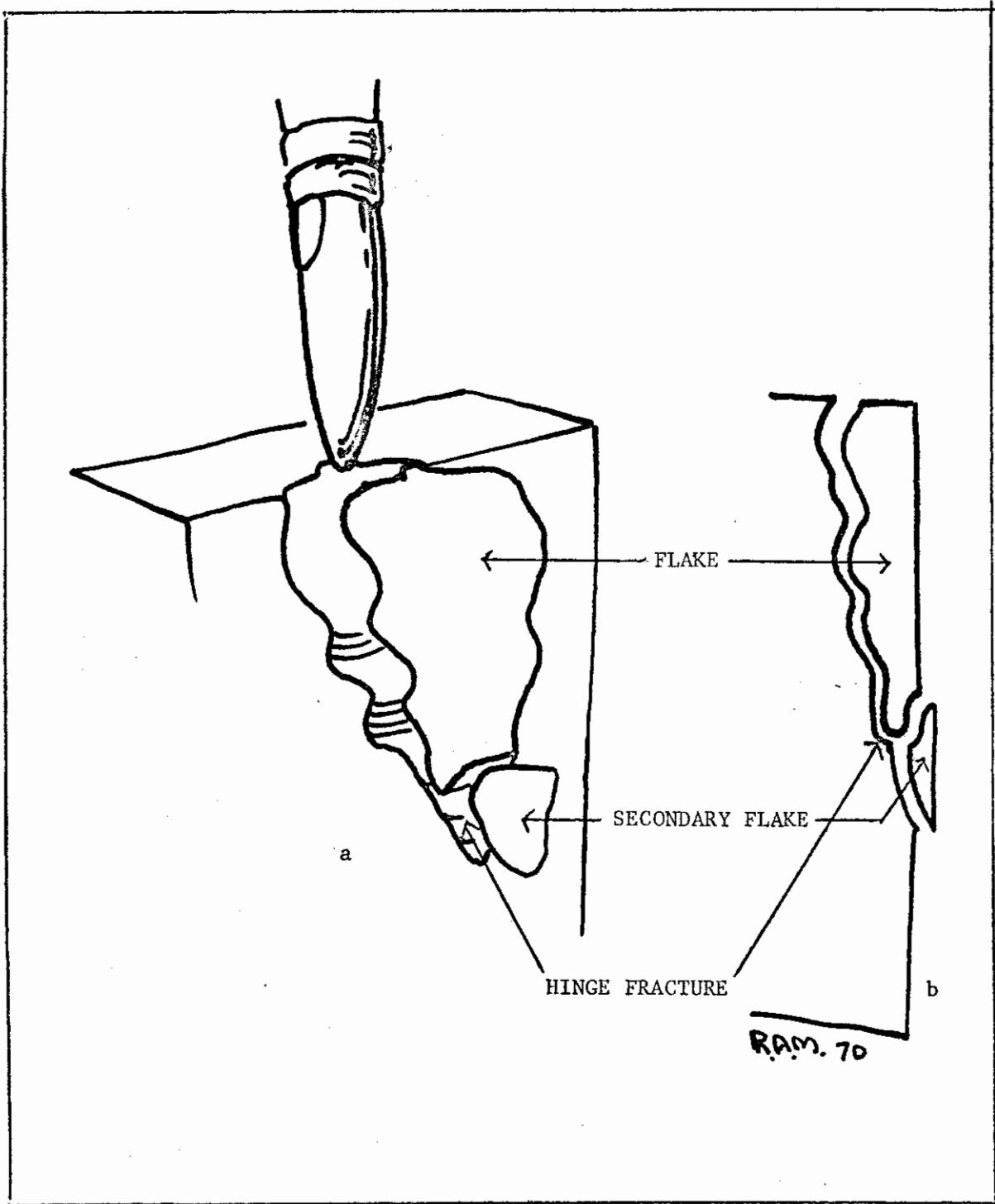


FIGURE 3- Hinge fracture: a- the flake detaching; b- side view of detached flake.

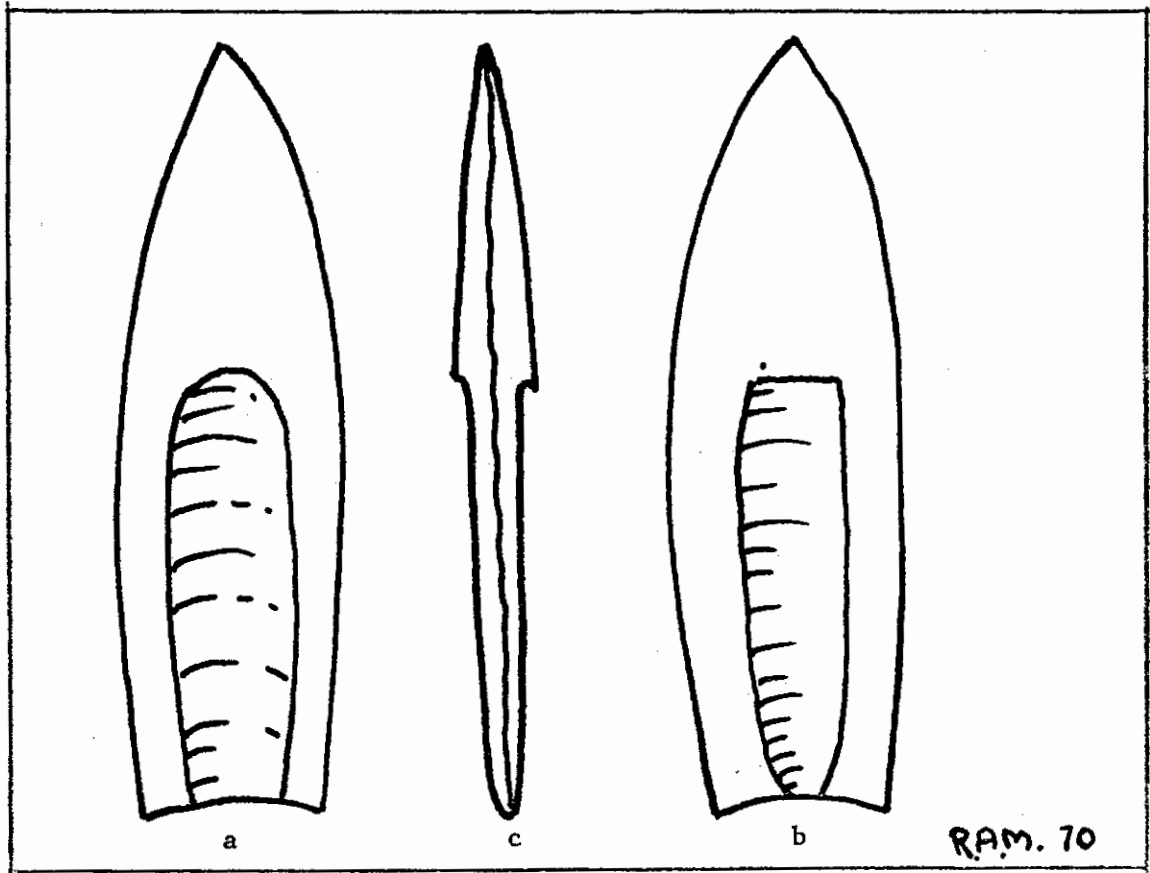


FIGURE 4- Fluted point exhibiting "ideal" (a) and hinge separation (b) of channel flake, and cross section (c) of separations (illustrations enlarged).

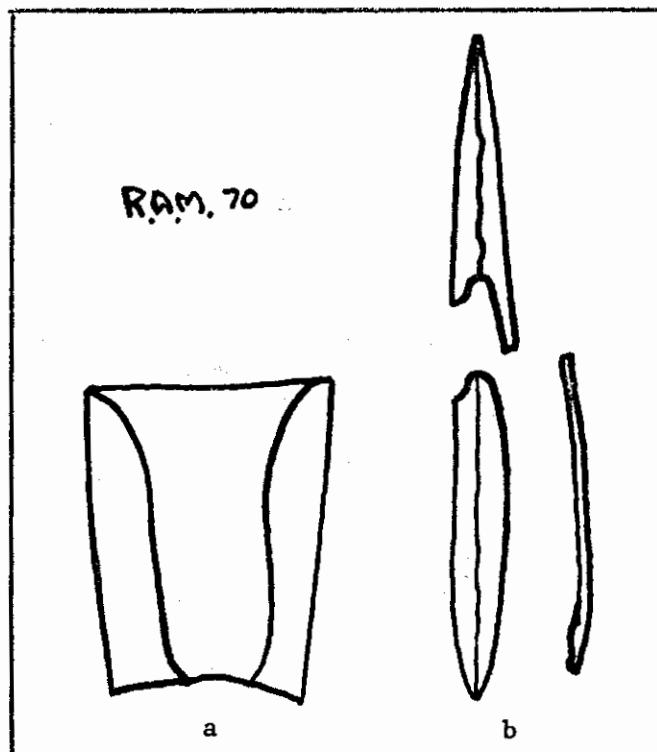


FIGURE 5- Reverse hinge fracture: a- the tangible evidence; b- cross section of fragments and remaining channel flake.

overlooking the Missouri River near Roachport, Missouri. A number of similar specimens have come from this and nearby sites and are in the writer's collection. Figure 4a shows a flute, the scar left after the channel flake was detached. The flute is one and one-half inches long. It ended in a normal detachment noted by the terminal feathering. This is a common type of flute detachment.

In Figure 4b, we see the opposite face of the specimen. It is a common hinge fracture, noted by the circular shape of the scar where the flake terminated. Figure 5 is another specimen from the same site. It is an example of abortive fluting, the reverse hinge fracture.

What happens when there is a reverse hinge fracture?

Elongated, narrow points, due to their design and lenticular or diamond shaped cross section are highly susceptible to breakage by reverse hinge fracture during the fluting process. The channel flake is highly restricted in its area of separation during detachment. In channel flake detachment or fluting, it appears that a shifting of stress from one edge of the detaching channel flake to the other, due to irregular blade shaping and unequal flaking, is sufficient to cause one edge of the separation to detach far ahead of the opposite edge, throwing the whole operation out of balance. Unless there are physical features on the blade's surface and energy dissipation factors from the pressure thrust to bring the detachment back into equilibrium, the fluting process instantly aborts in a reverse hinge fracture. The break occurs the instant the leading edge of the separation flairs out and engages the edge of the blade. The presence of ripple lines common to the surface of long flake scars indicates that considerable stress is produced by the sudden release of the applied energy (potential) into the kinetic energy of separation. The fracture makes the blade useless unless the break comes sufficiently distant from the base that a new tip can be shaped.

This type of breakage may have been more of a problem than previously suspected.

It appears that three basic shapes or patterns are produced by reverse hinge fracture during fluted point manufacture (Fig. 6). For the lack of better descriptive terminology these are designated equal or left and right. These breaks come about under slightly different circumstances.

When both edges of the channel flake flairs out and engage the edges of the blade simultaneously, the break will be equi-distant from the base or an "equal" reverse hinge fracture (Fig. 6b). In the event that one edge of the channel flake flairs out and engages the near edge of the blade before the other edge, the break swings diagonally to the opposite side, making a "left" or "right" reverse hinge fracture (Fig. 6a).

As a general rule the shock produced by reverse hinge fracture causes the channel flake to break off just opposite the hinge, making three pieces, the base, the channel flake and the severed tip of the point (Figs. 5 and 6a). Now and then, however, the channel flake will stay intact with the tip end (Fig. 6b). During these experiments described here, three such specimens of the latter type were produced. Such specimens had rather thick, more diamond shaped cross sections. At the Lindenmeier Site, Roberts recovered several such specimens which broke in the manner just described.

It would be unwise to draw very many conclusions from a simple experiment of this type. It does show, however, that manufacturing problems sustained by early man in the manufacture of fluted points can be duplicated and evaluated to a degree.

All of this leads to some interesting speculation. How much of a factor was this problem in the eventual abandonment of fluted points? Was the classic Folsom limited in time distribution and use because of a high percentage of this type of break? Due to the ultra fine workmanship of fluted point manufacturers, was the reverse hinge fracture higher with classic Folsom than with the larger Clovis types? Provided it could be established that reverse hinge fractures run 25% of the total points manufactured, a count of abortive pieces at a given site could provide a rough estimate of the number of points manufactured there. These are only some of the questions that can be raised.

As trained scientists continue to unravel the story of early man's inhabitation of North America, reverse hinge fractures recovered from workshop chipping debris may play an important part. Amateur archaeologists can help unravel this fascinating problem by acquainting themselves with what reverse hinge fractures look like, and report such finds to the professionals.

The reverse hinge fracture is unmistakable and tangible evidence of America's earliest manufacturing problem.

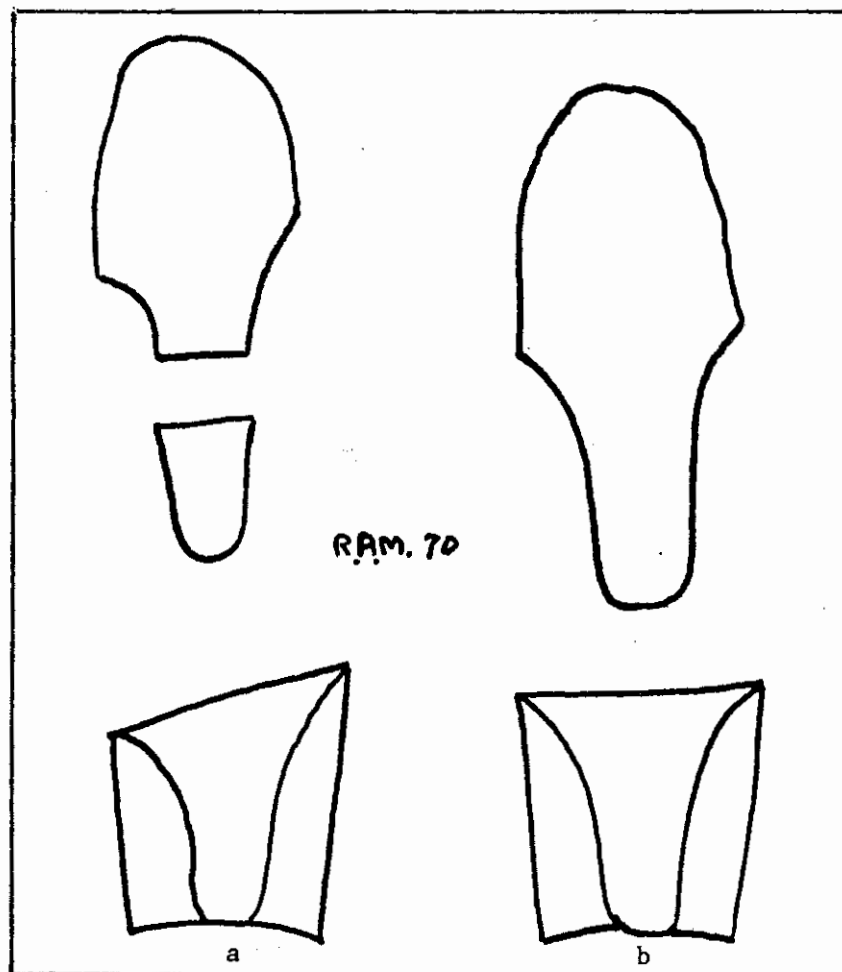


FIGURE 6- Reverse hinge fracture: a- "right" swing of fracture; b- "equal" fracture.

GUTHE- Well, he slipped off and took the glass instead of the flint. From the limited work I have done with Paleo-Indian material in Tennessee, it seems that one thing is evident; that is, the Indian had in mind a particular kind of material that he wanted to use. I think the fluted points, Clovis and Cumberland, and the variants of these found in Tennessee, are of a different material than we find in the late Archaic, and certainly in the Woodland.

MARSHALL- I think he made about 15 to 18 points out of native material and then switched over to this other simply because he could not get good native material.

GUTHE- Where did the Indian get this native material? That is one question I am curious about. Once in awhile I have seen some reverse hinge fractures on fluted points with bases that do not seem to have any grinding or rubbing on them. This suggests that something happened during the manufacture of it. On the other hand, once in awhile, you find one that has been reworked into another kind of point so that you are not sure how the flute came off.

WITTHOFT- This is really quite a complex problem, the mechanics of this, and has given us all sorts of trouble. In the samples that I have handled from eastern Clovis sites, like Williamson and Shoop, there is perhaps 1% of reverse hinge fractures in the total sample of points. When I looked at these eastern Clovis points that are made from rock crystal, in which they successfully use the stuff and which requires much more work than flint, I was really startled. I would say that a hinge fracture is the result of insufficient flaking force, and it comes about through a very complex set of harmonic reactions in the shock wave created inside the piece. The glass requires far less force to produce the same effect as the flint does. Rock crystal requires much more force.

(EDITOR'S NOTE: PORTION OF TAPE NOT TRANSCRIBED BECAUSE OF BAD BACKGROUND "HUM")

GUTHE- Does anybody else have a thought regarding the selection of materials? That is what I would like to develop here.

JONES- I have often wondered about all of the rejects that the young produced when they were trying to learn how to chip this stone, particularly this reverse hinge flake. Where are these? He (Witthoft) mentioned 1%; where are these rejects?

BROYLES- The rejects probably became another type.

JONES- I have not seen anything that looks like the rejects.

GUTHE- We do not have too many sites that produce a complex of fluted material.

(EDITOR'S NOTE: AN EXCHANGE BETWEEN BULLEN AND WITTHOFT NOT TRANSCRIBED)

MAC CORD- I wanted to comment on what seemed to be an amazing thing, to me at least. In spite of the small population in eastern Virginia during Paleo-Indian times, where there is at least 20,000 square miles available and probably only one place in that 20,000 square miles where a suitable chert outcrops that would meet the requirements of these people, occupying about one square mile in this large area, these Paleo-Indians found this outcrop and worked it. That is the Williamson Site. These people must have been able to smell flint or chert.

MC CARY- I do not know what the percentage of reverse hinge fractures is from the Williamson Site material, but off-hand I would say that it is not more than 3 or 4%. The Paleo-Indian at the Williamson Site could flute rock crystal and quartzite, one apparently as good as the other, and also rhyolite as well as the chert or flint.

WITTHOFT- It was amazing what those people could do with materials other than flint. This really speaks for their skills, just as the rock crystal points speak for their skills.

GUTHE- Ripley, do you have any comments in regard to Sewanee points? Are they fluted in this fashion or not?

BULLEN- The Sewanee points are not fluted. The percentage that are is very very small.

GUTHE- Which makes you wonder about continua.

WITTHOFT- In this experimental work (reference to paper read by Marshall) what sort of pre-forms, platform, was used? Did he try to duplicate the shoulder of Folsom or Clovis?

MARSHALL- The Folsom type.

(EDITOR'S NOTE: ADDITIONAL COMMENTS BY WITTHOFT CONCERNING THE BASE OF A FOLSOM POINT, UNABLE TO TRANSCRIBE BECAUSE OF BACKGROUND NOISE)

GUTHE- We have several Riker mounts up here containing what off hand look like projectile point types. In fact, I think they have projectile point type names on them, but I am struck by the variety of materials within each of them.

BROYLES- I have been saving all my comments in order to talk about the material from the St. Albans Site. There is definitely a difference in the lithic material that was used, or preferred, through time at St. Albans. In the zones occupied from 8000 to 7000 B.C. a large variety of multi-colored cherts and flint was being used. The predominant type of flint in the Kanawha Valley area is Kanawha Black Flint, occurring in outcrops along the river and many of its tributaries, therefore a large percentage of the artifacts were made of it. In one Ryker mount I have included fragments of blades and scrapers just because they were made out of unusual cherts. In the 1,000 year period (between 8000 and 7000 B.C.) there seems to be a larger number of artifacts made from non-Kanawha Black Flint. The source of these other types is unknown. Some resemble Flint Ridge Flint, but I do not believe that was the source. A study of the flint is planned before publication of the final report.

From 7000 B.C. to 6000 B.C. the vast majority of artifacts were made from Kanawha Black Flint, probably at least 90%. The chips found in the site correspond to the artifacts. For example, when we find a pink chert projectile point, we find chips of the same material in that zone, so it appears that points were being made at the site and not being carried in from elsewhere. Since we have not tabulated the chips I am not positive that there will be a correlation between all of the points and chips of material other than Kanawha Black Flint, but that is the way it appears in casually sorting through the material.

QUESTION- Could it be chips left from reworking?

BROYLES- I don't think so, since the chips do not appear to be part of an artifact. They are large chips that would have been removed in the original manufacturing process. There is some re-chipping on a few of the points, especially the Kanawha and Kirk types. There are two examples in this Ryker mount of drills made from projectile points. The ends are worn smooth as if they had been used as drills. Several Kirk Corner Notched points show this use. We also have one broken Kirk Corner Notched point that was made into a spoke shave.

The other thing that I wanted to comment on was the change in chipping technique through time at the St. Albans Site. The earliest point type that we have at present dates from about 7900 B.C. The chips on these points run at an oblique angle and form a medial ridge down the face of the blade. The later types (Kirk, St. Albans, LeCroy, and Kanawha) do not have that medial ridge or oblique flaking. They were chipped by very small random flakes and the faces of the blades are relatively flat. A few points (especially Kirk Corner Notched) have beveled edges, but the percentage is very small for any of the point types from the site. Almost all of the point types have serrated edges, but those from about 7000 B.C. (Kirk type) have deeper and better defined serrations. The very earliest types of points are still unknown, since there is still 19 feet of deposit unexcavated at the site.

BENTALL- I have seen some points from a surface site that look like the St. Albans point but that have the same type of flaking as the earlier type with the medial ridge.

BROYLES- I do not know the distribution of this type, since it is a newly defined type. I have two points from a previous summer, three from surface collections, and one more from this summer's excavation. I realize this is not a very large sample on which to base a type description, but it is a distinctive looking point and this is just a tentative name I have given it (Charleston Corner Notched). The distribution is something we will have to work on.

GUTHE- I think it is about time for some "wild statements". Does anybody have anything to suggest regarding distribution clues that they have a vague hunch about and want to check out with anybody?

WAUCHOPE- I was wondering whether the flute has to be perfect before the point can be used or whether some of these things can be used without having a good flute on it. What do we know about distribution of these things, or complexes of them?

BULLEN- There are an awful lot of basally thinned points that are not fluted.

WAUCHOPE- Fluted points in North Georgia were found in great quantities associated with the very earliest pottery, not fiber tempered but fabric marked. I couldn't believe that fluted points could really be associated with the pottery. The outcrops that they made these points from were all over the site, and I think that these are simply 7,000 year old implements picked up by the Woodland people.

CALDWELL- I have seen a lot of that material on which the fluting was so rudimentary that you wouldn't recognize it.

WAUCHOPE- I may be mistaken in calling this fluting, it may not have been, but it is there in contrast to the other stuff.

KOEHLER- (UNABLE TO TRANSCRIBE, BUT QUESTION CONCERNING BEVELED POINTS)

GUTHE- The so-called twisted points, rotating points, is this the sort of thing?

CALDWELL- I would swear that the stuff is early Archaic in the Savannah River area of Georgia around Augusta. In fact, Claflin, who dug the Stallings Island Site, pointed out that he had seen the stuff all over the Savannah River Valley but he had not found it with fiber tempered pottery. We know it is not later than Savannah River, so in that part of eastern Georgia and South Carolina it must be early Archaic. I suppose this is the best candidate I know of for a knife because I have seen these things resharpened so much that they are unrecognizable.

GUTHE- There is a characteristic beveled knife from west Tennessee. I am trying to think if I have seen anything later than Archaic. (SECTION NOT TRANSCRIBED) We used to speak of us becoming psycho-ceramic, maybe we are now becoming psycho-lithic.

(END OF FRIDAY SESSION)

SESSION III

Saturday Morning, November 9, 1968

ALFRED K. GUTHE:

The program this morning calls for a summary. I have tried to talk somebody into summarizing, but they said what is there to summarize? So evidently what we have had is a summary. The program for this Southeastern Archaeological Conference was conceived a year ago, at least the theme was, and at that time, you recall, we were talking about pottery types and had had a series of small sessions, regional gatherings, on pottery types. It occurred to me that typology has come to be, in some ways, our slave, or perhaps we are becoming slaves of it. We read papers, we read articles, we see literature passing before us which evidently is full of meaning, but the meaning there, I think, is somewhat remote at times. The types are used for comparative purposes, and to stratify these things or at least to set the sites and the dates into position. Typology then, as we all know, has been something conceived by some as an end in itself and by others as a tool. I would much prefer the concept that typology is a tool. That is why I thrust it upon us.

The theme then was "What can we learn from Lithic Technology?" What kinds of insights can be made regarding culture? After all, archaeologists are concerned with the reconstruction of prehistoric cultures. What do we really know about these cultures when we talk in terms of projectile point types or pottery types? Are we really getting a grasp of the ways of people, their problems, the lives that they had? Are we duping ourselves, kidding ourselves, into thinking that we are getting something? What can one learn from lithic technology? If we consider Linton's four criteria of traits which he defined back some years ago, about 1936, form, meaning, function, and use, to what extent can we as archaeologists looking at the lithic materials come up with any characterization of a particular trait in these four categories? I think yesterday we got some idea, at least I hope we did. As John Witthoft started to talk about the continuas which can be found in lithic materials, the thing that struck me as most important, or at least one of the most important, was that the projectile point or a lithic form can undergo changes through utilization which can produce, to the typologists mind, different types. Different projectile point types then, to put it in a nut-shell, can be defined as a result of a single piece of material being worked and reworked. Are we kidding ourselves into thinking that a projectile point type then is a discrete minimum unit which we can handle? Evidently projectile point forms, or those we call projectile point forms, may 90% of them be knives. After a while, if they are sharpened and resharpened, they ultimately pass to the point where we would identify them casually, or we would casually identify them, as drills. What clues do we have then? How can one look at these forms and come forth with the statement that this is this and this is that. Any of you who have worked with the amateurs at all recognize that this is what they want to know. What Indian tribe made this and how old is it? That is always the question, and you have to look at a form and say now this is this and this is that. But it isn't that simple, apparently. If we collect sufficient numbers of pieces from sites then, evidently there is this continuum, a change, a running from one kind of form into another kind of form although it was the same form originally. So the criteria of form that Linton suggested is a variable thing when it comes to archaeological material, at least. We cannot say that once a form always a form, because it may not be. What about meaning? This is the

thing we are really concerned about, but I am afraid we can't get very far on the meaning of the piece. What are the attributes of this projectile point form or this lithic form in the Indian's mind? So, let's skip that one and think about use and function.

Functionally there seem to be some variations. It might have been used for this or it might have been used for that. How can we determine this? How many of us have used a screw driver as an ice pick or as a wedge or something else. So, are we right in assuming that the Indians had only one idea in mind when he made a lithic form? These are some of the things we should think about. Witthoft also suggested to us that we might get some ideas regarding function by the study of fracture planes, shatter lines, and the like. That we might gain some ideas as to the function of the varieties of tool types that we see, the forms that we hold in our hands, by looking at the Ethnographic literature, and perhaps we should do some more along this line.

In the afternoon Don Wyckoff was suggesting to us that looking at the finished artifact was not all that had to be done. If I did not misunderstand him, it appears that at some times the points and the finished pieces, the artifacts, that you and I would normally handle were made of a material which was different from the material which constituted the flakes, the utilized flakes, the roughage, the debitage, etc. What does this mean culturally? Trade? Migrations? All we can say is there was lithic preference. We have to think of these things.

I had hoped that Dick Marshall was going to come up with some report regarding the nature of one kind of flint or kind of material that was particularly useful in flint producing Folsom or fluted points. But evidently we kind of got a reverse action on that because the man he was reporting upon simply indicated that he found working with lithic materials a little more difficult than he found working with glass, which of course is more regular in content. So actually, I guess, in a way we did learn from that contribution that there is more to it than having an idea in your head and a couple of fingers you can work around and go to it. How did the Indian pick his material? Why did he pick it? From where did he get it? These are questions we ask ourselves and, hopefully, in time we will get some answers.

This is what I got out of yesterday's meeting. It occurs to me that I may have read more into it or less into it than some of the rest of you. I am not denying the fact that the literature does have some information regarding some of these tools, some of the uses, and some of the mechanics. I, for example, and most of you, have seen the book by Semenov or the translation, and he goes into methods. This is one of the few books that is available. There are a few people who think that the archaeological material has not been utilized to its fullest, others who think it can not be utilized to the fullness of our anticipations. Some of you, I am sure, are aware of the argument by Social Anthropology, Cultural Anthropologists generally, that archaeology has no part in Anthropology. At least our literature indicates that. I wonder how a Social Anthropologist, if we can distinguish such, would react to our concern with culture based on clues from lithic technology. We have a couple with us and I heard some rumblings yesterday to the effect that they had some sort of comments to make.

SAMUEL LANG:

In hearing Dr. Guthe present his summary this morning I was struck by the use of the term "function", with no apparent distinction between function and use. Now in Social Anthropology we draw a distinction between these two terms. The use of a trait or a trait complex is the ends to which the society directs that trait or trait complex. Now, its function is the degree to which it perpetuates the continuation of the society as a whole through time. Now, I gathered that the terms are used synonymously. I am not quite sure, perhaps an elaboration is in order on that point.

Another point that I thought about yesterday upon hearing all of this-- I think it made me rather happy with my choice of discipline within Anthropology, and I mean that with all due respect-- and that is that what do all of these points tell us about human behavior in terms of function, in terms of adaptation, in terms of the group as a whole? It seems to me that when you deal with projectile points you are dealing with a trait that has a long continuum on a central theme, that theme being hunting, and perhaps to some extent defense. Now, it occurred to me in that context that perhaps projectile points are the least important indicators relative to human adaptation. Is this a possibility?

In summary... these two things: the idea of use and function, the almost synonymous use of the terms, and actual value of projectile points.

MAC CORD- I would like to respond to your use-function problem. Let's take our common old beer can opener. Its function is to open beer cans. Its use is to puncture the lid of a can so that it can be opened and the beer consumed.

LANG- Let's take an axe. Its use is to chop wood, its function is to provide shelter for a group as a whole, etc.

GUTHE- We should think about theory...

WITTHOFT- I was thinking about this use-function business. Essentially what we are trying to do in archaeology is recover ethnographic information and then use it in an ethnological manner, and we have to go through some of these relatively complex and time-consuming procedures to do this, to gain validity. Now, one of the very curious things about the white man's mind is his preoccupation with projectiles. The collector is constantly projecting his own psychic on the relics. When we really start to apply our techniques of observation to these tools, all our projectile points dissolve and these things take on new meaning. The cutting edges which were used for butchering, for flint work, for making wooden tools, are archaeology: its preoccupation with projectile points is definitely something we are trying to break through, a Freudian preoccupation with piercing.

GUTHE- Dick Yarnell indicated to me yesterday that he has, in his analysis of soil samples for the purpose of extracting pollen, encountered a number of small flakes of flint and bits of material which you and I have not been seeing in the samples that we send in to him. Fortunately, he is keeping them, separating them out from the dirt along with the pollen and is sending these back to the people who sent him material to analyze. So it looks like we are getting a lot of cooperation from people with specialities in other areas in terms of our analysis and approach to understanding archaeology.

WAUCHOPE- I would like to make one observation and then ask John Witthoft one question. It is certainly true that reconstructing ancient ethnology is one of the prime purposes of archaeology, if not the prime, and the average discussion of typology that I have ever read is hopeless. Krieger's observations involve ultimately guesses at either the use or function. As defined this morning, function is operating near the highest level of interpretation, but I do want to defend the archaeologists' ability to explore culture process without ever knowing either use or function. Suppose we didn't know what bannerstones were used for, but we had a long stylistic sequence of them through time and space, other than just being two measuring relationships, the cultural process, the rate of change, and the aesthetic versus technical change, can be understood, or explored at least, without ever knowing what a bannerstone was. The same thing applies to the little jade figurines in Meso America. We don't know what they were, but we can contrast aesthetic rates of change through decoration of jade figurines with those of pottery figurines of Meso America, also of which we don't know the use. So I don't think that we should forget entirely that we can work with artifacts without knowing their use or function.

Second is a question for John Witthoft. How are we to train ourselves in this use, in this specialized knowledge, of interpreting small chips? I can't do it and I don't think many can. Where do we, and especially the younger generation of archaeologists, learn this? Can it be self-taught or do we have to study under you somewhere?

WITTHOFT- It almost has to be self-taught, and a lot more material has to be published. Semenov's book is a start. There are other papers that are useful. The work that the lads at Michigan did, in extracting the micro-chips from the soil in their pits and distinguishing between the flakes that were from damaged edges or resharpening edges and the shatter pieces that break out of the scar of percussion when you are chipping flint on a site. They provide us with a technique by which you can say that a point was being worked here or it wasn't being worked here. People were making tools here or they weren't making tools here. In my teaching I find that in the introductory courses and even later courses, students have no concept of what lithic typology is or what the tools are from text book illustrations, from studies, from anything else except handling tools, teaching involving actual samples, in large quantities and great varieties. We have to start with our students from the very beginning using the standard laboratory teaching philosophy of the biological sciences, and we try to reach students through Anthropology I, Man's Place in Nature, actually the way we think Huxley would have liked to do it. With laboratories that involve the skulls, the bones, the casts, and the sequential group of tool types from the Olduvai tools up to the Bronze Age, to give them a feeling of objects and of the weight of materials of the objects, rather than teaching them a bunch of meaningless terms like Clactonian, the Levalloisian technique, etc. We need first to train students to think technologically about these things and once this starts to happen they can find the literature as they read it rather than worry about these details of landmarks on the flint scars, etc. Maybe I have over-emphasized the importance of chippage and debris and lithic rubbish because this is one essential part of the picture, but not all. The business of continuums is very important. I spoke, perhaps vaguely, yesterday about two overlapping kinds of continuums in the Ohio Valley. For each Adena and Hopewell stage in the so-called projectile points, I can lay out within the types a sequence of wastage from the rough-out that was made at the quarry, through the newly finished tool that was not used, down through every stage of resharpening and wear, until the end result is an unrecognizable stump. But there is another continuum and that is the taking of the more or less intact

specimens from each type and laying them out and they themselves fall into a continuum in which there is no break between the Adena industries and the Hopewell industries, and there are no breaks within the Adena or within the Hopewell. Now, all I can say from this is that there is a technological continuum through Adena and Hopewell and, in fact, from Hopewell on through later complexes right up into Fort Ancient and Middle Mississippi. We have a continuum which we can not break naturally, we have to cut them off artificially. We can see isobars in the continuum, but the whole assemblage of a large pooled sample of these tool types teaches us a lot about the history of technology. Now we can't go ahead and say, of course, there was also no break, no changes, in population composition. We can't say there may not have been changes in the language being used, but we can say there was a continuum in technology. Just as with the Canandagua early Owasco through every Iroquois stage there was some continuum of tradition in community. That doesn't mean we have solved the language history problem or the problem of the physical types. Their forms may be far more complex factors; we are only dealing with cultural continuums. We are looking at culture process in several different ways, in these sequential groupings of large pooled samples. If we could find out what they were used for, all the better. We use the bird-stones as time markers and we don't really know what they were used for.

WAUCHOPE- Could an average intelligent person take journal articles and descriptions that you can only read or maybe look at pictures of and hand them a microscope and teach himself?

WITTHOFT- He has to have problems to work with. One must have a study cabinet to type the objects just as the mineralogist has always typed his reference specimens and he can't teach without them. We can't teach without them very well either.

CALDWELL- That paper you gave yesterday morning? Will it be produced in our proceedings and rather profusely diagrammed?

WITTHOFT- I think I could do some drawings for that without too much trouble. I am in a position of representing a school or fad. I am over enthusiastic about trying to squeeze more information about culture history, culture process. So sometimes I over emphasize the importance of lithic trash, but it is important.

CALDWELL- If you could illustrate it.

WITTHOFT- The other thing that I find in teaching is that I must insist that people draw and learn the conventions of drawing flint tools and pottery, etc. Often I sit down with a stone tool and try to make one of the conventional drawings, using my scanners, and I find that I can not get it on paper. And when this happens I realize that I have misunderstood the technological sequence in the thing. But, drawing is another important method of learning, because it is handling the objects in profusion, which is one of the most important methods of learning. The biologist cannot teach without his laboratories, his field situations, etc., and I am beginning to feel the same situation, and feel fortunate in having access to a very large museum collection. I still find them inadequate and I have to go out and make random samples of groups of things that I need for teaching purposes, as well as for study.

GUTHE- Your question as to how one becomes knowledgeable of these things..... I guess it is a self induced study, from what I gather, and you have to have an interest first.

WAUCHOPE- If you are looking at a micro-organism for the first time through a microscope and you have a picture of it in the text book, half the time you can't see it in the microscope until the instructor looks through the thing and says see there it is over in the left hand corner. I think we should get John to tour the country.

WITTHOFT- In two months this summer, Frances and I collected one-half ton of debitage and artifacts from 40 sites that pertain to the Proto-Historic Shoshone of the Blacks Basin. This is one of the troubles with this whole thing. You can collect the stuff at a terrific rate, but the analysis, even the numbering of the specimens, takes a long time.

CLAY- Maybe I am mis-interpreting the way he works.... The important step here that makes more sense to other people is that you are working with a culture phase, or whatever it is. You are working with a known series of things you can identify as tools, plus chippage. You have to start with this realization that you have tools and chippage. How does the chippage result from the production of the tools? You have to have an idea of the tool in mind first or some notion of what the tools are. You just can't think about chippage and say this is a technology, you have to know the tools too.

WITTHOFT- Yes, but the chips tell you a great deal about the tool. Of course, in every pot-hunter's circle of archaeology, people get upset with us because we are so pre-occupied with trivial pottery types, and we are so pre-occupied with Paleo-Indian fluted points. We are working with all there is. Now when we get a site that is ideal, we get all the wood, fiber, skins, and cordage, and then we have some chance of seeing flint technology in cultural context.

(EDITOR'S NOTE: THE PAPER PRESENTED BY GEORGE NEUMANN ON SATURDAY MORNING WAS TRANSCRIBED FROM THE TAPE AND A COPY SENT TO DR. NEUMANN. HIS CORRECTED COPY WAS NOT RETURNED, THEREFORE THE TALK IS BEING PRINTED JUST AS IT CAME OFF THE TAPE. IN SEVERAL PLACES WORDS WERE OBLITERATED FROM THE TAPE BY SOME OUTSIDE NOISE, AND IT WAS IMPOSSIBLE TO TRANSCRIBE. THESE PLACES WILL BE INDICATED BY SEVERAL DOTS).

GEORGE NEUMANN:

I would like to begin by mentioning or going into the particular problem that has just been discussed, that is the matter of semantics, rather than any particular problem. So I would like to first of all define some of my terms in order to explain what I am trying to do and what kind of cooperation I can get from you. It is not a matter of physical anthropology versus archaeology, as has been just mentioned, but I am trying to identify myself as an archaeologist because of the community of aim. Archaeology is primarily, by definition, prehistory. The ultimate aim of it is to reconstruct history. Questions of typology, of establishing chronologies, of elucidating particular ecological relationships, are useful tools, but are not the whole story and are not the ultimate aim to the archaeologist nor to the physical anthropologist. But in typologies, it is generally assumed that you have a sort of pre-conceived set of criteria which are used as a classification, and then you see

how your universe fits into it. From the physical anthropologists standpoint we might use Roland B. Dixon's illustration, where he classified everything as being proto-Negroid, or some such division, on the basis of the combination of a long high head and a broad nose. If you use three criteria you are going to get twenty-seven categories out of it. You can do the same thing in archaeology and apply it to pottery. Assuming that there is a genetic or historical relationship between all cord marked pottery or all pottery all over the world, whether you get cord marked pottery in Egypt, in the Ankara Culture in Siberia, or in the Woodland culture here. The taxonomy, then, is not the ultimate aim, it is merely a tool. Too often the archaeologist tries to reconstruct history without man. It is a sort of an anomaly that you would have even anthropology without anthropos. To prove whether this Woodland pottery was brought from Siberia or in any problem to test whether you have a question of migration or diffusion, a number of things ought to be kept in mind for this. You should have, first of all, continuity in distribution. There should be temporal consistency, and also an examination of associations. If we find out that cord marked pottery with an Ankara Culture dates back to 1800 B.C. or that Woodland pottery, the earliest appearance say is 1800 B.C. again in North America, there would be a general temporal consistency. On the other hand if we find that the use of copper tools in Siberia dates to 2600 B.C. and the Old Copper artifacts in the Great Lakes area can be dated back to 5600 B.C., there is obviously something that has to be explained. So, if you are going to have the cord marked pottery imported from Siberia and have a culture continuity, it would behove us to examine them to see what physical types are associated with it.

The racial history of the American Indian is very complex. You have a long time span-- 10, 20, 30, 40 thousand years. A lot of things can happen in that time span. We are fortunate in having the American Indians descended from a common gene pool. According to my view they are all Mongoloids, but having this tremendous distribution from Eastern Asia, North and South America, possibly as much as 40,000 years of time. The possibility of various isolates, like going off into mountainous areas, into the desert plateaus, and islands and peninsulas, various kinds of refuge areas, being dispersed, you have a possibility of a full play of genetic drift, of adaptations to take place. The Maya Indian did not develop in Siberia some place, that particular type differentiated in the Maya area. The same thing is true of the Andean area. The same thing is true in the Plains area. You would not mistake a series of Cheyenne Indians walking at the Gallup Festival in front of a group of Indians from Jamez Pueblo as being exactly the same. In reconstructing history we can work in a biological way, in dealing with analogous structures and that would then provide a basis on resemblances. And here we have a classification, a regular taxonomic system, of phyla, order, classes, family, genera, species. The ultimate unit is the species. When you get to differences it becomes a matter of differences rather than similarities. The same thing can be applied to archaeological or cultural criteria. In biology, we have genetic continuity, in archaeology we have cultural continuity, although in genetics it is probably somewhat easier to prove whether two things are the same or not. because of the tremendous complexities that is involved in molecular biology. So, if you are going to work on differences rather than similarities, we first of all must accept the larger categories just as theoretic devices to see how the work out, to test them, and then our immediate problems would be the small classifications, their inter-relationships, and that would lead us to using some kind of continental analysis of all materials, to see to what extent there is an interplay of various kinds of groupings.

Statistically, if we computerize our materials, we have a very useful tool in multi-variant discriminant analysis in which you compare Group A and Group B to see what they share and exclude those traits, and see in what they differ, and these would provide the diagnostic traits to differentiate between Group A and Group B. As soon as you take a third group you may have to introduce other traits. What differentiated A from B may not differentiate B from C or C from A. The study then, if it is to be sophisticated, is a matter of relativity. I am sure that has been in the mind of all anthropologists, whether physical or cultural, or archaeologists, this matter of relativity. Certainly Boas in The Mind of Primitive Man in 1927 was not the first one to point out that language may be constant and there may be a racial and a cultural change, or that race may be constant and language and culture change, or that culture may be constant and both race and language can change. It is our problem to find out just what did happen. So, in order to reconstruct the history of the American Indian, it becomes a matter of not only condition of the history of a certain area, but the inter-relationship between areas: eastern archaeology, plains archaeology, southwestern archaeology, great basin archaeology, and we certainly can not leave out even South America, because we might find very close relationships where there have been few physical changes between say the Archaic Indian Knoll skeletal material and that coming from the San of Brazil. Or particular physical groups may have remained relatively constant from Paleo-Indian times to the Historic Period. There are instances of that. So here you have whole sets of variables: linguistic, physical, genetic, and cultural, and we try to make sense out of them.

In order to do that I began a long time ago on presenting these inter-relationships in a series of six maps. These are not maps illustrating physical types, but particular clusterings of traits as represented in certain populations, and I had to name them so that I can talk about them as a tool. I began with the Historic Period and after taking these larger groupings, these populations, to see how they were represented in various tribal groups. For this I took Driver's tribal distribution map to get an idea of the territorial distribution, and indicated what particular physical types, or combinations, are found in these various tribes. Much of the map had to be sort of dubbed-in because there is a scarcity of Historically identified skeletal material. The series is very small to begin with. Joffre Coe's Tutelo (?) material consisted of 28 skulls. This is historic eastern Siouan. There are 14 from Manhattan Island. There were 6 skulls definitely which were Shawnee; about a dozen Choc-taw; only about ten Creek; two skulls which are Yuma; two which were; about thirty which are; about ten Pima skulls; and about ten skulls of Yuma who cremated their dead. So you have to piece this together. I have an extensive collection of about 4,000 portraits of individuals where the tribal affiliation is known. Link the tribal appearance data with the archaeological and see how things fit together. Revisions have to be made as soon as the map is finished, and it is not at all final. I realize that I have made some very bad blunders in particular areas.

I just spent a month in Europe and discovered a treasure-trove of materials, especially South American material, and some Mexican material, and less North American material, in the British Museum, but, as a whole, what I found there substantiated what I had reconstructed from the available data. I have collected material since 1928, so it is a large accumulation and probably involves close to 10,000 crania of which I have detailed measurements and observations and indices. You can't work from any one of these along, and I distrust measurements as a rule, because they may just reflect a size difference. Indices will give you proportions, something in addition, but the real

thing that is inherited are the small morphological differences which can be used in a multivariate discriminate analysis.

I would like to point out the results of two of these studies which I have of this material and which was given to various graduate students to work on as thesis in a sort of preliminary way. They will be published as joint publications. One of these would be the Fort Ancient material which has just been completed, worked on by Louise Robbins, who took Charles Snow's place at Kentucky. After this Fort Ancient material was analyzed, close to a thousand crania, it was found that the relationships of the northern foci which Griffin set apart are to central Algonquian materials. We started out with half a dozen known Miami skulls, identified a Proto-Historic Miami series as Miami, and raised it to 100 to get a fairly good statistical sample of Miamis. If that is compared to the Andersonville focus material in southwestern Ohio and it has been shown to be identical. Now, we know that the Shawnee were a central Algonquian speaking group. Here we have a historic inconsistency. The archaeological Fort Ancient Aspect may not be exactly coterminous with the Shawnee tribal groupings. It is unlikely that only the Andersonville Focus material is Shawnee, that Madisonville is not; this is highly improbable. The relationships of the Madisonville material, which again in a temporal sense is late, dating perhaps as late as 1673 as Griffin suggested. The Baum and in Ohio may go back to 1100. There again in those sites the affiliation is definitely with the Algonquian groups or the groups to the northwest rather than related to the Tennessee-Cumberland material. So here, by studying the skeletal material, we are getting a pretty good idea as to what actually happened, to what extent Mississippian traits may have been taken over into the Fort Ancient Culture, or what was there to begin with which could have been influenced by these traits or do we have an actual migration of people. That was probably true with the Madisonville people themselves who probably took over Shawnee language, if they were Shawnee, if they can be identified as the Shawnee tribe.

Another problem that has been solved is the occupation of the Siouan speaking groups in the Ohio Valley. Ralph Anderson, another student, used the material that I measured in North Carolina, and tabulated the material morphologically, and got the measurements, the indices, the statistical constants. If you subject this to a multi-variant analysis you can..... it was found that the Historic Tutelo (?) material is identical to the Indian Knoll material. That brings back the Siouan group into the Ohio Valley in Archaic times and we know exactly those traits in which the Tutelo have changed from the Indian Knoll people, only very few. Now, if you compare this Siouan material that you get in the Plains, it is an entirely different "kettle of fish". There again it can not all be lumped, because the material that you get from Mandan sites and from the Woodland Sioux, Yankton, eastern North Dakota, and Minnesota, down into the Oneota peoples, their affiliations are to the Hopewell peoples, Woodland groups of about 2,000 years ago. On the other hand, if you take the division and the Dakota Sioux on the high Plains and up into Canada, their physical affiliations are clearly to the northwest, to Athabaskan speaking groups. In many cases you can not tell a Sioux from the high Plains from an Apache. Now, here then, linguistics give us a hint that there are Siouan speaking groups in the east. This is tested archaeologically. The material in North Carolina and Virginia, the Siouan material, has a Woodland affiliation. They may have been in contact during Early Woodland times and a continuity with the Ohio Valley may not have been entirely broken. Some of the Siouan groups that remained west of the Mississippi associated with the Mandans, who are the grandfathers of the Siouans, traditionally, probably

served as a center of distribution and giving the language to tribal groups to the west of them, and there became cultural differences. The differentiation of the Plains Culture, the Horse Culture mainly, becoming different from the Siouan Village Indian Culture. You had movements on the Mississippi by Caddoan groups bringing, to a large extent, Mississippian Culture traits, which became Oneota, beginning about 1100. It is significant that there is not a single skull among these eastern Siouan groups that can be mistaken for a Plains Dakota type, but it is all relative and you have to find out exactly in what respect each group differs from each other, rather than trying to use a general typology. Very spectacular similar developments can be expected in all areas of both the North and South American continents, if you do this on a continental scale; if you have archaeologically well documented material. That is what I am appealing to you for, to furnish that well documented material.

GUTHE- Thank you. It is kind of refreshing perhaps to bring things back into focus again. The typology, the distribution, and now here we are back on the track again.

(THE BEGINNING OF CALDWELL'S STATEMENT WAS NOT RECORDED)

CALDWELL- other point is that archaeologists could be, I think, of much more help to people like Dr. Neumann if they would define some of their cultural terms and historical terms; give them a more sociological and populational, such as not many of us use the word aspect any more, so we might but even worse than this I would say is that George is forced to talk in terms of Woodland, Mississippian, and Hopewell. I immediately think when he says Mississippian, does he mean the Tennessee-Cumberland variety or does he mean the Cahokian tradition, etc. In other words, to say Mississippian doesn't mean much to me, and if we could speak in more precise terms than Mississippian then we might be closer to actual populations as represented by cultural material, so that George would have something to work with. Similarly with Hopewell, I don't know whether George meant Ohio Hopewell or whether he meant Hopewellian. If he meant anything beyond Ohio Hopewell he meant a situation in which we probably have hundreds of societies all carrying a kind of ceremonialism which spread very rapidly over the country which is similar to Mississippian in acculturation situations, and you have people who were once Mississippian and then become something else, and you have people who were something else who became Mississippian. So the archaeologist, I think, could do a great deal to help the physical anthropologist if we could give him some more precise terms that he could use to zero in on populations.

(END OF SATURDAY MORNING SESSION)

SESSION IV

Saturday Afternoon, November 9, 1969

THE SLUICING SYSTEM USED AT THE ST. ALBANS SITE

Bettye J. Broyles

West Virginia Geological Survey

The St. Albans Site is located about 15 miles west of Charleston, West Virginia, on the south bank of the Kanawha River. The depth of the site (36.5 feet according to core samples taken in the area) and the type of soil necessitated the development of new procedures of excavation.

The soil in the St. Albans Site consists of a mixture of sand and clay, with the amount of each varying from zone to zone. At times the clay becomes very hard and compact, making hand screening (dry) virtually impossible. At other times the soil becomes very sticky, also preventing it from passing through a screen. The only solution to the problem appeared to be the use of water to separate the occupational debris from the soil.

A second problem, speed of excavation, was also solved by the sluicing process. The total number of artifacts retrieved thus far (about 1,000 worked pieces plus several thousand flint chips) is very small when compared with the almost 100,000 cubic feet of earth that has been removed from the site. The screening process enabled the excavation to proceed much faster since the soil did not have to be removed as slowly or as carefully.

When the sluicing system was first tried in 1965, the zones being excavated were about 25 feet above the level of the Kanawha River. The screen and a small gasoline pump were set up on the river bank (Fig. 1), and a method of getting the soil from the excavation to the screen had to be devised. This resulted in the erection of a corrugated tin chute, 40 feet long, held up by a frame constructed of 2 X 4's. As the excavation proceeded down, the tin chute was shortened and lowered. In order to dump an entire load of soil down the tin chute, a funnel at the mouth of the chute became necessary. The funnel consisted of an old truck hood (obtained from a junk dealer) which had the front end cut out to fit the curve of the tin chute. Eventually, the screen was moved nearer the excavation and the truck hood fastened to the end of the screen.

The sluicing system proved so successful that, by the middle of the summer, a second screen was constructed and an electric pump secured (donated by Standard Oil Company). This larger pump also necessitated other items, such as about 300 feet of 220 volt electrical wire (donated by Appalachian Power Company), a new transformer to carry the extra load, extra fuse boxes and switches, a series of capacitors to convert the pump from a three-phase to a single-phase motor, 3 inches diameter metal pipe for the intake of water rather than the 2 inches diameter rubber hose that had been used with the gas pump (the suction

by the electric pump was so great that the rubber hose collapsed), a check valve in the intake pipe, 2 inch diameter metal pipes to carry the water from the pump to the edge of the screen where they were connected to rubber hoses which were easier to handle and could be moved over the screen, nozzles on the end of the rubber hose to reduce the diameter of the opening so that the water will spray into the screen with greater force (an "L" shaped nozzle is very effective), and last, but not least, safety glasses for the crew members working on the screen. It was also advisable to wear swim trunks, since a wheelbarrow full of soil dumped into a screen full of water has a way of spraying muddy water over whoever is working nearby.

One experiment was tried during 1968. The question had been raised as to whether this system could be used on a site which contained pottery, animal bones, and mussel shell without breaking everything into small pieces. A large surface collection from Putnam County which had been made by Brooks Hutto, student assistant during 1968, was used for the experiment. Each piece of pottery, bone, and shell was numbered to ascertain breakage after the experiment. A sterile zone of clay was chosen in the St. Albans Site, and the material mixed with wheelbarrows full of soil. This was then dumped into the screen where it was allowed to remain until all of the clay was washed through. Actually, this was rougher treatment than the material would probably received under normal conditions. After about two hours, the pottery, bone, and shell were removed from the screen, mainly because the numbers were washing off. Breakage was minimal. A few of the smaller bones, such as bird, were broken in half, but both pieces were still in the screen and could easily be glued together. The thin edges of the mussel shell were also chipped, but the vast majority of the material remained intact. After completion of the experiment, we believe that this type of sluicing system could effectively be used on almost any type of site. A movie was made of the entire experiment.

CONSTRUCTION OF THE SCREEN

The screens used at St. Albans were constructed of 2 X 12's (Fig. 2) reinforced at the corners with metal "L" shaped braces. It is important that the corners fit tightly so that small thin artifacts or chips will not be washed through and lost.

The type of screening used on the bottom of the box is most important. It must be strong enough not to bend or break under the weight of a wheelbarrow full of soil being dumped into it. Hardware cloth, used on most hand screens, is not suitable. There are probably several types of strong screening, but the type used at St. Albans was expanded steel mesh. This mesh has diamond-shaped openings that smaller artifacts or chips could pass through, therefore two layers, turned in opposite directions, were used. This leaves holes that, in most cases, are smaller than the diameter of a pencil. The two layers should be "spot-welded" so that material will not be trapped between the two layers. Expanded steel mesh can not be bent by hand, but must be bent to fit the bottom of the screen-box by heat. The corners should also be welded and a steel band welded to the top edge of the mesh for strength and protection to the workers on the screen. If the screen is larger than 4 feet by 6 feet, additional steel bands can be welded to the mesh across the bottom. This is also an important point: any additional steel bands welded to the screen should be on the underneath side, not the top, because the weight of the soil will be pushing down onto the bar instead of pushing on the screen. After considerable use, the screen can eventually break away from the cross-bar if it is on the top. Also,

if it necessary to keep the clay in the screen stirred up (as it has been at St. Albans) the shovel or hoe being used will get caught under the cross bar if it is on top of the screen.

PLACEMENT OF SCREEN

The placement of the screen is important for several reasons, some of which are not obvious until after you have used a sluicing system such as the one at St. Albans.

Of course, the placement of the screen will usually depend on the water supply. The screen should be far enough from the pump so that the pump will not be sprayed with water, but close enough so that the water will not have to travel a long distance to the screen. (the shorter the distance the more pressure). A gasoline pump is usually not powerful enough to push the water a great distance and should be closer to the screen than the source of water. The gas pump used at St. Albans in 1965 was about 10 feet from the edge of the water and 5 feet from the screen. The rubber intake hose was floated about 6 inches under the surface of the water between two empty plastic clorox bottles. Posts were driven into the river bottom on each side of the hose to prevent it from changing position.

Of necessity, the electric pump must be placed on a stationary platform (Fig. 4). The intake pipe should be made air-tight at every joint. At St. Albans, since there was a danger of the river level suddenly rising, the joint between the pump and intake pipe (Fig. 4, B) was not a stationary connection. A sleeve was made so that the intake pipe could be slipped over it and taped into position. The tape used by heating contractors to seal joints between heat-duct pipes is suitable for this joint. "C" clamps were then placed over the tape and an air-tight joint formed. The larger check valve placed in the intake pipe was positioned so that the top could be easily removed if it became necessary to remove some matter lodged in the valve. A screen was placed over the end of the intake pipe to prevent larger objects (such as fish) from entering the three inch pipe. In the case of the electric pump used at St. Albans, it was necessary to prime it each morning before it would begin to pump water. A cap was placed in the top of the connection between the intake pipe and pump for this purpose (Fig. 4, A).

The screen should be placed as far from the excavation as possible, but still within easy access to the wheelbarrows. If it is too near the excavation every wheelbarrow load of soil dumped into the screen will cause the water to spray back into the excavation. Also, if there is as much water pressure on the hoses as those used at St. Albans, the spray from the hose, especially if it is not held properly, can reach into the excavation (the hose has on occasion caused an inexperienced handler to fall off the platform, the consequences of this act being that the hose "dauces" around in the air like a snake throwing water on everything and everyone).

The screen should be set on a permanent type of wooden platform, not on the ground surface (Fig. 5). The amount of water that escapes the screen and run-off chutes wets the entire area around and under the screen and, if this is soil, begins to undercut the supports under the screen. A wooden platform raised above ground (at least one concrete block high) will allow the excess water to run off and will save time replacing the supports under the screen.

It is also very important to have a tin chute under the screen to carry the water coming through the screen back into the river (Figs. 6 and 7). This should be directed so that the mud-laden water flows into the river slightly down stream from the end of the intake pipe. If it is deemed necessary to use a smaller mesh screen during the excavation this can be attached to the end of the run-off chute. Such a screen was used in 1968 during excavation of Kirk Zone 18 at St. Albans in order to ascertain loss of smaller material. Approximately 300 very small chips were retrieved in this small screen.

In past years at St. Albans, a wooden platform has been constructed about 3 feet above the screen (Fig. 8). The wheelbarrows are pushed from the excavation onto this platform and then dumped into the screen. The platform gets very wet and slippery, therefore should be constructed of metal grating or grill if possible rather than solid wood. The platform is supported by three large tree trunks sunk about 3 feet into the ground (Fig. 5). The back side of the platform rests on the bank, held in place by stakes driven into the bank. The screen is also fastened to the three large tree trunks, therefore they are the key to the stationary condition of the entire sluicing system.

There are, naturally, many other methods of setting up a sluicing system, but the one just described in use at the St. Albans Site has been most effective. Many problems that have been encountered and solved over the years have made the system even more useful. Hopefully, others can start a sluicing system without the initial experimental stages.



FIGURE 1

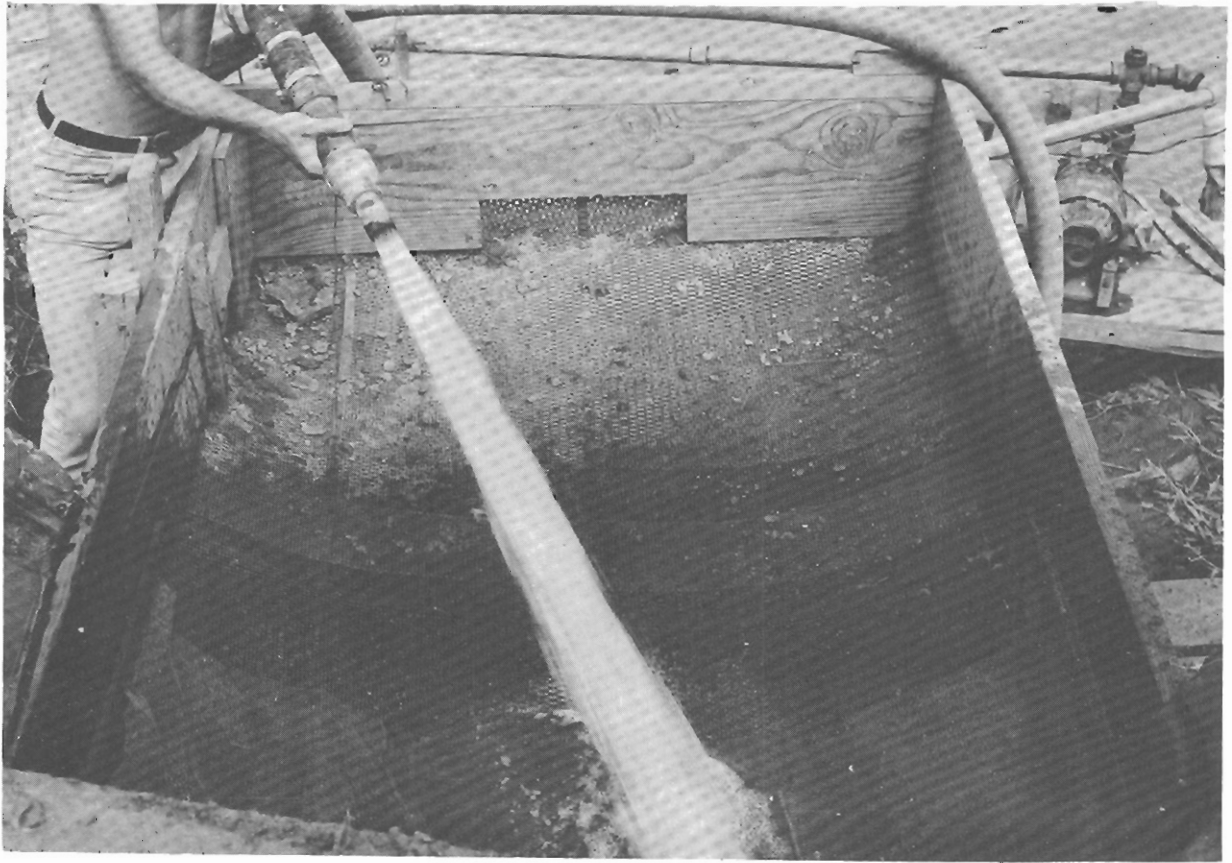


FIGURE 2



FIGURE 3

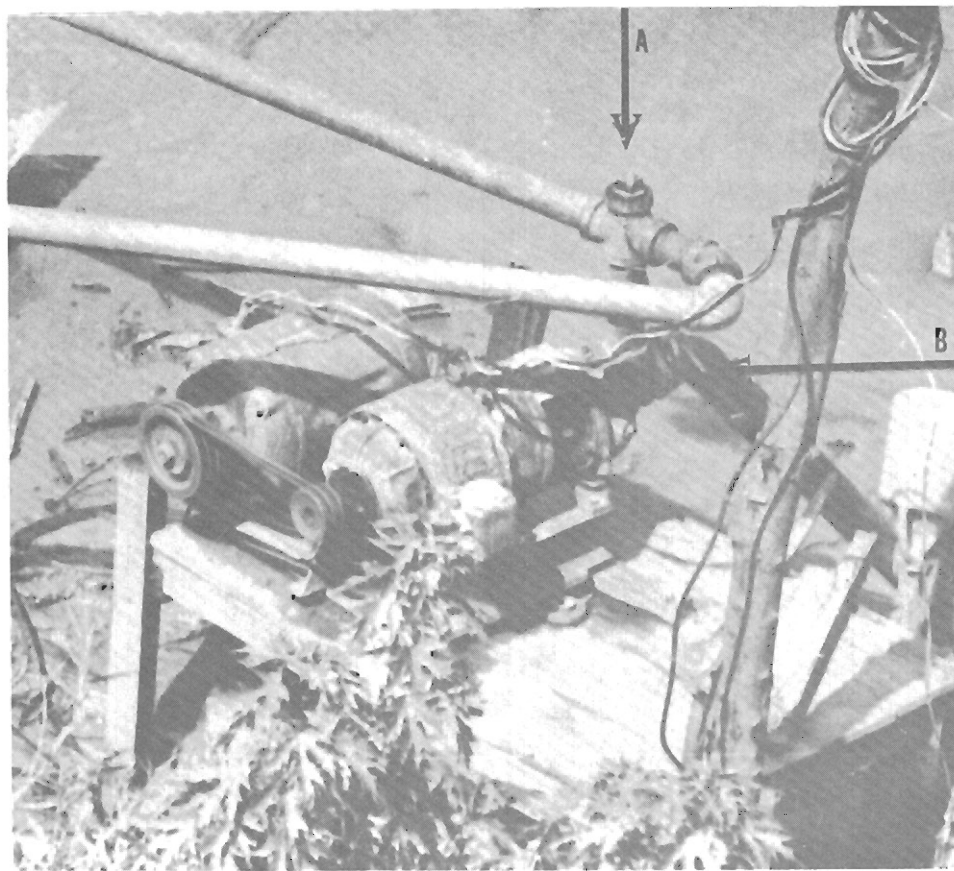


FIGURE 4



FIGURE 5



FIGURE 6



FIGURE 7

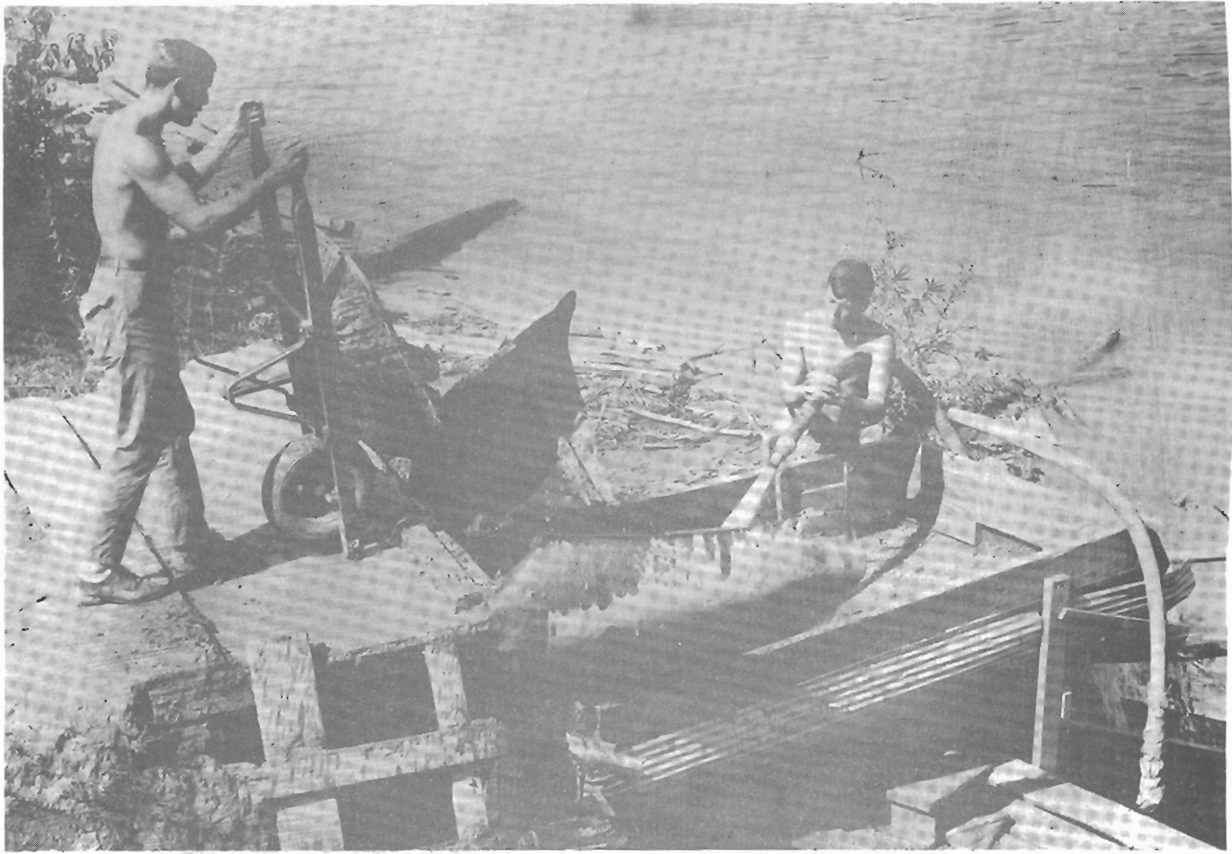


FIGURE 8

COMMENTS ON THE COPENA POINT AND ITS DISTRIBUTION

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The Copena point is a distinctive artifact that appears to have a limited spatial distribution in the Middle South. It was first described and illustrated by Webb and DeJarnette (1942:37, Pl. 29, Fig. 2) as a diagnostic point type for the Copena Focus. These writers noted the most diagnostic feature of the type, the blade shape, which "from the base, it first contracts and then expands, thus having edges concave from the base to two-thirds of its length, after which the edges become convex, and the blade comes to a sharp point" (*ibid.*:37). The type was formally described by Kneberg (1956:23) who noted the characteristic recurvate side edges and also observed that these edges and the basal edge were often lightly ground. Bell (1960:20) also describes this point although he quotes Webb and DeJarnette's and Kneberg's earlier descriptions. Bell's illustrations (Plate 10) are valuable for showing the variation in size and blade shape. The most recent authors to formally describe the Copena point are Cambron and Hulse (1969:25). Their contribution to the growing list of characteristics of this artifact include noting the occurrence of a long tapering (acuminate) distal end and establishing a minimum length of 80 mm. for the average sized artifact (*ibid.*:25).

A minimum of four salient characteristics can be established for the Copena point from the preceding descriptions:

- (1) It is a large, well-flaked artifact usually over 80 mm. in length;
- (2) The blade shape is recurvate with the greatest breadth about two-thirds the distance from the base to the tip;
- (3) The side edges and base are often ground;
- (4) The tip is sometimes tapered (acuminate).

Artifacts having these four characteristics appear to be limited to two areas of the Middle South; the Middle Tennessee Valley in northern Alabama, and the lower or western Tennessee Valley in Tennessee. In the former area they are definitely associated with the Copena Culture and have been found as grave goods in mounds of this Middle Woodland manifestation. Although they are not common burial accompaniments, 15 were found in the Fisher Mound in the Pickwick Basin, three of the exceptionally fine examples being in a cache (Webb and DeJarnette 1942:25-39, Pl. 29, Fig. 2). Both Kneberg (1956:23) and Bell (1960:20) call these artifacts projectile points, but their large size and fine workmanship suggest most of them are probably knives. This is further implied by their singular occurrence with a burial or their inclusion as small caches, although no study has been initiated to determine the actual function of these artifacts.

Copena points also occur in the western Tennessee Valley where they are associated with the Copena complex. Kneberg (1956:23) suggests some of these artifacts are found in a Late Archaic context. Although their cultural association is far from clear in this area, it seems more likely they are found in Early or Middle Woodland complexes. For example, the pottery-producing Late Archaic phases that Lewis and Kneberg (1959) believed lasted well into the Christian Era are now thought to be totally "Woodland" in cultural development. These Woodland complexes in the western valley were named the Decatur Focus by Lewis and Kneberg (1947:12), with one of the as yet undesignated phases of this

complex being coterminous and coeval with Copena of northern Alabama. The Copena points in the western Tennessee Valley probably occur in this unnamed phase.

A Copena-like point was found in Mound A at the Mandeville Site in Georgia (Kellar, Kelly and McMichael 1962:344) and Bell (1960:20) reports these artifacts in Mississippi. The latter reference may be to the Copena-type point found at the Bynum Site in the north-eastern part of the State (Cotter and Corbett 1951:Pl. 10, Fig. 19). Components on both of these sites show relationships to the Copena Culture.

Although these artifacts seem to appear sporadically south and west of the middle Tennessee Valley, their occurrence north and east of this area is questionable. For example, Copena points have not been specifically identified from any sites in the upper or eastern Tennessee Valley. Although Bell (1960:20) reports they are found in Kentucky, he cites no specific proveniences. The occurrence of the Copena point further north in the Ohio Valley proper is also doubtful. Webb and DeJarnette (1942:37-38) were informed that this artifact does not occur in the Ohio Hopewell Culture after ten specimens from the Pickwick Basin were sent to the Ohio State Museum for examination. A perusal of the more inclusive Adena literature (e.g. Webb and Snow 1945; Webb and Baby 1957) indicates its absence in this Ohio Valley Culture as well. A survey of the upper Ohio Valley did not record the presence of this artifact although some of the figured Plano-like lanceolate points are similar to the Copena point in shape (Mayer-Oakes 1955: Pl. 9).

The wider distribution sometimes reported for the Copena point could be due in part to the recent naming of a companion type that resembles certain point types in contiguous areas. This is the "Copena Triangular", formally described by Cambron and Hulse (1969:26) as a medium to large trianguloid point (43 mm.-74 mm.) with straight, lightly ground blade edges in the hafting area. Most of these artifacts are probably projectile points, as they are numerous on certain Copena habitation sites such as the Wright Village in Lauderdale County (Webb and DeJarnette 1942:176, Pl. 207, Fig. 1). A few of these Copena Triangular points have slightly incurvate blade edges, and could be mistaken for the "classic" Copena point although they are much smaller and more crudely made. Since in form they resemble other stemless triangular projectile points found in the Middle South (e.g. the Greeneville and Nolichucky types described by Kneberg 1957:64-65 of the Watts Bar Culture in upper East Tennessee) and also certain medium to large trianguloid and "leaf-shaped" knives from various prehistoric complexes in the eastern United States, it is easy to understand why the "Copena" appellation was indiscriminately used. To prevent further confusion, it is suggested the name "Copena point" only be applied to those artifacts conforming to the original type descriptions of this point found in the middle and lower Tennessee Valley.

REFERENCES

- Bell, Robert E.
1960 Guide to the Identification of Certain American Indian Projectile Points. SPECIAL BULLETIN NO. 2 OF THE OKLAHOMA ANTHROPOLOGICAL SOCIETY. Oklahoma City.
- Cambron, James W. and David C. Hulse
1969 HANDBOOK OF ALABAMA ARCHAEOLOGY, PART 1: POINT TYPES. University, Alabama.

- Cotter, John L. and John M. Corbett
1951 Archeology of the Bynum Mounds, Mississippi. NATIONAL PARK SERVICE ARCHEOLOGICAL RESEARCH SERIES NO. 1. Washington.
- Kellar, James H., A.R. Kelly, and Edward V. McMichael
1962 The Mandeville Site in Southwest Georgia. AMERICAN ANTIQUITY, Vol. 27, No. 3, pp. 336-355. Salt Lake City.
- Kneberg, Madeline
1956 Some Important Projectile Point Types Found in the Tennessee Area. TENNESSEE ARCHAEOLOGIST, Vol. 12, No. 1, pp. 17-28. Knoxville.
1957 Chipped Stone Artifacts of the Tennessee Valley Area. TENNESSEE ARCHAEOLOGIST, Vol. 13, No. 1, pp. 55-56.
- Lewis, T.M.N. and Madeline Kneberg
1947 The Archaic Horizon in Western Tennessee. TENNESSEE ANTHROPOLOGY PAPERS, No. 2. THE UNIVERSITY OF TENNESSEE RECORD EXTENSION SERIES, Vol. 23, No. 4. Knoxville.
1959 The Archaic Culture in the Middle South. AMERICAN ANTIQUITY, Vol. 25, No. 2, pp. 161-183. Salt Lake City.
- Mayer-Oakes, William J.
1955 Prehistory of the Upper Ohio Valley: An Introductory Archaeological Study. ANNALS OF CARNEGIE MUSEUM, Vol. 34. Pittsburgh.
- Webb, William S. and Raymond S. Baby
1957 THE ADENA PEOPLE NO. 2. Columbus.
- Webb, William S. and David DeJarnette
1942 An Archaeological Survey of Pickwick Basin in the Adjacent Portions of the States of Alabama, Mississippi, and Tennessee. BUREAU OF AMERICAN ETHNOLOGY, BULLETIN 129. Washington.
- Webb, William S. and Charles Snow
1945 The Adena People. UNIVERSITY OF KENTUCKY REPORTS IN ANTHROPOLOGY AND ARCHAEOLOGY, Vol. 6. Lexington.

NOTES ON TWO
HUMAN VERTEBRAE WITH PROJECTILE POINTS

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While examining a large collection of Indian material at Davis and Elkins College in Elkins, West Virginia, the vertebra pictured below was discovered.

A side notched projectile point made of pink and tan mottled chert (possibly Flint Ridge, Ohio, flint) entered the individual's body from the right side and lodged in one of the Lumbar vertebrae. As can be seen in the X-ray, the point did not shatter or break, possibly because of the spongy interior of the vertebra.

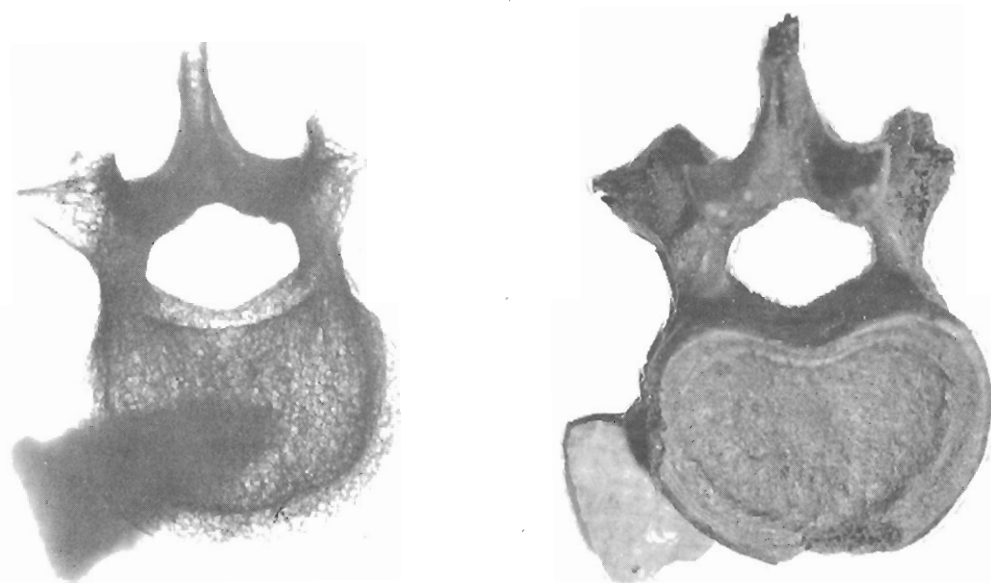


FIGURE 1- X-ray (left) and photograph (right) of vertebra containing projectile point. Photos are actual size.

Figure 2 illustrates a dorsal vertebra from the Late Prehistoric Buffalo Site in Putnam County, West Virginia.

Only one projectile point was actually embedded in the vertebra, although the other eight points were recovered from the chest area, indicating that all nine had entered the body. The skeleton was that of a young adult female.

The most interesting feature of this association of projectile points with a vertebra is that all of the points were made of a material (light tan chert) foreign to the Kanawha Valley. The vast majority of the projectile points (and other stone artifacts as well) from the Buffalo Site were made from the local Kanawha Black Flint. It may be possible to trace the source of the flint and show a direct association with another Fort Ancient group (most likely from Kentucky, but possibly from Ohio or somewhere else in West Virginia) who, apparently, were not too friendly toward the Fort Ancient people living at the Buffalo Site,

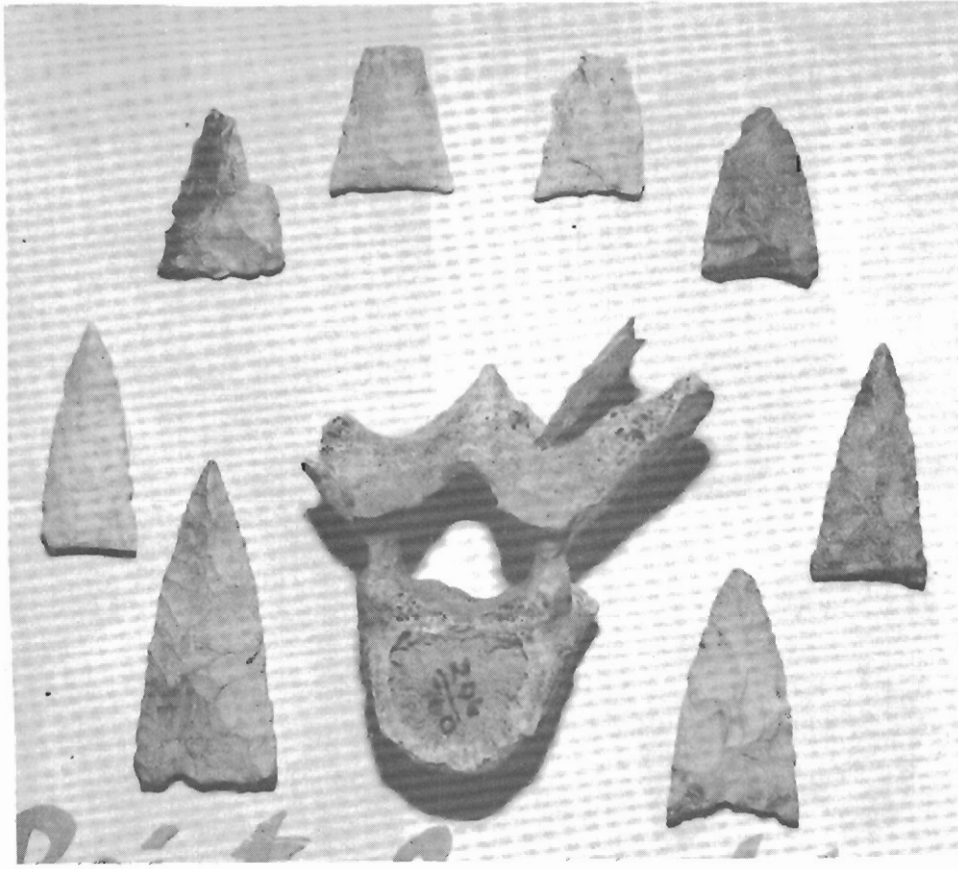


FIGURE 2- Human vertebra with one projectile point embedded and eight others which were found in the same skeleton at the Buffalo Site, Putnam County, West Virginia.

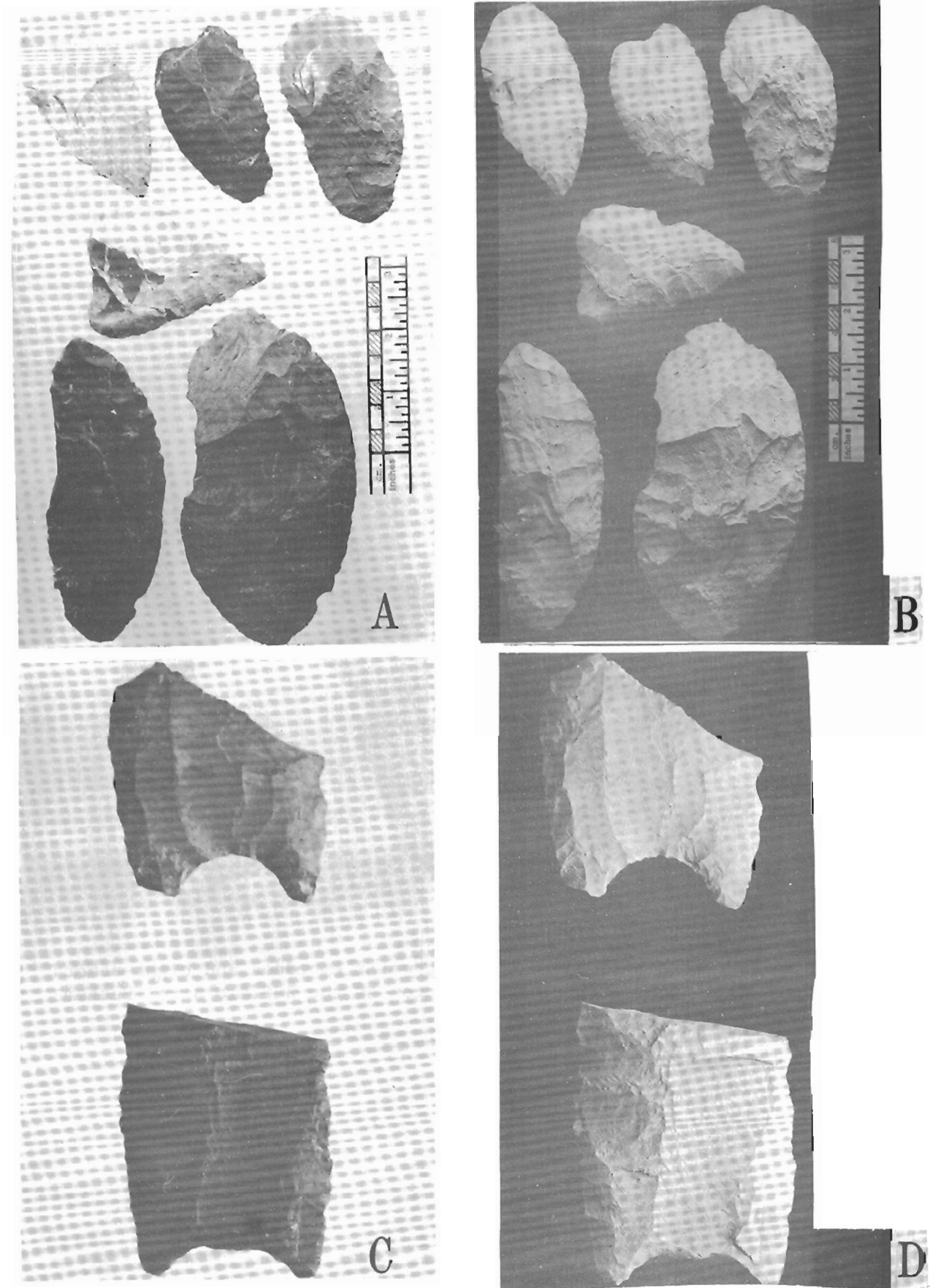


FIGURE 1- A, Paleo-Indian knives photographed without treatment; B, the same knives photographed after treatment with ammonium chloride powder; C- bases of two fluted points untreated; D- the same bases after treatment with ammonium chloride powder.

A SIMPLE AMMONIUM CHLORIDE GENERATOR FOR USE IN OBSERVING AND PHOTOGRAPHING
CHIPPING DETAILS AND WEAR EVIDENCE IN ARTIFACTS

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At the November, 1970, meeting of the Eastern States Archeological Federation I presented a slide illustrated paper concerning a newly discovered Paleo-Indian occupation site in New Jersey. In order to more effectively illustrate the superb lithic technology manifested on the fluted points, scrapers, graters, and other tools of specialized design, I prepared two sets of slides for simultaneous projection: one in natural color, and the other showing the artifacts coated with a microscopic film of dull white powder which effectively opaqued the artifact and reduced surface luster in order to accentuate chipping and fluting details and striations produced from wear. The visual effect was sufficiently dramatic to induce a number of those present to ask for an explanation of the process used. Your editor also thought the use of this technique potentially beneficial to the numerous professional and amateur archaeologists who were unable to attend the meeting, and therefore asked me to prepare this paper.

The use of an ammonium chloride generator for the preparation of specimens for microscopic examination or photography has been known for some time. I do not know who is entitled to the credit for this innovation in archaeological work, but the technique was first brought to my attention by Junius Bird of the American Museum of Natural History. It is, nevertheless, rather strange that even so technically oriented a scholar as Dr. S.A. Semenov does not mention this comparatively simple process in his otherwise very useful and informative book Prehistoric Technology (1964).

The process involves two common chemical liquids: hydrochloric acid ($H_3O^+ + Cl^-$) and Ammonia ($NH_4^+ + OH^-$). When unbottled, or when agitated, each of these substances produces a rather noxious gas: HCl and NH_3 respectively. However, our interest lies in the fact that when these two gasses are brought into contact with each other they produce a solid, i.e. a very fine white powder. In chemical terms this reaction is expressed: $HCl + NH_3 \rightarrow NH_4Cl$. This powder is controllable, and is very useful in neutralizing the translucency of flints, cherts, quartzes, and other lustrous substances without in any way damaging the specimens. The microscopic film will last as long as desired, provided you do not touch it or wet it, and the coating can be easily and permanently removed simply by wiping it off, or even more easily by dipping the specimen in water.

The equipment needed is readily available from any chemical laboratory or supplier. I have used the following, but certain substitutions are possible.

- 1 mouthpiece- "Y" or "T" connector, glass
- 2 bottles- 8 ounce
- 2 six-inch-long glass tubes- $\frac{1}{2}$ inch O.D.
- 2 two-inch-long glass tubes- $\frac{1}{2}$ inch O.D.
- 2 rubber stoppers with two holes to fit bottle openings and to receive the glass tubes. The tubes must fit snugly.
- 4 lengths of rubber tubing about 18 inches long- $\frac{1}{4}$ inch I.D. to fit "Y" connector and glass tubes.

Prepare the perforated bottle stoppers by inserting a long and short rod through each of the holes. The long rod should reach nearly to the bottom of the bottle when the stopper is in place, the short tube should just barely penetrate the stopper. Each glass tube should project about one inch above the stopper and the rubber tubes are attached to these glass tubes (Fig. 2).

Working by an open window or in an otherwise well ventilated area, fill one bottle about half full of hydrochloric acid; the other bottle about half full of ammonia. Place the prepared stoppers tightly into each bottle. Now join the hoses leading from the long glass tubes in each bottle to a mouthpiece (Fig. 2). Steadily blow air through the mouthpiece into the liquid in order to bubble it up. The vapor, under air pressure will issue from the hoses leading from the short glass tubes. The latter should be held in the right and left hands respectively and the open ends of the rubber hoses should be brought together over the artifacts being prepared. As the gasses emerge and contact each other they will produce a white mist or powder.

The amount of powder and the degree of whiteness on the artifact can be controlled by both the air pressure and the length of time the gasses are in contact over the artifact. If the hydrochloric acid and ammonia are of good strength the object can be coated in about one minute, depending upon size. Move the two hoses, and their vapors, back and forth across the artifact to insure an even coating.

Again I would caution you about working in a well ventilated area or by an open window, both the hydrochloric acid fumes and the ammonia fumes are asphyxiating. Given proper ventilation, however, the job is easily and safely done.

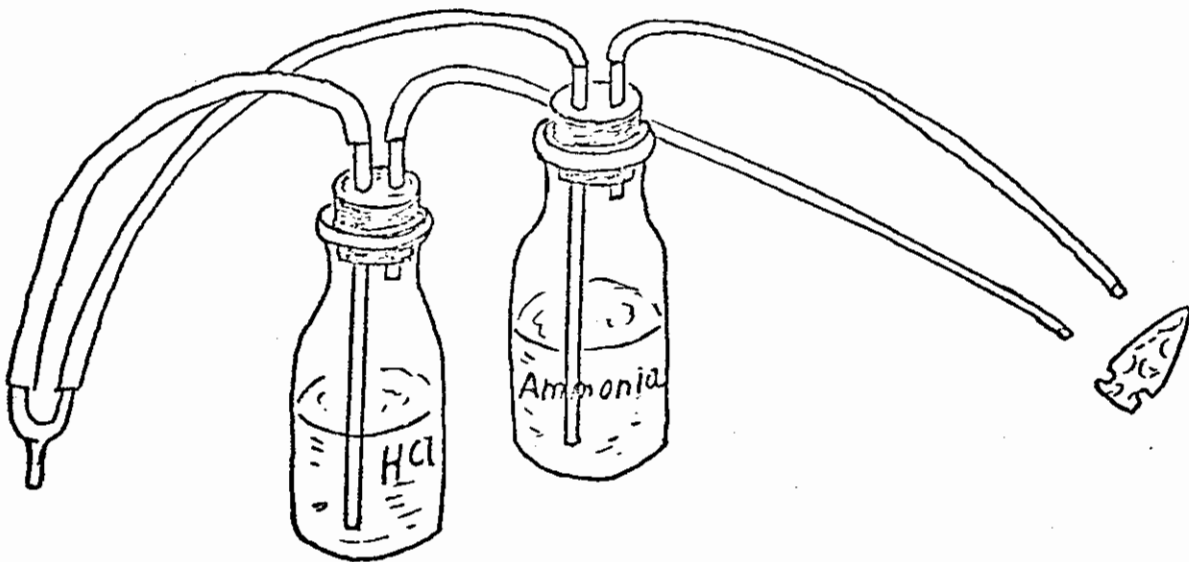


FIGURE 2- Ammonium chloride generator for ordinary use in coating artifacts.

The whitened artifact is best photographed on a piece of black velvet which provides contrast while at the same time absorbing the shadows. To transport the coated artifacts from the area of preparation to the photographic table or microscope, care must be exercised to avoid touching and thereby impairing the dusted area.

With prolonged use a condensation may occur which can drip on the artifact thereby spoiling the desired finish. To avoid this condition interpose a second set of empty bottles between the emerging vapor and the open ended tubes. The wet vapor can thereby cling to the walls of the empty bottle and collect while allowing the drier vapors to emerge for purposes of coating (Fig. 3).

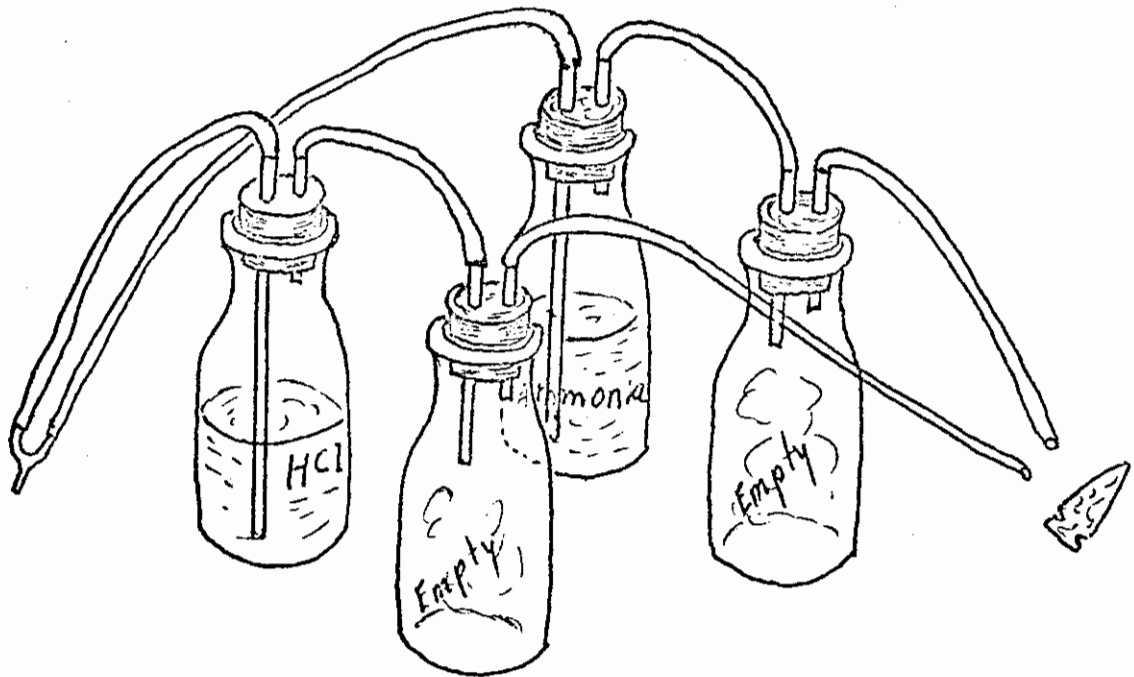


FIGURE 3- Ammonium chloride generator and condensation traps intended for prolonged use in coating artifacts.