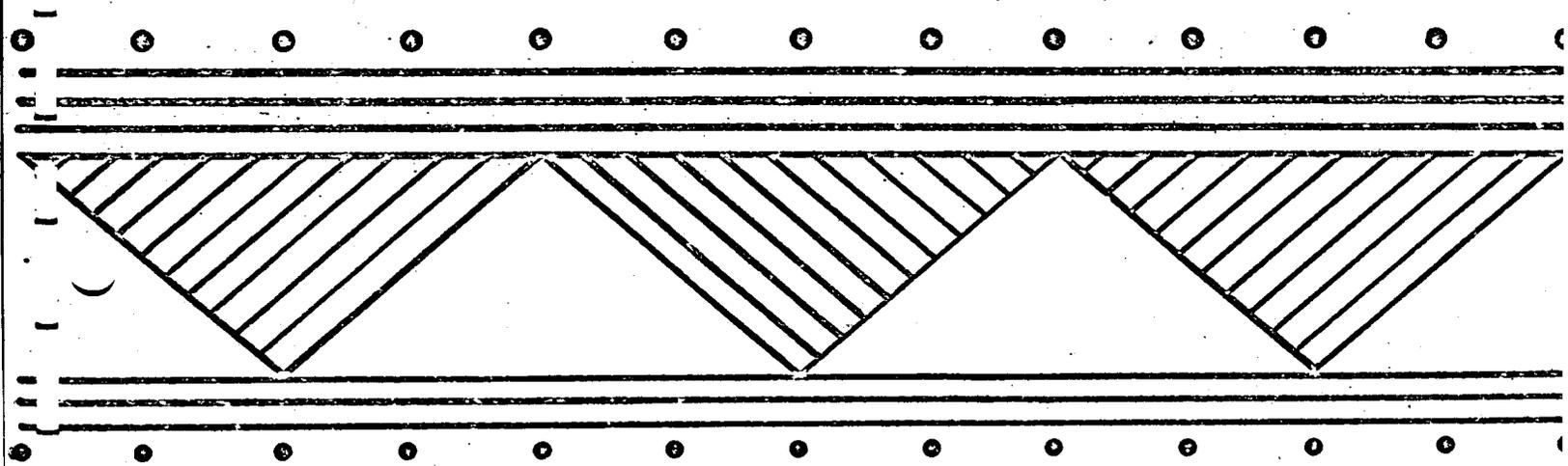


HOUSTON ARCHEOLOGICAL SOCIETY NEWSLETTER

NUMBER 45

APRIL 1974



The Newsletter is published four times per year by the Houston Archeological Society. Contributions of news items, short articles and information of archeological significance should be sent to the Editor - an R. Duke, 1706 Oaks Drive, Pasadena, Texas 77502.

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HAS Officers - 1973-74

- Chairman - Alexander Macnab, 6023 Portal, Houston, Texas 77024
Sec.-Treas. - Shirley Thompson, 3816 Ruskin, Houston, Texas 77005
Directors - David Salzar - Leland Patterson - Barbara Kuether

#

Past and Future Programs - 1974

- March - 1974 - Dr. Danuta Batorska, College of Arts, University of Houston spoke on "Archeology and the Art Historian".
April - 1974 - Mrs. Marjie Lohse, PhD Candidate, Rice University, discussed "Preparing for a Career in Archeology".
May - 1974 - To be announced.
June - 1974 - Dr. Reginald Wilson, Dayton, Texas, will speak on ancient Mayans of Okop and modern Mayans of Dzoyola.
July - 1974 - Review of the TAS Field School.

Coming Events - 1974

- April 26-28 - Archeological Society of New Mexico - Annual Meeting Hobbs, New Mexico.
May 10-12 - Rock Art Symposium, Farmington, New Mexico.
May 20 - Aug. 17 - Lubbock Lake Site Dig. Contact Claude Brown, 2415 31st St., Lubbock, Texas or Jim Word, 108 W. Missouri St., Floydada, Texas; for reservations.
June 8-16 - TAS Field School. McKinney Homestead Complex (41TV289), Austin, Texas. You must be a TAS member to attend so join up and get your Field School application in by May 20, 1974.
Nov. 1-3 - TAS Annual Meeting, Dallas, Texas.

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The Need to Publish

- J. W. Patterson

It currently appears that the gasoline shortage may limit field work this year. Limitations in field work can provide time for working on the publication of past finds. Isn't it time that you should be thinking of consolidating your previous work?

While reporting of new sites to the Texas Archeological Research Laboratory is important, to provide adequate records for future research, this is not sufficient for proper preservation of information, and providing of generally available data. The following are reasons why publication of archeological site information is important:

1. Limited manpower may mean that site information can not be published in most cases.
2. The original discoverer of a site is the only person with knowledge of all available details.
3. Archeological information is not useful to a maximum number of people until generally available in published form.
4. Prompt publication may aid a current research project in need of additional information.

Anyone familiar with the nature of archeological research comes to realize that published information is like the tip of an iceberg. A great quantity of information is submerged by lack of publication and is generally unavailable. Also, no matter how good is a given site report, some information can be lost if the original site discoverer does not participate in the published report.

United States archeology lacks the resources to do two very important things: publish all available data, and collect and correlate the distribution of artifact types over large areas. Amateur archeologists could be of great help on both of these subjects.

Harris County is a good example of inadequate publication. There are now approximately 270 archeological sites known in Harris County, but only a handful of these sites are published. In this area, generally available information is very small compared to potentially available data. As time goes on, information continues to be lost.

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Editor's Note:

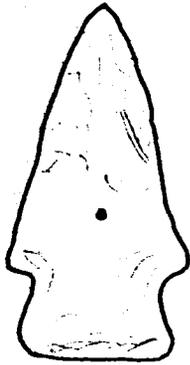
To Lee Patterson's comment in regard to lack of resources by United States archeology to do two very important things, your Editor would like to add a third: excavate important sites that are disappearing almost daily due to construction, subsidence and potholing. Many of the 270 known sites in Harris Co. are no longer recognizable and are lost forever. The "potentially available" cannot be salvaged unless sites can be excavated promptly. The amateurs and should help here under the direction of professionals or other knowledgeable, experienced personnel.

A.R.D.

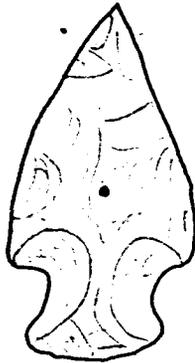
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How about that?

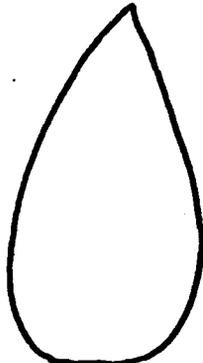
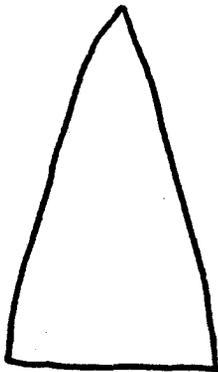
W.L. McClure



'A'



'B'



Generalized outlines.



Longitudinal sections.

Cross sections at
center of gravity.

These two dart points were found together where they had eroded out of the banks of White Oak Bayou at 41 HR 259 in Houston, Texas

On first examination the points appear to be quite different even though both would probably be classed as Ellis type. 'A' gives the impression of a triangle and 'B' suggests a pointed oval. In addition to the outline being different, the longitudinal section is not alike. The thickest point on 'A' is in the distal 1/3 while on 'B' it is in the proximal 1/3. The width of the stem and of the base are less on 'B'. The center of gravity is marked by a dot in each in the sketches. The type of flint is different. The colors are different but both have been subjected to considerable loss of color due to patination.

In spite of the differences there are many similarities. The weights are the same to the nearest 1/100 gram! Total length, blade length and stem lengths are the same. Maximum width and maximum thickness are the same. The expanding stem edges have been dulled somewhat on both. Bases are thinned.

These many similarities strongly suggest that the points were made by the same person with the intention that they perform the same function. If this is true then perhaps some deductions about projectile point production can be made. These deductions would be that the following characteristics were important: weight, length of blade, length of stem, length overall, thickness. The following characteristics were not important: outline of blade, longitudinal section, width and shape of base.

These deductions would apply to the production of this type of artifact by one artisan. Perhaps similar comparisons of other material will refute or reinforce these conclusions.

	'A'	'B'
Weight	9.29 g.	9.29g.
Total length	47 mm.	47 mm.
Blade length	34 mm.	34 mm.
Stem length	13 mm.	13 mm.
Maximum width	23 mm.	23 mm.
Minimum stem width	17 mm.	15 mm.
Base width	20 mm.	18 mm.
Maximum thickness	10 mm.	10 mm.
Color	10YR8/2	10YR6/6

measurements are to nearest mm.

Artifacts from San Jacinto

Alan R. Duke

April 21, 1974 is the 138th anniversary of the battle of Saint Hyacinth (San Jacinto) so it seems appropriate to discuss, in this April Issue of the Newsletter, some of the artifacts discovered in the battleground area by the writer and sons, Bruce and Gary. These artifacts were found on private land, outside the Park boundaries. Contacts with "old-timers" and longtime residents of the area indicate the bulk of the very limited amount of battleground material found, which was left behind by Mexican troops fleeing the wrath of the Texians, has been located in areas outside the park. A study of the topography of the area and early accounts of the battle support these observations. Certainly the greater portion of the Mexican military equipment lies under the mud of Aunt Peggy's Neck (Gap), a narrow outlet between Peggy's Lake and San Jacinto Bay, where a large number of Mexicans bogged down and drowned as they tried to escape. The victorious Texians must have salvaged equipment as the spoils of war and made good use of it after the battle. Perhaps there were some relics found within the Park boundaries before the area was declared a State Park but to the writer's knowledge battlefield material has not been found by authorized personnel on State land in recent years.

Following is a detailed description of the artifacts:

Figure 1 - Gun Flint

This "flint" is well made from a very good grade of dark gray flint. It is known that many of the early gun flints were made in Europe but in this case it is not known whether this piece is of Mexican, European or local manufacture. The flint is 1-5/8" x 1-3/8" x 3/8" with parallel top and bottom surfaces so that the jaws of the cock would hold it securely. There are some conchoidal fractures on the edges of the flint but these appear to have occurred during manufacture rather than through useage.

Figure 2 - Gun Sling Swivels

These swivels are particularly interesting. There are three types represented - one with beveled inner edges (top figure) one without beveling (middle figure). These first two have an oval cross-section while the third type is almost square. All three show the file marks of hand manufacture but the beveled swivel exhibits marks much deeper than the other two. All three swivels show heavy patination ranging from green to dark brown. They appear to be made from copper high in tin content - actually a bronze.

Figure 3 - Musket Balls

Four of the five lead musket balls found have the same diameter - 11/16". The fifth ball is slightly smaller - 5/8". All are patinated but the deposit is uniform and does not alter the symmetry of the castings. The larger balls weigh approximately 438 grains while the smaller ball weighs 263 grains. It is possible four of the five balls came from the same mold but the fifth obviously was poured in a different mold.

All the balls show a "belt" around their circumference which is typical of cast lead projectiles and occurs at the point where the halves of the mold come together. Belted musket balls were made, by design, for rifled muskets to improve their performance, but these "belts" appear to be mold marks.

Figure 4 - Decorative Plate

One can only speculate how this thin, patinated, copper (brass?) piece was used. It has a small hole ($3/16$ ") on one edge and is quite thin (about $1/64$ "). Perhaps it was used to decorate a musket. It is too fragile to be of utilitarian value. The piece is $3-7/16$ " long by $1-7/16$ " at its widest part.

Figure 5 - Medallion

This unusual piece may have graced the hat of a Mexican soldier. It is made of alloy copper with the figure of an eagle in relief. The bird is perched on prickly pear cactus with wings spread and its head facing right. (called a "facing eagle" by numismatists, it was adopted for national use on Mexican coins in 1824). The eagle does not appear to have the usual snake in its beak that is characteristic of Mexican coinage but then all early "facing eagle" coins did not include the snake either.

Two small holes on each side of medallion were used to secure the piece. A thin indentation outlines its periphery.

Figure 6 - Gun (Musket) Butt Plate

This massive butt plate weighs almost one half pound and is made of brass well covered with a green patina. It is $5-1/8$ " high and 2" wide between the beveled $3/8$ " holes used to secure it to the stock. A strap $3-1/2$ " long extended along the top of the stock with a small perforated tab extending from it at a 90° angle. Presumably this tab also helped to hold the plate on the stock - probably with a pin thru the wood.

The plate obviously is hand made with file marks evident on the edges. Levels on the screw holes are not uniform and were made with a hand drill. The screws, when secured, were recessed $3/16$ ".

Identifying marks are non-existent so it cannot be stated with any degree of assurance that the butt plate was of Mexican origin. Undoubtedly it is old and by association with the other artifacts and in view of the fact that the Texans probably did not leave their weapons, we can assume the plate probably came from a Mexican musket.

Sketches of the San Jacinto artifacts are shown on the next two pages. All drawings are full scale (actual size). The black and white sketches do not do justice to the pieces since the rich, green patina on most of the bronze must be seen to be appreciated. All the artifacts are remarkably well preserved after exposure for 138 years to the humidity and soil conditions characteristic of this section of the Gulf Coast.

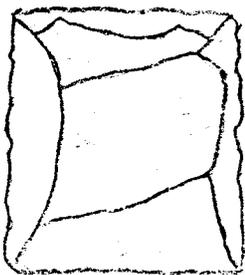


Figure 1
Gun Flint

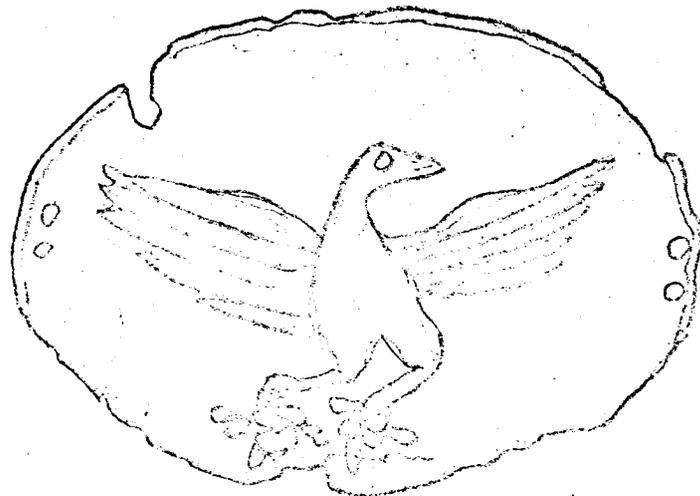


Figure 4
Medallion

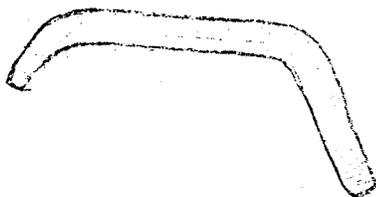
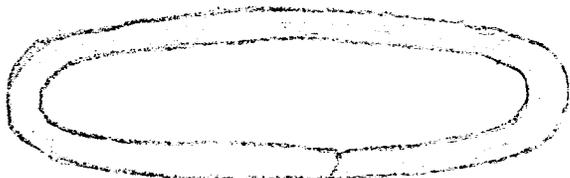
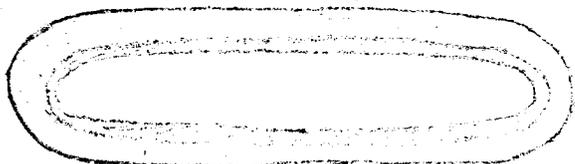


Figure 2
Gun Sling Swivels

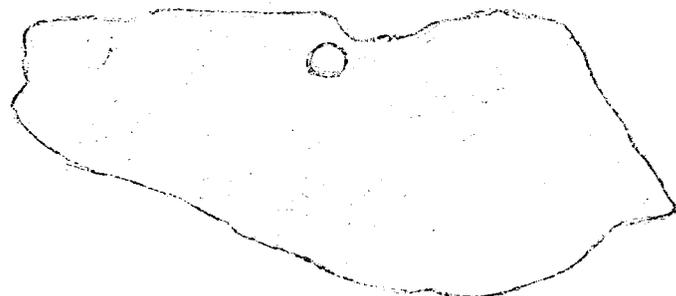


Figure 5
Decorative Plate

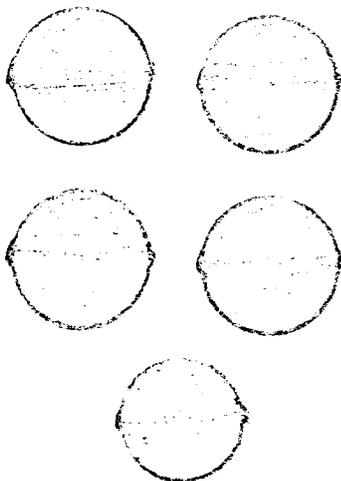
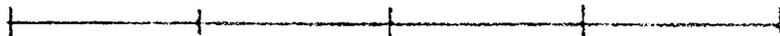


Figure 3
Musket Balls

All Figures Actual Size

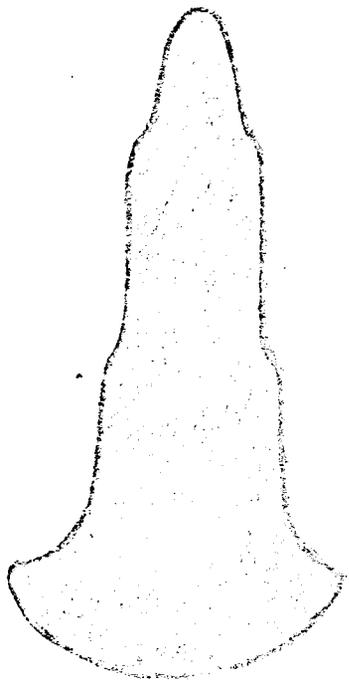


Inches

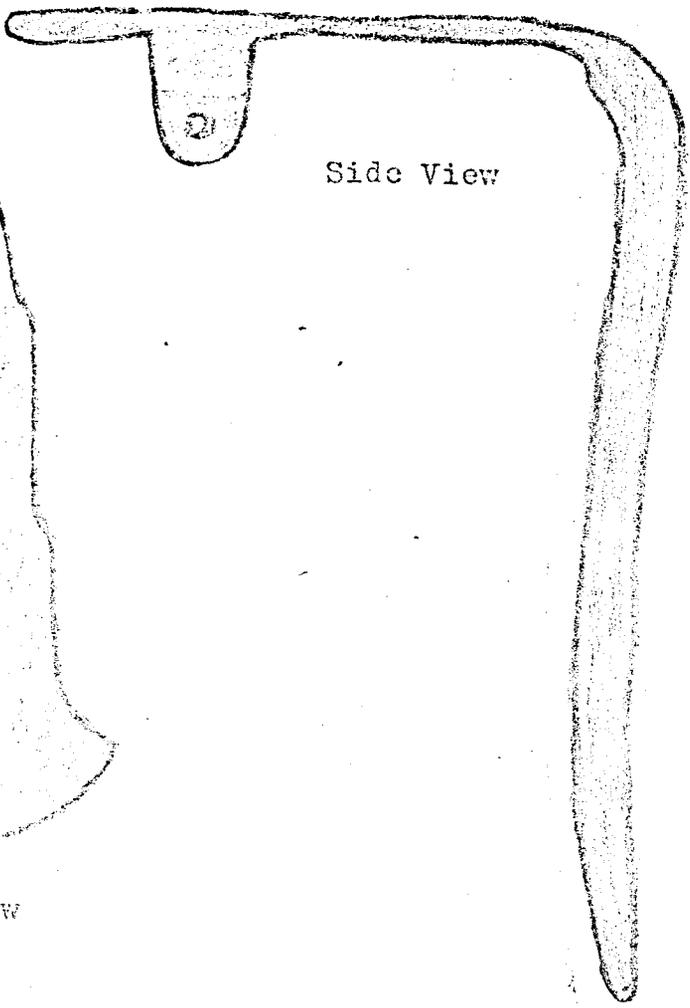
Artifacts From San Jacinto



End View



Top View



Side View

Figure 6
Gun Butt Plate

All Figures Actual Size

The writer would welcome information on other artifacts from the battle of San Jacinto. The dearth of battleground material in museums and private collections from this glorious moment in Texas history makes it difficult to analyze the pieces on hand. Please contact the author if you have knowledge or information on other artifacts from the battle.

A.R.D.

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#

Past and Future Programs - 1973-74

- October - 1973 - Charles Chandler spoke on "Lithic Technology and Classification."
- November- 1973 - Dr. Frank Hole discussed migration as it pertains to nomadic tribes in western Iran.
- December- 1973 - Dr. Dee Ann Story presented an illustrated lecture on "Artifact Conservation Techniques Used In Preserving Material From The Spanish Galleons."
- January - 1974 - Dr. Thomas R. Hester discussed "Perishable Artifacts From A Cave In Western Nevada."
- February- 1974 - Professor George Carter, Texas A&M University, will present his views and findings on "Early Man In America." Professor Carter has been the champion of the theory of the early arrival of man in the New World for over 20 years and has worked in Southern California in the La Jolla and San Diego areas. Suggested reading on the subject includes "Ancient Man In North America" - H. M. Wormington (Page 222); "Early Man In The New World" - Macgowan and Hester (Page 182-183); "Man, Time and Change in the Far Southwest" - G. F. Carter; "The Calico Site: Artifacts or Geofacts" - Vance, Haynes (July 1973, Vol. 181, No. 4097 - "Science"); "An Introduction to American Archeology" - Gordon R. Willey (Vol. I, pages 26-77); "Prehistoric Man In The New World" - Jennings and Norbeck (pages 117-148).

#

Former HAS Member, geologist and resident of Houston, Texas and Addis Ababa, Ethiopia, Jon Kalb, helped organize an expedition into the Afar Depression in northeastern Ethiopia in 1972. Discoveries included human skeletal material that Kalb says could predate by about one million years the oldest "human" bones found to date. The research expedition plans further work in the area in the near future as funds become available.

#

Comments on the "New" Archeology

T. W. Patterson

Anyone who deals very much with current archeological literature will sooner or later come across the term: the "new archeology", or at least the jargon which is its byproduct. There is much discussion among professionals as to the value and exact meaning of current trends in archeology. As an interested amateur, I would like to offer my comments on this subject.

The term "new archeology" as currently presented offers at one time a service and a disservice to its field. On the positive side, this terminology calls attention to recent general progress, and the wide variety of scientific and mathematical techniques now available. On the negative side, this terminology threatens to bury itself in its own unintelligible jargon and self-esteem.

In any other technical field or scientific research, this terminology would simply be equated to recent progress. However, in archeology a more complex viewpoint prevails. Archeology, which lacks a really cohesive body of theory, seems to be looking to newer work to develop a more meaningful framework. There is nothing wrong with a discipline attempting to find a richer meaning for itself, but carried to extremes this exercise becomes a semantic contest with no meaning connected to the real world. One has only to hear a few of the papers delivered by current graduate students, to realize that "progress" is being made toward classical models of beautifully precise statements that have little meaning in application.

It seems to me that American archeology is at the same place that industrial research was 15 years ago, with the start of electronic computers; and the attendant use of mathematical techniques previously not practical using manual calculations. A field called Operations Research developed, analogous to the "new archeology", which claimed to revolutionize business and scientific planning methods. Operations Research was, of course, an artificial science; and 15 years later we can now judge its real impact, just as later we will be able to do with the "new archeology". After a shakedown period, the useful techniques of Operations Research were integrated into normal business and scientific activities, and the superfluous techniques and jargon were discarded. As a person who has gone through this before, it is somewhat painful to hear words such as "algorithm" pop up again, in archeology. In a field such as archeology, where so much depends on the value of written communications, the use of new terms should be avoided, unless they serve a specific clarifying function. Some words favored by contemporary archeology are:

Algorithm: solution technique

Epistemology: the study of the nature of knowledg

Nomothetic: formulating general statements or scientific laws

Paradigm: model

Processual: any phenomenon that shows a continuous change in time

Substantive: having substance, real rather than apparent

Taxonomic: scientific classification

If contemporary archeology simply emphasized the latest technical and theoretical progress in its field, there would be little to discuss and pompous jargon could be ignored. In a field ever ready to espouse a consensus dogma, however, there are more serious implications. This is especially true

in a field with such limited monetary and manpower resources, that can not afford the luxury of unproductive activity, or the inability to communicate with the public that provides funds and a good deal of the basic site data.

Recent archeological activity has rightly emphasized the need for broader studies in environmental and adaptive processes. As new trends, however, these valuable additions to obtaining a more detailed understanding of man's past have tended to replace, rather than supplement, older goals and problems. One presentation (Leone 1972, p.21) even states that now a broad outline of pre-history exists, emphasis should be shifted to "processual" studies. What this neglects to state is that there is still much meaningful work to be done in establishing a better basic sequence of prehistory. As in my analogy of Operations Research, the "new archeology" has over-reacted as to its own importance. After a period of trial, it is predicted that archeology will be richer because of the enthusiasm and technical expertise given by the latest generation of archeologists. When the smoke clears away, however, the older basic goals and problems will still remain, but without the artificial isolation from current work.

In emphasizing current activities only, much knowledge can be lost. Archeology is a cumulative activity. Excavations are only done once, and many fine reports have been published and then passed out of public view. A continued effort to retain and synthesize past work should be made, instead of stressing the limited results of previous studies.

Aside from any gross misapplication of statistical techniques, the "new archeology" has not yet fully grappled with the classical "GIGO" (garbage in, garbage out) problem. Studies in the relationships of independent variables are sensitive to the quality of data and the structure of models. The writer can still recall his dismay in finding that a complex statistical regression analysis of chemical reactions gave results exactly opposite to correct theoretical predictions, when using actual oil refining data. Poor data gives poor results, so that solutions are not available for every formulated problem. My impression is that the Binford's (Binford and Binford 1966) model of Mousterian stone tool uses may fall into this category.

While the Binford's goal of relating specific stone tool groups to specific human activities is admirable, there are several difficulties to this type of study, as follows:

1. For the present at least, there is probably not enough information to test the accuracy of this model. A clear summary has been given (Binford and Binford 1969) of results, which even non-technical people can understand, but nothing is done to independently test the model. The Binford's themselves clearly state that further testing is required. Subsequent literature (Leone 1972, pp.193-194), however, accepts this work as a major contribution, before it has been established if this statistical model is accurate or merely gives the illusion of accuracy.
2. All statistical models that attempt to correlate the interaction of a large number of independent variables (say over 5) are suspect without rigorous testing. Statistical methods of this type are generally good for selecting a few of the most significant variables but not for correlating the "world". Large mathematical models generally start with the known behavior of variables, and seldom depend on initially establishing the function of each of a large number of variables. The Binford's claim success in establishing the significance of 16 out of 40 tool types as one

tool group, for example. This is somewhat difficult to accept, when compared with the successes and failures of statistical models in other fields.

3. Correlations involving a large number of independent variables are generally sensitive to the accuracy of the raw data. In the case of correlating stone tool functions, the accuracy of the initial data is not testable, let alone the model results, as stone tool classification must use a certain amount of subjective judgement. For example, no one is ever sure how many artifacts are really true burins.

I have dealt with this example in detail, as it seems to be used as support for the superior logical power of the "new archeology". For the technically minded, a distinction should be made between the Binford's work of isolating the interdependence of independent variables, functional uses of stone tools in this case, and Bordes' (1972, fig. 15) well known work of relating entire tool assemblages in space and time. Bordes uses an additive function of the entire tool group, which may become more accurate as the number of variables increases, since each variable becomes less important in a mass comparison.

There are several problems facing the more theoretical practitioners of the "new archeology" as follows:

1. They may not be engaged in the field of archeology at all, but may instead be engaged in the analysis of the logic of analytical systems. I find it highly significant that the latest issue of American Antiquity brings a philosopher (Levin 1973) to point out the nature and applicability of logical systems. Levin (1973, p.387), by the way, in his desire to meet theoretical archeologists on equal terms, has formulated a near perfect model of an obscure abstract. The content of his main article is well worth reading, however.
2. General models are difficult to apply, due to their complexity. Archeology sometimes also has the problem of mixing quantitative and qualitative information in a model, which can lead to a breakdown in application. The really successful uses of systems models have involved finite, manageable areas, that are of course not then widely generalized models. The term "general systems theory" may have some meaning to persons involved in the analysis of logic, but it is at best a vague term for working scientists. It involves a number of analytical techniques that may or may not have application in the empirical (real) world.
3. The quality and accuracy of information is important in any scientific endeavor. The limits of attainable knowledge is not a new problem for science, but newer complex analytical techniques sometimes give the illusion of accuracy, when data is forced to fit a theoretical model.

In a recent book on the history of archeology in North America, the editor (Fitting 1973, ch. 1) discusses the current "crisis" and "revolution" in archeology. After quoting a prediction by Hole and Heizer (1969, vi-vii) of significant future conceptual changes, Fitting spends some time pondering whether the "revolution" has passed, is arriving, or is still to come. My retort is, "which one?" I was fortunate enough to be able to ask Dr. Hole exactly what was intended by the 1969 prediction. If I interpret his reply correctly, he simply meant that application of new techniques would lead to great progress. Since this is the history of most active scientific disciplines, the terms "crisis" and "revolution" are only warranted to the

limited extent that each generation operates at a higher level of sophistication than the last. Fitting ignores the way that science normally functions, in favor of a revolutionary model. What appears to be a revolution within a scientific field is generally an observation of accelerating progress. The generation of knowledge is a cumulative process, no matter how revolutionary current activities appear to be. New theories and data are digested, with useful portions incorporated into regular activities. The models of archeological theory are as open to question as the models of culture that they generate.

If there is a crisis today in archeology, it is a crisis of identity. A crisis of status, not a revolution in methodology. Archeology is a branch of anthropology, with anthropology being defined as the study of man in its broadest sense. Archeology, together with the physical sciences, provides the study of what happened in prehistory, in terms of man's origins and cultural evolution. By the very nature of the data available, archeology is not sociology in the sense of being able to do detailed studies of social patterns and behavior. Archeology makes its own unique contribution to anthropology in areas otherwise not covered.

There are problems of identity and status within archeology, as archeologists are trained in departments of anthropology at most universities. Other branches of anthropology may present themselves as being somewhat superior, so that some archeologists feel that their discipline does not have equal acceptance in the overall field. This may be true within the politics of professional advancement at some universities, although I have no direct knowledge of this.

It seems to me that the quest for status in archeology is being confused with concurrent progress in methodology. As more information and scientific techniques become available, archeology has been able to shift from classification activities to more emphasis on interpretation. Some archeologists state that their field is now anthropological because it is interpretive instead of artifactual. Interpretive methods are not unique to anthropology. All maturing sciences shift from strictly classification activities to interpretation. This is happening now in archeology, because of the late start compared to other sciences, and has been noted for some time in the literature.

The idea that interpretation methods are somehow unique to anthropology has led some archeologists to borrow models from other branches of anthropology, or to propose models that would contribute to other branches of anthropology. Since these models can not always be applied directly to the real world of physical data from archeology, results have been limited. There seems to be an endless debate in the journals over the methodology of logic.

I do not question that ideas from other branches of anthropology can be used in archeology. Analogies from ethnology have been in use for some time, for example. What is questionable is that other branches of anthropology can provide archeology with interpretive models that have direct application. There is no shortcut to archeologists building their own models by plain hard work.

Archeological reports have been somewhat dull in the past, because in many cases they have been simply tabulations of artifacts, with no attempt at interpretation. A good deal of this has not been lack of interpretive methods, but poor individual efforts in technical writing, and a lack of interest in general questions that go beyond the limits of detail of a specific site.

Gordon Eckholm has discussed this attitude as "archeocentrism" (1964, p.492).

For anyone interested in activities and viewpoints of the "new archeology", recent book Contemporary Archaeology (Jeone 1972) is a survey of the current situation. A critical review of this book has also been made (Cotter 1973). Techniques of modern archeology are not so much criticised, but the overuse of theory is questioned. According to Cotter, the "new archeology" distains to show what the history of a site was, in favor of discovering the process by which the continuum of site history advanced. Cotter then observes that science may have to wait a generation or so before it becomes fashionable to utilize an understanding of process in order to determine more accurately what happened in prehistory. My opinion is that the current great emphasis in the direction of only considering localized processes is similar to describing an automobile with a series of microscope slides.

One can see the attitude of "processual" archeology in a recent book review (Gould 1973) on Australian aborigines. Comments on one paper are: "It is heavily autobiographical and in its emphasis on history and technology is strangely out of step with most other papers in the volume, which are largely concerned with culture-process, economy, and ecology." One has only to read some of the great past studies by Grahame Clark on the European Mesolithic to see that history and technology are compatible with ecology and economics. In respect to new techniques supplementing older problems, I feel that Flannery (1972, ch.11) makes a good case for the need of both the historic approach, which takes the macroscopic view; and the processual approach which takes the microscopic view.

A potentially disturbing statement made by one of the younger archeologists recently (Brose 1973, p.110), is that a tribute is given to major figures in archeology in their adaptability to the "processual" school. While adaptability is a trait to be admired in any field, one wonders if this merely means a conversion to the latest dogma in this case.

In summary, regardless of my critical comments on certain results of the "new archeology", this is not an all out attack on this subject. While the term itself is deceiving, it is an indication of the foment that takes place on the leading edge of any progressive scientific activity. If not taken in the absolute sense, it is a healthy sign of renewed vigor and accelerated growth. If taken as absolute, it is a closed symbolic system, without connection to the real world, no matter how elegant in itself. Adequate definitions of the fundamental goals of archeology have been available for some time, in a number of textbooks. Newer studies are not likely to change the basic nature of the study of man's past, but only the extent of recoverable information.

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Editor's Comments:

Lee Patterson's article should provoke thought and discussion among those who read the Newsletter. Your Editor would like to get the ball rolling by taking exception to Lee's statement that "Archeological reports have been somewhat dull in the past, because in some cases they have been simply tabulations of artifacts, with no attempt at interpretation".

First, archeological reports should present facts as accurately as possible even if the statistics or statements are not entertaining. These facts can be utilized by competent archeologists, even if "interpretation" is lacking.

Second, attempts at interpretation, without sufficient information, can be misleading and confusing - particularly to the amateur archeologist. Interpretation should be left for the professional or highly competent amateur with a thorough knowledge of the area covered.

Third, reporting by amateurs may be discouraged if the amateur with limited experience feels obligated to interpret his data. Getting information on paper and in the proper hands is the more important aspect of reporting.

Perhaps you may have some comments on this excellent article. Send them in and we'll be happy to publish them.

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