J. A. Cavallo

Proceedings of the 6th annual MIDDLE ATLANTIC ARCHAEOLOGY CONFERENCE

Edited by
W. FRED KINSEY, III

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John Cavallo

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TABLE OF CONTENTS

Preface i
A Proposed Sequence for the Development of Pottery in the Middle Atlantic and Northeast
by Cara Lewis Wise. 1
Comments on Cara Wise's Ceramic Sequence
by Louis Brennan. 6
Questions and Propositions for Ceramic Studies
by W. Fred Kinsey, III. 9
Early Pottery in Eastern North America: A Viewpoint
by William M. Gardner 13
General Discussion of the Papers on Ceramics. 27
Ecological Studies of Prehistory
by Daniel R. Griffith 30
Discussion. 39
Comments on Daniel R. Griffith's Ecological Studies of Prehistory
by Louis Brennan. 40
Environment and Prehistory in the Middle Atlantic Province
by Victor A. Carbone. 42
Discussion. 50
Late Woodland Floral and Faunal Exploitative Patterns in the Upper Delaware Valley
by Roger W. Moeller 51
Discussion. 57
Susquehannock Plant Utilization
by Carl Ameringer 58
Discussion. 63
Paleoecological Models: Efficient Exploitation and Explanation of the Archeologic Record
by Russel G. Handsman 64
Discussion. 77
Bibliography. 78

FIGURES

Fig. 1: Eastern Ceramic Development. 25
Fig. 2: Some Trends in Vessel Shape. 26
Fig. 3: Climax Vegetation Types. 36
Fig. 4: Subsistence-Settlement Model 37
Fig. 5: Model Implications 38
Fig. 6: Mean July Temperature Curve. 46

TABLES

Table 1: Amount of Seed Variety Per Level and Square 61
Table 2: Results of Five Tests 62
Table 3: Simulated Example of Differential Settlement Component Densities 76

PREFACE

The sixth annual Middle Atlantic Archaeology Conference was held at the North Museum, Franklin and Marshall College, March 21-22, 1975. In keeping with the casual traditions of this organization, the local arrangements and general program chairman has the responsibility to assemble and edit for publication the proceedings of the meeting. These Proceedings are the fulfillment of that commitment.

The 1975 meeting focused upon two concerns: ceramics, their origins and dispersal throughout the Middle Atlantic culture province; and ecology, settlement, subsistence, and model building. Cara Wise organized the session on ceramics, prepared and circulated, in advance, a proposition paper. Responding in one fashion or another to the Wise position were Louis Brennan, Fred Kinsey, and William Gardner who served as chairman for this portion of the program. The session on ecology was guided and stimulated by Daniel Griffith's proposition paper, also circulated prior to the meeting. Those presenting papers were Victor Carbone, Roger Moeller, Carl Ameringer, and Russel Handsman. Comments by Louis Brennan on the Griffith paper were prepared but were not read at the meeting. They are included herein because of their relevance to the discussion. Remarks and discussion of the papers were led by Charles Holzinger and John McDermott, faculty members at Franklin and Marshall College. Fred Kinsey was chairman of this session.

On the whole the program was stimulating with well prepared and thoughtful papers. They reflect the growing, albeit slow, trends of concern for the broader problems of Middle Atlantic archaeology. Unfortunately, the idealism and enthusiasm generated by the initial talks which led to the formulation of the M.A.A.C. have been largely dissipated. We are no closer to developing strategy and grand theory for the region than in 1970. There has been no integration of research designs and, more importantly, there are no specific goals. Lacking a unity there is little hope for the development of micro and macro processual and systemic archaeological theory for the area in which we toil. Conservation of archaeological sites is extremely important but our programs do not show an interest in this area. Rapidly dwindling archaeological and historical resources and the constant encroachment upon them by the so-called needs of modern urban society should be of paramount concern to Middle Atlantic archaeologists. In this regard most of us resemble the ostrich.

If the M.A.A.C. has a purpose and a future, forthcoming programs must be directed toward rekindling that original spark of idealism. We need to identify the critical intellectual, political, and practical problems so that methodologies and strategies may be formulated in order to cope with the multiple demands of archaeology during the final quarter of the 20th century. An important factor in achieving these ends is the formalization of the M.A.A.C. The M.A.A.C. cannot afford the luxury of being a non-organization whose only cohesiveness is manifested through an annual meeting. The M.A.A.C. should serve as the

regional spokesman and clearing house for Middle Atlantic Archaeology. This ought to be done at professional meetings and councils, additionally the M.A.A.C. could seek to develop clout with private firms and state and federal agencies whose programs often overlap and clash with our concerns. The M.A.A.C. is ideally constituted to go political. It is concerned with problems beyond state boundaries yet it is not so diffuse and does not have the heterogeneous constituency of the Eastern States Archaeological Federation. This is the challenge for the M.A.A.C.--to be an intellectual and political force for archaeology and not simply an annual spring corroboree.

* * * * *

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W. Fred Kinsey, III
Editor

July, 1975
Lancaster, Pennsylvania

A PROPOSED SEQUENCE FOR THE DEVELOPMENT OF POTTERY IN THE MIDDLE ATLANTIC AND NORTHEAST

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Since the publication of W. C. McKern's "An Hypothesis for the Asiatic Origin of the Woodland Culture Pattern" in 1937, thinking about the origins of pottery in the Northeast has been dominated by a search for non-indigenous sources for coiled, conoidal, cordmarked pottery, as well as other traits of "Woodland" culture. Most notable among the proponents of an Asiatic origin for Early Woodland ceramics has been James B. Griffin (1962, 1964, 1967), who has documented the evidence for this theory in a number of articles. An alternative hypothesis, suggested by Alice B. Kehoe (1962), relates developments in the Northeastern United States to the Scandinavian Ertebølle culture. Proponents of both of these hypotheses have been faced with an inability to find pottery using cultures of adequate antiquity in the areas between the North American Arctic on the one hand or Scandinavia on the other and the early pottery using groups of the Northeast.

The impetus for locating a non-indigenous source for the pottery of the Northeast is in part a result of the technological contrast between the coiled, conoidal, cordmarked pottery which is earliest in the Northeast and the contemporary modelled, flat-bottomed, smoothed pottery of the Middle Atlantic area. In addition, the early identification of Vinette I as the earliest ceramic in the Northeast has given it a psychological priority not necessarily related to its true position in the development of pottery in the Eastern United States.

In recent years, a number of investigators have made significant contributions to the knowledge of the kinds of pottery that were being made during the Early and Middle Woodland periods in the Middle Atlantic area (Gardner and McNett 1971; Kinsey 1972, 1973; Kraft 1971; Lewis 1972; Smith 1971; Wise 1974, n.d.). This new information, when combined with the bits and pieces of information available from a variety of site reports published over the years, indicates that it may not be necessary to look outside the Eastern United States for the origin of coiled, conoidal, cordmarked pottery. In view of this, I will propose the following sequence for the development of pottery in the Middle Atlantic and Northeast. Summaries of relevant ceramic types are given for each stage.

FIRST-- the concept of making fired clay vessels was grafted onto the existing stone bowl technology of the Late Archaic groups of the Potomac region, as the result of diffusion from the Southeast. The earliest pottery in the Middle Atlantic was the soapstone tempered Marcey Creek Plain with shapes which copied the stone bowls:

> Marcey Creek Plain--a soapstone-tempered, flat-bottomed, smoothed surfaced pottery probably made by hand-modelling from a ball of clay; opposing pairs of handle-like lugs frequent (Evans 1955:55).

This development had taken place before 1000 B.C.

SECOND--as the concept of making pottery spread northward, there was a period of experimentation and change. Although some investigators are of the opinion that evidence of this phase is not likely to be identifiable, some early types which reflect these changes have been identified. These are primarily short-lived, localized ceramic types, a number of which have probably been submerged in the overall classification Ware Plain, referring to flat-bottomed pots with any kind of temper other than soapstone. Other evidence might be found if examples of Marcey Creek Plain were carefully examined for evidence of technological differences. Among the early types which contribute to this picture of localized developments are:

Dame's Quarter Black Stone Tempered--a flat-bottomed vessel with a coiled base and a straight sided, smoothed body; there are a minority of modelled conoidal bases; cord and fabric impressed surfaces are also known (Lewis 1972; Wise n.d.).

Mica-Schist Exterior Cordmarked--appears to be similar to Marcey Creek Plain, although no bases are known; interior/exterior cordmarking also known; some evidence of coiling (Ira F. Smith, III: personal communication).

Light Plain--gneiss-tempered, flat-bottomed, and predominately plain surfaced, but exterior corded and interior/exterior corded variants known (Ira F. Smith, III: personal communication).

THIRD--within a very short time, possibly not more than one or two hundred years, pottery making had spread into the Northeast. By this time a pattern of coiled, conoidal, cordmarked vessels had been established. While these changes were taking place, the steatite-tempered, flat-bottomed pottery was still being made in some areas. There is a suggestion of some use of coiling in Marcey Creek Plain, although it cannot be determined whether or not this trait has chronological significance for this type. Examples of coiled, conoidal, cordmarked pottery types include:

Vinette I--a thick walled pottery tempered with crushed quartz or crystalline rock made by coiling with a conoidal base and both interior and exterior cordmarking (Ritchie and MacNeish 1949:100).

Exterior Corded/Interior Smoothed (Early Series)--thinner and with finer temper than Vinette I; cordmarking similar to Vinette I, but does not occur on the interiors of vessels (Kinsey 1972:453).

FOURTH--by about 750 B.C., the changes which took place as the concept of pottery moved north were being reflected in developments to the south. Pottery types which characterize this stage of development include:

Accokeek Cordmarked--a heavily sand and grit-tempered ware, generally reddish in color, marked with coarse, loosely twisted and widely spaced cords (Handsman and McNett 1974:4).

Bare Island Cordmarked--interior/exterior cordmarked, steatite-tempered; cording is smaller, fainter than on Vinette I; some incising occurs (Ira F. Smith, III: personal communication).

Selden Island Cordmarked--a steatite-tempered, cordmarked, coiled, conoidal pottery (Wise n.d.).

FIFTH--by 500 B. C., net impressed pottery was being made in the Middle Atlantic area and perhaps the Northeast. Some types of net impressed ceramics were accompanied by similar cordmarked groups. Examples of net impressed types include:

Popes Creek Net Marked--a sand and grit-tempered pottery, with distinctive interior scoring (Handsman and McNett 1974:5).

Coulbourn Net Impressed-- a thick coil constructed ware with conoidal bases and no identifiable temper; upper portions of the interior surfaces may be net impressed, while the lower portions may be scored (Wise 1974, n.d.); very much like Popes Creek Net Marked (Handsman and McNett 1974:17).

Brodhead Net Marked--medium to fine crushed quartz temper, fairly thick with conoidal bases, constricted necks, and slightly everted rims (Kinsey 1972:455-456).

Susquehanna Net Impressed--a coiled pottery with some variations in temper (Ira F. Smith, III: personal communication).

SIXTH--by 200 A.D., there is a differentiation into a shell-tempered tradition and a grit (crushed stone) tempered tradition. This trend continued into historic times, although in some areas fabric impressing had replaced both net marking and cord marking by about 1200 A.D.

This sequence, of course, in no way approaches any of the basic problems involved in the introduction and development of ceramic technology. I do not refer to conditions necessary for pottery to be invented--that is not a problem in this area--but rather to factors which encourage the acceptance of the technology when it is made available. Some factors such as the availability of the necessary raw materials cannot be dealt with adequately, but it is possible to make suggestions based in part on an examination of the earliest pottery making cultures in the Eastern United States and elsewhere in the New World.

The earliest known pottery in the Eastern United States was made by the people of the Stallings Island Culture of the Savannah River Region. There is considerable evidence that coastal sites in Florida of this period and earlier are submerged. Although it is possible that these submerged sites might yield earlier pottery, there is no reason to believe

that significant differences existed. As defined by Stoltman (1972:52), the Stallings Island Culture refers to all shellfish adapted groups living in the Savannah River Region regardless of the kind of pottery made or whether they made pottery at all. The Savannah River Region, as used by Stoltman (1972:37-40), includes the Savannah River Valley proper, adjacent segments of the piedmont and coastal plain, and the Atlantic littoral from the Altamaha River to the Santee River. The area was not much occupied until about 3000 B.C., when the upland-adapted Old Quartz people began to exploit intensively the Savannah River shellfish under influence from the Tennessee Valley. By 2500 B.C. fiber-tempered pottery was being made (Stoltman 1966). The important thing to note here is that the shellfish adaptation occurred before the introduction of pottery, not after.

The distribution of sites attributable to the Stallings Island Culture indicates a heavy concentration along the Savannah River and the Atlantic littoral. Very little material is found in the uplands away from the river. Stoltman follows the suggestion of Beardsley et.al (1955:139) in assigning the Stallings Island Culture settlement pattern to their "Central-based Wandering" type. This implies intensively occupied sites serving as "base camps" around which the annual gathering cycle revolved. Smaller groups would depart from time to time to exploit specific resources such as nuts, deer, stone quarries, etc., later returning to the primary settlement. Although the data are not presented in as usable a form as Stoltman's Stallings Island material, it seems clear that a similar settlement pattern existed at Valdivia (Meggers, Evans, and Estrada 1965) and at Puerto Hormiga (Reichel-Dolmatoff 1972).

It is my contention that this "Central-based Wandering" settlement system is the minimum level of social organization necessary for the successful and permanent adoption of ceramic technology. However, this is not a sufficient condition for the invention of ceramics, nor does the acceptance of ceramic technology necessarily follow the development of a "Central-based Wandering" settlement system. The example of the Northwest Coast cultures clearly illustrates this point.

If the "Central-based Wandering" type settlement system is necessary for the adoption of ceramic technology, there should be a shift in settlement pattern sometime before the introduction of ceramics into the Middle Atlantic. A survey of Kent County, Maryland currently being conducted by Steven Wilke and Gail Thompson indicates shell middens dating from 3600 B.C. The first evidence of concentrations of population over periods of time in Delaware and elsewhere on the Delmarva Peninsula occur during the very Late Archaic or Transitional Period. Similar shifts can be seen in the settlement patterns elsewhere in the Middle Atlantic and Northeast at about this time. Witthoft (1953) postulates a shift to a riverine orientation during this period. Concentrations of population also occur in New York State. Although on the coastal plain these concentrations of people were probably based on shellfish adaptations, riverine peoples could depend on runs of anadromous fish with perhaps a greater degree of seasonal fragmentation than would be necessary on the coast.

There are few early dates for pottery in the Middle Atlantic area, with only indirect dates before 1000 B.C. However, Orrin C. Shane, III, Kent State University, has a series of dates from Rais-Schwartz Rockshelter, Jackson County, Ohio, suggesting that ceramic technology was at least available as far north as southern Ohio as early as 1560 B.C. Pottery was also found associated with living floors dated at 1020 B.C. and 855 B.C. (Orrin C. Shane, III: personal communication; Swartz 1971:160). On the other hand, most dates for early pottery in the Ohio Valley cluster around 500 to 600 B.C. I am not willing at this point to limit the development of pottery in the Middle Atlantic and Northeast to the period after 1000 B.C. although presently available data indicates that the use of pottery was not widespread until about that time in the Middle Atlantic and not until about 600 B.C. in the Ohio Valley.

In view of the above discussion, I will suggest the following outline for the introduction and development of pottery in the Middle Atlantic and Northeast:

FIRST--the development of a "Central-based Wandering" settlement system on the coastal plain and in riverine littoral environments by 3600 B.C.

SECOND--the development of a stone bowl technology under indirect influence from the ceramic technology of the Southeast.

THIRD--tentative steps toward the development of a ceramic technology by 1500 B.C.

FOURTH--widespread use of ceramics by 1000 B.C. and the development of local styles through experimentation.

FIFTH--overall adoption of coiled conoidal vessels with paddled surfaces by about 750 B.C.

Very little concentrated work has been done on the period during which ceramics were introduced into the Middle Atlantic area. Even the information which has been obtained is poorly reported. The sequences suggested in this paper are necessarily tentative, but I feel that much of this will be substantiated by future work.

COMMENTS ON CARA WISE'S CERAMIC SEQUENCE

Louis Brennan
Editor, AENA

Cara Wise's "Proposed Sequence" states a very useful and tenable proposition, useful in that it is bound to initiate lively discussion and tenable in that she has assembled an argument for an indigenous origin of Woodland pottery after decades of vain search for other origins. However, I shall take the opposing view that it is not of indigenous origin even though we do not have empirical evidence to the contrary.

The facts are on Wise's side in contending that Marcey Creek and allied ceramics are earlier by some centuries than Vinette 1 and other primary Woodland types such as Fayette Thick and Half Moon Cord Marked. But in order to be convincing that Marcey Creek wares begat Woodland pottery you have to postulate that the Marcey Creek potters invented coiling, conoidal bases, malleating, with textiles, or cordage and tempering with grit, or you have to show where the influences came from that wrought these radical changes. If you want to insist that Marcey Creek potters more or less volitionally shrugged off their traditional technology as inadequate and thought up the Woodland technology you will have to produce more evidence of it than you have; it is inherently unlikely on the principle of conservatism of cultures. If you have to look elsewhere for the influences that caused Marcey Creek potters to adopt Woodland ceramics technology, where better to look than to the Woodland pottery region?

The cited evidence is compatible with the adoption by Marcey Creek potters of the much sounder Woodland technology. Marcey Creek pottery is obviously in the Stallings Island tradition of modeled, fiber-tempered, flat bottomed, smooth-surfaced wares. Point by point it is totally different from Woodland pottery and I do not see any plausible evolution out of it toward Woodland technology.

Contrasts

Woodland	Fiber-Tempered
Coiling	Modeled
Surface texture	Smooth
Conoidal shape	Flat-bottomed
Grit-tempered	Fiber-Tempered

Woodland ceramic technology is old and widespread in the Old World, particularly along the fringe regions, the northern Pacific coast and the Atlantic coast. I have never seen a Beaker Folk "bag-shaped" pot, but the descriptions of it and the illustrations, as well as statements to the same effect, make it out to be of Woodland technology and appearance. We are all familiar with Paul Tolstoy's

studies of the pottery of northeast Asia and its similarity to Woodland, and there are the studies of W. C. McKern and J. B. Griffin as well.

The Asiatic origin of Woodland ceramics can be discounted, especially since R. S. MacNeish pretty well disposed of that notion in his An Introduction to the Archaeology of Southeastern Manitoba where he pointed out that in this critical area for the introduction of pottery from Asia there was no sign of it. Instead, prehistoric ceramics in Manitoba are of southern origin, that is, from the Woodland regions of the Northcentral United States. The point to be emphasized here is that if Woodland ceramics had been introduced from Asia there would have to be convincing archaeological evidence, but there is no such evidence. Short of flying in, no Asiatic immigrant bearers of Woodland pottery could have gotten to the Lake Forest Region and upper New York State without leaving a trail of broken pots along the way. On the other hand if Woodland ceramics came from across the Atlantic there would be no such trail. The absence of a trail is not significant, and we would expect Woodland pottery to be earlier along the coast than inland, and thus earlier in the east than in the midwest. This is exactly what we have.

I am not familiar with Alice Kehoe's hypothesis of the Ertebølle origin of Woodland wares, but I am familiar with her "Small Boats Upon the Atlantic" about late Bronze Age crossings of the Atlantic. I have talked with her about it and how her paper came to be written--it is perfectly acceptable theory on the other side of the ocean. Both Ertebølle and the Beaker Folk appear to be about a millenium too early for the appearance of Woodland pottery in America. This is not to say that the pottery itself might not have continued to be made by others who went to sea in boats capable of surviving a crossing. Certainly if a Valdivia (Ecuador) landfall by Japanese Jomon pottery makers is plausible, and Atlantic crossing by sea-faring Europeans is even more so. The voyage is more direct and shorter, and the likelihood of survival much greater. James Ford's proposal for the origin of the Stallings Island-Orange tradition involves a shorter water route across the Gulf of Mexico.

The Delmarva peninsula is obviously at an interface between the southern smoothed and the northern Woodland cord-marked technological traditions, with the Woodland tradition eventually replacing the less sophisticated southern tradition. If Woodland ceramics are of European-Atlantic origin and their first place of eastablishment should turn out to be, say, Long Island then the Delmarva is about where we would expect an interface to occur. (In the Delaware Valley the early Orient culture appears to lack Woodland pottery but by about 2800 years ago ceramics are associated with fishtail points.)

Wise cites pottery in Jackson County, Ohio, that appears to date about 1560 B.C. but she does not say what kind of pottery it is, and I do not know the report. I would have to guess that, if the association of the pottery with the date is good, the pottery would have to be fiber-tempered smooth, from the southern tradition, which

appears at Poverty Point a little later than that. If the Jackson County ceramics are grit-tempered cord-marked Woodland ware (always providing that date association is valid) then we do have a whole new ball game. (Editor's note: I am certain that this is Woodland pottery.) But her Marcey Creek origination of Woodland becomes even less likely, since the Jackson County pottery is distinctly older.

As a final comment, I fail to see the relevance of central-based wandering to the adoption of pottery or its use. In the first place I doubt that a true central-based wandering pattern ever developed along the Atlantic seaboard, though Wapanucket No. 6 village and Lamoka Lake seem to be exactly that. (Incidentally, the suggestion that people as near the sea as the Savannah River "culture" had to learn to eat shellfish from the people of the Tennessee Valley is a howling absurdity). These "villages" are more probably collections of winter dwellings which the inhabitants left as soon as the weather permitted and did not re-occupy until the next winter set in. The village population probably broke up every March; each band going its own way for the outdoor season until November.

I infer that the reason Cara Wise believes in a more or less necessary relation between pottery and central-based wandering is that pots are fragile and cumbersome to transport and are, therefore, not suitable for a seasonal round pattern. Archaeologically, however, pottery, the whole time-range of it, is found on seasonal camp sites in the Lower Hudson. All the Northeast styles I know about, and certainly the rounded based ones, are for cooking; I don't know of any that are called storage vessels in the sense of having been made for storage. The evidence is that, when cooking baskets were abandoned, ceramic pots were carried to riverbank sites for customary food preparation practices. If pottery replaced cooking baskets then pottery had to go where formerly cooking baskets went. The supplanting of one trait by another in this inventory of artifacts does not necessarily relate to a change in settlement pattern although one trait, as Wise seems to suggest, might accord better than another with a given pattern. But ceramics first of all accorded better with cooking efficiency and nutrition.

QUESTIONS AND PROPOSITIONS FOR CERAMIC STUDIES

W. Fred Kinsey, III
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Introduction

The papers and comments have raised the specter of the old saw of diffusion versus independent invention for the origins of New World ceramics. In terms of archaeology in the 1970's this is not considered to be a very "hot" issue. Current archaeology is more concerned with deeper analysis and systemic relationships. But since the subject has arisen let us examine very briefly several New World ceramic complexes.

We all know that a decade ago Meggers, Evans, and Estrada proposed the Jomon connection to explain the sand-tempered, coiled ceramics found on the coast of Ecuador at Valdivia during the 3rd millennium B.C. This is not experimental pottery, instead it is decorated by almost all conceivable decorative techniques: incising, excising, slipped, brushed, combed, cord-impressed, rocker stamped, etc. Meggers, Evans, and Estrada relate that marvelous fable of how a boatload of fishermen leaving the Japanese island of Kyushu in October-November, the typhoon season, are blown off course and carried by the currents and winds for 9,450 miles to a landfall in Ecuador. Once there these Asiatics introduced ceramic technology and new religious beliefs, reflected in the clay figurines, to the simple Valdivians. To accept this theory you must believe:

1. Japanese fishermen could have survived the long sea journey in a relatively frail craft.

2. Fishermen were skilled in pottery technology or that a ceramist was on board.

3. The Valdivians were anxious to accept outsiders and adapt to new ideas. In other words a boatload of a few individuals made a tremendous impact upon an ongoing cultural system without reinforcements from the home base.

4. Also you must be a strong diffusionist and be prepared to deny any attachment to the possibility of multiple hearths or origins.

There are other troublesome matters concerning the Jomon connection, especially since Jomon ceramics range from 2000 B.C. to 9 or 10,000 B.C. It almost takes the whole span of Jomon ceramics to encompass the full variety of styles and decorative techniques found at Valdivia and dating to the third millennium B.C. More recent excavations question whether the appearance of pottery is actually sudden at Valdivia because in some deeper re-excavations by others, rude pottery has been recognized. Very recent work under the direction of Donald Lathrap reveal longterm stable concentrations of populations on the coast.

In northern Colombia at Puerto Hormiga on an arid alluvial plain of savannahs and tidal estuaries, G. Reichel-Dolmatoff found a culture adapted to a littoral lifestyle. Material culture and adaptive practices resemble Valdivia but there is an abundance of lump molded pottery. Two wares are present: a course fiber-tempered ware containing mosses and grasses and a better made and more decorated sand-tempered ware. The fiber-tempered pottery is not experimental; it is technologically inferior to that reported for Valdivia. Chronological priority favors Puerto Hormiga although there is inconsistency in the radiocarbon dates.

Twentyone Valdivia dates range from 3200 to 1500 B.C. while 5 Puerto Hormiga dates range from 3100-2550 B.C. But the oldest Valdivia date is an isolate and second oldest is 2670 B.C. For Puerto Hormiga there are 4 dates prior to 2800 B.C. Reichel-Dolmatoff believes that the earliest South American pottery will come from the general region of northern South America and it should be about 6000 years old.

Donald Lathrap's thesis is that either the central interior Amazon or Orinoco River Basins are the hearth for early root crop(manioc) agriculture and ceramics dating to the sixth or seventh millennium B.C. In the quest for desirable agricultural lands resulting from population pressure, Tropical Forest Indians established settlements in the fertile highland basins on the eastern slopes of the Andes. They brought warm root crops with them and also experimented with cold resistant crops. Eventually these Indians followed the rivers and mountain passes to the Pacific Coast. According to Lathrap, Valdivia represents a breakthrough of well-established Tropical Forest culture upon settled coastal fisherfolk and rude horticulturalists. Like a Scotch verdict, the case is unproven.

R. S. MacNeish describes the rise of ceramics in the Tehuacán Valley during early Preclassic times about 2300 B.C. These early plain ware pottery vessels resemble stone bowl counterparts. This is an unslipped and undecorated ware of globular shape. Other forms have flat bottoms and there are examples with flared rims. Tehuacan ceramics appear to derive from gourd and stone bowl prototypes and they are seemingly unrelated to the South American ceramics.

For North America, as Cara Wise has noted, fiber-tempered wares are the earliest pottery and they are established in the Stallings Island Culture of the Savannah River area in Georgia and along the St. Johns River of northeastern Florida by about 2500 B.C. The pottery could have been derived from northern South America via a Caribbean connection since it is a fairly easy diffusionary route and the nautical distances are not nearly as forbidding as the Pacific journey of the Japanese. However, other authorities have noted the similarity of form and continuity between sandstone bowls and the fiber-tempered pots and they argue for an in situ development.

We are all familiar with the stone bowl to flat-bottom soapstone-tempered development of the Middle Atlantic region and Wise has described a subsequent variety of coiled, conoidal, plain and cord-marked wares deriving from this technology. From soapstone-tempered to true Woodland pottery is not the great technological leap that Louis Brennan posits. There are sufficient intermediaries around to smooth out the transition. The Kehoe-Brennan Scandinavian connection for the introduction of Woodland pottery into the Northeast is only 1/3 as implausable as the Japanese trip since the Atlantic voyage is that much shorter.

Where does this leave us concerning possible origins for New World ceramics?

Proposition 1 New World cultural developments are fundamentally indigenous achievements. Searches for ceramic origins and hearths are fun but they are not likely to be very productive. Present data suggests that the technology of fired ceramics was grafted to ancient and universal hollow ware traditions. According to local factors this hollow ware may have been based upon stone, wood, bark, basket, or gourd containers. Whether the concept of fired ceramics ultimately derives from single or multiple New World hearths cannot now be determined and may never be solved. However, in areas where ceramics are early, they are part of a sequential series of technological achievements. In at least three regions there is evidence for initial ceramic forms to imitate pre-existing hollow wear. Ceramic technological innovations do not represent sharp revolutionary breaks with past technology. This is a case for *in situ* development.

Corollaries to Proposition 1

A. There is an early hearth for pottery in northern South America.

B. A Tropical Forest hearth is an intriguing possibility.

C. Ceramic development in the Tehuacán Valley is unrelated to South American pottery.

D. Fiber-tempered pottery of the southeast may have diffused from South America but a case can be made for a local *in situ* development.

E. Early Middle Atlantic ceramic development is linked to the preceding stone bowl technology.

F. The Jomon connection is baloney.

G. The Scandinavian connection is improbable.

Proposition 2 Very early ceramics made no significant impact upon existing cultural-social institutions and practices. Early experimental and crude pottery represented no advantage over existing hollow ware technology. In fact, it is likely that pottery vessels were not even as useful as the traditional containers. There was probably an extended period of experimentation before ceramic expertise was sufficient to readily produce durable, convenient, and portable pots. Ceramics gradually became an established technological practice among horticulturalists or people with a settled littoral adaptation. Economic surplus and sedentarism are factors conducive to experimentation.

EARLY POTTERY IN EASTERN NORTH AMERICA: A VIEWPOINT

William M. Gardner
Catholic University

It is the position of this paper that traditional archeologists, defined herein as those old line cultural historians who see the data of prehistory as little more than a non-randomly occurring series of objects to be ordered into some standardized classificatory scheme, placed in time and related to other objects along a spatial dimension and who have been afflicted with this sort of non-think since the earliest days of the Three-Age System, have never fully been able to adjust to what they perceive as discontinuities in what they would like to make an otherwise orderly developmental continuum. Since discontinuities do not fit with their perception of order, these archeologists, like the scientists who antedate Hutton, Lyell, and Darwin, seek explanation in catastrophism. The Vulcanism and Neptunism of those days is replaced by diffusionism or any other mechanism which will allow them to procede along their chartered course and attribute unexpected change to outside influences.

More often than not an implicit ethnocentrism or misanthropism lies just beneath the surface of such thinking and forms a Caucasophile paradigm that allows otherwise objective scholars to reject the idea that indigenous aborigines could invent complex technological items or develop sophisticated cultural systems. Hence enter migration, the panacea of the non-think traditional archeologist. We have witnessed the bringing forth of people out of Egypt or Asia or superhumans out of the limitless heavens to account for a myriad of what appears to be otherwise unaccountable events. Temples in Mexico are attributed to Phonecians or Atlanteans or Muans; agriculture in the Amazon to the high culture areas of Southeast Asia; Olmec heads to space jockeys from the farthest corners of the galaxy, and so on.

Traditional archeologists of this stripe owe a great debt to accidentally or deliberately mobile fishermen for without whom pottery would presumably never have appeared in South America and ceramic vessels with pointed bottoms and impressed surfaces would not be around to haunt 20th century sherd counts or battleship curve our typologies. From different shores these fishermen came, blown adrift by unrelated wind patterns. From the coast of Japan to the coast of Ecuador, from Denmark or wherever to somewhere along the middle or north Atlantic shores of North America, they landed bringing with them something so wonderous the lowly savages of the Western Hemisphere grunted with glee and abandoned much of what they had been doing before to take up the newly imported ceramic arts.

Jest and sarcasm aside, it is the purpose of this paper to present a model of ceramic development and adoption in the Middle Atlantic area that attempts to explain the origins and spread of pottery by drawing on what we know of the ecology, subsistence and settlement patterns, and overall archeological knowledge. The model attempts to demonstrate that we need neither long range ocean voyages nor visits by unidentified

flying objects to account for ceramics in Eastern United States. While it is recognized that diffusion in whatever form is a mechanism by which a cultural trait can spread, it is also recognized that to attribute the appearance of a trait to diffusion is a non-explanation. This is no more than the observation of a phenomenon. Retreating into diffusion also tends to stop us from investigating alternative possibilities. We all know deep in our hearts that much of Eastern North American prehistory has been written by looking outside the area in which we are working to explain what is occurring rather than looking within.

The dangers of this type of approach to culture history are replete in the literature. We can go back into our own intellectual history and see the arguments which raged over the Mound Builders. At one time it was thought Eastern North American mounds were built by a vanished race. Today there lingers the argument that burial mounds diffused from somethere in Asia. Aside from the fact that there is nothing in the intervening area which could tie Asian and Eastern North American mounds together, where are other lines of evidence which show such an idea to be a patent absurdity. It takes little effort to demonstrate that North American burial mound development can be traced (with few gaps) back into Late Archaic burial complexes having a circum-southern Great Lakes distribution.

To claim that we only know this through hindsight is to overlook the fact that the diffusionist mentality is what led people into erroneous interpretations in the first place. Had they not been seeking an exotic rather than an indigenous origin they would have never boxed themselves into such a corner. Had they but compared the many differences rather than concentrate on the few similarities, the whole matter could have been disposed of a long time ago.

With reference to the origin of ceramics in the New World there are two broad diffusionist hypotheses which attempt to explain their presence. The first of these has been most vocally presented by Betty Meggers, Clifford Evans and Emilo Estrada based on their work at the Valdivia site on the Pacific Coast of Ecuador. At the outset of their statements concerning the introduction of ceramics into the New World by off-course fishermen from Japan, there were a few believers but considerably more skeptics. The believers molded what they considered to be a convincing argument by taking isolated look-alike elements out of two complexes and saying "now, see, we have demonstrated enough similarity to prove our case". Scholars familiar with the Jomon-Valdivia ceramics said "oh, yes, there are similarities, but there are many more differences". Archeologists familiar with the type of argument used by Meggers, Evans and Estrada said "oh, yes, there are similarities, but one can find similarities concerning many different elements between archeological complexes around the world, but it takes more than isolated elements to demonstrate a generic relationship". Workers in South America familiar with the early ceramics of the area pointed to Columbia and the adjacent interior and said actually there are good antecedents for the Valdivia ceramics in the immediate vicinity so forget Japan.

This latter criticism struck at the heart of the Meggers-Evans-Estrada position. The major argument they had used for support of the Jomon-Valdivia connection was the apparent sudden appearance, without antecedents, of a wide range of decorative techniques and a sophisticated technology at the basal levels of the Valdivia site. In other words, everything appeared at once. As it turned out this was not the case at all. Donald Lathrap, his students, and a number of other workers have descended on the Valdivia area, made new excavations, particularly strata cuts, and have shown that, if the original excavations were not actually inadequately carried out, they were misread. They have also demonstrated that there is indeed a developmental sequence that goes from the simple to the complex, and the antecedents, if they lie anywhere, lie across the Isthmus of Panama and not across the ocean.

Can we fault Meggers, Evans and Estrada for what they proposed? Being charitable, one would have to say no we cannot. They possessed a melange of ceramic traits which they thought were not explicable by reference to any other collections in the area, but explanation could be found across the Pacific on the coast of Japan. There had been ample precedence for such a long range view. Such an Old World outlook is not uncommon among certain Mesoamericanists and Peruvianists. Diffusionists have long sought similarities between these areas and Asia in an effort to explain what they perceived of as otherwise unexplainable similarities in development. Yet in no instance has the demonstration of such origins been successful, for the diffusionists have invariably hit on isolated elements that can be explained by parallelism, by chance, or through the demonstration of local antecedents. In a less charitable vein, had Meggers, Evans and Estrada not been so imbued with the diffusionist mentality, they would have questioned their results and either re-excavated or thoroughly researched all local complexes in an increasingly wider circle, until they found an explanation that was far more logical and demonstrable than a precarious ocean voyage of several thousand miles.

The second hypothesis concerning New World ceramics and Old World parentage centers around a possible source for Eastern North American Woodland ceramics. In this case, two areas have been offered. The first and the earliest considered was Asia. The most recent sees the source as the Scandinavian-Northern Europe area. The Asiatic hypothesis has more or less been rejected because of the lack of connecting ceramics within the intervening land mass of the Canadian sub-Arctic, and because it was too closely linked with the origins of burial mounds.

The Northern European hypothesis remains in vogue among some workers because of presumed similarities between Woodland ceramics and the ceramics of such complexes as the Ertebølle culture of Scandinavia and the Bell Beaker culture of Northern Europe. The similarities which are seen are in the basic vessel shape, conoidal or round-pointed straight-sided jars and the use of cord or other fabric to roughen the surface. In both areas those attributes occur. In Europe their appearance is considerably earlier, although in some cases there is some temporal

overlap (We might also note this same vessel shape and surface treatment occurs in northern Japan and has been dated as early as 4 - 3,000 B.C.). It is not as easy to reject the European hypothesis because, after all, migrants using boats would not leave traces in the intervening areas (I am reminded of Emerson Greenman's attempts in Current Anthropology some years ago to bring Paleo-Indian across the Atlantic from the Solutrean by way of ice floes).

On the other hand there is no evidence that makes the European hypothesis remotely tenable. Congruencies or similarities in form and surface treatment on ceramics considerably removed in space and time cannot be said to be generically related simply because these similarities and congruencies exist. If we accept influence from across the ocean, then we cannot attribute this influence to simple diffusion through trade or some other mechanism; we have to attribute it to migration. But whether migration or trade is used we must be able to demonstrate that other elements in the system were also so influenced or transported. Migration can only be demonstrated if whole complexes are shown to appear in an area where few or none of the traits has demonstrable local antecedents. As William Sears said (and others have agreed), Woodland is little more than the Archaic with pottery added. Are we to then assume that Ertebølle or any other complex, whether it is in Europe or Asia, is little more than the Archaic with pottery added? Show me the agriculture that is associated with the Ertebølle or Bell Beaker culture. Show me other aspects of European technology or ceremonialism. Show me linguistic remnants of some branch of Indo-European, Sino-Tibetan, Finno-Ugric, or Paleo-Asiatic. Show me parallels in kinship, ocean going crafts, or giant menhirs arranged in some astronomical alignments. Show me anything other than one or two parallel similarities in form, in a not too difficult technological invention.

The ultimate refutation of the diffusionist's position, however, rests in the ability to demonstrate local antecedents for the traits that are being evaluated as imported. In the case of ceramics such antecedents already exists in the Eastern United States. To be sure gaps in knowledge are present, but there is already enough evidence to warrant everyone focusing attention in our own shores and to quit pantomining H. S. Gladwin and Elliot Smith. This evidence lies in the fiber-tempered ceramics of the South Carolina-Georgia-Florida coasts; the steatite bowls of the Atlantic seaboard; and the steatite-tempered ceramics of the Middle Atlantic. From this alone we have enough to marshall an argument and develop a model that should forever lay to rest Asian or European origins for ceramics in our area.

The model which is offered here has been developed on the basis of my own work in the Southeast and Middle Atlantic and through teaching courses in North American archeology for the past seven years. It is a synthetic model in that it takes known facts or observations and synthesizes them into an interpretive framework. No pretense is made in such a model that all facts are stated but at the same time accuracy

must be maintained, and distortion kept at a minimum. For purposes of presentation, the model is divided into phases. The phase is considered to be the core out of which other events proceed. Following the presentation of the initial phase, the model is divided into a Southeastern and Northeastern branch. The reasons for this will become apparent. Under the discussion of each phase such items as settlement pattern, subsistence, and ecological relationships are investigated where such elaboration is deemed pertinent. A summary will be presented at the end, and the origins of fiber-tempered pottery in North America will be considered.

PHASE I

The Fibered-Tempered Ceramic Core

2500-2000 B.C. — Ceramics in the form of fiber-tempered ware appear on the Atlantic coast of South Carolina, Georgia and northern Florida. This earliest pottery is undecorated. In terms of shape, vessels are either boat shaped with round of flat bottoms. Other, although perhaps somewhat later, forms include round, sharp-shouldered bowls with restricted orifices and straight walled cylindrical jars with flat bottoms. Molding is the only technique which has been demonstrated. This complex is called the Stallings Island. A somewhat later Florida variant is called Orange or Tick Island. The only settlement pattern known during this early phase is that associated with shell middens in the estuarine and brackish water areas with a number of offshore islands or else in the mouths and upper portions of tidally affected rivers. Those middens which were the least disturbed and which have received the most extensive excavations consist of circular embankments of shells and other midden debris encircling or enclosing extensive areas which were kept clean. The lack of fiber-tempered pottery in areas away from the coast suggests that the shell middens and shell rings were the loci of multiple-resource base camps which were occupied for reasonably long periods of time. Resource exploitation away from these base camps could have been accomplished through periodic forays by portions of the coastal site's population, or through seasonal break-up and extended stays. Settlement pattern studies will demonstrate this one way or another. Whatever the case, the midden debris from these shell middens suggests a high density, relatively stable, multiple-resource based population. The low frequency of pottery elsewhere at this period would indicate that pottery was predominantly a base-camp specific item, and not the sort of item carried with them in Piedmont or Mountain exploitation.

PHASE II

The Southeast Ceramic Area

2000-1000 B.C. — Decoration in a variety of motifs, principally some form of zoned incision and punctation appears on fiber-tempered pottery. Fiber-tempered pottery, where discovered in stratigraphic

context, invariably has undecorated types preceding decorated types. The temporal precedence therefore seems established fact. Also appearing in the middens at this time are sherds of carved steatite bowls. Decorated fiber-tempered ceramics occur over a much wider area than the earlier undecorated ware. It appears as far north as northern Alabama, south into Peninsular Florida, and west across the Gulf Coast as far as the lower Mississippi Valley. Known concentrations (the St. Johns; Coastal Georgia; Appalachicola Bay; Pensacola Bay; The Mississippi Delta) are predominantly manifested in coastal sites but others (northern Alabama) are associated with riverine settlements. Regardless of whether it is fresh or salt water resources to which these ceramic using components are adapted, it is basically a littoral orientation and the ceramics are found almost exclusively in such contexts. Thus pottery continues to be something that is associated with multiple-resource based base camps which occur in zones with considerable horizontal zonation, highly circumscribed micro-environments, and a high biomass that promotes a high degree of sedentism. That forays or wide-spread seasonal migration are present is attested to by finds of fiber-tempered ware in the inland portion of the Florida Panhandle. Such sporadic occurrences do not obviate the fact that pottery at this period in Eastern North America is part of the tool kit of those groups who, by gift of environment, traditional patterns, cultural choice, or whatever, find themselves in areas where increased sedentism is feasible and not maladaptive. Elsewhere in the Southeast other groups continue in their Archaic pattern without benefit of ceramics.

PHASE III

1000-500 B.C. — Fiber-tempered ceramics are replaced by ceramics tempered with some other form of aplastic. The evidence suggests that this is not an immediate replacement but a gradual de-emphasis of one type of tempering and the ascendancy and replacement by the new method. Actually the situation seems to be more complicated than this. In the past it has been suggested that there was a straight-line uni-linear evolution going from undecorated fiber-tempered to decorated fiber-tempered to sand-tempered ceramics. Recent work has shown that between 1500-1000 B.C., the fiber-tempered pottery contains increasingly less fiber and the paste holds more sand. Some sites have shown that sandy fiber-tempered pottery and non-fiber-tempered pottery can occur concurrently. Ripley Bullen, for instance, discusses what he calls a Florida Transitional which is characterized by semi-fiber-tempered pottery, which may contain simple stamping; St. Johns ware made in the shape of fiber-tempered vessels, and early limestone-tempered types such as Perico and Pasco. Sand tempered pottery without associations with fiber-tempered pottery has been dated as early as ca 960 B.C. Thom's Creek sand-tempered ware is another non-fiber-tempered pottery that may occur around 1000 B.C. or earlier and be coeval with decorated fiber-tempered pottery. While

this problem is still up in the air, what it seems to indicate is that there is no simple one to one replacement but a gradual change and that sand-tempered ceramics, e.g., the Thom's Creek, Deptford and similar series flow directly out of the fiber-tempered wares. The same would be true for the St. John's chalky ware and the limestone-tempered series. Most certainly there is no "disconformity" in this case. Other traits which definitely appear with the Deptford series between 800-500 B.C. are coiling rather than molding as a manufacturing technique and paddle malleating. This seems to begin with a linearly carved wooden paddle (Deptford Linear or Simple Stamped) or a paddle carved with checks (Deptford Check Stamped). Along the central Florida Gulf Coast we have the limestone-tempered Pasco Check Stamped. In the St. Johns region there is the essentially temperless St. Johns Ware. In the Lower Mississippi Valley, it is called Tchefuncte ware. Very little else changed in the tool kit and essential continuity can be demonstrated, so there is no need to bring in migrants from elsewhere to explain this "unconformity". Interestingly enough pottery now becomes quite widespread throughout the Southeast and is not necessarily or exclusively associated with littoral adaptations. The multiple resource base camp, however, still remains the site where most pottery is recovered. Smaller camps found in limited resource areas, while they contain pottery, express low sherd counts. Also during this period burial mounds appear over wide areas of the Southeast suggesting there is a general trend toward increased base camp sedentism. Whether this increased sedentism reflects environmental change, increased exploitative efficiency or storage capacity, cultural factors such as more efficient redistribution or greater general control by ceremonial aspects of social systems, or the introduction of horticulture, or combinations of all of these is not known. What is known is that there is a definite correlation between many aspects of the cultural assemblage and increased sedentism. It should also be noted that pottery becomes a very important item of grave furniture throughout much of the Southeast. During subsequent periods, the ceramics used in the graves become highly decorated with some form of zoning being the principal overall motifs. This moves us into the full Hopewellian era but even long after the Hopewellian phenomena dies out elsewhere it continues unabated along the Gulf Coast and the Lower Mississippi Valley. The Gulf and the lower Mississippi continue until terminal prehistory as centers of elaborately decorated and zoned decorated ceramics. Cordmarking, while it occurs, is never particularly common.

PHASE II

The Northeast Ceramic Area

2000-1000 B.C. — Carved steatite bowls appear in a number of archeological complexes scattered from North Carolina to Pennsylvania. These bowls are square or oblong forms with rounded or round flat bases. In most cases they have lugs. Sites with steatite bowls or bowl fragments

are generally concentrated along the major river systems of the Piedmont. The Piedmont and Piedmont-Fall line transition zone is where steatite occurs naturally. Sporadic occurrence of steatite bowl fragments is reported outside of the Piedmont, in the Coastal plain of Florida and Georgia and in the Ridge and Valley provinces of Virginia. In most of the Southeastern occurrences the association is with fiber-tempered ceramics. Where such associations have been demonstrated, fiber-tempered vessels occur stratigraphically earlier than steatite vessels. In the heartland of steatite bowl-using groups, the highest frequency of bowls or sherds of this material, outside the quarries, is along the major riverine camps. While sherds of steatite bowls are not absent from such places as rock shelters in the Piedmont uplands, they are the exception rather than the rule. In the Piedmont area the multiple resource based base camps are located in the riverine littoral and, like their counterparts in the coastal regions, such sites are the focal point for intense use of steatite. In the Northeast seasonal movements of camps are more frequent during the Late Archaic than are forays from a more or less permanent base camp. The base camp is the region to which the populations seem to have returned periodically for harvesting of particularly productive resources (this may be less true than we suppose in the Late Archaic--for instance, the Susquehanna Phase is largely confined, at least along the Potomac drainage to the major water courses--it is traceable to other zones but most commonly into the mountains on the western fringe of the Piedmont where their preferred lithic material, rhyolite, was quarried).

PHASE III

1000-750 B.C. — Steatite-tempered pottery occurs with and replaces steatite bowls. At least two types of steatite-tempered pottery occur. One of these, Marcey Creek Plain, basically copies in vessel form the carved stone bowl form. Exterior surfaces are smoothed or plain with occasional mat impressions on the base. Bases are apparently always flat. A closely related type, Selden Island Cordmarked, is also steatite-tempered but as the name implies, has cordmarked exterior surfaces. The base on this variant is round or round-pointed. Molding appears to be the sole manufacturing technique used on the Marcey Creek variant, while Selden Island is put together by coiling at least above the basal portion of the vessel. Based on the reported excavations, it is difficult to determine which, if either of these, is the earliest. Evolutionary logic has dictated that the plain variant came before the cordmarked variant. Such logic is based on other attributes such as vessel shape, and percentage seriations at excavated sites suggest that this may indeed be the case. On the other hand, presence and absence seriations do not support this interpretation, since both variants may occur in the same excavated contexts. This is certainly the case at the major reported site, mouth of Monocacy. Interestingly enough, at the Monacacy site a

conoidal based cordmarked sand-tempered variant occurs in the Early Woodland levels (a variant of the type Stony Creek Cordmarked, or what is now Accokeek Cordmarked). This type does seem to be definitely later than either of the steatite-tempered varieties. It is also the earliest pottery in the Coastal Plain of the Potomac falling somewhere before 750 B.C. based on dates at the Monocacy and Loyola Retreat sites. The distribution of steatite-tempered pottery is not much different than that of carved bowls. Northward along this distribution there is a tendency in some areas for crushed rock or sand to replace the steatite as the aplastic, with little change in form (e.g. the flat bowls of the Marcey Creek variant). Further north, and at a somewhat later time, steatite tempering is not used at all and the conoidal based jar similar to the Selden Island form is the principal shape.

By 750 B.C. the standard ceramic form used throughout the Middle Atlantic and Northeast is a conoidal or sub-conoidal jar form with cord or other fabric impressed surfaces. The manufacturing technique, at least above the thick basal portion, is coiling. This form, in one variation or another, remains the basic core of the ceramic tradition throughout the remainder of prehistory.

PHASE IV

750-400 B.C. -- Typical Woodland pottery appears on the western side of the Appalachians. At first the ware is generally crude and thickly tempered as if they are only hearing of the basic idea and not the refinements. Experimentation leads to the standardized Woodland jar form. The spread of pottery outside of the immediate confines of littoral based systems in the Northeast follows much the same pattern as in the Southeast. As increased duration of stays at multiple purpose base camps becomes the rule on heretofore seasonally shifting Late Archaic populations, pottery is more readily accepted into the technological inventory. Pottery becomes a usable item with positive adaptive value. By 500 B.C., if not slightly earlier, at least one aspect of ceramics diverges markedly west of the Appalachians. While cordmarked jars remain part of the inventory, zoned decoration on some vessels becomes increasingly common. As the Midwestern complexes, particularly in the Illinois region, become more and more caught up in the Hopewellian Interaction Sphere, this zoned decoration becomes much more important. With the decline of Hopewellian, there is a return to the cordmarked jar. Such decoration is rare east of the Appalachians until late prehistoric times.

SUMMARY

The core of ceramic developments in Eastern North America is seen as the fiber-tempered pottery which appears on the southeastern coast around 2500 B.C. Originally plain with a vessel shape limited perhaps to one or two basic forms, incised and punctated exterior surfaces become the rule by 1500 B.C. Vessel forms probably increase in variety at this time. Also by 1500 B.C. pottery spreads westward and southward, appearing in the Lower Mississippi Valley and Northern Alabama. Between 1000 B.C. and 750 B.C., pottery tempered with aplastics other than fiber appears side by side with the fiber-tempered material and ultimately replaces it. These new ceramic vessels have rounded or round pointed bases, and surfaces which are malleated with carved wooden paddles. Coincident with the treatment of surfaces with paddles is the appearance of coiling as the basic technique of vessel wall building.

Northward along the Atlantic in the Piedmont area, the fiber-tempered pottery of the southeastern coastal region is seen as stimulating the use of carved stone bowls by 2000 B.C. These bowls appear to be copies of the fiber-tempered vessel forms of the Southeast. The use of stone bowls diffuses northward to Pennsylvania. Somewhere in this area, possibly along the Potomac, pottery is once again reinvented. The vessels are steatite-tempered and are copies of the stone bowls. At first the vessels are molded but coiling and the paddle and anvil technique leads directly to jars with round or round pointed bases and surfaces which are cordmarked.

The coiling technique and paddled surfaces are seen as two linked traits. Pointed or rounded vessels are also seen as another possible linked trait (in this case a mechanical linkage derived from the annular ring technique). It would thus appear that within the confines of what we tend to call a single culture area there are at least two separate inventions of pottery which are well removed in time. The first of these is the fiber-tempered pottery. The second is the steatite and later grit-tempered pottery of the Northeast. Both are in some way generically related, but the relationship is not unilinear but is subtle and remains to be fully explored. The steatite-tempered pottery vessels are copies of the carved steatite bowls which in turn are copies of fiber-tempered ceramics.*

Pottery in the East is seen as a trait that, once it appears, is not adopted by surrounding populations until they are systemically receptive. Systemic receptiveness in this case is seen as reflected in a multiple resource subsistence base which operates with a high degree of sedentism in zones where resource variety and density are optimal because of either horizontal or vertical zonation. Ecological readiness in the East is first obtained by groups exploiting one or another resource in rich

*It should be noted that Ripley Bullen adopted without elaboration much the same position in a paper presented at the Southeastern Archeological Conference in the 1950s.

littoral zones where seasonal variation is spatially limited. This happens to be in the coastal regions of the southeast. From there the idea of containers made from something other than vegetal material spreads rather slowly until about 1000 B.C. and shortly thereafter, when reinvention and development of new techniques goes hand in hand with rapid spread. This last spread is seen as the manifestation of a new systemic or, perhaps in this case, cultural readiness, for concommitant with this spread is the spread of burial ceremonialism as manifested in burial mounds throughout the bulk of the east. The burial mound, while it is many other things, is also an indication of sedentism or some degree of restriction of movement and return to a given area.

The Ultimate Origins of Ceramics in the East

As far as I can tell from the archeological evidence, fiber-tempered pottery appears in the Southeast without any antecedents. The lack of antecedents brings us back to the question of unconformities. Does this technological disjunction indicate an independent invention, or does it represent the introduction of something invented elsewhere? In the case of the Southeast, and at the risk of contradicting myself, I find myself leaning very heavily in the direction of at least a part of the thesis put forth by James A. Ford. In other words, I see the appearance of fiber-tempered pottery at around 2500 B.C. as being the result of a migration (or some other intense contact mechanisms) of peoples from the Gulf coast of South America to the coast of the Southeastern United States.

The antecedents are in something like the Puerto Hormiga complex, where fiber-tempered pottery almost identical to the Southeastern variety has been dated at slightly before 3000 B.C. The Puerto Hormiga complex also has other traits which are similar to what we see in the Southeast, thereby partially satisfying the necessity for demonstrating something other than a single trait or set of linked traits to indicate migration. For instrance, the Puerto Hormiga complexes are also littoral adapted, subsisting on a variety of natural resources of which shellfish play an important part. The community patterns are not totally dissimilar from that of the Stallings Island complex: related habitation units arranged in a circular fashion with ultimate growth leading to a circular midden embankment with a relatively clean inner area.

There are, of course, enough differences between the Puerto Hormiga complex and the Stallings Island complex to take this out of the category of migration and reduce it to some other intensive social intercourse activity. The Stallings Island complex, as far as I can determine, belongs to the rest of the Southeastern Savannah River Archaic. I know nothing about the projectile points, for instance, of the Puerto Hormiga or related groups. Puerto Hormiga has decorated sand-tempered ceramics which are apparently coeval with the fiber-tempered ceramics. Still there is enough similarity in certain traits to indicate that there is a good case for the introduction of ceramics from somewhere in South America

at about 2500 B.C. At this point, however, I depart from Ford and pursue the explanation which I have already presented.

Looking at South America I am forced to wonder if the ecological readiness concept which I have developed for Eastern North America is applicable there. This I cannot answer, except to point out that the residential stability, the multiple resource subsistence base, and a number of other factors are present. Do we then have to postulate an independent invention or look elsewhere for influences? In the case of Puerto Hormiga, I think we can stop there and say that it was an independent invention and postulate that ecological readiness, if not littoral adaptation, is a necessary prerequisite for ceramic invention.

To support this, one can journey to Mexico where still another independent ceramic invention seems to have taken place. This is in the Ocos phase of the southern Mexican and Guatemalan coastal adaptations. To the concept of ecological readiness we can add another residential stability factor: horticulture. This all takes place according to Muriel Weaver around 1500 B.C., and, interestingly enough, pottery seems to develop out of stone bowl precursors.

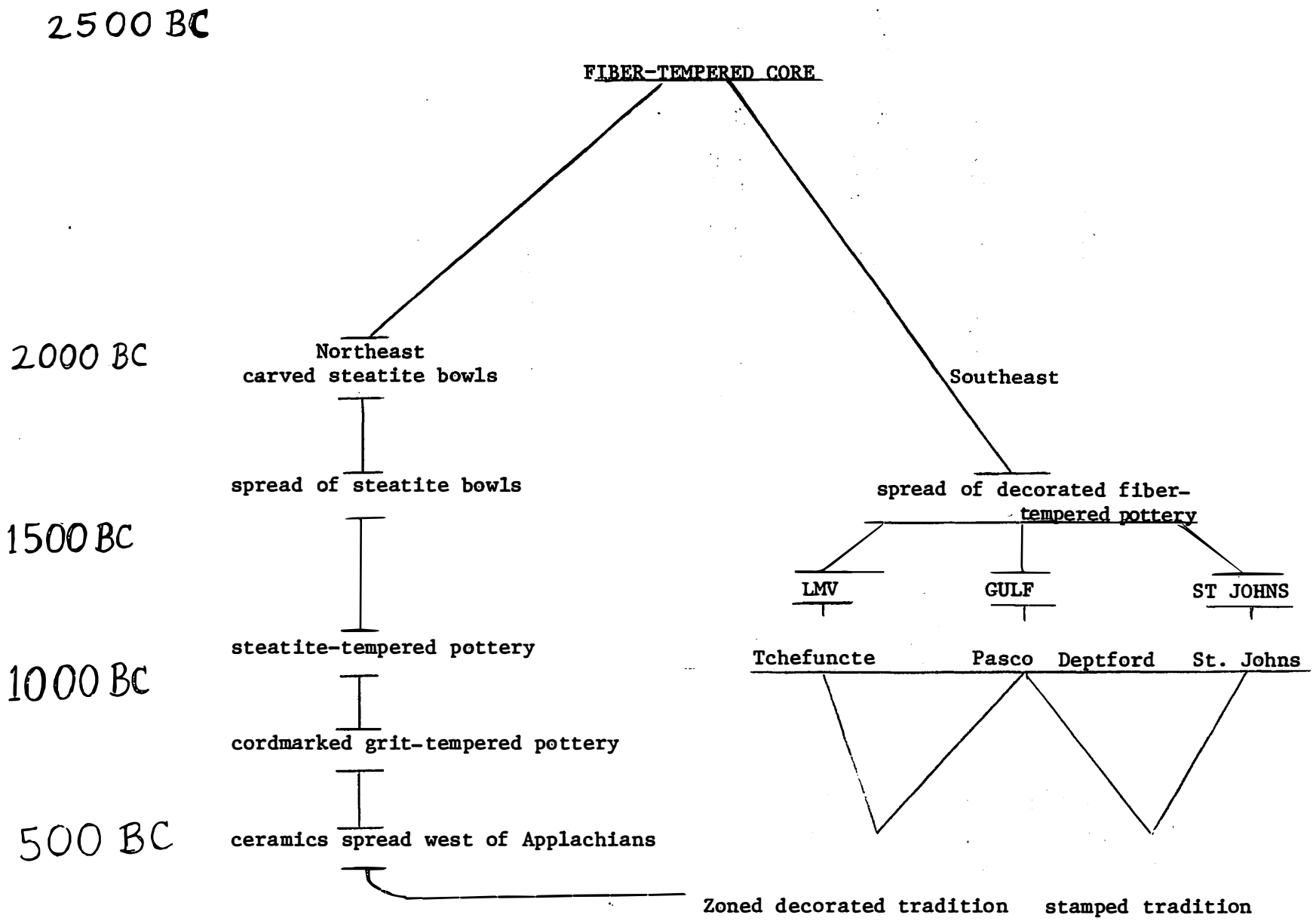


Fig. 1: Eastern Ceramic Development

SOME TRENDS IN VESSEL SHAPE

SOUTHEAST

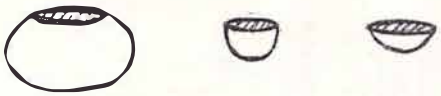
Plain fiber-tempered pottery

Stallings Island

Decorated fiber-tempered pottery

Orange and Tick Island

Decorated fiber-tempered and non-fiber tempered stamped pottery



Orange, Tick Island, Thoms Creek, Deptford, St. Johns

Non-fiber-tempered pottery

Deptford, St. Johns, Pasco, Perico, Tchefuncte

POTOMAC

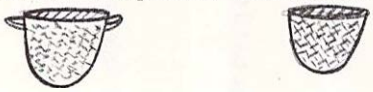
Steatite bowls

Susquehanna

Steatite-tempered plain

Marcey Creek

Steatite-tempered cordmarked



Selden Island

Sand-tempered cordmarked

Accokeek

Fig. 2: Some Trends in Vessel Shape

General Discussion of the Papers on Ceramics

M. Thurman: After pointing out the validity of Brennan's early criticism of Willey and Phillips, Thurman stated that Brennan's theory should not be rejected out of hand, even though it lacks supporting evidence. He then noted that it was important to make a distinction between cooking and storage vessels because the introduction of pottery might represent a change in subsistence patterns and food production which might indicate a cultural change between Archaic and Woodland. As an alternative to Gardner's theory of the origin of pottery in base camps, Thurman suggested that pottery could have developed in high density areas of seasonal subistence for storage purposes. Thurman also noted the need for further investigation of the relationship between pottery and stone bowls. As a final remark Thurman, commenting on Kinsey's third Valdivia supposition, warned against over-rejection of outside influence on cultures. He stated that there exists overlaps and interrelationships between cultures that contradict the theories of how culture works and which, if operating in the past, could obscure theories such as those proposed by Gardner and Wise.

W. Gardner: Initially agreeing with Thurman, Gardner pointed out that both periods of mixing and stability could be discerned from the archaeological record.

R. Regensberg: Commenting on the work at the Savich Farm site, Regensberg noted possible wooden, round-bottomed containers evident from stains in redeposited Archaic cremations. These bowls could represent precussors to soapstone bowls. In addition, the appearance of steatite spearthrower weights indicated knowledge of steatite. Regensberg also noted a lack of boiling stones.

L. Brennan: Brennan noted that a model is mere conjecture and not reality, therefore, one must avoid total commitment to a model.

M. Thurman: Commenting on Gardner's and Wise's models, Thurman noted that models are inadequate in and of themselves and since the models put forward by Gardner and Wise generate testable hypotheses, they should be tested.

W. Gardner: In reply, Gardner noted that some evidence supporting his model is available from the Shenandoah and Illinois Valleys. He also replied that there were not as many sherds at other sites as there were in the base camps.

R. Handsman: Handsman commented upon the inefficiency of Middle Atlantic archaeology due to a lack of evolving research patterns and the fact that Middle Atlantic archaeologists are doing the same old things, namely historical, chronological, and empirical generalizations. Included in this trend are untested models, which, Handsman stated, are merely elegant empirical generalizations and play no role in a delineation of the principles of reducing temporal variation to cultural variation. Questioning Gardner, Handsman asked for an explanation of Gardner's correlation, and stated that noting the existence of a correlation implies questioning its nature. Continuing, Handsman noted the importance of local sequences and damned the concept of Pan-Eastern archaeology. In conclusion, Handsman suggested the following changes in Mid-Atlantic archaeology:

1. Quests for adequate samples.
2. Re-evaluations of current hypotheses in light of new information.
3. Changes in the view of culture incorporating Gardner's concept of ecological readiness.

Also, Vinette I pottery should be considered in terms of what it means culturally on a local scale rather than exclusively considering what it means technologically.

W. Gardner: Speaking to Handsman, Gardner noted that data must be gathered in a holistic and controlled manner so that it can be used for a variety of inferences.

R. Handsman: Handsman replied that data is not gathered in such a manner in the Middle Atlantic.

W. Gardner: Agreeing with Handsman, Gardner added that any mental template affects excavation and research techniques.

M. Thurman: Thurman stated that the search for individual innovation is irrelavent to the understanding of cultural process. Commenting on typology, Thurman noted that in trying to block out large cultural and temporal units, types should be viewed as hypotheses and not culturally identifiable entities. Instead, more importance should be put on local sequences. Returning to typology, Thurman noted that morphological identity alone is not sufficient. The important unit of analysis is morphology combined with spatial correlates which will lead to an understanding of symbolic content.

D. Griffith: Griffith asked Handsman to give a new definition of culture to replace the one he termed insufficient.

R. Handsman: After characterizing the old definition of culture as being concerned with mental constructs and their relation to objectified behavior, attributes and the relationship of their clustering to types, and seriation's importance to chronology building, Handsman proposed a new definition of culture less dependent on seriation and more concerned about culture process.

H. McCord: Returning to the topic of ceramics, McCord noted, for Virginia, a possible transition from smooth, modeled vessels to Woodland types. (At this point Brennan noted the direction of the sequence was not definite.) Continuing, McCord pointed out that individual variants and idiosyncratic behavior can be recognized in the archaeological record although group behavior is more important. McCord noted that mixed populations can explain contemporary variation of artifacts.

L. Brennan: Citing culture heroes, Brennan pointed out the importance of the individual as an innovator.

F. Kinsey: Kinsey pointed out that for every successful innovator there is a large number of unsuccessful innovators thus showing the complex nature of culture.

C. Wise: Commenting that if the goal is explanation, as noted by Handsman and Thurman, Wise viewed her paper as successful because it generated discussion. Continuing, Wise cited James Griffin's definition of Woodland pottery as coiled, Griffin's definition of Woodland pottery as coiled, conoidal, and cord-marked, and stated that archaeology has been plagued by the early identification of Vinette I by William Ritchie. In addition, the supposition that because there are large amounts of Vinette I pottery in a given area it must be a core area and could not have developed elsewhere is invlaid. No technological or temporal priority should be given to Vinette I. In conclusion, Wise noted that archaeologists should stop thinking about a Woodland pottery entity and look at local sequences.

W. Gardner: Summing up, Gardner noted the diversity of viewpoints for the various papers.

ECOLOGICAL STUDIES OF PREHISTORY*

Daniel R. Griffith
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PROPOSAL 1

Ecological data should be gathered and retained with information as to its formal context in the course of any excavation. Collection techniques for the recovery of both faunal and floral data should be utilized. This is a necessary step in any excavation, even if there is no intent to analyze this data in depth.

This proposal is addressed to those who are not interested in studies of prehistoric adaptation. We intend to demonstrate that this information is of considerable use in solving several types of problems that have perplexed local researchers.

PROPOSAL 2

The understanding of prehistoric adaptation (land use) in the Mid-Atlantic will never be entirely comprehensible without a detailed knowledge of the regional resource pattern. Such a study will involve detailed paleoecological and paleogeographical studies through various time periods. Many of the seemingly "mysterious" and rapid cultural changes are explainable in terms of resource pattern shifts, shifts in the socio-cultural environment or, more likely a combination of both. Neither aspect can be studied to the total exclusion of the other and both are necessary to the explanation of prehistoric cultural processes. The following paper is addressed to the second of these proposals.

The Ecological View of Culture

Ecology is the science of the interrelationship between living organisms and their environment (Odum 1971). For the purposes of an ecological approach to archaeological data, the science of ecology is best viewed as a study of living communities interacting with their environment. This is known as synecology and is presented as the analytic framework for the studies in prehistoric human adaptation (Lewis 1974). Within this framework, one may view culture against an environmental background and emphasize the systemic nature of the relationship. Culture acts as man's adaptive mechanism and influences the ways in which energy is extracted from the biophysical environment. It is recognized that each cultural group may have different mechanisms for adapting to any given environmental situation and that the mechanisms employed may prove to be the best of several possible adaptive choices for their technological level and social system.

*Excerpted from a paper prepared for the Delaware Academy of Sciences Symposium, November 1974.

The term environment is used here in its broadest sense to include the socio-cultural as well as the biophysical. Factors in the socio-cultural environment may effect adaptive patterning to a greater extent than the pressures of pure efficiency of adaptation to the biophysical environment. In fact, this may be the rule rather than the exception. The actual adaptation of any group is guided by both sets of factors. It is the contention of this paper, however, that the effects of socio-cultural factors may be more easily isolated and evaluated when viewed against a background of biophysical adaptation. It is through a study of adaptive changes in known environmental situations that a clearer understanding of culture's role as an adaptive mechanism may be achieved.

The implications of this approach for archaeological study is to shift research from the traditional "thing" oriented study to a more systemic view of prehistoric objects (Watson et. al. 1971). The systemic approach views these objects as operating in and reflecting different levels within past cultural systems. For ecological studies, those objects which functioned directly or indirectly as a means of obtaining or processing resources of the biophysical environment reflect the adaptive mechanisms of the society involved. In other words, artifacts are viewed in a strictly functional sense and an analysis of their patterned distribution, both inter-and intrasite, provides a description of the adaptive processes.

The underlying assumption of such studies is that the biophysical environment itself is structured and that any culture adapts to this natural structure in a highly complex set of patterned relationships (Struever 1968b). This assumption permits the investigator the freedom to generate hypotheses concerning adaptation to the biophysical environment based solely on a knowledge of the structure of that environment. On a larger scale, models of adaptation may be presented based on this structure and an assumption concerning a culture's adaptation to it. It is to the logical steps followed in the generation of such models that this paper is addressed.

Models of adaptation are phrased in terms of the subsistence base and the settlement patterning of the society under study. Though subsistence and settlement are defined separately, they are intertwined in any such study. A subsistence pattern is a record of the seasonal uses of native and/or domestic food resources of a single society. This includes a study of their relative or absolute dietary importance during different seasons of the year. Settlement patterning is the way in which settlements are distributed over the landscape. Settlement is defined in its broadest sense, as any location exhibiting evidence of human activity regardless of intensity. For societies that relied almost exclusively on native food resources, as was the case during the majority of prehistory in Delaware, the settlement pattern is influenced to a large extent by the subsistence pattern. The articulation of the two patterns into a unified model of interaction between the biophysical environment, subsistence practices and settlement locations, produces an adaptational type that represents a given society's answer to physical survival.

Two classes of data are necessary before subsistence-settlement models may be proposed. The first is a study of the structuring of the biophysical environment. In the archaeological literature this is known as resource patterning (Shay 1971). Second, an assumption must be presented that allows the researcher to predict how the biophysical environment could be utilized. The following sections present an outline of each phase of research as it is being conducted in a study of Delaware's Coastal Plain.

MODEL GENERATION

Resource Pattern

The immediate concern of the Delmarva study is prehistoric subsistence potential and its relation to settlement patterning. The resource pattern survey, therefore, stresses the edible fauna and flora found in the study area. Other types of resources (stone, clays, reeds and grasses, etc.) are useful in understanding adaptation, but are not used in the formation of these models. The structure of the resource pattern is analyzed on three levels: 1--a listing of the resources available with a discussion of their abundance and nutritional values, 2--a discussion of the seasonal availability and fluctuations in those resources and 3--their geographical distribution.

The food resources are first divided into floral and faunal classes. Edible flora available is divided into five sub-classes: 1--fruits, 2--nuts, 3--seeds, 4--roots and 5--greens. The particular species assigned to each class is determined by a floral inventory of the Delmarva (Tatnall 1946) and selecting from that, those edible species as determined by a source on edible plant foods (Fernald, Kinsey, Rollins 1958). Those species known to be intrusive into the area during European colonization are excluded. Edible fauna are sub-divided into five sub-classes: 1--fish, 2--shellfish, 3--migratory waterfowl, 4--upland fowl, and 5--land mammals. Again, those faunal species known to be intrusive are omitted and, most importantly, include those known to have been present prior to contact.

Each species in the floral and faunal classes are analyzed in terms of nuturitional value, relative and/or absolute abundance, seasonal availability and population fluctuations. The abundance studies include an evaluation of the relative, and where possible, absolute carrying capacities in order to determine possible subsistence significance. The nutrition section treats the average nutritional values of each major food class so that a relative measure of the efficiency of each type of resource may be obtained. Seasonal availability, which refers primarily to migratory waterfowl, anadromous fish, and almost all classes of floral resources, is a determination of the time of year when each resource is at its level of greatest abundance. Most faunal classes are evaluated in terms of seasonal population fluctuations in both absolute size and relative density.

The geographic distribution of food resources varies in a comprehensible way with the parameters of the physical environment. These parameters (light, water, salinty, temperature, etc.) are analyzed to form sub-classes (known as micro-environments) of the general environmental setting. The definition of the established micro-environments are, in part, derived from a grouping of soil types with similar drainage characteristics (U.S.D.A. soils books) and in part from other sources (Tatnall 1946). Recognized micro-environments are; 1--poorly-drained woodlands, 2--transitional woodlands, 3--well-drained woodlands, 4--tidal marsh and estuarine, 5--permanent fresh water and 6--salt water bays and oceans (Fig. 3) These micro-environments are generally applicable to all coastal plain areas in the east, but the floral and faunal details must be determined for each area. The associated floral and/or expected fauna and their seasonal fluctuations are determined for each micro-environmental class. This type of information produces the distributional control necessary for discussing subsistence and settlement patterning. The major concern in the floral and faunal food distributions is in determining where the maximum amount of food would be available at a specified season. This does not imply that those foods are only available at those locations for it is obvious that the biophysical environment can not be so simplistically structured. However, this is sufficiently explicit to allow the generation of models with testable hypotheses.

At this point, the problem of micro-environment stability is of major concern to researchers dealing with long periods of time. Two factors effect the use of a modern resource pattern projected into the past: 1--climate change and 2--changes in the micro-environmental distributions. The resource pattern used in the recent Delaware studies is based upon flora and fauna presently existing or known to have recently existed in the study area. There is reason to believe that the content, though perhaps not the relative abundances of this pattern, has remained relatively stable for the past 9,000 years (Guilday 1966). Prior to this time climatic shifts and associated floral and faunal changes render the present resource pattern impractical for adaptive studies of those periods. Therefore, the content of the modern pattern is generally useful for only a portion of Delaware's past. This span of time covers much, but not all of Delaware's known prehistory. Resource patterns for adaptational studies of earlier time periods must be constructed through detailed paleoecological studies. Food resource distribution within the modern pattern has also changed. Data from recent studies (Kraft 1971) dealing with sea level rise and associated transgressive processes indicates that micro-environmental distributions have undergone constant changes. For example, the hypothesized function of an early site, that is currently coastal, will be considerably more precise when the site's location relative to the old shoreline and the accompanying micro-environments at the date of occupation is known. A present day coastal location may have been 30 or more miles inland at the time of initial occupation. It should be apparent that resource distribution studies within the modern pattern in any but the most

recent prehistoric periods must be founded in a detailed paleogeographic and, ideally, paleoecological analysis of the study area for the time periods involved. It is only after such studies that prehistoric adaptations in the earlier time periods may be fully understood.

Culture--the adaptive process

Models of human adaptation to the resource pattern must be founded on a principle that permits decisions as to which resources were utilized and how they were integrated into a subsistence-settlement system. The principle used to generate the models is known as maximization or "the principle of least-cost" (Fritz and Plog 1970). Briefly, it implies that those resources utilized were a combination of the most efficient available resources at any given time of year. Assuming that this is the principle which guided the subsistence choices, then those choices can be predicted. Three factors are assumed to influence the efficiency of a resource: 1--nutritional value, 2--reliability and 3--ease of procurement. All subsistence resources and conflicts in availability are weighed in light of these factors. The most efficient subsistence patterns are then offered.

The above patterns constitute the end product of the model generation if all the utilized resources occurred in close proximity; that is, all resources could be obtained from the same settlement. The micro-environment distribution map shows, however, that these different resource areas may be miles apart. This causes locational conflicts in the subsistence pattern. The manner in which these conflicts are resolved produces the completed subsistence-settlement models. Location and duration of settlements varies directly with seasonality and distribution of the desired (most efficient) resources and the mobility and division of labor of the society involved. The degree of settlement permanency should vary with the complexity of the division of labor and the technological ability of the group to efficiently transport resources to the settlement. (The above statements are particularly true for non-horticultural societies.) Based on these factors, four settlement types are possible: 1--seasonal camps, 2--permanent camps, 3--semipermanent camps, and 4--transient camps. These types of settlements are then combined, using the principle of least cost in terms of efficiency of movement to and from resources to complete the models. Each model is considered as an efficient way of utilizing the resource pattern (Fig. 4).

It should be apparent that the constructed models are based solely on a knowledge of the resource pattern and the principle of maximization of that pattern. The models are not based on prehistoric settlement data, and in fact, they need not assume that anyone ever lived in Delaware, such an assumption, however, is necessary to test these models.

MODEL TESTING

Each of the proposed models has a set of test implications concerning procurement and processing activities at the various settlement locations.

These are translated archaeologically as functional tool categories and resource refuse (Fig. 5). A major objective of any ecological analysis of archaeological data is the precise determination of the subsistence activity or activities at a site. Testable implications concerning expected activity sets and resource refuse frequencies can be derived from the subsistence-settlement models established in the resource survey phase. Activity sets are groupings of artifacts related to a single economic activity (Struever 1968b) and in a general way they may be divided into resource procurement and resource processing tools. Refuse frequencies can be calculated from counts and weights of the preserved remains at archaeological sites.

The use of activity sets in testing implications derived from environmental survey involves the use of functional analysis. The function of artifacts can be determined through several macro-and micro-visual techniques which identify modifications of the tools due to extensive use of certain kinds. Activity sets include tools and tool kits associated with both the procuring and processing of resources. This involves the actual acquisition of the resource, its primary processing for food (if an edible resource), and its secondary processing in such industries as hide preparation, bone tool manufacturing, etc. These are the implications of each model that must be fulfilled at an archaeological site before it may be assigned a place in any of the models. A research program is designed so that all possible micro-environmental locations are surveyed for the time periods involved and site fuctional studies carried out.

CONCLUSIONS

Ecological studies of past social systems are still in their infancy in the Mid-Atlantic area. Two sites in Delaware have been analyzed with this end in mind. The results of both analyses were inconclusive, partially because the sites were not excavated with the aim of solving these problems. Research in such a problem oriented framework, however, will eventually prove of significantly more value to the understanding of adaptive process than the "thing" oriented approaches of the past.

CLIMAX VEGETATION TYPES

on Delaware's Coastal Plain

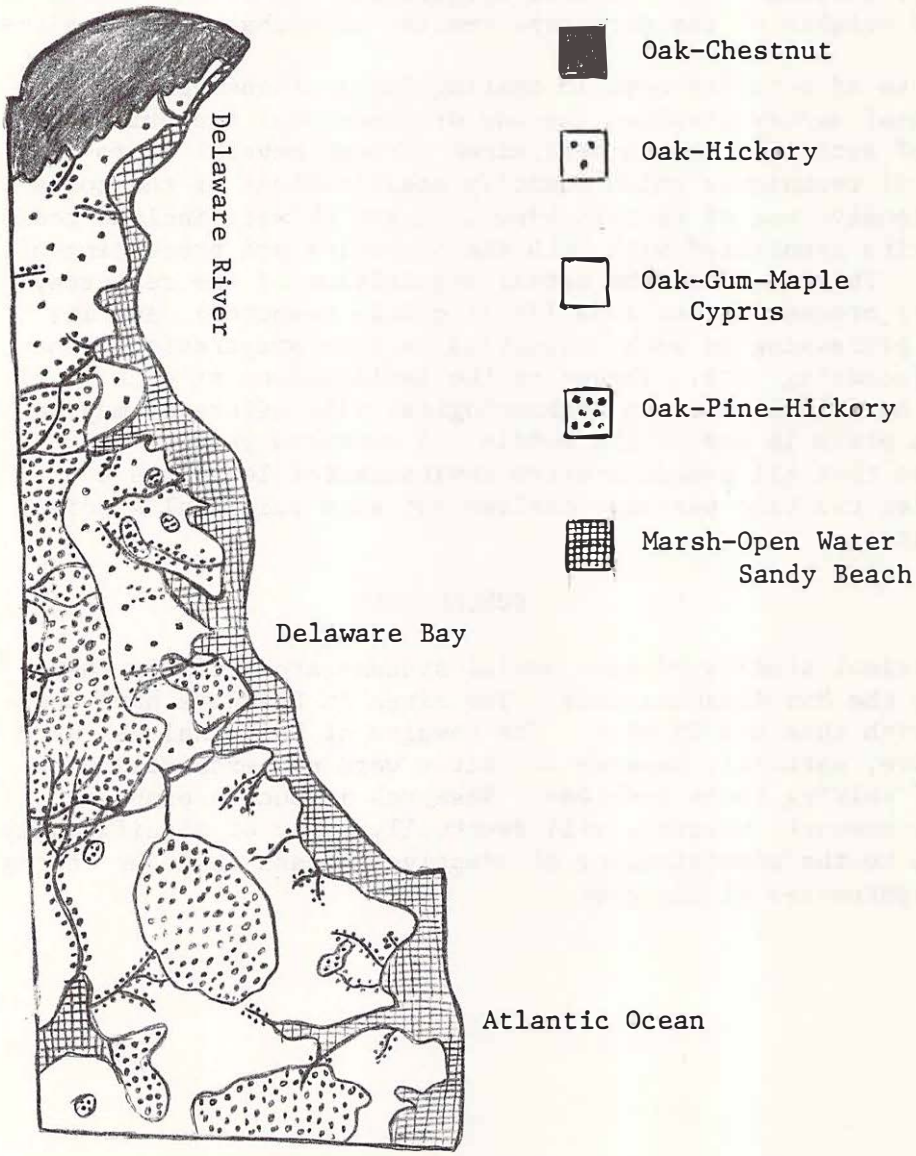

FIG. 3: Climax Vegetation Types

A SUBSISTENCE-SETTLEMENT MODEL

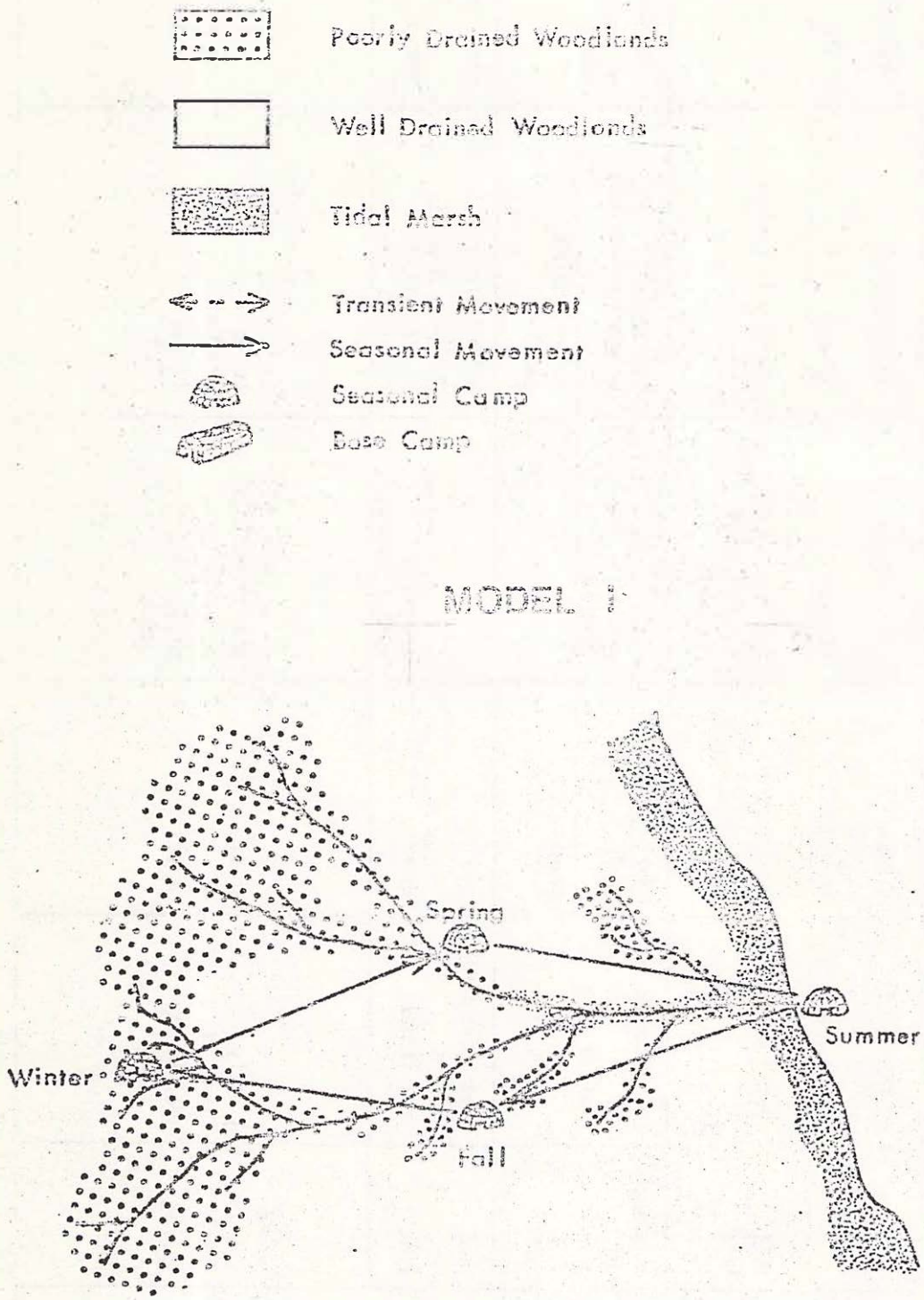

Fig. 4: Subsistence-Settlement Model

MODEL IMPLICATIONS

MODEL	SEASON	DOMINANT MICRO-ENVIRONMENT	SUBSISTENCE RESOURCE	PROCUREMENT TOOLS	PROCESSING TOOLS	SEGMENT TYPE
ONE	mid to late spring	well-drained woodlands	anadromous fish	net weights, gorges, hooks, harpoons	knives and flakes	seasonal camp
	summer to early fall	tidal marsh and/or bay	shellfish		knives and flakes	seasonal camp
			fish	net wts., hook, gorges	knive and flakes	
			seeds		muller, mortar, pestle	
	mid fall	well-drained woodlands	nut		pitted stones muller, mortar	seasonal camp
			deer	projectiles atlatl-weights	knives, beamer scrapers, awls	
	winter to early spring	poorly-drained woodlands	deer	atlatl-weights projectiles	knives, beamers, scrapers, awls, needles	seasonal camp
			ducks	projectiles, bolas	knives	

Fig. 5: Model Implications

Discussion

H. McCord: McCord called for a clarification of the assumptions and noted that if the sites in Griffith's model are not contemporaneous, then the model is futile.

L. Brennan: Brennan noted that site differences could be cultural rather than ecological.

D. Griffith: Griffith countered that all models begin with assumptions.

C. Holzinger: Stated that Griffith showed a lack of understanding of attributes which make certain questions in the model premature and once functional attributes are known the questions can be answered.

J. McDermott: McDermott noted that a very large data base is needed for environmental reconstruction and emphasized the need for studies of plant geography.

D. Griffith: Griffith replied that the scope of the ecological data depends on the background needed for the paleo-ecological model.

P. Cresthull: Cresthull asked for a clarification of site utilization.

D. Griffith: Griffith noted that pounds of meat can be used as a clarification of site utilization.

R. Handsman: Handsman noted funding problems as cited in Butzer and also he noted that the nature of explanation is the basis for most of the previous questions. In general the partitive approach is fruitless.

P. Cresthull: Cresthull noted that more advantage should be taken of ecological work being done at the Edgewood Arsenal.

J. McDermott: McDermott observed that past taxonomic and distributional studies in biology should be utilized in archaeological research.

L. Brennan: Brennan emphasized the problem of incomplete preservation of food at a site and that this lack of evidence makes it futile to construct ecological models.

COMMENTS ON DANIEL GRIFFITH'S ECOLOGICAL STUDIES OF PREHISTORY

Louis Brennan
Editor, AENA

Daniel Griffith's interesting and valuable paper on ecological models for the interpretation of prehistoric settlement patterns falls into two parts: the ecological model derived from a close study of Delaware micro-environments, and the application of that model to interpretations of prehistory. I have had the pleasure of reading the full Delaware study of the distribution of micro-environments, the methods and data of which Griffith lucidly presents. It is admirable work and shows clearly enough that in-depth archaeology cannot be done without the gathering and organization of such data. But the archaeological use of the model, or system of models, is something else again and brings up a question that has never been satisfactorily answered: how can these models be fitted to actual archaeologically existent sites and, indeed, can they ever be fitted in any but the most suggestive and tentative way?

The trouble begins with the site itself. Few who have done any digging will insist that the dig locus is undisturbed and as its last residents left it. Complex sites, occupied by a succession of cultures, have certainly been disturbed by each of their successive occupants and very few sites still exist which have not been disturbed by the even more disruptive activity of more recent residents. It is quite true that several features turn up under the shovel or trowel in the place where the people who were responsible for them placed them: burials, pits, structure patterns, shell middens, hearths, and these provide the gross lineaments of occupation. There is always the highest probability that what can be traced is only a fraction of what was once in existence.

While these may have vanished, or been truncated or distorted, they are in place. The same, however, cannot be said for artifacts and other easily displaceable evidence, such as sherds and bone. Sherds are instructive clues as to how severe the disturbances of much of the artifactual evidence must be. Ideally, if a site is being excavated that retains the aspect it had when occupied by its last residents, the sherds of a broken pot should be present and accounted for near the place where the pot was broken or discarded. But this is a situation almost never encountered except in graves. Sherds are scattered over most sites like leaves and rarely can more than three or four be fitted together.

If non-perishable artifacts and semi-permanent evidence is found in such disarray, the organic evidence of subsistence is 99.9% vanished. Most of it passed through the alimentary canals of site occupants and would be found as coprolites, which occur only under the most fortuitous of circumstances, in dry caves and other relatively rare contexts. Bones, shells, occasional finds of nut hulls and seeds provide evidence as to what was eaten, but not as to how much nor to the items of food that were eaten but not preserved. To derive anything

but the most rudimentary conclusions about subsistence from a site is an exercise in conjecture or mathematics, not an approximation of truth.

There is this disparity, then, between the Delaware study of micro-environmental models and the occupation models that can be derived from excavation. The environmental model is based on complete data and can be verified by observation again and again; it states what exists now as a living system which validates itself by being in living existence. But a site is dead evidence, it is incomplete evidence, it is evidence that cannot be reiterated for continuing observation, and it is not a diagram of cultural activity but of a state of cultural activity at the time of the site's last occupation. There is no way of knowing whether a site was abandoned at leisure, preparatory to removal elsewhere, or in haste, under pressure of an emergency. To attempt to fit the "perfect" environmental model to the very imperfect archaeological model is not going to effect a very scientific "explanation". And, the deeper in time we go the less reliable the environmental model becomes, resulting in two uncertainities.

To fit the environmental model to the site actuality Griffith has to resort to the principle of maximization or "least-cost" which, as much sense as it makes to the logical mind, is not founded, to my knowledge, on any great body of archaeological evidence. It is of itself a hypothesis to be tested, by what it purports to test. It is not objectionable or even unlikely. It simply will not do the job of relating an environmental model to an archaeological set of facts. What the model predicts would be found on a site could just as easily be predicted withouts the model or the maximization principle, on the simple definition of hunting-gathering.

Where Griffith's proposal breaks down, it is now clear, is in the archaeology. You cannot test good models with unsatisfactory cases. However, we are archaeologists, and it is archaeological models we want to verify, not environmental ones. The first job is to construct a model or an hypothesis on the data and observations obtained by excavation and then compare that hypothesis with such environmental models as the Delaware study. No "principle" need then be invoked to relate the one to the other; the data from the site falls into congruity with the model or they don't. If they do, maximization may be the explanation, but that is another matter altogether.

The fact is that archaeology cannot follow the explanation procedures of science, because it deals with the evidence of an accomplished, completed, long forgotten deed or series of deeds, replicatable or iterable experiments to confirm hypotheses cannot be performed. A site can be dug only once and, no matter how precise the recording that is not repetition or reconstruction or duplication, and no matter how precise the recording no one can be sure exactly what he dug. That the application of the environmental model to two sites resulted in no verdict is not to be wondered at. Sites are not designed experiments for the testing of models. Archaeological hypotheses should derive, it would seem, from archaeological data if they are to be tested on archaeological sites.

ENVIRONMENT AND PREHISTORY IN THE MIDDLE ATLANTIC PROVINCE

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Catholic University of America

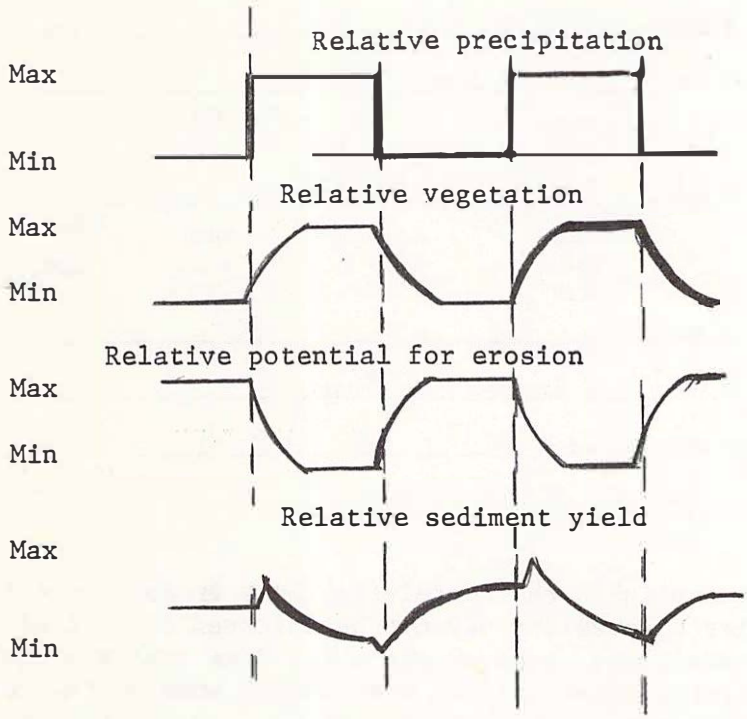
During the background literature investigations for the Flint Run Paleo-Indian project, differing points of view emerged on several key issues regarding the environmental changes that have taken place in the Middle Atlantic States, as well as the entire unglaciated portion of Eastern North America, in the last 20,000 years. One of the issues of contention is the nature of the climatic changes which have occurred. One side postulates a continuous warming trend from late glacial times climaxing in a hypsithermal (warm) interval in mid post-glacial times followed by a gradual decline to modern conditions (Deevey and Flint 1957). The other camp postulates abrupt climatic shifts characterized by rapid, step-like transitions in the climatic regimes (Bryson 1970).

Coupled with these differing views on the nature of climatic change in Late Quaternary times are also opposing notions on the character of the environmental responses to these changes. Braun (1955) contended that the vegetational character of most of the unglaciated east remained essentially unchanged except at the margins of the ice, while others, e.g. Martin 1958 and Whitehead 1965, 1973, countered that the southward movement of the ice resulted in significant floral and faunal displacements with the elimination of many deciduous elements in the temperate regions and their replacement by boreal species. Much of the recent evidence points to the latter explanation as a more satisfactory alternative, but acceptance of this point of view necessitates a reevaluation of some basic ecosystemic concepts. First, the question of ecosystem homeostatis---in other words, to what extent do modern biotic communities represent successional climaxes and if so how susceptible is their development to climatic oscillations; and secondly, the question of how well integrated are these communities. If displacements are forced, are the displacements zonal or azonal? Will communities move as entire units, or will individual members of the community react independently, resulting in azonal distributions?

In the following pages I will present some of the data which is available from the Middle Atlantic States in support of the episodic model of climatic and environmental change. This does not rule out the possibility of the Deevey and Flint model being correct for the two models are not necessarily mutually exclusive and the evidence indicates that both models are plausible but on different time scales. It is my contention that the episodic model can be an effective tool for the understanding of cultural/environmental relationships in the Middle Atlantic Province and I propose that this model be formally adopted by all archeologists working in the area.

The model assumes that a square wave function accurately defines climatic behavior and that the various biogeomorphic responses can also be represented by variants of this basic square wave function by introducing compensatory factors such as lag into the system. Having a record of the climatic history of an area it is then possible by utilizing the model to predict vegetational and geomorphic responses. Conversely, the predictive validity of the model can be evaluated by analyzing the record in a given field situation such as the Thunderbird site.

From the anthropological point of view it should also be possible to postulate a similar square wave function variant in terms of cultural response to changing environmental conditions. The major problem with this is the definition of suitable parameters which can be used to measure the behavior of the 'cultural' function. By adopting a techno-environmental determinist orientation it should be possible, through the analysis of changing artifact and assemblage variability to explore the relationships between the changing material culture subsystem and the changing setting.



(from Knox 1972)

In order to lend substance to this idealized model Wendland and Bryson (1974) analyzed two independent sets of data to arrive at times of large scale hemispheric environmental and cultural discontinuity. The first set consisted of all published radiocarbon dates of discontinuities in pollen profiles, glacial records, sea level heights and other geologic-botanic events. The second set utilized those dates associated with 'archeological' cultures. Different statistical techniques were utilized to arrive at times of major and minor discontinuity in the two sets of data and the following series of hemispherically synchronous environmental and cultural episodes were arrived at.

ENVIRONMENTALLY AND CULTURALLY SIGNIFICANT CHANGE DATES
(Cultural data stratified by significance value, "t")

		Discontinuities					
		Botanic		Cultural			
Episode	Subepisode	Major	Minor	>50	50-30	30-10	<10
Pacific							700
		850				830	
neo-Atlantic							1,150
					1260		
Scandic							
		1,680					1,820
sub-Atlantic							
		2,760		2510			
sub-Boreal	SB III					3110	
	SB II		3570				
	SB I		4240		4230		
		5,060					
Atlantic	A IV						5,680
	A III		6050			5900	
	A II		6910				7,230
	A I		7740				
		8,490					
Boreal							
		9,300			9530		
pre-Boreal							
		10,030					
late-Glacial							10,870

This table clearly establishes the correlation between environmental and cultural changes. Direct causality may not be inferred for all of the environmental/cultural transitions, however the table does indicate that a definite set of interrelationships exists, even though some of the cultural episodes are not indicated by the botanic-geologic record and several of the botanic discontinuities are not reflected in the cultural data. This could be the result of either reduced environmental stress during some of the transitions or in other cases it may reflect internally induced culture change.

The evidence for these environmental episodes in the Middle Atlantic Province comes from the various fields of Quaternary research including geomorphology, pedology, palynology, paleobotany and paleozoology. There is no single location in which the complete record exists, however fragments of evidence can be gleaned from site reports in which the collection of these kinds of evidence was not the major order of business. The sedimentary record at the St. Albans site in West Virginia, for example, which covers the period of the early post-glacial gives evidence of a sharp discontinuity in the rates of sedimentation both within the lower zones as well as between the pre- and post-6000 B.C. units. Likewise, the record at the Faucett site gives an indication of the episodic

nature of alluvial events by the sharply increased rates of accretion during the deposition of the pre-Vosburg component as well as during the transition from Perkiomen to Orient (Kinsey 1973). Pollen profiles, where dates are available also support these dates of discontinuity; the diagram from Buckle's Bog in Garret County, Maryland (Maxwell and Davis 1972), the diagram from the Shenandoah Valley (Craig 1969), and the sequences from the southeast (Whitehead 1965, 1967, 1973; Watts 1970) all indicate close adherence to the defined episodes.

In the Shenandoah Valley in conjunction with the excavations at the Thunderbird and related sites, the record is perhaps the most complete for the entire area since evidence from all the diverse fields has been purposely sought and brought together into a coherent scheme. In order to more accurately define the character of climatic change in the valley proper (and more generally in the Middle Atlantic) the pollen data from the Hack Pond diagram (Craig 1969) were transformed into quantitative estimates of climatic parameters by utilizing statistically derived transfer functions. This method was originally developed by Webb and Bryson (1972). They assumed a linear relationship between modern climate and modern pollen data in the following form:

$$C_m = BP_m$$

where C_m = modern climatic parameter

P_m = modern pollen percentage

B = transfer function

By utilizing a multilinear regression technique they solved for B. This value of B was then applied to the fossil pollen data in order to convert it to 'fossil' climate data. This technique then generated curves of the changing climatic regimes corresponding to the changing pollen profiles. The figure below represents the Mean July Temperature curve for the Middle Shenandoah Valley. Based on this curve it can be inferred that the Mean July Temperature in the valley was lowered by at least 5°F during late glacial times. The major climatic episodes are also clearly demarcated in the curve. Similar curves for Precipitation minus Potential Evaporation, Precipitation During the Growing Season, Precipitation as Snowfall, Hours of Sunshine, and Length of the Growing Season were generated and these have enabled us to better define each of the episodes. Indications are that the late glacial climate was at least as severe as the modern day climate at Big Meadows on the Blue Ridge at an elevation of 3500 feet.

At the Thunderbird site supporting evidence for the episodic sequence comes from several lines of evidence. Ham (1974) has described the alluvial depositional events at the Thunderbird site on the basis of detailed textural profiles and he has correlated these events with the climatic episodes by using the archeological evidence to provide a chronological framework. According to the model the density of vegetation, hillslope erosion and sediment yield are largely dependent on climatic

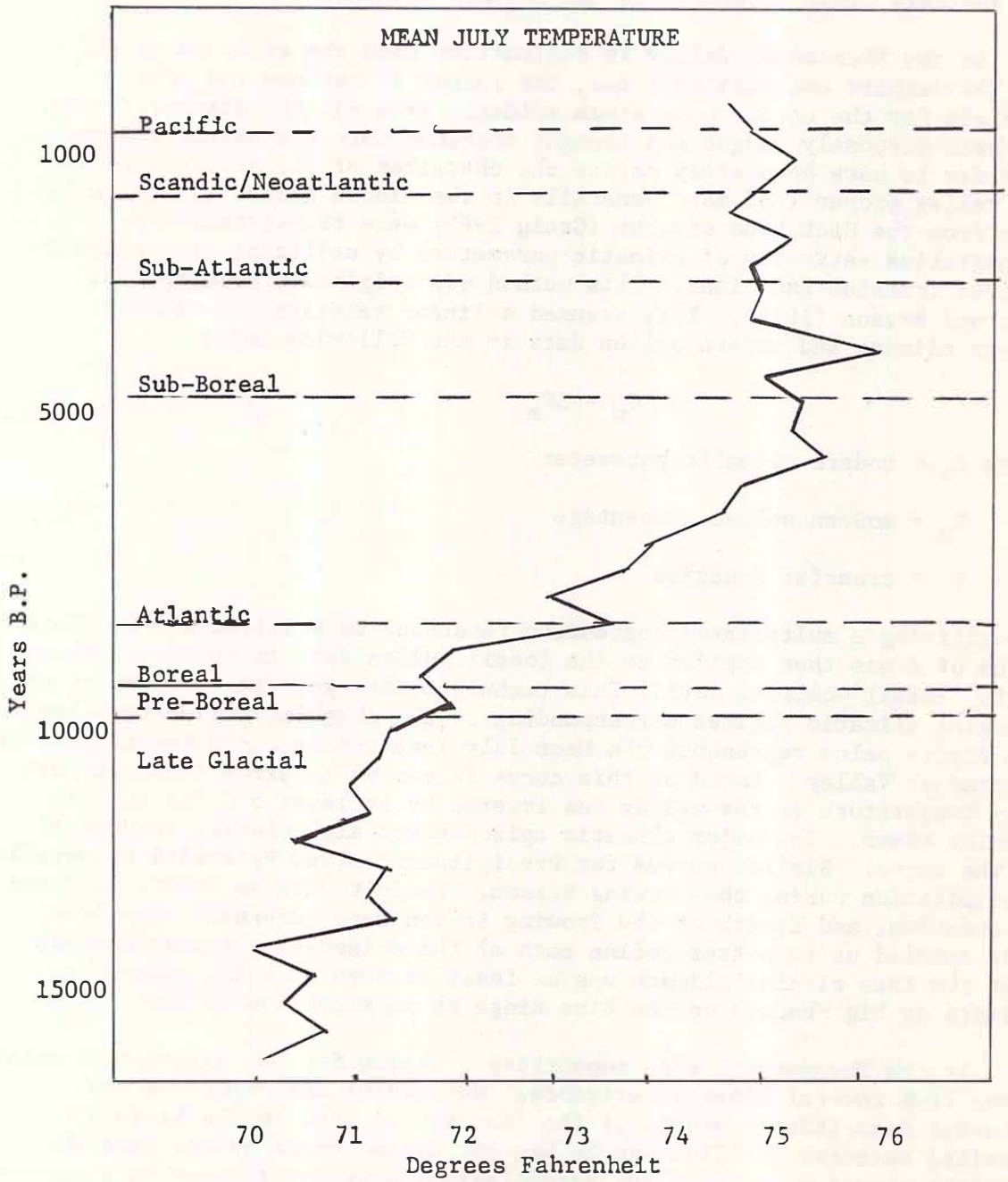


Fig. 6: Mean July Temperature Curve

parameters and therefore floodplain deposits should be reflective of climatic change. An excellent correlation was obtained between the predicted responses based on the idealized model and the archeologically dated tectural discontinuities, thus confirming and expanding Gardner's (1974) original interpretations. Verrey's (1974)analysis of the distribution of ice-borne lag deposits with respect to the different cultural levels has also helped to shed some light on the magnitude of the climatic change involved from the late glacial (Paleo-Indian) to the early post-glacial (Early Archaic) levels.

At the Fifty site the presence of a fragipan horizon which contains extensive Early Archaic to Middle Archaic materials provides another episodic marker. Although the genesis of these hardpan horizons is very much a matter of debate among soil scientists, one of the more acceptable hypotheses attributes the formation of the typical polygonal crack pattern to dessication. If this interpretation is accepted this can be taken as evidence for a mid- to late postglacial xerothermic interval, possibly correlated with the temperature peak in the Mean July Temperature curve during late Atlantic to early Subboreal times.

Vegetational patterns are a bit more difficult to reconstruct. The traditional palynologic interpretations have seen late glacial spruce-fir-pine-nonarboreal pollen zones as open boreal woodlands, i.e. parklike stands of conifers in a grassy open landscape. (The presence of temperate deciduous elements in the pollen flora has generally been dismissed as evidence of long distance transport.) These open boreal woodlands have been seen to give way to closed coniferous forests, followed by mixed conifer-northern hardwood forests, then temperate deciduous forests with a final phase in which there is a resurgence of conifers. The uppermost levels in some profiles show a relatively high percentage of composites and other non-arboreal pollen reflecting late prehistoric to contact forest clearance.

The problem with vegetational interpretations of the pollen record is underscored by Goodlett's (1954) study of the vegetation in Potter County, Pennsylvania in which a detailed vegetational study of the county revealed that two thirds of all the vegetational variation exhibited by the entire northeast was present in an area one sixtieth the size. Another problem involves the interpretation of the pollen data in the absence of absolute pollen frequencies. For example recent studies of the contemporary pollen rain in the subarctic have shown that the maximum percentages in spruce pollen are obtained outside of the boreal forest proper, to the north in the more open transitional areas. These difficulties can generally be overcome if locales amenable to good pollen preservation can be located and dated as has been our good fortune at the Fifty site. Here we have highly organic buried backwater swampy layers with a rich micro- and macro-flora including pollen, seeds, wood fragments and charcoals. The clayey matrix which characterizes the upper sediments in this part of the floodplain also provide an excellent medium for wood preservation. My own pollen studies combined with Ham's (1975) analyses of the seeds and Johnston's (1974) analyses of the woods and charcoals present throughout the profile have provided us with an excellent picture of the changing local conditions at the site.

Phytolith studies at the Thunderbird site have also yielded useful information for paleoenvironmental reconstruction. Phytoliths are small opaline silica bodies which are deposited in the cells of plants and when the plant material decays they become incorporated in the sediments. Phytolith assemblage characterization techniques are still in their infancy but based on gross quantitative studies of different soil horizons, as well as gross qualitative studies of the different morphologies of different assemblages one can make significant observations on the changing environment.

The problem of vegetational succession in the Middle Atlantic is further complicated by differing notions on the character of the original late glacial vegetational patterns. This is important in view of the strong bearing it has on the interpretation of Paleo-Indian lifeways. The traditional interpretation referred to above, of an open boreal woodland has been recently questioned by several investigators, e.g. Brown and Clelland (1968), and we are now confronted with two different points of view. As stated by Brown and Clelland (1968:115) the controversy is as follows:

> One holds that the vegetation maintained its zonal distribution in front of the ice sheet in response to climatic controls that operated in patterns similar to those of today (Martin 1958). The other recognizes that a 'hodgepodge' of vegetational components may have existed beyond the glacial border in response to climatic conditions and patterns different from those of today (Bryson 1966; Bryson and Wendland 1967).

The bulk of the evidence seems to be in support of the latter hypothesis. This includes:

a. the persistence of mixed deciduous pollen types, grasses, sedges, heliophytes, and hydrophytes within the broader coniferous matrix.

b. the presence of a varied megafauna in a number of deposits and differing contexts such as woodland musk ox, the grazing mammoth, the browsing mastodon, giant moose of swampy forests, grassland species of bison and horse, woodland peccaries, along with the smaller whitetailed deer, caribou, elk and giant beaver.

c. the presence of an extensive list of seemingly anomalous assemblages of small ecologically sensitive animals which today occupy extremely diverse environments, such as the yellow cheecked vole from the taiga, the collared lemming from the tundra, the whitefooted mouse and pine vole of temperate forests, two prairie species- the thirteen-lined ground squirrel and the sharp tailed grouse, the Hudson Bay toad and the common frog. Based on this evidence Brown and Clelland (1968:114) conclude:

> The vegetation grew in a mosaic of boreal, deciduous, and grassland communities rather than in zoned communities like those of today. The effect of the mosaic pattern of vegetation upon the distribution of mammals would have been to support species that would otherwise not be found together. The fossil record supports this deduction, and at the same time, the distribution of Paleo-Indian sites and spot finds (in the upper Midwest) can also be more satisfactorily understood.

In the Middle Atlantic context this interpretation also makes sense, especially since much of the faunal evidence utilized in the above analysis is from this area including the finds at New Paris No. 4 Sinkhole (Guilday et. al. 1964) in Bedford County, Pennsylvania, Natural Chimneys, (Guilday 1962) and Saltville (Ray et. al. 1967) in southwestern Virginia. Recent finds in the Georgia piedmont are also supportive of this interpretation (Voorhies 1974).

By adopting this interpretation one is faced with the problem of vegetational succession through the postglacial, an understanding of which is vital to the comprehension of Archaic cultural patterns. Did the mosaic pattern persist through the early postglacial times and if so is this reflected in the continuity of tradition from Paleo-Indian to Early Archaic as described by Gardner (1974)? Certainly the pollen and macrofossil evidence from 44WR53, a buried backwater swamp associated with the Fifty site, indicates the persistence of several markedly different environmental zones at 9300 B.P.. The pollen evidence from the valley also seems to indicate that the modern vegetational pattern did not become established until approximately 3600 B.P.. In this cultural context, the pronounced oak maxima which characterize so many of the regional profiles during the mid-postglacial is something that must be given due consideration when exploring mid-Archaic lifeways.

In terms of faunal shifts we can see the late glacial and early postglacial diverse fauna slowly become impoverished but the early postglacial records are meager. There is no reason not to expect that some of the megafauna elements may have persisted as late as 8000 B.P. although records in Pennsylvania of small mammal populations indicate a modern character as early as 9300 B.P. Hydrologic fluctuations during the mid- to late-postglacial xerothermic period (early subboreal times) must have affected riverine and estuarine resources, so that despite the presence of a modern mammalian fauna from early postglacial time onwards a consideration of the broader exploitive patterns must take into account the climatic fluctuations which must have affected other vertebrate resources.

In conclusion I would like to stress that although floral and faunal records may indicate that the environment has remained 'stable' the climate has not stopped changing. We may gain a false sense of stability when viewing the greater plateaus such as the full glacial and the mid-postglacial to present, but as far as the cultural record is concerned it is the smaller fluctuations which must concern us for they provide the clues to a more complete picture of changing lifeways in the past 10,000 years.

Discussion

C. Holzinger: Holzinger noted that final resolution of the problem lies with the paleobotanist and the geologist, not the archaeologist.

V. Carbone: Carbone replied that cultural evidence is important because it points out evidence of episodes that the geologist and paleobotanist might miss.

Bruberg: Brueberg asked if phytoliths are species specific.

V. Carbone: Carbone replied that phytoliths come from heavy flotation and can be used to distinguish between arboreal and non-arboreal remains.

J. McDermott: McDermott asked about the extent of the literature on phytoliths.

V. Carbone: Carbone noted that there are only a few articles on the subject.

P. Cresthull: Cresthull noted similar work being done in the Chesapeake Bay area.

Martin: Martin questioned if phytolith preservation was similar to pollen preservation.

V. Carbone: Carbone replied that phytoliths are silica bodies and are preserved in most environments.

LATE WOODLAND FLORAL AND FAUNAL EXPLOITATIVE PATTERNS IN THE UPPER DELAWARE VALLEY*

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This paper discusses recurring Late Woodland floral and faunal exploitative patterns manifested at the Faucett site, Bushkill, Pennsylvania. Of the 104 Late Woodland features excavated during the 1972 field season, 20 were assigned to 1 of 4 discrete occupations. These occupations are named for the associated ceramics: Owasco, Oak Hill, Chance, and Munsee. Floral and faunal remains recovered from contemporaneous features used during each of the occupations demonstrated an overall similarity in patterns of food resource exploitation. Differences among the 4 components are primarily of intensity rather than the variety of species exploited.

Excavation strategy enabled a determination of more than a listing of food resources exploited by each group. Seasons of encampment, duration of stay, econiches exploited, and non-food uses of floral and faunal resources were delineated.

Each feature was cross-sectioned by the troweling and sifting of arbitrary levels. After a vertical profile was drawn to scale, natural bands could be seen. Natural bands are the result of differential deposition of organic and non-organic remains with pit fill. They differ in soil color and consistency from adjacent bands and are believed related to the function(s) of the pit or cultural activities undertaken in the vicinity of the pit. When pit fill is homogeneous, 1 natural band is present indicating similar depositional circumstances throughout the period of the pit's usefulness.

Each natural band was flotated and interpreted as a discrete cultural unit articulating with other discrete units of the same feature. Features demonstrated to be contemporaneous to one another were interpreted as a series of interrelated cultural units revealing a range of activities undertaken by 1 group of people continuously occupying the site. This approach allows a determination of overall Late Woodland patterns of food resource exploitation by analyzing occupations known to be contemporaneous. Each occupation at a single site can then be compared to others at the same site to view exploitive patterns of groups occupying the same econiche at different times.

*Acknowledgments: This is a summary of my Ph.D. dissertation, Seasonality and Settlement Pattern of Late Woodland Components at the Faucett Site (Moeller 1975). Date were collected in 1972 at the Faucett site and in 1974 at the Michaels No. 4 site. Both excavations were funded through National Park Service grants administered through the North Museum, Franklin and Marshall College. A fuller version of the present paper is scheduled for publication in a forthcoming issue of AENA.

This approach is preferable to assuming that all features can be studied as an undifferentiated unit to determine Late Woodland lifeways, or each feature must be studied separate from each other one lest a mosaic view of many noncontemporaneous Late Woodland occupations be interpreted as a single one.

At the Faucett site a preliminary separation of pits based ceramic, lithic, and metallic typologies indicated the presence of 3 and possibly more occupations. Upon examining fitting sherds from different features, only 4 occupations were manifested. Addition of the pit morphology parameter added many more features not having fitting sherds and/or not having sherds with similar motifs.

Each pit in each of the 4 discrete occupations was examined in careful detail, natural band by natural band, to determine the floral and faunal resources exploited. To this basic archaeological data was added the results of a modern floral (tree) survey by Laura Goldman (1975), a biology major at Franklin and Marshall College, and ethnographic and ethnohistorical data relative to seasonality, settlement pattern, and plant utilization practices. The result was an interpretation of each occupation and a model of Upper Delaware Valley Late Woodland seasonality and settlement pattern which can be tested by future excavation.

Owasco

The 4 pits assigned to the Owasco occupation contained 3.3 gm mussels, 1 frog, 3 deer, 2 elk, 1 bear, 1 wolf, 1 chipmunk, bedstraw, goosefoot, amaranth, poke, smartweed, pigweed, corn, hickory nut, butternut, raspberry, grape, sumac, and pondweed. With the exception of elk and wolf, all of these are currently found in the vicinity of the site.

The only hunting weapon is a triangular projectile point. Fishing implements are 2 netsinkers and 5 notched and trimmed implements, which may also have served as netweights. Two teshoas are possible animal processing tools. Evidence for food processing functions of the 5 hammerstones is the large quantity of splintered mammal bone (for marrow?) and cracked hickory nuts and butternuts. Other food processing implements are a muller and milling stone for grinding the abundant seeds also evidenced.

Non-food uses of floral resources cannot be shown for any of the identified species in Owasco contexts. While dyeing, smoking, fabricating, and medicinal functions could have been served, they cannot be substantiated. The brevity of an occupation evidencing so many preservable food resources just prior to the onset of winter suggests this was an intensive collecting and processing camp. More food is available at this time than at any other time of the year. This was the locus having the greatest variety and quantity of plant and animal resources within an easy walk. While camped here for a month or so in the late summer and early fall, no permanent

shelters were needed. Few implements other than netsinkers, arrows, food processing implements, and a cooking pot were required. The presence of the vast majority of processing and procuring implements in the top 8" of pit fill also suggests that these cumbersome implements were left behind in order to carry the preserved food to their winter quarters.

Oak Hill

Food resources recovered from the 3 Oak Hill features were 2 deer, 1 elk, 1 gm. mussel, fish, bedstraw, goosefoot, corn, hickory nuts, thorn apple, and sumac. Evidence for hunting are 2 triangular points. Fishing with weighted nets is indicated by 3 notched and trimmed implements and a netsinker. A single teshoa is the lone animal processing tool.

Floral resources collected in the vicinity of the site and deposited in the pits show less variety and quantity than the previous occupation. However, the same pattern of late summer and early fall utilization does recur. Non-food uses of floral resources might include smoking, fabricating, and medicinal functions. Just before the occupation terminated, projectile points, food processing and procuring implements were deposited in the pits. Most were found 8" below the surface of the pit.

Chance

The 2 Chance features had 27,433.1 gm. mussels, fish, 1 deer, bedstraw, goosefoot, poke, smartweed, and raspberry. Processing and procuring tools for fauna are absent. An examination of the mussel shells revealed they were steamed. There is no evidence for crushing or cutting the shell to extract the meat. One milling stone reveals that plant food resources were collected for their seeds.

The raspberry seeds are the only reliable seasonal markers and indicate an occupation commencing as early as late June and terminating in September. The absence of hickory nuts, thorn apples, grapes, corn, and butternuts might mean the occupation had ended before the maturation of these resources. Another possibility is that the camp was only for the large scale exploitation of mussels and fish. Plant foods could have been collected at other nearby locations which were not excavated. Smoking, dyeing, fabricating, and medicinal functions could have been served by various parts of plants exploited for food. However, there is no solid evidence for this.

Munsee

Eleven Munsee pits yielded evidence of 1 deer, 1 elk, 5 turtles, 1 bird, 23.7 gm. mussel, fish, bedstraw, goosefoot, amaranth, poke, smartweed, corn, butternut, hickory nut, ground cherry, thorn apple,

raspberry, grape, sumac, pondweed, Jack-in-the-pulpit, and bulrush. Seven projectile points were for hunting while fishing implements are a bi-notched stone and 2 netsinkers. A possible ulu-like implement and 5 hammerstones could have been for animal processing while 3 milling stones and 2 mullers were for plant food processing.

The same pattern of an early September date for the first use of a pit and a terminal data in late October is found in all Munsee features. The only non-food function of excavated floral materials which can be strongly suggested is smoking. Although medicinal, dyeing, beverage, smoking, and fabricating functions are possible, the Munsee occupation is the only one with smoking pipes. Just before this occupation ended, plant processing and animal processing and procuring implements were deposited in the pits. Most of these were recovered in the top 8" of the pit.

Summary

Although the 4 discrete Late Woodland occupations delineated in this study comprise only 20 of the 104 features, they account for 80% of all sherds, 70% of all debitage, 99% of all bone, 57% of the identified seeds, and nearly all the notched and trimmed implements, netsinkers, notched stones, hammerstones, mullers, milling stones, teshoas, and projectile points. All faunal species and all but 1 floral species found at the site are also represented.

Each occupation was brief and had a limited tool kit, with few decorative/ceremonial items. Such an artifact inventory would be found in a small camp for the intensive collecting and processing of seasonally abundant wild food resources. In general, each occupation began in early September and could have terminated by October or at the latest, November. Before leaving the site, each group probably discarded their food processing and procuring implements in the pits rather than carry them to the next camp in their seasonal round.

Conclusions

In an effort to more fully understand the patterning of Late Woodland seasonality and settlement in the Upper Delaware Valley, the problem of spring and early summer occupations should be discussed. What are the expected ecofacts, artifacts, and features of such occupations? Could any or all features assigned to discrete occupations at Faucett have actually been utilized in spring and early summer? What are the archaeological indicators of spring and early summer encampments in the Upper Delaware Valley?

Large quantities of spring and early summer floral resources including stalks, shoots, leaves, tubers, and cambium of various plants and trees were probably collected. They were crushed, ground, powdered, and/or boiled. Due to the manner in which they were prepared and that they were for immediate consumption and not preserved for future use, these parts are seldom found archaeologically. If

carbonized remains of this nature were recovered, they would be poor seasonal indicators anyway.

In late summer and early fall the pattern of floral usage shifts to include seeds, nuts, and fruit. Remnants of these are more abundant because of their physical characteristics, modes of preparation, and techniques used in preserving large quantities of them for the winter. Portions of floral resources maturing at this time predominate any sample in which they are found simply because of their physical characteristics and cultural techniques employed in their use.

Most of the faunal resources in the Upper Delaware Valley are poor seasonal indicators. Exceptions are fetal remains of animals bearing young at only a particular time of the year and fish otoliths, scales, vertebral centrae, and operculi (Casteel 1972). Because fetal remains are soft and small, they are not likely to be preserved in the acidic soil. Fish remains are scarce, but usually well preserved when present. The second problem in recognizing a spring and early summer occupation is the lack of artifacts used only during certain seasons. Processing and procuring implements from Upper Delaware Valley sites are general purpose and could have been used at any time during the year. This is not to say that seasonal artifacts do not exist. They simply have not been identified. A third problem is the possible lack of a significant number of pits. The entire situation is dependent upon pit function. While there are insufficient data from Faucett for a detailed functional analysis of pits, several parameters are under consideration.

At spring-early summer camps plant storage pits would be nearly non-existent. Spring is not the time to set aside large good stores. Floral supplies available after winter would have been for immediate consumption and not for longterm preservation. There are no spring-early summer plant resources which could have been collected in sufficient quantity to warrant digging the large storage pits found.

Refuse disposal is not a primary function of pits. For example, Iroquois villages lasting for a decade or two have sheet middens which served that purpose. It does not seem likely that 1 group would expend a lot of energy burying biodegradeable refuse from a short-term processing camp, while groups in adjacent areas are living in year-round villages surrounded by the same sort of refuse. It is my contention that refuse disposal is an incidental function: the pits were there, they had outlived their original functions, and were now a convenient place to dump refuse.

The pits assigned to discrete occupations at Faucett had refuse indicative of intensive food processing, preparation, and/or storage during late summer of fall. Because the deepest portion of each had late summer-early fall seasonal markers and the uppermost had fall indicators, these features could not possibly have been used during the spring and early summer. This may mean that refuse filled pits

can be expected only at late summer-fall processing camps. It is possible only shallow cooking pits were needed during the rest of the year, and refuse was disposed of in sheet middens. If this is the case, intensive plowing has long since destroyed all evidence of these camps.

One possibility is that the Faucett site does have spring-early summer occupations which are not archaeologically visible. Of the 104 features found at the site, only 20 could be assigned to discrete occupations. This leaves 84 features containing very little bone, seeds, broken ceramics, and some debitage. Some or all of these might be the spring-early summer features dug by the same groups using the other 20 pits. The differences in quantity and types of food being prepared could account for the near absence of food remains. The absence of diagnostic artifacts could be accounted for if the tools were in use when the features outlived their usefulness. More pits could then have been dug for the intensive processing of late summer and early fall floral and faunal resources, and the artifacts were discarded in these pits before the inhabitants departed for the winter.

Despite poor archaeological visibility of spring-early summer occupations, they are feasible and probably even necessary to account for the corn. Corn must have been planted in the spring and had to have been tended and guarded against a variety of animals, especially deer, raccoon, rats, birds. A large population would not be necessary to conduct this activity. This in itself might inhibit recognizing or even finding these camps.

While tending the crops, Indians could have obtained sufficient floral and faunal resources throughout the spring and early summer to support themselves. A review of the carbonized seeds from the site and the modern floral survey results indicates the presence of a substantial and varied food supply. In many instances shoots, stalks, leaves, tubers, roots, and/or cambium of certain plants and trees can be eaten prior to the maturation of seeds, nuts, and fruit of the same species. The same food plants also could have provided materials for medicinal, beverage, flavoring, smoking, dyeing, and fabricating functions.

Discussion

J. McDermott: McDermott commented that the Faucett site was probably a spring occupation and agreed that more work should be done on present day fish remains and mussel growth.

F. Kinsey: Kinsey noted that because of polution it is hard to find mussels in the area for comparison.

C. Holzinger: After noting the completeness of Moeller's seed list and lauding the work, Holzinger suggested a year-round occupation and questioned systematic pit filling.

W. Clarke: Clarke suggested that the data base was skewed due to Moeller's pit selection methods.

R. Moeller: Moeller noted that the other 84 pits contained little important information and the selection of his 20 pits was based on temporal information including actual fitting sherds from different pits.

W. Clarke: Clarke also noted that in selecting only those 20 pits he could have lost the functional context.

R. Moeller: Moeller replied that the remaining 84 pits were studied in an identical manner to the reported 20 and these too were apparently filled when empty.

H. McCord: McCord suggested that the evidence could indicate that the people remained on the site in the winter using the pits as storage features and in the spring they threw the accumulated garbage into the empty pits.

R. Moeller: Moeller noted that there were few house patterns that could have withstood the winter, the pit fill was stratified by season, and tools showed insufficient wear for an entire year's use at a year-round occupation.

V. Carbone: Carbone questioned the use of pond weed as a seasonal indicator due to its presence in a Clovis living floor at the Thunderbird Site.

R. Moeller: Moeller replied that he knew of no literature on aborigional use of this plant but the tubers can be utilized.

M. Thurman: Agreeing with the seasonality assumption, Thurman pointed out that although Stuart Streuver has evidence of sedentary settlement in the Archaic, most Mid-Atlantic sites show seasonality. However, seasonality can not be assumed, but must be demonstrated, as Moeller has done.

SUSQUEHANNOCK PLANT UTILIZATION

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Introduction

During the Fall of 1973, 8 Franklin and Marshall students under the direction of Professor Fred Kinsey, as part of an independent study project, undertook the excavation of a portion of the Eshleman midden, explored by John Witthoft in the late 1940's. Located 12 miles south of Lancaster in Washington Boro, the area where the investigations took place was designated Haverstick site after the present property owner. The site is a portion of a large Susquehannock Indian village which has been the locus of numerous excavations.

The Washington Boro Basin is known for a great number of sites of different cultural affiliation ranging from Paleo-Indian to historic (17th century), Susquehannock. One of the reasons for such a high concentration of sites in one area lies in the region's favored environmental features. For instance, the soil is rich and the growing season is long. Only a small corner of Southeastern Pennsylvania has more frost-free days. Moreover, aquatic resources and large and small land mammals are abundant. The river was especially rich in migratory fish at the time of Indian occupation. Finally, since the region lies on the route of the Atlantic Flyway, migratory birds frequent the area.

The work of John Guilday and others (1962), on faunal resources was a splendid attempt to study one particular aspect of this rich environmental setting, but no studies of Susquehannock plant utilization were ever made. Professor Kinsey felt that such a study was long overdue, and thus, the excavation of the Haverstick Site was undertaken especially for this purpose.

Methodology

Cultural remains, ceramics, and glass beads, identify the Haverstick site as belonging to the Washington Boro Phase, ca. 1600-1625 A.D. However, our main concerns were with the shell, bone, and plant remains. All materials extracted from the Haverstick site were floated and analyzed over the winter and spring of 1974. Each student was assigned a different area of study and I elected to work on the seed remains.

Identification of the seeds centered on the use of resource materials, such as seed identification manuals, and included consultation with professors, students, and experts in the field. I was particularly fortunate to have for comparative purposes the use of the Dr. H. K. Groff seed collection located in the North Museum. This collection contains over 40,000 different species of seed varieties.

The basic method for identifying a seed was to view it under a binocular microscope, place it into its respective family, and then turn to the Groff collection and to Martin and Barkley's Seed Identification Manual (1961) to isolate the genus and sometimes the species. Finding the genus and species of many of the seeds was a difficult process because each seed's unique characteristics or landmarks were often lost over the years from weathering and erosion. I am indebted to Dr. Richard Craig of the Pennsylvania State University, Dr. Shively of Franklin and Marshall College, emeritus professor of Biology, and Roger Moeller for their assistance in making some of the seed identifications.

Owing to the limited time available, only the seeds from two surface levels and those from one deeper level were identified. The amount of material which we obtained from the Haverstick site was so immense that an entire year's work could not sort the tremendous quantity of bone, shell, and seed material from the floated samples. Nevertheless, I was left with a large and apparently representative sample of seeds to identify. For example, I identified a total of 2207 seeds, and yet, I would estimate that this represents only 20% of the total number of seeds we obtained from the site.

Results and Interpretation

The results of my seed identifications can be seen in Table 1. Listed on the left of the table are the genus and species of each seed variety. To the right are the respective amounts of each variety noted according to its proper level and square.

The two questions which this study seeks to answer are: 1--of the seeds identified, which can safely be said to be from the time of the aboriginal occupation? And, 2--of these seeds, which were used by the Indians and how were they used?

Two considerations must be taken into account before answering the first question. First, the environment has not changed much within the last 400 years. Most of the plant life appearing at the site now was there at the time of the Indian village. Some changes may have occurred by the introduction of new plant species from Europe and as a result of the perfection and cultivation of a particular species, and also because of the disruption of the aboriginal environment by clearing and plowing. The second consideration is that the seeds themselves were recovered by flotation from specific squares and levels of the site. Although the seeds may have been deposited by rain, animals, or other natural conditions, they were associated with diagnostic material identified as coming from the Washington Boro Phase (Washington Boro Incised pottery and small light-blue glass beads, etc.).

However, I have devised a method which attempts to establish further the relative antiquity of the seed variety, and which should give an accurate indication of the plant life used by the Indians.

This procedure involves five tests, ordered according to that test considered most significant to that test considered least significant. They are the following: 1--seed variety charred, 2--amount of seeds per variety, 3--depth of seed variety, 4--seed location, and 5--range of seed variety.

Using the procedure outlined, Table 2 gives a listing of the seed varieties and the tests they pass.

From Table 2, it can most accurately be inferred that the plant material from Robinia Pseudo-Acacia (black locust tree) and Galium (bedstraw) was used and exploited by the Indians. Of course, one could hardly contest the cultivation and use of Zea Mays (Indian corn) at this time. Furthermore, Phytolacca americana (pokeberry) and Rubus occidentalis (raspberry) were very likely collected by the Indians. Finally, Ampelopsis arborea (grapevine), due mainly to the charred quality of the seeds, was possibly used for food.

Although it is difficult to judge the specific use the Susquehannocks may have had for a variety of plant material, one can make some educated quesses. Robinia Pseudo-Acacia, (black locust tree) and Galium, (bedstraw) were undoubtedly exploited for industrial purposes. Black locust is an excellent wood and was probably employed by the Indians in the construction of long houses, stockades, tools, and for firewood. Bedstraw, due to the great quantity of charred seeds found, was, on the other hand, probably used to line the floors of the long houses or to provide bedding. Raspberries, Rubus occidentalis, and pokeberries, Phytolacca americana, were probably collected by the Indians to provide a supplement to their main diet of venison and Indian corn, Zea Mays. The root and seeds of the pokeberry are poisonous, but the young shoots may have been cooked and eaten.

Summary

Considering the large number of plant species exploited by Indians this is not a very extensive list, but at least, it is a start. I would suggest that much of the plant life represented, especially by the charred seeds, was put to some use. In many cases, this use is not known, but those bearing fruit (Amaranthus hybridus, wild beet; Potamogeton, pondweed) could have been eaten, and those in the herb category (Asclepias, milkweed; Lespedeza cuneata, bush clover; Tragia urens) may have been used for medicinal purposes.

Much more work needs to be done in the area of seed identification and analysis of the use of plant material at archeological sites. Work with the material from the Haverstick site is not completed. Eighty persent remains unsorted and particularly seeds from the lower levels should be identified in order to gain a picture of the seed distribution within the site. Moreover, the germinating qualties of the seeds should be tested, and comparisons should be made with work from other sites to discover how Indians in different areas utilized different local resources. Also, we need to know how use of plants evolved in a particular region. Hopefully, this project is a beginning for the Lower Susquehanna Valley.

Table 1

Amount Of Seed Variety Per Level And Square

Latin Name (Genus & Species)	N10E10, 49-67	N15E5, 32-60	N10E10, 87-93	Total Amount
Robinia Pseudo-Acacia	23	30	1	54
Ampelopsis arborea	3	0	0	3
Phytolacca americana	21	39	0	60
Rubus occidentalis	14	8	0	22
Rumex crispus	77	132	0	209
Rubus allegheniensis	3	0	0	3
Galium	1076	743	1	1820
Asclepias	2	1	0	3
Lespedeza cuneata	2	0	0	2
Setaria	3	2	0	5
Potamogeton	1	0	0	1
Tragia urens	1	1	0	2
Polygonum pensylvanicum	1	0	0	1
Polygonum persicaria	1	2	0	3
Cruciferae barbarea	2	0	0	2
Acalypha	2	0	0	2
Zea Mays	10	0	0	10
Prunus	1	0	0	1
Hemizonia congesta	1	0	0	1
Amaranthus hybridus	0	2	0	2
Toxicodendrum radicens	0	1	0	1
TOTAL	1244	961	2	2207

Table 2

Five Tests To Determine If The Plantlife Represented By Each Seed Variety Was Put To Use By The Susquehannock Indians

Latin Name (Genus & Species)	charred	amount	depth	location	range
Robinia Pseudo-Acacia	x	x	x	x	x
Ampelopsis arborea	x				
Phytolacca americana	x	x			x
Rubus occidentalis	x	x		x	x
Rumex crispus		x		x	x(E)
Rubus allegheniensis				x	x
Galium	x	x	x	x	x
Asclepias				x	x
Lespedeza cuneata					x
Setaria				x	x
Potamogeton				x	x
Tragia urens	x				
Polygonum pensylvanicum				x	x
Polygonum persicaria				x	x
Cruciferae barbarea	x			x	x(E)
Acalypha				x	x
Zea Mays	x	x		x	x
Prunus				x	x
Hemizonia congesta					x
Amaranthus hybridus				x	x
Toxicodendrum radicens					x

Note: under location, an indicates that either I found the plant growing near the site or that Groff found the seed variety in the vicinity
under amount, anything greater than or equal to 10 and evenly distributed between N15E5,32-60 and N10E10,49-67, the two surface levels, is considered significant
under depth, location in N10E10, 87-93, the only below surface level of the three, is considered significant
under range, E = native of Europe

Discussion

J. McDermott: McDermott questioned if Ameringer's test of significance was statistically quantified.

C. Ameringer: No.

J. McDermott: McDermott noted that the lack of statistical quantification causes problems in the analysis.

C. Holzinger: Holzinger questioned the methodology of Ameringer's tests for aboriginal usage.

C. Ameringer: Ameringer noted that they were cumulative tests with carbonization the most important.

C. Holzinger: Holzinger expressed surprize that several of the characteristic food sources were missing.

J. McDermott: McDermott questioned Ameringer's sampling techniques.

C. Ameringer: Ameringer replied that the sample was random due to temporal limitations and the large amount of material makes it representative.

C. Holzinger: Holzinger questioned how naturally occurring seeds were separated from utilized seeds in the midden.

C. Ameringer: Ameringer noted again that carbonization was the most important test.

J. Gruber: Noted that Ameringer's paper should be considered as a probe and when considered as such is interesting and significant.

King: King questioned why pokeweed seeds were in the midden if only the young spring shoots were used.

PALEOECOLOGICAL MODELS: EFFICIENT EXPLOITATION AND EXPLANATION OF THE ARCHEOLOGICAL RECORD*

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Introduction and Statement of Purpose

Recently, two articles appeared (Butzer 1975; Ford 1974) which reviewed the concept of "the ecological view of culture" (Watson et.al. 1971:88-107) as it is used by American archeologists, and implied that we were using diluted forms of ecological systems theory to structure regional research designs. Butzer's (1975:106) critical self-appraisal suggests that our theoretical frameworks are haphazardly borrowed: "Theoretical statements on archaeological methodology and prehistoric research have only given lip-service to the integration and implementation of ecological concepts." Using these two articles as organizational devices and beginning with one basic premise, the purpose of this paper is to critically examine the scope of ecological research in Eastern North America and to suggest schemes for reorienting our approaches. My basic premise is that sites, assemblages, tool kits, and artifacts represent "coded information of great variety and capable of direct interpretation or misinterpretation by individuals both inside and outside the manufacturing culture" (Clarke 1968:120). As Clarke (1968:59) and Leach (1973) have suggested, prehistoric culture systems represent incompletely observable systems whose internal mechanisms, like the classic Black Box problems, are never fully available for inspection. Some may insist that there is nothing new about approaching a site or region from this perspective, that the concepts introduced here have always been used by archeologists. I would reply that, if there has been any use of the following concepts, it has been on a non-explicit, intuitive, and non-operationalized basis. At best, we have been taking advantage of only a minimum of the available information (see Taylor's 1967:93 point about potentialities of archeological data); at worst, we have drastically underestimated the nature of the information contained within the archeological record.

I would further insist, with apologies to Willey and Phillips, that archeology is explanation or it is nothing. On the other hand, we must not restrict ourselves completely to the new "mouthtalk" without prod-

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ucing viable results. This has happened all too often in the past and has resulted in Fowler's (1973:46) characterization of the New Archaeology as the "leisure of the theory class with conspicuous assumptions."

One of Schoenwetter's (1967:104) principles of cultural ecological research (statements of method) states that a rational basis for collection of cultural ecological information is provided through an understanding of either the cultural or natural variables critical to the relationship involved. Middle Atlantic archeologists in each region do not have access to a comprehension of either set of variables and thus must be careful to not oversimplify research in either domain.

Efficient Exploitation

All of us within the archeological community are aware of the present crises in American archeology, resulting from many centuries of urban growth and site vandalism. It should come as no shock that sites are noticeably dwindling in number; Thomas Jefferson complained about the detrimental effect of land leveling practices on archeological sites as early as 1788. While not wishing to evoke controversy, I would suggest that, following a shift from resource exploitation to resource conservation (Lipe 1974) we should still be able to undertake longterm problem-oriented research with an explicitly scientific design.

At this point, efficiency and scientific become synonymous since both would, at the very least, revolve around the use of statistical sampling procedures (Redman 1974), a method still used only rarely in the Middle Atlantic area. Before beginning a somewhat lengthy discussion of the relationship between paleoecology and regional sampling designs, a further note on site conservation should be added. Griffith's (1975) proposal indicates that collection techniques utilized for the recovery of floral and faunal products are a necessary step in research, even if the excavator has no interest in these remains. I would agree and also insist that such procedures border on ethical behavior. That is, once we agree that resource conservation is our aim, then maximized data preservation from all excavations becomes a necessary corollary. All archeologists are directly responsible to their colleagues to be sure that they collect whatever data that presents itself in a manner amenable to detailed analysis. Such collection procedures preserve portions of data for future scholars in the absence of the ability to conserve archeological remains.

This statement does not leave me open to the criticism that an archeologist can never collect all relevant data, since one's understanding of relevance is bounded by the state of our pseudo-science. Obviously, we cannot collect "farfels," which someone in 1980 will "prove" useful in demarcating the sex of pre-historic potters, since today we do not know what "farfels" are. On the other hand, to be guided by pre-determined research designs, hypotheses, and testable implications (Fritz and Plog 1970) may mean non-collection of irrelevant but recognizable data. It is to the latter situation that my comments and, I assume, Griffith's proposal, are directed.

Probably the one aspect of Middle Atlantic archeology that is most disturbing is the lack of application of sampling methods, especially since their use is justifiable from the point of view of finances and manpower resource conservation. Obviously, the statistical selection of only a portion of the data available is also significant for culture resource preservation and management. But how should the sampling procedure proceed so that one can be sure that a sample has been selected which will be representative of the variation present across the entire population or universe of sites in one's region? For instance, the lack of adequate sampling of plowzone units at Faucett suggests that Moeller's (1975) analysis would be significantly affected. The proper response is to implement a sampling procedure in which data are systematically gathered from portions of the universe, with each portion defined on the basis of being a recognizable segment of variation.

Redman (1973, 1974) has suggested that such segments be defined on the basis of "environmental zones," thereby viewing paleoecological research as a key step in the first stage of his multistage research design. I would agree since regional habitat diversity can be viewed as one of the primary regulators of the distribution of prehistoric population aggregates. I am not suggesting that there is a one-to-one causal (deterministic) correlation between habitat zones and types of settlement components (Chang 1972). However, the behavior of groups in any one region is most easily described on the basis of regional habitat diversity existent at that time. The definition of major and minor ecological communities (Odum 1971:140) must be a problem specific especially since there is little stability in the Middle Atlantic area since the early Holocene. Wendland and Bryson (1974:20) suggest that there has been significant worldwide climatic variation over the past 10,000 years with major discontinuities dated at 9300 B.P., 8490 B.P., 5060 B.P., 2760 B.P., 1680 B.P., and 850 B.P. Certainly such research forces us to reevaluate traditional assumptions about the nature of postglacial readaptation, including Guilday's (1967) supposition of relative resource stability over the past 9000 years. One must be skeptical of Griffith's (1975:5) assumption that the modern resource pattern is useful in analyzing a great deal of Delaware's prehistory.

At the same time, Moeller (1975) should be wary of his use of recent vegetation transects (Goldman 1975) to establish vegetation diversity models in the Upper Delaware Valley. Such models are at best useful for analyzing the distribution of Late Woodland populations, although it might be suggested that recent diversity at least partially represents secondary succession and man's disturbance (Anderson 1973:192), since little primary virgin forest remains. One procedure which could be utilized with some degree of adequacy is to test the goodness of fit between their distributions and those abstracted from early land surveying records before widespread clearing for agriculture and forestry purposes. Zawacki and Hausfater's (1969) study is a case in point. A detailed examination of a selected aspect of Northeastern prehistory should allow us to place sampling procedures, postglacial readaptation, habitat diversity, and ecological taxonomic units in a proper perspective.

Evidence for Early Archaic assemblages in the Middle Atlantic area and the Northeastern United States has been relatively sparse until recent years. This lack of material, traditionally correlated with low population density, was "explained" (Fitting 1968; Ritchie 1969:212-213; Turnbaugh 1973:62) on the basis of a correlation between postglacial readaptation and unfavorable ecological conditions (Ritchie and Funk 1973:37), summarized under the rubric of the low carrying capacity of boreal forests (Butzer 1971:145-154).

A brief examination of Butzer's (1971:150) data reveals some problems. First, his measure of carrying capacity (biomass) is based upon figures from a study by Bourliere (1963) on the ecology of large African mammals. Bourliere's (1963:50) figures on biomass of herbivores in boreal forests are based upon studies from Slovakia, considered not to be an adequate representative sample. Second, biomass figures do not adequately represent carrying capacity itself since small mammals, fish, some carnivore species, and floral resources are not included. In addition, as Butzer (1971:145) admits in his second edition, elk, moose, and caribou would have at least seasonally occupied portions of the "boreal forest zone."

This carrying capacity/population density hypothesis also suffers from a somewhat uncritical use of palynological sequences to comprehend the nature of postglacial readaptation. As Schoenwetter (1967:48) points out, pollen analysis was invented by Von Post as an independent means of testing a then current hypothesis of postglacial climatic variation. Though techniques have greatly improved over the past century, pollen analysis was not developed to investigate the complex spatial and temporal (including secondary succession) patterning of vegetation communities across a region, patterning which is the result of interaction between geomorphological, microclimatic, pedological, and cultural agencies (Anderson 1973:192). These problems are compounded by those specific to palynology itself, the over-representation of species such as hazel, pine, and birch; underrepresentation of beech, oak, elm, willow, maple, and chestnut; and differential pollen dispersion and filtration (Tauber 1967). Schoenwetter's (1967:49) warning should be sufficient:

> Paleoecological techniques [including palynology] do not, in fact, reconstruct natural environments. They are designed to allow recovery of very specific sorts of information about the environments of the past. These types of information may not be critical for evaluation of cultural ecological relationships (amendment mine).

Pollen analysis, like seriation, sacrifices examination of regional diversity in space of biological or cultural elements. Such strategies allow one to develop hypotheses about the full glacial and postglacial cycles of regional habitat change. Most popular is the conception of the latitudinal lowering of life zones including the Canadian and Transitional or Carolinian (Dillon 1956), an effect which Turnbaugh (1973:70-71) attempts to correlate with the Valders episode and Paleo-Indian adaptation. Turnbaugh (1973:64) is also impressed (he refers to it as causal!, Turnbaugh 1973:70) with a correlation between the range of Early Archaic peoples in

the West Branch of the Susquehanna and Dice's Carolinean biotic province. This biotic association based upon broad clusters of vegetation, flora, fauna, climate, soil, and physiography was not presented as a final classification (Dice 1943:3); in fact, Shelford's (1963) continental scheme is very different. The studies of Dice, Shelford, Braun, and others must be viewed as somewhat static taxonomic associations, with only Shelford's showing any real understanding of the dynamic nature of ecosystem development (Odum 1969; Wright 1974). In addition it has been suggested by Brown and Cleland (1968) that the traditional scheme of postglacial readaptation is far too simplified in terms of its zoned linear sequence from open spruce parkland through deciduous forests. They would suggest, following Bryson (1966) and Bryson and Wendland (1967), that a hodgepodge or mosaic pattern of vegetation zones and communities existed in the Upper Great Lakes area and perhaps further to the east. Such mosaic patterns would not only make our analysis of pollen sequences more difficult, it also suggests the need for more complex models of cultural adaptation and change. Shelford's (1954, 1963:89-114) study of the complex development of vegetation communities on floodplain and terrace deposits suggests a similar effect taking place within a smaller time range. It might be further added that such mosaic patterns would greatly increase the carrying capacity of any latitudinal zone, perhaps to the point of disturbing Butzer's (1971:151) taxonomic scheme. It is not being suggested that the hypothesis of boreal forest/low population density/minimal archeological recognition has been disproved. Certainly, it must be re-evaluated given somewhat more sophisticated understanding of basic paleoecological concepts by archeologists.

Obviously, the problem of the dependence of proper research sampling on an understanding of habitat diversity through time becomes exceedingly complex if not unmanageable from the point of view of solely using palynological data. But if we were to add an additional source of paleoecological data (geomorphological processes) to pollen sequences, sampling designs may become easier to construct. There was a time when it was fashionable to undertake spatial plots of the distribution of early point types and relate the resultant patterns to episodes of geochronology. The work of Mason (1958, 1959), Quimby (1958), Prufer and Baby (1963), and Ritchie (1957) are cases in point. More recently, the distributional aspects of Luchterhand's (1970) study of Early Archaic points in the lower Illinois Valley reveal an archeological abuse of geomorphological processes.

Luchterhand's (1970:33-40) distributional analysis revealed that the density of Early Archaic points if four times greater outside the Major Valley Bottomlands than within them (Luchterhand 1970:37). While he interprets this patterning of artifacts as being a valid indicator of cultural activity, he does suggest that alluviation and the lack of adequate samples from deeply buried deposits is an alternative explanation. This suggestion raises the possibility of the presence of deeply buried components in major river valleys in the Middle Atlantic and Northeastern areas, a restatement of Coe's (1964) most valuable contribution. These represent resources to be systematically exploited given proper understanding of the Holocene sequences of terrace formations. The recent discovery of the Shawnee-Minisink site, the Harry's Farm and Plenge sites, and other assemblages in the Delaware Valley, in comparison to Mason's (1959) study, are just one

regional indication of the need for (in the research design) systematic understanding of Holocene habitat diversity.

Luchterhand's arguments against the alluviation hypothesis have been echoed for many years in the Northeast and should be dealt with. Primarily, he cites the large quantities of Late Archaic as opposed to Early Archaic material and states that this is a valid indication of assemblage potential unless the rate of alluviation significantly changed prior to 2500 B.P. As was pointed out before, this could indeed be true if one accepts Wendland and Bryson's (1974) postulated Holocene climatic sequence. So as not to fall into the trap of oversimplistic models of correlation between continental climatic change, sea level rise and fall, and graded/non-graded stream regimes, let me explore the implications of Luchterhand's argument a bit further.

I am primarily concerned with an explanation of Holocene sequences of terrace formation in major river valleys such as the Delaware and Illinois. Here terrace formations, overbank deposition, and the formation of natural levees are controlled by factors primarily within and not exterior to the drainage system. In both the Illinois and Delaware Rivers, there are multiple sets of fluvial terraces and, in the Upper Delaware, glaciofluvial deposits such as kame terraces and outwash terraces. Some of these features were available for occupation and subsequent burial at differing times. Systematic studies of this variability, not accounted for by Luchterhand, would result in the valid sampling of deeply buried components in valley floor habitats. I would suggest that at least one crucial indicator which could be used would be the extent of the formation of the natural levee deposit (supposedly an indicator of fluvial stability), not only on the present-day floodplain, but also on higher terrace formations as well.

It should also be noted that natural levee deposits, or other features guaranteeing annual overbank deposits, are probably the one location within valley floors where a maximum potential for vertical separation of assemblages can be found. Kinsey's (1972, 1973) work at Faucett, Broyles' (1971) work at St. Albans, and Coe's (1964) work at Doerschuk have revealed this correlation. Such separation and isolation of living floors, first documented by Coe (1964) for Doerschuk but not Hardaway, facilitates the construction of local and regional sequences. The lack of exploration of such Early Archaic deposits elsewhere in the Northeast (Staten Island by Ritchie and Funk 1971 and the Upper Delaware by McNett and MacMillan 1974) has complicated our research. I am not suggesting that compacted profiles such as Harry's Farm, Shawnee Minisink, and the Staten Island complex cannot be analyzed into discrete settlement (not taxonomic) components. They can be, given proper strategies like site information models, but if we measure efficiency in terms of cost expenditure, fluvial separation and isolation is to be selected and desired.

In summary, American archeologists have traditionally been borrowers, to the extent that they have not developed a systematic science of material culture. Such tendencies towards borrowing are never exceedingly dangerous, unless they involve taking a method from outside one's specialty and applying it to one's area of interest:

> First, the technique, method, analysis may be misunderstood [carrying capacity/biomass] or not grasped in its entirety with all its constraints, boundary conditions, presuppositions [palynology], and implications [post-glacial readaptations and zone migration]. Second, by going outside of one's own area, one may be unaware of problems with, objections and limitations to [climax forests, static versus dynamic ecosystem stability] the analysis one is attempting to borrow from the other field (Morgan 1973:259; amendments mine).

In order to deal with archeological abuses of other natural sciences, theoretics, and concepts, it can be suggested that first and foremost we need to return rapidly to and embrace interdisciplinary rather than multidisciplinary team research (Butzer 1975, Schoenwetter 1967:102-104, 112). Second, as Schoenwetter (1967:106-124) discusses, archeologists should control the entire regional research project, thus placing a burden of expertise on members of the profession interested in ecological systems models. This burden suggests that a radical reorientation of the educational process be undertaken so that archeologists expand their theoretical base.

Efficient Explanation

Following the usage proposed in the preceding section, efficiency is here interpreted as the combined effects of two factors. First, efficient explanation does not employ oversimplified concepts to establish correlations between archeological and ecological assemblages (Wise's 1975 suggestion of a relationship between the appearance of ceramics and "Central-based Wandering" and shellfish exploitation is one example.). Second, even if these correlations do/did exist, they cannot be considered to be even low level or formal (functional) explanations. At best, these statements are high level description and thus rarely contribute to the development of archeology as a natural science. This lack of potential contribution to the discipline plus the loss of manpower, finances, and culture resources during the course of such research leads me to label such activities as non-efficient.

The purpose of this section is to more completely develop this argument and explore the related issues of structure and process (Miller 1965), adaptation, and final explanation. After dealing with these epistomological issues, an ontological discussion of settlement archeology is presented, resulting in the suggestion that archeological sites can be considered to be information systems.

There is no need to discuss the discipline's bias towards description and chronology since much has been written on that topic. However, I would like to suggest that there is a viable difference between chronology and history as an idiographic, as opposed to nomothetic, discipline. Chronology, or culture history (Watson 1973), is the temporal and spatial ordering of archeological deposits, while history is that plus the potential for the

explanation of the resultant orderings. This view of history recognizes recent development in the field (Trigger 1973) and is essentially analogous to the argument made by Taylor (1967:23-42) about historiography and anthropology. In fact, it was this potential for variation in approach (antiquarian, chronological, historiographical, anthropological) which lead Taylor (1967:41) to identify our discipline as no more than "a method and a set of specialized techniques for the gathering of cultural information", a horrifying statement if lifted out of its proper context.

Probably one of the most complex issues argued about in contemporary American archeology is the proper role of deductive explanation (see Clarke 1972; Levin 1973; Morgan 1973; Tuggle et. al.'s 1972 evaluations of the D-N strategy of Watson et. al. 1971; Watson et. al.'s 1974 rebuttal to Morgan 1973; Morgan's 1974 reaction to this rebuttal). I have decided to approach this issue by suggesting that deductive logic is the required basis to determine adequacy of proposed hypotheses, but it does not need to play a major role in the formulation of initial problem orientation. That is, the strength and usefulness of a deductive approach lies in its ability to serve as a generator of testable implications (Hill 1968), and not as a link between nomological laws (law-like statements) and initial hypothesis formulation. This further allows one to properly understand the role of induction and abduction in hypothesis formulation (Plog 1974:18-19). It also suggests that the logic of hypothesis testing is atemporal; therefore, the order of data collection, deduction of testable implications, and testing of these implications is irrelevant to the validity of the reasoning involved (LeBlanc 1973:200). There are practical differences in the order of data acquisition and analysis. These differences are best understood from the point of view of sampling theory and resource management.

Explanation itself must be contrasted with empirical generalizations. The latter may pass for laws, law-like statements, or explanations but actually represent inductively generated statements never adequately tested. Most often, these statements are revealed as high order descriptions of developmental sequences (Stuart 1971:29-30). Much of Steward's (1955) writings and those of Carneiro (1970) and McNett (1970, 1973) are cases in point. In addition, these issues are made even more complex by the unselective utilization of untested generalizations as principles to structure research.

Griffith's (1975) use of the "principle of maximization (mini-max behavior) to generate a model for testing in a "goodness of fit" situation obscures the fact that this principle is but an hypothesis which needs adequate testing (see Hill 1971 in comparison to Fritz and Plog 1970) before it could be used as a law-like statement from which testable hypotheses could be deduced. This is important since its use by Griffith could lead to a circular and non-ampliative situation of low level explanation. That is, suppose we did undertake a regional sampling scheme to test Griffith's subsistence-settlement model. Let us further suppose that the "implications" of the model (Griffith 1975: Fig. 5) perfectly correlates with the archeological situation. Is this any kind of explanation or simply a high level description of a static conception of structure and process? In fact, it is analogous to reductionism in which the

"explanation" of the entire subsistence-settlement system is undertaken in terms of properties of the parts (Stuart 1971:64). Some of us have apparently lost perspective of the fact that the systemic nature of archeological entities, as they represent objectified behavior (Taylor 1967:99), exhibit characteristics which are far more complex than the simple sum of the attributes of the components and are more predictable than that of individual components (Clarke 1968:132). Explanation, whether functional or final, is not the description of structure, the arrangement of the subsystems and components of a system in three dimensional space at a given moment in time (Miller 1965:209). Before contrasting structure with process and moving on to adaptation and functional explanation, a further point must be made about Griffith's model.

The situation described above as non-ampliative would result simply because the produced correlation would be interpreted as an explanation. Thus no adjustment of the original "principle" would be called for, leading to the confirming of this hypothesis as a universal law, even though it had never been adequately tested. This is especially crucial for minimax behavior since this economic hypothesis is far more complex than Griffith would have us believe as Hill (1971:56), indicates.

In contrast to structure, the concept of process is dynamic and stresses change in the matter-energy or information of a system over time (Miller 1965:209,211). It includes function (reversible actions succeeding each other from moment to moment) and history or historical processes, which alter both the structure and function of the system:

> So there is a circular relation among the three primary aspects of systems-structure changes momentarily with functioning, but when such change is so great that it is essentially irreversible, a historical process had occurred, giving rise to a new structure (Miller 1965:209).

Functional studies are common in anthropology and include some often cited as classics of cultural ecology (Steward 1955; Rappaport 1968; Leeds and Vayda 1965). What ties these together with the more organismic structural-functional explanation is the level of explanation produced: "The aim of functional explanation is to account for the behavior or operation of cultural items, and not for their presence or origins in a given culture (Collins 1964:2)." Stuart (1971:26) views these explanations as minimally describing the system-specific internal processes. There is not anything wrong with this type of explanation except that: 1) it is inadequate in explaining presence or origins of prehistoric behavior patterns: 2) it can be one-sided in its focus on utility, integration, and harmony (Flannery 1973:52); and 3) there can be no objective testing of hypotheses in functional analysis because of the lack of empirical referents for crucial terms, Hempel's (1965:297-330) criticism (Vayda 1968: ix).

Problem one (1) is easily handled by Collins' (1964, 1965) suggestion that functional explanations do not have to include notions of a trajectory. In fact, this is one of the crucial distinctions between functional (formal) and final explanations. Problems two and three have been adequately dealt

with through the injection of general systems theory into culture ecology (Stuart 1971). Rappaport's (1968) volume and Lee's (1972) analysis are classic examples of the gathering of empirical data needed to test these explanations.

In order to explore the archeological use of functional explanations, a contrastive study of three recent ecologically oriented reports will be summarized. All are concerned with temporal considerations of adaptative processes in the region of coastal New England (Bourque 1973; Braun 1974; Snow 1972). All three are characterized by several levels of integration beyond chronology. Each carefully describes a correlation between the subsistence base, settlement, pattern, and regional habitat. Each also introduces notions of functional explanation under the guise of adaptation and discusses the processual relationships between subsystems and components of the structure. Each also adds an historical element, but these temporal considerations do not allow us to refer to the studies as examples of final explanation, Collins' (1965:278) second component of explanation:

> Accordingly, a second component consisting of the general laws that are exemplified by the changes in variables and by the operation of mechanisms is necessary to complete the explanation. In other words, the system-specific laws must be shown to be derivable from general laws unlimited in scope of prediction given the conditions under which the system operates.

Thus final explanation may be recognized on the basis of three elements: 1) an adequate explanation of the presence or origins of prehistoric behavior; 2) the ability to view system-specific explanations as being derived from general laws of human behavior; and 3) an entailment of a patterning of variables and their logical relationships through time that create a situation in which, given the stated interactional rules, the phenomenon to be explained would logically result if the variables were assigned values (Meehan 1968:vi).

None of the three articles cited above are final explanation; in fact, I conceive of them as archeological analogs to Eggan's (1954) method of controlled comparison, organismic notions with temporal considerations. Before departing from these epistemological issues, two further points are in order. First, final explanations will require the use of a general systems theoretics (Stuart 1971) and simulation procedures (Doran 1970) to replace our old-fashioned, static ecological models. Such theoretics must be made more amenable to empirical referents but not so simplistic that meaningless studies such as Flannery's (1968) are produced. Second, adequate explanations of the origins of "adaptive" behavior must not only trace changes in behavior representing subsystem articulation but also identify those situations which would give positive selective value to modification of structure (Levins 1968:6).

Now that the epistemological issues have been clarified, it is time to explore the ontology of the archeological record, that is, how is the universe of materials that we deal with structured and how do we go about

exploring this structure. I return to the basic premise of this paper, that artifacts and their distributions represent "coded information of great variety and capable of direct interpretation or misinterpretation by individuals both inside and outside the manufacturing culture," (Clarke 1968:120).

It is being suggested that archeological sites are primarily information systems which have the capability of communicating data concerned with the interaction between behavioral patterns and transformational activities in a series of successive states progressing through a system trajectory. One's explanation of that data is bounded by one's understanding of this process of interaction as well as by data acquisition and analysis procedures (methods). Essential attributes are the site's information while inessential attributes are termed "noise," the result of n and c transforms (Schiffer and Rathje 1973) and the nature of the acquisition procedure and analytical methods (Clarke 1968:89). This information system or the site's successive states should not be confused with the information system in use by the inhabitants of any particular site. The latter views the site as an information system, some of whose essential and inessential attributes are behavioral patterns as they represent the occupant's culture. Thus a site's information system could be made up of a successive series of states, each state partially but not completely understandable through the analysis of that state's occupants' culture system. Examination of a site's trajectory must account for the modification of behavioral patterns by non-cultural mechanisms.

An archeological site is therefore a dynamic and continuous information system whose entities change by infinitesimal transformations (Clarke 1968:75). The output of this successive series of states is the data gathered by the archeologist. The input is composed of behavioral patterns while the internal mechanisms are the regulators and transformers.[1] Given the output, archeologists must seek to understand the input phenomenon and internal transformation activities, a Black Box problem at its worst. We must produce a series of alternative explanations, each of which entails a patterning of variables and their relationships, such as that given the stated interactional rules, the final state of the site's trajectory would logically result if the variables were assigned values (Meehan 1968:vi).

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[1]Transformers and regulators are processes operating in the past (transformers) and at the time of data acquisition and analysis which will modify the nature of the essential attributes and the relationships between them. One example would be an acquisition strategy which precluded recording the coordinates on "in situ" phenomena. Therefore one's understanding of a state or trajectory would be regulated by the acquisition unit size (grid unit). These units may be so large as to obscure what were behavioral patterns. Whallon (1973) has addressed himself to this kind of regulator in the spatial analysis of occupation floors.

It is my contention that American archeologists have seldom explicitly discussed individual sites as complex information systems. Intuitively, they may juggle a series of elements, the results of behavior patterns, which they see as being systematically related and eventually arrive at a "type" designation for their phenomenon, e.g. quarry sites, base camp, limited activity station, and specialized procurement focus. Stratified sites are rarely viewed as systems of successive states within trajectories.

It is suggested that the basic unit of site information analysis and interpretation is Chang's (1972:9) notion of a "minimal node for articulation." Such depositional units are of continuous horizontal and vertical expanse in a stationary state, one in which changes occur within bonds of constancy and without upsetting the overall alignment of cultural elements (Chang 1967:33).

Examples of minimal nodes for articulation include isolated (horizontally discrete) features (granary, storage pit, burial, hearth, workshop, overnight camp of short duration), deposits of rubbish or other occupational debris in a continuous area during which no significant changes occured, and continuous areas with all or any combination of the above (Chang 1972:9).

After the determination of which minimal nodes are contemporaneous, the archeologist constructs components of settlement patterns. Difficulties arise on two levels. First, how is it that we recognize minimal nodes in the absence of "discreteness"? Second, how does one articulate contemporaneous nodes? Archeologists are constantly struggling with time dimensions, temporal relationships between pairs or sets of nodes. For such a fundamental problem, there is a surprising lack of discussion (Taylor 1972 cites a similar lack of concern for context). It is crucial since component construction and interpretation is predicated on an understanding of nodes. For instance, is it possible for two nodes to be excavated which are identical but the result of entirely different activities and social or task units? An example will suffice:

> ...a pile of industrial waste accumulated by ten flint knappers in one day and a pile accumulated by one flint kanpper in ten days would be very difficult, if not impossible, to distinguish, but the distinction is one of some qualitative importance for the characterization of a settlement (Chang 1972:10).

Detailed analysis of such piles may help the resolution of our problem. The point is that such analysis needs to be applied, especially in situations in which there are not sedimentological or pedological features to define "occupation floors." For example, if the Bushkill complex (Kinsey 1973:73-98, 234-245) represents a taxonomic amalgamation of numerous settlement components and/or nodes or articulation, then density ratios applied to these units may not reveal significant patterns of intensiveness and extensiveness. Intuitively, I suspect that there is a very little change between the settlement densities of Early Woodland and Middle Woodland components. A simulated example will help.

Table 3: Simulated Example of Differential Settlement Component Densities

Settlement Component	Grand Lithic Total/ Number of Square Feet	Density Ratio One item per x Square Feet
A. Orient Comp. (Kinsey 1973:112)	1035/4500	1:4.3 sq. feet
B. Rossville Comp.	1156/16,100	1:13.9 sq. feet
C. Bushkill Comp. (Kinsey 1973:84)	3467/16,000	1:4.5 sq. feet
D. Rossville Comp.	230/300	1:1.03 sq. feet

Suppose that one wanted to calculate the densities of settlement components for Early Woodland and Middle Woodland settlements at Faucett. Let us further assume that we are sometimes able to recognize contemporaneous nodes and their articulations. The comparison of A and C in Table 3 is identical to that depicted by Kinsey (1973:84, 112). Obviously, in the absence of further analysis, both are assumed to be intensive components, perhaps the result of long term habitation activities by relatively large groups. On the other hand, let us compare A and B, in which we have isolated, within the Bushkill complex, a Rossville settlement component. Obviously, our interpretive framework must shift. Again, suppose our entity B is made up of a number of temporally distinct groups of nodes. We manage to isolate one such group (D). Comparisons of its ratio to all others reveal a series of alternative hypotheses for the differences revealed. Kinsey's use of these ratios with little explicit attempt at subdivided analysis is not unique. Fitting's (1969) review of Upper Great Lakes data and Winters' (1969) monograph on Riverton use similar notions. It is not being suggested that these interpretations are wrong, however, without proper analysis and interpretation within a site information model, these studies are considering only a minimum of the wide range of explanatory hypotheses available for consideration. This tactic characterizes much of recent intrasite analysis including the publication on Hatchery West (Binford et. al. 1970).

Once one has accepted the validity of the need for detailed intra-site analysis, the recognition of minimal nodes of articulation remains. Only detailed artifact morphological and functional, distributional, and associational analyses will help. Each of these must be applied to a subset of the data (postmold, fire-cracked rock, lithics, ceramics) available with a wide range of hypotheses and deduced implications checked and cross-checked. The procedure is at best laborious. It may not be that such studies will replace deductive-nomological or general systems approaches, but they do make a contribution to the halting process of development of a science of material culture.

Discussion

C. Holzinger: Handsman, when understandable, is always reasonable, however, the opaque systems theory nomenclature makes him obscure and at times seemingly unreasonable. Holzinger stressed that it is imperative to avoid unnecessary complications.

R. Handsman: Reply: The language of systems theory must be learned and to translate "mouth talk" would be an unnatural simplification.

J. McDermott: Handsman's paper is a reasonable proposal for the use of a multi-disciplinary approach which has been effectively applied in biology.

R. Handsman: Reply: He again pointed out the dangers of over-simplification.

M. Thurman: He agrees with Handsman up to a point, but noted that Edmund Leach's tendency to equate epistemology with theory creates problems. Two individuals with a similar epistemology may have radically different theories. It is possible to be a "new archaeologist" and still not embrace Louis Binford's epistemology.

L. Brennen: Enjoyed Handsman's paper and suggested that there was no generation gap. Brennan questioned whether Handsman's methods could be applied to field situations.

W. Gardner: Replied to Brennan: The work at the Thunderbird site proves that Handsman's methods can be applied. Gardner noted that you do not know what will work until you try and it is not really important whether or not people agree with Handsman until they put his theories into practice on their own excavations.

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