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The Conference on Historic Site Archaeology Papers 1972 - Volume 7

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The Conference on Historic Site
Archaeology Papers 1972

Volume 7
THE CONFERENCE ON HISTORIC SITE ARCHAEOLOGY PAPERS

1972

Volume 7

Part 1 - PRESENTED PAPERS at the Thirteenth Annual Conference
Morgantown, West Virginia

Part 2 - THE HISTORICAL ARCHAEOLOGY FORUM
Archaeology of Black Settlements

Part 3 - CONTRIBUTED PAPERS
Historical Archaeology: Method and Theory
A Debate on Ethnohistory

Part 4 - THE JOHN M. GOGGIN AWARD PAPERS

Stanley South, Editor

Additional copies of this volume are available for $ per copy.

Stanley South, Chairman
The Conference on Historic Site Archaeology
The Institute of Archeology and Anthropology
University of South Carolina
Columbia, South Carolina 29208

August, 1974
PREFACE

THE CHAIRMAN'S REPORT

The Thirteenth Annual Conference on Historic Site Archaeology was held at the Holiday Inn in Morgantown, West Virginia on October 12, 1972. This Volume 7 of The Conference on Historic Site Archaeology Papers includes some of the papers presented at that conference as Part I. Part 2 contains three papers on the archeology of Black settlements, which is the theme of this year's HISTORICAL ARCHAEOLOGY FORUM.

In addition to the above usual parts to these volumes, this volume introduces two new parts which should result in a far greater flexibility on the part of the editor in compiling these volumes. The new Part 3 of this volume is a CONTRIBUTED PAPERS section, which contains papers not presented at the annual conference, but which the editor includes from among those submitted papers from conference members. This section in this volume includes some papers in method and theory in historical archeology, and a lively debate on ethnohistory.

This volume also introduces the papers submitted for competition for the JOHN M. GOGGIN AWARD FOR METHOD AND THEORY IN HISTORICAL ARCHAEOLOGY, with the winning paper and the other submitted papers being Part 4 of the volume.

This volume has been delayed in publication, and it is hoped that this situation will not be repeated. The primary reason this chairman has maintained sole editorial responsibility for these papers is to theoretically insure that the volumes continue to appear in a regular manner. It is hoped also that Volume 8 will soon appear, which should close the present gap in publication scheduling.

I would like to thank those who participated in the forum and contributed papers sections of this volume by submitting papers for publication. Thanks is again due to those at the Institute of Archeology and Anthropology at the University of South Carolina who assisted with the preparation of this volume. I would like to thank the Director of the Institute, Robert L. Stephenson for his continued support of the Conference. I would also like to thank MaryJane Rhett for her vital role in handling the budget for the Conference, as well as the sale of memberships.

The Conference is now affiliated with the University of South Carolina, and the financial affairs are handled through the University auditing and bookkeeping system. In order to insure that the Conference has perpetuity it has been formally aligned with the office of the Director of the Institute. As of December 1, 1972 the officers of the Conference on Historic Site Archaeology are Stanley South, Active Chairman, Robert L. Stephenson, Co-Chairman, Executive Secretary, MaryJane Rhett, Board of Directors Members, John D. Combes and Leland Ferguson.

Stanley South, Chairman
The Conference on Historic Site Archaeology
INTRODUCTION TO THIS VOLUME

The original concept of the Conference on Historic Site Archaeology was to provide a vehicle for the presentation and publication of papers on historical archaeology. In the early years the emphasis was necessarily on classification and description of archeological materials, with the chairman urging synthesis of the analyses being conducted and presented in the form of Conference papers.

In recent years there has been an increased concentration on method and theory reflected in the Conference papers, giving these volumes a unique character not seen in other historical archaeology volumes. There will always be a need for the basic groundwork provided by studies concentrating on descriptive analysis, but there is now emerging within historical archeology a greater concern for its primary function, the integrative explication of patterned material remains of culture stemming from human occupation.

In the greater flexibility of recent volumes provided by the HISTORICAL ARCHAEOLOGY FORUM section, and in this volume through the introduction of CONTRIBUTED PAPERS and the JOHN M. GOGGIN AWARD PAPERS, a continued emphasis is being placed on synthesis and integration rather than on description and analysis, by those submitting papers for publication. This reflects the dual direction that historical archaeology is taking in solving the problems with which it is faced. On the one hand there are those historical archaeologists who are concentrating on the demonstration of the archeological parallels for documented historical data, while others are focusing their efforts on defining archeologically revealed patterning in the material remains of culture toward the explication of progenital cultural patterns stemming from human occupation. Regardless of which orientation the historical archaeologist takes in the handling of his data, he must utilize modern method and theory if he is to move with the current of the twentieth century toward archaeological science.

Stanley South, Chairman
The Conference on Historic Site Archaeology
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Part 1

PRESENTED PAPERS

Introduction

This part was the original one from which the present Conference on Historic Site Archaeology Papers volumes grew. The original concept was to publish only those papers presented at the Conferences, but then as those who presented papers more frequently failed to turn in these papers to be published, and others asked to have their papers published, the present format of the volume emerged. As the format evolved papers that were presented were transferred to other parts of the volume for thematic consistency, or for inclusion in the HISTORICAL ARCHAEOLOGY FORUM, etc., so this part in the present volume is represented by a single paper only. However, this is not seen as a detriment since the Conference still functions as a stimulus for the exchange of ideas, and the flexibility of the present format provides for the maximum presentation of viewpoints centering around the process of historical archaeology, and the data derived from it.

In this volume, for instance, the paper by Schuyler, several by South, and the paper by John Combes were originally presented at Conferences, but have been taken out of the PRESENTED PAPERS part and placed in another for better presentation from a thematic point of view. Hopefully this clustering of types of papers will assist the reader in locating groups of papers with similar topical information.
AN ANALYSIS OF A CACHE OF GUNFLINTS
FROM A SITE IN NEW ORLEANS: GALLIER HOUSE
(16 OR 46)

Jack C. Hudson
and
Kay Goelzer Hudson

The Gallier House, located at 1132 Royal Street in the Vieux
Carre district of New Orleans, was the focus of an extensive restoration
project starting in August, 1970, and continuing through most of 1971.
In October, 1970, archaeological investigation of the site was instituted
to recover as much data as possible from construction excavations, test
squares in selected areas, and excavations to record structural details
of wall footings and foundations of earlier buildings on the site.

Gallier House was the home of James Gallier, Jr., a prominent New
Orleans architect. The house was built in 1857, on land which had
originally been part of the Ursuline Convent grant laid out in about
1722. In 1825 the Convent sold the entire square of which the Gallier
property was a part for use as building lots, and a rapid buildup of
homes and commercial establishments followed.

Although the property has an early eighteenth century heritage
as part of the Convent grounds, only one sherd (from a blue on white
faience plate) from this early period was recovered. Due to the
evolution of this particular block, on which the Gallier House was
one of the last to be built, the lot received most of the material
which was later recovered there during a period from about 1830 until
the house was built in 1857. After this time the property was closed
in by the house and garden walls. Any materials deposited after 1857
would be largely restricted to the cistern and woodshed areas, and to
a much lesser extent to the garden and planter, since the rest of the
yard was under a flagstone walk and a grassy plot with a fountain.

During the monitoring of the spoil pile from a plumbers' excava-
tion in the vicinity of a flagstone walk in front of the kitchen door,
a number of honey-colored gunflints were recovered. When a walk was
removed for rebedding, a small square was put in near the door, and
thus the remainder of the flints were found beneath the walk, in the
upper four inches of the square. A total of 124 gunflints were re-
covered, in association with sherds of blue transferware, banded ware,
one small Bordeaux bottle, and various glass fragments.

Such a large sample from such a small area presented an interesting
opportunity for analysis, and the flints were typed according to the
criteria used by Lyle Stone (1971) and compared with his much larger
sample from Fort Michillimackinac. This sample is from a site which
was occupied by the French from 1715 to 1761, and by the British from
1761 to 1781. Over a period of several years (1959 to 1966) 2536 gun-
flints were recovered, including 348 prismatic flints, 2183 gunspalls,
and 4 blade spall flints (Stone 1971:9).

Stone has divided the sample into Series (defined by techniques of manufacture), Types (based on shape in cross section), and Varieties (based on color of material). For purposes of measurement, length is defined as the axis parallel to the gun when the flint is in place.

Series A consists of blade (prismatic) gunflints, all made of French beeswax chalcedony. Type 1 is defined by three transverse flake scars, with the central or face scar parallel to the bed. Type 2 has no back flake, showing only two transverse scars on the face, with one of these parallel to the bed. Type 3 also has two transverse flakes, but neither is parallel to the bed, and the flints are triangular in cross section. Within Series A, Stone assigned 50 specimens to Type 1, 94 to Type 2, and 66 to Type 3 (Stone 1971:11-13). (For counts and measurement ranges, see Fig. 1 below).

Stone calls Series B specimens blade-spall gunflints. He found only four examples of this type. They have only two flakes, both tapering to the edges and lacking the usual gnawing on the back edge. These flints are dark grey in color, and Stone suggests that they may represent the salvageable by-products of spall making (1971:16).

By far the largest proportion of Stone's sample (86.08%) are Series C, spall gunflints. These were made by the removal of individual spalls from a flint or chert nodule, and generally have a convex face. All the specimens in the series are grouped into Type 1, defined as wedge-shaped, and divided into varieties by color. Variety a includes grey to brown specimens, totalling 2032. Variety b included 139 dark grey to black flints, and Variety c includes only 4 brownish red specimens (Stone 1970:17-19).

The vast majority of the Gallier House gunflints (88%) are blade flints of French manufacture, almost exactly the reverse of the Michilimackinac proportion. There were no English gunflints in the Michilimackinac sample, and there is only one from Gallier House. This is a rectangular black flint, 23 mm. long by 19 mm. wide. One edge shows some signs of use, although very few of the French flints in the sample appear to have been used at all. There is one other anomalous flint in the sample, a small white specimen. It is made in the French style (Series A, Type 1) and measures 18 mm. long by 14 mm. wide. It appears to be made entirely of the white discoloration or cortical material which appears in small amounts in almost all of the honey-colored flints.

Series B, blade-spalls, accounts for only two gunflints. Both of these are made on beeswax chalcedony, and one has considerable white discoloration. Neither shows the gnawing typical of French flints.

Another very small proportion of the Gallier House sample (3.2%)
<table>
<thead>
<tr>
<th>SERIES A</th>
<th>210</th>
<th>109</th>
</tr>
</thead>
<tbody>
<tr>
<td>Type 1</td>
<td>50</td>
<td>20 large</td>
</tr>
<tr>
<td>length</td>
<td>18.3 - 26.1</td>
<td>17 - 32</td>
</tr>
<tr>
<td>average</td>
<td>22.85</td>
<td>27.3</td>
</tr>
<tr>
<td>width</td>
<td>18.6 - 32.0</td>
<td>20 - 27</td>
</tr>
<tr>
<td>average</td>
<td>27.12</td>
<td>24.7</td>
</tr>
<tr>
<td>thickness</td>
<td>3.9 - 8.8</td>
<td>6 - 10</td>
</tr>
</tbody>
</table>

| Type 2   | 94  | 26 large |
| length   | 16.4 - 31.8 | 20 - 34 |
| average  | 23.54 | 29.5 |
| width    | 16.0 - 36.3 | 22 - 30 |
| average  | 27.01 | 26.0 |
| thickness| 3.9 - 11.8 | 4 - 11 |

| Type 3   | 66  | 1 large |
| length   | 18.0 - 27.1 | 24 |
| average  | 21.95 | 24 |
| width    | 21.3 - 32.8 | 24 |
| average  | 27.56 | 24 |
| thickness| 5.0 - 11.1 | 6 |

<table>
<thead>
<tr>
<th>SERIES B</th>
<th>4</th>
<th>2</th>
</tr>
</thead>
</table>

| SERIES C | 2032 (a) | 139 (b) | 8 large |
| length   | 15.9 - 38.3 | 21.2 - 32.5 | 25 - 32 |
| average  | 25.11 | 27.68 | 27.9 |
| width    | 18.3 - 36.4 | 24.5 - 36.2 | 25 - 30 |
| average  | 27.85 | 19.28 | 27.4 |
| thickness| 4.0 - 10.4 | 5.0 - 11.9 | 7 - 11 |

FIGURE 1
can be placed in Stone's Series A, Type 3. Three of these are quite small. A larger and rather more crudely made example might have been placed in Series B but for the obvious gnawing on the back edge. All four of these flints are triangular in cross-section and are honey-colored.

Series C, spall gunflints, is represented in this sample by 11 flints, 8 large and 3 small. The 8 large flints are all made on grey to brown material and have convex faces with a noticeable bulb of percussion. The three smaller flints are quite different, all are made on honey-colored French material. More importantly, their faces are quite definitely concave. Very possibly these three flints represent a later type which was not present at all at Fort Michilimackinac.

The largest portion of the sample from Gallier House (84.8%) falls into Stone's Series A, Types 1 and 2. In these two types there appears to be a definite division into at least two size ranges, a division which was not noticeable in the Michilimackinac sample. Series A, Type 1, includes 20 large and 11 small specimens, all are beeswax chalcedony and have scattered white inclusions. The smaller ones appear lighter in color, due to their delicacy and relative thinness. Series A, Type 2, shows a similar breakdown into large and small categories. There are 26 large and 48 small specimens, all honey-colored French material and most showing scattered white inclusions.

Scattergram representations (Figs. 2 and 3) of Series A, Types 1 and 2, specimens from Gallier House show a neat clustering of large flints at one end of the scale and small ones at the other. There is a less well defined group at what might be called the small end of the large group. There may be another size lurking in here, but there is as yet no definite evidence of a break with the large group, and no visual impression of a third size appears in an inspection of the flints themselves.

More than half of the flints (53.2%) in the Gallier House sample fall into the small size group. However, flints of this size are rarely, if ever, mentioned in the literature. Peterson (1956:228) shows actual size illustrations of the nine traditional standard gunflint sizes, as produced by a modern English knapper. These flints range up to 47 mm. long by 39 mm. wide (for cannon), but the smallest size, for pocket pistols, is 21 mm. long by 18 mm. wide. Only one of the small gunflints from this sample is that large.

The gunflints in Stone's sample were consistently wider than their length. About 80% of the Gallier House flints, however, are longer than their width, and on more than half of the rest width and length are equal.

The proportion of Series A and Series C gunflints at the two sites were almost exactly reversed, while Series B was negligible at both. The Michilimackinac sample contained 86.08% spall gunflints.
Figure 2

LENGTH-MM:

WIDTH-MM:

TYPE AI
Figure 3
and 13.72% blade flints, while the Gallier House sample included 88.0% blade flints and 8.9% spalls. Within Series A (blade flints), the Michilimackinac sample consisted of 23.9% Type 1, 44.8% Type 2, and 31.4% Type 3, while the Gallier House sample included 28.4% Type 1, 67.7% Type 2, and only 3.7% Type 3.

One other gunflint sample which might be compared with the Gallier House collection is that from Old Fort Pierce, Florida, a Second Seminole War Site. These would represent military flints from the 1830's. The sample consists of 110 flints, 95 of which would apparently fall into the Series A (large size) group. They are not classified by technique or manufacture. Most were made from French material, and ranged from 25 to 34 mm. in width, 4 to 12 mm. in thickness, and averaged 24 mm. in length. Unlike the Gallier House flints, their width is greater than their length. The other 15 flints in the sample are grey to black, range from 20 to 25 mm. in width, and some are slightly longer than their width (Clausen 1970:14). This collection is interesting because its size range appears to fall at the smaller end of the large size group from Gallier House, strengthening suspicions that there may be another discernable size range between what we have called small and large.

Taken as a whole, the flints from the Gallier House can be easily and effectively classified under Stone's system. The major differences between the samples, that is, Series proportions and size categories, suggest a chronological, rather than a typological divergence, as discussed below.

During the course of background research for the Gallier House Site, we discovered that during the period from 1830 to 1850 most of the gun dealers in New Orleans were French. Toward the latter part of the period, several English gunsmiths appeared in the city directory. The gun dealers supplied the citizens of New Orleans with a variety of fowling pieces and small pocket pistols, and apparently the most popular were flintlocks of British manufacture. If our sample is representative of the period, however, French flints were still preferred over English (Goldstein 1972).

The high proportion of blade gunflints and the large number of very small flints both suggest that the Gallier House sample is considerably later than the eighteenth century sample from Michilimackinac, much as we might expect from the respective histories of the two sites. Part of the difference may also be related to the status of Fort Michilimackinac as a frontier post and that of New Orleans as an early urban center.

A problem in any late dating of the Gallier House sample is the almost complete lack of English gunflints, said to be predominant in America during the nineteenth century. The people of New Orleans, while they traded heavily with England, always preferred French wines and many other French products, many of which could be easily obtained
Figure 4
from English trading companies. They may, for reasons of their own, have preferred and specifically ordered French gunflints. New Orleans was trading directly with Europe, and was not dependent on western traders who carried only English flints.

We feel that the history of the site and the artifactual evidence obtained during the archaeological investigations at Gallier House place the deposition of these gunflints between 1830 and 1857. Our impression is that these flints represent the use of civilian sporting and personal defense weapons rather than military arms. Hopefully we will be able in the future to make comparisons between our sample and those from other early nineteenth century urban centers.

ACKNOWLEDGEMENTS

The authors wish to acknowledge with gratitude the help of the Gallier House Museum, which funded the archaeological investigations at the Gallier House Site, made available the material for study, and encouraged our research.
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PART 2

HISTORICAL ARCHAEOLOGY FORUM

On the Theme of Archeology of Black Settlements

INTRODUCTION

This forum centered on the theme of archeology of Black settlements instead of concentrating on one paper alone, as was the format of the previous fora. These papers by Robert Schuyler, John Combes, and Charles Fairbanks all address themselves to examining a particular aspect of Black history and culture through historical archeology. They are among the few to have dealt with this area, and reflect a growing interest in Black history research throughout the field of historical archaeology as questions are being asked that only archeology can answer.
Staten Island, now one of the five boroughs of New York City, legally became a political part of greater New York only in 1896. This late incorporation is seen today in the fact that the Island still contains much open land and maintains certain of its rural aspects. Such a situation is the product of a long standing paradoxical relationship. Since the Dutch Period the Island has been relatively marginal and isolated and yet Staten Islanders have always been intimately tied into the urban center on Manhattan. In the Revolutionary Period the Island was linked to New York as the last enclave of British power in her colonies, while by 1840 the urbanization of Manhattan and its areal influence is reflected in the Federal Census listing more of Staten Island's population as being involved in specialized vocations and industries than in agriculture. In fact, the oyster industry alone was developed to a point that saw it, according to a contemporary scholar (Akerly 1843), leading to a "neglect" of farming on the Island.

Archaeologically this relationship has produced a range of sites on Staten Island, dating from the eighteenth and nineteenth centuries, that includes the expected farm house but also factories and other specialized remains. One such specialized community, Sandy Ground, in part the product of the nineteenth century oyster industry, has been investigated during the last two summers by the City College of the City University of New York (CCNY) Archaeological Field School.

Scattered along Bloomingdale and adjacent roads there exists today a number of houses centered around the African Methodist Episcopal (AME) Zion Church. These structures are the remains of a settlement (Fig. 1) that has existed for almost a century and a half and is one of the longest continuously inhabited Black communities in New York.

Located in southwest Staten Island this community occupies a sandy soil zone that is in marked contrast to the rich loams and clays of most of the island. Although some horticulture is possible, especially in the past the growing of strawberries (Wilkins 1943b: 31), the area has low agricultural potential but is ideally located for easy access to marine resources either west on the Arthur Kill or east at Prince's Bay (Fig. 2a) on Lower New York Harbor. Prehistoric middens throughout the New York harbor area amply attest to the former extent of native shell beds. By 1715, however, colonial over exploitation of these indigenous beds had set off a process of decline that by the end of the eighteenth century resulted in exhaustion. Planned transplanting of Chesapeake
FIGURE 1
AERIAL VIEW OF SANDY GROUND TAKEN IN JUNE 1971 LOOKING WEST

View shows the heart of the community transversed by Bloomingdale Road running north-south. The AME Zion Church can be seen in the left hand corner of the scene while above it, surrounded by an oval driveway, a new residence occupies the original church site. Adjacent to this house is the Sandy Ground Cemetery. Public School 31 is also visible (with flag pole) off Bloomingdale, and between it and other houses on the road is an isolated patch of woods along Bloomingdale (center of photograph) which is the Purnell property, the location of Structures A, B, and C and Feature 5.

Much of the wooded area interspaced between various structures was produced by the great fire of 1963. The wide, barren swath in the background is the route of the Staten Island West Shore Expressway, now under construction, which has also some outlying sections of the community.
FIGURE 2

FIGURE 2a: AERIAL VIEW OF PRINCE'S BAY, STATEN ISLAND, TAKEN IN JUNE 1971 LOOKING SOUTHWEST

View shows Prince's Bay, one of the centers for the former oyster industry, and part of Lower New York Harbor. The Prince's Bay Dental Works can be seen on the northern end of the Bay that juts out into the harbor.

FIGURE 2b: AERIAL VIEW OF SOUTHERN STATEN ISLAND TAKEN IN JUNE 1971 LOOKING EAST TOWARD NEW YORK HARBOR

Clear evidence of the rural nature of Staten Island and the recent impact of urbanization is presented. In the foreground the extensive zone destroyed by the West Shore Expressway, which will indirectly be a key factor in the destruction of the remaining wooded areas, is visible. Running off of it (from the bottom of the photograph to the center) are Bloomingdale (bridge construction) and Winant Roads that end in the photograph at Sandy Ground. The wooded area between these two roads is, in the upper section, the Winant-Bloomingdale Grid within which over 100 dumps were located.

Sandy Ground is seen as a scatter of houses situated between a new housing development (the square, cleared area) and the Expressway route. In the distance Prince's Bay and the harbor are just visible. Position of the airplane is directly over Rossville on the Arthur Kill, the narrow waterway that divides Staten Island from New Jersey.
seed oysters preserved and expanded the industry until eventually over 9,000 acres of beds (Smith 1970: 149-151) covered the harbor bottom.

Transplantation not only introduced Chesapeake shellfish into New York but also Marylanders and Virginians, including free Blacks, involved in the trade. For example, a former oysterman of Sandy Ground, who was seventy-eight in 1943, originally worked as a boy on a boat that ran between Norfolk and Prince’s Bay until he settled on Staten Island (Wilkins 1943d: 12). Growth of oystering as a major commercial industry in the Chesapeake Bay is apparently a nineteenth century phenomenon (Vokes and Edwards 1968: 178-179), which possibly explains why free Blacks were able to gain a foothold and successfully expand this adaptation as the industry expanded. Even today, well after the heyday of oystering, Blacks still work the oyster beds of the Chesapeake (Corddry 1972).

This success, however, as well as the increase in Maryland, a slave state, of a free Black population to over 50,000 by 1830 led to persecution. Specifically a series of laws restricting the freedom of Blacks in the oyster industry were passed and this as well as other factors led to the migration of several Black families out of Snowhill, Maryland, Fig. 3, to New York.

Snowhill had a long history of involvement in the coastal trade that centered on New York (Albion 1939: 138-139), and its Black population was quite familiar with New York Harbor (Wilkins 1943a: 1). As they were in no sense impoverished; in fact, some owned their own oyster boats or later became owners of such craft as the Fannie Ferne and the Independence, respectively a thirty and a forty foot sloop, (Wilkins 1943b: 26), they were able to plan a systematic migration north.

Historically the community of Sandy Ground (or Woodrow to use its legal designation), which in large part was created by this migration, has undergone four major phases in its development: (1) formation, (2) a long successful period of stability and growth, (3) a period of decline and disintegration, and (4) the present situation which is not very clear as far as future trends are concerned.

**Formation (1820-1850):**

On the famous British occupation map of New York City (Clinton 1781) a path is shown following the present route of Bloomingdale Road, but, although colonial farms were already present on the Arthur Kill in the Rossville area, there were no structures within Sandy Ground itself. A few White and Black families seem to have settled in the general area (Westfield) in the 1820's (Wilkins 1943b: 26), but the core of the settlement coalesced from both these sources and the movement of individuals and families from Snowhill, at least six of which are historically well known (Bishop, Landin (Landen?), Hinman, Johnson, Purnell, and Robbins) (Wilkins 1943a: 3), and other areas on the Chesapeake in the 1830's and the 1840's.
Figure 3

MAP OF THE NORTHEAST-MIDDLE ATLANTIC REGION INVOLVED IN THE HISTORY OF SANDY GROUND
By 1850 the Black sector of Sandy Ground, which has always been a mixed Black-White community, was settled enough to found the Zion African Methodist Episcopal Church (Wilkins 1943b: 25), which originally stood where the Sandy Ground cemetery is now located. The settlement pattern at this time centered on or just off Bloomingdale and Woodrow Roads (Butler 1853; Walling 1860), with many of the houses having been constructed by the settlers who traded oysters for bricks (Wilkins 1943b: 32) and other needed materials in New York City.

Period of Florecense and Stability (1850-1900):

For over a half of a century the people of Sandy Ground maintained a high standard of living based upon a flourishing oyster industry. Some of them worked for the wealthy White oystermen but a number owned their own boats and beds. There was no overt discrimination against Blacks in the industry (Wilkins 1943d: 12). For example, Robert Landin, the owner of the sloop Pacific, was wealthy to the point that he frequently carried $500 in cash (Wilkins 1943b: 26) which he freely lent out to Blacks and Whites alike.

Sandy Ground almost from its inception boasted a private school taught by Esther Furnell, a member of an original Maryland family, and in general women were deeply involved in church and other activities. Economically most activity in the community centered around oystering but some local crafts (baskets and tools), horticulture (strawberries), and specific local industries were also important. The latter is seen in 1888 when William Bishop, a son of a Maryland migrant, set up a blacksmith shop which is still being operated by the Bishop family today (King and Marone 1970: 90). This successful economic base established in this period eventually produced one dentist, two physicians, several public school teachers, and a member of the New York Police Force.

Economic growth is also seen on the ideological level. The Zion African Methodist Episcopal Church always has been and still is the focus for the community. In 1875, however, a schism caused the congregation, or part of it (?), to join the Methodist Episcopal Conference, and this is reflected on maps from that period in the listing of several "churches" or church owned lots. Shortly after 1875 the church was moved to Bloomingdale Road and a substantial structure was erected (Wilkins 1943b: 25), which is still in use.

Period of Decline and Disintegration (1900-1965):

Ironically the success of Sandy Ground was also tied into its undoing. Urbanization and economic activity in the New York metropolitan area is what produced and stimulated the growth of the oyster industry, but these same processes also initiated pollution that has today turned New York Harbor into a sterile blight. As early as 1884 pollution from
human and industrial wastes were having a deleterious impact on the oyster beds (Smith 1970: 151-152), and by the turn of the century the decline of both the Black and White communities involved in this industry commenced. Finally a series of typhoid epidemics traceable to the beds forced the abrupt termination of oystering in the harbor by the New York Department of Health in 1916.

Economic collapse caused deterioration of the settlement and many of Sandy Ground's inhabitants were forced for the first time to take jobs in the general labor market. Racism which seems to have been unimportant in the previous period now also became an important factor, as is seen in the refusal of the Prince's Bay Dental Factory (Fig. 2a), which employed 600-700 people, to take on Blacks (Wilkins 1934c: 6; d: 12). Many of them then turned to the very factories in New Jersey that were in part responsible for the polluting of the harbor, but a core of families maintained their residence in Sandy Ground and thus preserved the community.

Disintegration accelerated during the Depression when some families lost their homes (Wilkins 1943c: 6; Anonymous 1934), and even more so in April, 1963 when a major fire on the Island destroyed many structures forcing several families to move completely. For example, the Purnell family moved to New Jersey breaking more than a century of residence in Sandy Ground.

Contemporary Situation (1965-1972):

Until 1928 Staten Island had no land connection to New Jersey or New York and was completely dependent on ferry service, one of which had been run by a Sandy Grounder out of Rossville. On November 21, 1964 this pattern of isolation was radically altered with the opening of the 4,260 foot long Verrazano-Narrows Bridge linking the Island to Brooklyn (Smith 1970: 221-222). Since that date a rapid process of urbanization has set in and final plans are now being drawn up that will turn even the southern part of the Island into a true suburbia.

At present rising property taxes, real estate speculation and metropolitan building projects would seem to foreshadow the eventual extinction of Sandy Ground as well as the other small towns in southern Staten Island. An aerial view of part of the Island (Fig. 2b) amply demonstrates the recent results of this process with Sandy Ground being compressed between a new housing development on one side and the construction of a massive highway system on the other. The latter is still under construction as of 1973 and has already destroyed part of the community and equally important will soon open it up to much more direct contact with New York City.

Socially the impact is seen in growing vandalism in the area, probably in the main by children from the new housing project, and the movement of a Moose Lodge into what was formerly Public School 31, built in Sandy Ground in 1904. The exclusive racial policy of this group has
Sandy Ground, however, is still very much a vital community with over twenty families, including some of the original ones, centered in a congregation of about fifty members of the AME Zion Church. The continuation of the settlement may also be reflected in the fact that at least some of the houses have been remodeled or new ones constructed after the fire and that, according to one resident, some of the younger descendants of Sandy Grounders are moving back onto original land lots.

GENERAL RESEARCH DESIGN

A number of historical archaeologists with an anthropological orientation (Adams 1973; Kelly and Ward 1972; Schuyler 1970) have recently advocated a combined documentary, archaeological, and ethnographic (including oral history) approach to historic sites. Such a research design was originally set up for Sandy Ground in 1970 on the expectation of recovering a varied but total range of data for all the major periods in the history of the community.

Historically Sandy Ground is well documented by an excellent range of land, probate, census and cartographic records especially for the periods postdating 1850. The extent of primary internal records, such as diaries, has not been ascertained, but church records and cemetery inscriptions are extant.

Archaeologically one might expect trash deposits in the area from every decade, at least between 1840 and the initiation of modern trash collecting by New York City, while the great fire of 1963 created many house foundation sites. Even during the few years since the fire vandalism has destroyed several additional structures including a very early house on Woodrow Road that was burnt in 1972. Internal variation would include domestic units, barns, stores (e.g. the large Bogardus Store at the corner of Bloomingdale and Woodrow Roads also leveled in 1963), and small structures such as ice houses, storage sheds, coops, and outhouses.

Ethnographically a contemporary community study of Sandy Ground in the 1970's is possible in reference to the traditional focus of the AME Church or the more general and now regional economic and social networks, but only a few inhabitants are still living that even have an indirect tie into the oyster days. Fortunately this dearth is counterbalanced by the excellent work of Dr. Minna C. Wilkins, a psychologist, who in 1943 studied the history of Sandy Ground for the Staten Island Historical Society. With the orientation of a behavioral scientist rather than purely that of a historian she used ethnographic field techniques to capture the memory of the last group of key informants still alive from the oyster industry period. Even when she carried out her study, twenty-three years after the end of the oyster trade according to one of her informants, Sandy Ground was in decay and was mainly inhabited by older
residents and children (Wilkins 1972). She interviewed over thirty people, with three of the older males being her key informants, and studied in detail the histories of five of the Maryland migrant families. Her success in this endeavor made the first part of this article possible in that she recorded much information that will not be found in primary written sources and which is now not available from any source. She has published a succinct report on her findings and has deposited her original field notes in the archives of the Staten Island Historical Society in Richmondtown.

ARCHAEOLOGICAL RESEARCH

A historical cartographic examination of the development of the community structured the initial choice of archaeological sites within the total study area.

In an attempt to locate an early structure that had been occupied throughout the historical span of Sandy Ground the house site of J. S. Guyon that appears on the 1853 Butler Map of Staten Island or Richmond County, New York was selected. When this dwelling (actually two abuted buildings) burnt in 1963 it was the residence of William and Martha Purnell, one of the original Maryland families. Property research in the Deed Books of Richmond County showed that the home remained in the Guyon family, a prominent White family on Staten Island since colonial days, until April 23, 1904 when Louise J. Staudt purchased the house and land, except for the new schoolhouse lot, for $1,800. Staudt in turn died in 1939 and in 1945 the Purnell family took over and lived there until the fire of 1963 forced them to move to Bayonne, New Jersey.

Appearing in several atlases, such as that of Robinson and Pidgeon (1907), as a single but complex unit, it finally is clear on the very detailed Borough of Richmond Topographical Survey (1913) that two structures are involved as well as some out buildings at the rear of the property.

Structures A and B:

Excavations did reveal two separate but connected buildings. The older one (Structure B) consisted at first of a rectangular wood-frame house built upon a foundation of large, unmodified natural boulders and containing two end fireplaces and perhaps an internal partition wall. Later additions expanded the house, including the addition of a third fireplace, but it is the original core that is almost certainly the 1853 J. S. Guyon home. Its construction date is not yet clear, but is probably in the late 1830's or early 1840's.

Almost total excavation (Fig. 4) revealed a three strata sequence of (1) an extremely rich, above-floor, ash layer of badly burnt materials from the 1963 fire, (2) a thin subfloor, brown sandy layer containing mid-nineteenth century artifacts associated with the original foundation
FIGURE 4

FIGURE 4a: STRUCTURE B DURING EXCAVATION

View is looking east toward the junction of Woodrow and Bloomingdale Roads. Excavation has reached the sterile orange soil, but a block of ash, supporting the remains of a chimney fall, superimposed on the brown sandy layer is still unexcavated.

Trees and shrubbery appearing in the photograph were left in place because of the desires of the land owner and in an attempt to conceal the site from vandals.

FIGURE 4b: STRUCTURE B AFTER EXCAVATION

Orientation is the reverse of that in the above view and the bases of the end fireplaces and the boulder foundations are now fully exposed. A possible partition wall is also evident, or perhaps this line of stones was simply a support for the floor. Coverage includes the original core of Structure B, but not the various additions that run off to the north and are unexcavated in this photograph.
and inhabitation, and (3) a sterile, orange soil. Even the superficial analysis accomplished to date on the artifact assemblage from above the floor clearly shows several activity areas within the structure or its additions (e.g. a kitchen area, bathroom, and storage zone) and its overall domestic nature (e.g. remains of four beds, a convertible couch, a crib, and a storage trunk) along with the fact that it must have been a two-story affair. Subfloor material, which in contrast was slight in quantity, included the earliest dated object recovered, a 1795 Washington Head penny, but in the main these items were early to mid-nineteenth century in date.

Structure A (Fig. 5) abuts at a right angle to Structure B and is obviously much later, perhaps almost turn of the century, building. Both were joined, however, into one dwelling unit. Initial clearing exposed a large brick basement foundation for a large wood-frame building. This basement had been dug two to three feet into the red sterile clay that underlies the entire site and was constructed of four brick walls of three courses set upon a concrete floor.

Stratigraphy within the basement was cultural in origin and consisted of a recent, post-1963 trash and brick fall deposit, a thick layer (1/2 - 7 inches) of ash from the 1963 fire that was rich in artifacts, the 3 inch thick concrete floor, and finally the sterile native clay. Artifacts from the ash layer indicated a mixed domestic (e.g. furniture parts and personal items) and specialized function. Large quantities of melted bottle glass, mainly soda pop, were discovered and may (?) be related to the use of the structure as a store a number of years before the fire.

As a total site the Purnell property presents a picture in the 1960's of an extended family, not well to do but not poor, occupying a large complex domestic residence. On the other hand the nineteenth century situation, although also certainly domestic, is not nearly as clear at least at this point of analysis.

Structure C:

Another small outbuilding was also excavated on the Purnell property during the 1971 season, and it has produced the most complexities as far as interpretation is concerned. Although it appears on the 1913 map, implying it was then in use, the descendants of the property owners as well as the local neighbors had no knowledge of its function and were only vaguely aware of its existence. This indicates it had been abandoned well before the 1940's.

Very small in actual activity area, in fact too small to be an ice house at least when compared to those designated on the 1913 map, Structure C (Fig. 6a) consists of a well-constructed, but oddly complex brick foundation for a wooden superstructure. From the surface the unit appears as a small square structure with an external walkway (?). Excavation, however, revealed an internal division created by the presence
FIGURE 5

FIGURE 5a: STRUCTURE A AFTER CLEARING BUT BEFORE EXCAVATION

Deposits of modern trash fill the foundation and are probably both local and non-local in origin. The site is directly on Bloomingdale Road and thus inviting as a dumping area from trucks and cars, but the presence of rustic homemade items should be noted. Sandy Ground residents also use the depression as a dump and as a source of bricks.

FIGURE 5b: STRUCTURE A AFTER INTERIOR EXCAVATION

Two blocks of the 1963 ash deposit have been left in place, but otherwise the structure is totally excavated except for external test pits dug later to see the relationship of stratigraphy to the walls. A test through the concrete floor exposes the sterile red clay typical of the area.
FIGURE 6

FIGURE 6a: PLAN VIEW OF STRUCTURE C AFTER PARTIAL EXCAVATION

Seen from the surface the structure looks square with the niche created by an addition of bricks to the interior southwest and northwest corners. Scales (as in all figures) are in feet and inches.

FIGURE 6b & c: PLAN AND HORIZONTAL VIEWS OF THE CORNER OF STRUCTURE C

Further excavation revealed that the niche is in fact an integral part of the construction. The arrows in Fig. 6b designate the direct placement of the initial brick layer on the subsoil and that the interior concrete floor does not pass under the walls. Mortar from the brick work, superimposed over the corner of the floor, did, however, show these features to be contemporary.
of a small chamber or nitch that may have been set off by a door, and when the walls were disassembled it became apparent that the nitch (Fig. 6b & c) was a carefully planned feature of the construction.

In reference to function the associated artifacts are in no way conclusive. Perhaps it served for specialized storage or as a root cellar.

Winant-Bloomingdale Grid:

Examination of maps and land records revealed that as Sandy Ground expanded it enveloped on three sides a large tract of land, between Winant and Bloomingdale Roads, that had never been extensively built upon (Fig. 2b). This created a natural dumping area for the community, at least from the mid-nineteenth century on, and a preliminary survey verified the presence of numerous surface dumps.

An extensive grid, based on squares 200 feet on a side, was extended over this area running 1,800 feet out from Sandy Ground toward Rossville on the Arthur Kill. Using students moving simultaneously in a line, all stray artifacts were collected within each square after they had been outlined with red cloth. At the same time over 100 dumps were located (Fig. 7a) within the first twenty-three squares of the grid and, with a few exceptions, were totally excavated. This produced materials that were, with a few possible outside intrusions, from Sandy Ground. Unfortunately as the project unfolded it became evident that almost all of the dumps, which were surface or barely subsurface deposits, had been systematically looted by bottle hunters.

Feature 4, 5, and 120:

During the first summer one surface dump, Feature 4, was found concealed under a thick layer of humus and leaves in a natural depression within the general grid and it was completely intact. This find has produced a rich terminal nineteenth century assemblage (Fig. 7b) and its location directly across from houses occupied by Blacks during that period is a strong indication that it is a product of their activity. A specific find, a small bottle from the F. A. Shipley Central Pharmacy in Seaford, Delaware, which is not far from Snow Hill, Maryland, and which was deeply involved in the Chesapeake oyster industry, may reinforce the connection to the Sandy Ground oyster trade period. Chronologically the contents of the dump suggest a period of c. 1890 to 1905, and the Shipley Central Pharmacy (Tull 1972) was in operation between 1891 and 1909.

During the second season of excavations another undisturbed surface dump, Feature 120, dating slightly later than Feature 4, was located outside of the grid but within Sandy Ground.

Supplementing these two surface features is a stratified turn of
FIGURE 7a: FEATURE 8, A TYPICAL LARGE SURFACE DUMP LOCATED IN THE WINANT-BLOOMINGDALE GRID.

FIGURE 7b: FEATURE 4 DURING EXCAVATION.

Most of the artifacts were in the humus layer or just into a layer of decomposed sandstone bedrock. Presence of several bottles shows that the site has not been looted, and the metal loop (many of which were found) may indicate barrels within which trash was transported to the dump.

FIGURE 7c: FEATURE 5 DURING EXCAVATION.

A stratigraphic sequence was revealed consisting of a dark, organic deposit over a sandy layer, over a thin burnt brick layer, which in turn rests on red sterile clay.

FIGURE 7d: ORGANIC LAYER OF FEATURE 5 DURING EXCAVATION.

Along with many domestic artifacts a number of scythes, and other farming items, were recovered.
the century dump (Fig. 7c) clearly associated with the Purnell site house. Feature 5 contained a number of strata that probably span the Guyon and Staudt occupations, both being White families, that exchanged the property in 1904. A good collection of domestic and farm oriented materials (Fig. 7d) was recovered.

**Sandy Ground Cemetery:**

Above ground features include abandoned and occupied houses, some of which display quite early architectural traits, and the headstones and other attributes of the Sandy Ground Cemetery.

Located at the first site of the church, this Black burial ground has a fine range of nineteenth and twentieth century stones as well as many unmarked graves (Fig. 8). One stone, that of Clara Cooley, may carry an 1839 date, or it may be an after the fact inscription, but in general the dates start in the 1860's and run to the present. Inscriptions referring to thirty-four families and sixty-two individuals are readily evident. Minna Wilkins had made some use of this historical source but the entire corpus is recorded here (Appendix 1) because a number of stones are disintegrating or eroding.

In general appearance the graveyard seems overgrown and the wealth changes in the community are reflected in several burials that do not have headstones. The state of the graveyard itself may not, however, be related to an inability to keep it gardened but to deeper causes. Many graves also display small deposits of broken ceramics, glass, and plastic flowers, and there are larger deposits of such materials along the paths and at the ends of the cemetery. Whether this reflects a pattern similar to that reported by Combes in this volume for South Carolina Black graveyards, and thus rooted ultimately in Afro-American heritage, is not clear. The Middle Atlantic origin of part of the Sandy Ground population and the fact they were outside the slave system, perhaps at an early date, might be significant factors in this regard. At least it is certainly evident that the Sandy Ground Cemetery is in marked contrast to White graveyards on Staten Island. It is overgrown but there is extensive evidence of activity there, care given to individual graves, and a continuous involvement of the Black community with the burial ground.

**CONCLUSION**

Being unique gives a community like Sandy Ground both major strengths and shortcomings as a research unit. Its strengths lie in the ability to clearly delineate the settlement temporally and culturally from the general region in which it is situated. Sandy Ground offers the historical archaeologist an opportunity to approach a series of major problem areas that have long been of interest to anthropologists and other social scientists.
Both 19th and 20th century stones are visible as well as an equipment storage yard adjacent to the cemetery.
Migration:

As a product of a planned movement of a social group from one area to another, but with retention of the same economic base, the community is singular for Staten Island and probably for much of the nineteenth century pattern of Black migration from the South to the North. Specifically there is still a significant Black population in the Maryland area from which the settlers of Sandy Ground originated and even some of the key Staten Island Black family names continue in Worcester County. In fact, the possibility of locating and excavating the former Maryland house sites of the first Sandy Ground immigrants is probably quite high as that part of the Delmarva Peninsula is still undeveloped.

This Maryland-Staten Island potential range of data offers a framework for the testing of hypotheses concerning migration, especially the long discussed suggestion that people, in planned movements, select environments, natural and cultural, that are most similar to those they are leaving. Sandy Grounders not only selected a similar tidewater, rural but urban oriented (cf. Baltimore) setting, but also an almost identical set of ecological relationships. However, a major weakness in studying such questions of migration or readaptation after migration at Sandy Ground has been the failure to get an adequate archaeological sample of the period from 1830 to 1850, when the initial settling-in occurred.

Evolution of the Community:

Once fully established Sandy Ground grew in population as its economic base expanded. An intriguing problem is that of the interaction of this economic growth and the evolution of the Black church, the focus of the community. The schism of 1875, as well as the numerous "church lots", and perhaps actual contemporary church structures, that appear on several maps, must be tied into the economic success of the people. Why were there apparently several churches at the same time which have since disappeared or been reunited in the present Bloomingdale edifice built after the community "had become proud"? There is a possibility that this relationship might be investigated archaeologically as a "church" structure (probably in actuality a private residence) appearing on the 1913 map has probably been located in a field survey.

Economic growth must have also been correlated with the internal and external social patterns of the community. Wilkins collected enough data to show close ties, including marriage, to other local Black populations, especially in Manhattan. The extent of endogamy, evident in land records showing marriages between members of the original Maryland families, and exogamy, and the fluctuation of the two (Leone 1968) as the economic base grew, and later collapsed, offers one such research area.

Interaction between Black and White residents of Sandy Ground was, and still is today, complex. The settlement pattern of Sandy Ground
stands out when compared to that of the general area of southern Staten Island as having tightly clustered sites on small parcels of land as against larger land units with isolated houses. The former is a product of specialized industry, oystering, while the latter is the expected farming pattern. According to Wilkins’ (1972) informants, the White inhabitants of Sandy Ground were not in the oyster trade and the agricultural orientation of the Guyon-Staudt dump would seem to support this difference.

Again, unfortunately, there is an almost total dearth of requisite artifactual materials from dumps before 1880.

**Economic Collapse and Cultural Instability:**

After 1900 the community was in decline and after World War I it rapidly lost its economic base. Migration out from Sandy Ground and the temporary removal of many of the young adults during working hours (Anonymous 1934) must have caused drastic social changes in addition to a general decline in the standard of living.

Archaeologically the period between 1890 and 1920 is what is best represented in the total artifactual assemblage produced by two summers research. Since these decades span the economic collapse of Sandy Ground, and as many of the artifact categories carry historical information on origins and distribution, these assemblages offer the possibility of proposing various models and hypotheses on the impact of localized economic collapse upon a specialized community in a general industrial economy.

Retrenchment of trade networks and reduction in the quantity, variety and value of purchased goods would seem the obvious expectation from such a collapse. However, it should be kept in mind that Sandy Ground, specialized as it was, was never autonomous from the regional economic patterns of Staten Island, the metropolitan area, or the Northeast in general. The broader picture would therefore make a number of working hypotheses possible concerning local collapse in an industrial setting:

1) logistic networks would retract and value and variation of goods decrease,

2) logistic networks and variation would expand, while the value of goods would decrease,

3) logistic networks and variation would remain constant, with only price fluctuation appearing in the artifactual inventory,

4) logistic networks, variation, and value of goods would all remain basically constant.
"Logistic networks" refers to trade patterns, especially the distance between origin points of goods and Sandy Ground, while "variation" refers to the number of different categories of artifacts involved in specific types and the total assemblage. "Value" in turn refers to contemporary, turn of the century, pricing of goods.

As the Sandy Ground economy retrenched it can be proposed that the logistic networks would, with some lag, follow suit. But it is possible that such collapse might force the people of Sandy Ground, women as well as men, into a much broader labor market. To some degree this did happen when they turned to New Jersey factories after local Staten Island firms would or could not hire them. Such a job location expansion might result in an even broader range of sources for purchased goods. If one works in New Jersey or New York City why not buy there also, especially if cheaper and more plentiful and varied goods are available.

On the other hand the fact that Sandy Ground at the turn of the century was part of a highly industrialized society might lead to areal and regional patterns of trade, production, and economic fluctuations totally overriding or masking local vicissitudes. Indeed, this might even be pervasive enough to hide the economic failure of Sandy Ground even in reference to the value of goods as cheap, mass produced items became available in the late nineteenth century (Ingersoll 1971: 70) to the general northeastern population.

Hypotheses, such as those listed above, which it is readily admitted are not grounded in established bodies of economic theory, are very tentative. The archaeological implications for their testing are, in fact, not yet available at Sandy Ground for reasons which will be outlined shortly. Potentially, however, the rich range of distributional data available in nineteenth century, especially late nineteenth century, sites is highlighted in an examination of bottles found in Feature 4 (c. 1890-1905). Excluding canning jars, about seventy individual bottles from this dump carry embossed data that presents a striking if limited picture of trade networks at Sandy Ground. The data fall into two different categories: (1) user of the bottle for a product, and (2) producer of the bottle; which may be the same, or not differentiated, or only one or the other being given.

Seen in the form of a distribution map (Fig. 9), the provincial nature of the zone from which Sandy Ground drew its bottles, and perhaps certain other artifact groups, is both evident and surprising. Only four bottles are from a great distance, three from Europe and one from California (San Francisco); the rest being from the Northeast-Middle Atlantic region and the vast majority (fifty-four items) falling within a fifty mile radius of the site. A series of concentric intervals of ten miles will demonstrate an even closer fit, although an elliptical form for the interval is more accurate than a circle. A thin scatter from the towns on the New Jersey shore of New York Harbor is present, an area that was partially industrialized by 1900, but basically there are two nodes or clusters; one in New York City (southern Manhattan) and the other on Staten Island itself. Whether this distribution is a
FIGURE 9

A HISTORIC SITE AS A SOURCE OF BASIC DATA ON ECONOMIC HISTORY

On the distribution map 1 3/4 inch equals 6 miles. When concentric intervals are superimposed on the map, with Sandy Ground as the center, then fifty-one of the bottles fall within a tight twenty mile radius. Actually an elliptical pattern, created by one major cluster in Manhattan and another on Staten Island, is more appropriate and would encompass an even higher proportion into the orbit of the metropolitan area.

Data on which map is based involves the product in the bottle rather than (in most cases) the producer of the bottle. When both types of data are combined the same basic pattern is repeated. The only items that truly fall outside of this tight cluster are:

- Philadelphia, Pennsylvania 3
- Seaford, Delaware 1
- Millville, New Jersey 11*
- Oswego, New York 2
- Buffalo, New York (?) 4
- Boston, Massachusetts 1*
- Lowell, Massachusetts 3
- San Francisco, California 1
- London-Paris (?) 1
- Berlin, German Empire 1
- Austro-Hungarian Empire (?) 1

(*bottle producers outside of the local area)
Figure 9

FEATURE 4 DISTRIBUTION NETWORK BASED ON BOTTLE DATA

Figure 9
41
product of the general transportation and trade systems within the metropolitan area or of the specialization of Sandy Ground, and its New York City orientation arising from the oyster trade, is yet to be established. It may be that a more local Staten Island orientation will emerge from the analysis of dumps accumulated by contemporary White farming families. Nevertheless the degree of local Staten Island production of both products and bottles is striking. Apparently almost every small town had its own bottler (Anderson 1972) with several being important businesses. The expense of transport, location of resources (sand) and labor, and problems of preservation would all have been factors helping to create this pattern.

Production of the bottles themselves was also local but interpretation is more difficult as a number of embossed trademarks have not yet been historically traced. A number of companies on Staten Island (Hadkins in Tottenville and Prince Bay Bottling Company), in New York City (Karl Hutter), and the extensive Whittall Tatum & Co. glassworks at Millville, in southern New Jersey (Pepper 1971: 224–230), were the chief sources. As far as contents are concerned, soda pop, beer, and food are local in origin, while medicine, perfume, and some more exotic alcoholic beverages are from greater distances. The highly industrialized zone around Lowell, Massachusetts is one of the few non-local production areas that is also clearly delineated.

A comparison of these Feature 4 data with that from Feature 120, which is chronologically slightly later, may give a framework for the testing of the hypotheses already discussed. Unfortunately the hope of having almost a hundred dumps, from the Winant-Bloomingdale Grid, as a data base for the entire development of Sandy Ground did not materialize. First all the dumps encountered at Sandy Ground fall between 1880 and the twentieth century, and more importantly the grid dumps are missing most of their bottle content because of looting. This fact will make meaningful comparisons very difficult if not impossible.

Another problem, already mentioned, with the testing of the listed hypotheses are their isolation from any established body of economic theory. They are of course specific statements on the interaction of several variables that all emerge from the general situation of major economic change. One possible general framework is the "cultural focus" concept proposed by Herskovits, the economic anthropologist, in his work (Herskovits 1948; 1955). As originally stated it emphasized the fact that any given society tends to concentrate its energies in particular areas such as economics or religion. This general concept was refined by Claude Warren (1964) to apply to the economic subsystem and to an archaeological situation. According to Warren if a society has a successful economic orientation and then because of ecological or technological change has to adapt to new conditions it will not have the ability to select from the total spectrum of new possibilities. Rather it will be predetermined in its choice by its previous "economic focus".

Warren applied the concept to changes in the prehistoric economy
of La Jollan and Yuman cultures in southern California. At Sandy Ground, although the specific facts are quite different, a number of possibilities arise when the situation is viewed within the "cultural focus" concept. Perhaps the people of Sandy Ground would have been in a better position to adapt to new industrial careers as they, unlike many of their White neighbors, were already focused within a highly specialized industry. An example would be their familiarity with New York City and contractual relationships well beyond the local community. At the same time the fact that their former economic focus was not exhaustively oystering, but also included crafts, horticulture, and specific industries, might have enabled them to expand these formerly minor elements in their economy into significant areas of income. Joel A. Cooley, for example, a member of Sandy Ground although he lived slightly outside the community, became a famous dahlia grower. He originally was in the oyster industry with his gardening only being a hobby, but in later years it grew in importance in his life as is seen in the fact that he was also the first to cultivate figs on Staten Island (Wilkins 1943b: 31). Today there are greenhouses in Sandy Ground at least one of which is run by a Black family.

What is needed for the testing of the specific hypotheses within this more general model is quantified historical as well as archaeological data on how many took factory jobs, or developed local, already established industry, such as the Bishop blacksmith shop which does have some records, Wilkins did not concentrate on this question, and the data at present are very incomplete.

Just as the uniqueness of Sandy Ground is its greatest strength, so, as has been alluded to, it is also its greatest weakness as far as archaeological research is concerned. If part of the record (archaeological, historical, or ethnographic) is lacking there is no second Sandy Ground, at least in the area, to turn to as a backstop. For this reason the research design used at Sandy Ground will in the near future be shifted to a larger regional unit as part of a long term project similar to such investigations (Struever 1968; 1971) in prehistoric archaeology. Research at Sandy Ground, especially archival investigation, will continue, but further archaeological exploration will be of a limited, testing nature.

Sandy Ground Excavations as Industrial Archaeology:

Research at Sandy Ground, as reported in this article, is properly considered an example of Industrial Archaeology. Although originally defined by some as the study of obsolete industrial monuments, it is clear that this field needs and is getting (Buchanan 1970; Lee 1972; Schuyler 1972) a broader perspective. It may surprise some archaeologists to read of the excavation of an assemblage dating from 1963 (as in Structure A and B), but the study of the material manifestation of the Industrial Revolution can not be arbitrarily limited to the nineteenth century or to even before the 1920's. Industrial Archaeology must
obviously also include the study of contemporary industrial and industrializing societies, and thus will be a key element in redefining archaeology as the scientific study of material culture rather than the study of only past cultural manifestations.

ACKNOWLEDGEMENTS

Various citizens of Sandy Ground, especially Martha Purnell who gave permission to excavate on her property, made this article possible. Dr. Minna Wilkins in her previous research and more recent cooperation with the author was also instrumental in its creation.

My field work depended on a number of energetic site assistants, Jerome Schaefer (University of Arizona), William Askins (CCNY), Joel Klein (NYU), and Hanson Wong (NYU), as well as the thirty-three students who participated in two summer's work. Mr. Schaefer has been particularly essential in overseeing much of the field activity.

Rosemary Fitzgerald and Loring McMillen of the Staten Island Historical Society made it possible for me to consult Dr. Wilkin's original notes, and during the first summer the Society also provided a historic house near Sandy Ground as a field laboratory.

Both maps were drawn by William Askins and credit for the photographs is: Figure 1, 2a, 2b, 4a, 4b, 5a, 5b, 6a, 6b, 6c, 7a, 7b, 7c, and 7d by Mark Rosenzweig of Brooklyn College; Figure 8a and 8b by Jerry Fischer. Mrs. Vera Chimene was also instrumental in supplying the airplane used in aerial photography.
Inscriptions were recorded on April 3, 1972 and data from all standing gravestones were copied. A few stones were eroded, buried in brush, or had sunk into the soil. No attempt was made at recording that would have involved any disturbance of a stone. The list that follows is, nevertheless, almost complete for the cemetery and is arranged alphabetically by family name.

**BAGWELL:**

Grave: Thomas Bagwell  
Died Jan. 31, 1892  
Age 64 Years  
His Wife  
Elizabeth Cooley  
Died Jan. 16, 1900  
Age 71 Years

**COLEMAN:**

Grave: John Warner Stevens  
Born April 10, 1853  
Died July 31, 1912  
Stella Maud Coleman  
Born July 3, 1877  
Died [?] 3, 1910

**COOLEY:**

Grave: Rev. Baylor Cooley  
Born April 1, 1868  
Age 71 Years  
His Wife  
Clara Cooley  
Died June 17, 1839  
Age 45 Years  
[adjacent stone, "MOTHER"]

**BENEDICT:**

Grave: Frederick A. Benedict  
Born July 8, 1893  
Died April 17, 1905

**BISHOP:**

Grave: Daughter Eva T. Bishop  
1891 – 1936

Grave: Geo. W. Bishop  
Born July 16, 185[?]  
Died December 11, 1904  
Age 51 Years

Grave: Eliza Cooley  
[see above stone]

Grave: In Memory of  
Eliza Cooley  
Wife of  
Philip Cooley  
Died December 19, 1884  
in the 81st Year of Her Age

**BROWN:**

Grave: Joseph Brown  
New York  
January 20, 1925 –  
May 4, 1957  
CK# USNR – NY

Grave: Clara Cooley  
[see Bagwell stone]

Grave: Elizabeth Cooley  
[see Bagwell stone]

Grave: Rev. William J. Colley  
Died [?] 1901  
Lucy A. Cooley  
Died October 15, 1910[?]
HAMMON:

Grave: Albert A. Hammon  
NY Pvt. 51 Co. 153 Depot.  
Bridge WW I  
Aug. 22, 1888 -  
Dec. 10, 1959

HAMMOND:

Grave: Chas. L. Hammond  
NY Tec. 5 4295 QM  
Gas Sup. Co. WW II  
May 10, 1923 -  
August 21, 1948

COOLEY (cont.):

Grave: Philip Cooley  
[see Eliza Cooley stone]
Grave: Rueben H. Cooley  
Died Dec. 24, 1925  
Age 69 Years
Grave: Sarah Matilda Cooley  
Died Oct. 27, 1917  
Age 50 Years
Grave: Rev. William J. Cooley  
[see Lucy A. Cooley stone]
Grave: Philip Cooley  
[see Eliza Cooley stone]
Grave: Rueben H. Cooley  
Died Dec. 24, 1925  
Age 69 Years
Grave: Sarah Matilda Cooley  
Died Oct. 27, 1917  
Age 50 Years
Grave: Rev. William J. Cooley  
[see Lucy A. Cooley stone]
Grave: Philip Cooley  
[see Eliza Cooley stone]
Grave: Rueben H. Cooley  
Died Dec. 24, 1925  
Age 69 Years
Grave: Sarah Matilda Cooley  
Died Oct. 27, 1917  
Age 50 Years
Grave: Rev. William J. Cooley  
[see Lucy A. Cooley stone]

COOPER:

Grave: Alvin Clinton Cooper  
NY SP3 HQ Co.  
32 Signal BH  
Dec. 17, 1935 -  
Sept. 9, 1957

COOPER:

Grave: Alvin Clinton Cooper  
NY SP3 HQ Co.  
32 Signal BH  
Dec. 17, 1935 -  
Sept. 9, 1957

HARRIS:

Grave: Harris  
Charles H.  
July 9, 1889 -  
November 21, 1948

HARRIS:

Grave: Harris  
Charles H.  
July 9, 1889 -  
November 21, 1948

DECKER:

Grave: Abram C. 1882-1956  
Arlena F. 1886-1969

DECKER:

Grave: Abram C. 1882-1956  
Arlena F. 1886-1969

GIBBS:

Grave: J. Clayton Gibbs  
1915 - 1917  
John W. Gibbs Jr.  
1924 - 1930

GIBBS:

Grave: J. Clayton Gibbs  
1915 - 1917  
John W. Gibbs Jr.  
1924 - 1930

GLENN:

Grave: Sarah S. Glenn  
--- ? ---

GLENN:

Grave: Sarah S. Glenn  
--- ? ---

GREEN:

Grave: GREEN  
Beloved Mother  
CARRIE  
June 26, 1880 -  
May 3, 1954

GREEN:

Grave: GREEN  
Beloved Mother  
CARRIE  
June 26, 1880 -  
May 3, 1954

HENRY:

Grave: Francis M. Henry  
1863 - 1930  
MOTHER  
Mary E. Henry  
1862 - 1910

HENRY:

Grave: Francis M. Henry  
1863 - 1930  
MOTHER  
Mary E. Henry  
1862 - 1910

HINES:

Grave: John R. Hines  
NY CPL 367 Infantry  
92 Division WW I  
Aug. 19, 1895 - Jan. 20, 1950

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Grave: John R. Hines  
NY CPL 367 Infantry  
92 Division WW I  
Aug. 19, 1895 - Jan. 20, 1950
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APPENDIX 1 (Continued)

HUNTER:
Grave: Hunter
       Celia
       1877 - 1928

Grave: Hunter
       1877 Edith 1938
       1869 George -?-?

Grave: William F. Hunter
       1899 - 1955

JACKSON:
Grave: Capt.
       John Jackson
       Died
       June [?] 12, 18[?]
       July ---?---

JONES:
Grave: Mary Jane Jones
       Born Nov. 28, 1850
       Died Aug. 8, 1898

Grave: Wilbur Earl Jones
       New York
       October 19, 1920 -
       May 3, 1957
       TEC 5 463 AMPH. TRUCK

JUNIOR:
Grave: William P. Junior
       NY PVT Co. B Inf. NYNG
       World War 1
       March 12, 1900 -
       Nov. 29, 1963

LANDIN:
Grave: Dawson Landin
       Born Dec. 18, 1826
       Died Feb. 21, 1899

MANGIN:
Grave: MANGIN
       Jane
       1872 - 1937
       John
       1872 - 1937

McCoy:
Grave: James McCoy Jr.
       1927 - 1965
       [also a general McCoy stone for a family plot]

MITCHELL:
Grave: Melvina Mitchell
       ---?---

PENNYFEATHER:
Grave: PENNYFEATHER
       1906 Evelyn Henry 1970

PURNELL:
Grave: Elseym Purnell
       Co. E
       9 U. S. C. I.
       [also a bronze medallion:
       Post 100 GAR]

ROACH:
Grave: In Loving Memory of
       Theodore A. Roach
       Born May 13, 1902
       Died Sept. [?], 1918
APPENDIX 1 (Continued)

ROBINSON:

Grave: Marion
    [in family plot]

Grave: ROBINSON
    Mary L. Robinson
    Born July 23, 1862
    Died October 24, 1912

Grave: Theodore
    [in family plot]

SCHNIDT:

Grave: Sarah M. Schmidt
    Died Nov. 14, 1925
    Age 87 Years

SELBY:

Grave: Mary A. Selby
    Died Sept. 18, 1913
    Age 91 Years

STEVENS:

Grave: In Memory
    Annie A. Stevens
    Died Feb. 11, 1876
    Aged 1 Year 11 Days
    Sleeping in Jesus

Grave: John Warner Stevens
    Born April 10, 1853
    Died July 31, 1912
    Stella Maud Coleman
    Born July 3, 1877
    Died [?] 3, 1910

WALKER:

Grave: Susan A. Walker
    Born Mar. 10, 1853
    Died Mar. 25, 1912
    A Faithful Friend

WALLEN:

Grave: Lydia L. Wallen
    1875 - 1952
    Rest In Peace

WATTS:

Grave: WATTS
    George C. Watts
    Born June 28, 1878
    Died March 22, 1927

WHALLEN:

Grave: BERTHA WHALLEN
    1899 - 1952
    Rest In Peace
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ETHNOGRAPHY, ARCHAEOLOGY AND BURIAL PRACTICES AMONG COASTAL SOUTH CAROLINA BLACKS

John D. Combes

Black burial practices, especially in the South Carolina coastal regions, are not particularly well known. This is true more so archaeologically than ethnographically. Recently, the Institute of Archeology and Anthropology at the University of South Carolina conducted some limited archaeological testing which revealed some important data that has lead to a further examination of nineteenth and twentieth century burial practices. Ethnography has provided the stimulus for this little known important aspect of Black culture.

The Institute has been intimately involved with the development of the Charles Towne Landing (1670) Site since it was first purchased by the State and the decision was made to create at this location a Tricentennial Park. Stanley South of the Institute staff was the principal investigator of the archaeological work from the fall of 1968 until the fall of 1969. The writer was co-investigator of the initial project and Richard Polhemus, formerly of the Institute staff also has had minor involvement at the site. All members of the staff have been called upon on numerous occasions to "clear" various areas archaeologically prior to further construction activities at the park.

We were all pleased with the purchase the State had made because totally by accident the 250 acre piece of real estate happened to contain, in addition to the Landing Site, an additional 8,000 years of human history! Included was an important Indian ceremonial center, some Revolutionary War fortifications and an old plantation complex ruins as well as the present day plantation buildings and gardens. What could be a more appropriate birthday present to the State on its 300th birthday anniversary than a development of such an historical treasure? The outcome of this tricentennial park is far from being close to what was envisioned by us but on the other hand a considerable amount of scientific data was recovered by the archaeologist including the topic of interest here.

It has already been pointed out that prior to any subsequent earth moving operation at the site the Institute is to be notified. This usually requires that one of us visit the site and do some testing, follow the trenching machine or whatever. Based on the field inspection plus discussion with all of the Institute personnel a decision is made regarding the fate of the area in question. In the spring of 1972 we were called regarding a two or three acre area that was being considered for a "touch and hold" children's zoo. A man was dispatched from the Institute to check out the area and he returned reporting that there didn't appear to be anything there other than some late nineteenth and twentieth junk scattered throughout the area. South and the writer both remembered the area and had written the location off as being late period garbage of no interest. We had, it now seems in retrospect, been "tuned in" to seventeenth and eighteenth century material in 1968. That turned out to be an unwise decision.
FIGURE 1: The remains of the grave goods placed on top of a burial.

FIGURE 2: A modern example of the random layout of grave sites in a coastal cemetery.
The Tricentennial Park continued with its plan for the development of the zoo. A week or so later a young man and his wife visited the Institute and they were both rather perturbed with our decision to allow the zoo in that particular location. Our discussions with them brought into focus that the proposed zoo would be placed right in the center of a Black Cemetery. His observations were, indeed, interesting and it turned out that the existence of the so called "junk" was what gave him the clue to the presence of the cemetery. This young couple was Mr. and Mrs. Gregory Day who were in the area studying Black crafts on a Smithsonian Institution Grant. They had spent almost a year studying the Blacks of the Carolina Coast and had gained a considerable amount of information regarding Black Culture including burial practices.

Based on this informative discussion, the writer and a crew of three visited the proposed children's zoo location for the purpose of authenticating the burial ground and determining its perimeter. Several exploratory trenches 2 feet by 50 to 100 feet in length were excavated. These explorations were designed to locate the grave excavations as well as the extent of the cemetery area. The trenches were shallow, going only to the limit of the surface disturbance. Over 100 grave pit excavations were located by this technique. A considerable amount of this late nineteenth and early twentieth century "junk" was located throughout the exploratory trenching.

In addition to finding as many grave locations as possible we were also interested in examining one of the grave pit features to verify that the features were actually burials. This excavation found the grave fill to contain human bone and grave offerings throughout. Quickly this puzzle was solved when it became apparent that the grave excavation had cut through at least three other graves thus accounting for the human bone and offerings that had extended down through the grave fill. It was also noteworthy that all of the burials intruded on were oriented differently.

The grave pit dimensions were 2.4 feet by 6.7 feet and 4 feet deep. Depth to the top of the coffin was 3.1 feet. The casket was held together with cut nails, possessed iron handles and plated ornaments plus a "bust window". The individual was placed on his back, hands crossed over the pelvis with the head oriented in a westerly direction. Over each eye a penny had been placed, one dating 1870 and the other 1882.

There seemed to this writer to be many unusual aspects to this cemetery and the individual burials, at least when compared to Native American and White burials. First of all there were the grave goods which were placed on top of the grave as well as the nature of the items chosen (Fig. 1). Also, the graves themselves seemed to be placed throughout the cemetery with no order and many of the graves intruded through burials previously interred (Fig. 2). There were also questions pertaining to marking and care of the graves and cemetery.
FIGURE 3: Grave offerings recovered from an excavated burial from Charles Towne Landing.
None of these graves was marked or if they were the markers had not survived.

At this point further consultation with the ethnographer provided a number of answers. The burial area is a special place of utmost importance to the Black community that is controlled by many spirits. These spirits are readily available and an individual may communicate with them at some particular place in the home or perhaps in the woods. One must, for example, obtain permission to enter the grave yard and to enter without it would be unthinkable. The most important aspect of the burial area or for that matter the whole burial phenomenon is the importance attributed to the final resting place of the deceased spirit. It is imperative that the deceased be buried with the spirits of the other members of the family. The penalty for not being interred with the family spirits is, indeed, serious and results in a wandering spirit having no final resting place. There is not one other thing more important in one's life than to insure one's place in the family cemetery. During the nineteenth century and early twentieth, the burial areas were not associated with church yards. Later, when the proper conditions were met and the cemetery was transferred to the church yard the fear of being banished from the family final resting place gave the clergyman a tremendous hold on his parishioners.

What we see then is an emphasis on the burial area as a final resting place for the family spirits of the deceased. Perpetual care was not significant nor was a carefully laid out arrangement of graves. In fact, this investigation at the Charles Towne Landing Site (and since then elsewhere) illustrated clearly that even intruding a new grave into others was acceptable and may well have been desired. Mr. Day also pointed out that small structures used for prayer may have been associated with the cemetery complex.

Typical of most all early Black graves in Coastal South Carolina and in the interior as well is the wide assortment of grave offerings that may be seen on top of and around the grave (Fig. 3). This material consists mainly of cups, saucers, bowls, dishes, tumblers, kerosene lamps, clocks, medicine bottles, pitchers, various cut glass pieces and just about any other household item imaginable plus coins. There also may be many pretty sea shells with the conch being the most popular displayed around the grave. Near the turn of the century when concrete became accessible some of the grave offerings were pressed into homemade monuments prior to its hardening (Fig. 4). The same was done in the slabs that were sometimes poured over the grave. Parsons (1923) has an excellent observation from South Carolina regarding burials:

... the cup and saucer used in the last sickness should be placed on the grave. The medicine-bottles are placed there, too; and if any medicine is left in the bottle, the bottle should be turned upside down, and the cork loosened, "so
FIGURE 4: A homemade monument with a grave offering (plate) pressed into the concrete.

FIGURE 5: Two inhumations marked with stakes or boards.
med'cine suck up de grave". On most graves a cup or piece of cut glass, bottles, and quite often a lamp, may be seen. According to some, it is "somet'in de bes' in de house, not partikler belong to dead," which is placed on the grave. On the grave, too will be placed cut flowers, and conch-shell, -"jes' a dressin' up de grave." ... anything in the house, "what have glass, cover up or tu'n back on the wall; an' stop de clock from runnin'." Not to take these measures is bad luck.

... graves scattered without symmetry, and often without head-stones or head boards, or sticks, but invariably dug east and west, the head to the west (Parsons (1923: 214-15).

Items recovered from the burial at the Charles Towne Landing Site may be seen in Figure 3. Medicine bottles are very common including one favorite item, Carters Little Liver Pill containers. Regarding how the various items are chosen for placement on the grave, Mrs. Day provided an unusual story. She reported that the mother of a deceased girl had told her that after the funeral she thought all was done properly at the grave but for several nights she was restless and was unable to sleep. Finally, her daughter came to her in a dream and said everything was OK but that she had forgotten to leave her hand lotion on the grave and she needed it. The mother then hurriedly took care of this overlooked item by placing the lotion on the grave. That solved the problem, thus putting an end to her insomnia. There are numerous other stories that could be told to illustrate the importance of the spirit world.

Another point of interest to the archaeologist is the concern with sympathetic magic, particularly with respect to hair, nail clippings, teeth and possibly severed fingers, limbs, etc. All of these items were considered representative of the individual (Herkovits 1941: 238). The face-down burial, especially among children, is another phenomenon the archaeologist might have trouble interpreting. Burying an infant face-down is a technique of tricking malevolent spirits. Among the Geechee Negroes of Georgia it is believed that "if you cannot raise your children, bury on its face the last one to die and those coming after will live" (Herkovits 1941: 189).

The writer has observed several dozen Black burial locations during the past two years ranging from large heavily used cemeteries to small forgotten plots now almost taken over by the woods. All of those observed exhibit the traits discussed earlier in the paper. Many of these practices are no longer carried out, however the evidence for them having been done usually shows up if the area is given a careful scrutiny.
Many of these practices clearly have African origins which is in itself of interest but an area already studied extensively most notably by Herskovits. The emphasis here has been primarily with the archaeological aspect. It is also of interest that in many areas today in the Carolina Coastal regions these old burial practices are still very much alive. For example, within the present city limits of Charleston, South Carolina exists a Black cemetery located in a Black community in which all of the discussed traits are still being observed. Grave offerings not excluding coins remain undisturbed by the community including the children!

It is also of significance that the practices appear to be persisting much longer along the sea coast regions than they have in the interior regions. Driving inland from the coast it appears that the further one gets from the sea the older these practices become. Many of the cemeteries that can be seen today that are in church yards and are near a white cemetery tend to take on the characteristics of the latter. It is difficult to distinguish between the two - no grave offerings, no shell decorations - and there is order to the arrangement of graves as well as grass and perpetual care to some degree. These cemeteries with the "new look" are usually the result of a clean up and if the grounds are carefully examined, deep in the grass may be found an occasional medicine bottle, shell or fragment of cut glass. With a little more effort one can look near the edge of the cemetery at the piles of old floral arrangements, jars, and other related paraphernalia and find deep in these heaps the broken remains of the former grave offerings that have been raked away.

Along with the floral decorations now placed on the burial it appears as if styrofoam objects may be taking over to a degree. It is not uncommon to see clocks and an occasional guitar dressing up the grave.

Monuments or grave markers vary considerably. Some had none, stakes and boards were common (Figs. 5 & 6) but may not last and the most recent innovation has been the ones hand-made from concrete with the inscription put in with a stick or the finger before the concrete hardens. Occasionally commercially-made monuments of granite are seen. In graves from early in this century it was common to see grave pits lined with brick then covered over with a concrete slab forming a subterranean vault with the name of the deceased written in the concrete along with shell ornamentation. It is also common to see grave offerings placed in the concrete. One was located near the coast that had made use of a discarded toilet tank for a monument (Fig. 7). Another example from Georgia included the use of a toilet bowl placed on the grave as a planter (Wightman and Cate 1955: 22). The possibilities are unlimited and seem to vary regionally.

This brief encounter with Black burials has pointed up several things the archaeologist working in the Southeast should be aware of. Nineteenth and twentieth century "junk" found scattered in the woods may be an indi-
FIGURE 6: The remains of a carved wooden head marker from an early grave site.

FIGURE 7: An inverted toilet tank used as a grave monument.
cation of a burial location. There need not be markers but even if there were and they were only stakes they can still be found if searched for. The graves will not have regular orientation and intrusion into one another is common. While excavating, location of isolated teeth or appendages may well be attributed to a substitute burial and a face down burial was probably done to trick a malevolent spirit. It is also possible to find the remains of a small prayer structure associated with the cemetery.

The Black burial complex is extremely important to the archaeologist. Being able to recognize the clues which indicate a cemetery have already been used by this investigator. Further study of these practices will add considerably to our understanding of the plantation era and Black history in general. This is also a classic example of how ethnography can be used by the archaeologist to interpret cultural phenomena.

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THE KINGSLEY SLAVE CABINS IN DUVAL COUNTY, FLORIDA, 1968

Charles H. Fairbanks

INTRODUCTION

Although the institution of slavery in America ended slightly over a hundred years ago, there has been no concerted effort by archeologists to study the remains of this cultural institution. Recognizing this hiatus in our research as well as the contributions that excavation could make, the author determined to begin an examination of the material remains of plantations in the Old South. It was hoped that this archeological program, while concentrating on the actual slave quarters, could also investigate the locations of craft activities and perhaps of the actual productive areas of the plantations. The excavations at the Zephaniah Kingsley slave cabins are an early phase of what is hoped will be a long-range comparative exploration of the physical remains of slave communities in the South. Because of its proximity to the University of Florida and the relatively complete remains, the Kingsley Plantation offered an ideal beginning point. A modest grant from the Florida Park Service afforded us an opportunity to begin this important work.

The institution of chattel slavery has long interested historians and humanists. While contemporary accounts of Southern slavery have appeared for nearly two hundred years, much is yet to be learned about this peculiar institution. Almost always written from the viewpoint of the superordinate caste, they are generally lacking in specific information about the daily circumstances of the slaves. Such accounts as Fanny Kemble's almost neurotic attack (1863) on slavery failed to give us specific information on how the slaves lived and the details of their housing, crafts, family life, and daily activities. These are supplemented by excellent political and economic studies of the slave system (Olmsted 1861; Flanders 1933; Grant 1954).

It is precisely in this sort of situation that archeology can supplement and extend the understanding offered by written history. Archeology is consistently concerned with process, rather than events, with technology rather than politics. It can thus broaden and enrich the knowledge of our American heritage at a time when that tradition is in the midst of rapid and often baffling change. Some archeology has already been done in the "big houses" of the southern plantations (Caywood 1955; Noël Hume 1962, 1966). Ivor Noël Hume has reported the excavation of a trash pit believed to have been associated with slaves at Tutter's Neck (1966). Adelaide and Ripley Bullen reported the excavation of the home of a freed slave in Massachusetts (Bullen and Bullen 1945). James A. Ford excavated the remains of a sugar mill, presumably slave-operated, on the Georgia coast (Ford 1937). This author participated in the partial excavation of a slave cabin at the Ryefield site on Cumberland Island (Ascher and Fairbanks 1971). There is, however, a general dearth of reports on the excavation of slave areas in the available literature.
The area now known as Kingsley Plantation has a long and colorful history and is fittingly preserved as a unit of the Florida Park System. The original inhabitants were Timucua Indians, but with their disappearance before 1700, the area was inhabited by refugee Guale Indians from the central Georgia coast. At the mission of San Juan del Puerto, about two miles south of Kingsley Plantation, they established a large community under the care of the Franciscans (McMurray n.d.). Although it is probable that some occupation of the island was maintained after the passing of the Guale mission, no clear picture of the later part of the eighteenth century is now available. About 1795 John McQueen received the island as a grant from the Spanish crown. Although McQueen had been an officer in the American Navy during the Revolution, he removed to Spanish Florida, swore allegiance to the Crown, and became known as Don Juan McQueen. Little is known about McQueen's occupation except that he built one or more houses on the island, one reputed to be standing at the plantation.

The next owner was John Houstoun McIntosh, a wealthy planter and the grandson of John Mohr McIntosh. While McIntosh may have built the main house of what is now the Kingsley Plantation, the holdings on Ft. George Island seem to have remained in second place to his earlier Plantation, Refuge, on the mainland near St. Mary's, Georgia. John Houstoun McIntosh became director of the abortive Territory of East Florida and suffered financial reverses as a result of the collapse of that freebooting enterprise (Patrick 1954). From this financial crisis he had been saved by a loan from Zephaniah Kingsley, certainly one of Florida's more colorful citizens.

Zephaniah Kingsley, Scottish born in 1765, had come with his family to Charleston, South Carolina. Arriving in Florida probably as early as 1803, he had already developed an extensive trade in slaves and other commodities with the West Indies. Having taken the required oath, he received a grant of land at Laurel Grove, just south of the present Orange Park in Clay County, Florida (American State Papers, Public Lands, Vol. 4: 440). That plantation remained his home until he acquired the Fort George Plantation in 1813. A minor note of interest is that Zephaniah Kingsley was an uncle of Anna McNeill Whistler who has become famous as the subject of the portrait usually known as "Whistler's Mother". His principal claim to fame, however, rests on the somewhat unconventional establishment he maintained on Ft. George Island.

There is a persistent report that Kingsley maintained not only a slave importing station on Ft. George Island but that he used his extensive plantations as a slave training school (May 1945: 150-151). No direct contemporary confirmation of the existence of such a "school" can be found, but interest in it served as one of the principal reasons for beginning an archeological investigation of the slave quarters at Kingsley Plantation. Whether or not the school did actually exist, Kingsley's Ft. George Plantation was unconventional enough to cause trouble for him and his descendants. His first wife was Anna Madigene.
Jai, the daughter of an African tribal chief with whom Kingsley had business dealings. Zephaniah maintained that they had been married according to tribal custom but he so feared retaliation by the American territorial authorities and by collateral heirs that he removed Anna and her children to Haiti in 1835. There were also other wives while Anna was still in residence. Among these were: Flora H. Kingsley of Camp New Hope, Sarah M. Kingsley and her son with the Indian name of Micanopy (Will of Zephaniah Kingsley) and Munsina McGundo and her daughter, Fatima. To the latter wife he deeded a two-story tabby house on the south end of Ft. George Island. During his lifetime Kingsley realized that this marital arrangement did not meet the contemporary standards of his community and attempted by various devices to assure that his wives and children would properly share in his large estate.

Kingsley was the author of two works on slavery vigorously defending the institution that was already coming under attack in the western world. He published his first defense anonymously in 1828 as: *A treatise on the patriarchal, or cooperative system of society as it exists in some governments, and colonies in America, and in the United States, under the name of slavery, with its necessity and advantages.* By An Inhabitant of Florida. The second edition, published in 1829, carried the endorsement of the author. New editions, all apparently privately printed, appeared in 1833 and 1834. The work is an energetic defense of slavery when it was tempered with "justice, and benevolence". He also wrote *The Rural Code of Haiti* published in 1837. While the two works are largely devoted to a philosophical and economic defense of slavery, he does seem to have been sincere and perhaps to have recognized the fact of hybrid vigor in the offspring of different races.

While in New York, preparing to visit his wife and children in Haiti, Kingsley died in 1843. As he had anticipated, the settlement of his generous estate was prolonged and acrimonious. His will was challenged by the Territorial authorities and by numerous collateral heirs. The complaint was that it was against public policy to allow the progeny of miscegenation to profit. The will was eventually upheld and the heirs received substantial sums. Anna Madigene Jai Kingsley returned to Duval County after the American Civil War, although little is known of her life in those later years.

While Zephaniah Kingsley was a fascinating person, the major factor in beginning an excavation project was the desire to find any evidence of the slave school in plantation crafts which is supposed to have been maintained on Ft. George Island. The remaining slave cabins are an impressive group, possibly one of the most extensive in the southern states. It is hoped that additional excavations can be carried out in the near future.

The excavations during the first two weeks of August, 1968 were carried out by four University of Florida students under the direction of the author. The work of these students, Richard Brezina, Samuel Furgason, Randy Nimnicht, and James Zahler, was part of the regular
Field Session in Archeological Techniques at the University. Subsequently Samuel Furganson served as laboratory assistant in the analysis of the collections.

These slave cabins are located in a broad arc trending east and west some thousand feet south of the main house on each side of a north-south road bordered by cabbage palms (Fig. 1). The eastern segment consists of 16 cabins with the first and last in the series being noticeably larger than the intervening ones. The western segment now consists of only ten cabins, although it is assumed that it originally contained an equal number as the eastern arc. Again the first cabin is the largest. To the west of the standing ruins there are two or more areas of tabby remains that support the assumption of two symmetrical arcs, thus making a total of thirty-two houses. The 1968 excavations investigated the remains of the large cabin immediately west of the road and conducted a limited test in the first cabin of the eastern line, which is scheduled for restoration by the Florida Park Service.

Between the first and second cabins on both east and west sides there is a shallow depression in the ground that suggested the location of a well. The western depression was investigated and did indeed contain the remains of a well.

All of the cabins are of poured tabby with interior fireplaces of both tabby and clay brick. While the tabby is clearly poured it seems to differ in a number of respects from the conventional tabby construction of the Georgia coast. That the shell for the tabby was derived from an Indian shell midden is clear from the presence of a sherd of Spanish Majolica, Aucilla Polychrome (Goggin 1958), embedded in the wall of Cabin #2 of the western arc. Some of the cabins were re-occupied during the ownership of John F. Rollins after the Civil War. A number of the houses have standing walls as high as the eaves line, while in others the walls are only a few feet in height.

THE EXCAVATIONS

The area was staked in a modified grid system so arranged that it covered the first cabins on each side of the road. The grid lines were laid out north and south, although the cabins are not arranged in a true orientation to the cardinal directions. All dirt not troweled out was screened through a powered sifter with diamond mesh 3/4" x 3/8" in size. All artifacts, segregated by level and square, were washed at the end of each day's work and a preliminary inventory prepared. In the laboratory necessary repair was carried out but lack of funds has prevented any systematic conservation treatment of the many iron objects. Horizontal and vertical control was maintained with a transit located adjacent to the excavation on a temporary benchmark.
Cabin #1 West was most extensively tested by means of cross trenches. The first trench, ten feet wide, was started ten feet south of the cabin and carried up to the southern wall (Fig. 2). Within the cabin the trench was reduced to a width of five feet and carried forward to the north wall. From the midpoint of this trench, another trench was dug from east wall to west wall. This latter trench bisected the fireplace located in the middle of the western wall. It was felt that, short of complete excavation, these trenches gave us a highly adequate sample of structural features and artifacts of the cabin.

A test trench through the depression north of the space between Cabins #1 West and #2 West was laid out so that its western wall provided a profile through the center of the anticipated well. It was taken down about three feet below the surface where it became clear that a well was indeed present, as indicated by the remains of a wood casing.

As Cabin #1 East was scheduled for restoration, we felt that extensive excavation would weaken the structure in undesirable ways. A short trench, five feet wide, was cut from the eastern room through the doorway into the western room. It stopped short of the fireplace at the east end of the western wall. The primary results of this trench were to determine original floor level, nature of the floor, and reveal details of the doorway. The tabby floor in the trench was covered with a polyethylene sheet before backfilling in order to facilitate clearing during restoration.

CABIN #1 WEST

Noticeably larger than the remaining cabins in the western arc, the first ruin measures 24.5' WNW - SSE by 18.6' NNE - SSW with walls mostly rising to a height of 7.3' above the present ground surface. The interior of the eastern wall was the best preserved with what appears to be original plastered coating present irregularly for about 1.5' at the top and 2.0' just above the ground level. About 2.0' of the western wall and 11.0' of the southern wall are missing adjacent to the southwest corner. At present there is a door on the north face and two small windows in the eastern half at north and south. The exterior walls are 1.3' thick. The building is divided by a north-south wall 1.0' wide. The eastern room measures 16.1' by 8.2', the western 16.1' by 12.6'. Located in the center of the western wall is a fireplace of simple style but rather elaborate construction. Two cheek walls project 2.2' into the room and are faced with brick on a poured tabby core. This tabby core seems to be an integral part of the western wall and must have been poured at the same time as the wall. The firebox proper as well as the hearth are of clay brick measuring 9 1/2" x 5" x 4 3/4". The parts of the fireplace less exposed to heat are faced with tabby brick measuring 7 1/2" x 3 1/2" x 2".

The tabby walls show layering, but the layers are not of equal height varying from 0.9' to 1.4'. Peg holes are large and rectangular,
KINGSLEY SLAVE CABINS
DU-108

Fig. 2

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measuring about 2" x 4". These construction features differ significantly from the standard Colonial tabby of the Georgia coast with its regular courses and round peg holes. From the irregularity of the coursing, the integral tabby core of the fireplace cheek walls, and the large peg holes, I have come to the conclusion that the walls were poured in one form as is modern monolithic concrete. That is, wooden forms were erected up to eaves-line for the whole structure, including the fireplace core. Batches of tabby, of varying volume were then poured into the forms. This would account for layers or courses of varying height. As soon as the tabby hardened the forms were removed, brick faces added to the fireplace and the chimney constructed of tabby brick. Evidently wooden jambs and sills for doors and windows were placed in the forms before pouring the tabby, although they are not well represented in Cabin #1 West. Finally the relatively smooth interior and exterior walls were roughened by random chopping with what appears to have been a narrow hatchet. On this "scratch coat" a smooth, fine plaster was applied. Erosion has very largely removed this finish coat.

Excavations were begun with a ten foot square outside and overlapping the center of the south wall. The square was cut down in two sections, one to the west, followed by the eastern half. Below the sod, the black humic midden ranged from 0.5' to 1.0' in depth, being deeper away from the house. Beginning at a point 1.3' south of the cabin wall and extending to 1.9' south, there was a shallow depression about 0.2' deep. This appears to be a dripline marking the outer edge of the eaves which would thus have extended out 1.5' from the house.

The upper humus was mixed with shell, mostly oyster, and contained a number of artifacts. These will be discussed at greater length below. It is worth noting, however, that these seemed to be a concentration of objects against the wall of the cabin. In particular we found an iron ax head and an eye hoe close to the cabin wall and just east of the largely obliterated back door. These evidently represent tools stacked against the house and gradually covered with humus.

Below the upper humus was a very thin layer of black sand with very little shell extending out about five feet from the wall. It was not cut by any construction trench and evidently represents humus existing when the house was built. Below this was tan sand, undisturbed, and extending down at least three-and-a-half feet. We found little specific evidence of any aboriginal occupation in the area. While there are scattered Indian sherds in the collections, the lack of any occupational concentration suggests that they were brought in with the shell used in the tabby.

Within the cabin our excavations were a five-foot trench north-south and a similar trench east-west. As it was desirable to intersect both the doorway between the two rooms and the fireplace at a favorable spot, the two legs of the east-west trench were offset five feet (Fig. 2). The humus was a black sand containing numerous artifacts as well as fragments of tabby, brick, and shell. From the condition of much of the
Fig. 4
shell it was probable that some of them represented food remains rather than decayed tabby. Ranging in depth from 0.6' to 1.9', this black humus represented accumulations on the floor as well as debris which had collected after the abandonment of the building. While it was not easy to identify any floor deposit, the black sand immediately in contact with the floor was excavated separately and the artifacts segregated.

The east room was separated from the west by a tabby partition one foot thick. This was cut by a door at the center which is now 5.8' wide. As both faces are now broken and eroded, showing no evidence of form marks, we cannot tell how wide the door may originally have been. In the doorway was a badly eroded tabby door sill, again with no evidence of forms or wooden bucks. Its upper surface was 0.5' below the present surface and it extended downward one foot without any evidence of any construction trench. As black humic sand extended under it, detection of any footing ditch would have been virtually impossible. The eastern exterior wall of the building was 1.3' thick. Again no good evidence of a construction trench could be found. Below the present surface, the face was eroded to a depth of 0.4', evidently the level where recent humus had begun to develop after the cabin had been abandoned. This level, 0.4' below present surface may well represent the floor level of the room, although we could find no definite proof of this. Below the presumed floor level the wall showed the impression of wooden forms for 0.3 feet. From that point to the base of the wall at 1.05' below the present surface the wall was irregular but not eroded.

We can reconstruct the building sequence with fair accuracy from these details. If we assume that the base of the wall forms represents the original land surface, the builders dug a footing ditch about three tenths of a foot deep and about sixteen inches wide. Either a footing was poured in this ditch or forms were erected along it and both wall and footing poured monolithically. I incline to the belief that no separate footing was poured as I could see no pour seam at that point. At any rate, once the walls were poured, dark sandy humus was filled in to a depth of about four inches. We found no evidence of either a packed dirt or a wooden floor. Once the cabin had been abandoned, and probably after the roof collapsed, the tabby wall eroded down to this level. The black humus appeared completely homogeneous from the present surface to a maximum depth of about a foot. Below that was a somewhat mixed zone of tan sand with black disturbances, evidently roots, grading downward to undisturbed tan sand.

It is noteworthy that the wall footings were very shallow and were not spread at all. As the sandy soil here is quite soft this does not appear to be a very stable type of construction. About 85% of the walls, however, are still standing as high as seven feet above the present surface. Evidently tabby walls, lightly founded on soft sand, will stand for at least 150 years.

The western room, with its large fireplace was considerably larger than the eastern room, being 13.3' wide compared to the 8.0' width of the
eastern room. In addition there was a tabby floor throughout virtually all of its area. This tabby had been poured 0.2' to 0.3' thick virtually up to the walls on all sides. The edges were irregular and do not appear to mark the presence of any furred wooden walls. Rather, the tabby simply was spread around with no particular pains to seal it to the side walls. In patches, particularly toward the east, the floor was covered with one to two inches of a grey ashy sand which appeared to be a floor accumulation. The upper surface was somewhat irregular, apparently as much from original uneveness as from wear.

FIREPLACE

Dominating the western wall was a large fireplace with an overall width of 8.7 feet, measuring to the outside of the cheek walls. These walls extended out into the room 2.2' and were composed of a tabby core faced with brick. The actual fireplace opening measured 6.1' wide at the front, narrowing to 4.0' wide at the back wall. While the core and the chimney were built of tabby bricks, the bricks facing the actual fire area were of baked clay. This, of course, makes sense as tabby bricks break down when subjected to heat much more rapidly than do fired clay brick. When we began excavation the fireplace was mounded with brick rubble, tabby mortar, and humus, being the highest point within the cabin. We dug the north half of the fireplace and kept a profile baulk east and west through the center of the hearth. The features will be described from the bottom upward, in the order in which they were originally constructed.

The tabby floor of the westward room was about 0.4' lower in the fireplace area than in the rest of the room although the base of the brick back was at the same general level as the room floor. Some of this depression seems to have been caused by the gradual slaking of the tabby footing under the hearth. Although only brick fragments remained in place at this lower level, we believe that the floor of the hearth was originally laid in clay brick. The hearth area extended out into the room slightly less than a foot being rounded in a gentle curve. The outer edge was a slight depression, 3" wide, which suggested a curb of brick set on edge along the edge of the hearth.

Covering the original hearth level was a compact mass of ash with some fairly fresh shell. This fresh shell was even more concentrated to the front of the hearth, strongly suggesting the remains of oysters roasted in the fireplace. On top of the ash and shell a second hearth had been laid of tabby bricks some 0.5' above the original hearth level. The lack of artifacts in the fill between the two hearths does not allow us to fix the time which elapsed between the laying of the first and second brick pavements.
Fig. 5
THE WELL

About ten feet west and twenty feet north of the northwest corner of the cabin was a roughly circular depression in the otherwise level ground. A trench five feet wide and ten feet long was excavated to a depth of 2.5' across the eastern half of this depression. While the well was important and interesting we had neither the equipment or funds to excavate it completely. As soon as it was clear that we were indeed dealing with a well, the excavations were closed. The well had been dug by the pit method rather than the ring method. A large pit, extending beyond our excavations, had been dug to an undetermined depth. In this square well casing of wood had been constructed and the pit backfilled to this wooden lining. The wooden casing was preserved, although in poor shape. Our heaviest concentration of food bones came from the fill of the well proper, while the pit outside the well casing contained few artifacts of any kind. This strongly suggests that the well was filled with rubbish and that it had been originally dug early in the occupation of the site before much trash had accumulated.

CABIN ONE EAST

As this cabin was scheduled for restoration, we excavated only enough to secure data on the floor level and composition. A single trench, five feet wide and seven feet long was excavated spanning the doorway between the east and west rooms. A hard, fairly even, tabby floor was present in both rooms. The surface of the floor was 0.3' lower in the western room than in the eastern one. The door area showed the impression of a wooden sill 0.4' wide. This had extended 0.5' into the walls at the sides of the door. The vertical faces of the doors showed the impressions of wooden vertical door casing. Very few artifacts were found in this limited cut within Cabin One East.

THE ARTIFACTS

The abundant artifacts recovered in the Kingsley Slave Cabins are important not for dating the site, but for the information they provide about the activities at the site and the style of life there. As with many sites of the historic period, the period of construction and use is well known from a series of written documents. What we hoped to learn was how the people who lived in these cabins carried out the mundane activities of home and family life. The artifacts recovered would also provide items for interpretative exhibits. For these reasons, no exhaustive tabulation of objects by stratigraphic units will be presented. I will, instead, discuss the various categories and styles of artifacts and attempt to draw conclusions about their significance for reconstructing a past segment of American culture.
DOOR SILL DETAILS

DU-108

KINGSLEY PLANTATION SLAVE CABINS

CABIN I EAST

Fig. 6

SCALE—FEET

76
CERAMICS

Ceramics were fairly abundant and comprised a rather broad range from coarse earthenware to thin, white porcelain. The range of types and styles strongly suggests that no specific table ware was provided for the inhabitants, but that perhaps a miscellaneous collection of ceramics found its way into the cabins. The most common single type was a group generally called Ironstone China, 217 sherds (Noél Hume 1966: 131). It is generally thick with a crazed glaze and little or no decoration. Hard and porcelaneous in the body, it is a serviceable table ware, most commonly found after the middle of the nineteenth century. The high proportion of Ironstone China in the Kingsley Cabin evidently derives from occupation just prior to and after the Civil War. The small samples involved here, in badly disturbed contexts, do not distinguish early and late occupational strata. Of probably similar date are fifty-two sherds of white porcelain of better grade than that classified as Ironstone China. Ten sherds are thin and represent fine china, evidently single pieces, perhaps discarded from the main house. The remaining forty-two are thick and represent a better grade of utility ware.

The next most common ceramics were a group of color-decorated refined earthenwares. They include pearlware and white ware and were all small sherds, making detailed analysis difficult. In many cases I cannot determine in which of the bodies a given decorative feature is to be described. I have therefore used these decorative styles as the classificatory units. Twenty-six sherds are shell-edged refined earthenware, mostly on pearlware (Noél Hume 1966: 131). The majority (23) are blue edged with only three being green edged. Execution ranges from neatly impressed shell-edging and a deep pigment to mere painting of the edge with little or no impression. The greatest popularity of this type was probably during the 1820's, which would fall within the earlier part of the site occupation. The greatest concentration was found in the basal floor deposit and in the pit fill of the well.

Gaudy Dutch was fairly common with 34 sherds, mostly of the earlier blue and white substyle (Noél Hume 1966: 129). There seems to be a very little of the more flamboyant polychrome versions of this style. These would seem to date from about 1795 to 1815 and thus from the earlier occupation at the cabin.

Annular ware was the most common refined earthenware type found with 37 sherds. The most frequent form was that of colored horizontal bands, although the Mocha dendritic, wormy, and eye designs are represented. As a group, the banded ware sherds would seem to fit into a time from about 1830 to 1850, although the sample and the individual sherds are both too small for any precise dating (Noél Hume 1966: 131; Van Rensselaer 1966: 337-341).
Transfer printed white refined earthenware occurred 27 times. Blue-on-white was the major color with 20 sherds (Noël Hume 1966: 128-130). Magenta, sepia, and black each occurred twice, while there was only one pink specimen. The collection could date from any decade from 1820 to the end of the century, although the black, sepia, and magenta transfer styles tend to have relative early dates. In the absence of any identifiable marks the collection is of little diagnostic value.

Heavier, utilitarian wares were also present in some quantities. Salt-glazed stoneware was the most common in this group. Forty-five sherds of stoneware seem to have been largely from the common jug. Many have a thin, dull chocolate brown glaze on the interiors and seem to be mid-nineteenth century. Another 26 salt-glazed sherds were classified as Ginger Beer bottles on the basis of curvature, characteristic short neck, or makers marks (Noël Hume 1966: 78-79). One complete mark of

VITREUS STONE [ware]
WARRANTED NOT ABSORB [ANT]
J. BOURNE & SON
CODNOR PARK
NEAR DERBY

was found. This refers to a pottery established by Joseph Bourne in 1812 and moved to Danby in 1856 (Mankowitz and Haggar 1957: 30). The total of 71 salt-glazed stoneware sherds comprise 14% of the 505 ceramic sherds from the excavation. This seems a relatively low percentage for early nineteenth century houses, however, it reflects the fact that the slaves were not commonly supplied with the contents of these jugs and bottles — beer and whiskey.

Earthenware comprised a very small part, only 18 sherds. Two of these are olive jar sherds and probably are derived from the Spanish Mission period site at Du-53 to the south. They may well have weathered out of the tabby made from mission site shell. The remaining sixteen earthenware sherds, all small, have interior lead glaze and cannot be precisely dated.

As a whole the ceramics represent a time span from perhaps as early as 1820 to at least 1850. As this is the documented active life of the site as slave quarters, they offer little in the way of dating. What they do seem to indicate is that the cabin utility and table wares reflect changing ceramic styles for the first half of the nineteenth century. The presence of late eighteenth century creamware certainly represents a style largely outmoded at the time the cabins were used. The other styles are a mixture of types commonly in use in America of the period. Evidently slave cabins were not furnished with a special class of wares.
This differs somewhat from the picture of clothing provided the slaves which was usually of special types judged more durable and "suitable" for laborers (Kemble). I cannot escape the conclusion that the slaves were supplied with dishes discarded from the plantation house.

There are two clusters of sherds in this collection. Each seems to reflect slightly different periods of occupancy of the house. In Group I belong: Shell edged pearlware, Gaudy Dutch pearlware or white ware, annularware, and the transfer printed pearlware and white ware. Group II consists of the Ironstone China and the brown salt-glazed stoneware bottles. Group I types have earlier beginning dates than do the types in Group II. South's Mean Ceramic Date method (1972) was applied to the dating of the Kingsley Cabin, although we know in some detail the dates of occupancy. It was used, rather, to see if the Mean Ceramic Date would offer any clues as to the cultural processes at work there. The assumption was that the Mean Ceramic Date method would reveal the date of a collection where that date was unknown. As the time-spans of the ceramic types were fairly well known I felt that an application of the dating method might show some skewing which in turn might in fact throw some light on the significance of the ceramic assemblage. Mean Ceramic Dates were determined for each of the two groups defined.

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<td>Gaudy Dutch</td>
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<td>Annular wares</td>
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<td>GROUP II</td>
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<td>1860</td>
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<td>1857</td>
<td>Ironstone China</td>
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While I realize that the sample is very small and thus may not be statistically valid, the results are interesting. The total sherd sample had a Mean Ceramic Date of 1845.7, that for Group I was 1819.8, for Group II was 1857.1. It is clear that Group II, and thus the whole sample, dates after the major occupation of the site. Group I, however, with a Mean Ceramic Date of 1819.8 seems early for a site occupied from 1813 to 1843 at the death of Zephaniah Kingsley. The Mean Ceramic Dates for Group II and the collection as a whole fall outside the major occupation.
Figure 7. Artifacts from Kingsley Slave Cabin

A-D Metal buttons
E Latch lifter
F Shutter pintle
G Iron ax
H Large wrought iron spike
I-J Bone buttons
K Milk glass buttons
L Perforated clay bead (at different scale)

The shutter pintle and ax have been cleaned by electrolysis.
for the site. As the slave cabins were presumably not built immediately on Kingsley's moving to the site in 1813, we can probably postulate a mean date of occupation of the cabins of about 1830. This would allow something like 10 years for the construction of the houses and the importation of the occupants to full strength. The Mean Ceramic Date is thus some 10.2 years too early. Coupled with the lack of any clear cut dominance of any one style of sherds in Group I, this suggests that the table wares of the slave cabins represent a gradual discard from the big house of the plantation. As sets became fragmentary and new styles were purchased, the odds and ends found their way to the slave cabins. Only further excavations of slave cabins and further testing of South's Mean Ceramic Date Formula will determine whether these theses are valid. The exercise does, however, suggest ways in which this method may be used for other purposes than dating alone.

ABORIGINAL CERAMICS

Indian sherds were found in a number of contexts totaling 69 pieces. Most of them came from the upper levels of rubble where they were probably derived from the erosion of the tabby walls. As indicated earlier, the shell for the tabby seems to have been taken from the Indian shell middens at Du-53 several miles to the south. Most of the types present at that site were represented: St. John's Plain, St. John's Check Stamped, Sand Tempered Plain, and San Marcos Stamped. The latter was in an extreme minority in marked contrast to its predominance at San Juan del Puerto. The great majority of the sherds, 48 or 68.1% were found outside the cabin to the south. Along with the Indian ceramics was found a sand tempered perforated dumbbell shaped clay object (Fig. 7L). This may be a slave-made bead as it does not resemble any usual Florida Indian artifact. These aboriginal artifacts suggest that there was a minor Indian occupation in the cabin area before the buildings were constructed and probably before the historic period. The material found is not really diagnostic and no cultural affiliation can be made except to say that the sand tempered material does not appear to be either the St. John's tradition or the San Marcos complex.

NON-CERAMIC ARTIFACTS

The rest of the artifacts recovered could be classified under the rubrics metal, stone, and glass. These categories seem to have little usefulness, however, and I will discuss them under the functional headings of building hardware, domestic equipment, clothing, foods remains. This method should throw more light on the activities at the cabin than would the more usual discussion by material classes.
Figure 8. Ceramics from Kingsley Slave Cabin

A-C  Banded pearlware cup sherds
D-F  Direct painted blue-on-white pearlware cup sherds
G-I  Blue shell-edged pearlware plate sherds
J-L  Blue transfer printed plate or saucer sherds
M   Brown salt glazed stoneware bottle base
N   Brown salt glazed stoneware jug sherd
O   Greyish alkaline glazed stoneware sherd
P   White ironstone
Q   Brown lead glazed redware
Fig. 8
BUILDING HARDWARE

Nails were by far the most common object found, the total reaching 718 whole and fragmentary specimens. Of these 663 or 92.3% were cut nails. Only 4.9%, 35 specimens, were wire nails while wrought nails were even less common, 18 or 2.6%. Two nails or 0.2% were copper cut nails. Slightly over half of the nails (394) were found outside the cabin in the southern approach trench. While some of these were probably lost during construction and others represent debris from the collapse of the wooden elements of the building, this distribution is peculiar. Clearly there was no interior lath or little other wooden work within the building. I believe the abundance of nails south of the cabin points to a wooden structure in the area. This may well have been a lean-to shed or porch. The presence of various tools in the same area strengthens this assumption. As we found what appeared to be a drip line from 1.3' to 1.9' south of the southern wall, this shed or porch must have been added late during the life of the building. Perhaps it represents an addition after the Civil War using wood, and nails, salvaged from other cabins.

Hardware other than nails was relatively scarce and related largely to doors. One large and one small pinion were found just to the south of the cabin. The larger pinion evidently came from the back door, while the smaller one evidently belonged to a window shutter (Fig. 7F). A curved latch lifter was also found outside the cabin. Among the miscellaneous iron pieces are a number that could well enough be the less distinctive parts of a simple latch. Within the cabin was found part of a box type door lock. At least two parts of rectangular butt hinges are represented within the cabin. I feel that the box lock and butt hinges are late replacements or additions to the cabin hardware.

Less surely late are fragments of two padlocks. One is a cordate face plate with pivoted brass keyhole cover. The other is an isolated brass keyhole cover. A small iron key from Cabin 1 East almost certainly indicates the presence of at least one padlock there. Evidently the cabins were secured at some time during the period at least. Plantation diaries, such as that of Fanny Kemble, indicate that some slaves were entrusted with the keys to larders and other storerooms. They never seem, however, to record that the residential cabins were usually locked.

The building hardware indicates the expected construction early in the nineteenth century with perhaps repair late, after the invention of wire nail machines. The supposed existence of a frame lean-to at the south must be demonstrated in further excavations.

DOMESTIC EQUIPMENT

Items of domestic equipment were very common and comprise a number of material categories: glass, metal, and clay being the most common.
Of these glass was by far the most abundant, there being 901 pieces recovered. The great majority are common green bottle glass, all badly fragmented. No whole or reasonably whole bottles were recovered, but an inspection of the sherds seems to indicate mainly dip-mold bottles. No clear indications of two-piece molds could be discerned in the small fragments available. The large number of sherds probably does not represent a very large number of whole bottles. The most common type is a tall dark green bottle of about one fifth gallon capacity, to judge from available sherds. A second category is represented by smaller clear glass bottles which probably represent medicine bottles. Two necks are illustrated in Figure 7M and 70.

Flat pieces of glass that may represent window panes or mirrors were very scarce, only 28 small pieces being found. This seems to mean that windows were not glazed. This would agree with the postulated presence of shutters as suggested by the small iron pinion.

Somewhat finer glass was present in small quantities. Several sherds of pressed glass with a wavy line design were found along with a cushion from the base of a wine goblet stem. These sherds evidently represent a small delicate stemmed wine glass or goblet (Fig. 7P). Some fragments of cut and polished glass were found, consisting of two cruet stoppers with expanded, faceted heads and one curved cylindrical piece of faceted glass. This fragment appears to be part of a small handle. These cut glass fragments do not seem to match with any containers found in the excavation. All the pressed and cut glass fragments are of a clear metal of high quality, representing relatively expensive glass tablewares.

Tobacco pipes were relatively scarce, a total of only fifteen pieces being found. Of the eleven stem sherds, two had impressed designs. One decoration is a fine raised grid of squares touching at their corners. The other bears the impressed mark "OUDA", evidently for "Gouda", the site in Holland where many white clay pipes were made in the late eighteenth and nineteenth centuries. The three bowl sherds are the rather large form common in the 1830's and 1840's. It appears probable that all of the pipes are of Dutch manufacture as Gouda exported widely. Two sherds of a short stemmed, plain buff clay pipe were found. This bears every indication of the common pipes of the middle nineteenth century with a detachable cane stem. Widely manufactured in Europe and the United States, they gradually replaced the older white clay pipes with long integral stems. One of the white clay stems has a bore diameter of 4/64", the remaining 10 are all 5/64" bores. The smallness of the sample and the known late date suggested that any attempt to date the pipes by bore diameters would be misleading.

These pipes all appear to be typical of the period and only remarkable for the small quantity recovered. I suspect that the scarcity does not reflect the true frequency of smoking at the cabins, wooden or corn cob pipes may well have been in use. Even if Kingsley did not supply his slaves with tobacco we would expect that it would have been grown in the gardens that the slaves were allowed to tend in their free time.
SUBSISTANCE

As might be expected, remains of foods, food preparation equipment, and food procurement were found in some abundance. Perhaps the most surprising artifacts fall into the final category, that of food procurement. Several spherical lead balls, and one somewhat expended musket flint were found in the debris on and above the floor. These balls are between 50 and 60 caliber, evidently representing shot for hunting pieces. It is a common statement of the numerous plantation journals that firearms were not allowed in the hands of slaves. A partial exception was that some individuals were designated hunters for the master's table. The fact that these musket balls were found in what appears to have been a slave driver's cabin strongly suggests that others besides hunters did in fact have access to guns, at least on occasion. Cylindrical, perforated lead weights were also found that strongly resemble modern weights for small cast nets. There is thus a strong suggestion of fairly regular hunting for game along with fishing for shrimp or perhaps mullet.

Food remains consisted of the bones of fish, cattle, pigs, raccoons, turtles, clams, and oysters. The largest collection came from the fill around the well. Clam and oyster shells formed a sizeable heap in the fireplace, spreading out onto the adjacent floor. These evidently represent the remains of shellfish roasted in the cabin. Three small fragments of corncob were the only remains of plant foods recovered. We anticipate that the excavation of the well will produce a much broader range of vegetable remains.

Cooking equipment was rather poorly represented, consisting of fragments of a cast iron skillet and a cast iron pot. Both the skillet and pot had three legs, evidently as they were used for cooking in an open fireplace. Fragments of a rather large iron spoon, a three-tined iron fork and two table knives represent eating as well as cooking equipment.

Considering the food remains, cooking equipment, and table ware as a whole gives a fairly complete picture of the food habits of the families at the Kingsley Plantation. While some foods, such as beef and pork, were evidently drawn from plantation supplies, these were extensively supplemented by wild game, fish, and shellfish from the surrounding area. The absence of forest game such as deer suggest that the adjacent tidal streams and marshes were the major hunting area. The raccoon were probably derived from the same marshes where today they are gathered by rural coastal populations. The tidal salt marsh is perhaps the richest part of the coastal environment and has been exploited by men from the earliest times onward. Not only does it produce great quantities of calories per acre, but exploitation is relatively simple once the proper techniques have been learned.

Cooking equipment was at a minimum and would not serve for any elaborate cuisine. While some table wares were of finer types, they appear to have been odds and ends discarded from the main house. It is
clear, however, that cooking was done at the cabin fireplace, at least for the shellfish, and probably for the other game foods as well. This is in rather sharp contrast to the usual pattern of plantation food customs which involved central preparation and distribution of cooked meals to the workers. To what extent this was a peculiarity of Kingsley's Plantation is not yet clear.

Two small pieces of tannish, probably foreign, flint have been heavily battered along the edges and corners. The spalls are very irregular and show no evidence of skilled knapping. They evidently represent flint strike-a-lights which were locally made. No accompanying steels were found, perhaps because any casual piece of steel would serve.

Just outside the south (rear) door were several pieces of a cast iron stove frame. I suspect that this stove was a late addition to the cabin, perhaps in the period of the Rollin's ownership after the Civil War.

FARMING EQUIPMENT

This category was rather disappointing as we had hoped to find evidence of specialized craft instruction if Kingsley did in fact conduct a slave craft school. The range is somewhat limited, the sort of collection that might be expected at any small southern farm. Near the back door were a heavy ax head, an eye hoe with broad blade, and a cast iron plow share. The ax blade (Fig. 7C) had a number of cuts on the faces and evidently had served as a makeshift anvil at some time. It does suggest some fairly unusual activity, although I cannot select one from several possibilities. The broad hoe is of a common type used from colonial times well into the twentieth century. Other farm equipment consisted of an iron bale from a bucket, an iron strap buckle, a piece of chain, the trace hook from a whippetree, what appears to be a blacksmith's center punch, and a piece of whetstone. The punch strongly suggests the existence of the more advanced craft of smithing, but is the sort of handy tool beloved by handymen the world over. The remainder of these objects are simply the debris of the usual farm activities. None can be closely dated by either material or form.

CLOTHING AND MISCELLANEOUS

This category includes, besides items indicating the clothing complex, those items which can reasonably be regarded as personal property. They give some dim picture of personal appearance and activities of the inhabitants. One large copper U.S. Liberty Head Cent dated 1832 was the only coin to be recovered and clearly indicates that this was not, for the slaves, a money economy.

Buttons were by far the most common and varied item found, probably indicating that clothing was of odds and various ends, like the dishes from which they ate.
Fifteen buttons, classed into at least eight more or less distinct types were recovered. Below, in tabular form the buttons are classified according to the schemes proposed by Olsen (1962, 1963) and South (1964). Measurements are in millimeters.

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Military issue with, in relief, an eagle in flight, olive branch in left talon, arrows in right, below letters.

U.S. in a plain oval wreath. Stamped in reverse: six stars and "United States".

The military button is the most closely dated, while the others mostly range from the rather late eighteenth to mid-nineteenth century. The exceptions are the three white porcelain buttons, evidently from underclothes that date from a somewhat later period. As a whole, the buttons might be expected to have been lost from used, probably reused, clothing during the first half of the nineteenth century. The bone buttons may well have been made locally, either as spare-time work or as part of plantation crafts.
While the buttons give the impression of a rather mixed bag of clothing, the recovery of the iron handle of a flatiron does indicate that clothing was cared for by the inhabitants. Two badly rusted folding pocket knives are probably also to be considered as personal property.

The only item of personal adornment, aside from the double clay earspool of doubtful ascription described under ceramics, was a single pale blue glass bead of the faceted type so common from about 1780 to well into the nineteenth century. In another report (Ascher and Fairbanks) this was referred to as an "Ambassador Bead" because it is of a type given that name in Africa during the same time period. I now have strong doubts that this can represent an item brought from their original home by slaves. These faceted beads, usually in a pale blue metal are quite common in the New World, occurring in large quantities in Seminole graves from about 1780 to well into the nineteenth century.

CONCLUSIONS

The brief excavation of one of the larger slave cabins at the Kingsley Plantation revealed some definitive information on the construction of the cabin that is useful for restoration. It also indicated some specific things about the life of slaves during the period from 1813 to 1843. After Kingsley's death in 1843 it seems highly likely that the cabin was re-occupied at least sporadically. The sherd profile strongly suggests this continued occupation. The sherd assemblage also strongly suggests that the table wares of the cabin were discards from more formal groupings. It seems entirely likely that slave quarters were not supplied with any regular class of slave ceramics. The buttons, containing a number of eighteenth century types, also strongly suggest hand-me-down clothing rather than regularly purchased work clothes.

It was surprising that no surely African elements in the material culture could be identified. It has long been known that blacks arrived in this country with nothing but their chains. They did manage, however, to leave survivals of their language and other behavioral traits in the slave culture of the south which survive in Afro-American culture until the present. I felt that the special circumstances of Kingsley being a slave-importing station, and Kingsley's permissive attitude toward his charges, would assure that some elements of African material culture would have been recreated in the plantation situation. Pottery, ornaments, game pieces, or ritual objects might well be expected in such a milieu. We found nothing, however, that could surely be identified as such. The only possible exception was the dumbbell shaped bead of crude manufacture which is simply an exotic piece without any clear cultural identification. A possible explanation is presented that not sufficient time elapsed for the African individuals to recover from the shock of enforced migration and begin the process of recreating
the material aspects of their former cultures. The fact that most
material culture items were supplied by the plantation owner would, of
course, mitigate against such a recreation, except perhaps for highly
specific items that could not be part of plantation supplies.

In matters of diet we were somewhat more fortunate in that we
recovered evidence that at least some food was prepared in the cabin,
food remains being rather abundant. While the standard descriptions
of plantation life usually state that slave rations were prepared in
central kitchens, the evidence is clear that at least some food was
prepared in the cabins. All the Kingsley cabins had fireplaces, the
ones in the three existing larger end cabins being quite commodious.
Cooking seems to have been a regular event in the fireplace excavated
by us. Bones of cattle and pigs may be part of rations issued to
individuals or families. Chicken bones may represent flocks raised
by the slaves. The racoon, fish, and shell fish were present in
ample quantities, representing certainly spare time activity of the
residents in this cabin. Their quantity is enough to strongly suggest
that the wild foods were a significant supplement to the diet. The
marshes and lagoons seem to have been the major source of these wild
foods, reflecting a continuing pattern of exploitation of these
abundant resources that continues among rural and even some urban
people in the area today. The presence of firearms is indicated by
rather scant but nevertheless definite evidence. Here again the
archaeological evidence seems to contradict the available documenta-
tion of plantation patterns.

In spite of our expectations, no evidence of training in, or
practice of, specialized crafts was found. The tools were only those
that might be expected of a normal farm of the period. As the end
cabin in the row was surely a slavedriver or foreman, I have not ruled
out the possibility that further excavation will reveal specialized
craft occupations. Our sample here and of slave cabins in general is
still too small to be able to yet identify such craft occupations on
the basis of specialized tool assemblages.

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VAN RENSSELAER, SUSAN
PART 3
CONTRIBUTED PAPERS

Historical Archeology: Method and Theory
A Debate on Ethnohistory

INTRODUCTION

This part of this volume is in two sections, the first being a series of papers on method and theory in historical archeology. These are contributed papers, though two papers by Stanley South were presented at the 1973 Conference, and are included here because of their emphasis on method and theory. A major paper by Iain Walker is also in this section on method and theory, in which he discusses various theoretical points of view, particularly stimulated by the work of Lewis Binford.

The second section of this Part 3 is a lively exchange of ideas in the form of a debate on ethnohistory between Melburn D. Thurman and James H. Howard, which was originally stimulated by Clyde D. Dollar's "Letter from Mexico" in Volume 3 of The Conference on Historic Site Archaeology Papers, and the film "A Man Called Horse", for which Clyde Dollar was technical director.
Historical Archaeology: Method and Theory

The idea behind the HISTORICAL ARCHAEOLOGY FORUM section of the Conference on Historic Site Archaeology Papers introduced some years ago as a part of this volume was to allow a forum in which thematic topics of concern to historical archaeologists could be examined at some length and to some depth. This thematic grouping of several papers has been followed in this new CONTRIBUTED PAPERS section of the Conference on Historic Site Archaeology Papers by including several papers on method and theory in historical archaeology. The primary difference between this group of papers and a FORUM grouping is the fact that none of the papers have to have been submitted at the Conference.

Several papers by Stanley South are included here due to their thematic relationship, though some were presented in Conferences in 1972, 1973, and 1974. These papers on model building, observation in the archeological process, methodological phases in the archeological process, evaluation of analysis situations, and a plea for a new direction in historical archaeology report writing should be of interest from a methodological point of view.

Iain Walker's paper on Binford's approach to archaeology provides another example of Walker's continuing concern for the examination of theoretical concepts in historical archaeology. This paper may well stimulate response from readers of this volume, in which case it is hoped such replies will be submitted to this editor for consideration for inclusion in the CONTRIBUTED PAPERS section of Volume 8 of these papers.
THE HORIZON CONCEPT REVEALED IN THE APPLICATION OF THE MEAN CERAMIC DATE FORMULA TO SPANISH MAJOLICA IN THE NEW WORLD

Stanley South

In a recent article I outlined an analysis tool in the form of a mean ceramic date formula for use in interpreting the median occupation date represented by English ceramics from British-American sites of the eighteenth century (South 1972: 71). In that study the manufacture period of the ceramic types was used to arrive at a median manufacture date which was applied in the formula, along with the frequency of occurrence of fragments to produce a mean ceramic date. The median manufacture dates were determined from data compiled by Ivor Noel Hume (1970), and up-dated through an interview with him. From a total of sixteen sites for which the median historic occupation dates were known, the ceramic formula tended to overestimate the median historic date by 1.1 years, on the average (South 1972: 217-218).

The explanation of why the ceramic formula dates tend to parallel the historic median occupation dates is seen in the broad and rapid spread of the ceramic types from their sources of manufacture at any one point in time. This horizon phenomenon has been explained by Willey and Phillips (1958: 31-34) as:

a primarily spatial continuity represented by cultural traits and assemblages whose nature and mode of occurrence permit the assumption of a broad and rapid spread.

The archaeological units linked by a horizon are thus assumed to be approximately contemporaneous (Willey and Phillips 1958: 31-34).

It was also suggested that:

Colonial French and Spanish ceramics could also be arranged in a similar historical chronology provided the manufacture dates are known for the ceramic types (South 1972: 76).

With this in mind the present study was undertaken to examine the application of the mean ceramic date formula to Spanish majolica in the New World based on the data compiled by John M. Goggin (1968).

Constructing the Majolica Model

The first step in constructing a majolica model was to examine Goggin's majolica types and the temporal brackets assigned by him. Twenty-three majolica types were used to determine median dates, and these are shown in Figure 1.
### MAJOLICA TYPES WITH GOGGIN DATES AND SOUTH INDEX DATES

<table>
<thead>
<tr>
<th>Majolica Type Ref. Number</th>
<th>Majolica Type Name</th>
<th>Goggin Date Range (ca.)</th>
<th>Ref. Page No. (Goggin 1968)</th>
<th>Goggin Median Date</th>
<th>South Index Date</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Columbia Plain</td>
<td>1493-1650</td>
<td>124</td>
<td>1572</td>
<td>1535</td>
</tr>
<tr>
<td>2</td>
<td>Isabela Polychrome</td>
<td>1490-1560</td>
<td>128</td>
<td>1525</td>
<td>1445</td>
</tr>
<tr>
<td>3</td>
<td>Yaya1 Blue on White</td>
<td>1550-1600</td>
<td>130</td>
<td>1575</td>
<td>1532</td>
</tr>
<tr>
<td>4</td>
<td>La Vega Blue on White</td>
<td>1525-1575</td>
<td>131</td>
<td>1550</td>
<td>1507</td>
</tr>
<tr>
<td>5</td>
<td>Caparra Blue</td>
<td>1500-1560</td>
<td>135</td>
<td>1530</td>
<td>1487</td>
</tr>
<tr>
<td>6</td>
<td>Santo Domingo Blue on W.</td>
<td>1550-1630</td>
<td>133</td>
<td>1590</td>
<td>1547</td>
</tr>
<tr>
<td>7</td>
<td>Ichtucknee Blue on Blue</td>
<td>1550-1650</td>
<td>139</td>
<td>1600</td>
<td>1675</td>
</tr>
<tr>
<td>8</td>
<td>Ichtucknee Blue on White</td>
<td>1615-1650</td>
<td>150</td>
<td>1633</td>
<td></td>
</tr>
<tr>
<td>9</td>
<td>San Luis Blue on White</td>
<td>1630-1690</td>
<td>157</td>
<td>1660</td>
<td></td>
</tr>
<tr>
<td>10</td>
<td>Fig Springs Polychrome</td>
<td>1610-1660</td>
<td>154</td>
<td>1635</td>
<td></td>
</tr>
<tr>
<td>11</td>
<td>Blue and Orange Polychrome</td>
<td>1625-1650</td>
<td>166</td>
<td>1638</td>
<td></td>
</tr>
<tr>
<td>12</td>
<td>Puebla Polychrome</td>
<td>1650-1700</td>
<td>180</td>
<td>1675</td>
<td></td>
</tr>
<tr>
<td>13</td>
<td>Puebla Blue on White</td>
<td>1700-1850</td>
<td>194</td>
<td>1775</td>
<td></td>
</tr>
<tr>
<td>14</td>
<td>San Luis Polychrome</td>
<td>1660-1720</td>
<td>169</td>
<td>1690</td>
<td></td>
</tr>
<tr>
<td>15</td>
<td>Abo Polychrome</td>
<td>1650-1700</td>
<td>172</td>
<td>1675</td>
<td></td>
</tr>
<tr>
<td>16</td>
<td>Aranama Polychrome</td>
<td>1750-1800</td>
<td>198</td>
<td>1775</td>
<td></td>
</tr>
<tr>
<td>17</td>
<td>Aucilla Polychrome</td>
<td>1650-1685</td>
<td>163</td>
<td>1668</td>
<td></td>
</tr>
<tr>
<td>18</td>
<td>Tallahassee Blue on W.</td>
<td>1635-1700</td>
<td>159</td>
<td>1668</td>
<td></td>
</tr>
<tr>
<td>19</td>
<td>Castillo Polychrome</td>
<td>1685-1704</td>
<td>185</td>
<td>1695</td>
<td></td>
</tr>
<tr>
<td>20</td>
<td>Mt. Royal Polychrome</td>
<td>mid-century</td>
<td>161</td>
<td>1650</td>
<td></td>
</tr>
<tr>
<td>21</td>
<td>Puaray Polychrome</td>
<td>1675-1700</td>
<td>183</td>
<td>1688</td>
<td></td>
</tr>
<tr>
<td>22</td>
<td>San Agustine Blue on W.</td>
<td>1700-1730</td>
<td>189</td>
<td>1715</td>
<td></td>
</tr>
<tr>
<td>23</td>
<td>Huejotzingo Blue on W.</td>
<td>1700-1900</td>
<td>195</td>
<td>1800</td>
<td></td>
</tr>
</tbody>
</table>

**FIGURE 1**

Using the Goggin median date for the majolica types the ceramic formula was used with eight collections for which there was controlling historic occupation period information.

Where $X_i$ = the Goggin median date for each majolica type

$$f_i$$ = the frequency of each majolica type (sherd count)

$n$ = the number of ceramic types in the sample

The mean ceramic date, $Y$, is expressed:

$$Y = \frac{\sum_{i=1}^{n} X_i \cdot f_i}{\sum_{i=1}^{n} f_i}$$
The eight collections for which historic median occupation dates are known are shown in Figure 2, revealing a comparison between the known historic occupation median date and the ceramic formula date. This comparison reveals that the five earliest sites have a ceramic formula date averaging over forty-two years later than the known historic median occupation date. This lack of correlation between formula date and historic occupation date is seen in the graph on the left in Figure 3. The three sites from the seventeenth and eighteenth centuries reveal a reasonable degree of correlation between the historic median occupation date and the formula date, but clearly something must be done to the earlier sites in the model to insure a greater correlation between the two sets of dates.

In order to produce a majolica model formula meeting this requirement an index date was assigned to the first seven majolica types to replace the median date derived from Goggin's data (Fig. 1). Columbia Plain majolica was assigned an index date 37 years below the Goggin median date, and Isabela Polychrome majolica was assigned an index date 80 years earlier than the Goggin Median Date, with Types 3 through 6 being assigned dates 43 years lower than the Goggin median dates. Type 7, Ichtucknee Blue on Blue, was assigned a date 75 years later than Goggin's Median Date. This was done to bring the seventeenth century sites into line with the generalized dates assigned by Goggin, and recent studies on seventeenth century sites (Deagan 1972; Milanich 1972).

Using the assigned Index Date for the first seven majolica types, and Goggin's Median Date for the remaining types, the Mean Ceramic Date Formula can be used to determine a date for use in interpreting the median occupation date represented by the sample. The result of this adjustment to produce the model is seen in the table in Figure 2, with the sherd counts for these eight collections in Appendix I. The sum of the differences between the historic median dates and the formula dates using the Goggin Median Dates is seen to be plus 210.4 years for the eight collections, whereas using the Index Dates for the first seven majolica types and Goggin's Median Date for the remaining types produces a sum of differences of only 5.5 years. The comparison between the historic median and the formula dates using the Goggin Median Dates entirely, and using the combination Index Date and Goggin Median Date, can be seen in the graphs in Figure 3.

With our adjusted model producing a sum of differences for the eight collections of only 5.5 years, for an average overestimate of the majolica formula of .69 years above the known median dates, we can have some degree of confidence in our model. In order to infer from this small sample the range in which the total population mean might fall, we use the formula (D. South 1972: 165):

\[ \bar{Y} \pm t\alpha/2 \times \frac{S}{\sqrt{n}} \]

From this (Fig. 2) we determine that there is a 95% confidence that the total population mean (μ) would fall between 6.217 and -4.837. This, plus the fact that the majolica formula overestimates the known historic median...
COMPARISON OF HISTORIC MEDIAN DATE, GOGGIN MEDIAN DATE, AND MAJOLICA FORMULA DATE SHOWING DIFFERENCE, MEAN, AND STANDARD DEVIATION

<table>
<thead>
<tr>
<th>Site Ref. No.</th>
<th>Site Name</th>
<th>Historic Median Date</th>
<th>Goggin Median Date</th>
<th>Difference Date</th>
<th>Majolica Formula Date</th>
<th>Diff.</th>
<th>Diff.²</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Falcon, Texas</td>
<td>1770</td>
<td>1777.2</td>
<td>+ 7.2</td>
<td>+ 7.2</td>
<td>51.84</td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>Aranama, Texas</td>
<td>1771</td>
<td>1773.0</td>
<td>+ 3.0</td>
<td>+ 3.0</td>
<td>9.0</td>
<td></td>
</tr>
<tr>
<td>7</td>
<td>San Luis, Fla.</td>
<td>1697</td>
<td>1684.0</td>
<td>- 13.0</td>
<td>- 13.0</td>
<td>169.0</td>
<td></td>
</tr>
<tr>
<td>19B</td>
<td>La Vega Vieja, Dom. Rep.</td>
<td>1528.5</td>
<td>1567.8</td>
<td>+ 39.3</td>
<td>1528.5</td>
<td>0.0</td>
<td></td>
</tr>
<tr>
<td>19A</td>
<td>La Vega Vieja, Dom. Rep.</td>
<td>1528.5</td>
<td>1571.3</td>
<td>+ 42.8</td>
<td>1534.0</td>
<td>5.5</td>
<td></td>
</tr>
<tr>
<td>20</td>
<td>Nueva Cadiz, Venezuela</td>
<td>1530.0</td>
<td>1570.2</td>
<td>+ 40.2</td>
<td>1532.5</td>
<td>2.5</td>
<td></td>
</tr>
<tr>
<td>21</td>
<td>Jacagua, Dom. Republic</td>
<td>1536.5</td>
<td>1570.5</td>
<td>+ 34.0</td>
<td>1532.0</td>
<td>- 4.5</td>
<td></td>
</tr>
<tr>
<td>23</td>
<td>Isabela, Dom. Republic</td>
<td>1498.0</td>
<td>1554.9</td>
<td>+ 56.9</td>
<td>1502.8</td>
<td>4.8</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>210.4</td>
<td>5.5</td>
<td></td>
</tr>
</tbody>
</table>

\[ t_a/2 = \text{number used when confidence coefficient desired is } x \text{ percent and } n \text{ observations are used} \]

\[ \bar{Y} = \frac{\sum Y_i}{n} = 5.5 + 8 = .688 \]

\[ S = \sqrt{\frac{n \sum Y_i^2 - \left( \frac{\sum Y_i}{n} \right)^2}{n-1}} = \sqrt{\frac{309.63 - (5.5)^2}{8}} = \sqrt{43.69} \]

\[ S = 6.61 \]

\[ S^2 = 43.69 \]

\[ t_a/2 = 2.365 \text{ with a confidence factor of 95% (Weast 1968:A-161)} \]

\[ \bar{Y} \pm t_a/2 \frac{S}{\sqrt{n}} = .69 \pm (2.365) \frac{6.61}{\sqrt{8}} = .69 \pm 5.527 \]

Figure 2
COMPARISON OF THE CERAMIC FORMULA DATES WITH THE MEDIAN HISTORIC DATES USING THE GOGGIN MEDIAN AND THE INDEX DATE ON MAJOLICA SAMPLES FROM EIGHT SITES

UNADJUSTED MODEL
Formula Dates Using Goggin Median Dates for all Majolica Types
Ceramic Formula Date
Median Historic Date

ADJUSTED MODEL
Formula Dates Using Index Dates for Seven Majolica Types

Figure 3
100
dates by an average of only .69 years, allows us with some confidence, to apply our model to data from sites for which the historic dates are not known, but for which there is some relative chronological data derived from seriation and stratigraphy. If our model formula replicates the temporal sequence revealed through seriation and stratigraphic excavation, we have additional data to support the validity of our analysis tool.

Application of the Ceramic Formula to Goggin’s Stratigraphic Data

At Huejotzingo, Mexico, Goggin has stratigraphic data by six inch levels to a depth of 54 inches (Goggin 1968: 99). Application of the ceramic formula to this data revealed the following sequence.

<table>
<thead>
<tr>
<th>Level</th>
<th>Formula Date</th>
</tr>
</thead>
<tbody>
<tr>
<td>0-6&quot;</td>
<td>1727.7</td>
</tr>
<tr>
<td>6-12&quot;</td>
<td>1698.8</td>
</tr>
<tr>
<td>12-18&quot;</td>
<td>1697.9</td>
</tr>
<tr>
<td>18-24&quot;</td>
<td>1654.6</td>
</tr>
<tr>
<td>24-30&quot;</td>
<td>1643.3</td>
</tr>
<tr>
<td>30-36&quot;</td>
<td>1636.7</td>
</tr>
<tr>
<td>36-42&quot;</td>
<td>1636.1</td>
</tr>
<tr>
<td>42-48&quot;</td>
<td>1635.0</td>
</tr>
<tr>
<td>48-54&quot;</td>
<td>1635.0</td>
</tr>
</tbody>
</table>

Goggin interprets the 24-30" level as representing an occupation dating around 1650, and the formula date for this level is 1643.3, with the level above having a formula date of 1654.6, which is entirely in keeping with Goggin’s interpretation. The sherd count by majolica type for each level is seen in Appendix 2.

From the Convento De San Francisco, Dominican Republic, Goggin reveals stratigraphic data from levels to a depth of 85 inches (Goggin 1968: 109). Application of the ceramic formula to this data produced the following sequence.

<table>
<thead>
<tr>
<th>Level</th>
<th>Formula Date</th>
<th>Goggin (1968: 113) Interpretive Date</th>
</tr>
</thead>
<tbody>
<tr>
<td>0-8&quot;</td>
<td>1603.3</td>
<td>Post-1800</td>
</tr>
<tr>
<td>8-16&quot;</td>
<td>1605.7</td>
<td>&quot;a sudden increase in European chinaware dating from the second half of the 18th century.&quot; (108)</td>
</tr>
<tr>
<td>16-24&quot;</td>
<td>1547.0</td>
<td>1750-1800</td>
</tr>
<tr>
<td>24-32&quot;</td>
<td>1629.3</td>
<td>1700-1750</td>
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<td>32-40&quot;</td>
<td>1708.2</td>
<td>1650-1700</td>
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<td>40-48&quot;</td>
<td>1649.8</td>
<td>1615-1650</td>
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<td>48-51&quot;</td>
<td>1636.0</td>
<td>1580-1615</td>
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<tr>
<td>51-59&quot;</td>
<td>1557.1</td>
<td>1500-1580</td>
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<tr>
<td>59-67&quot;</td>
<td>1534.5</td>
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</tr>
<tr>
<td>67-79&quot;</td>
<td>1534.8</td>
<td></td>
</tr>
<tr>
<td>79-85</td>
<td>1531.7</td>
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</tbody>
</table>
The bottom seven levels produce a consistent sequence from the second quarter of the sixteenth century to the early eighteenth century. At the 24-32" level, however, there begins a reversal of ceramic formula dates, clearly reflecting a change in the cultural factors relating to majolica that resulted in the consistent sequence observed in the lower levels. One explanation for this phenomenon could be that the site in the area of this stratigraphic cut was subjected to a cultural use varying dramatically from that represented in the deeper levels of the deposit, resulting in a greater disturbance of the ground in the higher levels. However, another, more likely explanation is seen in cultural phenomena of a broader scope, i.e. a dramatic change in the role played by majolica in the culture represented by the deposits above the 32" level. The explanation is clearly seen in Goggin's statement that with the 24 to 32" level there was "a sudden increase in European chinaware dating from the second half of the 18th century" (Goggin 1968: 108). This decrease in the importance of majolica in the culture is also reflected in the fact that from the 32' level up to the surface only 32 majolica sherds were recovered, whereas below this more than this number were recovered from each level. It is clear then, that the ceramic formula has sensitively reflected cultural phenomenon, in terms of a temporal sequence in the lower levels when majolica was a major cultural item, and through a discontinuity of dates in the upper levels when majolica had virtually been replaced by European ceramics in the eighteenth century. The fact that the ceramic formula dates reflect this phenomenon is an indication of its sensitivity as a research tool. The majolica sherd counts for each level of this stratigraphic excavation are seen in Appendix 3.

Application of the Ceramic Formula to Goggin's Seriation Chart

Using seriation Goggin placed collections of majolica from 23 sites in a temporal sequence which he illustrates in his Figure 1 (Goggin 1968: 25-27). The 23 sites have been assigned a reference number according to the sequence revealed by Goggin's seriation. If the majolica ceramic formula is a valid research tool it should replicate the seriation sequence constructed by Goggin. If it can be seen to do this, we have additional support for the formula as a tool for deriving a date of value in interpreting the occupation period represented by the majolica sample from historic sites of the sixteenth, seventeenth and eighteenth centuries. The following is the sequence of ceramic formula dates compared with the seriation sequence derived by Goggin, and with the historic median dates.
### COMPARISON OF THE CERAMIC FORMULA DATES WITH GOGGIN'S SERIATION CHART
(Figure 1 in Goggin 1968: 25-27)

<table>
<thead>
<tr>
<th>Site Reference Number and Name in Goggin Seriation</th>
<th>Ceramic Formula Date</th>
<th>Historic Median Date and Goggin Comment</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 Falcon Reservoir, Texas</td>
<td>1777.2</td>
<td>1770</td>
</tr>
<tr>
<td>2 Aranama, Texas</td>
<td>1773.0</td>
<td>1771</td>
</tr>
<tr>
<td>3 Quiburi, Arizona</td>
<td>1770.3</td>
<td></td>
</tr>
<tr>
<td>4 N. Senora de la Leche, Florida</td>
<td>1718.9</td>
<td></td>
</tr>
<tr>
<td>5 Pine Tuft, Florida</td>
<td>1676.0</td>
<td></td>
</tr>
<tr>
<td>6 Zetrouer, Florida</td>
<td>1677.3</td>
<td></td>
</tr>
<tr>
<td>7 Fort San Luis, Florida</td>
<td>1684.0</td>
<td></td>
</tr>
<tr>
<td>8 Scott Miller, Florida</td>
<td>1676.8</td>
<td>&quot;terminal date ca. 1685&quot;</td>
</tr>
<tr>
<td>9 Beaty, Florida</td>
<td>1667.2</td>
<td>&quot;late 17th century&quot;</td>
</tr>
<tr>
<td>10 Wright's Landing, Florida</td>
<td>1653.2</td>
<td>&quot;early 1650's&quot;</td>
</tr>
<tr>
<td>11 Darien Bluff (Pt. King Geo.), Fla.</td>
<td>1639.1</td>
<td></td>
</tr>
<tr>
<td>12 Mt. Royal, Florida</td>
<td>1633.3</td>
<td>&quot;middle of seriation [1640?]&quot;</td>
</tr>
<tr>
<td>13 Fig Springs, Florida</td>
<td>1615.7</td>
<td>&quot;1615-1650 postulated&quot;</td>
</tr>
<tr>
<td>14A Maurica, Venezuela (Rocx 15)</td>
<td>1633.6</td>
<td>&quot;between 1620 &amp; 1645&quot;</td>
</tr>
<tr>
<td>14B Maurica, Venezuela (All units)</td>
<td>1627.2</td>
<td>&quot;between 1620 &amp; 1645&quot;</td>
</tr>
<tr>
<td>15 Punta Mosquito, Venezuela</td>
<td>1620.7</td>
<td>&quot;early 17th century&quot;</td>
</tr>
<tr>
<td>16 Obispo, Venezuela</td>
<td>1646.3</td>
<td>&quot;about 1630&quot;</td>
</tr>
<tr>
<td>17 Richardson, Florida</td>
<td>1620.2</td>
<td>&quot;ca. 1615&quot;</td>
</tr>
<tr>
<td>18 Cepicepi, Dominican Republic</td>
<td>1615.9</td>
<td>&quot;ca. 1600&quot;</td>
</tr>
<tr>
<td>19A La Vega Vieja, Dom. Rep. (1952)</td>
<td>1534.0</td>
<td>1528.5</td>
</tr>
<tr>
<td>19B La Vega Vieja, Dom. Rep. (1953-4)</td>
<td>1528.5</td>
<td>1528.5</td>
</tr>
<tr>
<td>20 Nueva Cadiz, Venezuela</td>
<td>1532.5</td>
<td>1530.0</td>
</tr>
<tr>
<td>21 Jacagua, Dominican Republic</td>
<td>1532.0</td>
<td>1536.5</td>
</tr>
<tr>
<td>22 Juandolio, Dominican Republic</td>
<td>1520.4</td>
<td>&quot;early 16th century&quot;</td>
</tr>
<tr>
<td>23 Isabela, Dominican Republic</td>
<td>1502.8</td>
<td>1498.0</td>
</tr>
</tbody>
</table>

**FIGURE 4**

From this comparison it is evident that there is only a minor difference between the sequence arrived at by Goggin, and that resulting from the application of the ceramic formula. The Fig Springs, Florida site and the Obispo, Venezuela site being those most out of place in the seriation. The majolica sherd counts for each site in the seriation are seen in Appendix 4. The known historic median dates for eight of the collections are also shown in this figure, and as has been pointed out the formula dates overestimate these historic median dates by an average of .69 years, with the greatest discrepancy being the -13 years for the collection from Fort San Luis, Florida (see Fig. 2).

A slight difference is seen in the listing of the sites here from that of Goggin, in that there is a 14A and 14B, and a 19A and 19B. This was done as a check against the ceramic formula. Collections 14A and 14B are from Maurica, Venezuela, with Goggin using the majolica from one excavation unit (Rocx 15) as representative of all those excavated (14A).
The majolica sample 14B represents the entire collection from all units including Rocx 15. The date for the one excavation unit used by Goggin was 1633.6, and the date for all the majolica from all units was 1627.2, a difference of only 6.4 years.

A different comparison is seen in collections 19A and 19B, from the La Vega Vieja, Dominican Republic site. Goggin used 19A, a collection made in 1952, in his seriation. Collections made in 1953 and 1954, and combined, are designated 19B. The 1952 collection from the site produced a ceramic formula date of 1534.0, and the combined collections of 1953 and 1954 produced a formula date of 1528.5, only 5.5 years apart, with 19A being 5.5 years removed, and 19B the same as the historic median date of 1528.5. The majolica sherd counts for all collections used in the seriation are seen in Appendix 4.

Application of the Ceramic Formula to Various Archeological Sites

A number of collections of majolica from various sites were discussed by Goggin that were not used in his seriation. Those for which he had some temporal comment are included here along with the ceramic formula date.

<table>
<thead>
<tr>
<th>Site</th>
<th>Ceramic Formula Date</th>
<th>Goggin's Temporal Range and Comments</th>
</tr>
</thead>
<tbody>
<tr>
<td>Awatovi, Arizona</td>
<td>1668.6</td>
<td>1629-1680</td>
</tr>
<tr>
<td>Tumacacori, Arizona</td>
<td>1777.1</td>
<td>1701 -</td>
</tr>
<tr>
<td>Kuaua, New Mexico</td>
<td>1675.0</td>
<td>before 1680</td>
</tr>
<tr>
<td>Puaray, New Mexico</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(First Sample)</td>
<td>1678.6</td>
<td></td>
</tr>
<tr>
<td>(Second Sample)</td>
<td>1747.7</td>
<td></td>
</tr>
</tbody>
</table>

Goggin says that these two samples apparently represent "two occupations, one previous to the revolt of 1680 and a second in the 18th century" (Goggin 1968: 84). The formula dates support this interpretation.

Adaes, Texas

1737.6

1721-1773

Goggin felt that there must have been two settlements represented by this collection because of the presence of Types 12 and 15 of the seventeenth century, and the presence of 37 fragments of Type 13 of the third quarter of the eighteenth century. One settlement he thought would have been "about 1680" and the other during the documented period of 1721-1773 (Goggin 1968: 81). However, the ceramic formula indicates a date only 9.4 years from the known historic median date for the eighteenth century occupation of the site. Types 12 and 15 reveal a ceramic formula date of 1675.0, with Types 22 and 13 producing a formula.
date of 1770.5, which is certainly in keeping with Goggin's interpretation, if we divide the collection as Goggin did.

Fox Pond, Florida  
1635.1 1630-1650

Middle Plateau Trading Post, Macon, Georgia  
1684.2 1690-1710

Goggin felt this sample of 12 sherds "equates perfectly with the supposed date of the trading post" (Goggin 1968: 79), but the ceramic formula date certainly indicates a date earlier than the middle of Goggin's historic time range. The sherd counts for the majolica in these collections are seen in Appendix 5.

Explanation in Terms of the Horizon Concept

The sites from which the majolica collections used in this study came are from a broad area including Georgia, Florida, Texas, Arizona, and New Mexico, as well as Mexico, Venezuela, and Dominican Republic. Any patterned relationships existing between majolica types having temporal consistency, such as demonstrated through the application of the ceramic formula in this study, is a clear indication that there was a broad and rapid spread of majolica throughout the area involved in this study. This is interpreted in terms of the horizon concept of Willey and Phillips (1958: 31-34), with any one point in time being reflected in similar majolica type relationships from contemporaneously occupied sites.

Summary

In this paper we have constructed a model ceramic formula based on data compiled by John M. Goggin from Spanish majolica found on sites in the New World (Goggin 1968). It was found that the median date for six sixteenth century majolica types was too late for producing a ceramic formula date closely approximating the median historic occupation date for the sites for which these dates are known. One seventeenth century type was seen to have a median date too early to produce formula dates closely approximating Goggin's estimates for seventeenth century sites. Because of this an Index Date was assigned to these seven majolica types, and when these dates were used along with Goggin's median ceramic dates for seventeenth and eighteenth century sites, the ceramic formula model produced dates that are seen to overestimate the known historic median occupation date for the sites by an average of only .69 years.

Using this majolica model formula with the stratigraphic data gathered by Goggin it was found that the ceramic formula dates closely replicated the stratigraphic sequence. Applying the ceramic formula to the sites
used by Goggin in his seriation chart also produced a close replication of the sequence arrived at by Goggin using traditional seriation methods.

The fact that the majolica formula is seen to work as well as it does within the limits of the Goggin data illustrates that it is likely a reliable means of expressing the Goggin data. This study has attempted to construct a model based on Goggin data and expressed in terms of a formula, that can be used to compare with data from sites not included in this study and for which there is some chronological control other than majolica. Since Goggin’s data was used to construct the formula, the formula cannot then be tested by reference to the same data. Internal consistency between the model formula and Goggin’s data can be demonstrated, such as we have done with the seriation and stratigraphic data comparisons and comparisons with collections from sites of known occupation periods. Testing, however, in terms of reliability must come through application of the formula to data lying outside that used by Goggin. If subsequent research demonstrates that the formula is invalid for dating majolica collections, then this may reflect an area where the formula was not internally consistent with Goggin’s data, or it may represent a need to adjust Goggin’s conclusions in the light of new evidence, and thereby the Index Dates whereby the formula date is derived.

Since the majolica formula is a model designed to express the Goggin data through statistical means, we are free to manipulate the Index Dates toward the end of producing consistent ceramic dates from the formula that are in keeping with the Goggin data. It is not necessary, therefore, that the Index Dates correlate with Goggin’s estimates for the time period during which each majolica type was being deposited on occupation sites, so long as the resulting mean ceramic date obtained from the formula is reasonably consistent with the chronology outlined by Goggin. The Index Date represents, therefore, a functional expedient with unlimited flexibility for use in arriving at ceramic formula dates that can be used, with some degree of reliability, as an interpretive aid in establishing the occupation period represented by majolica samples. The Index Date is not the median manufacture date such as was used in constructing the Mean Ceramic Date Formula for British Ceramics (South 1972), nor does it represent the period of maximum use of the majolica type to which it is assigned. It is an index number designed to produce consistent results from the majolica formula that are internally consistent within the Goggin data. As more data become available specifically dating sites on which majolica is found, using controls other than majolica, the Index Dates assigned here may well have to be revised to accommodate the new data. Cultural variation may well be found to be reflected in the formula dates, for instance Indian-occupied as opposed to Spanish-occupied sites, where we may find that the formula dates from Indian-occupied sites will be earlier than Spanish-occupied sites of the same time period. As we discover and program new data into the majolica formula we should eventually have a formula that will be so firmly rooted in research that its reliability will be high enough to allow it to become a basic chronological tool.
When the above point is understood it should be easily seen how this concept could be applied to prehistoric ceramic sequences for which there is a well-defined series of ceramic types within a relatively short period of time, and for which there is some comparative control, such as dendrochronology or two or three radiocarbon dates. A firmly established seriation such as this, verified by stratigraphic control could be the basis for constructing a model where index dates were assigned to the various ceramic types, using the radiocarbon or cross dating dates as control for the chronology. Once such a model was constructed, the South Mean Ceramic Date Formula used in the majolica study and in the study of British ceramics could be applied. The formula dates would first have to be seen to have internal consistency within the sequence used to construct the model, then the formula could be tested by application to site collections in the same area where the ceramic types are found. Once reliability was demonstrated by temporal controls other than those of the ceramics themselves, the formula could be applied with confidence that the resulting mean ceramic date could be used to interpret the occupation period represented by the ceramic collections with perhaps a more sensitive degree of temporal separation than is now enjoyed through traditional seriations. An important application would be in quick relative temporal placement of a site from a surface survey, where pottery is the primary data recovered. The application of the formula to prehistoric collections should focus on temporally confined ceramic sequences for the most effective model.

As was emphasized in my paper in which the mean ceramic date formula was used to analyze ceramics from British American sites, the explanation for why the formula works as it does relates to the fact that there was a broad and rapid spread of these artifacts at any one point in time (South 1972). The fact that the ceramic formula is seen to be applicable to majolica collections as demonstrated in this paper illustrates that the horizon is the cultural phenomenon responsible for this patterning. It is emphasized that any site not subject to the trade contacts producing the broad and rapid spread of majolica, would obviously not produce data lending itself to analysis by means of the ceramic formula due to the absence of majolica from such sites.

The patterning in the archeological record seen in our research in British ceramics and Spanish majolica and expressed through the formula, has been explained in terms of culture process by means of the horizon. This does not mean, however, that the use of the formula would have to be limited to the horizon as the explanatory phenomenon. In some instances it may be found that the formula can be applied to data representing a tradition within a restricted geographic area. Answers to questions such as these, however, can only come through the application of the formula model concept to the archeological data.

The formula approach presented here and in the analysis of ceramics from British-American sites (South 1972), has implications far beyond
the use of formulas for analysis of historic ceramics. Richard Carrillo (1972) has used this same conceptual base in an analysis of English wine bottle attributes to construct a statistically based chronology which provides a means for independent temporal comparison with that derived from use of the ceramic formula. Much broader implications are inherent in the formula approach in that if seriations anchored in historical control (such as Goggin's) are valid then we may have some assurance that prehistoric frequency seriations constructed in a like manner might have validity. If the cultural data upon which such seriations are based can be seen to be reliably expressed in terms of statistical formulas, then we will have moved toward a better understanding of culture process represented by the archeological record.

The following is a list of the events in the process of development of the majolica formula, and a paradigm of the role of the formula model in explaining culture process from the archeological record.

SEQUENTIAL EVENTS IN MAJOLICA RESEARCH

1. Majolica type manufacture period unknown.
2. Majolica types accumulated on occupation sites.
3. Goggin collected majolica from occupation sites of known historic periods.
4. Majolica types were assigned temporal brackets based on occurrence or non-occurrence on sites of known historic periods.
5. Majolica collected from sites of unknown historic period was used to assign interpreted occupation period for the site.
6. Stratigraphic tests were used to clarify the temporal relationship of majolica types.
7. Seriation was used to aid in determining the temporal position of sites for which no documented period was known.
8. Seriation was used to clarify temporal relationships among majolica types.
9. Sites of known occupation were used as a controlling framework for the seriation.
10. Goggin's majolica median dates were used with South's Mean Ceramic Date Formula to test the fit of the formula to majolica data.
11. Index Dates were assigned to seven majolica types to adjust the fit of the formula dates to the documented median occupation dates for sites and Goggin's estimates of the occupation period represented by majolica collections from occupation sites.
12. Formula dates were compared with Goggin's stratigraphic test to check for internal consistency of the formula to the strata dates assigned by Goggin.

13. Formula dates were compared with Goggin's seriation sequence of sites based on majolica types.

14. The formula dates were seen to have a high degree of correlation to the median historic occupation dates and with Goggin's estimates of the occupation period represented by the sites from which the majolica samples were recovered.

...The extent of present research...

15. The next step is to test the formula by applying it to majolica samples from sites where there is some independent temporal control: historical documentation, artifact analysis of known artifact types, cross dating of artifact types of known temporal period, dendro-chronology or radiocarbon dating.

16. If the formula dates for majolica from many such sites can be statistically demonstrated to have a high degree of correlation with the independent temporal control prediction, then confidence can be placed in the reliability of the formula dates.

17. When this point is reached the formula can, for the first time, be reliably used to arrive at a date upon which interpretation can be made as to the occupation period represented by the majolica sample from an archeological site.

18. When such reliability is established we will have demonstrated that the patterning in the archeological record resulting from culture process can be expressed by means of a formula. In so doing we will hopefully have taken a step toward testing some of our assumptions regarding frequency seriation, and toward the eventual application of the formula concept to prehistoric data.
The Mean Ceramic Date, Y, is expressed as:

\[ Y = \frac{\sum_{i=1}^{n} x_i f_i}{\sum_{i=1}^{n} f_i} \]

Where \( x_i \) = median manufacture date, median deposition date, or assigned index date

\( f_i \) = frequency of each ceramic type

\( n \) = number of ceramic types in sample

**The Mean Ceramic Date Formula**

---

**Explanation of the Culture Process Represented by Archeological Data**

**THE GOAL**

- Reliably obtaining mid-date represented by the ceramic collections from sites of unknown occupation period

**APPLICATION OF THE FORMULA AS A DATING TOOL**

- DETERMINATION OF STATISTICAL CONFIDENCE AND CORRELATION CHRONOLOGY

**TESTING FOR VALIDATION OF THE FORMULA**

---

**Paradigm of the Role of the Formula Model in Explaining Culture Process from the Archeological Record**

---

**Figure 5**

**Paradigm of the Role of the Formula Model in Explaining Culture Process from the Archeological Record**

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**Levels of Present Research Toward A Formula Model**

**HISTORICAL ARCHAEOLOGY**

**PREHISTORIC ARCHAEOLOGY**

---

**Stanley South, Archeologist**

**INSTITUTE OF ARCHAEOLOGY AND ANTHROPOLOGY**

**University of South Carolina**
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WEAST, ROBERT C.  

WILLEY, GORDON R. AND PHILIP PHILLIPS  
APPENDIX 1

APPLICATION OF THE CERANIC FORMULA TO MAJOLICA COLLECTIONS USING THE GOGGIN MEDIAN DATE AND INDEX DATE FOR CONSTRUCTING THE MAJOLICA MODEL FORMULA

<table>
<thead>
<tr>
<th>Majolica Type</th>
<th>Goggin Median</th>
<th>Sherd Count</th>
<th>Median</th>
<th>Index Product</th>
</tr>
</thead>
<tbody>
<tr>
<td>Site Reference No. 1</td>
<td>FALCON RESERVOIR, TEXAS 1760? - 1780? (Goggin 1968:82)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>13</td>
<td>1775</td>
<td>90</td>
<td>159750</td>
<td></td>
</tr>
<tr>
<td>17</td>
<td>1800</td>
<td>16</td>
<td>28800</td>
<td></td>
</tr>
<tr>
<td>22</td>
<td>1715</td>
<td>1</td>
<td>1715</td>
<td></td>
</tr>
<tr>
<td>16</td>
<td>1775</td>
<td>45</td>
<td>79875</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>152</td>
<td>270140 ÷ 152 = 1777.2</td>
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</tr>
<tr>
<td>Date using Goggin Median = 1777.2</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Site Reference No. 2</td>
<td>ARANAMA, TEXAS 1749 - ca. 1793 (Goggin 1968:82)</td>
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<tr>
<td>22</td>
<td>1715</td>
<td>25</td>
<td>42875</td>
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</tr>
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<td>13</td>
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<td>23</td>
<td>1800</td>
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<td>16</td>
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<td>25</td>
<td>44375</td>
<td></td>
</tr>
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<td></td>
<td></td>
<td>373</td>
<td>661325 ÷ 373 = 1773.0</td>
<td></td>
</tr>
<tr>
<td>Date using Goggin Median = 1773.0</td>
<td>Historic Median Date = 1771</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Site Reference No. 7</td>
<td>FORT SAN LUIS, FLORIDA 1690 - 1704 (Goggin 1968:76)</td>
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<td></td>
<td></td>
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<tr>
<td>14</td>
<td>1690</td>
<td>63</td>
<td>106470</td>
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<td>12</td>
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<td>19</td>
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<td>8375</td>
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<td>9</td>
<td>1660</td>
<td>7</td>
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<td></td>
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<tr>
<td>22</td>
<td>1715</td>
<td>2</td>
<td>3430</td>
<td></td>
</tr>
<tr>
<td>17(?)</td>
<td>1668</td>
<td>1</td>
<td>1668</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>123</td>
<td>207138 ÷ 123 = 1684.0</td>
<td></td>
</tr>
<tr>
<td>Date using Goggin Median = 1684.0</td>
<td>Historic Median Date = 1697</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Site Reference No. 19B (1953-54 Collection)</td>
<td>LA VEGA VIEJA, DOMINICAN REPUBLIC 1495? - 1562 (Goggin 1968:29)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>1572</td>
<td>1535</td>
<td>149</td>
<td>234228</td>
</tr>
<tr>
<td>3</td>
<td>1575</td>
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</tr>
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<td>1525</td>
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<td>7</td>
<td>10675</td>
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<td>5</td>
<td>1530</td>
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<td>11</td>
<td>16830</td>
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<td>4</td>
<td>1550</td>
<td>1507</td>
<td>8</td>
<td>12400</td>
</tr>
<tr>
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### Site Reference No. 19A (1952 Collection)
**LA VEGA VIEJA, DOMINICAN REPUBLIC**
**1495? - 1562** (Goggin 1968:28)

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**NUEVA CADIZ, VENEZUELA (Ex. 5)**
**1515 - 1545** (Goggin 1968:43)

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**1511 - 1562** (Goggin 1968:29)

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**1493 - 1503** (Goggin 1968:24)

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Historic Median Date = 1498.0
### APPENDIX 2

**APPLICATION OF THE CERAMIC FORMULA TO STRATIGRAPHIC DATA AT HUEJOTZINGO, MEXICO** (Table 6 in Goggin 1968:99)

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### APPLICATION OF THE CERAMIC FORMULA TO STRATIGRAPHIC DATA
**AT CONVENTO DE SAN FRANCISCO, DOMINICAN REPUBLIC**
*(Table 12 in Goggin 1968:109)*

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### Application of the Ceramic Formula to Goggin's Seriation Chart

(Figure 1 in Goggin 1968:25-27)

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APPENDIX 4 (Continued)

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APPENDIX 5
APPLICATION OF THE CERAMIC FORMULA TO VARIOUS ARCHEOLOGICAL SITES

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| "two occupations, one previous to the revolt of 1680 and a second in the 18th century."
| Second Sample | 15 | 1675 | 2 | 3350 |
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|               | 13 | 1775 | 8 | 14200 |
|               | 11 | 19225 | 11 | 1747.7 |
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THE FUNCTION OF OBSERVATION IN THE ARCHEOLOGICAL PROCESS

Stanley South

Archeological sites are located through surface survey, aerial photography, resistivity and magnetometer survey, topographic mapping and historical documentation, as well as other survey techniques. Such activity can become so involved that a specialty in such techniques can be developed. However, once the archeologist begins excavation of a site the process of field observation and recording of data is of primary concern. The quality of the observation and recording process has a direct relationship to the problems the archeologist is attempting to solve, in that the sophistication of the hypothesis depends on equally sophisticated field observation for meaningful explanation to emerge.

Traditionally archeologists have dealt with features, postholes and burials, under an implied assumption that "a posthole is a posthole", when careful observation reveals a wide variety of attributes of value in recording and interpreting features for componential analysis. The more distinctions the archeologist draws between features at the observational level, the more sophisticated his hypotheses can become. The Accokeek Creek Site is an excellent example of posthole recording resulting in very limited interpretive data as a result of the lack of distinctions drawn between the various postholes (Stephenson-Ferguson 1963: Fig. 6). Here thousands upon thousands of postholes were recorded by Mrs. Ferguson, but no structures other than a series of palisades could be identified by Robert Stephenson who analyzed the data. If a variety of attributes had been used to draw distinctions between the postholes as they were observed during excavation a number of architectural structures may well have been identified and various components isolated. Many other reports could be cited revealing similar lack of posthole and feature recording based on a wide range of attributes observable in plan at the excavated level of the site. The features illustrated in the chart in Figure 1 reveal various attributes observable in the field that allow for separation of features into classes useful in architectural, componential, functional and cultural identification.

In observing features for multi-attribute recording a consistent recording technique must be utilized, which means that one group of postholes and features is not recorded in plan in one area when the ground is powder dry, and other features recorded when the ground is moist from a recent rain. In order to consistently observe features for recording the excavated level must be kept moist enough to allow for maximum observation. This means an ample source of water for wetting down areas to be observed must be at hand. Fire engines, water wagons, pumps and fire hoses have been used to dump thousands of gallons of water a day on sites I have excavated in order to insure this consistency of observation and recording of the data. The archeologist cannot hope to consistently record the archeological record if he cannot observe it, and yet sites are frequently examined under such dry, baked...
conditions that thorough or consistent data cannot possibly be recovered. Under such conditions the archeologist may well find that his data consists primarily of masonry ruins and other obviously observable features, and he may come to believe that because of this no postholes and other features requiring more sensitive observation are present. Under dry conditions delicate soil distinctions are always lost, and even features that show up dramatically under moist soil conditions will totally disappear when the sand or clay surface is allowed to dry out. Occasionally drying may reveal features through more rapid evaporation of moisture from disturbed areas, and some archeologists are coming to rely on this technique in lieu of moist earth observation. However, relying on this technique in lieu of moist earth observation is like preferring braille over visual observation. It can be used, but is definitely secondary to primary observation of features in moist soil. Certain areas, because of their unique soil conditions, may not lend themselves to moist soil observation, but I believe these would be more the exception than the rule.

Once the features are revealed through removal of the plowed soil zone or other overlying soil layer, the surface must be schnitted (cut clean) using trowels or shovels. Scrapping or brushing of moist soil only obliterates the data to be observed. When this process of schnitting is completed over an area as large as possible, recording of each posthole and feature should be undertaken immediately by the data recording crew. Photographs, elevations, horizontal position, width and shape of feature outlines, and the attributes observable in the fill are recorded, with care being taken by them not to add confusion to the scene by footprints and disturbance of this cleaned level. While the recording process is under way it is often necessary to have men with spray cans of water going over the area constantly spraying a mist of water to keep the soil in good condition for observation and recording of the attributes of the features at this level. On the chart in Figure 1 it can be seen that of the 44 types of features listed, 35 can be observed and recorded before any excavation into the features themselves is undertaken, emphasizing the need for thorough observation and recording at this stage in the archeological process for maximum recovery of data.

A typical posthole visible at the subsoil level is a dark humus filled area from four to eight inches in diameter, with the edge of the original hole no longer a sharp line, but blended by the action of worms (Fig. 1: 1). This action of worms is often so extensive that it is difficult to observe just where the original edge of the posthole was located. Unfortunately archeological reports reveal that this type posthole is most often the only designation assigned, i.e., "humus filled posthole". However, some postholes can be seen to have a higher relationship of sandy fill than others, some have a high percentage of charcoal flecks in the fill than others, and some may contain fragments of daub visible at the excavated level, or perhaps red clay from a collapsed daub-plastered palisade (Fig. 1: 2-4). At the Indian ceremonial center at Charles Towne, South Carolina the subsoil matrix was
sandy loam, and a clear contrast could be seen between those humus filled postholes and those containing flecks of red clay (interpreted as coming from a clay-plastered palisade). By recording this observable attribute it was possible to locate ceremonial sheds, and to separate one of the palisades from the other two (Fig. 2).* Similar posthole attributes and feature attributes can be separated on almost any site on the basis of the relationship of the color and/or texture of the various soils comprising the fill. Another means of observation and recording of postholes for separating various components is to record the presence of an especially dark humus area within the posthole representing the post itself. Postmolds and burned posts are dramatic attributes for revealing architectural features distinguished from other posthole data (Fig. 1: 11-12). Posthole and feature shape, whether oval, round or irregular is important in determination of associated postholes or pit features.

Because of the recent age of historic postholes there are fewer worm holes to blend the edge of the feature with the subsoil matrix, and consequently the edges of more recent features are still relatively sharply defined. These features are also easily separated into groups based on the presence of postmolds or surviving posts in the hole (Fig. 1: 7-10). The observation that historic period features have less worm hole blending might be used to form an hypothesis regarding the use of worm hole concentration as a temporal index, similar to taking a blood count. The methodology might involve the use of a small grid for counting the worm holes, and from this a series of indices created for use in comparison with features for which radio-carbon or other dates were known. The technique might have only single site or area usefulness, but illustrates the fact that theory is born of observation.

Another attribute of the historic period features is the presence of square or rectangular postholes, footing holes and features (Fig. 1: 9-10, 13). Such features cannot simply be plotted by a central point with the diameter recorded, as one might do with circular features; rather, three points at least must be recorded to obtain the proper orientation of such angular features. This must be done even if (particularly if) the feature is a small one such as a square posthole only six inches on the side. The feature in Figure 1: 10, for instance, requires no less than six measured points for accurate recording. In recording such features for meaningful interpretation a roughly triangulated plotting from grid stakes is not sufficiently precise, and transit and tape, or alidade and tape recording of the most exacting nature should be employed. This caution would seem to be an obvious standard procedure, but careless horizontal plotting of features is often the rule rather than an exception. This is illustrated by the fact that an historic brick ruin measuring 40 by 87 feet on a side cannot be plotted to reveal a measurement of 40.1 by 86.9, and roughly triangulated points from grid stakes do not normally yield this accuracy unless the most exacting care is utilized in controlling the reference points and recording procedures.

*Figures 2 through 7 are in a jacket pocket accompanying this volume.
Using the square posthole attribute, and the sharply defined, non-worm-blended edge of the features at the site of the Charles Towne Indian ceremonial center we were able to identify a nineteenth century barn complex and associated fence lines through differential plotting of this type feature in plan (Fig. 2), thus isolating these features as a separate component from the Indian occupation of the site.

Archeology of the historic period also reveals characteristic features of masonry, such as wells, footings and foundation walls. These are accompanied by their construction ditches which must also be plotted and carefully excavated, though many historical archeology reports fail to mention these important features associated with the obvious masonry (Fig. 1: 14). Prehistoric masonry structures are also often characterized by an emphasis on the masonry, such as kivas where excavation is not carried beyond a foot beyond the masonry wall, thus successfully eliminating any chance of discovery of any associated features. Masonry features are accompanied by their construction ditches which must also be carefully recorded and excavated, though again many archeology reports fail to even mention these important features associated with the obvious masonry (Fig. 1: 14).

Sometimes the geology of a site is an aid to the classification of certain features, when the geology is known from previous excavation. For instance, at Town Creek Indian Mound in North Carolina there is an orange clay subsoil clay underlyling by several feet the red clay subsoil just beneath the plowed soil zone. As a result of this phenomenon those pits that were excavated into this orange subsoil zone and then back-filled almost immediately (such as burials), contain flecks of orange clay in the fill (Fig. 1: 15). These pits are easily distinguished, from those dug into the orange subsoil zone and allowed to fill up with an accumulation of midden, by the absence of the orange clay flecks. At Town Creek then, burials can be tentatively identified on the basis of flecks of orange clay in the fill of pits before excavation into the feature is carried out.

Another type of feature that can often be identified before excavation is begun into the contents is the shaft and chamber burial with collapsed chamber (Fig. 1: 16). The collapse of the chamber produces a fault-line when the chamber drops, allowing the soil above it to sag into the depression. This produces what appears to be a later intrusive pit into an older pit, since the same type of soil is sometimes seen in the collapsed chamber area that appears in the plowed soil zone. However, this can be distinguished from an intrusive pit by the indistinct edge caused by the fault as opposed to an edge caused by digging the burial shaft. Once this type feature is observed it can be correctly interpreted in most cases before excavation is begun on the shaft and chamber. A non-collapsed shaft and chamber burial cannot be so easily identified, appearing as an oval or round pit, but its depth can sometimes be interpreted from the presence of deeply lying subsoil flecks, and thus its interpretation as likely a burial, prior to beginning of removal of the contents of the feature.
Linear features, such as lines of palisade posts, palisade trenches with or without the postmolds, and fortification ditches are particularly interesting in that they provide linearity and architectural identity, drawing a distinction between areas of the site (Fig. 1: 17-19). The width of from two to fifteen feet for fortification ditches clearly distinguishes them from palisade trenches, that may be from eight to eighteen inches in width. Fortification ditches when excavated reveal in profile, and often in plan before excavation, the evidence needed to determine on which side of the ditch the accompanying parapet was located by the position of the subsoil-like fill (on the parapet side) in contrast with the darker humus fill (on the side opposite the parapet). This is a characteristic of most fortification ditches, though particular cases may reveal exceptions to this pattern.

Another class of postholes are those with tapering ramp trenches leading toward the bottom of the hole, resulting from installation of the post. These are usually major posts such as the ball ground poles excavated at Town Creek Indian Mound. These often have stones placed against the post when it was slid into the hole and raised upright to hold it in position (Fig. 1: 20). One of these at Town Creek had no stone wedges, but instead was furnished with a trench at right angle alignment to the installation trench, which I interpreted as representing a seat for a log wedge to support the pole once it was raised into position. This proved to be a functionally valid interpretation in that the same technique was used to advantage when a 45 foot pole was replaced in the original five and one-half foot deep hole (Fig. 1: 21).

An interesting variation of the posthole with an installation trench was found by Leland Ferguson at Earth Lodge No. 2 at the Garden Creek Site in Haywood County, North Carolina (Dickens 1970: Fig. 20). Wall posts for the earth lodge had tapering trenches toward the inside of the lodge, and Ferguson has interpreted these as having been the result of replacing wall posts while the structure was still standing (Fig. 1: 22). If wall posts needed to be replaced in an earth lodge a trench would have to be dug to remove the old post or to insert a new post beneath the wall plate. When similar postholes are seen in excavations of other structures, the likely function can be interpreted before excavation of the postholes themselves is undertaken. Such postholes are also valuable in defining the structure through drawing a distinction with other postholes not a part of the structure.

There are times when a visual examination of the subsoil level of excavation reveals no features, but when the same area is photographed using infrared photography, disturbed humus-bearing features can be observed (Fig. 1: 23). Other features can be located on occasion by using the texture of the soil as a clue for separating disturbed from subsoil areas. The moisture content variation, as has been mentioned, is another clue to observation of disturbances in the subsoil matrix when the direct visual observation is not sufficient. Chemical treatment of the surface of an excavated level is being used to react with
humus or residual chemicals in wood or bone to reveal features and burials. This method is also being used to identify rodent holes (Van Der Merwe and Stein 1972: 245). Enriched vegetation over wells and midden deposits is also being used as a survey technique in locating sub-surface features. Any of these, or other methods of observation of attributes can be used to draw a distinction between groups of features for componential analysis (Fig. 1: 24).

Some features through their association are immediately identified as a single component representing a single moment in time. Such features are postholes from non-intruded architectural features representing a single structure (Fig. 1: 25). Seldom is the archaeologist presented with such clear, straight-forward situations to interpret. A classic means of separating components on a site is through intrusion of one feature on another, with the intrusive feature being later (Fig. 1: 26).

At the Dodd Site is South Dakota, Donald Lehmer (1954) was aided in his interpretation of the components by the fact that rectangular houses were intruded on by later round houses, and though his house floors were stratigraphically one above the other, he could still have isolated the components on the basis of structural classification had the features been on the same level (Fig. 1: 27).

Spatial separation of features, along with similar diameters often allow a number of features to be associated as elements of a single structure (Fig. 1: 28). Geometric alignment is a frequently used means for separating architectural components related in time and space. A palisade is a primary example of a geometric alignment of postholes that even the most cavalier observer can recognize immediately. Other more widely spaced postholes are not so easily distinguished and associated. During the historic period square footings, fence postholes and even landscaping bushes are, through their alignment, associated with property lines and other features of similar period (Fig. 1: 29-30, 34).

Linear features such as fortification ditches, palisade trenches and geometrically aligned footings and fence postholes provide excellent componential separation through sequential intrusion (Fig. 1: 30). The site of Williamson's Fort, Holmes' Fort and the town of Cambridge at the Ninety Six Site in South Carolina, is a classic illustration of this type of componential separation (Figs. 3-6). Williamson's Fort was the site of a three day engagement in 1775 between Whigs and Tories, with the fort being thrown up quickly around John Savage's barns. It was said to have been made of "bevee's hides", straw and fence rails. It wasn't until excavation was carried out that it was known that the rails had been placed in a palisade trench connecting the several barns, the footings of which were also found (Fig. 4). This 1775 component was intruded on by the construction of Holmes' Fort in 1780, and again through archeology it was found that Savage's barns were again used as blockhouses within a hornwork shaped fortification thrown up around
them. A burned retaining wall ditch with small postmolds was found to parallel this major fortification ditch, thus associating the features geometrically, and temporally (Fig. 5).

Intruding on the 1780 Holmes' Fort features were footings from the town of Cambridge which was begun in 1783 and continued until the 1850's (Fig. 6). By geometric alignment these Cambridge postholes, footings and cellars were associated and separated from the earlier components. The entire group of features revealed at the level just below the plow zone can be seen in Figure 3, with each component being separated through sequential intrusion and illustrated in the Figures 4 through 6. In this instance these components were separated by only a few years in time, from 1775, 1780, and 1783 and later. Similar separation can be accomplished on the basis of observation of features at the excavated level, before the removal of the contents of the features themselves is undertaken with any site where features are carefully observed and recorded according to their distinguishing attributes, then plotted on plan on this basis. If, however, features are recorded only as "postholes, pits and burials", we can hardly hope for more than a limited separation of components for analysis and interpretation.

Analysis of features on the basis of magnetic-astronomical orientation was reported by Binford at the Hatchery West Site (1970), producing some impressive cultural interpretation (Fig. 1: 31). Trees, bushes, plow scars and rodent holes are all features on a site with which the archeologist must deal and interpret (Fig. 1: 32-35). These features can be non-cultural or they can act as recipients of artifacts that may have fallen into them when they were open. Plow scars reveal clues to the erosional history of the site, and the direction of plowing, often providing for clarification of features disturbed by plowing. Some bushes and trees, particularly on historic sites, are cultural in that they were part of a landscaping plan, and for these reasons they are observed and recorded and interpreted along with other observable data on the site. Non-cultural features such as geological changes in subsoil characteristics, and veining, often appear as misleading pseudo-features that must also be interpreted by the archeologist, if for no other reason than to recognize their non-cultural aspect.

So far we have discussed the attributes observable in features in plan at the excavated level. Additional feature attributes can be determined from the excavated features that can be used to classify and associate certain features. At Town Creek Indian Mound Joffre Coe has used the aerial mosaic technique in recording each ten foot square photographically and joining these to make a master mosaic of every feature on the site. From this exacting record, plus the square sheet data from the square ground area in front of the mound no structures could be interpreted from the galaxy of postholes in the square ground area. However, in 1956, I used the depth of each excavated posthole as an attribute for recording with a color-code the various postholes and features, and was able to isolate a rectangular square ground shed.
Bennie Keel (1972: 120-122) used another attribute to accomplish a similar result at the Garden Creek Mound No. 2, in Haywood County, North Carolina. He noticed that some of the excavated postholes contained a sandy fill near the bottom, and by plotting these in plan with a different key than other postholes he was able to define a house (Fig. 1: 37).

Stratified structures represented by postholes at different elevations can be separated on the basis of the top of the postholes, a classic means of temporal separation of components (Fig. 1: 38). Excavated postholes can also be classified on the basis of the angle of the postmold or posthole (Fig. 1: 39), such as the leaner wall posts forming the outer ring of an earthlodge (Stephenson 1971: 29). From the angle of the leaner postmold in relation to the position of the main wall postholes, the height of the main wall can also be determined. Posthole and postmold shape can be used to classify posthole features, with the straight-cut farmer's post contrasting markedly with the more tapered Indian postmold impressions in profile. Also, a posthole digger dug hole is recognized in some cases by its higher center (Fig. 1: 43).

After considering these forty observable feature attributes, plus any other known to the archeologist, he can then turn his attention to classification of features distinguished on the basis of artifact association with features (Fig. 1: 40-42). Unfortunately, the tendency has been, and still remains in many instances, to view features primarily as recipients of artifacts from which data can be recovered. As the chart in Figure 1 indicates there are a multitude of attributes constituting data that must be recorded before the cultural items are recovered and analyzed. Postholes, pits, burials, ditches, trenches and construction ditches for foundation walls are all valuable recipients of cultural items from which analyses and interpretations are made. A series of postholes can be classified into different cultural components on the basis of the artifacts recovered from them. The basic principle of terminus post quem is used to determine temporal periods represented by the artifacts recovered from these features (Fig. 1: 40). Sometimes the presence or absence of particular items can be used as a classification device, such as the use of bone or stone wedges in postholes. A series of postholes with bone wedges might well form an architectural pattern allowing for the isolation of a house, or temporal, or cultural interpretations might be demonstrated (Fig. 1: 41).

Cross-mending of artifacts is an important means of associating features at one moment in time, such as the recovery of fragments of a white salt-glazed stoneware teapot from a number of features. The gluing of these fragments together joins the features as well, an observation adding valuable information for the interpretation of the features. The same applies to cross-mending of fragments from various stratigraphic layers which bonds the stratigraphy into a single temporal unit (Fig. 1: 42).
The Function of Observation in the Archeological Process

FIELD OBSERVATION

THEORY

HYPOTHESIS

FIELD OBSERVATION

Recording and Interpretation of Features for Componential Analysis

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ANALYSIS

Preservation and Interpretive Explanation of the Archeological Document

HISTORIOGRAPHY

HISTORICAL ANALYSIS

CULTURAL-HISTORICAL INTEGRATION AND INTERPRETATION

PROCESSUAL EXPLANATION in Terms of the Hypothesis and Theory

NEW HYPOTHESIS FORMATION

FIELD OBSERVATION

Paradigm

Corollaries

Sherry Redd, Archetype
Institute of Archeology and Anthropology
University of North Carolina
1972-75

FIGURE 1
The classification of features on the basis of functional interpretation and designation by culturally functional oriented nomenclature is based on a group of attributes characteristic of particular features. Earth ovens, smudge pits, burials, cooking pits, storage pits, rock hearths, house floors, living floors, and use areas are observable data assigned cultural designations for analysis and interpretation (Fig. 1: 44). Binford at the Hatchery West Site conducted an analysis of rock hearths, earth ovens, pits, houses, and burials through cluster and attribute analysis in order to define the cultural components represented by these features (Binford 1970). This type of multi-attribute feature analysis combining a galaxy of attributes; width, depth, shape, texture, color, associated artifacts, orientation, ethno-botanical objects, and use area debris results in a most sophisticated componental and cultural analysis.

The purpose of this paper has been to point out some of the observations of feature attributes made by the archeologist allowing for making distinctions between features for componental and cultural analysis. To some archeologists this presentation has only stated the obvious, a standard archeological procedure used for decades. However, archeological reports still appear with the classic "pits, postholes, foundations, and profiles" level of observation and recording, suggesting a definite need for more rigorous observation and recording of data. For instance, there are many historical archeology reports revealing structural foundations, and large expanses of supposedly observed and recorded excavated areas adjacent, but no sign of a posthole is seen. Scaffolding holes, postholes, and other subsoil disturbances almost always accompany historic structures, so a drawing showing only foundations is a highly selective type of data recording.

Other indications that a more rigorous observation and recording of feature data is needed are seen in the following: postholes recorded as stylized symbols instead of as they actually are observed in the field; straight interpolated lines for fortification ditch edges instead of actually plotted edges as observed in the ground, making for a neater drawing, but hardly accurate; failure to record trees and bush features; failure to record postmold as well as the posthole, the hole being a general representation of the position of a structure, with the postmold representing an exact position; inconsistent recording of posthole and feature data, postholes being recorded only as incidental to some other problem of interest, or as they fortuitously are seen on wet days, with little effort being made to systematically record every posthole on the site; palisades shown as stylized, schematic representations with no details and specific post positions shown; entire site reports presented primarily through profiles, with little recording of plan data; disregarding stratified data in features, and emphasizing primarily the artifacts recovered from the feature, thus missing possible data of value in the interpretation of seasonal activity, or temporal-functional relationships within the feature; entire site reports presented on the basis of a series of five-foot squares, with emphasis on stratigraphic data at the expense of features in plan, resulting in a lacunae in our
knowledge of structures and settlement patterns compared with our problem oriented studies emphasizing temporal sequences. Problems such as these can be overcome through more careful observation and recording of features and other data on a broader base, emphasizing a multi-attribute approach in drawing distinctions between archeological features.

Besides emphasizing the need for more rigorous field observation, the purpose of this paper has also been to emphasize the function of observation in the archeological process. The primary, basic and central function of observation is seen illustrated in the paradigm in the chart in Figure 1. Theory with hypothesis makes fertile the observation of the data. When the archeological process of observation and analysis is sufficiently developed an explanation emerges to account for the culture process responsible for the observed patterned phenomena. The explanation is a genetic offspring of the parent theory and hypothesis, but was gestated in the fertile environment of field observation. This descendent tests the parent concepts and is the source for new hypothesis and theory, leading to more refined field observation. This paradigm of the archeological process clearly reveals the central function of observation, and is followed by several corollaries. Theory and hypothesis do not produce explanation without observation. Thorough observation allows for more sophisticated analysis and problem solving, resulting in new and refined theory. Inadequate, inconsistent, incomplete and careless observation will not develop into a reliable interpretation or explanation regardless of the sophistication of the theory and hypothesis. Observation, regardless of how sophisticated, without the parent theory is sterile, and will not produce explanation. Theory is born of observation, thus observation is basic in the archeological process.

An important by-product of this archeological process is the preservation and interpretive explanation of the archeological document through exhibits of ruins, fortification ditches, parapets, burial houses, reconstructed earth lodges, structures and palisades. It is emphasized, however, that this by-product is not the goal of the archeological process, merely a shell produced from the gestation of cultural-historical interpretation and processual explanation. This paradigm is visually illustrated in Figure 1.

The archeologist should guard against allowing the problems dictated by sponsors interested in structural detail for purposes of reconstruction for public interpretation to become his archeological goal at the expense of integrative analysis and cultural interpretation based on broad and in-depth observation. However, if the archeologist accepts the responsibility of executing the archeological process to achieve his own scientific as well as his sponsor's developmental goals, he also has a responsibility to produce a product of some real use to the sponsor. An archeological report strictly limited to explanation of the archeologist's goals might still leave the sponsor wondering what to do next toward development of the historic site. Therefore, the archeologist should provide some suggestions toward a master plan for the preservation of the archeological document, and toward the development of the site within the framework of
the archeological data. The stabilization map in Figure 7 is an example of the type of assistance the archeologist can offer to the sponsor and the contractor whose responsibility it is to actually execute the work of transforming the archeological data into an explanatory, interpretive exhibit on the site. Without such help in the form of plan and profile drawings and suggestions in a report to the sponsor, the archeologist has no reason to complain when the explanatory exhibits in the form of exposed ruins, rebuilt parapets and palisades do not conform to the archeological evidence. He does have a responsibility toward insuring that the explanatory exhibits do not violate the archeological document.

Historical archeology is particularly encumbered with problem oriented studies of narrow scope, wherein the problem consists of locating the foundation of a structure, or a fort site. Indian site archeologists also have their albatrosses in problem oriented studies centered on a narrow goal; the skeletal material from a site, sometimes recovered at the neglect of other types of data; the number of structures to be found in a stratigraphic cut of a temple mound, with no data recovered as to what the floor plan looked like; or the temporal sequence represented by the ceramics from a site through five foot test squares, with no information as to structural form or village plan that could emerge if the paradigm only called for the one hundred yard square instead of the traditional five-foot or one meter albatross. Our problem in such cases has been not so much a lack of problem, but a concentrated focusing of our observation on specific problems rather than detailed observation of attributes of value for studies of broader scope. Some advocates of the "New Archeology", in their enthusiasm for specific, problem oriented studies, are encumbered with this same albatross in that their explanations cannot scientifically be broader than the scope of observed data on which they are constructed.

Another basic traditional approach to the archeological process has emphasized the responsibility of the archeologist to observe intensively and carefully as many attributes of the data as possible so that a broad base for interpretation can emerge from the observation and recording process. This basic attitude has come under criticism for its frequent "lack" of problem orientation, and its sometimes apparent concern with observation and recording of data as an end in itself, resulting in challenges arising as to the value of site reports (Zubrow 1971: 482). It is obvious that no archeologist can possibly observe and record all the data that might be needed to answer all problems, but it does not follow that problem oriented studies in the new idiom are the only type problems justified (as pointed out above the difficulty has often been a too refined and narrow problem rather that a question of no problem at all). There is a basic corpus of data that must be observed and recorded in addition to any unique data requirements for specific problem solving, and it appears patently obvious that what we need is not more narrowly focused observation for specific problem solving, but a broader base of exacting multi-attribute data recording from which our hypotheses relating to culture process can be formulated. It is also apparent that with a greater concentration on observation and data recording that the
scientific archeologist has an obligation to abstract pattern and offer explanation in terms of hypothesis and theory in the evolutionary framework basic to the archeological process (South 1955).

Our problem solving is limited by our observation, and our questions can only be as sophisticated as our field observation and data recovery methods. The trend now is to construct specific problems and collect specific data to provide the answers, in spite of the fact that an anthropologically or historically based discipline would imply a broader focal angle. Students of the "New Archeology" emphasize theory and problem, science and processual explanation, but some are remarkably naive when it comes to relating observation of archeological data to anthropological theory, to the explanation of culture process, or to the recording of data other than that specifically applying to their problem. They appear to be "New" in the sense of a new puppy, unfamiliar with the fundamental, competent, data recording methods dictated by the traditional "Old Archeology". Not having mastered the techniques of observation and data recording, they are often seen to be caught with their methods down, an awkward position from which to explain why their nomothetic paradigms were not adequately supported.

I see the archeological process diagramed as a pyramid with a broad data base of competent observation and data recovery, leading through evolutionary theory to explanation of the culture process, represented in the diagram by the capstone tip of the pyramid. From some of the misguided "New Archeologists", however, I get the impression of an up-side down pyramid, poised precariously on its narrow point of selective data observation, on which unsure base a mass of nomothetic paradigms are uncertainly balanced, enveloped in a camouflaging cloud of verbosity promenading as processual explanation. This is certainly not the scientific archeology Binford has urged us to undertake, yet "New Archeology" is burdened by misguided disciples whose approach is likened unto a pyramid with its point buried in loose sand.

The following questions have emerged from having watched the misguided efforts by disciples attempting to "do New Archeology". The same disciples vociferously in concord, frequently criticized the "Old Archeology" as an ever-present whipping boy in contrast to their "New" approach. Are we justified in throwing Archaic Period hearths out in our back dirt because our paradigm calls for plotting profile information relating to the pottery making period of occupation on the site? Are we really being scientific when we record postholes according to only three attributes, width, depth, and horizontal location, and then run this through a computer to determine the relationships that might be obtained in a sample of fifty postholes? Are we "doing science" when our problem calls for plotting each sherd, chip, bone, and shell fragment, in an effort to determine clustering or scatter pattern, when the thirty foot square excavated area being so treated is an occupation surface of a Mississippian midden? What possible valid postulates could support an hypothesis justifying this examination of a mixed village midden deposit surface in such a restricted area? When the primary data we have on shell rings are profile sections, with no architecturally related features in plan, how can we justify a research design centered around obtaining another profile section to add to the collection? The error here
is in microscopic vision of data at the expense of the broader view, which view is seen as the antiquated pursuit of the "Old Archeology". The depth of scientific archeology demands rigorous, controlled, consistent observation, with a broad base to support specific research designs. Theoretically-weighty research designs and microscopic observation of data at the expense of the broad archeological record, are not compatible within the paradigm of scientific archeology!

In conclusion I would like to emphasize two points, the first being that observation and competent data recovery is prelude to any theory, and forms the body from which analysis proceeds and new hypotheses and theory are created. The second point is that I, along with a number of my colleagues, are committed to the development of archeological science, and are disturbed by those who parade under the banner of the "New Archeology" but besmirk that brave standard through narrowly focused pseudo-science or sweeping generalizations and nomothetic paradigms based on a minuscule quantity of selected data. Such an approach demonstrates a lack of concern for the basic element in the traditional as well as the scientific archeology paradigm: competent observation and data recovery.

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METHODOLOGICAL PHASES IN THE ARCHEOLOGICAL PROCESS

Stanley South

The archeological process can be viewed as eight phases, four of which relate to the collection of data in the field, the excavation phases, and four phases concerned with explication:

**EXCAVATION PHASES**

In this paper

1. Site Survey
2. Exploratory Excavation
3. Detailed Excavation
4. Excavation of the One Hundred Yard Square

**EXPLICATION PHASES**

Not in this paper

5. Analysis
6. Synthesis and Interpretation
7. Explanation of the Culture Process Reflected by the Data
8. Explanatory Exhibits on the Site

**PHASE 1**

The first phase in the examination of an archeological site is the location of sites through surface survey, study of maps and aerial photographs to locate potential sites, and historical documentation.

**PHASE 2**

The sites located in phase 1 are examined by sinking exploratory squares and trenches to obtain data regarding stratigraphy and superposition, and to locate areas of major concentration of cultural data, postholes, pits, artifacts, etc.

**PHASE 3**

Once the concentration of cultural material is determined, the spot is chosen for opening a larger exploratory area for more concentrated excavation of a more detailed nature. This area is usually some fifty feet square, or a long trench twenty or thirty feet wide and perhaps a hundred feet long. The approach to excavating this area in more detail is determined to a great extent by the data revealed in the second phase of the project.
The third phase is used particularly where an individual house, camp site, chipping station, mound or ruin requires a more detailed stratigraphic or tightly controlled horizontal recovery of data, such as scatter pattern data, or lenses representing occupation levels. The decision as to what type of data recovery method is used is made by the archeologist based on his evaluation of the data revealed in Phase 1 and Phase 2 of the project. This is a major role of the archeologist, the application of judgment in the choice of methods he uses to extract the most data from the site in the quickest amount of time at a resulting maximum data - minimum cost ratio. Thus Phase 1 and Phase 2 predicate the research design of Phase 3 and the phases to follow in keeping with the overall research design.

Phase 3 is applied where Phase 2 tests revealed stratigraphic zones of cultural material and/or humus zones representing old ground surfaces or stabilized zones and/or occupation zones. If these occupation zones are deep beneath an overlying mantle of soil, it is necessary to remove the overlying soil by machine to make the best use of time and money in obtaining the data these deep deposits have to reveal. In so doing the data from the top occupation zone may be destroyed, but again the archeologist must evaluate the situation and make a judgment as to which data is most valuable. In any case the top cultural zones should never be destroyed by machine until adequate sampling of these zones is carried out under Phase 2 procedures.

Once the overlying mantle of soil is removed to within a few inches of the deeply lying cultural deposits the machine should be removed from the area and the zone approached by use of carefully controlled hand labor. The depth of the machine cut should always be controlled by constant supervision by the archeologist, using the deep trenches cut during Phase 2 as a guiding control.

If the site has several cultural components that are located in the upper soil zone of the site, and if this soil zone is a foot to several feet in depth, with no visible stratigraphy, then the dissection of the deposit by arbitrary levels may be called for until enough data is collected to determine the superposition that may be present. This is a primary purpose of Phase 2, and if answered by the data recovered in Phase 2, the approach to the site in Phase 3 may be entirely different.

If the top soil zone contains virtually a single component, then it hardly makes for the best utilization of resources, human, temporal, financial, and logistic, etc., to utilize a technique designed to reveal stratigraphic separation through superposition analysis. Such an unnecessarily precise and time consuming process sacrifices data such as features in quantity, house patterns, village patterns and relationships obtaining between them that can be acquired by using the procedures outlined in Phase 4. Phase 3 can well be carried out on a site at the same time that Phase 4 techniques are being applied nearby. Phase 3 is the traditional detailed excavation approach to layers, levels and features, and is always used once the features are located through Phase 4 methods of stripping of one-hundred-yard squares to reveal the features.
PHASE 4

If the site is a single component site as revealed by the cultural material recovered in Phase 1 and Phase 2, and this component is located primarily in the plowed soil zone, with features extending into the subsoil zone below, then an ideal situation exists for application of Phase 4. A front loader or belly-loading excavator can be brought to the site to strip the overlying mantle of soil from the level at which the archeologist wishes to obtain a broad look at all features.

The machine should be carefully supervised by the archeologist, with an effort being made to leave a slight layer of buffer soil above the level of the subsoil surface. The surface of the subsoil or level to be examined is then schnittd (shovel-cut) using a gang-schnitt technique, with the entire crew lined up in formation, with careful supervision throughout the slicing process to insure a uniform cut of the soil level being examined. The features so revealed by this slicing method are then plotted with transit or alidade, followed by Phase 3 detailed excavation of the features themselves. To insure the most consistent reading of the soil document the schnitted surface should be kept damp by means of mist spray.

Features revealed by this method can be excavated and their contents analyzed, producing more data than would be possible in the same amount of time if the topsoil zone were removed and sifted by hand labor. Artifacts from features have a much greater time-capsule and cultural-context character, and are conducive to a far higher data producing analysis than the analysis of potsherds from the plowed soil zone, regardless of how meticulously that plowed soil zone is excavated. The plowed soil zone has been subjected to a mix-master process of the plow for a hundred years or more on many Southeastern sites, not eliminating the usefulness of the sherds there, but certainly contributing to a characteristically small size in most instances.

Needless to say the approach of Phase 4 would not be used on sites where no plowing has been carried out, and the objects lying in the topsoil zone are virtually in-situ as left by the occupants of the site. Most of our Southeastern bottomlands have been subjected to extensive plowing, and are therefore characterized by the "plowed soil zone".

If a research design is outlined wherein horizontal distribution of plowed soil zone materials is desired to produce data for comparison with underlying features, then of course, no machine stripping such as outlined in Phase 4 should be undertaken. An important point emphasized here is the fact that the nature of the site should be used along with the questions being asked in the basic research design, to determine the method the archeologist will use in examining his site.

If settlement patterns are a vital question of concern to the archeologist and constitute a major element in his research design, then excavation of five foot squares and trenches such as outlined in Phase 2 and Phase 3 will not reveal this data. If more data as to an Indian village is desired then the "possible" edge of a house...
and a few associated pits in a 20 by 100 foot long trench excavated in the manner characterized by Phase 3, then archaeologists are going to have to begin to carry their excavations beyond the first three phases of the archeological process outlined here.

If the revealing of five Indian houses through their posthole patterns can be achieved through the use of machinery to strip the overlying soil mantles from a level where these house patterns can be observed as described in Phase 4, can we continue to justify the expenditure of the same amount of money to recover a couple of pits and a few postholes of a "possible" house through concentration on the methods of Phase 3 only?

Even when the overlying deposit of soil may have stratigraphic, or superimposed cultural material in a black soil zone two feet thick, are we going to always concentrate on obtaining this stratigraphic data at the expense of the settlement pattern data, the feature data that can be obtained through the procedure of Phase 4? Are there not some instances where we can now say that from the presence of X, Y, and Z types of pottery that we can assign a stratigraphic relationship of 1, 2, and 3, with a temporal range of 1200 to 1500 A.D., and then proceed to answer other questions? If we cannot, and must forever examine each site as though it were the first of this type ever seen by the eye of man, and therefore has to be dissected in all meticulous detail, then we haven't learned much from the last half century of archeology! If our traditional techniques of Phase 1-3 have not produced enough data in certain areas so that sometimes at least we might not examine a site as though ceramic chronology were the only question being asked, then it is indeed time we turn to new methods to recover our data for us. Here we are not suggesting abandoning Phases 1-3, but urging that when the situation calls for the use of Phase 4, that we not hesitate to apply it.

We are beginning to ask broad questions of our archeological data, and these cannot be answered if we do not move into the twentieth century with our methods and begin adapting our approach to our research designs predicated by the questions we are asking. We are no longer justified in excavating two seasons on an exploratory effort using Phase 3 procedures designed strictly around chronology when the data revealed in Phase 2 has already shown that the major soil zone is characterized by the presence of a single component! Such an excavation may well emerge at the end of a second or third season and not yet have the first indication of an architectural feature, or relationships that obtain beyond the microscopic area examined in the Phase 3 project. Under such a research paradigm even the perimeter of the occupation area is often a mystery after excavation is complete. If we insist on stopping at Phase 3 we should not ask questions that can best be answered through the application of Phase 4 methods.

When Phase 2 has sampled adequately the various areas of the site and determined the relationships that obtain between the various ceramic levels and pre-ceramic components, as well as the relative concentration of
cultural material in various areas of the site, the archeologist must ask himself the question as to whether a repetition of this data through a Phase 3 project from the surface down is more valuable, or whether gathering data from a broad area of the site at a particular level would be the most productive of data recovery, through Phase 4 methods.

After adequate sampling of Phase 2 has been carried out the archeologist may well make the decision to remove the upper, later components in order to reveal what is, in his judgement, a more important body of data in the deeper-lying strata of the site. It is emphasized that this move must be predicted on the completion of Phase 2 with its recovery of control data on upper occupation zones before machine removal of these zones to get at the lower "more important" zones is undertaken. If, however, the upper zones contain relatively rare data in themselves, Phase 3 methods should be used throughout the depth of the stratigraphic cut, regardless of the time required to acquire such data. Destroying valuable data for "more important" data is not justified, and it is only when more data of value will be gained than lost that upper levels can be judged as "expendible". If the most data can be obtained by spending three seasons on a single house site, then this Phase 3 type procedure should be executed, by all means. This decision making process is a role that the archeologist must play if he is to recover the most data. The point emphasized here is that too often we find a slavish allegiance to methods long outmoded for answering the questions we are asking of our archeological data. Hopefully we can begin to design our methods to fit our questions.

The following is a statement made some years ago that contrasts the archeological project that utilizes only Phase 2 and Phase 3, with one that launches into the methods of Phase 4, which:

...method provides for maximum speed, efficiency, and flexibility ...to recover data from sites such as towns, cities, and forts whose features sprawl over many acres through woods and fields, valleys and hills. It is time to look beyond the womb-like comfort of the involvement with dissecting burials, cellar holes and five foot squares if we are to meet the interpretive challenge presented by villages, ceremonial centers, towns, cities and fortified areas.

Too long have we practiced the ritual of the cult of the square, impotently arriving at feeble interpretations of complex cultures in extensive settlements from the meager evidence presented by a few postholes and a stratigraphic sample from a five foot square. We have often failed to adapt out tools to the scope of the project. We have used a spoon on villages and towns as well as burials. We have looked at cultures through keyholes when we should have been opening doors. This does not suggest the abandonment of the five foot square, but it does emphasize that there are times when
it is a totally inadequate tool, like excavating a village with a spoon. Through exploratory trenching to determine the nature and scope of the features, then totally removing large blankets of topsoil from extensive areas of the site, stripping football field size "squares" instead of minuscule five foot areas, we can begin to open a few doors. Once the archeologist is rewarded by the view of the culture revealed through such doors he is thereafter highly unsatisfied by peeping through keyholes (South 1971:48).

SUMMARY

The archeologist should go into the field with a theoretical research design relating to questions he is asking regarding the examination of data relating to past cultures, the remains of which he expects to examine. However, he should be prepared to fit his research design to the dictates of the site as the data the site produces is revealed through archeology.

The phases outlined here are the means whereby this accommodation of theoretical research design to the archeological realities of the site is achieved.

EXCAVATION PHASES

Phase 1 The sites cannot be studied until they are located. This is the goal of Phase 1, Site Survey.

Phase 2 The nature of the sites as to their underlying potential, stratigraphically and horizontally, cannot be known until exploratory testing is carried out in Phase 2, Exploratory Excavation.

Phase 3 Detailed dissection of important areas of the site for stratigraphic control and horizontal patterning cannot be accomplished without the microscopic approach of Phase 3, Detailed Excavation.

Phase 4 Questions as to settlement patterns, relationships between structures, types of structures, use areas of sites such as ball grounds, burial areas, dwelling areas, ceremonial areas, relationships between classes of features, etc., can best be answered by the methods outlined as Phase 4. If we know that a village site was spread out along a bottomland for a mile, would not the 100 yard square
approach of Phase 4 be a better sampling method for studying the village than the microscopic view afforded by Phases 2 and 3, the traditional approach to the problem?

Phases 5 through 8 are not discussed in this report, constituting as they do, the laboratory analysis, synthesis, writing of the report, and the explanatory exhibits developed on some sites. These four phases are as followed:

EXPLICATION PHASES

5. Analysis of the Archaeological Data
6. Synthesis and Interpretation of the Data
7. Explanation of the Cultural Process Reflected by the Data
8. Development of Explanatory Exhibits on the Archaeological Site

The extent to which the archaeological analysis can reveal the patterns of culture represented by the archaeological data; the extent to which the analysis results in cultural synthesis and interpretation; and the extent to which explanation of cultural process represented by the data can be undertaken all depends on the approach of the archaeologist in the field. If he stops his examination at the end of Phase 1, the amount of data is limited to surface finds, and his conclusions must be blanketed with speculation. If he stops his excavation at the end of Phase 2, his results can provide statements as to chronology and aerial distribution, but he can say little beyond. If he stops his examination at the level of Phase 3 he may be able to make a tentative statement about one house or structure, or part of a house or structure, and he may be able to make a more detailed statement as to chronology and stratigraphy, and on deep deposited Archaic Period sites dissection of the most microscopic type reveals abundant data on occasion, as well as detailed dissection of individual houses, mounds, etc., but such excavations do not usually provide broad, horizontally distributed data on settlement patterns, groups of structures, and other data depending on a broad scope view for the most effective interpretation. It is in this instance that Phase 4 is most effective and productive of abundant data.

There are sites that cannot benefit from the use of Phase 4 methods, such as sites relatively undisturbed, and masonry sites, where machines would do severe damage to the archaeological ruins. Again, the judgment of the archaeologist must be brought to play to keep machines away from such sites.

Phase 8 brings a whole new concept into the discussion, with the use of explanatory exhibits on the site, such as palisades placed in the original ditches discovered by the archaeologist, stabilization of ruins so that they can be exhibited and yet can withstand the rigors of being exposed to the elements, rebuilding of parapets of earth beside the fortification ditches discovered by the archaeologist, are all examples of such
exploratory exhibits. Sites such as Ocmulgee National Monument in Georgia, Town Creek Indian Mound, and Brunswick Town State Historic Site in North Carolina, and Jamestown in Virginia, are examples of on-site exploratory exhibits of archeologically revealed features, but this phase of the archeological process is not discussed in this paper.

This paper has concentrated on the first four phases in the archeological process, with emphasis on Phase 4, Excavation of the One-Hundred-Yard Square. It has urged archeologists to add to the traditional three phases, this most important fourth phase, with the hope that it can be employed more frequently in the recovery of archeological data, with the view to bringing our methods in closer harmony with the questions we are asking in our research designs.

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EVALUATION OF ANALYSIS SITUATIONS RELATIVE TO THE ARCHEOLOGICAL DATA BANK

Stanley South

Any analysis of archeological materials must be oriented to a statement clearly defining the provenience of the data. Analysis of data from the plowed soil zone representing perhaps hundreds of years of occupation has a different analytical weight than data from a pit representing one moment of time.

If we have an archeological site known from documents to have been occupied from 1720 to 1730, then our chronological period is established by documentation until archeology is able to confirm, deny, or elaborate on this document. When we excavate the site and find that none of the artifact classes about which we have chronological information indicate that the site was occupied at a time other than the decade indicated by the documents, then we have confirmed the historical documentation. The entire group of associated artifacts then have a feed-back value into our data bank of knowledge. Thus we use our knowledge of certain classes of artifacts, such as ceramics, pipestems, and wine bottles as a check against the known temporal period, and if this is found to agree, then we have reason to assign the same temporal bracket to the entire group of artifact classes recovered from this provenience.

The same situation prevails when we have the same documentary control data, but upon excavation we find from the artifact analysis that there is obviously an occupation at a later time than indicated by the documents. Since we have tight stratigraphic and/or feature provenience control we are able to separate an earlier component from a later component, and we find that the earlier archeologically separated component has no class of artifacts dating later than our documented period of occupation. We then have reason to relate this group of archeologically associated artifact classes with our documented time bracket. The other, later artifact classes are then assigned a later chronological position both by virtue of their higher stratigraphic or provenience separation and by what knowledge we have in our data bank regarding the temporal position of these artifacts.

If, however, our excavation reveals a mixed deposit with no significant separation of materials by provenience, and artifacts are present from a period later than the documented time period, then we are forced by the archeological data to deal, in our analysis, with the entire temporal range represented by the artifact classes.

This basic conceptual premise can be illustrated in a "Data Flow Diagram for Evaluation of Analysis Situations Relative to the Data Bank of Archeological Knowledge" (Fig. 1). The short time span represented by data from a narrow documented occupation period and/or a tightly provenieneced archeological data results in a flow of associated data as a contextual unit toward the data bank of archeological knowledge. This data bank can be seen as a piggy bank into which information coins are placed, such as: 1) the chronological association of artifact classes as a time capsule, 2) the associative-functional, artifact-feature
relationships, 3) the spatial associations, 4) meaningfully provenienced horizontal and stratigraphic data in association with site features, architecture, etc., 5) historical documentation, and 6) the associated data reflecting cultural patterning and process as a contextual unit. Such analysis situations produce more data than required from the data bank, and therefore have Primary Research Priority.

When the analysis unit represents a long occupation period and/or no provenience control, the result is that there is a data flow of information coins from the data bank toward the archeological components being analyzed. Since there is a long occupation period involved and no provenience control, virtually all information such as function, comparative data, chronology, spatial relationships, associations, documentation, typology and cultural patterning and process must come from our data bank of knowledge toward the analysis and interpretation of the analysis unit. Because of this requirement for more data than it produces for the data bank, this analysis situation has a Secondary Research Priority.

There is one situation where two occupations can be suggested for an analysis situation representing a long period of time, and this is when the sequence of artifact types is broken by the absence of a type or types that should be present if the occupation had been a continuous one. Such a situation still requires more data than it produces for the data bank, and is still a secondary research priority situation, but it does have a limited feedback value into the data bank somewhat higher than when negative data is not present.

An example of the time when we can validly split a long time span ceramic collection is seen where white salt-glazed stoneware and other mid-eighteenth century ceramic types are present, as well as pearlware of the 1780's and 1790's, but creamware characteristic of the 1770's is virtually absent. In the fact of such negative data, and in the absence of other data to the contrary, we might validly suggest two occupation periods represented by the ceramic collection, separated by a period of non-occupation in the 1770's. This does not allow us, however, to suggest that the bone or any other classes of artifacts can be similarly divided into groups reflective of two occupation periods.

From this evaluation of analysis situations it can be seen as axiomatic that the value of an archeological analysis unit is in direct proportion to the degree to which there is a data flow from the analysis unit to the data bank for use in interpreting the archeological record. A corollary to this is that in a primary or a secondary research situation the value of the data to future research is in direct relation to the competence of the archeologist in obtaining significant provenience analysis, interpretation, and explanation of the data in relation to the hypotheses being examined in the research design.

In view of the above it becomes apparent for the purpose of defining the occupation period represented by the artifact classes in an analysis unit, we cannot validly select the artifact types belonging to the documented time period as indicated by the records, and ignore or separate those that date later. In such an instance, the archeological record has demonstrated the incompleteness of the written record, and we should
then deal with that occupation record. If we concern ourselves with listing artifacts used at particular time periods, and divide our collection on this basis, we need not have done archeology to carry out what is primarily an exercise in the temporal arrangement of artifact types!

The archeologist faced with the analysis of a poorly provenienced and/or long-time-span group of artifact classes is sometimes seen to resort to what he may term "functional analysis" to avoid the mere exercise of temporal arrangement of artifact types. Limited information can be extracted from such analysis, such as the conclusion that plates were used to eat from, mugs to drink from, jars to store liquids, nails to hold wooden members together, shovels to dig with, lamps to provide light, drawer-pulls to open drawers in furniture, and other equally interesting conclusions. There is certainly nothing wrong with functional analysis, but again it is evident that the most data will emerge from our analysis situations when there is a narrow documented occupation period and/or tightly provenienced archeological data. In such primary research priority analysis situations there is more data flow toward the data bank than from it, for functional or other analysis.

If the archeologist finds himself involved with a secondary priority analysis situation where his level of operation is on that of the collector of relics or an antique dealer, then he may well ask whether his time might not be better spent in other pursuits. If in arriving at functional, socio-economic, status, and other cultural interpretations from archeological data the archeologist finds himself leaning on the documents as a crutch, and using archeological data primarily as padding to the historical record, then he is bastardizing the archeological profession. He should use documentary data, but the foundation of his interpretation should be archeological when his historical-temporal, historical-social, historical-status, historical-function explications emerge from the archeological process. There should be a direct and positive nexus between the archeology and the documents in interpreting the cultural process represented by the patterning seen in the archeological record. If there is not this connection, then we are frosting history or writing fiction as a veneer over the data with which we began.

The archeological process requires a systematic, scientific, carefully cited presentation where any conclusion follows from documented, demonstrated patterning of data. An alternative approach is characterized by terms such as "we might expect", or "it can be assumed", or "it stands to reason" that many wine bottles equals a tavern; porcelain equals a rich man; coarse earthenware equals a poor man; and from this "data" we leap to describing the life style of the colonial period in our "cultural explanation". Such an approach does not produce coins of information for depositing in our data bank of knowledge for use in the analysis and interpretation of archeological data.

Our comments here have been designed to emphasize the importance of data flow from archeological sites to the data bank of our knowledge. If our research designs are such that the questions we are asking of
our sites can be answered primarily through a data flow from our existing knowledge to the sites we are excavating, then perhaps we should re-examine our questions and our research designs. If we find that we are excavating site, after site, after site with our reports reflecting merely a descriptive statement of the architecture, the profiles, the features, and the artifacts as interpreted through existing data bank knowledge, then perhaps we should begin to turn our attention to those research situations having primary research priority. Kiln sites, stratified sites, short time span sites, specialized use sites, such as those used by silversmiths, blacksmiths, goldsmiths, and other craftsmen as well as sites representative of those areas where architectural or artifact chronology data is lacking are primary research priority sites. This is a direction easier pointed out than carried out since our archeological financing is most often not based on these research considerations. However, by constructing our research designs and our methods around an emphasis on data flow from research situations to data bank, we hopefully can increase the amount of usable archeological data emerging from our excavations.
Primary Research Priority for Data Analysis

Secondary Research Priority for Data Analysis

Figure 1
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HISTORICAL ARCHEOLOGY REPORTS: A PLEA FOR A NEW DIRECTION

Stanley South

The report emerging from any archeological excavation will reflect the theoretical base upon which the archeologist based his research design, and therefore a discussion of archeological reports necessarily involves a consideration of the theoretical base underlying the research. Archeology is increasingly being called on to provide basic data for the interpretation and development of sites considered important enough to warrant scientific investigation. The sponsors of such projects have a right to expect that the result of archeological work will have at least some relation to the questions for which they need some answers. Thus archeologists have two masters, so to speak, the sponsor of their research, and their scientific responsibility to their profession. The fact that the sponsor may require architectural data for the purpose of reconstruction goals for public interpretation, or that his primary concern is with the temporal period represented by an archeological site for purpose of authentication, need not bind the archeologist and prevent him from formulating a valid set of problem oriented research goals of his own relative to the data that might emerge from the site. He does, however, have an obligation to achieve his own scientific as well as his sponsor's developmental goals, and hopefully produce a report that will be of use to archeologists as well as to his sponsor.

Archeologists should clearly spell out to their sponsors in their research proposals what type of information might be expected to emerge from an excavation of an archeological site. Frequently sponsors are expecting from archeology answers that are not going to result from excavation, and it is the archeologist's responsibility to explain where archeology can contribute to our knowledge of the site and those areas where it is likely to produce little. Often the sponsor is looking for some direct parallel between the historical documentation and the archeological record, and such an expectation is highly unrealistic in many cases.

Because the archeologist must satisfy the demands of his sponsor and his professional responsibility he should not neglect either in his report. This being the case the report should clearly and fully outline the research goals of both the sponsor and the archeologist. This should be followed by a statement of the theoretical base from which the search for these goals will be launched. It should then proceed to explain how these goals were sought through the archeological process, with a synthesis of the nature of the observations made being presented. The data recovered should be presented in the form of a synthesis of the various analyses that were conducted on features, distributions, relationships, artifacts, etc. The cultural-historical integration and interpretation emerging from the synthesis should follow, with any resulting processual explanation in terms of hypothesis and theory being presented in synthesis form. Specific suggestions for further work should be made, as well as recommendations for historic site development if such is planned. In other words, the basic scientific procedure should be followed in report
writing of goal and hypothesis formation, observation and data collection, analysis, interpretation, and synthesis and explanation of the results, with suggestions for new hypothesis formation, future research needs, and recommendations for the stabilization and interpretation of the archaeological remains. With this format the goals of the sponsor of archeological projects, and those demanded of the archeologist by his role as a scientist can be met. This basic outline is summarized as follows:

1. outline of research goals and hypotheses
2. theoretical base from which the archeologist is proceeding
3. outline of the archeological process used to attempt to achieve these goals
4. synthesis of the analyses conducted on the various classes of data
5. cultural-historical integration of the data
6. processual explanation in terms of hypothesis and theory
7. suggestions for further archeological research
8. recommendations for stabilization and interpretative development of the archeological remains

When a sponsor of a project wishes to evaluate an archeological report he can refer to this basic outline and see whether or not the report he has in hand meets these basic minimum requirements. If what he has been presented is primarily a description of postholes, pits and potsherds, then he has good reason to complain of its lack of depth. The comments to follow will focus on a plea for a new direction on the part of historical archeologists to orient their efforts toward the scientific, synthesizing format reflected in the above outline.

The historical archeologist has an increasingly expanding responsibility to inquire beyond the mere validation of an historic site through correlation with documentary evidence; beyond merely listing the presence or absence of artifact types for establishing the temporal position of the site; beyond the revealing of architectural features for the purpose of reconstruction and restoration; beyond exposing ruins for the entertainment of the visiting public to historic sites; and beyond the process of recovery and preservation of relics from the past hoarded into repositories and museums! His view must be as broad as the questions being asked by archeologists, sociologists, anthropologists, ecologists, biologists, archaeo-parasitologists and other scientists who are increasingly turning to historical archeology to reflect some light on their special problems and spheres of interest. However, although archeology is broadening its scope, the primary emphasis will continue to be in the area of material culture where so much must still be explored... (South 1968;1970: 54).

The demonstration of patterning of the material remains from archeological sites, and the integrative synthesis of these data in terms of the explanation of progenital cultural patterns, is the direction historical archeology must take to emerge from the sterility of purely
In historical archeology there is a present emphasis on goals aimed at greater accuracy, authenticity, validity, correlation, personalization, and public interpretation of "historical reality". This emphasis places the focus on history, with archeology acting as a literal hand-maiden to the written record. This situation stems from the fact that historical archeology is stimulated and supported by our national historic site preservation-restoration-reconstruction-nostalgia phenomenon. Archeology does make a contribution toward goals dictated by this phenomenon, but these goals are secondary by-products of its primary function, the integrative explication of patterned material remains of culture stemming from human occupation.

The usual emphasis of historical archeology site reports is one of the following:

1. Archeology is used to "fill in" historical documentation.
2. Archeology is used to locate architectural features.
3. Archeology is used to recover artifacts which are then described in great detail, often to no apparent end (pseudo-analysis).
4. Archeology is "correlated" with historical documentation.

Historical archeology site reports seldom rise above one of these levels of presentation, and the reason lies, in this writer's opinion, in the absence of a concentration on the discovery and synthesis of patterned material remains of culture stemming from human occupation. With such a guideline the emphasis must be on synthesis based on detailed analysis. Site reports must be firmly anchored in archeological data, with emphasis on integrative synthesis rather than on the analytical description of data, unless such analysis makes a useful contribution to our knowledge!

Therefore, to conduct an analysis of six gunflints or six projectile points from an archeological site, or an analysis of anything, requires a research hypothesis under which certain attributes are called for in relation to the design. The recording of no more involved an attribute than "feather-edging" on creamware is on the same level as the multi-attribute recording of a complex set of data for the purpose of determining pattern through sophisticated statistical analysis, provided both statements are made within the framework of the postulates and hypotheses of a research design. The meticulous recording of attributes as an exercise contributes nothing new to our knowledge without the explanation for such data-recording within our research design. Thus the illustration of artifacts simply as a matter of record is a useless procedure if better illustrations of the objects have been published elsewhere, since such illustration does not add to our accumulation of knowledge.

In 1955, J. C. Harrington recognized that historic site archeologists had a compulsion to illustrate every object recovered from a site, and unfortunately such is still often the case:
Unfamiliar as he is with the cultural material encountered, the reporter on historic site excavations feels that he must describe and illustrate every object. This procedure was often necessary with his Indian materials, for he had not been privileged to work with ceramic types which could be neatly characterized by such simple phrases as, for example, "Wedgwood creamware" or "Lambeth delftware". He is inclined, therefore, to devote unnecessary space in his report to lengthy objective descriptions when a single word or phrase would suffice. In some cases, however, careful descriptions are needed, as of, for example, the products of local craftsmen. Here, as in field methods, the necessary judgment and selectivity can be acquired only from training and experience (Harrington 1955:1127).

Harrington's statement about "training and experience" might lead one to infer that only through experience could you acquire a sufficient grasp of the historic site materials to successfully avoid the description and illustration of masses of artifact data, but this is just not so for the scientific archeologist. With the numerous sources available for research of historic site materials, with illustrated examples of ceramics, glassware, etc., often in color plates, an archeologist with a scientific frame of reference can, through a careful study of attributes, etc., write a cogent synthesis of his data at least as good as the usual descriptive reports, and considerably more useful.

Ivor Noël Hume has recently emphasized the need for archeologists to rid their reports of unnecessary descriptive weight:

...the illustration of a few rim sherds of common 18th-century ceramic forms that are already on record as having been found from southern Australia to northern Canada, contributes virtually nothing—unless they happen to be incorrectly described, and so warn the reader to beware of the whole report. I am not saying that this material should not be recorded or that any detail should be omitted from the final manuscript. But I am saying that a small number of copies of that report, cheaply duplicated, and housed in safe, known repositories, is all that is needed. Much more valuable to fellow archeologists, curators, and social historians, are research studies on specific topics, stemming from excavations and which have something new and useful to say. When money and publishing outlets are scarce, it is these studies that will be of the greatest practical value. (Noel Hume 1973: 7)

The phrase "research studies...which have something new and useful to say" is the critical one for reflecting the attitude that can be used as the basic yardstick for evaluating the contribution made by an archeological report.

In 1955 the field of historical archeology was not ready for Harrington's advice. Only Harrington and a handful of colleagues...
were around to listen, and fewer still have heeded his remarks, as emphasized by Nöel Hume's recent reiteration of the same point. However, within a decade, historical archaeology will be flooded with young minds bringing to the field the best of theory, statistics, and a scientific base of operation. Hopefully their reports will not be merely descriptions of artifact attributes, but will be within a framework of a research design anchored in a firm theoretical base of scientific analysis and synthesis.

As archeologists we must depend on our archeological tools for our interpretive statements of archeological data, and not resort to the easy expedient of superimposing our historical data onto the archeological record. In our final interpretive statements we do, of course, use both the archeological and the historical data, but we should not use the documented history of the site as an interpretive crutch to prop up our statements purporting to be archeological in nature. If we develop such habits, and then find ourselves in a situation where there is no documentation to lean on, we may well find that our archeological tool kit is empty, or that we do not know how to use the tools we have available with which to make interpretive statements of archeological data. Such a leaning-on-the-arms-of-history approach to historical archaeology is rendering a disservice to archeology by not utilizing to the fullest the patterned data it is capable of producing.

There is apparently an assumption in historic site archeology that archeological data must have a direct historical counterpart. There is, of course, nothing wrong with archeological-historical connections, but this is certainly not the primary archeological goal for the historic site archeologist. As archeologists we are dealing primarily with material culture, the patterning in the archeological record reflecting the cultural patterning responsible for that record, with the forces creating that patterning very likely not recognized at all by the individuals or the society from which the patterns emerged. Therefore, archeologists should focus their efforts toward the discovery and explication of patterns of material culture (See Harris 1968: 359, for a statement of this position). The patterning he discovers may well have absolutely no historical counterpart, and indeed mutually exclusive data sets between the historical and archeological documents almost appear to be the rule rather than the exception.

Our appeal here has been to urge historic site archeologists to become more selective in their presentation of their data. This admonition is aimed at the goal of making archeological data from historic sites more useable not only by the sponsors of the excavations, but by historic site archeologists themselves. The presentation of data is always a selective process. We cannot possibly list all the attributes conceivably of use to someone someday, and attempts at this have often led to heights of absurdity that would be laughable if they were not so tragic. This is admirably exemplified by one writer by the measuring in millimeters of the size and thickness of the broken sherds of English ceramics! (Krause 1972: 82).
In our efforts at interpreting patterns of culture let us not engage in pseudo-science mis-directed toward meaninglessly translating a potsherd into a series of mathematically expressed numbers; or pseudo-history attempting to discover archeological equivalents to historical events; or pseudo-archeology involving endless descriptions of artifacts and features to no apparent end. Rather, let us systematize our selectivity, and direct our efforts toward synthesizing patterns of material culture from our archeological data, and in doing so reveal the patterns resulting from cultural activity. Such patterning may well allow us to gain insight into the behavior patterns of the people responsible for the archeological record, and allow us to make explanatory interpretations relating to culture process.

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SOUTH, STANLEY

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A NOTE ON THE SOCIETY FOR
AMERICAN ARCHAEOLOGY SEMINAR ON REPORT WRITING

Charles R. McGimsey III, President of The Society for American Archaeology, through a grant from the Interagency Services Division of the National Park Service, is exploring new directions for the Society and the profession of archeology by means of six seminars during the summer and fall of 1974. The seminar on archeological report writing was held from July 31 through August 5 at Airlie House, a conference center in Virginia. The goal of this seminar was to prepare guidelines for the preparation and evaluation of cultural resource management studies which would be of use both to the sponsors of archeological projects, and to archeologists.

The seminar, composed of Keith Anderson, Hester Davis, Rob Edwards, Michael Schiffer, Stanley South, and Gwinn Vivian addressed itself to the construction of guidelines for the preparation and evaluation of reports resulting from expended Federal, state and private funding of archeological research. The primary objective was to define the content of cultural resource management studies. It was also the Seminar's objective to make it clear that in order to further the aims of the discipline of archaeology investigators should not only address themselves to the questions of the project sponsor needs, but to those relating to current archeological research needs as well. In order to achieve these objectives it was necessary to consider archeological reporting within the framework of general scientific standards of reporting and professional expectations. Cultural resource management studies are one variety of empirical research in archeology. The seminar outlined general content guidelines for reports of empirical research more precisely than had heretofore been done, and this basic scientific format was then used to address the specific responsibility with which the seminar had been charged; drafting guidelines for the preparation and evaluation of cultural resource management studies.

The relationship between sponsor planning and archeological research goals, the archeological process, the general scientific guidelines for report preparation, the various classes of reports dealing with cultural resource management are illustrated in the accompanying heuristic device, "Milking the Archeological Cow".

A report on the seminar will be distributed to the members of the Society for American Archaeology in order that they may have the opportunity for comment prior to any action which might be appropriate by the Executive Committee at the November meeting in Mexico City.
Milking the Archeological Cow

S.A.A. Seminar Stall No. 2
Charles R. McGimsey, III, Mgr.

Archeology

General Scientific Guidelines for Report Preparation

CULTURAL RESOURCE MANAGEMENT STUDIES

Cultural Resource Management Reports

RESEARCH GOALS
CULTURAL RESOURCE MANAGEMENT PLANNING
National Environmental Policy Act
Corps of Engineers, Dept of Interior
Arch Conservation Act, BLM, Etc.
State and Local Agencies, Etc.
"A paradigm, a paradigm, a most ingenious paradigm"

In his recent memoirs Binford describes himself as inviting students for relaxed discussions under his spreading trees and encouraging them to take him on in intellectual arguments (Binford 1972a: 450-451). This picture of benign scholarship does not wholly square with two equally recent papers by Binford - his comments on South's "Evolution and Horizon as Revealed in Ceramic Analysis in Historical Archaeology" (Binford 1972b) and his response to Hanson's criticisms of the Binford pipestem-dating formula (Binford 1972c) - and it is certain of the comments and concepts in these latter two papers and some of their relationships to the Binford memoirs and the so-called New Archaeology that will be discussed here.

If one word can sum up Binford's belief in what archaeology should be it would appear to be "scientific", and if another word can sum up his opinion of those archaeologists who are not of his persuasion it appears to be "historians". Thus faced with Hanson's criticisms (Hanson 1971) of his pipestem-dating formula (Maxwell and Binford 1961: 107-09; Binford 1962), Binford responds that Hanson's conjectures indicate

...that I did not know how to calculate a regression, and secondly that I am unable to round numbers and correctly add, etc. This is the most patent kind of insult and, in addition, supplies us with a very nice example of the kind of methodological difference separating historians from scientists.... A scientist being fully aware of the role of assumptions in any warranted argument is generally interested in their validity. Historians seem blissfully naive regarding their own thought processes and even defend this innocence by scorning theory and philosophical problems of epistemology (Walker 1970a). Hanson may well protest at this point that I too am making an assumption, that he is an historian. I don't know what his training has been but he behaves like one.... Hanson should have written me regarding his questions about the data and my summary of it before engaging in "conjectural history" based on a false assumption.... I hope that this exposition of tobacco pipe stems will help to demonstrate some of the differences between the approaches of scientists and those not so committed (Binford 1972c: 234-235, 251).

We thus have the essential difference, in Binford's mind at least, between scientists and historians. Historians suggest scientists might not always get their arithmetic correct, they scorn theory, and worst
of all they do not write Binford for a personal explanation of what he really means before criticising his publications. Apparently if we cannot understand what Binford says the fault cannot be Binford's: it is merely our ignorance, caused by our naive, untheoretical, unphilosophical, and unepistemological education. Also sprach Binford! The scientificness of this approach escapes me, but then according to Binford I am an historian and therefore naive, untheoretical, unphilosophical, and unepistemological, so perhaps to the New Archaeology at any rate this approach is perfectly scientific.

Binford, however, is forced to admit that his presentation of his pipestem-dating formula was inadequate. The reasons apparently were that:

At the time the formula was calculated it was done as a personal expedient for testing the validity of Harrington's observations. Later it became clear that it was of general utility. By that time I had misplaced the original data and it appears that at the present time the original data is no longer available.... In fact, I am not sure that I even made any attempt to save it at the time I calculated the formula since I was toying with Harrington's observations simply to evaluate their utility as a possible research tool.... Since there was a subrosa [sic] knowledge of my formula among historic sites archaeologists I orally reported on it... and a transcript of this report was published.... (Binford 1972c: 231, 235).

This explanation of what really happened has all the hallmarks of being correct, for the two published accounts of the formula are from internal evidence just what Binford now says they were: the incidental inclusion of the formula in the Fort Michilimackinac report as it had been used at that site (Maxwell and Binford 1961: 107-09), and the oral presentation in 1960 and subsequent publication of a formula already known through the professional grapevine (Binford 1962). However, the Authorized Version implies a much more formal status for at least the latter presentation:

Archaeologists must explore the statistical and mathematical techniques available from other fields to increase their abilities in isolating and measuring relationships. This kind of searching is clearly demonstrated in my Kaolin pipe paper. Experimenting with statistical techniques has occupied much of my research time, and in some cases the results are clearly evident in my published papers (Binford 1972a: 330, cf 9).
There is no suggestion here that the formula was something Binford had been only "toying with", nor that its 1962 presentation was simply designed to formalize something already known informally in the field; further, despite Binford's quite valid observation (1972c: 248-50) that the cases of inaccuracy which will arise with his dating system are as important as the cases where the dating will be accurate, it is quite clear from the original presentations of the Binford formula that it was designed simply as a dating technique, unlike South's ceramics dating formula which was tied from the first to the equally-important, and interpretive, concept of cultural horizons. We are left to assume Binford's published presentation is an accurate, adequate, and of course scientific example of what archaeologists should be doing and on what Binford has been spending much of his research time. Until and unless the elusive Ur-Binford text turns up, however, we are forced to reply on an undocumented secondary account, which is hardly a shining example of accuracy, adequacy, or the scientific approach, unless I am missing something through being naive, untheoretical, unphilosophical, and unepistemological.

The key word which all of us working on pipe material and utilizing the Binford formula for the past dozen years have apparently overlooked, and on which we could have been enlightened if only we had written the guru and asked him what he really meant, is the word "original" in the statement "This I was able to do by using Harrington's original percentages and converting them to mean hole diameters for the given time period" (Binford 1962: 19; quoted in Binford 1972c: 235 with "original" underlined). This, we should all have known, refers not to the Harrington article (Harrington 1954) but to the original data from which Harrington derived the graphs in his article. Apparently a course in textual analysis and exegesis is necessary to interpret Binford's article. Are we really to believe this is a scientific presentation? The tone of Binford's attack on Hanson leaves no doubt that Binford realizes he has been caught with his tweed coat down and is mad about it.

Earlier I quoted Binford observing that "A scientist being fully aware of the role of assumptions in any warranted argument is generally interested in their validity" (Binford 1972c: 234). Having been dubbed an historian, and therefore presumably being unable to produce warranted arguments or consider evidence of validity, I may be being presumptuous, but let us examine some of Binford's assumptions on the subject of clay pipes. First, he says they are made of kaolin. They are not: they are made of ball-clay as I have stressed in three previous articles (Walker 1970c: 160; 1971a: 26; 1971c: 19). Like Binford I initially assumed they were made of kaolin — American-archaeological literature still invariably so labels them — until I investigated pipe-manufacture; and had Binford been interested in pipes as socio-economic history rather than as merely a vehicle for toying around with some mathematics he too would no doubt have discovered this fact. Not perhaps an earth-shattering error, but one which should not have been made by a scientist.
Secondly, the inaccurate median date derived from the Binford formula for the Fort Michilimackinac occupation span is attributed to an increase in population in the later period of the fort and to "increased logistic efficiency" (sic - Binfordese for more supplies?). As a "scientist" Binford has no time for historical explanations, so the major historical event in the lifetime of the fort, its transfer in 1761 from the French to the British, is not even mentioned, and so the possibility of there being an historical or even political reason for an inaccurate date is utterly ignored - even though Harrington had noted (1954: 3) that Dutch pipes tended to have narrower stem bores than English pipes of comparable date and even though Omwake had identified Dutch pipes at Fort Michilimackinac (Maxwell and Binford 1961: 109). Indeed, a far more obvious historical event occurring about the time Binford says his formula ceases to be accurate is the American Revolution, which certainly caused both short-term and long-term disruption in established trade patterns - the cessation of trade in English Buckley earthenware, for example, and the appearance in Revolutionary contexts of French faience and Dutch pipes (Walker 1972a: 129 and refs.). Certainly first the Stamp Act troubles of 1765-66 and then the American Revolution practically killed the Bristol pipe-industry, which did not begin to revive until the first decade of the nineteenth century (cf Walker 1972b: 11). All these factors, however, are connected with British political and economic history and therefore presumably ignored by Binford. That increases of population and an increased supply of pipes could effect the Binford date for a site is not denied, either for Fort Michilimackinac or anywhere else, but what Binford has done is to fall into the "fallacy of a single cause" (Beveridge 1950: 117, 1961 ed.).

Thirdly, regarding the breakdown in accuracy of his formula after ca. 1780, Binford states:

In the way of explanation it is quite obvious that with the influx of pipes manufactured in Montreal and at other seats of American [sic] pipemaking there is a corresponding re-occurrence of certain "early" styles, in addition to the appearance of a new style of elements. This break in the traditional direction of stylistic change is responsible, I feel quite sure, for the breakdown in the correlation after roughly 1780 (Binford 1962: 20).

As this explanation is so obvious to Binford presumably lesser researchers should have written him to obtain his evidence, for none is given here nor are footnotes cited (unless the evidence was in the missing text). His evidence for a Montreal pipe-industry ca. 1780 would be particularly welcome, seeing that present available evidence shows the Montreal pipe-industry to be wholly a phenomenon of the second half of the nineteenth century; the first known pipemaker there being recorded in 1847, the last in 1907, and the zenith of the trade being in the 1870's (Walker 1971c: 25). The only other North American pipemaking centre of any size known
from present research is Detroit, where the industry came from Montreal in the late 1870's as the Montreal industry began to decline and where it too died out at the beginning of the twentieth century (Walker loc. cit.). The Glasgow pipe-industry, which if Binford knew anything about pipes he would know was far more predominant in nineteenth century North American markets than any native industry, only begins to achieve any importance in the early nineteenth century and present evidence does not suggest it was taking over the North American market before ca. 1840 (cf Walker 1971c: 23, 25).

If a change in source of supply is indeed responsible for the breakdown of the Binford formula the evidence is going to be forthcoming from identification of makers of marked pipes from datable contexts (or alternatively identification of datable regional pipe styles) and an examination of trade patterns, both of which approaches are straight history and neither of which needs to rely on the Binford formula for assistance. Thus, Liverpool-made pipes started appearing on North American sites from the 1760's into the early nineteenth century (paralleling a sudden expansion of the Liverpool pipe-industry) while Bristol pipes had been declining in frequency on North American sites from the 1730's (cf Walker 1971c: 22-23; 1972b: 10-11). Professor Lehrer has suggested that with the New Maths (or Math) it is more important to understand what one is doing than to get the right answer (Lehrer 1965a): apparently with the New Archaeology it is not even important to understand what one is doing.

And still on Binford's alleged causes for the breakdown of his formula ca. 1780, one would like to know what he means by "the appearance of a new style of elements" and the "break in the traditional direction of stylistic change": to what "elements" is he referring, and what is the "traditional direction" which apparently breaks? Is he suggesting that a change in bore diameter is a stylistic change? Were all these points discussed in the lost text too?

And fourthly, though it appears not in Binford's comments on Hanson's criticisms but in his comments on South's paper (1972c: 249), we have an excellent example of the over-generalization far too common in the field, in this case resulting in a serious distortion of statements this writer made some years ago. Binford, talking about factors "which would tend to bias the pipes present in one area in favour of some manufacturers" [sic], says "I need only cite Walker's evidence for higher frequencies of Dutch made pipes in the Northeast (Walker 1965)". What in fact Walker (1965) showed was that at one site in eastern Canada at one particular period of time within the lifetime of that site a sizeable number of Dutch-made pipes did occur and that at that one site the historical evidence - naive, untheoretical, etc., as it is - suggested the French occupation of that site was the source of that Dutch material and that this possibly was the cause of inaccurate Binford dates. Neither in my 1965 paper, nor in my detailed examination of pipes from certain areas at Louisbourg (Walker 1971b) of which
the 1965 data was a part and with which Binford appears not to be familiar, do I make any suggestions whatsoever that on the basis of my work one can say there are "higher frequencies of Dutch made pipes in the Northeast". What Binford's statement here means is that any archaeologist working in the Northeast who finds his Binford dates are inaccurate can say, and cite Binford's statement as proof, that the reason is the presence of Dutch pipes. From this of course one can then say there are Dutch pipes on the site even if one could no more identify a Dutch pipe from the literature than one could a checked stamped pot; in fact, one does not need to use such naive, untheoretical, etc., sources as history because Binford has "proved" the presence of Dutch pipes scientifically.

Binford says elsewhere (1972b: 122) that he believes historical interpretation "is dependent upon valid general propositions which can serve as pivotal points for interpretive arguments treating the specific facts of a specific case". A wide range of researchers, myself, South, philosophers of science like Hempel, physical anthropologists like Washburn, animal pathologists like Beveridge (Walker 1972a: 139 and refs.; South 1972: 79, 102 and refs.) agree that theories and hypotheses be invented to account for observed facts rather than be derived from them, but the facts have to be observed and the theories and hypotheses made accountable. Binford's "valid general propositions" here are either whole-cloth fabrication - such as the explanations as to why the Binford formula breaks down towards the end of the eighteenth century - or one-swallow-makes-a-summer house of cards such as the above remark on Dutch pipes in the Northeast. For my part I specifically stated, after tentatively suggesting the substantial quantities of Dutch pipes in certain deposits at Louisbourg were the cause of inaccurate Binford dates, that "Further deposits containing large amounts of Dutch material would have to be analysed before a definite statement could be made" (Walker 1971c: 119). Binford as in so many other cases where he makes dogmatic assertions on pipes, offers no original work of his own or even an adequate examination of others' work to suggest the single specific example I gave can be accepted as a, let alone the, reason for inaccurate Binford dates in the Northeast. In theoretical discussion Binford can talk of hypotheses "which must be tested against independent data" (1972a: 93), but in practice he betrays a total inability to handle basic data. One can hardly have valid general propositions if one cannot handle the evidence which might validate them.

Further, in generalizing about the Northeast - New York, New England, and the Atlantic Provinces - Binford is fatuously ignoring the very different historical backgrounds in these areas: Dutch pipes from sites in eastern Canada and other parts of the continent where there was French settlement in the eighteenth century do in fact appear on present evidence to indicate French occupation, but seventeenth century Dutch pipes in New York and New Jersey certainly do not indicate French influence but reflect the Dutch occupation of that area. Dutch eighteenth century pipes in northern New York state, however, could plausibly be associated
with French influence. In New Jersey indeed, Dutch pipes in mid-seventeenth century contexts might at least as easily belong to its period as a Swedish colony, for the New Sweden Company in fitting out the settlement of the colony is known to have obtained Dutch pipes (quoted in Omwake n.d.: 8) and Dutch pipes were certainly common in Sweden in the seventeenth and eighteenth centuries (Puktorne 1968: passim). And to jump continents, Dutch pipes from West African historical sites need by no means indicate Dutch occupation or trade, for in 1719 it is noted (quoted in Donnan [ed] 1930-35: II, 241-42, 1965 ed.) that though it was forbidden for English traders to obtain Dutch pipes for sale in England they were now permitted to buy them for trading on the African coast because Dutch pipes were cheaper than English. Thus Dutch pipes on West African sites can mean either English or Dutch trade, and indeed possibly French, Danish, Swedish, and Brandenburg trade as all these countries had posts at one time or another in West Africa and appear to have relied heavily on Dutch pipes. But all this requires a sound background in historical evidence, and all Binford has is his desk calculator (Binford 1972a: 9, 188), to which, Linus-like, he seems inseparably linked. If Binford thinks it funny that Griffin was hailed in 1938 as a master of statistical technique when all he had done was count something (Binford 1972a: 3) is it any less funny that Binford is hailed a generation later as the father of the New Archaeology because he uses an adding machine?

It is a supreme irony that Binford can quote Washburn's remarks about the need to get beyond mere accurate description (Binford 1972a: 99, also quoted in South 1972: 79) yet fails to meet the required scientific scholarship necessary to present his data so that it can be meaningfully used in this fashion. Again, in generalizing as to the cause of Dutch pipes in northeastern North America Binford falls into the fallacy of a single cause, and he compounds this with two other unscientific errors. First he ignores the fact that experimental results are valid only for the precise conditions under which the experiments are conducted and that hypotheses are true only for the particular circumstances prevailing in these experiments. Secondly, he forgets that generalizations can never be proved and are accepted in practice only after all attempts to disprove them fail (Beveridge 1950: 88, 1961 ed.), something which Binford conspicuously fails to do here as elsewhere, despite his urging that "Anthropologists smugly displaying their scorn of historians must stop working as historians and start working as scientists to meet the need for valid general propositions" (Binford 1972b: 122). The physiologist Claude Bernard could have been thinking of Binford when he said "Men who have excessive faith in their theories or ideas are not only ill-prepared for making discoveries; they also make poor observations" (quoted in Beveridge 1950: 49, 1961 ed.).

At best, the Binford formula correctly used would give a date differing from that expected (if the context had been known, and accurate, historical dates) which would tell the archaeologist to examine his pipes to see if he had some "non-standard" material, and if so
describe this material; the historical approach, based on a knowledge of pipes, would be to examine the pipes, recognize some were Dutch, and offer an historical reason for their presence. Neither of these last two stages could be achieved by a Binford archaeologist unfamiliar with the historical evidence, however, and as that archaeologist, being unfamiliar with pipes, would be unable to give an adequate description of his pipes, let alone discern which were "non-standard" (Walker 1972a: 142-43), one can see no advantage, but rather several disadvantages, in using the Binford approach rather than the historical approach. Only the historical approach provides identification and interpretation. The only claim to advantage of the Binford approach is that one can get away, to a limited degree, with pure ignorance.

Until those involved in historical archaeology realize that the Aristotelian belief that someone who has studied the nature of inference can judge the validity of inference without any special knowledge of the subject-matter is wrong and put study of cultural remains and their contexts before philosophical interpretation they are going to remain nothing better than technicians, which is what Binford and others like him are (cf Walker 1972a: 140-41, 142-43, 145-46, 146-47). Because of this preoccupation with doing things backwards the Maxwell and Binford report on the 1959 season at Fort Michilimackinac (Maxwell and Binford 1961) is useless as a provider of information on the material culture of an eighteenth century frontier post with first French and then British occupation. South emphasizes in the strongest terms in his "Evolution and Horizon..." paper the need for an adequate knowledge of ceramics on the part of the archaeologist if his ceramic dating formula is to be meaningful:

The degree of refinement of the model is dependent upon the degree of sophistication of the archaeologist's ceramic knowledge.... For the formula to be used, therefore, a knowledge of ceramic types is necessary, which can be learned from the many references available. This reference work must be combined with a familiarity with the archaeological specimens. A knowledge of the ceramic type attributes cannot be overemphasized for there are far too many meaningless descriptions appearing in the historic site literature now in spite of the availability of numerous excellent sources to act as guides for learning (South 1972: 80, 86).

yet Binford entirely ignores this vital basic point in his comments on South's paper. To the extent that only the archaeologist excavating the site can interpret what he is excavating R. H. Thompson is correct when he provocatively states:

The individual investigator with his unique combination of interpretive skills provides the only possible
means for the reconstruction of the cultural context of an archaeological collection. The final judgment of any archaeologist's cultural reconstructions must therefore be based on an appraisal of his professional competence, and particularly the quality of the subjective contribution to that competence (R. H. Thompson 1956: 331).

A simple, well-illustrated, corpus of the ceramics from Fort Michilimackinac instead of the offered hodgepodge of inaccurate and meaningless terminology would have been ten times as valuable as comments on vectors of skewing or the results of chi-square tests. As it is, if the data so scientifically analysed at Fort Michilimackinac was based on the farcical sub-categorizing indicated in the section on ceramics in the Fort Michilimackinac report (Maxwell and Binford 1961: 94-95) with its groupings such as "white-glazed delft" and "tin-glazed delft" then Binford was skewing nonsense and chi-squaring rubbish; and it is a mercy for historical archaeology that since his Fort Michilimackinac days Binford has not been engaged in that field (Binford 1972c: 231). Binford sub-titles his comments on South's paper "A Step Toward the Development of Archaeological Science" (Binford 1972b: 117): how would he know if the paper were scientific? - his lack of knowledge on historical ceramics would prevent his ever being able to use it.

If Griffin, according to Binford, has never gone beyond the study of artefacts it is no better that Binford attempts to do so without first studying artefacts. Certainly if one never goes beyond using artefacts to date a site little of the potential of archaeology has been realized, but if one insists on interpretive speculations without first having settled the essentials of the site such as its date then one cannot complain if people ask why (cf Binford 1972a: 10-11). As Beveridge points out, more discoveries have come from intense observation of very limited material than from statistics applied to very large groups: the value of the latter lies mainly in testing hypotheses arising from the former (Beveridge 1950: 105, 21, 1961 ed.). Darwin, who is commonly accepted as a scientist, maintained "I must begin with a good body of facts, and not from any principle, in which I always suspect some fallacy" (quoted in Beveridge 1950: 85, 1961 ed.). Binford, however, insists in going at it backwards - for he clearly knows nothing about pipes or any other historical artefacts - and thus comes up with absurdities. For similar reasons I have criticized several "scientific" approaches to historical artefact analysis (Walker 1972a: 140-43): it would be a major advance in the field of historical archaeology if those moving into it knew something about the data whereof they spoke.

But, it might be argued, such criticism of Binford based on detailed knowledge of a small and specialized field is unfair in that it obscures the usefulness of Binford's concepts even if they do not fit the particular evidence of a specific situation. There is some truth to this - a common tendency among historians reviewing Toynbee was to praise him for his
breadth of concepts then pan him for all the errors of fact made in that part in which the reviewer was an expert, and more recently Clarke's *Analytical Archaeology* has received similar criticisms - so we can examine Binford's comments on South's "Evolution and Horizon..." paper to see whether they exhibit a more scientific approach. Alas, this does not appear to be the case. Binford starts out:

Stanley South's paper is excellent. It argues a closely reasoned justification for the development of a research tool which when properly applied should be of great value to historical archaeology (Binford 1972b: 117).

He then goes on to discuss the recent exchanges in the field of historical archaeology theory stimulated by the 1967 South Forum when Clyde Dollar's "Some Thoughts on Theory and Method in Historical Archaeology" (Dollar 1968a) was presented. Binford's comments, mainly directed against the alleged shortcomings of historians and historical research, continue for six and a half pages and are then tied into two pages of general comments on South's paper. For obvious reasons, I would be the last to criticize papers in the South Fora for their rhapsodic nature: indeed, the most stimulating thing about these Fora is that one can - preferably using the theme paper as a basis - get well beyond the mundane book-review sort of critique; but the Forum contributor has some responsibility to analyze critically the concepts of the pivotal paper, and simply to say as here that South's paper is "excellent" and "closely reasoned" is not only naive, untheoretical, unphilosophical, and unepistemological but grossly unscientific. Once again - Thus spoke Binford! Where is the rigorous scientific method, the objective quantification of evidence, the explicated epistemology and nomothetic paradigms of the New Archaeology? The best, and by far the most incisive, appraisal of South's paper was L. M. Stone's five pages (1972): could not Binford, co-founder and father-figure of the New Archaeology, have given us such an old-fashioned display of simple logic? - it would not even have needed a knowledge of historical ceramics in this case, so Binford would not have been disadvantaged.

The simple fact of the matter is that historical archaeology by definition relies on historical interpretation for explanation. As Dollar put it a few years ago, "the name of the game is History, and if you have not played it according to its rules, then you have played in vain" (Dollar 1968b: 188). To fall back on facile generalization such as "logistic efficiency" and "the traditional direction of stylistic change" as explanations when historical evidence can offer far more precise and concrete suggestions is to betray an utter ignorance of the field of history and how its evidence can be used.

That such blissful ignorance is widespread is suggested by more than one study in the field of historical archaeology by anthropologists. Cleland's worthwhile study of faunal remains from Fort Michilimackinac
(Cleland 1971a) correctly suggests that the difference between the British dietary pattern and that of the French can be plausibly explained by the former having superior naval power and a more active colonial policy, thus permitting a much better supply system; but the historical generalities in the conclusions read rather like a Rule Britannia interpretation of Noël Coward Englishmen transporting British culture overseas and establishing some corner of a foreign field that would be forever England. In fact, this caricature of British imperialism and its attendant Ryder Haggard and Sanders of the River folklore — from which Cleland appears to have derived his model — is one of the later nineteenth century and has nothing to do with events 100 years earlier at Fort Michilimackinac. It is naive, if not actually untheoretical, etc., to deduce from the evidence that the French displayed no tendencies to maintain or imitate the traditions of their native land while the British did; the evidence suggests simply that the British were able to but that the French had fewer opportunities to. (On the matter of mastering historical data one might point out that crépes suzette [Cleland 1971a: 18] would not have been known to Frenchmen of any class at this time: like British imperialism, they are nineteenth century in date [Hale et al 1968: 727]. Tracing the origins of coq au vin [Cleland loc. cit.] proved difficult: one source [Oliver 1967: 148] appears to indicate that coq au vin is also less than 100 years old and that the use of wine in this fashion only became important from the beginning of the nineteenth century; the Larousse Gastronomique [Montagné and Gottschalk 1938: s.v. Coq] quotes only "une recette ancienne", while Fisher's The Cookery of Provincial France suggests it is a traditional Burgundian dish [Fisher 1968: 24, 119-22]. It may be suspected that cooking flesh with wine must have been known from time immemorial in wine-growing areas; in any event coq au vin is not haute cuisine, as the Cleland reference implies, but cuisine bourgeoise.)

The idea of history as "specific things and events ordered in time" (Cleland and Fitting 1968: 132) or some similar piece of jargon is one which appears to be standard among anthropologists. Leslie White, whose ideas Cleland and Fitting paraphrase, commences his paper "History, Evolutionism, and Functionalism: Three Types of Interpretation of Culture":

There is a widely held view in contemporary anthropology according to which there are two, and only two, kinds of interpretative studies of culture: the "historical" and the "scientific". Historical studies, according to this view, are those which deal with chronological sequences of unique events (White 1945: 221)

and while he goes on to expound his belief that there are not two but three kinds of interpretative studies of culture his definition of history does not change:

the temporal process [is] a chronological sequence of unique events, the study of which is history;....
the historic process deals with events determined by specific time and space coordinates, in short with unique events (White 1945: 222, 230).

This is, as we shall see, an outdated concept of history, and it is undoubtedly this which contributes much of the confusion to the field. The result is a straw man: history is only specific things and events ordered in time and how much more lofty and cosmic a field of endeavour is anthropology, the study of man. And so it is, given the above definition of history; but as that definition of history is about as realistic as one portraying anthropologists as individuals studying nothing but the sex-lives of modern primitives the whole comparison falls to pieces.

Paul Chace, in his interesting paper "The Chinese Horizon in America: the Archaeology of Railroad Camps" given at the autumn 1972 meeting of the Council for Northeast Historical Archaeology at Oneonta, New York State, prefaced his talk by stressing that history was only specific things and events etc., but that his study exemplified the anthropological approach; however, he then went on to use precisely the same sources and give the same analysis as any competent historian would have done. As for visiting the sites of railway camps, which it might be argued constituted a different approach to that which an historian might use, the book History on the Ground - which contains the comment that a good pair of boots are part of minimum equipment of an historian in the field (Beresford 1957: 249, 1971 ed.) - was written by an economic historian nearly 20 years ago; and Beresford is only one modern representative of a British tradition of field archaeologists and historians which goes back to the fifteenth century and includes such observers as William Camden who in the 1594 edition of his Britannia noted, identified, and correctly explained crop-marks (quoted in Ashbee 1972: 42).

Again, Dethlefsen and Deetz in their equally-interesting study of Massachusetts tombstones (Dethlefsen and Deetz 1966) stress the anthropological nature of their work when in fact they are studying social, economic, religious, and art history, plus historical geography and some industrial archaeology - all aspects of the field of history. The appearance of a so-called Doppler effect in plotting rates of stylistic change (Deetz and Dethlefsen 1965) hardly converts the study of a science.

Binford in his comments on South's paper is much exercised about the "uniqueness" of historical events, which is what he apparently feels causes the innate inferiority of history compared to anthropology (Binford 1972b: passim). Of course historical events are specific (not unique, a term grossly misused in this context), and so are archaeological sites: so what? It is from these specific pieces of data that history is written. History, as I noted in discussing South's paper and elsewhere, is the interpretation of whole series of interrelated events, their causes, and their effects (Walker 1972a: 145; cf 1970a: 64*; 1968a: 27). The modern

*In the 1970a: 64 reference - middle paragraph - a proof-reading error on my part prevented the removal of the phrase "- they do not...some lofty summit." which, as the astute reader will have observed, contradicts both the general tenor of that paragraph and the second paragraph on the following page.
American historian Wish agrees, commenting that nineteenth century "scientific" approach to history was fruitless because:

Literally taken, the idea that history consisted of wholly unique facts made even history itself impossible, for at the core of historiography was the idea of change and development — a process assuming some continuity, direction, and meaning. Aristotle had long ago exposed the fallacy of uniqueness by demanding a context of classification to make each fact meaningful. Uniqueness was a half-truth useful for the unimaginative (Wish 1960: 160-61).

I suggest historians will be as surprised as scientists — which latter point will be discussed later — to find Hempel suggesting that it is still "a rather widely held opinion that history, in contradistinction to the so-called physical sciences, is concerned with the description of particular events in the past rather than with the search for general laws which might govern these events" (1965b: 231). Historians most certainly look for useful generalizations to further their research, though like modern scientists they no longer look for "laws" in the nineteenth positivist sense of that term. Almost half a century ago, Trevelyan, one of the great historians of his time, pointed out that historians do not just collect facts — they think about them (Trevelyan 1927: 26). One of the leading historians of the present day, Christopher Hill, talking of the complexities of the English Civil War, said:

One easy refuge is to say that it is all so complex that no interpretation at all is possible. The historian can only record the multifarious things that happened, but must not attempt to make sense of them. I believe this is to abdicate the historian's function (Hill 1958: 38, 1968 ed.).

and elsewhere he commences a standard textbook in the seventeenth century:

History is not a narrative of events. The historian's difficult task is to explain what happened (Hill 1961: 13, 1969 ed.).

Even Binford admits this, preferring to concentrate (Binford 1972b: 117, 118, 119) on what he conceives (with the aid of some sadly outmoded ideas), as to how historical evidence is examined, quoted from Hempel (1965b) and Dray (1957) (neither of whom is an historian), and from an article by Joynt and Rescher (1961), (which flogs a dead issue in terms of an outmoded nineteenth century conception of science and some hangovers from an equally-outdated concept of history), to be the inferior methodology of historians. The idea that history is only collecting unique facts is utter rubbish, and either a red herring used by some anthropologists to
extol their own field as some superior ultimate plateau of knowledge or the reflection of an outmoded and inadequate training in history.*

Collingwood saw the distinction between history and data-collecting thus:

We preserve these relics, hoping that in the future they may become what now they are not, namely historical evidence.... This task of keeping relics against the time when they become material for history is the task of pure scholars, archivists, and antiquaries. Just as the antiquary keeps implements and pots in his museum without necessarily constructing history from them, and as the archivist in the same way keeps public documents, so the pure scholar edits and emends and reprints texts of, for example, ancient philosophy without necessarily understanding the philosophical ideas they express, and therefore without being able to reconstruct the history of philosophy.

This work of scholarship is often taken for history itself; and as so taken becomes a special type of pseudo-history, which Croce calls philological history. As thus misconceived, history consists of transcribing, translating, and compiling. Such work is useful, but it is not history; there is no criticism, no interpretation, no reliving of past experience in one's own mind. It is mere learning or scholarship (Collingwood 1946: 203-04, 1961 ed.).

Elton writing in the New Cambridge Modern History puts it more tersely:

The attraction of the sweeping and enlightening generalisation, however dangerous, is legitimate; what distinguishes the historian from the collector of historic facts is generalisation - preferably successful generalisation (Elton 1958: 20)

and Carr is terser still: "It is nonsense to say that generalization is foreign to history; history thrives on generalizations" (Carr 1961: 64,

*There is an interesting discussion of historical evidence in Winks's "Introduction" to the collection of extracts entitled The Historian as Detective: Essays on Evidence (1969), a book well worth reading, though its title will doubtless confirm in Binford's mind that historians, like Braidwood, believe their field to be "like a detective story, full of mystery and romance" (Binford 1972a: 11). An excellent example of the complexities of historical evidence, though unfortunately the footnotes and references have been omitted in the extracting, is The Origins of the English Civil War: Conspiracy, Crusade, or Class Conflict? (1960) ed. P.A.M. Taylor, 16 extracts from 13 writers on the English Civil War covering seventeenth, nineteenth and twentieth century views.
1964 ed.). Depending on one's definitions and inclinations, indeed, one could extol history as the superior field and relegate anthropology to the status of data collecting: some years ago I cited (against Willey and Phillips's dictum that American archaeology is anthropology or it is nothing) the view that anthropology has to become history or become nothing (quoted in Walker 1968a: 25), and Bibby in an entirely offhand remark observes that "so long as changes cannot be detected within a period as long as that intervening between the Pilgrim Fathers and our own day, then we are not writing history but only anthropology" (Bibby 1961: 274).

Binford's own view of history appears to be curiously superficial. He remarks (1972b: 119) that Dollar and I take a stand championed by what he calls the German School of the second half of last century. The term "German School" is bad, for there was a whole variety of German thought last century on history: what Binford is referring to is what is usually called the Geschichtsphilosophie school, which dates in fact to the last decade of the nineteenth century, and as Binford notes evolved the concepts of nomothetic science (science in the common meaning of the word, which had as its purpose the formulation of general laws and was the theory of knowledge) and idiographic science (history, which was the description of individual facts and was the theory of value). This philosophy, first put forward in 1894 by Windelbrand, was systemized two years later by Rickert, who saw two distinctions between science and history where Windelbrand had suggested only one. These distinctions, Rickert maintained, were between generalizing and individualizing thought and between valuing and non-valuing thought. From this, he produced four types of science: non-valuing and generalizing (pure natural science), non-valuing and individualizing (quasi-historical sciences of nature such as geology), valuing and generalizing (quasi-scientific sciences of history such as sociology), and valuing and individualizing (history proper). Obviously as Windelbrand defined history as the theory of value (as opposed to science, which was the theory of knowledge), the logical conclusion would be that history is not knowledge (Collingwood 1946: 165-70, 1961 ed.).

I cannot speak for Dollar, but I feel mildly surprised at being assigned to this sort of "general stance" - only mildly, however, for it is clear that in spite of some citing from works on the philosophy of history, Binford knows little history and therefore cannot analyze historical thoughts. So far as historical thought in Britain is concerned, these German philosophic historians were, as Carr has noted (1961: 20, 1964 ed.), ignored. They may, however, have had some influence in North America - Binford asserts the idiographic approach has "recently" been reconsidered by historians (Binford 1972b: 119), by which he possibly means American historians - for the "individualizing" concept has obvious affinities to the "uniqueness" concepts anthropologists have of history while the "theory of value" concept of history closely corresponds to Cleland and Fitting's "low level" concept of history (Cleland and Fitting 1968: passim); and certainly the Cleland and Fitting chart
showing history as temporal particularizing and anthropology as temporal generalizing (Cleland and Fitting 1968: 132) has close connections with Rickert's concept. Where this leaves Binford is somewhat unclear, unless possibly hoist with his own petard, for he and Cleland, both New Archaeologists, fit very much better into the philosophy they disparage than do I.*

The only reason I can see Binford might assign those disagreeing with him to the Geschichtsphilosophie school is that this school arose in opposition to, though deriving from and coloured by, the German positivist school which some years ago I suggested formed the basis for American anthropological thought and with which I specifically contrasted my views (Walker 1968a: 25). This school with its pseudo-scientific "objective" approach wie es eigentlich gewesen - simply to show how it really was - and its desire to frame laws was only the New History** of its day, complete with pseudo-scientific terminology such as Kulturwissenschaft (culture-science) for Geschichte (history) and like the New Archaeology "designed...to save historians from the tiresome obligation to think for themselves" (Carr 1961: 9, 1964 ed.). Historians went through their New Archaeology phase three generations ago, and in finally rejecting it, moved to a far more realistic level of history and interpretation.

Where the Geschichtsphilosophie school erred was in starting with the positivist principle that natural science was the only true form of knowledge - just as the New Archaeologist believes that unless archaeology can be made a "science" it is meaningless - when in fact such an assumption is nonsense. Collingwood attacked this assumption by asserting "facts" meant quite different things to a scientist from what they meant to an historian: "In science...facts are empirical facts, facts perceived as they occur", whereas in history a fact was "arrived at infer-

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*I do not believe it is by any means so certain that even American historians have only "recently" begun to consider the limitations of idiographic history, whatever Binford may take Joynt and Rescher's article to imply. As early as 1910 Carl Becker, a very eminent American historian, rejected Rickert's philosophy (Becker 1910: 531-32); and I do not find evidence of Geschichtsphilosophie outlook in the works of any of the American historians whose work is discussed by Wish (1960). The concepts of history suggested by the definitions advanced by Binford and by Cleland and Fitting are hopelessly inadequate for a field whose complexity has to be tackled in books, not in glib one-sentence definitions. History cannot be adequately defined in the abstract, and when it is examined in the flesh it becomes clear its writers have an infinite variety and gradation of views in practice. Wish's survey of American historiography presents a very readable account of this complexity.

**The term New History is used here simply as a parallel to Binford's term New Archaeology; it does not refer to the American New History of Robinson, Beard, Becker, and others.
entially by a process of interpreting data according to a complicated set of rules and assumptions" (Collingwood 1946: 133, 1961 ed.); but one might argue from more recent philosophic thoughts on the meaning of science (e.g. Beveridge 1950; 1961 ed.) and in particular from a famous Cambridge physicist's definition of a scientific truth as "a statement which has been publicly accepted by the experts" (J. Ziman, quoted in Carr 1961: 61, 1964 ed.), that scientists have come closer to the philo­sophy of post-positivist historians. As Carr puts it: "The historian has some excuse for feeling himself more at home in the world of science today than he could have a hundred years ago", for physicists now study events, not facts, and scientific laws in the eighteenth and nineteenth century sense of these terms are an outmoded concept (Carr 1961: 58-59, 1964 ed. and chapter 3 passim).

Binford in a summary statement made in 1968 about the aims of the New Archaeology states:

We seek to replace these inadequate propositions [the old principles of interpretation, allegedly mainly intuition and inference] by laws that are validated in the context of the epistemology of science, so that we may gain an accurate knowledge of the past (Binford 1972a: 121).

Such a positivistic view of science, far from being new, is as old and as outmoded as that of positivistic history: no less a figure than Max Planck argued strongly against it (Planck 1932: Chapter II, cf pp. 34-35). The physicist J. Robert Oppenheimer wrote "The ineluctable element of chance introduced into twentieth century physics heralds the end of the Newtonian paradigm of certain predictions of the future from the knowledge of the present" (quoted in Yankelovich and Barrett 1970: 208). As Beveridge points out (1950: 62, 1961 ed.), the teleological view that scientists should wonder "how" and not "why" is rejected by present-day science. Cohen and Nagel, two American philosophers on scientific method, noted that scientists "obtain evidence for principles by appealing to empirical material, to what is alleged to be 'fact'; and we select, analyse, and interpret empirical material on the basis of principles" (Cohen and Nagel 1934: 396): in other words, discoveries and new knowledge come not from establishing precise and universal laws but by forming hypotheses from available evidence which open the way to further inquiries and fresh discoveries. Binford's view represents the later Victorian optimism of Darwin and Huxley, or as a theological historian has put it, the "faith in science as a redemptive instrument guaranteeing progress in human affairs" (Farmer 1964: 181). By a curious irony Binford's definition of the "New" Archaeology now sounds like the conservative and reactionary elements of current politics and society:

The marked tendency has been to consign whatever is not fully and articulateley available in the waking consciousness for empirical or mathematical manipula-
tion to a purely negative catch-all category (in effect, the cultural garbage can) called the "unconscious" or the "irrational" or the "mystical" or the "purely subjective" (T. Roszak, The Making of a Counter-Culture, quoted in Smith 1972: 286).

Yet this philosophy has clearly been followed by exponents of the New Archaeology, including those in historical archaeology: Cleland and Fitting offer (1968: 130-31) a concise positivist statement about their biggest problem, the "mystique of expertise":

Thus specialists can distinguish German from Dutch earthenware on the basis of "experience" or "feel" but are reluctant to set forth specific criteria. We expect these criteria are either nonexistent or are undefinable, untestable, and therefore indefensible (italics mine).

This appears to be the archaeological equivalent of the McNamara fallacy, explicated thus by Daniel Yankelovitch, who as head of a social research firm may know something about quantification:

The first step is to measure whatever can be easily measured. This is okay so far as it goes. The second step is to disregard that which can't be measured or give it an arbitrary quantitative value. This is artificial and misleading. The third step is to presume that what can't be measured easily really isn't very important. This is blindness. The fourth step is to say that what can't be easily measured really doesn't exist. This is suicide (quoted in Smith loc. cit.).

In 1967 Binford, discussing his ideas on the role of analogy, declared (1972a: 48-49) that his procedure was:

...appropriate in the context of a positivistic philosophy of anthropology and archaeology. It denies categorically the assertion of antipositivists that the final judgment of archaeological reconstruction must be based on an appraisal of the professional competence of the archaeologist ([R. H.] Thompson 1956: 311). The final judgment of the archaeological reconstruction presented here must rest with the testing through subsidiary hypotheses drawn deductively.

This is straight Hempel (the "positivistic philosophy" Binford refers to is neo-positivism, not the Comteian positivism of last century in which as noted above North American anthropologists still misguidedly
think historians are enmeshed): but Hempel also observes (1966: 15) that "The transition from data to theory requires creative imagination" - which certainly includes intuition. Further, Hempel talks of the transition from data to theory, and notes the importance of an "antecedent knowledge of specific facts" (1965a: 5) and a "thorough familiarity with current knowledge in the field" (1966: 15). Therefore when he maintains (1965a: 6) that "What determines the soundness of a hypothesis is not the way it is arrived at (it may even have been suggested by a dream or a hallucination), but the way it stands up when tested" one can assume that he did not mean the dream or hallucination part to be taken as literally as Binford appears to have done with for example his general propositions regarding pipes, that he expected the person propounding the hypothesis had a knowledge of the subject-matter, and that he supposed those testing the hypothesis deductively would be able to correctly judge the results of the tests.

In fact, both deductive and inductive reasoning are used by scientists but neither is sufficient, either alone or in partnership; Binford's conceptions appear to be based on a lack of appreciation of the complexities and limitations of inductive and deductive logic examined for example by Salmon (1973), though a year later Binford is admitting (1972a: 118) that "we must continually work back and forth...between the contexts of proposition formulation (induction) and proposition testing (deduction)" and a year later again was saying that "scientific method proceeds in the context of complementary inductive-deductive methods" (1972a: 112-113), so that he now appears to unwittingly agree with Cohen and Nagel's description of scientific method noted above and with E.H. Carr's definition of history (1961: 30, 1961 ed.) as a continuous process of interaction between the historian and his facts.

Deductive reasoning can lead to no major advances in science, for no new generalizations can come from applying a general principle to particular instances. Inductive reasoning, on the other hand, while more productive because from it one can derive new theories, is less trustworthy because one can often derive several theories by it, only some or even none of which may be true (Beveridge 1950: 84-85, 1961 ed.). Further, as regards deductive logic, there is the danger of falling into the "fallacy of affirming the consequent" (Salmon 1973: 77) - that is, of arguing backwards from the truth of the conclusion to that of the premise - a fallacy which Binford on the evidence of his statement that "The final judgment of the archaeological reconstruction...must rest with testing through subsidiary hypotheses drawn deductively" appears in danger of committing. The so-called "scientific method" which would have us believe that reason is the main or even the only means by which science advances was the conception of certain logicians of last century who had little real understanding of research: "taste...and the important roles of chance and intuition" form the basis for most scientific breakthroughs, and "Only the technicalities of research are 'scientific'" (Beveridge 1950: 137, 138, 1961 ed.). The anthropolo-
gist Levi-Strauss is essentially correct when he says that:

The principle underlying a classification can never be postulated in advance. It can only be discovered a posteriori...by experience (quoted in Yankelovich and Barrett 1970: 402).

Where Binford and other North American anthropologists err is that they have ideas on history and science which are half a century out-of-date.

Collingwood (1946: 126, 1961 ed.) defines positivism as "philosophy acting in the service of natural science". As indicated above, historical positivists conceived natural science as comprising two parts: first, ascertaining facts and secondly, forming laws. The former were ascertainable through perception by the senses, the latter were framed through generalizing from the former by induction. Sociology was systemized by Comte as a sort of super-history to discover in true positivistic style the causal connections among these facts, the facts being supplied to the sociologist by the historian, and it is presumably from this that the idea originates (still widely believed among American anthropologists) that history is only raw data and specific things to be transmuted by the superior science of anthropology. The view of White in his paper "History, Evolutionism, and Functionalism" cited earlier (p. ) certainly fits into this pattern in every way, and in his account of the history of thought on the two or three types of interpretation of culture he takes this definition well back into the nineteenth century and cites E. B. Tyler as endeavouring "to trace a chronological sequence of unique events, to reconstruct history" (White 1945: 224). Tyler, however, was writing in the heyday of positivism, from which these ideas clearly come. Even more revealing is a passage from White's The Pattern of Culture quoted by Binford (1972a: 105) where White quotes the Second Law of Thermodynamics - that matter is becoming less organized and energy more uniformly diffuse - and contrasts this to "living material systems" where:

...the direction of the cosmic process is reversed... Biological evolution is simply an expression of the thermodynamic process that moves in the direction opposite to that specified for the cosmos as a whole.

This use of scientific laws is pure positivism; indeed Wish notes the use by the American positivist historian Henry Adams (1838-1918) of just this particular law (Wish 1960: 175-77). As Binford's philosophy owes a great deal to White (Binford 1972a: 341, 7-8), though unfortunately his writing style does not, it seems clear this is where Binford derives much of his determination to find laws. Winks (1969: 487-88) rather cynically describes the difference between the historical and sociological approach thus:

Recently a graduate student in sociology defended to me a project I regarded as worthless - since he
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proposed to prove something historians had proved long ago - on the ground that while historians had to justify their existence by producing new data, sociologists justified theirs by testing their methodologies. This is, one hopes, not the whole of the truth, but it is a little part of it, for many social scientists seem to enjoy disputatious inquiries into their own methodologies without ever getting around to the delicate task of applying these methods to anything that ten sane men would regard as important. They enjoy packing their bags for trips they have no intention of taking, as one of their number has remarked....

A rather similar situation prevails in present-day psychology and psychoanalysis, two equally positivist-derived fields, according to Yankelovich and Barrett (1970: 6-8 and passim); as they put it (p. 217), "The ghost of Newton, having suffered at the hands of modern physicists, has taken full revenge within the sciences of man."

Where historical positivism fell apart was in searching for facts, for it turned out there was no end to facts, so that in an endless quest for objective data it made historians encyclopedia-compilers, degenerated into sheer antiquarianism, and finally sank in a sea of facts. Lord Acton, writing about his teacher Dollinger, said on one occasion that "it was given to him to form his philosophy of history on the largest induction ever available to man" and on another that Dollinger "would not write with imperfect materials, and to him the materials were always imperfect" (quoted in Carr 1961: 15 and n.1, 1964 ed.). One practitioner went so far as to define the approach of the scientific historian thus: "If a certain philosophy emerges from this scientific history it must be permitted to emerge naturally, of own accord, all but independently of the will of the historian" (Fustel de Coulanges, quoted in Becker 1932: 232-33).

"What had gone wrong", as Carr (1961: 15-16, 1964 ed.) says, "was the belief in this unending and unending accumulation of hard facts as the foundation of history, the belief that facts speak for themselves and that we cannot have too many facts". Beveridge points to the same misconception in science, observing that "it is a common error among philosophers and writers of books on the scientific method to believe that discoveries are made by the systematic accumulation of data until the generalisation is a matter of plain logic, whereas in fact this is true in probably a minority of cases" (1950: 151, 1961 ed.). Objective history, as the American historian James Harvey Robinson used to say (Wish 1960: 269), had become history without an object. As Carl Becker put it, the philosophy of the scientific historian was:

that by not taking thought a cubit could be added to his stature.... surely the most romantic species
of realism yet invented, the oddest attempt ever
to get something for nothing! (Becker 1932: 233).

This is precisely what South (1972: 86, 102) and this writer (Walker 1972a: 139 and refs.) have castigated as an all-too-prevailing outlook in historical archaeology. Even Binford agrees that "facts do not speak for themselves" (Binford 1972b: 120), and here he is curiously
near the relativism of Becker who in 1932 noted that "Left to themselves facts do not speak; left to themselves they do not exist, not really, since for all practical purposes there is no fact until someone affirms it" (Becker 1932: 233) and who 22 years earlier (1910: 528) indicated that "The 'facts' of history do not exist for any historian until he creates them, and into every fact that he creates some part of his individual experience must enter". Like the positivist historians, far too many anthropologically-trained historical archaeologists have become bogged down in searching - usually by typologies, seriation, and statistical analyses - for objective "facts". The only difference seems to be that whereas the legacy of positivism was "a combination of unprecedented mastery over small-scale problems with unprecedented weakness in dealing with large-scale problems" (Collingwood 1946: 131-32, 1961 ed.) technicians in historical archaeology have so far not even been able to establish the least mastery of small-scale problems, such as artefacts (Walker 1972a: 139-44). The New Archaeologist, however, philosophizes as to how his data should be and how it should be found but without having mastered the essentials of interpreting the data from which he would indeed be able to draw inferences. He knows the price of everything and the value of nothing. History, to quote Carr again, "is a continuous process of interaction between the historian and his facts, an unending dialogue between the present and the past...the two processes of what economists call 'input' and 'output' go on simultaneously and are, in practice, parts of a single process" (Carr 1961: 30, 28, 1964 ed.). It is about time historical archaeologists started doing history.

Because of his nineteenth century beliefs in the scientificness of facts, Binford is obsessed with the superior quality of anthropological evidence because it can be quantified or otherwise scientifically and objectively studied. In particular he worries lest I and other historians* contaminate the pure science of archaeological research with

*As the Bellman said, what I tell you three times is true: I would have hoped that having already specifically indicated on two occasions, both in papers from which Binford quotes, that my formal training was in a field other than history (Walker 1968a: 23; 1970a: 63), people would remember this. Presumably I am an historian to Binford either because he uses the term intuitively - as an insult - or as an anthropological generalization - based on inadequately tested assumptions to fit general propositions which can serve as pivotal points for interpretive arguments treating the specific facts of a specific case. Possibly I have not insulted as many historians as anthropologists, but I have no starry-eyed beliefs about the nature of historical evidence particularly when it has been applied to the field of historical archaeology (cf Walker 1968a: 23; 1968b: 120; 1970b: 100; 1972a: 145).
such subjective approaches as "intuitive tests of plausibility, internal consistency, or critical evaluation of the accuracy of the facts cited" (Binford 1972b: 120). Apparently, according to Binford, I have no philosophical beliefs, so I cannot make deductions because deductions are made from propositions which specify relationships between things and events, and conclusions are warranted only to the degree that their relevance to such propositions can be established and the conclusions justified logically. Once again we have an anthropological red herring: apparently anyone whose philosophical beliefs differ from Binford's has no philosophical beliefs. This seems to be one of the more whimsical cornerstones of the New Archaeology, for Binford repeats it again with specific reference to me when belabouring Hanson for criticizing the methodology of his pipestem dating formula (Binford 1972c: 234) and Cleland and Fitting took precisely the same line a few years ago because I said much the same things about anthropology when commenting on Dollar's paper as I am saying now (Cleland and Fitting 1968: 126; cf Walker 1970a: 62). If I happen to think the philosophy of Binford, Cleland, et al is a collection of bankrupt ephemera I can see they will object, but to argue that I am a- or anti-theoretical because I disagree with their philosophy seems to indicate they have an impoverished philosophy in the first place.

At least I do not believe I have ever deliberately destroyed evidence because I was unable to comprehend it and so disbelieved in its possible usefulness (Binford 1972a: 130-31). Similarly, if indeed Leslie White did say that Boas was muddle-headed and that his writings were like the Bible, wherein one could find anything one liked (Binford 1972a: 7-8), I do not see the relevance of the implied corollary that nothing in Boas or the Bible is worthwhile. Bacon noted that one should "Read not to contradict and confute, nor to believe and take for granted...but to weigh and consider" (quoted in Beveridge 1951: 3, 1961 ed.). One should remember the dictum of Housman:

A scholar who means to build himself a monument must spend much of his life in acquiring knowledge which for its own sake is not worth having and in reading books which do not in themselves deserve to be read; at illa iacent multa et praeclara relicta (Carter [ed.] 1961: 159).

The limitedness of outlook arising from preoccupation with a "scientific" approach is illustrated by the opening two sentences in Deetz's Invitation to Archaeology and the extensive footnote appended thereto. Anyone who thinks there is only one real archaeology - anthropological archaeology - and that classical archaeology is only concerned with art of the Mediterranean world (Deetz 1967: 3 and n. 1) has such a limited idea of what archaeology is, to say nothing of what the nature of evidence is, that it is hardly worth trying to correct his outlook. Archaeology, as I have noted before, is a grossly inexact field of research, at least as much so as documentary research, a field with whose evidence historical archaeologists have equally to struggle (Walker 1970b: 106). Archaeology has indeed been said to be "at best, the
delicate balance of probabilities" (A. L. F. Rivet, quoted in Thomas 1973: 8). Noël Hume's "Creamware to Pearlware: A Williamsburg Perspective" (Noël Hume 1972) provides an excellent coverage of the conflicting vagaries of inadequate data which faces an historical archaeologist dealing with material represented in archaeological, documentary, and museum sources. Both history and archaeology rely a great deal on tests of plausibility and internal consistency and on critical evaluation of the accuracy of the fact cited, and the material which Binford and the New Archaeology so zealously quantify to prove the scientificness of their field is usually based on just such subjective and incomplete data.

As for the use of intuition, which so bothers Binford as being unscientific, problem-solving intuition plays a very important part in scientific discovery. Beveridge devotes a whole chapter to it (1950: chapter 6, 1961 ed.) and elsewhere (pp. 55, 57) quotes Planck and Einstein as stressing its importance. Newton, the inventor of scientific laws, is supposed to have said "No great discovery is ever made without a bold guess" (quoted in Beveridge 1950: 149). Even Binford would be hard put to prove Newton, Planck, and Einstein were unscientific.

The intuition of identification, which is perhaps to what Binford objects, is equally valid both in general living and in research. As Hercule Poirot put it:

what is often called an intuition is really an impression based on logical deduction and experience. When an expert feels there is something wrong about a picture or a piece of furniture or the signature on a cheque he is really basing that feeling on a host of small signs and details. He has no need to go into them minutely - his experience obviates that - the net result is the definite impression that something is wrong. But it is not a guess, it is an impression based on experience (Christie 1936: 171, 1941 ed.).

In a specifically archaeological context it has been concisely summed up by R. H. Thompson as "the combination of the investigator's anthropological background or training in fact and theory, his archaeological experience which is often called familiarity with the material, and his intellectual capacity" (R. H. Thompson 1956: 328).

When Binford worked in forestry and wildlife conservation I very much doubt that every time he referred to an oak or an elm - or even to a chinquapin oak or a wych elm - he felt obliged to prove his identification. His identifications must have been intuitive, based on his expert knowledge of trees: he abstracted from his experience, his "familiarity with the material". Yet when an archaeologist with similar expert knowledge of artefacts makes identifications, those who, as Housman once put it, read too little and attend too little to what they do read and
crown these defects "with an amazing and calamitous propensity for reckless assertion" (Carter [ed.] 1961: 91), rise up in protest, preferring not to be biased by any knowledge of the field (Cleland and Fitting 1968: 130-31). Cleland and Fitting admit (1968: 132) that "Higher level analysis can only be as good as the data produced [from excavation and artefact analysis] allows", but in practice they show little practical ability to handle the basic data in historical archaeology. There is no contradiction between Housman emending lines from Classical verse by combining a meticulous mastery of language and a first-hand knowledge of how poets express themselves to produce readings some of which were confirmed from manuscripts discovered only after his death (Wilson 1952: 76, 1962 ed.), and the theme in his 1911 Cambridge Inaugural Lecture that subjective impressions based on taste are a dangerous basis on which to emend an author's text particularly when the author wrote in a dead language and lived 2,000 years ago: textual criticism has to be based scientifically on observed or observable fact, and when science has done its best, art takes over, in the form of judgements within the limits prescribed by science in the preliminary analysis (Gow 1936: 34-35).

As Beveridge says above, "Only the technicalities of research are 'scientific'".

Plausibility, internal consistency, and critical evaluation of the accuracy of the facts cited are equally important to scientists. Beveridge quotes (1950: 103, 1961 ed.) a director of Medical Sciences for the Rockefeller Foundation as saying:

Most of the knowledge and much of the genius of the research worker lie behind his selection of what is worth observing. It is a crucial choice, often determining the success or failure of months of work, often differentiating the brilliant discoverer from the...plodder.

In South's paper, for example, claimed as noted above by Binford as "excellent" and "closely reasoned", intuitive decisions have been reached as to the validity of using mid-range dates, critical evaluations have been made in identifying the ceramics involved, and the internal consistency of Noël Hume’s work has been assumed.

Binford's rejection of such "unscientific" reasoning presumably goes back to his unhappy studenthood, when he found artefacts did not speak to him as they did to Griffin (Binford 1972a: 4-6). There are, however, possible reasons for Binford's failure to understand other than these forms of reasoning being wrong, one being that Griffin did not adequately explain on what he based his deductions from the artefact, another being that he did but Binford was unable to follow it. More subtle, but more probable, is the likelihood that some of Griffin's conclusions were right, some wrong, some had varying degrees of right or at least probability, and some were possible but unprovable: such complexity might well confuse someone who wanted to have a "scientific" approach which would give
definite yes and no answers. "Learning", as an Oxford don of last century put it, "is a peculiar compound of memory, imagination, scientific habit, [and] accurate observation" (quoted in Sparrow 1956: 7). If my particular approach is "empathic", and if empathy is "unscientific" (Binford 1972b: 118) - a term which Binford persistently equates to "inferior" - I remain unrepentant: "dead archaeology is the driest dust that blows" (Wheeler 1954: 13, 1956 ed.). Whatever model we use, it is one we have invented, and as Fowler puts it, talking of the English pre-Roman Iron Age:

...what worried Joe Celt was not whether he was conforming to the La Tène III norm or producing enough artefacts for statistical analysis but whether the spring would dry up this year, whether he could push his field further into the woods, or whether his feckless neighbour at Dindum was going to curb his pyromania these coming winter nights (Fowler 1969: 124).

We are studying people, and anything else is a waste of time.

Hill has remarked that:

Recorded history is like a photograph of an iceberg: it deals only with what is visible above the surface.... in commending the actions of men of the seventeenth century, as we should, in noting the very real constitutional, economic, and intellectual advances, let us also remember how much of the lives of how many men and women is utterly unknown to us (Hill 1961: 264, 266, 1969 ed.).

Hill has perhaps done more to illuminate the lives and thoughts of ordinary people in England in the seventeenth century than any other historian; archaeology, when it excavates the residences of those of various social classes or better yet when it tackles whole villages as part of a co-ordinated interdisciplinary study sheds illumination on the lives and sometimes even the thoughts of ordinary people - this is why Here Lies Virginia (Noël Hume 1963) is so successful as "an archaeologist's view of colonial life and history", to quote its subtitle.

The other use of historical archaeology is in economic history: distribution of artefacts leads to generalizations about trade patterns which are simply the opposite side of the coin seen in studies of trade records and port books by economic historians. Hudson's Bay Company records show that the East London clay pipe manufacturing firm of Ford of Stepney, known on other evidence to have been in business 1823-1909, held a monopoly to supply pipes to the Hudson's Bay Company from 1831 to at least 1870 (later Hudson's Bay Company records have not yet been studied): so far, all Ford of Stepney pipes found archaeologically in North America appear either to come from Hudson's Bay Company posts or
from sites, such as native sites, convincingly shown to be near or within the trading area of a Hudson's Bay Company post. None to my knowledge has so far occurred on any other type of site, from which we may generalize validly that in the light of present knowledge Ford of Stepney pipes if found in North America indicate Hudson's Bay Company trade. London trade directories list Ford as an exporter 1856-77 and 1880-1909: is this an inaccurate reference only to his Hudson's Bay Company contracts or did he export to a widespread market despite the apparent lack of his products in North America outside Hudson's Bay Company posts? The latter seems likely, for probable Ford pipes have been found in Australia and on Ascension Island (Walker MS). When one knows one's data there is no lack of valid generalizations even if tomorrow's data modifies or ultimately negates those generalizations and produces new ones.

It is perfectly possible for historians to be as rigorous and scientific in examining their evidence as it is for scientists with theirs - indeed one of the most rigorous examinations of historical evidence I know, and one which is far more scientific than anything Binford has ever written, is Oscar Cullmann's study of the apostle Peter (Cullmann 1952, 1960 ed.), a book which the author incidentally describes in the foreword to the first edition as ein Beitrag zur Geschichtswissenschaft - a contribution to the science of history. Another example from the same field is G. M. Styler's analysis of the priority of Mark among the gospels (Styler 1962) (not that either example is by any means the last word in its argument: Carl Becker noted that "In the history of history a myth is a once valid but now discarded version of the human story, as our now valid versions will in due course be relegated to the category of discarded myths" [Becker 1932: 231] and as Christopher Hill has said "all accepted truths, just because they are accepted, tend to become lies" [Hill 1965: ix, 1972 ed.]; and this is every whit as applicable to the hard sciences as to the traditional arts). If we write history to the greater glory of God and because we enjoy it (Winks [ed.] 1969: xxiii) who is to say this is wrong, and if we seek to learn from the past one can note the words of the American historian Allan Nevins, who said that every generation must rewrite its own history because each generation wishes to draw from the past meanings that will help bring some order into the chaos of the present (quoted in Winks op. cit. 273).

Certainly the fact that anthropologists apply mathematical techniques to their study does not entitle them to call their field a science: does the application of statistics to the book of Isaiah (Radday 1970) or to the Pauline epistles (Morton and McLeman 1966) make theology a science or the X-ray examination of the Ghent Altarpiece (Coremans [ed.] 1953) make art history a science? Or, to descend to the commonplace, does calling the general dimensions of an artefact "metric attributes" (Cleland 1971b: 86) add to our information? Radday is very careful to insist his examination of Isaiah results in no more than probabilities, and there are still reasoned objections to a multi-authorship of Isaiah. Indeed, Radday indicates his main aim was to objectivize the controversy "by strict quantification which does not allow any personal convictions,
religious prejudice, or literary taste to influence conclusions" (1970: 73); but even this overstates the case, for however strict the quantification it does not in itself remove the biases noted.

The assumption that "scientific proof" is obtained by this sort of approach is one of the more spectacular pieces of idiocy now in vogue among some archaeologists. Earlier, I quoted Binford saying the original use of his pipestem formula was "for testing the validity of Harrington's observations": the Binford formula does nothing of the kind, it merely expresses Harrington's observations differently. Again, as noted earlier, Binford hails South's "Evolution and Horizon . . ." paper as "A Step Toward the Development of Archaeological Science": at that point I observed that Binford was in no position to judge whether South's paper is scientific or not, but what is it in the paper that makes Binford maintain (intuitively, one supposes, since he offers no explication) it is scientific? Presumably the use of a mathematical formula makes it so, yet as L. H. Stone points out (1972: 180), the formula has to work given its straightforward statistical nature, provided Noël Hume's identifications and dating of historical ceramics is correct. In other words, it is a clear example of the limits of deductive reasoning.

Several years ago Cleland and Fitting indicated they felt Noël Hume's work to be "low level" (Cleland and Fitting 1968: passim); now Fitting retracts this opinion because "South has demonstrated beyond question the value of Noël Hume's work as a starting point for other types of analysis" (Fitting 1972: 158). Apparently South's formula is felt to prove the validity of Noël Hume's work, a patently absurd conclusion. Noël Hume's work, moreover (Noël Hume 1970), certainly involves tests of plausibility, internal consistency, critical evaluation of facts cited, intuition, and all the other techniques rejected by the New Archaeology: so where does this leave its practitioners? Once again, caught in their own devices, for they have committed the unscientific error of not checking their primary sources (which they surely would have pronounced unscientific and inadequate) before extolling the virtues of their secondary application. Further, they unwittingly underscore the limitations of deductive reasoning, the introduction of which Binford notes (1972a: 89-91) as a keystone of the New Archaeology.

What is particularly regrettable about Binford's lack of rigour is that others are falling into the same pit by blindly following his unsubstantiated claims. Thus Carrillo claims that Binford and South have taken "data previously compiled [by Harrington and Noël Hume] and have transformed it to construct testable mathematically controlled models structured within an general scientific paradigm" (1973: 1) and that his own statistical examination of bottles is "a mathematically testable model" because "the results obtained by dating [from Noël Hume's bottle typology] are visual and subjective, and cannot be tested for reliability" (1973: 4). As a matter of historical accuracy it was Harrington who applied a mathematical model to pipestem fragments, not Binford, who as noted above only re-expressed Harrington's data in a different form; and in fact of all the mathematical models noted here only Harrington's
has any independent validity, because Harrington used statistics correctly - to test a hypothesis by a method allowing comprehension of data in the mass. Pipestems, unlike ceramics and bottles, are not (unless, obviously, they carry makers' or other identifiable marks) generally datable: Harrington's process was to get samples from various differently datable contexts and examine them for significant change, and from this to extend the graphed changes to sites not readily datable; the South and Carrillo formulae, on the other hand, take dated material and simply express the date in another form.* They provide neither proof nor reliability, because they add no new evidence or independent data to the material being examined. Further, Carrillo's decisions as to what measurements he should use to determine evidence for chronological change, to say nothing of more subtle decisions as to precisely where on an object measurements should be taken for that particular dimension, are in themselves "visual and subjective, and cannot be tested for reliability". We are, if we follow the quest for "mathematically testable models" along these lines, in very great danger of ending up working completely in circles: the loss of innocence is one thing (Clarke 1973), but the loss of sense is another.

A problem nowhere tackled by Binford is why the existence of "laws that are validated in the context of the epistemology of science" (Binford 1972a: 121) are a legitimate assumption in the study of the past and therefore ultimately findable in the first place, and why archaeology should be forced to be a science. Are these intuitive assumptions? Hanson (1972: 255-6) comments here:

Finally, Binford and others have set out to make archeology a science. Archeology is no more a science than medicine. No amount of statistical manipulation or reams of historical documentation can alter the archeological record. In the end we must either interpret this record in the light of our manipulations and/or documentation or we

*It is true that one could argue that because Carrillo is using bottles bearing specific dates and extending his findings to bottles with no dates that he is working in a manner similar to Harrington; but this would be misleading, for it would be ridiculous to suppose each dated bottle represents that year's specific style (even if the date given is assumed to closely relate to manufacture date, something which might be queried). The most these single dates give is an approximation of date of manufacture (more specifically, a presumed terminus post quem) and thus an idea of when the shape was in use. This sort of evidence, together with that of material from datable stratified deposits, gives usable date ranges for various bottle-shapes. The resulting typology was published by Noël Hume a dozen years ago (Noël Hume 1961) and it is this rather than the later but much briefer discussion by Noël Hume in his A Guide to Artifacts of Colonial America (1970) used by Carrillo which should be used as a primary guide to dating bottles.
must use our manipulations and/or documentation to support our interpretation of the record. The difference between the "new" and the "old" archaeology is simply a matter of whether the end justifies the means or the means justifies the ends. The only room for improvement is the technology by which the archaeological record is gathered so that it can be better utilized no matter which course is chosen.

When Binford talks of steps towards an "archaeological science" he thinks in terms of his validated laws; when Seminar Press talk of "archaeological science" they are thinking in terms of support science, as their series "Studies in Archaeological Science" shows, comprising as it does the titles The Study of Animal Bones from Archaeological Sites; Methods of Physical Examination in Archaeology; Land Snails in Archaeology; and Ancient Skins, Parchments and Leathers. As M. S. Tite puts it in the second of these books, "in spite of the increased range of data made available, the archaeologist should still remain in full control of the final co-ordination and interpretation of this accumulated data and it is at this fundamental stage that the subject retains its humanistic aspects" (Tite 1972: 5).

It is when Binford misunderstands the nature of science and then extols his methodology as "superior" to others and tries to fit his particular field of interest to that "superior" methodology that the situation becomes ridiculous. G. K. Chesterton said that when he became a Roman Catholic it was like leaning up against a brick wall: this does not entitle one to presume the ways of Rome are "superior" to those of other religious groups, it only means that the ways of Rome were to Chesterton emotionally satisfying. The same feeling occurs among historians who have moved away from laws (in the Binfordian sense) they feel to be restrictive to their research: those who gave up Marxist beliefs noted that "when we lose the comfortable formulas that have hitherto been our guides amid the complexities of existence...we feel like drowning in an ocean of facts until we find a new foothold or learn to swim" (Werner Sombart, quoted in Carr 1961: 60, 1964 ed.). Binford and the New Archaeology apparently feel that validation in the context of the epistemology of science (albeit an outmoded epistemology) provide them with their brick wall and emotional satisfaction, but it does not entitle them to set up as latter-day Jesuits - there are, after all, many mansions.

Moreover, Binford and others appear to assume that "science" is a monolithic structure, but such is not the case. Mathematics and chemistry, for example, use different principles of reasoning. Collingwood noted that:

anything that is a science at all must be more than merely a science, it must be a science of some special
kind. A body of knowledge is never merely organized, it is always organized in some particular way.

Meteorology, he noted, was organized by collecting observations concerned with events of a certain kind which the scientist can watch happen though he cannot produce them at will; in chemistry scientists not only observe events as they happen but can make them happen under strictly controlled conditions; in other fields scientists do not observe events at all but make certain assumptions and proceed with the utmost exactitude to argue out their consequences (Collingwood 1946: 249, 1961 ed.). Each field of research is autonomous: "The way in which knowledge is related to the grounds upon which it is based is in fact not one and the same for all kinds of knowledge" (Collingwood 1946: 253, 1961 ed.). As the physicist Werner Heisenberg puts it, any truly distinct field of enquiry calls for its own basic postulates which can never be borrowed from another field (quoted in Yankelovich and Barrett 1970: 229). Archaeology is basically only a set of techniques used to produce evidence about man's past and thus add to his knowledge of history: it has nothing to do with "science" except that it can and does fruitfully use scientific techniques to augment its accumulation of data. Tricking out archaeology in the garments of science - and the cast-off nineteenth century garments of science at that - no more makes archaeology a science than did dressing up convert Gilbert's ape into Darwinian man.

Talking of the stock market one writer has noted that "Unlike chemical formulas, investment formulas, if they become widely accepted, tend to self destruct by distorting the very environment from which they are derived" (quoted in Smith 1972: 233): this is what Hill meant when he said all accepted truths, just because they are accepted, tend to become lies; and this was why Carr noted that historians emerging from the comforting "laws" of Marxist theory felt like drowning in a sea of facts until they readjusted their thinking.

Generalization, on the other hand, as we have seen, is a valid and fruitful means of furthering historical research - the difference between generalizations and "laws" in the study of the past is that the latter restrict development because they are in fact not laws: they are only generalizations artificially given the status of laws by those proposing them and they inhibit research because their originators imply that laws, being laws, can only be broken by fools, knaves, or charlatans and at the risk of scorn and censure by all established scholars. As Carr points out, nobody today would call Weber's thesis about the relationship of Protestantism and capitalism a "law" though it might have been so classified at an earlier period; and Marx's dictum that the hand-mill gives a society with a feudal lord and the steam-mill a society with an industrial capitalist is not a "law" either, even if Marx would probably have claimed it was: both generalizations, however, are fruitful hypotheses "pointing the way to further enquiry and fresh understanding" and "indispensable tools of thought" (Carr 1961: 60, 1964 ed.).
Even as hypotheses, however, Binford's "laws" fail to point the way to further enquiry and fresh understanding, for at least so far as historical archaeology is concerned Binford's valid general propositions can be seen to be invalid by anyone who knows his data, which latter knowledge as indicated earlier is one conspicuously lacking among most of those applying preconceived "laws" to historical archaeology. Here lack of historical understanding (or to be fair, a belief that historical understanding is unnecessary) is the problem. Historical data is far too complex to admit of the facile approach advocated by Binford: it is not a case of one piece of historical evidence being right and another wrong - it is a case of handling evidence from which one may be able to postulate several divergent interpretations. Verification is conducted on many planes, and its technique is not fixed (Barzun and Graff in Winks [ed.] 1960: 216). Psychoanalysts working away from the dead end of false categorization have asked the question "What experiences interact with what innate variables and universals at what time leading to what behaviors?" (John Benjamin quoted in Yankelevich and Barrett 1970: 405), an approach of which most modern historians would approve, echoing as it does Carr's definition of history cited above. Even Binford admits he had to learn data (1972a: 12, 132) - unfortunately he and others appear never to have tried to master historical data before entering the field of historical archaeology.

It might be instructive to find out what researchers in the hard sciences feel about the quest for "scientificness" in anthropology and archaeology. Certainly, hard scientists in Canada do not appear to consider anthropologists to be scientists, for in 1969 they let it be known that anthropologists would no longer be eligible for grants from the National Research Council of Canada, the federal government's scientific body, and suggested anthropologists address themselves to the Canada Council for the Arts, the federal government's body for the arts, instead.

One cannot entirely disagree with scientists who feel this way when one reads of misuse by archaeologists of scientific terms such as feedback. Flannery's use of the concept of positive feedback, even metaphorically, is only partially correct; and his summary of its application to the development of agriculture in Meso-America shows he is confusing feedback with chain-reaction:

Starting with what may have been (initially) accidental deviations in the system, a positive feedback network was established which eventually made maize cultivation the most profitable single subsistence activity in Mesoamerica. The more widespread maize cultivation, the more opportunities for favorable crosses and back-crosses; the more favorable genetic changes, the greater the yield; the greater the yield, the higher the population, and hence the more intensive cultivation.
There can be little doubt that pressures for more intensive cultivation were instrumental in perfecting early water-control systems, like well-irrigation and canal-irrigation [references]. This positive feedback system, therefore, was still increasing at the time of the Spanish Conquest (Flannery 1968: 80).

The initial steps, that of maize being cultivated on a more widespread basis and thus becoming more open to crossing and back-crossing, hence genetic improvements and from this greater yield, is – possibly – acceptable as being metaphorically speaking feedback; however, the effect on population, hence increased agriculture and more sophisticated agricultural techniques, is simple technological development, not feedback. In fact, Flannery's concept is not different from Childe's concept of the Neolithic Revolution in the Near East and Europe. With the misuse of the terms positive and negative feedback in that well-known science the advertising business – feedback to mean customer reaction, positive being favourable, negative unfavourable – and their consequent popularization it is hardly surprising everyone who is not a scientist misuses them: Binford for example uses it in a vague way to mean reaction (Binford 1972a: 63, 324-25).

Even within the soft sciences objections have been raised about archaeologists lifting terminology from these fields to bolster their theory: geographers, for example, have protested "the indiscriminate application of principles and techniques derived from studies in contemporary geography to historical situations" by writers such as Clarke (Baker, Hamshere, and Langton 1970: 21 and n. 37).

It is difficult to avoid the conclusion that the new jargon is, to misquote Milton, but old archaeology writ large (and in the case of Watson [1972: 210] spelled wrong). Despite Binford's denial of this, and his attempted justification of the New Archaeology on outmoded concepts of nineteenth century scientific philosophy quoted earlier, Jennings is essentially correct in being cynical about the newness of the New Archaeology (quoted in Binford 1972a: 120). As another writer has observed (Hogarth 1972: 301, 303), New Archaeology is merely Newspeak Archaeology – the operation of simultaneous location determinative, detectional and status-analytical programs relative to primary source data retrieval contributive to reconstructive syntheses of pre-current socio-cultural entities is still fieldwork – and new only to those unfamiliar with the old. The matalanguage of explanation too often takes us backwards, not forwards: commenting on the abuse of language in psychoanalysis and citing the sentence "There are two techniques of restoring a feeling of being loved (of increasing the libidinal cathexis of the self)" the analyst and philosopher H. J. Home observes: "The first part...seems to me perfectly comprehensible; the second part is, I believe, meaningless" (quoted in Yankelovich and Barrett 1970: 279). Researchers, even social scientists, have an obligation to make their arguments comprehensible, and to quote Professor Lehrer again, if a
person cannot communicate the very least he can do is to shut up (Lehrer 1965b). Among the origins of Newspeak Archaeology, Hogart suggests (1972: 302), is the system of education common in North America where children instead of receiving the immediate benefit of the knowledge of their elders are encouraged to "discover" things for themselves, which while a valuable exercise as one of a general range of educational techniques runs the risk when elevated to a "method" of allowing the child to believe he has discovered for all mankind.

This analysis would certainly agree with my comments (Walker 1972a: 145-46) on the recent "discovery" that literary sources, defined for the occasion as analogues, are useful for dating archaeological material from historical sites. It would also explain some of the more idiosyncratic differences adduced by Binford as separating the Old and New Archaeologies: in his "Smudge Pits and Hide Smoking: the Use of Analogy in Archaeological Reasoning" (in Binford 1972a: 33-51), for example, we are told in a singularly tedious and laboured fashion that analogies are not always correct just because they seem plausible and that most archaeologists until Binford have not been rigorous enough in considering their analogical evidence; elsewhere we are informed that large complicated sites are difficult to understand and that from this one may generalize that it is better to excavate little simple ones first - apparently the Old Archaeology judged the importance of sites on the amount of artefacts which could be discovered - (Binford 1972a: 130); most archaeologists until Binford were apparently committed to inductive reasoning only, hence their limited understanding until Binford introduced deductive reasoning (Binford 1972a: 48-49, 89-91, 111, 133); before Binford invented scientific archaeology all we had were appraisals of the professional competence of the archaeologist (pp. 48, 87, 117), ad hominem arguments, common sense, conjecture, hunch (p. 99), inference and intuition (pp. 120-121). The mind boggles at what new revolutionary and scientific discovery Binford will hurl next against the cracking bastions of traditionalist archaeology: is his assertion of "higher frequencies of Dutch pipes in the Northeast" discussed above an example of the new art of deductive reasoning? One is reminded of the comment on Freud, that what was new in his writings was of doubtful validity and what was valid was not new.

As for Binford's ideas about history, if he really does believe that history is specific things and events ordered in time, and that historians are naive, untheoretical, unphilosophical, and unepistemological, we can only shake our heads and reluctantly conclude that Braidwood was in 1964, and still is, correct (Binford 1972a: 11). If Binford is really interested in how historians work I suggest the following reading list, to be taken in the order given: E. H. Carr's What is History? (1961, 1964 ed.); C. Becker's "Detachment and the Writing of History" (1910) and "Everyman his own Historian" (1932); R. G. Collingwood's The Idea of History (1946, 1961 ed.); R. W. Wink's The Historian as Detective: Essays on Evidence (1969); R. Wish's The American Historian: A Social-Intellectual History of the Writing of the American Past (1960);

Until then, while it would be unrealistic to ask Binford to return to his first field of forestry and wildlife conservation he might at least have the good grace to stay out of historical archaeology - a field about which he has amply demonstrated he knows nothing - until such time as he has demonstrated some ability to comprehend historical evidence.

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A Debate on Ethnohistory

The exchange of ideas in the papers to follow would normally be classified as a typical FORUM exchange. However, the paper that simulated this exchange was not presented at the Conference, but was submitted as a contributed paper by Melburn D. Thurman. In order to provide a perspective for Thurman's comments I requested a review by James H. Howard, and from this the replies and rejoinders began to fly, leading to this most interesting debate on ethnohistory.

The debate was triggered by Clyde D. Dollar's comments in "Letter from Mexico" in Volume 3 of these papers, and the film "A Man Called Horse", for which Clyde Dollar was technical director. Clyde was not involved in this debate, but may well wish to comment in this CONTRIBUTED PAPERS section in a later volume.

This area of historical archaeology is one not often seen in the volumes of this series, and therefore this debate contrasting the traditional versus the systemic approach to ethnohistory is most welcome. This area is also emphasized in the JOHN M. GOGGIN AWARD PAPERS section of this volume, where the award winning paper by Brain, Toth, and Rodriguez-Buckingham centers around a problem in ethnohistoric archaeology.
THE RESURRENCE OF ANTIQUARIANISM IN ETHNOHISTORY AND ARCHEOLOGY:
CLYDE DOLLAR'S "LETTER FROM MEXICO."

Melburn D. Thurman

Clyde D. Dollar (1970) has claimed in this journal that the film
"A Man Called Horse," for which he was technical director, presents an
authentic view of the life of the Teton Dakotas in the 1820's. Dollar
wrote that "those producing and directing this film have so far spared
neither expense nor time in order to make it one of the most researched
films about the early Plains Indians, and hopefully therefore, one of
the most authentic dramas about such people." Dollar felt that the
economic (box-office) success of the film would have an impact "...on
certain aspects of the historical/anthropological professions in
general..." The nature of this supposed impact was not made clear,
but apparently Dollar meant only to argue that if an "authentic" Indian
film did well that the makers of succeeding films on Indians would also
strive for "authenticity." Dollar wrote that "if it [A Man Called Horse]
fails [at the box-office] ... there seems to be every reason to believe
that the normally seen 'Hollywood Indians' will be with us for some
time to come."

Dollar stated that he was faced with two problems: 1) differentiating
the authentic from the non-authentic and then filling in the "gray" areas
(apparently those areas lacking explicit data); 2) transmitting this infor-
mation to the various departmental heads to assure the "visual
reality" of the film. It is noteworthy that the only "gray area" dis-
cussed by Dollar concerned the re-creation of the color of the tipis.
Dollar wrote that "this was but one of the many 'gray' areas that re-
quired considerable research before sets, costumes, and props could be
presented on film." It would appear that Dollar's research was concerned
mostly with items of "material culture", and to a somewhat lesser degree
with customs. In dealing with a living culture, however, we are not
concerned merely with the identity of cultural traits, but rather with
how these traits are ordered. "A Man Called Horse" is a failure (in terms
of the achievement of "authenticity") because Dollar showed no concern
for the structure of Teton Dakota society in the 1820's. I will return
to this point after touching on the plot and certain other aspects of
the film.

Sometime in the 1820's (as the protagonist mentioned leaving Britain
after resigning his army commission in 1820) an Englishman, hunting in
the vicinity of the Black Hills with three hired guides, was captured
by a war party of Teton Dakotas. In the course of capturing the English-
man (and murdering his three companions), a Dakota warrior was killed.
The Dakota warriors had never seen a white man (as they repeatedly rub-
bed the Englishman's face after his capture), nor goods such as mirrors
which the Englishman possessed.

Upon reaching the village, Yellow Hand, the chief who led the war
party, gave away the items which had been taken on the warpath. In the meantime the English captive ran to the cemetery adjacent the village, where the Dakotas refused to tread, even to recapture him. Finally Yellow Hand managed to subdue the Englishman. Shortly thereafter Yellow Hand refused an offer of a horse and other goods for the hand of his sister. After the suitor had been rejected, the mother of the warrior killed when the Englishman was captured gave away all her possessions and cut up her tipi. Later scenes showed her scavenging for food. When the winter snows came, the homeless woman froze to death.

Shortly after his capture, the Englishman met an English speaking captive (a Cree as I recall), whom he persuaded to teach him Indian ways, that he might escape. The Englishman's test came with the return of good weather. A women's work party, which the Englishman ("Horse") accompanied, was approached by two Shoshone scouts. The Englishman killed the Shoshones, scalped them, and captured their horses. The Englishman was given a coup feather by Yellow Hand, but he wanted this chief's sister as a wife. Yellow Hand asked him to undergo the "sun vow". The Englishman was skewered, as in the Mandan Okipa, and afterward obtained Yellow Hand's sister for his wife, and thereupon put aside the idea of escape.

In the fall, a large Shoshone war party attacked the village of Yellow Hand. The Shoshones were driven from the village (thanks primarily to the Englishman directing volleys of arrows against them), but the Dakotas suffered heavy casualties, including Yellow Hand and his sister (the wife of "Horse"). The Dakota dead were buried in the cemetery adjacent the village. The time sequence is not absolutely clear at this point, but apparently the next spring the Englishman left the Dakotas to return to civilization. Spring is inferred as the goods placed with the dead from the Shoshone attack showed considerable age, and flowers were blooming.

The portrayal of a returning war party (except for the lack of cries of lamentation for the dead warrior), of mourning customs, and of the fate of the aged are approximately accurate (Denig 1930: 547-8, 571-2, 576-7; Lowie 1963: 91), but there are a number of lapses in authenticity in other customs and in portrayal of material culture. For example, one of the members of the war party which captured the Englishman possessed a Navaho blanket of a style not known from the 1820's (Amsden 1934: 205-37; Maxwell 1963). The existence of a Navaho blanket should have suggested a "gray area" to Dollar. If a Dakota could have a blanket made by the Navaho several hundred miles to the southwest, why wouldn't he have a gun from traders little more than one hundred miles to the east? Dollar admitted that "the existence of certain trade items...[must be] postulated [at this date]," but "our Sioux have not yet received guns...." Secoy's map of the diffusion of the horse and gun on the plains show that the gun frontier (spreading from the east) was well to the west of the Black Hills by 1790 (Secoy 1953: 104-6). Further, in light of the nature of the middle-Missouri River trade (Jablok 1950) it would be strange indeed
if a group of Dakotas in the 1820's had never seen a white man. Many other errors and misconceptions could be pointed to as in the misrepresentation of plains ceremonialism.

Even if one accepted Dollar's apparent definition of "authenticity"—the placement of cultural attributes to a particular area and time—the film "A Man Called Horse" could hardly be called authentic; there are simply too many errors of attribution. But more important, Dollar's view of authenticity is that of an antiquarian. By being overly concerned with objects or customs in themselves, rather than the contexts of these objects or customs, Dollar has contributed to the production of a film that greatly distorts the culture and life of the Teton Dakotas of the 1820's.

There were two kinds of cultural adaptation to the northern plains in the 1820's. The semi-sedentary Hidatsas and Mandans, who spent about half the year in earthlodge villages, who practiced horticulture, and who had what Eggan (1955) called a "lineage type" kinship system, represented one kind of adaptation. A second kind of adaptation was represented by the "nomadic" tribes, such as the Dakotas. The Dakotas lived in tipis throughout the year, had no permanent villages, did not practice horticulture, and had what Eggan referred to as a "generation type" kinship system. A systematic comparison would disclose a great many other differences in cultural content.

If the film "A Man Called Horse" is considered in terms of cultural context, the claim of "authenticity" is absurd. In the film the Dakota tipis were clustered about an earthlodge. Earthlodges were built on the northern plains, but by the semi-sedentary tribes. There is not one piece of evidence for earthlodges being built by the Dakotas west of the Missouri River. Interestingly, even by Dollar's definition, the earthlodge in the film is inauthentic. In the film the outer wall of the earthlodge was erroneously constructed in the manner of a frontier log house, with the logs lying parallel to the ground. There are good published data on the details of earthlodge construction. Two of the numerous plans published by Wilson would have been sufficient in themselves for an accurate reconstruction of an earthlodge (Wilson 1934: figures 15, 16).

The existence of an earthlodge implies at least a semi-sedentary existence. It is not surprising, therefore, that the Dakotas of the film led a semi-sedentary existence, an existence even more sedentary than the real Mandans or Hidatsas, who were themselves considerably more sedentary than the real life Dakotas. The village did not shift location during the entire film, the events of which supposedly unfold through a period of at least one and one-half years. The picture of village life is grossly misleading, and one would obtain from the film no more than a vague notion of subsistence patterns and the overwhelming importance of horses in Dakota society. The information concerning horses is often misleading, as the horse corrals which are visible during the
Shoshone attack. Ewers (1955: 328) pointed out that corrals were probably of European origin. A last observation about the film's erroneously implied sedentariness concerns Teton Dakota cemeteries. There is no question that occasionally the Dakotas placed the dead on scaffolds in their villages, as shown in a Bodmer drawing (Maximilian 1841, plate 11), but the evidence seems to indicate that Dakota cemeteries came into existence after contact with Europeans. Those Dakota cemeteries which have been reported invariably were in the vicinity of a fort. They were placed thus in the hope that the presence of Europeans or Americans would afford the burial scaffolds protection from the enemies of the Dakotas (Bushnell 1927: 29–49). The Mandans and Hidatsas, on the other hand, buried their dead on scaffolds outside their villages (Bushnell 1927: 65–78). A Mandan cemetery of this kind was shown in the background of one of Bodmer's drawings (Maximilian 1841, plate 25).

In general (except for some questionable Indian traits, such as arrow volleys) Dollar's errors in attribution were not errors in the placement of attributes in time and gross space, but rather stemmed from an ignorance of cultural process and what might be called "cultural space". By "cultural space" I mean the loci occupied by the people participating in a particular cultural symbol system.

Artifacts, linguistic symbols, and gestures are cultural symbols in terms of which social interaction takes place. Symbols do not have a random distribution in space; symbols of all three categories have definite spatial correlates. For example, at a Southern Baptist Church service social interaction takes place by means of artifacts; the minister stands at the pulpit (an artifact) and faces his flock seated on pews, chairs or benches (also artifacts). The minister and his congregation join together in song, reading the words from hymnals (also artifacts). The minister might hold his arms in front of him, palms upward, and raise his hands as a signal for the assemblage to rise. This gesture symbol has a spatial locus, it is not a gesture which the minister would often use outside church. The hymn sung by the congregation is a macro-linguistic symbol which also has a primary locus (church). The artifacts, gesture, and linguistic symbols in this case are systematically linked at a particular spatial locus. Anthropologists are not concerned with artifacts, gestures or languages in themselves, but rather with the nature of the social interaction with these symbols.

Since Dollar did not concern himself with the systematic interaction of people with cultural symbols, his film interpretation of the Dakotas in the 1820's is a distortion. Dollar's viewpoint is that of the antiquarian, concerned more with artifacts and behavioral attributes in themselves than with the interaction of people with symbols. Although Dollar's approach in his "ethnohistorical" investigations for the film led to absurdities, he has advocated the same kind of approach in historic sites archeology (Dollar 1968).
Some years ago Spaulding (1960) approached archeological data in
a way analogous to the viewpoint I have advocated here. Spaulding
referred to culturally produced "containers" of archeologically recovered
artifacts, as in the grave goods of a single burial which "...becomes
a descriptive and comparative unit, /when/...the spatial interrelation­
ships of the component artifacts are presented in a formal description
of the unit as a sort of superartifact." Although Spaulding did not
dwell on the point, "superartifacts" are useful descriptive and com­
parative units because they distinguish a systematic relationship of
behavioral patterns and symbols with the behavioral patterns inferred
from the spatial correlates of symbols (artifacts). In Kaplan's (1964)
terms, an archeologist determines, on the basis of the spatial correlates
and associations, if different occurrences of the same "sign" represents
the same "symbol" or different "symbols". Differences in association
in space often indicate differences in behavior, and hence imply that
a single sign might represent more than one cultural symbol.

In short the argument made here is that in anthropology (ethnography,
ethnohistory, and archeology) the proper descriptive and comparative
unit is composed of a symbol (or group of symbols) and the activities
carried out in relation to that symbol (or group of symbols). Such units
can be established from archeological data by inference from spatial
correlates, because of systematic relationships of non-tangible symbols
and behavior with concrete symbols (artifacts) which are archeologically
recoverable.

In light of what has been said here, it should be apparent that the
problem of film "authenticity" is a specific case of the general problems
of anthropological typology. The standard typological approach to
"material culture", concerned only with morphology, tends to lead those
workers employing it to the antiquarian path---the concern for artifacts
in themselves. Obviously some sort of preliminary categories established
on the basis of morphological attributes are a necessary preliminary to
anthropological inquiry. The error in the standard typological approach
is the assumption that the same sorts of behavioral relationships, neces­
sarily obtained in regard to all the artifacts of a particular morpho­
logically defined type. Such a procedure is in fact an assertion of
identity. The assumed relationships, rather than being accepted as given,
should be taken as hypotheses for further investigation.

Morphologically a Folsom fluted point is a Folsom fluted point, but
a Folsom fluted point, or any other artifact, does not have inherent
symbolic content. A fluted point embedded in a mammoth has different
cultural meaning than a fluted point involved in recent Huichol ceremon­
ialism (Weigand 1970). This is "obvious" in the case cited, but this
is precisely the point missed by Dollar. Formally the Dakota burial
scaffold was pretty much like the Mandan scaffold, but the difference
between single scaffolds and scaffold cemeteries involves differences
in symbolic content and cultural meaning.
We must move away from antiquarian involvement, from the assertion of identity of cultural content based on morphological identity. Dollar's work in ethnohistory and archeology represents a resurgence of the old antiquarian approach. He has nothing to offer to further our understanding of culture, cultural symbols, or cultural process. Dollar's film, while not an "authentic drama", has one thing in its favor—it is visually beautiful. If "A Man Called Horse" succeeds at the box-office it will be because of this beauty, not because of its "authenticity".
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COMMENTS ON "A MAN CALLED HORSE", ETC.

James H. Howard

After reading Mr. Thurman's paper "The Resurgence of Antiquarianism in Ethnohistory and Archeology: Clyde Dollar's 'Letter From Mexico'", I went to see the film "A Man Called Horse" for a second time. This time I attempted to concentrate on the points raised in both Mr. Dollar's and Mr. Thurman's papers on the movie (Dollar 1970; Thurman, this volume).

First, I would like to note that I enjoyed the film immensely, and that I found it so superior to the usual Hollywood depiction of American Indians that I am willing to forgive a great deal. My feelings regarding the film are mostly positive. Thus I found myself disagreeing with Thurman's statement (p. 2) that "Dollar showed no concern for the structure of Teton Dakota society in the 1820's". Had he said "inadequate" or even "insufficient" I might agree, but "no concern" is a bit strong. Certainly the customs of offering a horse and other goods for a bride and the staging of an itúhan or Give-away by the mother of a slain warrior mentioned at the bottom of the same page indicate that some attempt was made in this respect.

The second error in Thurman's paper is the identification of the "English speaking captive" as a Cree. The dialogue clearly identifies him as a mixblood Flathead, not a Cree. How could Thurman have missed that misshapen head?

The point he makes in regard to the Navaho blanket is correct. The Teton did not secure their first Navaho blankets until 1858-59 (Mallery 1893: 325). I am not so certain as to the "misrepresentation of Plains ceremonialism" cited by Thurman. Did the Teton at this time have the Sun dance or did they have a similar ceremony closer to the Mandan Okipa? The High Dog winter count states that the Dakota performed the Sun dance for the first time in 1820-21 (Howard 1960: 364). Prior to that time they may very well have had a ritual like the Mandan Okipa. The usual "model" of Teton culture, and the one apparently employed by Thurman, stems from a later period in their history (ca. 1860-1880). In 1820, however, the purported date of the action in the film, the Teton had been on the High Plains only a few years, and as Johnson (1965: 351) has pointed out "The Teton moved onto the Plains as an Eastern Prairie tribe, not as a Woodland hunting people as is often thought. The area in which they lived ... is a prairie zone adjacent to the mixed grassland-deciduous forest area of central Minnesota. Here they probably practiced a mixed horticultural-hunting economy as did the other Dakota tribes." In other words, we would expect, as Dollar has correctly shown, to find a strong "Prairie" or better "Missouri valley" influence in their culture at this time. Only later did they become the nomadic bison hunters of popular lore.

Thurman's point about "two kinds of cultural adaptation ... semi-
"sedentary and nomadic" stems from his late nineteenth-century model of Plains culture, likewise the "pigeon-holing" tendency in American ethno­logy, long conditioned to thinking in terms of rigid culture areas. Actually the two adaptations were not as distinctive as painted by latter day anthropologists. The Teton did occasionally plant corn and other crops (Howard 1960: 365). They definitely did build earthlodges and live in earthlodge villages for long periods of time, and recorded this fact in their winter counts or calendrical histories for the years 1815-16 and 1817-18 (Cf. Mallery 1886: 109, 136; Curtis 1908: 172; Howard 1960: 364). Thurman, not Dollar, has failed to do his homework in this instance. Thurman is correct in pointing out that the earthlodge in the film is erroneously constructed. What is pictured is an Omaha wacipi dance hall of the 1880's and 1890's. These were copied after the earlier earthlodges but employed log cribwork for the vertical walls.

Thurman's point on the inaccuracy of horse corrals is well taken. Horses were herded near the village by boys, not corraled. At night the best buffalo runners and war ponies were picketed near the owners' tipis.

Thurman's best points are in the last section of his paper, i.e., his remarks concerning social interaction. The huge crowds which appear in the film whenever "Horse" does this or that are most unconvincing. Even more absurd is the notion that an alien White man, speaking through a low status interpreter, and in a time of extreme crisis (the attack on the village) could marshal untrained Dakota warriors and have them stand in ranks to fire volleys of arrows at enemies already inside the village! I was quite impressed with the use of volleys of fire to repel an overwhelming enemy force when this was done by British troops in the motion picture "Zulu", as I recognized this as a standard military tactic of the British army. When the same tactic was attributed to individualistic Teton warriors I was reduced to hysterical laughter. I would thus agree with Thurman's comment, in regard to the "social interaction" in the film that "The error in the standard typological approach is the assumption that the same sorts of behavioral relationships necessarily obtained in regard to all the artifacts of a particular morphologically defined type. Such a procedure is in fact an assertion of identity. The assumed relationships, rather than being accepted as given, should be taken as hypotheses for further investigation."

This completes my commentary on Thurman's paper. Should anyone be interested, here are a few other errors which I noted in the film: (1) Richard Harris is shown wearing a brown leather headband following his "Indianization". Headbands were not used by Teton men or women until ca. 1920, as a quick check of old photographs will show. (2) Harris' buckskin shirt, though purportedly of Indian manufacture, is laced together with leather thongs. This is a "White man" trait. Indian clothing was carefully sewed together with sinew thread. (3) The Indian dancing shown in the film was unlike any Indian dancing I have ever seen. It seemed to be aimless, mad, capering. (4) The flute shown was not a Teton style flute. (5) The ridiculous unison singing of Inkpata (a Dakota love song).
as a sort of charivari or wedding serenade for "Horse" and his bride has no basis in any ethnographic source known to me. (6) The endless repetitions of words and phrases in Lakota by the "Indians" seemed most uncharacteristic. In fact a Dakota friend of mine who saw the movie was highly incensed at this behavior. (7) The notion that the Teton of this period would name a man "Sunkawakan" or "Horse" is absurd. He would have been termed Šaglása (Englishman) or Tóka (Enemy), never "Horse". The notion of a horse as a beast of burden, something less than a man, is a European, not an Indian idea. (8) The horsemen in the movie mounted their steeds from the left hand side. Teton Dakota would have mounted from the right.

On the positive side I enjoyed the attempt at accurate costuming. There were no "standard" type flaring warbonnets and no roach headdresses, both of which were probably unknown to the Teton of this period. I also enjoyed the attempt to use Dakota music as background music, and the fact that Lakota was spoken throughout (though sometimes garbled by non-Indian actors). Coup counting, brid purchase, the Inípi or sweat lodge, and the Hamblichevá or Vision quest rite were all more or less accurately depicted. Finally, I would award "A Man Called Horse" an "E" for "Effort" in at least making an attempt to depict Indians as people, not as: (a) frenzied killers, savage brutes or (b) merely a part of the background, local color.
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Professor Howard's disagreement with my comments on the film "A Man Called Horse" exemplifies the type of work criticized in my paper (Thurman 1973). Howard shows himself to be little concerned with the theoretical and methodological issues which must be dealt with if we are to know the past on the Plains (or any other area). His relative lack of concern for these issues leads him into mutually contradictory positions. To answer Howard it is necessary to touch on aspects of his work as well as considering the specific charges of error leveled against me in his paper (Howard 1973).

According to Howard: "Thurman's point about 'two kinds of cultural adaptation...semi-sedentary and nomadic' stems from his late nineteenth century model of Plains culture, likewise the 'pigeon-holing' tendency in American ethnology, long conditioned to thinking in terms of rigid culture areas. Actually the two adaptations were not as distinctive as painted by latter day anthropologists. The Teton did occasionally plant corn and other crops...They definitely did build earthlodges and live in earthlodge villages for long periods of time, and recorded this fact in their winter counts or calendrical histories for the years 1815-16 and 1817-18...Thurman, not Dollar, has failed to do his homework in this instance". Howard then attacked my statement that Dollar misrepresented Plains ceremonialism; he suggested that the Teton Dakotas may have had an Okipa-like ceremony before they developed the sun dance, which, according to Howard's interpretation of the High Dog winter count, was "...performed...for the first time in 1820-21".

In light of the history of the development of conceptions of Plains Indian culture it is strange that Howard would charge "error" both to my distinguishing two cultural adaptations on the Plains and in my "...thinking in terms of rigid culture areas". Eggan's (1952) lucid discussion of the varying conceptual bases used in Wissler's and Kroeber's treatment of the Plains culture area is a better reply to Howard's statement than I could hope to make. I will later deal in more detail with Howard's own conceptual bias; here it is only necessary to say that Howard apparently does not understand that most generalizations in the behavioral or social sciences are of a probabilistic form and that exceptions do not necessarily invalidate a generalization.

If we turn to specific "facts" the inadequacy of Howard's approach to the Plains is even more apparent. Howard seems to think that "facts" have inherent meaning. Even if one accepted this simplistic viewpoint, Howard's charges of error would be wrong. Howard has an uncanny ability in proving statements which I have never made to be wrong. It is more embarrassing when he agrees with statements I did not make. These comments will be clarified through examination of Howard's source analyses and his theoretical bias.
Originally I wrote that "there is not one piece of evidence for earthlodges being built by the Dakotas west of the Missouri River" (italics added). Howard apparently interpreted this to mean that I claim that the Dakotas never built earthlodges. His citation of High Dog's winter count for 1820-21 refers, in his interpretation, to a Dakota earthlodge on Peoria Bottoms. The last time I was in South Dakota, Peoria Bottoms was on the east side of the Missouri, not far from Pierre.

The Dakota winter counts, of which more than a score have been published, bear examination. Howard (1955: 14) pointed out that winter counts went through three stages of development: 1) pictographs on bison hides; 2) pictographs on cloth; 3) short texts, lacking pictographs, written in account books. In the first and second stages pictographs were mnemonic devices. There was one sign for each year, hence by counting backward from the last sign of known date, each sign could be attributed to a particular year. Three of the mnemonic Dakota winter counts are especially interesting, as the similarity of the great majority of their pictographs and their absolute coincidence in dates indicates a close relationship. Two of these three winter counts were by Teton Dakotas (The Flame, believed to have been of the Two Kettles division, but who lived with the San Arcs, and The Swan of the Minneconjou division); the third winter count was by Lone Dog, a Yanktonai Dakota.

In The Swan's winter count (as in the other two counts) there is a sign made up of parallel marks supporting a triangle. In 1823-24 there is no doubt that the sign (associated with a man in a hat firing a gun) refers to an earthlodge as the interpretation dealt with the attack of Americans and Dakotas on an Arikara village, an event for which independent verification exists (Correspondence 1824). Nevertheless, for 1838-39 the same sign (by itself) was reported as a Minneconjou medicine lodge, and in 1828-29 the sign (with a man in hat sitting under it) was reported as a trading post (Mallery 1886: 111-12, 1,14-15, 117; Plates 16, 18, 21). This suggests that by the time ethnologists set down the interpretation of the winter counts, their informants had only a vague understanding of the meaning of some of the mnemonic devices. The sign referred to obviously was a general sign used for any large structure.

The interpretation of vagueness in regard to the mnemonic devices is strengthened if one examines the varying interpretations of the sign for 1820-21. In the three winter counts the same sign (with none of the variations in associated signs observed in the cases above) was interpreted as indicating an earthlodge, a trader's post, and a Dakota medicine lodge. The Flame's count reported "large dirt lodge made by Two-arrow". Lone Dog's count gave this as "the trader, la Conte, gave Two-arrow a war-dress for his bravery". The Swan's count was reported thus: "a Minneconjou Dakota, named Two-arrows, built himself a dirt medicine-lodge. This the interpreter calls, rather inaccurately, a headquarters for dispensing medicines, charms, and nostrums to the different bands of Dakota" (Mallery 1886: 110-11, Plate 15).
The three winter counts thus far discussed drew from one another or from a common source. They do not represent independent sources. The problems multiply when independent winter counts are compared. In the winter count of American Horse, a Teton Dakota of the Oglala division, the winters of 1818-19 and 1819-20 were each marked with what is obviously intended to be a house with a door, window, and chimney. The commentaries, respectively, stated that "a large house was built", and that "another house was built [;] the Dakotas made medicine in it" (Mallery 1886: 136, Plate 41).

In light of the difficulties in interpreting winter counts it is not difficult to see how George E. Hyde (1957: 37-38), the man largely responsible for initiating ethnohistorical studies of the Teton Dakota, concluded that

...The count-keepers themselves were very hazy as to the meaning of many of the pictographs. Thus it is stated that in 1815-1816 the Oglalas built a house like a white man's house and lived in it all winter; in 1818-1819 they built another house, and in the following winter a third one. These entries in the counts of course refer to the building of trading-posts by the whites, near which posts the Oglalas wintered.

It is strange that in attacking my statement that there is no evidence for Teton Dakota earthlodges west of the Missouri, Howard did not refer to the winter counts for 1838-39, which mentioned a dirt medicine lodge "in the Black Hills", perhaps on the Moreau River (Mallery 1886: 117, Plate 21). By the winter of 1830-31 there appears to have been a trading post on the Moreau (Abel 1932: xxvi). It would seem likely that the reputed earthlodges, as Hyde suggested, were actually trading posts. There is little question that the Peoria Bottoms "earth-lodge" was near white traders and it would seem likely that the "earth-lodge" "in the Black Hills" or on the Moreau River was too.

Howard's simplistic assumption that the winter counts mean exactly what the interpreters said and that they can be used without contextual analysis has led him to other shaky conclusions. For example, Howard "agreed" with me about Navaho blankets. He wrote that "the Teton did not secure their first blankets until 1858-59 (Mallery 1893: 325)."

This reference (the Baptiste Goode winter count) says only that Navaho blankets were brought to the Tetons by traders, not that these were their first blankets. Yet in Lone Dog's winter count for 1853-54 there was a reference that "Spanish blankets were first brought to the country" (Mallery 1893: 283; Mallery 1886: 121). Since the Navaho were in the Spanish (then Mexican) country before 1846 (when the land was taken over by the United States), it would seem highly probable that "Spanish blankets" were Navaho blankets. Why did Howard overlook this reference? No doubt Howard was aware that the winter counts of American Horse and Cloud Shield which had blankets for the winter sign stated that in 1851-
52 the Teton received annuity goods for the first time. Perhaps Howard was concerned that there was too much chance that the winter counts of American Horse and Cloud Shield for 1851-52 referred to the same event as Lone Dog's 1853-54 count. Why then would Howard accept Baptiste Goode's account of the events of 1858-59 as truly independent of the counts variously recorded for 1851-52 and 1853-54? There is no explicit basis for the statements Howard gives; he cites a particular count if it "documents" the point he wants to make.

Howard's particularistic approach to ethnohistorical source analysis is precisely the kind of approach that must be superseded if we are to make progress in ethnohistory. Howard himself has made an extensive comparison of winter counts, but even though ostensibly he was concerned with "the relative accuracy of the various counts", he provided no rules by which judgements can be made. Howard simply listed references together by year, apparently assuming that a particular event could be dated by the year given a plurality in his tabulation of the winter counts (Howard 1960).

Howard claimed that the winter counts showed that Teton earthlodges were constructed in 1815-16 and 1817-18. I have shown that various counts refer to these events in 1813-14 and 1814-15, in 1815-16 and 1816-17, in 1816-17 and 1817-18, and in 1818-19 and 1819-20. My analysis is based on over a dozen winter counts. This represents almost half the winter counts which have been published, and about three-quarters of the winter counts published in virtually complete form. My analysis shows that the accuracy of a winter count can be assessed in terms of which I call a "prime sequence" of events. For example, between 1800 and 1825, fifteen events provide the "prime sequence". All these events are not given on all winter counts, but when two or more events from this "prime sequence" are reported in any winter count, these events are the same number of years apart. In the few cases where there are discrepancies in "main sequence" events, the nature of the chronological deficiencies can be specified (Thurman MS). By considering the few events of known date a reliable chronology can be drawn up. In the case of the "earthlodges" (or medicine lodges or trading posts) referred to by Howard, these two "events" are on my "main sequence" and can be dated to 1815-16 and 1816-17, not to 1815-16 and 1817-18 as Howard suggest in his criticism. It is interesting to note that earlier Howard (1960: 264) accepted the 1815-16 and 1816-17 dating.

As I wrote in my original paper "the existence of an earthlodge implies at least a semi-sedentary existence", but the existence of planting does not necessarily imply semi-sedentariness. For example, the Cheyennes made plantings long after the period in which they were supposed to have lived in earthlodges. Grinnell (1915: 1) stated that the Cheyennes "as recently as 1850...tilled the soil to some degree, and men have described to me their mother's corn patches on the Little Missouri at that date". More directly to the point, in the late pre-reservation period the members of the Crow Tobacco Society planted
tobacco, but no one could realistically speak of their horticultural adaptation (Lowie 1935). Further one could point to the Crow "lineage type" kinship system as an exception to my original statement that the nomadic groups had "generational type" systems.

Howard's "theoretical" bias could more properly be termed "atheoretical".Howard does not appear to understand the role of generalizations, models, and hypotheses in science. There can be "no cognition without recognition". The most basic of all generalizations are "identifications", which mark out "...enduring or recurrent constituents in the flow of experience" (Kaplan 1964: 85). My "models", in Howard's terms, represent a starting point for the processual analysis of Plains cultures; these "models" are my assumptions as to how the data should be ordered and what "identifications" are useful. It is not my intention to belittle the contributions of the scholars who have been concerned with the Plains, but in the light of present knowledge there cannot be anything other than assumed "identifications". One could with great ease point to a number of citations which flatly contradict Howard's statement that my "model" of "nomadic bison hunters of popular lore" dates from only circa 1860-1880. For example, in 1823, Paul, Duke of Wuerttemberg (1941: 406, 409) although mentioning that the "Sioux...had only a few good horses", also wrote that "the Sioux are great wanderers and their camps therefore have strikingly nomadic appearance. In this respect they approach the Bedouins more than any other people". Further, the few studies available on the dynamics of culture change among the semi-sedentary Plains tribes strongly suggest that the semi-sedentary societies were becoming more like the "nomadic" societies (for example, Schmitt and Schmitt 1952; Deetz 1965). Hence, I would agree with Howard that the "...two adaptations were not as distinctive as painted by latter day anthropologists" but my reasons for saying this differ greatly from Howard's.

Even had I insisted that my "models" were taxonomic laws, or established "identifications", Howard would be wrong if he argued that an exception invalidated the "identification". As Kaplan has shown such a viewpoint is only an idealized reconstruction of the nature of scientific laws. Generalizations can function as laws "...when the exceptions are regarded as subject to being explained away: special though unknown conditions [might] obtain..." (Kaplan 1964: 96-97, 111-12).

I am interested in the explanation of cultural processes, whereas Howard is interested in things in themselves, be they dance steps, head-dresses or customs. Howard cannot see the forest for the trees, or more accurately, he cannot see the culture because of the artifacts and customs. In looking for exceptions to assumed "identifications" or generalizations, and in implying that exceptions invalidate them, it is virtually impossible for Howard to deal with cultural process. Howard's approach to cultural process resembles that of a mystic; a classic example of mysticism being Howard's (1965: 4) suggestion that "it was no mere accident that... [some of the] descendants [of the Middle Mississippian culture] became known to Whites as the 'Five Civilized Tribes'". Another example of
Howard's inability to deal with cultural process is his willingness to deal with culture in terms of undefined cultural "influences". This is exemplified by his statement that "...we would expect...to find a strong 'Prairie' or better 'Missouri valley' influence in...[Teton Dakota] culture...." Until the data are systematically organized in systemic terms, argumentation about the nature of cultural adaptation and the cultural processes that existed on the Plains will be resolved by faith alone.

If culture provides (or as some would say, is) man's adaptation to his environment, it follows that if we are to understand a particular adaptive (cultural) system, we must define the structure of the cultural system and relate this structure to the effective environment of the system. Further, if we are to explain the cultural processes of that system, we must define the range of variation permissible within the system; in other words determine how much fluctuation can be tolerated by a particular structure before a new system develops. In practice such a procedure often involves the study of one society through time or of many societies of a single kind at one rather restricted point in time. Ideally we would study all societies of a similar type through the entire span of their existence. This approach lies behind my original formulation of two adaptive types on the Plains.

My viewpoint assumes that the external world is knowable, that concrete systems exist in the external world, and that General Systems Theory provides the systematics best suited for making "identifications" and for explaining the processes of the concrete systems of the external world. Hence the kind of types I argued for in my original paper (a combination of symbol and associated interaction), might be referred to as "ontological types" or as I prefer "systems types", as opposed to morphological types.

Although I use the term "structure" as used in systems theory such usage is not contrary to one of several standarized usages of the term made by social anthropologists. The customs referred to by Howard as examples of my error did not elucidate the structure of Teton Dakota society. My statement, that Dollar showed "...no concern for the structure of Teton Dakota society..." attacked by Howard as "a bit strong", needs no modification. It is therefore somewhat surprising that Howard stated that "Thurman's best points are...his remarks concerning social interaction". Howard's own work represents a negation of what I argued for. An example is Howard's (1957) study of Plains mescalism, which suggested that there was a "mescal bean cult" on the Plains preceding the widespread use of peyote. A few years later, Troike (1962) showed that rather than a single "cult", Plains mescalism could be divided into at least four distinct kinds of ceremonial complexes. Howard was obviously more concerned with the definition of a morphologically defined type than with diverse patterns of interaction, or with "ontological types". Howard in recognizing a "cult" confused a configuration of traits with a structure.
Howard asked: "Did the Teton...[in the 1820's] have the Sun dance or did they have a similar ceremony closer to the Mandan Okipa?" This question cannot be answered until the adaptive significance of the Sun dance and the Okipa can be specified in systemic terms. One can answer this approximately, if, as appears likely, the Sun dance and Okipa had adaptive significance (or, in other words, if they were significant variables in the structure of Plains systems), by specifying when the specific situation arose for which the Sun dance or Okipa were adaptive responses. This is not to say that we can date the solution of an adaptive problem to the time the problem was created, but rather suggests a means of providing context for contextless statements. If, as I believe, the Teton Dakota had a subsistence-settlement system of the "nomadic" type well before 1820, and if the Sun dance was crucial to this mode of adaptation, references without context, such as those references to "medicine lodges" in the winter counts (if they were not trading posts), can best be interpreted as references to the Sun dance. The question Howard asked is irrelevant to understanding cultural process if the Sun dance did not have systemic significance. Be that as it may, even Howard admitted that the Teton of 1820-21 had the Sun dance. My statement that Dollar misrepresented Plains ceremonialism does not need modification.

After all the rebuttals I have made it is gratifying to agree with Howard on one point; he pointed out that the captive in Dollar's film was a Flathead. I tried to indicate, by writing "as I recall", that my memory might be at fault in labeling the captive a Cree. Howard is also correct in pointing out that it is certainly difficult to miss a "misshapen head". Nevertheless, I enjoyed Howard's comments although not "...reduced to hysterical laughter" (as Howard was by an error made by Dollar); I enjoyed Howard's paper just as I enjoy the bear in "Pogo" who can write but can't read.

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REJOINER TO THURMAN

by

James H. Howard

Thurman's reply to my "Comments" on his criticism of "A Man Called Horse" seems to be mostly nit-picking and hardly worth an extended reply. My only comments would be to repeat that the use of earthlodges by the Yankton, Yanktonai, and Teton Dakota in the early and middle 19th century has been well established historically, archeologically, and ethnographically. Thurman's feeble attempt to demonstrate that the Teton earthlodges mentioned in so many winter counts "were actually White trading posts" smacks of the reasoning employed by 18th and 19th century antiquarians of the "Mound Builder" persuasion: if a somewhat complicated structure (i.e. an earthlodge) is found in use among "savages" (i.e. the Teton "nomads" of Thurman's conceptual model) it must be the work of some superior group who built it for them (i.e. White traders, Prince Madoc and his Welsh colonists, etc.). Thurman's reference to George E. Hyde, a writer who never bothered to indicate the sources of his data, as "the man largely responsible for initiating ethnohistorical studies of the Teton Dakota" in this connection made me smile, and serves to indicate Thurman's general lack of control of fact and theory in regard to the Plains area.

Mr. Thurman is very skeptical of my statement that "we would expect to find a strong 'Prairie' or better 'Missouri valley' influence" in Teton Dakota culture of the 1830's-1840's. To me this seems a reasonable supposition since the Teton had entered the High Plains only a few decades before. Yet Thurman is unconvinced: "Until the data are systematically organized in systemic terms, argumentation about the nature of cultural
adaptation and the cultural processes that existed on the plains will be resolved largely by faith alone." Obviously Mr. Thurman has no faith in my reconstructions, or in fact in the ethnohistorical reconstructions of any other scholar who does not share his own theoretical or methodological views. His extreme skepticism in this case reminds me of the old farmer who, when a friend pointed out of the train window at a nearby flock of sheep, and mentioned that they had been sheared of their wool, replied dourly, "Sheared on this side, anyway!"
Howard's rejoinder is a pathetic attempt to shrug off criticisms which have exposed the fragile methodological underpinnings of his own work. Archeologists, in dealing with baffling situations, often have had recourse to calling items of unknown use "ceremonial objects". Howard's use of "nit-picking" is analogous to an archeologist's "ceremonial objects"; he is trying to ignore the existence of something with which he is unequipped to deal. The comparison of a systemic approach to the work of "eighteenth and nineteenth century antiquarians" and the charge that I "lack... control of facts and theory in regard to the Plains area", betrays Howard's gross confusion about theoretical and methodological matters. Hence, to marshal like-minded individuals, Howard waves the banner of theoretical bias, erroneously charging that "Mr. Thurman has no faith...in the ethnohistorical reconstruction of any other scholar who does not share his own theoretical or methodological views". Howard could not make such a statement were he familiar with my work. I respect the work of many competent scholars who have theoretical biases other than my own for example Anthony F. C. Wallace's fine studies of the Delaware.

There are two points I wish to make concerning the evaluation of George E. Hyde's work and ethnohistorical source analysis in general. Howard's attack on Hyde is unjustified and proclaims both his lack of breadth in controlling the Plains literature and his lack of analytical power. Were I not in the field at the present time I would more adequately document the evaluation of Hyde which follows. It is time for Hyde to receive his due. George E. Hyde was a "Research Assistant" for George B. Grinnell. Hyde's correspondence with Bent and the Bent "memoires" are important sources on the Cheyenne. Hyde's Pawnee Indians is still the most comprehensive published historical account of the Pawnee. Hyde had a fairly intimate knowledge of both the Cheyenne and the Pawnee, the two Plains groups most intimately tied to the Teton Dakota (one as friend and ally, the other as arch-foe). Further, in regard to the Teton themselves, Hyde's Red Cloud's Folk was the first serious attempt to deal with the Oglala, his book on the Brulé added significantly to our knowledge, and his Sioux Chronicle made the first explicit statement of the position that Indian history did not end with the reservation period. It is true that Hyde did not cite "chapter and verse" when referring to a source, but one familiar with the Plains literature can usually determine Hyde's sources. Hyde's reputation has suffered because of some very bad work such as Indians of the High Plains, but a scholar should not assume, as Howard apparently does, that because one makes some errors that all of that person's work is fallacious. A scholar must separate the good from the bad in anyone's work.

I have shown elsewhere ("A Case of Historical Mythology: The Skidi Pawnee Morning Star Sacrifice of 1833," Plains Anthropologist, 1970) that source analysis is an essential preliminary to ethnohistorical
studies. There have been few analyses of Plains sources. Howard, it will be noted, did not try to rebut my statements that his work has ignored sources analysis. Howard and some other workers have been content to substitute repetition of erroneous information for analysis. Howard's story about the skeptical old farmer is precisely the attitude with which we should approach our sources.

Nothing anyone can say will change Howard's approach if he does not wish to change it. He is certainly entitled to dance at Indian pow-wows and write articles for Indian craft magazines if he so desires, but his work is neither good history nor good anthropology and it certainly does not deserve to be called ethnohistory. My personal philosophy is similar to a statement attributed to Adlai Stevenson: "I believe in the forgiveness of sin and the redemption of ignorance." Perhaps there is still hope for Howard.
HOWARD’S FINAL COMMENT

James H. Howard

Since I have pretty well expressed my views in previous communications and since this exchange probably began to bore our readers some time back, I will simply comment with a "no comment" to Thurman's reply to my rejoinder. Perhaps I am badly in need of elucidation regarding the niceties of the systemic approach in archeology. Perhaps Mr. Thurman, since he believes in the "forgiveness of sin and the redemption of ignorance", and since he seems to be the only one among us who possesses the TRUE WORD, would consent to give me basic instruction. Since Thurman seems to be fascinated with my occasional participation in Indian dances, I therefore make the following offer: In return for lessons in the TRUE WORD of the systemic approach in historical archeology I will offer commensurate instruction in the systemic approach to Plains Indian War dancing. There is but one condition — before said lessons begin Mr. Thurman will have to take his foot out of his mouth.
Several points of interest to general anthropology have been raised in this exchange. These include a number of theoretical issues: the nature of the interrelationships of archeological and ethnographic observations; the nature of typology; the nature of explanation of cultural processes. The most important methodological issue raised concerned the necessity of source analysis as a preliminary to ethno-historical investigation.

In addition to the problems of wider import, a number of specific points of Plains ethnohistory have also been touched on. In general, these points were raised as specific examples of the wider problems of ethnohistorical source analysis. The specific plains issues include: the interpretation of statements given in calendrical counts; the existence of Teton earthlodges west of the Missouri River; the time the Sun dance developed among the Teton and its relationship with the Okipa; the time the "nomadic" culture type developed.

I would be glad to discuss any of the theoretical, methodological, or specific Plains problems with any scholar interested in them, either in print or by letter.
THE JOHN M. GOGGIN AWARD
FOR METHOD AND THEORY IN HISTORICAL ARCHAEOLOGY

In 1959 The Conference on Historic Site Archaeology was organized
to present papers emphasizing artifact analysis. The following year
John M. Goggin urged that the "conference get down to brass tacks...to
the kind of details that archaeologists deal with. In other words my
feeling is that as archaeologists we deal with artifacts; and with few
exceptions colonial artifacts have not been analyzed or classified by a
method suitable for the archaeologist to handle. Therefore it is up to
us to do so, and I would like to see it started." Since 1960 The Confer-
ence on Historic Site Archaeology has published papers presented at
the annual Conference, with the participants being urged to emphasize
analysis and synthesis in their presentations. In keeping with this
philosophy the John M. Goggin Award of $500.00 is offered by the Confer-
ence to encourage scholarly research in method, theory, and interpretation
in historical archaeology.

Any member of the Conference is eligible to submit a manuscript for
judging by the Award Committee. The John M. Goggin award manuscript will
be published in The Conference on Historic Site Archaeology Papers along
with other entries selected by the Award Committee. No award will be
given in years in which submissions fail to meet the standards of the
Award Committee.

The John M. Goggin Award will be presented at the annual meeting of
The Conference on Historic Site Archaeology, at which time the volume of
The Conference on Historic Site Archaeology Papers in which the award
paper appears will be made available for sale and distribution to Con-
ference members.

The manuscript should be an original, unpublished work, not over
30,000 words, and should be submitted as a typed, double-spaced, ribbon
copy (the author should retain a carbon copy). The footnotes and bib-
liography should follow the format used in American Antiquity. Maps,
charts, graphs and other illustrations should be in final form for repro-
duction, and should be submitted with the manuscript.

The John M. Goggin Committee invites Conference members to submit
papers on method and theory in historical archaeology for this competi-
tion. Papers should be received by the Chairman by June first of each
year. Since the purpose of the John M. Goggin Award is to emphasize
quality papers on method and theory, no award will be given by the Com-
mittee when the submitted papers do not meet the quality requirements
of the Committee.

The John M. Goggin Award Committee is composed of Stanley South,
Chairman, Lewis R. Binford, James E. Fitting, Roderick Sprague, and
Iain C. Walker.
The winner of the $500.00 John M. Goggin Award for Method and Theory in Historical Archaeology was a paper "Ethnohistoric Archaeology and the De Soto Entrada into the Lower Mississippi Valley" by Jeffrey P. Brain, Alan Toth, and Antonio Rodriguez-Buckingham. This paper, and the two other papers submitted for this competition are presented here as Part 4 of this volume.

The John M. Goggin Award is offered in memory of a highly productive, and outstandingly versatile anthropologist-archaeologist-ethnologist. In his studies of material culture his use of "controlled sample collecting," "cultural patterns," "traditions," seriation, stratigraphy, typology, modes, environmental factors, underwater archaeology, historical research, and chronological studies place him foremost among those using these methodological tools in historical archaeology. His work in this field clearly reveals the advantage gained from a broad theoretical base.

The John M. Goggin Award Committee is happy to present these papers as a step toward the furtherance of quality in the field of historical archaeology through an increased emphasis on method and theory. The quality of these papers in Part 4 is encouraging.

THE JOHN M. GOGGIN AWARD FUND

The John M. Goggin Award Fund was created by withdrawing from the budget of The Conference on Historic Site Archaeology the sum of $1,000. from the sale of memberships. Dr. Margaret K. Goggin added to this fund through the contribution of $350. which had accumulated in royalties on the sale of one of John's books. This gift was most welcome and appreciated. A gift of $50. was presented by an anonymous donor at the Memphis meeting of the Conference, and this gift was also greatly appreciated. The fund will have to be supported in this manner in order to provide enough funding to keep the award continuing from year to year.
ETHNOHISTORIC ARCHAEOLOGY
AND THE DE SOTO ENTRADA INTO THE LOWER MISSISSIPPI VALLEY

Jeffrey P. Brain
Alan Toth
Antonio Rodriguez-Buckingham

ABSTRACT

The concept of "ethnohistoric archaeology" is introduced and explicated. In ethnohistoric archaeology, a multi-discipline approach is applied to historic contact situations operating in a native context. The special problem chosen as a case study to illustrate the approach is the De Soto entrada into the Lower Mississippi Valley in 1541.

INTRODUCTION

It is given, here, to examine the utility of a multi-discipline approach to the study of aboriginal contexts relating to the historic contact period. Specifically, it is our contention that to consider phenomena operative within the native milieu, from the first European contact to the final acculturation, we are privileged and also required to draw upon the three disciplines of ethnography, history and archaeology. The domain and relationship of each discipline is diagrammed in Figure 1. Each of these sources has its own data base and distinctive problems of interpretation, but in a given context can be supportive and supplementary at crucial points. By bringing all this information to bear it may be possible to resolve some impasses that have developed individually. The special problem we have chosen, the De Soto entrada into the Mississippi Valley, is illustrative of this point.

Leaving aside the relatively common archaeological foundation, ethnohistoric archaeology differs from historic archaeology precisely as indicated by the semantics. Historic archaeology has customarily been confined to our own Euro-American history, and is usually identified by period or event, e.g., colonial, revolutionary or frontier. Ethnohistoric archaeology, however, refers to the contemporary native contexts and benefits from the addition of ethnographic data, methods and interpretations, a perspective we are not traditionally wont to apply to ourselves. It is precisely in integrating this benefit that the utmost care must be exercised. "Ethnographies" may actually be little more than ethnohistories, a perfectly valid source of data, but one with its own problems of evaluation and interpretation.

The difference between history and ethnohistory is on the one hand the quantity of historic documents, but, on the other, and more importantly, the quality of the reportage. There is an inescapable qualitative difference between in-culture accounts and extra-cultural descriptions, especially from the scientifically less sophisticated period with which we are concerned. Observations made within a cultural milieu by
THE SPECIAL POSITION OF ETHNOHISTORIC ARCHAEOLOGY

Figure 1
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an initiate of that cultural tradition may be supposed to have a higher index of accuracy for interpretation than those which have been interpreted across cultural lines. Of course, this statement is subject to the exceptions inherent in all generalities (and does not even begin to consider evaluation of the particular documents themselves), but remains a valid caveat in all our investigations. The ethnohistoric archaeologist must be aware that his historic data may be garbled and therefore require a certain cultural filtering, and this can be accomplished only through an understanding of both the historic and the ethnographic cultural milieus.

Thus, for example, it is not enough to rely on historic accounts that a village of tribe X was located five leagues from point A on the left bank of a certain river. It is also necessary to have a feel for where in this locale the cultural pattern of the tribe would most likely have them situated. So also, it is not enough to know that chamber pots were traded to tribe X without knowing what new use they were put to—perhaps the material for some merriment, but an important cross-cultural indicator. Further, it may be true that the chieftain of this same hypothetical tribe had despotic authority rivaling that of Louis XIV, but it must be considered what this actually meant in the tribal context. And so forth: ethnographic data is thus a necessary catalyst; but it provides only one of several bases. In other words, there may be more sources of data available to the ethnohistoric archaeologist, but their actual utility must be carefully evaluated, individually and corporately.

To take a case study, we may consider the De Soto entrada into the southeastern United States. For an early exploratory attempt, this mission is exceptionally well documented by at least four extant narratives, some of which are remarkably detailed. Immediately upon inspection, however, a number of discrepancies in details are apparent in these records, often rendering identification and interpretation difficult even though it is clear that the same events are being recorded. These discrepancies, and the equally serious reverse situation of omission, are common problems of historiography, but in this case they may be resolvable by the rigid application of certain criteria to the interpretation and evaluation of the records (historiography), counterbalanced by ethnographic and archaeological data. In any particular investigation of ethnohistoric archaeology any one of the three data sources may be the focus of attention with the others playing supporting roles, depending upon the problem. The problem we have chosen to discuss here, the initial perambulations of the De Soto expedition within the Mississippi Valley, is founded in the historic records, and the primary emphasis shall be upon the evaluation and interpretation of these sources, supported by data from later ethnography and current archaeology.

The new resources that we can bring to bear on the De Soto problem are the stricter methodology in the use of the sixteenth century historic

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1"Soto" is the proper nomen, but we follow the prevailing American orthography, which is deeply entrenched in the literature.
documents, a greater degree of historic and archaeological correlation with seventeenth and eighteenth century "ethnographies" and, most important of all, a considerable advance in the archaeological development of the past, especially relating to the late prehistoric and early historic periods. The archaeological advance has developed from the application of methodological innovations to a vast accumulation of raw data, which provided the base for more sophisticated interpretation. It was our increased confidence in the control of the archaeological data that first led to our reopening of the De Soto case, and in the testing of our interpretations that our confidence was first rewarded.

The last major attempt to trace the De Soto entrada into the Mississippi Valley was also founded on the perspective afforded by the input of the first modern archaeological investigation of the general region. In a survey conducted during the 1940's, Phillips, Ford and Griffin (1951) gathered an impressive sample of controlled data. The bulk of the artifactual data was potsherds, which were classified into types. The distribution of these types through time and space was then plotted, agglomerates of types (pottery complexes) were recognized and seriated, and finally ceramic chronologies were developed. In this fashion, site occupations and contemporaneity of occupations could be predicted with reasonable accuracy. However, in reference to the De Soto problem, although considerable new insight was provided which led to stronger hypotheses, the control was not yet finite enough to overcome the concluding equivocations.

Continuing archaeological work in the succeeding twenty-five years has added significantly to the basic corpus of data, one result of which was to provoke more sophisticated methodological innovations in its organization, which in turn has brought finer control and more precise interpretations. Thus, Phillips (1970) devised a classification in which the old pottery types were reorganized and subdivided into varieties that are regional culture-historical markers with more rigidly defined parameters in time and space. The complex of Lower Mississippi Valley pottery varieties that mark the mid-sixteenth century, and thus contemporaneity with De Soto, are listed in Table 1, and the special diagnostics are illustrated in Figure 2. Other cultural clues, such as settlement patterns, site plans and intra-site features, will be noted below as we develop the case.

While these refinements have still not brought us to the specific year of 1541, they nevertheless do make it possible to identify sites that were almost certainly occupied at this date, and to exclude from consideration those that were not. For our present purposes, this is sufficient in choosing between the possibilities that correlate with the ethnohistory. As already noted, the ethnohistoric framework is developed from the De Soto narratives, corroborated by later ethnographic data. The key to this development rests in the evaluation of the narratives.
Table 11: Diagnostic Varieties of Pottery Types in Northwestern Mississippi and Eastern Arkansas circa 1541*

<table>
<thead>
<tr>
<th>Types</th>
<th>Varieties</th>
<th>Special Modes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Avenue Polychrome</td>
<td>Avenue</td>
<td>Effigy Vessel</td>
</tr>
<tr>
<td>Barton Incised</td>
<td>Kent</td>
<td>Strap Handles</td>
</tr>
<tr>
<td>Bell Plain</td>
<td>Bell</td>
<td>Rim Adornos Vessel</td>
</tr>
<tr>
<td>Carson R/B</td>
<td>Carson</td>
<td>Rim Adornos Vessel</td>
</tr>
<tr>
<td>Fortune Noded</td>
<td>Fortune</td>
<td>Strap Handles</td>
</tr>
<tr>
<td>Hollywood White</td>
<td>Hollywood</td>
<td></td>
</tr>
<tr>
<td>Leland Incised</td>
<td>Blanchard</td>
<td></td>
</tr>
<tr>
<td>Mississippi Plain</td>
<td>Neeleys Ferry</td>
<td>Rim Adornos</td>
</tr>
<tr>
<td>Mound Place Incised</td>
<td>Chickasawba</td>
<td>Rim Adornos</td>
</tr>
<tr>
<td>Nodena R/W</td>
<td>Nodena</td>
<td>Effigy Vessel</td>
</tr>
<tr>
<td>Old Town Red</td>
<td>Beaverdam</td>
<td>Effigy Vessel</td>
</tr>
<tr>
<td>Owens Punctated</td>
<td>Owens</td>
<td>Strap Handles</td>
</tr>
<tr>
<td>Parkin Punctated</td>
<td>Parkin</td>
<td>Strap Handles</td>
</tr>
<tr>
<td>Parkin Punctated</td>
<td>Harris</td>
<td></td>
</tr>
<tr>
<td>Rhodes Incised</td>
<td>Rhodes</td>
<td></td>
</tr>
<tr>
<td>Rhodes Incised</td>
<td>Horn Lake</td>
<td></td>
</tr>
</tbody>
</table>

*Other types and varieties may be present, but are not diagnostic. It is unlikely that all of the varieties listed here will be found in the same archaeological context as they relate to several discrete archaeological phases in the area. However, they are all closely contemporary and where a significant number characterize the assemblage, then a mid-16th century date line may be identified.
<table>
<thead>
<tr>
<th>Types</th>
<th>Varieties</th>
<th>Special Modes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Vernon Paul Appliqué</td>
<td>Vernon Paul</td>
<td>Strap</td>
</tr>
<tr>
<td>Walls Engraved</td>
<td>Walls</td>
<td>Handles</td>
</tr>
<tr>
<td>Walls Engraved</td>
<td>Hull</td>
<td></td>
</tr>
<tr>
<td>Winterville Incised</td>
<td>Ranch</td>
<td>Strap</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Handles</td>
</tr>
</tbody>
</table>
Figure 2. Examples of decorated pottery diagnostic of the southern frontier of the Mississippian culture in the Lower Valley ca. A.D. 1541. a, Fortune, b, Harris, c, Parkin, d, Ranch, e, Vernon Paul, f, Kent, g, Rhodes, h, Owens, i, Nodena, j, Avenue, k, Walls, l, Carson, m, Horn Lake, n, Bell, o, Blanchard, p, Chickasawba, q, Beaverdam. (After Phillips 1970).
Proper evaluation of written documentary sources centers on the recognition of a number of factors. Some of these factors must include an understanding of the time, place and circumstances under which the document was registered, and the recognition of the fact that historical documents are usually recorded relative to one person's set of individual experiences. Because of this last reason, it is common to find discrepancies, and even contradictions, between sources which record the same event. In cases such as this, it is important to determine if there is agreement concerning the general fabric of the related event. Thus, criteria must be formulated by which the information may be structured into levels where one account may be accepted over another based on historical probability, the methodology known to have been used by the chronicler, and data available from other sources. In this fashion, the proper pieces of the puzzle may be gently matched rather than being hammered into place or rejected on subjective grounds of sheer frustration.

For the reconstruction of the path which the main column of De Soto's army followed into the Lower Mississippi Valley, the fact that there are four well-known accounts in existence—those of Elvas, Biedma, Rangel and Garcilaso—has been both a blessing and a curse. It is fortunate that they exist because by comparing them the analyst may develop stronger interpretations. On the other hand, it is unfortunate that this comparison has also been used to either support or refute preconceived notions. Because of this danger, it is our intention to formulate criteria of acceptance or refusal of the information that comes from the various accounts.

A consideration of the four sources narrating the entrada reveals information that may be structured into two main categories. On the one hand, there is a statistical type of information which refers to distances, time lapses, numbers of people, and similar quantitative data. On the other hand, there is a descriptive type of information which has to do with ethnographic or geographic descriptions and delineations of events. The source which best exemplifies the first type of information is Rangel's account; that which exemplifies the second is Garcilaso's. Nevertheless, these two, as well as Biedma's and Elvas' narratives, exhibit a mixture of both types of information interwoven in a sometimes
indistinguishable manner. Elvas' account is very complete for the pro­
tion of the route here studied, but for the reasons given below, it is
utilized mainly to complete the general picture. Biedma's account is
less detailed than that of Elvas and has also been utilized as a supple­
mentary source. In the paragraphs which follow, these criteria will be
explained.

The strongest aspect of Elvas' input lies in its nearness in time
to the narrated events and its value as a firsthand, eyewitness account.
In this sense, both his statistical and descriptive information should
be very valuable. However, since he was a chronicler who preferred to
remain anonymous, little can be stated as to the goals he hoped to ac­
complish with the narratives and, particularly, what type of methodology
he used in his work. It is for this reason that we think of him as
corroborative on both levels of information.

Biedma's account carried the weight of being an eyewitness narra­
tive addressed to the "King of Spain in Council". However, this very
fact also implies the risky possibility of its having been especially
arranged for that purpose. But, more important to our study is the fact
that for the section of the route here in question, Biedma's account is
rather brief and undetailed. For this reason, the Biedma narrative is
also considered as corroborative.

An extremely important point has hitherto been overlooked by most
researchers using Ranjel's account. That is, that since the story is
told by Ranjel to Oviedo, it is the latter not the former who actually
narrates it. Thus, in order to assess the information in this narrative,
a distinction must be made between Ranjel's input and that of Oviedo.
It will be relevant at this point to briefly discuss this dual aspect
of the narrative.

Don Gonzalo Fernández de Oviedo y Valdés is known as the first
chronicler of the New World. His great work, the Historia general
y natural de las Indias began to appear in 1526 and continued through­
out the centuries in augmented editions up until the nineteenth century.
For our purposes, the value of the Historia general is that, in it,
Oviedo combines detailed narrative with the usage of a variety of sources.
But, this great quality of Oviedo's work is overshadowed by its lack of

3 In many ways, as we shall see, Ranjel and Oviedo have the same
relationship as Carmona, Coles, and the anonymous friend (Gonzalo
Silvestre) hold to Garcilaso.

4 For a recent biobibliographical study of Oviedo, see Turner, 1966.

5 In the course of this study, an extract from Oviedo's account was
noted to have been printed in the English language in London in 1577
(Eden, 1577).
order and systematic methodology. To this effect, he himself admits:

And with determination I state that the matters that come to my mind are so abundant that I find a great difficulty in writing them and confirming those that are relevant to the story we follow (Translated from Oviedo, 1851-1855: Vol. 1, 161).

That Oviedo occasionally yields to his powerful imagination was indicated in a study by Salas (1959). In the same study, however, Salas also points out that Oviedo follows, with great care, both his written sources and the accounts of his informants. In the case of Ranjel's account, it is sometimes easy to detect Oviedo's own views. However, on the more subtle level of ethnographic descriptions, it may not be quite as easy since Oviedo had traveled in various parts of the New World and often interwove accounts of his own observations. Because of the possibility of this intrusion, we are primarily following his information on the statistical level where it is safe to assume that distortion by the chronicler should be almost non-existent. This assumption is strengthened by remembering that Ranjel was the secretary to De Soto and, thus, in the unique position of having to keep accurate records of supplies, distances and lapses in time. Oviedo, in his official capacity of Chronicler to the Crown, would have been careful to register this information. It is for this reason that, though we understand Ranjel's account within the context of Oviedo's Historia, we will continue to refer to his input as Ranjel.

Of all accounts of the history of the entrada, that of Garcilaso has probably been the most used and the least understood. Attacked by some and patronized by others, Garcilaso's narrative was either conveniently utilized or harshly tossed aside by nearly all researchers depending on what appeared to be the most convenient course of action at a given time.

Phillips, in agreement with the approach that we use in this report, notes the misuses of Garcilaso's account:

This is in effect a rather free compilation of all four primary sources, giving reference in the generally accepted order of reliability, as follows: Ranjel, Biedma, Elvas, and Garcilaso. The last-named is commonly regarded as completely unreliable and resorted to only in cases of desperate necessity when one wants very much to prove a point. On the other hand, though not always to be trusted in matters of time, distance, population, and battle statistics, the Inca is far more generous with descriptive detail than the other three chroniclers. Such material, carefully screened for exaggeration and sheer invention, can be very useful in determining what kind of people the Spaniards encountered and how they lived (Phillips et al., 1951: 349).
For our purposes, then, Garcilaso's account is most valuable on the descriptive level of ethnohistoric information. A most unique aspect of his work is seen in the effort he puts into presenting and explaining his sources. In the preface of the book, he admits that statistical information was confused in his original accounts. Later, he emphasizes this point as follows:

But even though in this stage of my history as in others I have given the route that the army traveled when it left some one province for the purpose of going to another, I have not shown the latitude of each province or pointed out strictly the course that our men took. For, as I have already stated elsewhere, in spite of the fact that I have endeavored to learn these details, it has not been possible for me to do so because he who gave me the account was neither cosmographer nor mariner and as a result did not know them. Furthermore, the army carried no instruments to take the elevation of the land and had no one to obtain or consider it, for with the disgust that all bore at not finding gold or silver, they learned nothing well (Garcilaso, 1951: 484).

The methodology he uses stresses the fact that his informants gave their accounts relative to their set of experiences. If we remember that the main column of the army was preceded by search parties (avanzadas), the confusion in the reports of the time, distance, and other measurements may be understood. Mental pictures of specific events are not easily forgotten or contradicted. Thus, while Garcilaso's sources often confused the statistical picture, they added to the descriptive frame of information. An example of the reliability of Garcilaso on this level of information may be illustrated by his description of Pacaha which, as will be seen later, holds a relationship to the archaeological evidence. On the other hand, his omission of Aquixo shows his limitation at the statistical level of information.

Last, but not least, it must be kept in mind that Garcilaso wrote during the time of the European Renaissance which, in Spain, took the form commonly referred to as the Golden Age of Literature. Thus, he wrote during an age deeply concerned with aesthetics and style which gave giants in these fields such as Lope de Vega, Cervantes, and Gongora. When accuracy of minutia was unavailable, style would fill the gap.

6These were Alonso de Carmona, Juan Coles, and an anonymous friend whom we agree was Gonzalo Silvestre. The role of these men in the events is a crucial factor in the evaluation of Garcilaso's account.

7For example, an interesting relationship appeared at first to exist between some of the figures given in the text and the particular book in which they appear. Thus, in Book IV there seems to exist a higher usage of the number four and its multiples. A second check, however, reveals contradictions to this possible stylistic device. Yet we feel obliged to mention it to cover the possibility of its being more than a mere coincidence.
Summarizing the points thus far made about all four primary sources, it may be stated that our evaluation differs from that of previous researchers in that we see all sources as equally valuable, though relative to the point in the framework of information at which the input is acceptable. While Ranjel and Garcilaso are at the two ends of the scale, Biedma and Elvas help us to complete the picture.

The set of information has been divided into two categories:

1) Descriptive type information, such as ethnography, geography and specific events
2) Statistical type information such as dates, distances and directions

According to the criteria here defined, the accounts given by the chroniclers in question are related to these categories. Garcilaso's account is said to belong to the descriptive level of information and exemplifies this category at its best. Ranjel's account is defined as belonging to the statistical type of information and also exemplifies that category at its best. Elvas' and Biedma's accounts are utilized at both levels, particularly when there is discrepancy among the sources and judgement renders this utilization necessary. On these bases, the De Soto entrada into the Mississippi Valley is evaluated and related to the archaeological and ethnographic evidence.

A REAPPRAISAL OF EXISTING HYPOTHESES

The route of De Soto, and especially the location of his crossing of the Mississippi River, has been a popular problem of historians for many years. As a result of extensive scholarly activity in this area, a number of hypotheses have been generated, the most important of which have been carefully reviewed by Swanton (1939) and again by Phillips (Phillips et al., 1951: 348-91). In general, scholars have employed old maps and a comparison of local topography to the several firsthand narrative descriptions in order to trace the route of the De Soto entrada. The effective application of archaeology as an additional research tool was initiated by Phillips and his co-workers (1951) using information collected by the Lower Mississippi Survey. Their conclusions and the review by Swanton (1952) represent the maximum exploitation of all evidence available at the time.

Two decades of continued research by the Lower Mississippi Survey have contributed enough new information to justify a re-evaluation of existing hypotheses as a prelude to presenting the conclusions of the present study. The three most widely accepted theories place De Soto's crossing of the Mississippi in the vicinity of 1) the Chickasaw Bluffs near Memphis, 2) Commerce Landing in Tunica County, Mississippi and 3) Sunflower Landing in Coahoma County, Mississippi. The possibilities are
illustrated in Figure 3. In each case, events on the other side of the river in Arkansas hold the key to the correct identification.

The Memphis Hypothesis

The notion that the Spaniards crossed the Mississippi River near the present city of Memphis was widely accepted during the nineteenth century. The eminence of such historians as George Bancroft and John Gilmary Shea lent strong support to the identification. Garcilaso's statement that the army traveled north from Alibamo so as to avoid the sea is used as the primary evidence for the Memphis hypothesis. The existence of an old trail between the historic Chickasaw capital and the lower Chickasaw Bluff, combined with the difficulty of crossing the backswamps of the Yazoo Basin, provide additional details which have been employed by Malone (1919) and others to affirm Garcilaso's account. Young (1927) has provided a summary argument for the Memphis route.

The archaeological situation connected with the Memphis hypothesis has been outlined previously (Phillips et al., 1951: 389-90). The Shelby Site (12-P-2) and the Bradley Site (11-P-2) are possible candidates for Quixúz and Aquixo respectively. A group of sites on the Tyronza River or Little River that might represent Casqui have not been located. The case for Pacaha is better since there are several St. Francis type villages in the right location. They consist of Carson Lake (10-P-1), Upper Nodena (10-Q-1), Notgrass (10-P-4), and Bell (10-P-2). All contain enough of the marker ceramic varieties to suggest that they were occupied in 1541. Upper Nodena would be the best choice for the capital as it is the only one with any type of a mound. Difficulty again arises in locating Quiguate. A group of large St. Francis type villages including Neely's Ferry (11-N-4), Vernon Paul (11-N-0), Williamson (11-N-13), Parkin (11-N-1) and Rose Mound (12-N-3) are nested between the St. Francis and Tyronza Rivers. The recent find of a small metal bell at Parkin (Davis, 1966: 11) indicates that this site at least was used into the period of European contact. However, the sites as a group lack sufficient quantities of the ceramic markers postulated for the 1541 date line to lend strength to the Memphis hypothesis. It would seem that their principal occupation was perhaps a century too early.

In summary, the route in Arkansas which must result from a river crossing at Memphis is poorly supported archeologically. Although there

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8A St. Francis type of settlement is a planned village laid out in a roughly rectangular form. Houses were concentrated around a plaza and its adjacent mounds. A palisade, and sometimes a ditch, surrounded the entire settlement. Accumulation of refuse within the town frequently resulted in the entire area being elevated several feet above the surrounding terrain. For a complete description see Phillips et al., 1951: 329-31. The St. Francis type villages match perfectly the type required by the De Soto narratives for this portion of the journey.
Possible De Soto Routes

- Memphis Hypothesis
- Commerce Hypothesis
- Sunflower Hypothesis

Figure 3
are some sites in the region that were probably occupied as late as 1541, they are not in all the right places. Specifically, good candidates for the important towns of Casqui and Quiguate are lacking. Considering our comments on the reliability of Garcilaso for statistical information pertaining to the route, there is really very little evidence to testify to a crossing of the Mississippi opposite the Chickasaw Bluffs at Memphis.

The Commerce Landing Hypothesis

Not everyone was satisfied with having De Soto's army cross the great river at Memphis, especially not a group of partisan scholars from Mississippi who by the early 1900's had contrived a very plausible scheme which relocated the crossing south into their state. The originator of the theory, Theodore H. Lewis, proposed very precisely that:

The crossing was made either at Council Bend or Walnut Bend, in Tunica County, in a straight line some 25 to 38 miles below Memphis (Lewis, 1927: 18).

The proposal received enthusiastic endorsement from Dunbar Rowland, the Mississippi State Archivist, and Charles A. Barton who was one of the first to employ archaeological data in support of the theory (Rowland, 1927; Barton, 1927). Barton's identification of the Quizquiz villages, having absolutely no control of the time element, was archaeologically naive but, nevertheless, a worthwhile contribution for it cataloged many sites in the Commerce region. Phillips, Ford and Griffin (1951: 384-89) brought a more sophisticated archaeological perspective to bear on the problem and, while not endorsing the Commerce Landing hypothesis outright, were very favorable to the idea.

In brief, the Commerce Landing hypothesis holds that De Soto crossed the northern part of the Yazoo Basin which is characterized by the sluggish streams and marshy places mentioned by Elvas and Ranjel. Finding the towns of Quizquiz in the vicinity of Commerce Landing, the army crossed the Mississippi and traversed difficult swamps surrounding Fifteen Mile Bayou until they reached the capital of Casqui near the mouth of the Tyronza River. From there, they continued on to Pacaha which Lewis (1927: 20) locates around Osceola, Arkansas. After sojourning in Pacaha for a month, De Soto backtracked to a Casqui town on the St. Francis (the river of Casqui) from whence he departed for Quiguate, "the largest town in Florida." Lewis (1927: 20) places Quiguate on the west side of the St. Francis River a little above its junction with the Languille. From Quiguate, the Spaniards changed course to the White (the river of Coligua) and passed out of the Mississippi Valley.

The archaeological evidence for the Commerce Landing route has been described in detail (Phillips et al, 1951). However, the finer chronological control which now can be brought to bear on the evidence
results in an evaluation which is less favorable to the Commerce Landing hypothesis. There are, nevertheless, some very important sites along the proposed route which quite certainly were occupied at the time of the De Soto expedition.

As Barton recognized in 1927, a very strong case can be made for the identification of Quizquiz. Just below Commerce Landing and located about a league apart are two large sites, Commerce (13-0-11) and Hollywood (13-0-10). Both have large pyramidal mounds, some twenty feet in height, as well as many smaller mounds arranged around a plaza. An abundance of ceramic markers suggesting a 1541 occupation are present in the collections from both. Another large village site in the vicinity, Indian Creek (13-0-2), may date from the same period. In short, there is no difficulty in speculating that Hollywood might be the Quizquiz capital and Commerce the large town near the Mississippi River.

Pursuing the Commerce route to Aquixo, one again finds some imposing sites. Belle Meade (13-0-5), Beck (13-0-7), Barrett (13-0-1) and Pouncey (12-0-2) all have yielded ceramics which have been used to define the 1541 date line. Belle Meade, especially, exhibits a wide variety of all the ceramic markers. The site is a large St. Francis type village with good evidence of a palisade. About the only difficulty there is in conceptualizing the Belle Meade group as Aquixo is that the sites are so impressive one wonders at the poor notice the Aquixo towns received in the narratives.

The Commerce Landing hypothesis receives a severe setback when the route finally wends its way to Casqui. As discussed as a possible Quiguate candidate for the Memphis hypothesis, the Parkin/Rose Mound/Neeley's Ferry group simply does not seem to have had a substantial occupation as late as 1541. Diagnostic ceramic markers are rare and the sites probably represent a slightly earlier settlement of the St. Francis Basin. There is also a problem with topography, specifically the absence of a suitable stretch of high ground nearby as remarked upon by Garcilaso and Elvas.

Continuing the Commerce route, with less enthusiasm, one finds a cluster of sites that could pass for Pacaha. The satisfactory qualifications of Carson Lake, Upper Nodena, Notgrass, and Bell have been detailed previously, again in the context of possible towns of Pacaha, for the Memphis hypothesis.

A final obstacle to the Commerce route is met at Quiguate. The only appropriate sites in the region include Castile Landing (13-N-21), a St. Francis type village with a nine-foot pyramidal mound, and two village sites with smaller mounds, Lakeside (13-N-18) and Riverside (13-N-19). Besides falling short of expectations for "the largest town in Florida," these sites lack the appropriate ceramic markers and were probably settled at the same time as the main occupation at the Parkin group—about a century before De Soto.
The Commerce Landing hypothesis thus has the same problem as the Memphis hypothesis: there are no sites along the route which meet the qualifications for Casqui and Quiguate. In addition, there is the serious difficulty of identifying the White with the river of Coligua, an association which creates many problems for the itinerary beyond the Lower Mississippi Valley which cannot be discussed in a paper of this scope.

The Sunflower Landing Hypothesis

A crossing of the Mississippi River at Sunflower Landing was championed by the U.S. De Soto Commission (Swanton, 1939), and the theory was the one most fully evaluated archaeologically by Phillips, Ford, and Griffin (1951: 361-84). The details on which the hypothesis is built are so adequately described in the official report that it should suffice to quickly summarize the proposed route.

According to the Commission's findings, the Spaniards marched directly west from Alibamo, across the Yazoo Basin, to a point adjacent to the present Sunflower Bend. There the crossing was made. The route continued north through the backswamps of tributaries to the White River as far as Crowley's Ridge where the villages of Casqui were located. Another march to the north brought the army to the floodplain above the mouth of the St. Francis and to Pacaha. The return route to the south went back past Casqui, across the White, and beyond to Quiguate which the Commission proclaimed to be in the delta between the Arkansas and White rivers.

The problems involved in linking the Sunflower route with archaeological sites have been defined fully (Phillips et al, 1951), and all that can be added here is the endorsement provided by improved chronological control. Of the sites shown in Figure 3 along the Sunflower River—well to the east of the Mississippi's Sunflower Bend—all but Mattson (16-N-9) and possibly Oliver (16-N-6) are ceramically dated to the proper period. Spendthrift (16-O-2) and Myer (16-N-10) are substantial sites with large platform mounds, and the fact that they were almost certainly occupied in 1541 makes it hard to believe that the army entered Quizquiz by this route without attracting attention.

Turning to the other sites shown in Figure 3 near Sunflower Bend, Bramlett (16-N-7), Stokes Bayou (16-M-6), Mount Olive (16-M-5) and possibly Alligator (16-N-2) have enough of the right ceramic markers to suggest that they were contemporary to the Quizquiz stormed by De

9It is altogether possible that if a larger collection from Mattson were available, it too would be assigned to the De Soto period.
Soto. For a Sunflower Landing route, Bramlett and Alligator are the only ones in a position to be the towns of Quizquiz, yet both sites seem to be too far from the 1541 channel of the Mississippi to be compatible with the narratives. In all, the archaeological situation concerning Quizquiz makes a Sunflower Landing crossing very awkward to espouse.

There are better candidates for Aquixo. Both Avenue (16-M-1) and Dupree (16-L-6) are firmly linked by ceramics to the De Soto date line. They are large sites, however, and would seem to merit greater political importance than that allotted to Aquixo in the narratives.

Investigations of the past twenty years have disclosed no new sites which would support the Commission's contention that the towns of Casqui were located on the southeastern corner of Crowley's Ridge around Helena, Arkansas. We can only reaffirm the conclusion of Phillips and others (1951: 379) that there seems to have been no substantial settlement of this district in the late prehistoric period.

Moving past Crowley's Ridge to the floodplain adjacent to the confluence of the Languille and the St. Francis, there is a cluster of sites where Swanton (1939: 251) predicted Pacaha would be found. The sites have been well summarized (Phillips et al., 1951: 380) and it can be added simply that Moore (14-N-1), Greer (13-N-17), Grant (13-N-11), Starkley (13-N-16) and Kent (13-N-4) all have the appropriate ceramics for the De Soto period.

Finally, the Commission insisted that the river of Coligua must be the Arkansas and not the White. In conformity with this identification, Quiguate was located near the junction of these rivers. Two large sites in the region, Menard (17-K-1) and Wallace (17-K-3), are certainly commensurate with the descriptions of Quiguate. Swanton (1952: 161) maintained to the end a firm conviction that Menard was, indeed, De Soto's Quiguate:

As to one archaeological site I remain heretical if that is necessary, for the remains about Menard are exactly where the great town of Quiguate should have been...

Assuming for the moment that Swanton is correct in his geography, the problem for archaeologist to decide is whether the Menard and Wallace sites were inhabited in 1541. It is clear that the main occupation was somewhat later, for remains of the early historic Quapaw of the late seventeenth century predominate (Ford, 1961). A review of collections from both sites disclosed some of the ceramic markers indicative of an earlier occupation in De Soto's time, but whether the settlement was extensive enough to constitute the "largest town in all Florida" is impossible to determine without additional excavations. For now, the possibility cannot be rejected.

After a lengthy review of the archaeological evidence for the
Sunflower Landing hypothesis, one is left with the same feeling that results from examining the Memphis and Commerce hypotheses. There were plenty of large sites along the route that seem to have been occupied in 1541, but the sequence in which they are found does not fit the narratives. In this case, there are no suitable sites where Quizquiz and Casqui should be. In short, while all three of the major hypotheses seem to fit segments of the route, none of them are convincing enough in their entirety to generate a secure conviction that "this is the trail followed by the De Soto expedition."

NEW PERSPECTIVES ON THE DE SOTO ENTRADA

At the outset of the present discussion, let it be stated explicitly that there is still no direct and conclusive evidence of De Soto at any site in the Lower Mississippi Valley. By this we mean that no sixteenth century Spanish materials have been uncovered in a closed archaeological situation. The same holds, so far as we know, for identified bones of the European swine (Sus scrofa). It is true that a handful of items, such as distinctive sheet brass bells from Mississippi and Arkansas, are so convincing as to be almost certainly survivals from the entrada. However, none of them can be associated with confidence to a specific occupational level of a specific site.

Considering the small amount of really large-scale excavation that has been done in the Mississippi Valley, it should not be surprising that positive evidence of De Soto is unavailable. It is extremely

10The distribution of the small bells of a very distinct type from those introduced later by the French and English is remarkable in the way it parallels the route of the De Soto expedition (Brown, 1971). They have been reported from the Dunn's Creek site in Putnam County, Florida (Smith, 1956: 13-15), the Citico site in Monroe County, Tennessee (Thomas, 1894: 376), the Oliver site in Coahoma County, Mississippi (Brown, 1926: 358), and the Parkin site in Cross County, Arkansas (Davis, 1966: 11). Not all of these sites are precisely on the proposed route, but they are certainly within the range of normal trading and/or pillaging. The bells at Citico and Parkin were associated with the burials of children. Spanish paintings of the sixteenth century show similar bells used on horse trappings, and it is in this capacity that they might have accompanied De Soto's army. We also know that Cortez carried such items as gifts for the natives (Diaz del Castillo, 1956: 41). However they were acquired, further evidence of their origin may be documented in Henri Joutel's account of his trip from Texas to the Arkansas Post in 1687: "...whilst we halted on the bank of a river to eat, we heard the tinkling of some small bells, which making us look about, we spied an Indian with a naked sword-blade in his hand, adorned with feathers of several colors, and two large hawks' bells, that occasioned the noise we had heard...I observed that it was a Spanish blade he had, and he took pleasure in ringing the hawks' bells" (Cox, 1922: 183-84).
difficult to isolate a "slice of time" at archaeological sites that were occupied before and after the instant in question. The few days, or even weeks, that the Spaniards spent at each town along their itinerary are reflected in a virtually invisible layer of a deposit that may have taken hundreds of years to accumulate. Instances where the army burned a town offer, perhaps, the best hope of defining a De Soto level. To further complicate the situation, by the time the Spaniards reached the Mississippi Valley they were exceedingly frugal with their metal artifacts and other possessions; the bulk of which they had lost at Mabila and Chicaça.

The above comments are not offered by way of apology, but rather as an explanation to critics who might question our indirect approach. Until a great amount of needed excavation is accomplished, all speculations on the De Soto expedition must be limited to hypotheses which best fit all historical, archaeological, and ethnographical conditions. A reappraisal of existing hypotheses failed to uncover one which meshed satisfactorily with all the necessary conditions.

During the reappraisal, a number of chronologically suitable sites were confirmed. Once these sites were plotted on a map with a reconstructed river position for 1541 (see Fig. 4), the main task was to resuffle them to find a combination that satisfied all the historical, archaeological, and ethnographical requirements. As the reconstruction progressed, one line of evidence began to support another until a unified picture emerged which we offer in the following summary.

Alibamo To Quizquiz

Linguistic and ethnographic similarities link the town of Chicaça with the historic Chickasaw who controlled a territory centered in northeastern Mississippi. In the early eighteenth century, the main village of the Chickasaw was approximately six miles northwest of the modern town of Tupelo (Swanton, 1939: 220), and this location should not be far from the Chicaça where De Soto and his army wintered in 1540-1541. Archaeological research by Jennings (1941) at Chickasaw oldfields tends to confirm the association, but the De Soto Commission settled on a location a little to the southeast in Pontotoc County, Mississippi (Swanton, 1939: 222). Either location fits our proposed route into the Mississippi Valley, and since we have no new evidence we can only conclude that both are equally acceptable.

After narrowly escaping total annihilation by the Chickasaw, the army set out from Chicaça on 26 April 1541. According to the account of Biedma, they headed in a northwesterly direction and reached the settlement of Alibamo that day. Badly needed supplies were not to be had at Alibamo, and De Soto quickly dispatched scouting parties to search for other villages. Apparently, it was known that the route immediately ahead of them was sparsely inhabited and that provisions were unlikely to be had along the way. One scouting party discovered
Reconstructed channel of the Mississippi River, from the Lower St. Francis to the Arkansas, and associated sites Ca. A.D. 1541.

Figure 4
a barricade or fort which must have been constructed at some choke point for the expressed purpose of harassing the expedition. The barricade was heavily defended, but the Spaniards broke through without excessive casualties. The incident probably influenced De Soto's decision to press on directly without finding the desired supplies.

At this point in the story, the narratives are all pervaded with an aura of intense discouragement. The Spaniards had not fared well at Mabila, they had suffered through a severe winter at Chicaña, and they had met their match on the battlefield. Logistics were in shambles, the wounded were many. Although never stated explicitly in the narratives, it is entirely realistic to speculate that the army's condition at this point in time may have prompted a shift in direction from north to west. As De Soto surely knew, to the west lay the Mississippi River and the most expedient avenue of escape to the sea. Temporarily, at least, even the Adelantado may have had enough of Florida.

Departing Alibamo on 30 April, the army continued along a route that is difficult to reconstruct. By all accounts, the journey was through an unpopulated, or at least sparsely populated, region and the going was not always easy. Elvas provides, perhaps, the best description of the terrain. He says that they:

...marched seven days through a wilderness, having many pondy places, with thick forests, fordable, however, on horseback except for some basins or lakes that were swum.

This lush, waterlogged terrain perfectly describes the stagnant lakes and backswamps characteristic of a route through the Yazoo Basin. Biedma's account says only that the journey was through a "wilderness," but Ranjel's version adds a new twist:

Saturday, the last of April (1541), the army set out from the place of the barricade and marched nine days through a deserted country and by a rough way, mountainous and swampy, until May 8, when they came to the first village of Quizqui...

The word "mountainous" has been used by proponents of the Memphis hypothesis to support a more northerly route, but as Swanton (1939: 235) has shown the confusion results from a mistranslation of the term monte which Ranjel consistently uses to mean forests. The only adjective that Garcilaso uses to describe the region is "unpopulated".

There is little agreement in the narratives regarding the length of the journey through the wilderness. Garcilaso says that it took only three days¹¹; Elvas logs seven; Biedma allows eight; and Ranjel

¹¹Garcilaso, as noted previously, is an unreliable source for statistical information.
records nine. Considering the wounded and the difficulty of the terrain, the time span of eight to nine days is the most sensible for a journey of roughly ninety miles or more. In accordance with our policy of giving priority to Ranjel in most cases pertaining to times and distances, we will suppose that the journey took nine days.

Archaeological evidence, or rather lack of it, confirms that the northeastern part of the Yazoo Basin was sparsely inhabited during the sixteenth century A.D. The most likely interpretation of the narrative descriptions is that the army crossed the Yazoo Basin in a direction generally west from Alibamo. Remembering that it can be very hot and humid in the region by early May, it is unlikely that the large body of men, horses and pigs would have traveled far from ample water resources. Therefore, the route probably took them down the Yocona River or the Little Tallahatchie, past the junction of the Tallahatchie and the Coldwater rivers, and on across the floodplain to the vicinity of Clarkdale, Mississippi. As far as archaeological sites are known, this route would have taken them through completely unpopulated terrain—the only route that would have done so. This route also coincides with the known eighteenth century trail between the Chickasaw villages and the Mississippi (see De l'Isle 1701 and Crenay 1733 maps), and may be supposed to have had greater antiquity.

Before examining De Soto's entry into Quizquiz, it would do well to pause and review one alternative route from the south which also has some historical precedence. Guillaume De l'Isle's Carte de la Louisiane of 1718 (Cumming, 1958: plate 47) shows the De Soto route cutting across from Mobile Bay to the Pascagoula/Leaf river system which it crosses at a latitude above the present Hattiesburg, Mississippi. The route continues north to the headwaters of this river system where De l'Isle shows the towns of Chicacha and Alibamo in what would be the central portion of the state. It is important to note that this Chicacha is not the main Chickasaw town which he also shows in its correct location. From Alibamo, De l'Isle brings the army west across the Yazoo River and then northwest to Pointe d'Oziers which Swanton identifies with Sunflower Landing.

Two features of the De l'Isle map are extremely tantalizing. First, the route from Chicacha would be generally north as Garcilaso says it was. Secondly, such a location for Chicacha and Alibamo accords well with the known history of the later Tunica who La Harpe claims were driven in 1706 from their territory in the lower Yazoo region by an alliance of Chickasaw and Alibamu (Swanton, 1911: 311). The alliance is very plausible when one envisions not the cooperation of major tribes centered around Tupelo, Mississippi and Montgomery, Alabama, but rather the cooperation of their affiliated villages in central Mississippi.

The De l'Isle route from the south encounters, however, the same difficulty as Swanton's route to Sunflower Landing. The army would have had to pass a solid line of occupied sites from Stokes Bayou and
Mount Olive on the west to Myer and Spendthrift on the east (see Fig. 3). The unlikelihood of passing these large sites unnoticed is our main reason for rejecting the De L'Isle route and accepting the established location of Chicago. After evaluation of these and other possibilities, we still feel that the route along the Yocona or Little Tallahatchie rivers was De Soto's most probable approach into the Lower Mississippi Valley.

The Location Of Quizquiz

The narratives all agree that De Soto entered the province of Quizquiz undetected, but they are not so consistent in detailing the towns that were visited. It is necessary, then, to review the narratives before attempting to sort out the discrepancies. Ranjel is probably the most complete in providing details of Quizquiz geography:

(the army)... came to the first village of Quizqui, which they took by assault and captured much people and clothes; but the Governor promptly restored them to liberty and had everything restored to them for fear of war...

A league beyond this village they came upon another with abundance of corn, and soon again after another league, upon another likewise amply provisioned. There they saw the great river.

Elvas gives an account which is essentially the same as Ranjel's except that he omits mention of the middle town:

He arrived at a town of Quizquiz without being des­cried, and seized all the people before they could come out of their houses...There was little maize in the place, and the Governor moved to another town, half a league from the great river, where it was found in sufficiency.

We do pick up from Elvas the little detail that the last town of Quizquiz was half a league from the Mississippi. In his characteristically concise style, Biedma seems to contradict his companions in several important respects:

One mid-day we came upon a town called Quizquiz, and so suddenly to the inhabitants, that they were without any notice of us, the men being away at work in the maize-fields. We took more than 300 women, and the few skins and shawls they had in their houses...The town was near the banks of the River Espiritu Santo (the Mississippi).
It is not so bad that he only mentions one town, but unfortunately he mixes details of the first town which was stormed by surprise and the last town near the great river. To make matters worse, Garcilaso also talks of only one town of Quizquiz to which he attributes several of the same details that Elvas and Ranjel split between two or more towns:

...after they had journeyed for three more days through an unpopulated region and toward the north so as to flee from the sea, they came to Chisca (Quizquiz), a village lying near a great river. Now since this was the largest of all the rivers that our Spaniards discovered in Florida, they called it the Great River and never gave it any other name...

Because of their continuous war with the people of Chicaza, and because of the wilderness lying between the two provinces, the Indians of Chisca knew nothing of the coming of the Spaniards to their land and as a result were not on the lookout. So when our men caught sight of the town, they charged upon it without order, and in addition to capturing males and females of all ages, pillaged everything that they found within the place...

Resolution of discrepancies in accounts of Quizquiz is a difficult matter, although one is tempted to dodge the problem entirely and rely solely on the normally reliable Ranjel and Elvas. To do so, however, would throw out several very important details supplied by Biedma and Garcilaso. Thus, the most conservative solution is probably that of Phillips who created a composit model consisting of several villages, strung out along intervals of approximately a league, at least one of which was very near the Mississippi (Phillips et al, 1951: 368). This is the model we have employed in examining the archaeology and ethnography of the region.

If the army had followed the Yocona River or the Little Tallahatchie River to their confluence and then continued past the union of the Tallahatchie and the Coldwater, their route would have taken them through the present town of Clarksdale, Mississippi. It is here on the Sunflower River that we locate the first Quizquiz town (see Fig. 5). The modern community has grown up and obliterated the Clarksdale site (16-N-26), but fortunately some early descriptions have been preserved. Thomas (1894: 256) illustrated a plan of the Clarksdale works drawn by Col. P.W. Norris. Since it is primary evidence, it is worth quoting portions of the site description:

At Clarksdale on the Sunflower river, is a group consisting of an inclosure and six mounds... (there is) a semicircular inclosure fronting the river, the surrounding earthen wall partially obliterated by the
Figure 5. Topography and settlement pattern of Quiaquiz region ca. 1541. The army crossed the river between Montgomery (15-N-6) and Old Town (15-N-3) in the vicinity of the current Old Town Bend south of Friars Point.
The largest and most interesting of the mounds is No. 1 ...situated within the inclosure and directly on the bank of the river, so that the slope of the west side of the mound is continuous with the slope of the bank. It is rectangular in form, consisting first of a platform 5 feet high, which forms the base, projecting as a narrow terrace on all the sides except that next the river. Above this rises the mound proper, 20 feet high, 153 feet long at the base, and nearly 100 feet wide. The top is flat and level and on it now stands the village church, but formerly there stood on it a little conical mound 5 feet high and 25 feet in diameter...

The other mounds were small and conical. One aspect of the site description is remarkably parallel to part of Garcilaso's, namely the large mound at one end of the town (see Fig. 6). Assuming that Garcilaso's informant remembered details of the first town that was stormed but got the location of the town itself mixed up with the one by the Mississippi, the narrative description agrees well with that of Thomas. Garcilaso relates:

Off to one side of the town was the dwelling place of the Curaca. It was situated on a high mound which now served as a fortress. Only by means of two stairways could one ascend to this house. Here many Indians gathered while others sought refuge in a very wild forest lying between the town and the Great River.

The Clarksdale site thus fits the scanty descriptions we have of Quizquiz. But was it occupied in 1541? The best evidence to the affirmative is provided by Brown (1926: 97-100) who relates that a Mr. Henry Davis obtained from the site "...a handsome earthenware bottle 10 inches high and 7 in diameter, decorated with four sets of concentric circles in color." One can be assured that the colors were red and white, the vessel shape and decorative motif being identical to a number of Nodena Red and White bottles (see Fig. 2) which have been found at many sites in Arkansas and Mississippi which date from the De Soto period. Thomas (1894: 258) also gives a hint as to the date of the Clarksdale occupation:

Except for the better preservation of the skeletons (in Mound 7), the mode of burial and accompaniments and everything found in this mound were in all respects similar to the Old Town burials. But the pottery, of which only two entire vessels were obtained, like that from Dickerson's mound, is lighter colored and thinner than usual.
Site Plans

a. Clarksdale

b. Montgomery

c. Old Town

d. Belle Meade

e. Dupree

Reproduced from the following sources: (a) Thomas, 1894; fig. 158; (b) Thomas, 1894; plate 11; (c) Thomas, 1894; fig. 142; (d) Phillips et al., 1951; fig. 63; (e) McGimsey, 1965; fig. 1. Scales unknown.

Figure 6
As will be discussed shortly, both Dickerson (15-N-10) and Old Town (15-N-3) have ceramics compatible with a 1541 date line. Finally, one might add that the plan of the Clarksdale site is certainly that of a late Mississippian fortified town. Embankments around a site of this period are often left as the result of earth banked against a palisade for reinforcement. A clue that the Quizquiz capital was indeed fortified is contained in Garcilaso's rationalization of why the Spaniards thought it prudent to maintain peaceful relations with the inhabitants:

Again, they perceived that the site of the town, both inside and outside, was very favorable to the Indians and unfavorable to themselves...

The phrase "inside and outside" does not seem to connote an open village.

Moving about a league northwest of Clarksdale there is a site on the Little Sunflower River which could very well be the second town mentioned by Ranjel. The Rufus Davis site (16-N-4) consists of a large flat-topped mound, approximately 150 by 160 feet in dimension, and a smaller dome-shaped mound. A surrounding village area is marked by fragments of daub and by potsherds (Brown, 1926: 106). Unfortunately, a ceramic sample from Rufus Davis was unavailable for re-evaluation, but the original survey report (Phillips et al, 1951: Fig. 19) lists a trace of shell-tempered ware in addition to varieties suggesting an earlier Baytown occupation. In sum, Rufus Davis seems to satisfy the conditions dictated by the narratives. If occupied in 1541, it was a small village and not the type normally of interest to De Soto. For this reason, Ranjel says they passed it by after collecting the maize to be found there, and the other narratives forget it entirely.

In accordance with Ranjel, another league past Rufus Davis and close to Fisk's reconstructed channel 15 of the Mississippi River is a major site which Phillips rightly prophesied must be reckoned with in any solution of the De Soto problem (Phillips et al, 1951: 373). Montgomery (15-N-6) is part of an extensive cluster of mounds and village remains which Holmes illustrated as the Carson Group (Thomas, 1894: plate 11). The Carson Group has been divided into three sites: Montgomery, Stovall (15-N-7), and Carson (15-N-8). It is Montgomery, of course, that concerns us here as the Quizquiz town by the great river.

The Montgomery site consists of a rectangular embankment enclosing a dominant mound (see Fig. 6), quite similar in construction to the one at Clarksdale, and a number of smaller mounds and house sites arranged about a plaza. It is shown as "Inclosure A" on the Holmes site plan. Thomas (1894: 253) provides the following description:

The inclosure fronts west for a distance of 738 feet on a cypress swamp, probably an open bayou or one channel of the Mississippi when these works were constructed. It is in the form of a parallelogram, the wall on three sides measuring 1,173 feet long, and embracing an area of about 5 acres. This wall is from 15 to 30 feet wide at the base, and from 3 to 5 feet
high. A ditch is distinctly traceable along the whole length of the outside, but it is not exhibited on the plate.

Within this area, a little northwest of the center, is a circular mound (a), 192 feet in diameter at the base, 15 feet high, and 66 feet across the nearly flat top. There appears to have been originally a platform some 5 or 6 feet high, on which the mound proper was built.

In all respects, Montgomery is a typical fortified Mississippian town, the kind with which the Spaniards were quite familiar by this stage of their travels.

If Montgomery satisfies well enough the description of the Quizquiz town in the narratives, it does even better ceramically. Collections from Montgomery show a full complement of all the ceramics markers used to define a De Soto date line.

Biedma says that the Quizquiz town on the banks of the River Espiritu Santo was "...with many towns about there, tributary to a lord of Pacaha, famed throughout all the land." There is archaeological evidence that Biedma's "many towns" refers to more than just the Clarksdale, Rufus Davis and Montgomery sites which appear to have been along the direct route of the army. Collectors have located a very rich cemetery, and presumably an associated village, less than a league southwest of Montgomery. The site, designated Humber (15-N-12), has yielded literally dozens of whole vessels which are extremely similar to the finest vessels from Arkansas representing the late prehistoric period. The vessels actually provide the best collection available for defining the De Soto date line ceramically in the Quizquiz region. Many of these vessels are presently on display at the Winterville Mounds Museum near Greenville, Mississippi.

Humber is far from the only additional site in the region with ceramics similar to Montgomery. East of Horseshoe Lake and some two leagues northeast of Montgomery is another major mound site with virtually identical ceramics. The site is Parchman (15-N-5). It has a well-defined plaza arrangement dominated by a large platform mound some twenty feet in height (Phillips et al., 1951: 372). Between Parchman and Montgomery are two smaller sites, Craig (15-N-11) and Dickerson (15-N-10), which were probably occupied at the same time although available collections from these sites do not suffice to confirm the association. Finally, another league northeast of Parchman is the Salomon site (15-O-1) which has an even larger platform mound flanked by lesser mounds. Collections from Salomon seem late enough to date from the 1541 period, but the assemblage is not an exact copy of that at Montgomery and Parchman.

To summarize, we feel that there is good reason to hypothesize that De Soto passed through the sites of Clarksdale, Rufus Davis and
Montgomery. In addition to other large sites in the region, they seem to date securely within the De Soto period. They correspond to the three towns of Quizquiz mentioned by Ranjel.

Before turning to the location of the river crossing, a few comments can be made which may link the towns of Quizquiz with the ethnography of the Tunica. In discussing the army's approach into Quizquiz, we included an excerpt from the Biedma narrative which mentioned that the Spaniards encountered little resistance because the men were away working in the maize fields. The historic Tunica were well known for their heavy reliance on agriculture. In fact, La Source exaggerated that they lived entirely on maize and spent all their time in the fields to the exclusion of hunting (Swanton 1911: 317). Even though La Source overstates the case, it seems a fact that the Tunica were fully agricultural and that the normal division of labor in the Mississippi Valley did not apply to them. Gravier adds the following:

The men do here what peasants do in France; they cultivate and dig the earth, plant and harvest the crops, cut the wood and bring it to the cabin, dress the deer and buffalo skins when they have any. The women do only indoor work, make the earthen pots and their clothes (Swanton 1911: 317).

The agricultural duties of Tunica men is hardly enough to link the tribe with the Quizquiz of De Soto. However, there is one other bit of evidence to support the association:

Chickasaw and Choctaw tradition places 'Tunica oldfields' on the Mississippi river near Friar point, not many miles below the present Helena, Ark., which would indicate that they had formerly lived in the neighborhood (Swanton 1911: 307).

Friars Point refers to land in a bend of an old channel of the Mississippi that is now occupied by Horseshoe Lake. It is precisely adjacent to the Montgomery site. Archaeology thus suggests that this is indeed a case where tradition has preserved a historical fact.

The River Crossing

The narratives, excluding Garcilaso's, agree that the crossing was not made from the Quizquiz town itself, but rather from a camp a short distance away. Biedma says that they left Quizquiz and "...went to

The Garcilaso account differs so much from the others on this point that we feel there must be some error in his information. He burdens the Spaniards with a four day march upriver in search of a suitable place to cross, supposedly because of steep cliffs on both sides of the river which could not be negotiated by the army. This does not mesh with the topography of the Friars Point region or anywhere else as far north as Memphis and beyond. Strange as it may seem, his informant must have confused the terrain along the Mississippi with some other crossing—notably the approach into Chicaçu which fits the topography perfectly. A clue to this possibility can be seen in Garcilaso's name for Quizquiz (Chisca) which suggests confusion with his name for Chicaçu (Chicaza).
encamp by the riverside, to put ourselves in order for crossing." Elvas recalls the same facts but with more detail:

He went to look at the river, and saw that near it there was much timber of which piraguas might be made, and a good situation in which the camp might be placed. He directly moved, built houses, and settled on a plain a cross-bow-shot from the water, bringing together there all the maize of the towns behind, that at once they might go to work and cut down trees for sawing out planks to build the barges.

Ranjel also adds a detail in confirming the other's stories:

Saturday, May 21, the force went along to a plain between the river and a small village, and set up quarters and began to build four barges to cross over to the other side.

The detail added by Ranjel is the small village, apparently one not previously mentioned, that was close to the camp. Unfortunately, the identification of this village cannot be established at the present time.

The army camped for a month on the east bank of the Mississippi while constructing the barges necessary to effect a river crossing. In discussing the events which transpired during this period, the narratives reflect a profound change in the mood of the expedition. After initial difficulties, the stay at Quizquiz was generally a peaceful interlude. The army was resupplied and rested—both factors providing a psychological lift after the discouragements at Chicaça. But more important, the Spaniards found a very formidable force opposing them:

The next day the Cacique arrived, with two hundred canoes filled with men, having weapons. They were painted with ochre, wearing great bunches of white and other plumes of many colors, having feathered shields in their hands, with which they sheltered the oarsmen on either side, the warriors standing erect from bow to stern, holding bows and arrows. The barge in which the Cacique came had an awning at the poop, under which he sat; and the like had the barges of the other chiefs...

Elvas' famous description, from which the above excerpt was selected, names the cacique as Aquixo but Ranjel adds that the men were subject to a still more powerful lord of Pacaha. The Spaniards were obviously impressed by the military discipline and grandeur of the men in the canoes, and Elvas pays them a high compliment:
These were fine-looking men, very large and well formed; and what with the awnings, the plumes, and the shields, the pennons, and the number of people in the fleet, it appeared like a famous armada...

In short, what the army witnessed was a vigorous Mississippian culture representative of the highest level of achievement by aboriginal peoples in native North America. Rather than instilling them with fear, the "armada" revived the Spaniards' hopes of finding their fortunes in the New World. From this point on, the narratives suggest renewed optimism and spirit for the duration of the army's first stay in the Lower Mississippi Valley. If there were any previous thought of retreat down the great river it was forgotten for the time being.

Notwithstanding the show of force, the passage was left unchallenged and early on the morning of 18 June 1541 the whole force crossed the Mississippi without incident. The precise point where De Soto landed remains, and no doubt always will remain, unknown. However, the location can be narrowed considerably to somewhere in the vicinity of Friars Point, most probably opposite the Montgomery site. In addition to correlating with the archaeology and the river geology, a crossing at Friars Point has cartographic support. A French and English map engraved by Tardieu in 1796 (see Fig. 7) shows the river using Friars Bend, and a label at this location marks the place where De Soto first crossed the Mississippi River. The "ancient village of the Arkansas" matches the position of the Parchman site, one of the Quizquiz villages at the time of De Soto.

A number of other maps, notably those of De L'Isle (1718), Mitchell (1755) and Ross (1765), document the crossing near an easterly meander labeled Pointe d'Oziërs. A mature channel 15 easterly meander did indeed exist just below Friars Point (see Fig. 5). Assuming that Fisk's reconstruction is correct, the meander was cut off within a century after De Soto's crossing, and by the time of the eighteenth century cartographers a new easterly meander was forming even further south near the location of the present Sunflower Bend. If legend preserved the fact that De Soto crossed near a prominent easterly meander, the approximate location of which was also known, it follows that the early map makers would settle on the closest similar feature they could find—Pointe d'Oziërs, or an early version of Sunflower Bend. Such an explanation accords well with the known evolution of Mississippi Valley cartography and with river hydraulics. Further, a crossing somewhere around Friars Point is the only one examined to date which is fully compatible with the archaeology, especially the evidence in Arkansas to which we now shall turn.

The Location Of Aquixo

Immediately upon crossing the Mississippi, Biedma asserts the army "...found some good towns on the other side..." He fails to mention
English version of the Tardieu 1796 map. Friars Point is identified as the location of De Soto's discovery of the Mississippi River. Note, also, the emphasis upon the swampy condition of the land on the opposite side of the river, which coincides exactly with the description in the De Soto narratives.

Figure 7
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them by name, but the omission is corrected by Ranjel:

And on Saturday, June 18, the whole force crossed this great river in the four barges... Soon, on Sunday, they came to a village of Aquixo... Tuesday, June 21, they went from there and passed by the settlement of Aquixo, which is very beautiful, or beautifully situated.

Even more detail is supplied by Elvas who thus becomes the primary authority for the province:

The Rio Grande (the Mississippi) being crossed, the Governor marched a league and a half, to a large town of Aquixo, which was abandoned before his arrival... The Governor slept at the town one night, and the day following he went in quest of a province called Pacaha... He passed through large towns in Aquixo, which the people had left for fear of the Christians. From some Indians that were taken, he heard that three days' journey thence resided a great cacique, called Casqui.

Garcilaso does not mention Aquixo at all. This could be because his primary informant, Silvestre, was part of the force which Elvas says was detailed to take the barges and equipment upstream while the main body of the army marched overland.

The Bureau of American Ethnology mound survey directed by Thomas in the late nineteenth century located an ideal candidate for Aquixo. The site, Old Town (15-N-3), is located between a very old oxbow lake of the same name and Fisk's channel 15. The Village plan, including construction details of the earthworks and the primary pyramidal mound, is remarkably parallel to those at Montgomery and Clarksdale:

The works... consist of an inclosing wall surrounding a space somewhat in the form of a quadrant of a circle; a large truncated pyramidal mound with terrace... and other smaller conical or oval mounds and numerous saucer-shaped house sites... According to local tradition (the wall and inclosure) were built by Moscoso and the remnant of De Soto's army while preparing their brigantines for the descent of the Mississippi.

Mound No. 3 in the large inclosure is a truncated pyramid, nearly square, 96 feet long by 86 in width at the base; the first or lower platform is 4 feet high, and forms a terrace 36 feet wide on two connecting sides of the mound proper; this rises 8 feet above this terrace, and is 50 by 60 feet at its base and 20 by 30 feet on the flat top (Thomas, 1894: 234-35)
Based on site plans (see Fig. 6), Aquixo would seem to be closely related culturally to the Quizquiz towns, although the narratives claim that both provinces were politically dominated by Pacaha.

Ceramics collected from Old Town and the adjacent Buie site (15-N-4) further support cultural ties between Aquixo and Quizquiz. They match the assemblage at Montgomery and are thus compatible with a 1541 date line. The one vessel which Thomas (1894: Fig. 143) illustrates from Old Town is a variant form of hooded bottle with a human effigy head. It probably can be classed as Old Town Red and, like the one vessel from Clarksdale, is extremely diagnostic of the late prehistoric period.

If De Soto crossed the Mississippi just below Friars Point and somewhere west of Montgomery, the league and a half allotted by Elvas to reach the first village of Aquixo is acceptable. Several leagues farther upstream, the Ellis (15-N-2) and Fitzhugh (15-N-1) sites may represent other Aquixo villages. Small collections from both sites contain a trace of the 1541 markers.

Before following the army to the province of Casqui, a few words can be added to elaborate on Thomas' statement that local tradition ascribed the Old Town works to De Soto. Also speculating on the location where the army built the vessels for their final descent of the river, a certain Col. Dupre comments as follows:

> When I learned that Old Town Lake, below Helena, had been the channel of the Mississippi, I went to see it, with Irving's 'Conquest of Florida,' in my hand. I found that the embers and calcined earth of blacksmith's forges, old musket barrels, fragments of saddle-trees and oxidized bullets had there been picked up, and the cruciform handle of a dagger with a cornelian in it (Claiborne, 1880: 11).

> Although our study contradicts the idea that the Spaniards camped by Old Town Lake on their descent of the river, it is somewhat comforting to find that folk history supports the presence of De Soto in this locality. We do not mean, however, to overemphasize the importance of such indirect and questionable evidence.

The Province Of Casqui

The narratives are not consistent concerning the exact chronology of the eventful period between 18 June when the Mississippi was crossed and 29 June when the army reached its destination of Pacaha. However, there is no major discrepancy concerning the amount of time consumed over-all or the general sequence of events. The army spent some time at Aquixo—probably as long as it took to dismantle the barges to conserve the iron nails—and then set out to the north. After negotiating "the worst tract for swamps and water that they had found in all Florida,"
the Spaniards reached high ground and the province of Casqui. Eventually, they came to the capital of Casqui where they met the cacique who became their guide and ally for the remainder of their journey to Pacaha.

Since archaeology pertaining to the De Soto period is virtually non-existent between Old Town and the northern side of Crowley's Ridge, we can do little more than speculate on this portion of the route. In accordance with our established policy, we will follow Ranjel's chronology (see Table 2) which is the most specific and closely paralleled by the Elvas narrative. For descriptive details along the way, we will rely more on the fuller accounts of Elvas and Garcilaso.

Had the Spaniards followed the Mississippi north from Old Town, their route would have been along a series of natural levees which should have presented no difficulties. Since the narratives all mention a terrible swamp, one can only conclude that the army cut around behind Old Town Lake and got caught up in the backswamps which form the eastern margin of the Big Creek/Lick Creek drainage. Such a route would lead to drier ground at the southwestern corner of Crowley's Ridge as the De Soto Commission concluded (Swanton, 1939: 249).

Archaeological sites dating from the De Soto period are unknown in the backswamp region, and the situation is similar on Crowley's Ridge near Helena, Arkansas (see Phillips et al, 1951: 377-79). However, it is from Crowley's Ridge that we can again pick up the trail of the De Soto entrada. The vivid mental picture conjured up by the following passage from Garcilaso is not one likely to be fabricated without a factual basis:

Four more days they marched through uninhabited regions, and on the fifth came to the summit of some high hills where they beheld a town of four hundred houses, situated beside a river larger than the Guadalquivir at Cordova. Along the entire bank of that river and throughout the vicinity, there were numerous fields of corn and a great number of fruit trees, all of which proved the land to be fertile.

There are no large rivers on this portion of Crowley's Ridge, but the St. Francis River flows close to its base on the northeastern side. We would interpret Garcilaso's description to mean that the Spaniards looked down from Crowley's Ridge and saw the St. Francis and the first Casqui town. In fact, the Moore site (14-N-1) lies in an ideal position to provide such a view. The site consists of a small house mound, approximately four feet in height, surrounded by a rich village area.

13The St. Francis fits admirably the analogy to the Guadalquivir at Cordoba, Spain in terms of size, current and turbidity (Gwynne, 1912: 213).
<table>
<thead>
<tr>
<th>Date</th>
<th>Event Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>SAT 18 JUNE</td>
<td>The army crossed the Mississippi River.</td>
</tr>
<tr>
<td>SUN 19 JUNE</td>
<td>The first Aquixo village was entered.</td>
</tr>
<tr>
<td>MON 20 JUNE</td>
<td>The army remained in the first Aquixo village, probably while the barges were</td>
</tr>
<tr>
<td></td>
<td>being dismantled.</td>
</tr>
<tr>
<td>TUE 21 JUNE</td>
<td>The second Aquixo village was passed.</td>
</tr>
<tr>
<td>WED 22 JUNE</td>
<td>The army crossed a very difficult swamp.</td>
</tr>
<tr>
<td>THUR 23 JUNE</td>
<td>The province of Casqui was entered and</td>
</tr>
<tr>
<td></td>
<td>several small villages were passed.</td>
</tr>
<tr>
<td>FRI 24 JUNE</td>
<td>De Soto reached the Casqui capital.</td>
</tr>
<tr>
<td>SAT 25 JUNE</td>
<td>The Spaniards erected a huge cross on a</td>
</tr>
<tr>
<td></td>
<td>mound in the Casqui capital.</td>
</tr>
<tr>
<td>SUN 26 JUNE</td>
<td>The army set out for Pacaha and spent</td>
</tr>
<tr>
<td></td>
<td>the night in another Casqui village.</td>
</tr>
<tr>
<td>MON 27 JUNE</td>
<td>The army crossed a swamp or stream separating the provinces of Casqui and Paca-</td>
</tr>
<tr>
<td></td>
<td>ha.</td>
</tr>
<tr>
<td>TUE 28 JUNE</td>
<td>No mention of events that transpired.</td>
</tr>
<tr>
<td></td>
<td>The Spaniards probably rested and prepared for a possible battle at Pacaha.</td>
</tr>
<tr>
<td>WED 29 JUNE</td>
<td>The army reached the main town of Pacaha.</td>
</tr>
</tbody>
</table>
Ceramics from Moore can be assigned confidently to the De Soto period. If not the exact Casqui town referred to in Garcilaso's description, Moore must at least have been one of the small villages Ranjel says the army passed their first day beyond the swamps and into Casqui territory.

Ranjel allows one day's march from the first small Casqui villages, of which Moore might be one, to the capital. The allotted time matches perfectly with the distance from Moore to a group of sites situated just north of the junction of the St. Francis with the Languille. Starkley (13-N-16), Grant (13-N-11) and Greer (13-N-17) all have single large pyramidal mounds surrounded by village areas. Ceramics from Starkley and Grant show a good range of the 1541 marker varieties, and it is likely that a similar assemblage would be identified from Greer were a collection available for study. Any one of these three sites is suitable for the Casqui capital where De Soto erected a large cross atop a mound.

The Casqui towns suggest a settlement pattern that differs somewhat from that found in Quizquiz and Aquixo. Instead of large fortified sites like Montgomery and Old Town, there seems to be more numerous but smaller villages. The narratives all mention a number of Casqui towns, Garcilaso being the most specific as to size:

All of the land was very fertile and inhabited, though the villages were small and contained no more than fifteen, twenty, thirty and forty houses.

The pattern continues northeast from the Starkley group along the natural levees of the Mississippi. Similar sites which are ceramically dated to the 1541 period and can be considered as Casqui towns include Red Oak (13-N-9), Davis (13-N-5), Kent (13-N-4), Murdock (13-N-12), Barrett (13-O-1), and Nickel (13-N-15). Except for Red Oak which was a small village at best, all of the sites have a moderately large pyramidal mound, sometimes surrounded by several smaller house mounds, and a village area.

Leaving the Casqui capital, Ranjel reports that the army passed several villages (possibly Red Oak and Davis) and spent the night in another Casqui town. The most likely candidates for this town are the adjacent Kent and Murdock sites which Moore (1911: 406-10) describes as Kent Place. Moore's illustrated vessels from Kent Place, including three Nodena Red and White bottles, supplement the survey collections and are diagnostic of a De Soto date line.

The second day past the Casqui capital the Spaniards arrived at a swamp or lake which according to Elvas was "of great depth and swiftness of current." It marked the border between the provinces of Casqui and Pacaha. The only possible water barrier in the region in 1541 was a relic channel 11 tributary of the Mississippi which is now called Crooked Bayou. Council Lake, which at present also would impede the proposed route, is a remnant of a recent meander of the Mississippi and...
was not in existence at De Soto's time. The swamp (more likely a swollen stream) was crossed on a bridge which Casqui had ordered built for the Spaniards. The Barrett site, apparently occupied in 1541, is located quite near the Crooked Bayou channel and, therefore, possibly served as a frontier settlement.

The ethnic identification of the Casqui towns is linked to a poorly known tribe, the Casquinampo (Swanton, 1939: 52) or Kaskinampo (Swanton, 1946: 143-44), who formerly inhabited Crowley's Ridge. The linguistic similarity is certainly clear, but unfortunately there is very little ethnographic evidence with which to evaluate the association.

The Location Of Pacaha

The narratives leave little doubt that the dominant political power in the portion of the Mississippi Valley under consideration was located in the province of Pacaha. Both Quizquiz and Aquixo are described as being under the hegemony of Pacaha, and Casqui apparently was under constant pressure to preserve its independence. Thus, the Spaniards had great expectations as they marched towards the renowned capital with their Casqui allies paralleling their advance by river.

By this point in their adventures, the army was at last becoming familiar with native American cultures. One result of the better understanding seems to have been improved powers of observation, for the descriptions of Pacaha are far more complete than those of previous towns they had visited. The fuller details provide greater potential for comparing the mentioned towns with archaeological sites. Thus, we will review the Pacaha accounts quite closely.

Biedma elaborates more than usual on the events that took place at Pacaha, but he remains frugal in his site description:

We travelled two days, and then discovered the town on a plain, well fenced about, and surrounded by a water-ditch made by hand.

From Ranjel, we learn more about the town's fortifications:

On Wednesday they came to the village of Pacaha, a village and lord of wide repute and highly thought of in that country. This town was a very good one, thoroughly well stockaded; and the walls were furnished with towers and a ditch round about, for the most part full of water which flows in by a canal from the river...

Elvas confirms the other accounts and adds a few details concerning the surrounding towns:

...the Governor entered Pacaha, and took quarters in
the town where the Cacique was accustomed to reside. It was enclosed and very large. In the towers and the palisade were many loopholes. There was much dry maize, and the new was in great quantity, throughout the fields. At a distance of half a league to a league off were large towns, all of them surrounded with stockades.

In one nearby town, a quarter league from the capital, Elvas later says the cavalry captured many Indians who were delivered to the Casqui allies.

The descriptive powers of Garcilaso are nowhere more obvious than in his discussion of Pacaha. The mental picture he generates corresponds so closely to what is known of St. Francis type villages that we include part of his description in toto:

This town was the frontier and defense of the entire province against that of Casquin and in consequence was fortified in a manner I shall describe. It consisted of five hundred large and good houses, which were located on a site somewhat loftier and more eminent than its surroundings, and it had been turned into almost an island by means of a man-made ditch or moat ten or twelve fathoms deep and in many places fifty feet wide, but never less than forty. The moat was filled from the previously mentioned Great River, which flowed three leagues above the town; and water was drawn into it by human effort through an open canal connecting it with the river, a canal which was three fathoms deep and so wide that two large canoes went down and came up it side-by-side without the oars of the one touching those of the other. Now this moat, of the width we have said, lay on only three sides of the town, for it was as yet incomplete. But the fourth side was fenced off by a very strong wall of thick wooden boards that were thrust into the ground, wedged together, crossed, tied, and then plastered with mud tamped with straw...

The type of town which Garcilaso describes—one that is elevated above the surrounding terrain, heavily fortified and containing many houses—is not the same type found in Quizquiz, Aquixo or Casqui. With a few exceptions, the settlement type is distributed mainly in the St. Francis Basin northward from Pacaha, and it is related to Mississippian settlements even farther up the great river. The Pacaha town plan also differs from those discussed for the other provinces in having a greater number of pyramidal mounds arranged around a more clearly defined plaza.

Situated on an old Mississippi levee a little over one league past our hypothetical position of the bridge on Crooked Bayou is a large St.
Francis type village which must be considered a prime candidate for the Pacaha capital. Indeed, the Belle Meade site (13-0-5) matches Garcilaso's description of Pacaha so precisely that we feel it represents the most convincing identification along our proposed route. As noted in the original survey report, Belle Meade is one of the few sites in the district which shows evidence of being palisaded (Phillips et al., 1951: 386). The site is elevated some five to six feet as a result of debris accumulation within the walls, thus fitting perfectly Garcilaso's statement that the houses were on a site "somewhat loftier and more eminent than its surroundings". There is a stepped pyramidal mound, ten feet in height, and five lesser mounds arranged about a plaza 275 feet in length (see Fig. 6). Most striking of all, there are artificial ditches on three sides of the raised area. Of all the known sites in the survey area, there is none that matches the descriptions of Pacaha better than Belle Meade. Further, it is precisely where one would expect it to be in relation to the other provinces we have identified.

The location of Belle Meade also accords well with the narrative descriptions of the surrounding area. Fisk's reconstruction of the Mississippi River positions (1944: sheet 22, plate 5) shows a small slough running north from the site to join Fish Bayou, a channel 14 tributary of the Mississippi (see Fig. 8). The slough may well represent the remains of the canal that connected the moat to the Mississippi via Fish Bayou which was almost certainly an open stream in De Soto's day. On a larger scale, Belle Meade is quite close to the 1541 channel of the Mississippi, the "River of Pacaha".

Just south of Belle Meade is another site, Beck (13-0-7), which corresponds well with the town a quarter league from the capital where Elvas says the cavalry caught a large number of Indians. There are eight mounds at Beck, and the field notes of Phillips and Ford, dated March 1940, describe the site as "a replica of nearby 13-0-5 on a smaller scale".

Before continuing with other identifications around Pacaha, a few words are appropriate concerning the pottery from Belle Meade and Beck. In our initial review of ceramic collections, and long before the present hypothesis began to gel, we noted the material from Belle Meade and Beck as one of the best samples of all the 1541 markers (e.g. see Fig. 2). The ceramics had more red slipped and painted wares and more effigy modes than in most of the other collections. In general, the Belle Meade material was fancier and more varied than that from Starkley, Montgomery and the other sites in the regions we have since defined as the provinces of Casqui, Aquixo and Quizquiz. The Belle Meade pottery, however, compares very closely with collections from the Hollywood and Commerce sites on the opposite side of the Mississippi and we would thus extend the province of Pacaha in that direction.

Returning to the De Soto story, and to the narratives, the army entered the capital of Pacaha unopposed for the cacique and most of his people had fled to a fortified island in the great river. Biedma describes the situation as follows:
Topography and settlement pattern of Pacaha region ca. 1541.

Figure 8

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Hastening on as fast as possible (to Pacaha), we came near and halted, not daring to enter there; but going about on one side and the other, and discovering that many people were escaping, we assailed and entered the town, meeting no opposition. We took only a few people, for nearly all had fled, without, however, being able to carry off the little they possessed.

Garcilaso enlarges the description to explain where the inhabitants had fled:

The Cacique Capaha was within the town when his enemies, the Casquins, hove in sight; but feeling that his own forces were too few and unprepared to resist their adversaries, he gave way, and before they came into the place, entered one of the canoes which he kept in the moat and went out through the canal to the Great River to take refuge on a well-fortified island which he held there. Those of his people in the town who were able to obtain canoes followed their lord, and those who could not fled to the nearby forest.

Elvas also mentions the retreat to "an islet between two arms of the river" and describes the ensuing battle which took place there—one which the Spaniards never really won.

To complete the Pacaha investigation, then, it is necessary to look for an island in the Mississippi somewhere near our hypothesized location of the capital. We realize that there are few things on earth as ephemeral as an island in the Mississippi River, but such features can be preserved under certain circumstances—such as when the island existed within a mature meander loop that was cut off. Strengthening the identification of Belle Meade with Pacaha, and to our amazement, exactly such a situation happens to apply in this case. As can be seen in Figure 8, channel 16 of the Mississippi meanders westerly to a point about one mile east of Belle Meade.14 Within the meander loop, a channel 16 stream that is now Goose Lake divides the river to form a large

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14 Those who remember our discussion of the hydrography in the Quiz-quiz region will note that we selected Fisk's channel 15 as the most probable course of the Mississippi circa 1541 in that locale. Nevertheless, we do not hesitate to switch to channel 16 for the Pacaha region. It is not unusual for discrepancies to exist in the reconstruction between segments of the river which are over fifty airline miles apart as in this case. Further, the character of the river in channels 15 and 16 is the same for the meander opposite Belle Meade, the only difference being the Goose Lake diversion which forms the island. Considering that the dating of Fisk's channels is merely an estimate anyhow, either channel 15 or 16 could apply to the De Soto period. Our evidence favors channel 16 in this instance.
island. If one were to test the area just south of Goose Lake, we have every confidence that the remains of Pacaha's island stronghold would be found. In short, everything in the vicinity of Belle Meade satisfies the requirements of the rather complete descriptions of Pacaha.

The ethnographic identification of the Pacaha group constitutes a knotty problem for which we can offer no easy solution. At one time, Swanton (1911: 186) favored identification with the Quapaw, but by the writing of the De Soto Commission report his ideas had changed:

It has been thought that the formidable Pacaha nation was identical with the Quapaw of later history who were, indeed, found in that approximate location... While this identification is still not beyond the range of possibility, the glimpses we get of the culture of Pacaha, such as the presence of a temple, seem to ally them with the lower Mississippi tribes and Swanton is inclined to recognize in them a branch of the Tunica Indians although they may have been affiliated with the Natchez. (Swanton, 1939: 51-2)

After carefully reviewing the seventeenth century French accounts of the Quapaw, Phillips again evaluated the possible Pacaha/Quapaw connection:

If they (the Quapaw) came from the Lower Ohio they could have brought with them an advanced Mississippi type of culture. Specifically, they could have brought rectangular temple mounds and the bastioned stockade such as Garcilaso described at Pacaha. Granting too, that they may have been a numerically strong and warlike group at this time, is it inconceivable that De Soto found them just at the height of their power when they had brought their softer neighbors to the south under an uneasy and probably short-lived rule? Yes, it is inconceivable somehow. (Phillips et al, 1951: 420)

To support his decision, Phillips listed a number of discontinuities between the sixteenth century culture of the region as described by the Spaniards and that which the French found some 130 years later.

Despite the similar conclusions of Swanton and Phillips, we lean toward the original view that Pacaha does represent part of the Quapaw movement down the Mississippi Valley which ended in a seventeenth century location near the mouth of the Arkansas River. To begin with, it is extremely difficult to explain away the striking similarity between

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15The name Quapaw is used synonymously with the name Arkansas. See Phillips et al, 1951: 394 for a discussion concerning the derivation and usage of both names.
Garcilaso's name for Pacaha (Capaha) and La Metairie's name for the seventeenth century Quapaw village of Kappa (Kapaha). La Metairie, a notary who on a document dated 9 April 1682 recorded La Salle's own verbal account of the taking possession of Louisiana (Cox, 1922: 163-4), indicates further that the chief of the village was called Capaha (Phillips et al, 1951: 402).

Supplementing the linguistic parallels, archaeological and cartographic evidence can be used to formulate a hypothesis that there was indeed a population movement down the Mississippi Valley which began prior to De Soto's entrada. As noted previously, the main occupation at the large St. Francis type villages of the Parkin group, on ceramic evidence, seems to predate the De Soto period. The same type of bastioned town is next found to the south at Belle Meade with ceramics compatible with the 1541 date line. The eighteenth century maps of Ross (1765) and Tardieu (engraved 1796) show an "ancient village of the Arkansas" near Friars Point—about half way from Belle Meade to the mouth of the Arkansas River—perhaps marking another advance which took place between the time of De Soto and the first French explorations into the Mississippi Valley. There is considerable continuity in ceramics and settlement pattern between adjacent segments of the hypothetical movement, and approximately 250 years of cultural change must be considered in assessing the discontinuity between the Parkin and Quapaw phases which are at each end of the spectrum. Admitting the highly speculative nature of the hypothesis, we still would initiate future research with the assumption that De Soto's Pacaha is ancestral to the historic Quapaw.

The Route Beyond Pacaha

After negotiating a contrived peace between Casqui and Pacaha, the army rested in the Pacaha capital for a month. As the narratives make clear, the army then retraced its steps to the Casqui capital. From there, Ranjel and Elvas say the army marched two days to a last Casqui town near a good-sized river across which the cacique assisted them with his canoes. The discrepancies in the narratives concerning times and distances for this journey as compared to the army's first march northward through Casqui make it difficult to identify the river with the St. Francis. Indeed, although we have scrutinized all lines of evidence as closely as for the preceding portions of the march through the Mississippi Valley, we cannot confidently follow the army beyond the Casqui

The narratives are clear that Quizquiz was already partially submissive to Pacaha by 1541. An interesting passage in Garcilaso conveys the impression that the Pacaha were an aggressive people whose expansion had not ended: "And since these lords (of Pacaha) were more powerful in both vassals and lands, they had pushed and were still pushing Casquin into a corner and almost to the point of surrender..." The phrase "pushed and were still pushing" uncannily describes our view of the cultural movement down the Mississippi Valley.
capital. However, we would like to outline a few tentative thoughts about the continuation of their first sojourn in the valley.

The narratives never say explicitly that the Spaniards followed the river near the Casqui capital—which we identify as the St. Francis—to its junction with the Mississippi. The so-called "De Soto Map", of which there are at least two versions (Cumming, 1958: pl. 5; Rowland, 1927: 11), shows a closed river system with three streams near Casqui joining the Mississippi at both their ends. The portrayal suggests to us that the army could not have followed any of the rivers very far, but rather assumed that their courses eventually ended up at the great river. If so, two streams of similar appearance could easily have been mistaken for the same feature. The fact that both ends of the rivers are portrayed as joining the Mississippi suggests the Spaniards were also confused about the direction of flow. The concept of such a closed system could explain how the "River of Casqui", which is logically the St. Francis, could have been extended by Ranjel as far as Quiguate. One tributary of the St. Francis, Bear Creek, actually cuts through Crowley's Ridge and its origin is separated by only a mile from the upper reaches of Lick Creek which is, in turn, a tributary of Big Creek and ultimately the White River. It is remotely possible that the Spaniards paralleled such a route without realizing that they had changed river systems. They then could have followed Big Creek and other tributaries to the White.

As to where this might put Quiguate, we can only speculate. One site, we do think, must be considered carefully in any final solution. The Dupree site (16-L-1) on Bee Bayou has a large platform mound, ten feet high, with a conical rise of three feet on one corner (see Fig. 6). More important, Phillips, Ford and Griffin counted some forty house mounds arranged about an oval plaza approximately 600 by 250 feet in size. In comparison to the visible house mounds at other sites mentioned so far, Dupree could well represent "the largest town found in Florida". The one house which has been excavated at Dupree was rectangular, and clearly it had been burned (Moselage, 1965: 1). Perhaps the fact that the house had been destroyed by fire is a coincidence, but one would expect such features at Quiguate for Elvas recalls that the army occupied one half of the town and:

...after a few days, discovering that the Indians were dealing in falsehoods, he (De Soto) ordered the other part to be burned, that it might not afford them cover should they attack him at night, nor be an embarrassment to his cavalry in a movement to repel them.

Unfortunately, the dating of Dupree presents a problem quite similar to the situation at Menard. The sherd counts from several samples, as listed by McGimsey (1965: 8), reflect a historic Quapaw component and Phillips (1970: 943) was led to include the site in his Quapaw phase. However, a number of the "minority" varieties from Dupree (such as Kent,
Owens, Avenue, Carson and Nodena) are the very markers we are using to define the De Soto date line. They are minorities in most collections. Review of the original survey material did not rule out the probability that Dupree had a sixteenth century component as well as later ones. The apparent overlap between the 1541 and Quapaw assemblages should not go unnoticed. It betrays the relative antiquity of the Quapaw movement into the Mississippi Valley.

The location of Quiguate at Dupree accords well with the narratives in one important respect: it allows for the "large towns" to the south, mentioned by Elvas, where "the Caciques governed wide territories, with numerous people". The densely populated district downstream, which is also documented by Ranjel, can be found at the Memard and Wallace sites. It could be reached easily by going down Bee or Middle bayous to the White River (see McGimsey, 1965: Fig. 3). Bee Bayou also connects Dupree with the Avenue site (16-M-1) for which a De Soto component is securely substantiated by the diagnostic vessels illustrated by Moore (1911: 401-05). Hence, Dupree was strategically located on an ideal byway between the Mississippi and White rivers.

Finally, the direction from Dupree to Coligua, which Swanton (1939: 253) identifies with Little Rock, would be northwest as Elvas says it was. Thus, our final speculation is that the Spaniards left Quiguate and, as Ranjel records, "made a journey over four swamps and days' marches" across the difficult backswamps of the White drainage, eventually reaching the Arkansas River. Thus ended the army's first journey through the Mississippi Valley.

Summary Of The Route

Despite our attempts at brevity, the complexities of the De Soto problem have prevailed in making our discussion as long and rambling as past efforts. Thus, a fast recapitulation of our basic argument may be in order. The route we hypothesize for De Soto through the Mississippi Valley is illustrated in Figure 9 as are the sites we feel there is a good probability he visited and the possible boundaries of the provinces mentioned in the narratives.

In essence, we believe that the route which best satisfies all lines of evidence takes the Spaniards from Chicaça down the Yocona or Little Tallahatchie rivers and across the floodplain to the vicinity of Clarksdale, Mississippi. They made their barges near the Montgomery site.

17It is appropriate at this point to acknowledge that Swanton (1952: 161), in a last bit of second guessing, arrived at similar identifications for portions of the route: "...we may be able to extend... (the Casqui province) to the Greer, Grant, and Starkley mounds, the first mentioned being possibly on the site of the Casqui capital...If that identification should be correct then the site of the Pacaha Province might be carried higher on the Mississippi, perhaps to the Belle Meade group. Such a readjustment appeals to me as likely..."
Route of De Soto into the Lower Mississippi Valley. Provinces described in the narratives are identified with archaeological phases.

Figure 9
(Quizquiz) and crossed the Mississippi just below Friars Point, in the vicinity of the current Old Town Bend. After preparing to continue their march at the Old Town site (Aquixo), they headed north through the swamps on the eastern margin of the Big Creek/Lick Creek drainage. From the high ground on Crowley's Ridge they spied the first Casqui villages, and following the St. Francis River upstream they found the capital of the province near the junction with the Languille. The army continued northeast along the natural levees of the Mississippi, passing more Casqui towns, until at last it reached the Belle Meade site (Pacaha). They located the cacique on an island in a westerly bend of the Mississippi which has been fossilized by a cutoff that left Horseshoe Lake. From Pacaha, De Soto went back to the Casqui capital and then presumably cut south along Big Creek and its tributaries to the vicinity of the Dupree site (possibly Quiguate). Finally, the entrada pushed west across the backswamps of the White to the Arkansas River which took them out of the Mississippi Valley.

Obviously, our reconstruction based on the events of 1541 is not likely to be correct in every last detail. Some sites, like Montgomery and Belle Meade, are far stronger identifications than others. Nevertheless, we are convinced that a search along the basic route we have outlined will provide the greatest likelihood of finding positive evidence of the De Soto entrada. Similarly, we feel that a hypothesis which makes the identifications of Quizquiz with the Tunica, Casqui with the Kaskinampo, and Pacaha with the Quapaw will provide the most productive assessment of the transition from late prehistoric to early historic phases. The ultimate test of our hypotheses, of course, will be made in the ground and we look forward to participating in this phase of the investigation, hopefully before all sites along the route are completely destroyed.

DEMOPGRAPHY OF THE LOWER MISSISSIPPI VALLEY CIRCA 1541

The De Soto entrada into the Mississippi Valley occurred within the native context, a context which was not only viable, but at a particularly successful zenith of socio-cultural and political accomplishment. The very advanced state of this development gave encouragement to the disheartened Spaniards who had been ready to forsake their venture as the change in route from Alibamo suggests. The result was that they then devoted two more years in pursuit of their elusive goals, but unhappy found themselves increasingly playing the lesser role in a cat-and-mouse game. The observations and reactions of the "conquistadors" to this situation provide a rare insight which we wish to exploit by way of overall summary.

Passages from the narratives that we have quoted are in complete agreement in specifying a number of "provinces", ruled by chiefs, some of whom were, in turn, subservient to one or more paramount chiefs. To this point, the observations are not dramatically different from other situations already encountered. However, the chroniclers are especially effusive in their descriptions of large populations and towns. They were
obviously impressed by the chief (identified as Aquixo, but probably supported by his overlord, Pacaha) who could turn out his minions in a brilliantly arrayed and carefully ordered water-bourne host as the army prepared to cross the Mississippi. Detail such as this provides the basis for the reconstruction of politico-demographic boundaries which can be tested against archaeological and ethnographic data. In this manner, we demonstrate yet again the integration of our sources, and in a parallel maneuver, validate our conclusions regarding the De Soto entrada.

Once more, we may draw upon recent archaeological achievements. In his recent magnificent synthesis, Phillips (1970) delineated a number of regional culture "phases" (Willey and Phillips, 1958: 22) to order his artifactual data. The A.D. 1541-contemporary phases of late Mississippian culture in the area under consideration are illustrated in Figure 9. The coincidence of these phases with the politico-demographic units described in the De Soto narratives is remarkable. For the portion of the route discussed here, the provinces of Quizquiz, Aquixo, Casqui and Pacaha may be positively identified with the respective phases of Parchman-Hushpuckena, Old Town, Kent and Walls. The only changes we have found it necessary to make to bring Phillips' phases into conformity with the historic units are insignificant: viz., the combination of Parchman and Hushpuckena, and the inclusion of the Belle Meade, Commerce and Hollywood sites in Walls rather than Kent. The first action is justifiable on the grounds of contemporaneity--Phillips had combined Hushpuckena with a later historic phase, Oliver, because of the spatial coincidence and lack of finer temporal control, but as we shall see the cultural relationship is tenuous.

The provincial affiliation of Belle Meade, Commerce and Hollywood is crucial to our hypothesis and so deserves special attention. Phillips (1970: 938) noted that these sites were transitional in cultural content to both the Kent and Walls phases, but felt that the evidence favored assignment to the Kent phase. We feel, however, that the more reliable socio-political evidence preserved in the nucleated settlement patterns and site plans--of Belle Meade, especially--identify more closely with Pacaha/Walls than the small scattered sites of Casqui/Kent. Therefore, we assign these sites to the southern border of Walls, which accords well with the description of the town of Pacaha/Belle Meade in the

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18One tubular blue glass bead, suggestive of the most typical kind known from sixteenth century Spanish contexts elsewhere in the New World, was found by C.B. Moore (1911: 415) at the Rhodes site (12-0-6). Although further north in the Walls region than we figure the main army to have penetrated, this single artifact provides yet another tantalizing clue to their presence in the general environs. We could only wish that both it and the sheet brass bell from the Parkin site, already mentioned, conformed more closely to our hypothesis. However, we are not unduly dismayed in light of the highly portable nature of these artifacts, as well as the fact that their presence and significance may well have been overlooked in our favored locales.
Garcilaso narrative: "this town was the frontier and defense of the entire province against that of Casquin..."

Other archaeological phases of the area may also be related to those already given. All were regional manifestations of the Mississippian culture, but not all were contemporary. The Nodena phase, north of Walls, was coeval as evidenced by a radiocarbon date of A.D. 1525 ±150 from a context in the Banks site (11-P-8) which displays the appropriate ceramic markers (Perino, 1966). The Parkin phase to the west is partially contemporary, but obviously has earlier components that have not yet been separated out, which decrees caution in the consideration of individual sites. Apparently too early in all components was the Quitman phase in the Yazoo Basin east of Quizquiz. Artifactual evidence suggests abandonment prior to the 1541 date line, thus leaving the region despoblada, as recorded in the accounts.

The remainder of Phillips' phases in the area, Quapaw and Oliver, are much later and relate to the French contact period. We have already noted that Oliver followed the Hushpuckena phase in the same region, and there are clear indications in the archaeological record that a new, although kindred, people may have moved in (Belmont, 1961: 130-32). These people may reasonably be related to the same Quapaw who gave their name to the historic phase on the west side of the river. Both phases are characterized by much smaller populations, a rather different set of diagnostic ceramic markers, and less impressive site plans.

Our identification of Quapaw with the Pacaha (Capaha) of the sixteenth century would seem to lose some validity in the light of the eighteenth century location of this tribe. Nevertheless, when all sources are brought to bear, a reasonable solution presents itself. The cultural dynamics reflected in the archaeology clearly evidence that the late Mississippian peoples were pushing southwards into the Lower Valley, also alluded to in the narratives by the pressure exerted upon Casqui by the more northerly Pacaha. By coincidence, the forefront of this advance in 1541 was at the latitude where De Soto discovered the Mississippi. On ethnographic grounds, we have already identified Quizquiz, the southernmost Mississippian frontier at this period, with the historic Tunica. The Tunica, however, were found by the French much further to the south on the banks of the tributary Yazoo River in 1682. Their own legends nevertheless suggest that they had recently moved from further upriver (Haas, 1950: 133-43), in fact, from the specific locale identified in Chickasaw and Choctaw traditions as Friars Point (Swanton, 1911: 306). Thus the clue which brings the historic accounts into agreement: whether following a previous trend, or actually pushed by the Quapaw19, the Tunica migrated further downriver after 1541 and were replaced by the Quapaw, where they were found by the French. The apparent

19Such an event may have been contributory to the fact that when La Salle rediscovered the Quapaw (Arkansas) and Tunica in 1682, he found them to be blood enemies.
archaeological break in the record between the Hushpuckena and Oliver phases is thus explained, as well as the ethnographic void in the former region of Pacaha noted during the eighteenth century.

Finally, lest this summary appears to be only a morass of archaeological and ethnographic data, thinly supportive of our speculations, there is the very human touch in the historic accounts that the Aquixo-Pacaha armada observed on the Mississippi by the Spanish was composed of especially "fine-looking men", and the remarkably similar statements that the Quapaw encountered by the French were "far better made (than the Indians further north)" (Membre', 1682, in Cox, 1922: 137), even "the best made, frankest and best disposed men that we have seen" (La Source, 1699, in Shea, 1861: 79). These were unique tributes to be drawn from usually arrogant European explorers. A small point of correlation by itself, but once again the historical evidence coincides with the archaeological and ethnographic data.

CONCLUSION

We have sought in this paper to develop a multi-discipline approach to historic contact situations. The rubric "ethnohistoric archaeology" does not denote a new methodology, but rather a purposeful coincidence and selective integration of the special data and methodologies of ethnography, historiography and archaeology. We have applied the concept to a particular case study, the De Soto entrada into the Lower Mississippi Valley. The ultimate success of our venture awaits further validation, but we feel a certain accomplishment. This is not a self-serving statement, so much as a recognition of the strengths and weaknesses of the overall approach, as well as the consideration of the De Soto problem.

The obvious strength of the overall approach rests in the diversity of data bases and methodologies which may be brought to bear upon a particular problem. When one line of inquiry is developed to an apparently insoluble impasse, it is possible that other alternatives may provide the resolution. Or, put more mundanely, each component discipline of ethnohistoric archaeology is supportive of the other two.

The weaknesses in the approach are partitive and relate to the particular case study. There is, first, the variation in the quantity and quality of the discrete data bases which may be brought to bear, and, second, the resolution of the discrepancies which may develop both within and between these sources of data. It is for this reason that we have stressed methodology in the development of this study. For, with the exception of archaeology, it is unlikely that any significant new ethnographic or historic data will be forthcoming, although the discovery of a lost manuscript is always possible. Recent archaeological activity, however, has provided new data, and did indeed give the impetus to the present investigation. It is to be expected, too, that archaeology will continue to provide a certain input, even (hopefully) to uncovering some
tangible evidence of De Soto, which will test some of the hypotheses presented in this paper.

As a concluding note, we wish to emphasize that in (ethno)historical reconstruction, which is what this is all about, the obvious must be given its due. The approach we have chosen to elucidate is obvious; but so, too, the interpretations that are hidden in the data. Too often, we are misled by the seeming conflicts in the minutiae, and the commitment to a particular methodology. It is to be hoped that we have presented some sort of catalyst to these problems.

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THOMAS, CYRUS

TURNER, DAIMOND

WILLEY, GORDON R. AND PHILIP PHILLIPS

YOUNG, J. P.
This paper proposes to present the results of a statistical analysis conducted on English wine bottles dated between c. 1652-1834. The data comprises an attempt at constructing a statistically tested model based on a sequence of dated bottles. The intent of the model is to serve as a chronological base which has comparative applicability to archeologically retrieved samples.

The framework upon which this study is based is comprised of the theoretical anthropological constructs of evolution and horizon which have been demonstrated successfully by preceding studies (Binford 1962: 19-21; South 1971: 71-116).

For a period of time now, since historical archeology has emerged as a substantial field of study in itself in this country, considerable information has been produced with regard to its basic concept vis-a-vis, the collection of archeological data in conjunction with integration of historical documentation which results in further deductions of the lifestyles of the American past. It is now evident that an increasing number of anthropologically trained archeologists are orienting their efforts toward synthesis, scientific analysis and explanation through the application of culture theory to data emerging from historical archeology. This has been stimulated, largely, by their recognition of the fertile promise and receptivity of historical archeology data to the seminal theoretical concepts of anthropological theory (Binford 1962: 19-21; Deetz 1966: 502-510; South 1971: 71-116). Some of these archeologists (Binford 1962: 19-21; South 1971: 71-116) have utilized previously compiled data (Harrington 1954; Noël Hume 1970: 102-150) and have transformed it to construct testable mathematically controlled models structured within a general scientific paradigm. The obtained results serve the purpose of validating theoretical anthropological constructs and demonstrating the applicability of these constructs to historical archeology. These results, in turn, contribute to historical archeology by the formation of firm temporal and spatial frameworks, and as such, become useful as functional dating tools.

The evolutionary concept is demonstrated by the restructuring of form and objects through time. This process causes an object to become a specific representative of a particular recurring type for a somewhat specific temporal period. In essence, the evolutionary concept of forms changing through time is the factor which determines the dictates which are set forth with regards to utilizing a specific object or an integral part of the whole object as a dating tool.
The horizon manifestation and its relationships to archeology as a whole is defined by Willey and Phillips as:

... a primary spatial continuity represented by cultural traits and assemblages whose nature and mode of occurrence permit the assumption of a broad and rapid spread.

The archeological units linked by a horizon are thus assumed to be approximately contemporaneous (Willey and Phillips 1958: 31-34).

Stanley South evidenced the horizon manifestation through the excavation of a variety of eighteenth century sites. He states:

I have become increasingly convinced that groups of ceramic types from different ruins of the same time period are similar enough to allow them to be used as dating tools for determining site occupation periods. This seems to be so regardless of whether the site is a remote frontier fort, a Cherokee village, a congested port town house, or a mansion (South 1971: 73).

As a result of his observations, he was able to construct a paradigm from which were derived meaningful analytical tools for use in determining the occupation dates for eighteenth century British-American sites with a remarkable degree of reliability.

THE PROBLEMS

It was apparent that ceramics and clay pipes (stems) were applicable to the dating of archeological sites due to what appears to have been a phenomena of rapid distribution from their centers of production. If this premise is true then theoretically the same phenomena should occur simultaneously with other aspects of material culture, in this particular case, bottles.

Glass bottles have been in use dating back to the early periods of colonization of America. The majority of bottles recovered from colonial sites are assumed to be English in manufacture since very little is known concerning American bottle making prior to the Revolution (Noël Hume 1970: 60).

Considerable information is available with regards to the evolutionary development of the English wine bottle, and attempts have been made at isolating the change in form on a temporal basis to provide datable results. The trouble with the utilization of bottles for dating has been as Noël Hume states:

In broad terms these efforts have been fairly successful, and it is possible to tell the difference between bottles of, say 1650, 1690, 1730, 1760,
1780 and 1820 without much trouble. The difficulties arise when we try to pin down the transitional forms that link these dates together (Noël Hume 1970: 60).

In *A Guide to Artifacts of Colonial America*, Noël Hume (1970: 63-68) was successful in ordering seal-dated bottles beginning from c. 1652-1834. This has resulted in a considerable aid since a visual comparison can be made, and thereby link non-inscribed archeologically derived bottles subjectively to somewhat more specific temporal units than had previously been possible. Still, the results obtained by dating in this manner do not allow them to be tested for reliability. To use bottles as a dating tool with an acceptable degree of reliability necessitated the development of a mathematically testable model.

**THE MODEL**

The construction of this model was based on the inscribed bottles illustrated in *A Guide to Artifacts of Colonial America*. An approach was used similar to a method developed by Dr. Anta M. White at the University of Kansas concerned with utilizing polar coordinates in analysis of lithic artifacts (White MS). The method was modified in that angle deviations were used to enable the dimensions of attributes to be consistently obtained regardless of bottle size.

Four attributes were decided upon which were considered important based on visual and measurable criteria. These are maximum height, maximum width, kickup, and basal ring width (this attribute was chosen over maximum base width primarily to facilitate measurement). The dimensions of width and basal ring width were restricted to the left half of each illustration since utilizing the whole bottle would only result in repetition of these same attributes.

Linear measurements were derived from the above attributes for 44 of the 49 bottle illustrations. Five bottles were not included; two (1740 and 1770) because of their different shape; three (1732, 1733, and 1734) were excluded due to the inability to determine the maximum width location. The measurements were then transferred to a form (Fig. 1). The form served the purpose of accommodating the linear measurements on a larger scale. The exterior edge of the basal ring acted as the point of reference from which lines were drawn to the other three points comprising the width, height and kickup height, with the line upon which the bottle base rests acting as the 0° - 180° axis. Angles were thus derived for these attributes. To obtain the angle comprising the basal ring width, the vertical axis was used, acting as the 0° - 180° line, and obtaining the angle (4) from attribute #2, the height.

The angles derived for each bottle were plotted separately against their counterparts through time (Fig. 2). Two observations resulted. The primary concern was to initially establish whether a combination of the four attributes was non-recurring through time, which did not occur. The second observation indicated that there was considerable more fluctuation between the years 1652-1740 evidenced in the attributes representing height, width and basal width than after 1740, when these attributes
BOTTLE FORM

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FIGURE 1

Bottle No. 38AK4-50E-1
Bottle Date _______
Scale 40

Key: A = Width  
B = Height  
C = Kickup  
D = Base Ring Width

DIMENSIONS
A = 82.5
B = 264.0
C = 50.0
D = 75.5

CORRESPONDING ANGLES
1 = 85.5
2 = 106
3 = 143.5
4 = 38

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TEMPORAL COMPARISON OF ENGLISH WINE BOTTLE ATTRIBUTES
INSTITUTE OF ARCHEOLOGY AND ANTHROPOLOGY - UNIVERSITY OF SOUTH CAROLINA

![Graph showing temporal comparison of English wine bottle attributes with time periods from 1610 to 1850. The graph plots Kickup, Maximum Height, Maximum Width, and Basal Width attributes over time.](image-url)
appear to have become somewhat uniform. The attribute comprising the kickup oscillates considerably through time as opposed to those comprising height, width and base ring width. Although the height, width and basal ring width tend to undergo less variability after 1740, the kickup oscillates considerably, although appearing to be following somewhat of a general trend. Ivor Noël Hume (personal communication) noted that:

... the degrees of flexibility in dimensions declined through the eighteenth century, but was tremendously variable between the mid-seventeenth century and around 1740.

This phenomenon was evidenced by the attributes of width, height and base ring width, especially width, thereby illustrating by quantitative data what had previously not been determinable except subjectively.

With the consultance of statisticians and computer programmers, a regression analysis (Polynomial Regression - BMD05R) was decided upon to establish predicted values from the observed data. This would make it possible to determine developmental relationships existing between bottle forms through time. The observed values comprising the attributes were treated as being representative of a mean derived from a population sample since, in most cases, the illustrations comprise one bottle for a specific year.

Based on the fact that bottles became more uniform after 1740, the bottles dated between 1652-1740 were subjected to a separate regression from those dated between 1740-1834 (Figs. 3 - 6). Of the four attributes, the attribute comprising the kickup based on the observed value oscillates considerably from the predicted value (Fig. 5). In contrast, the observed values for the attributes comprising height, width and basal ring width correspond considerably closer to the predicted values (Figs. 3, 5 and 6), especially the height. Comparison between the observed and predicted values of the four attributes indicate that the kickup appears to have considerable variability during any one specific time period in contrast to the other attributes, although still appearing to occur within a general trend (Fig. 5).

To determine the feasibility and reliability of the model, it was tested utilizing different sources and other temporal controls independent of the data which brought about its inception. A series of bottles were used which had been recovered from archeological sites which are in whole, or in part, attributable to the British-American complex of Colonial America. These sites are: Fort Moore (1716-1766), South Carolina; Newington Plantation (c. 1680-1845), South Carolina; Fort Michilimackinac (1715-1781, British occupation 1761-81), Michigan; Fort Stanwix (1758-1781), New York; Brunswick Town (1734-1776), North Carolina; and Spanish Town (King's House, 1761-1872), Jamaica, W.I. (Appendix I).

The Institute of Archeology and Anthropology, University of South Carolina conducted two separate excavations in different locations at
CLASS R-REGRESSION ANALYSIS
AND POLYNOMIAL REGRESSION

PLOT OF OBSERVED
AND PREDICTED
VALUES FOR WIDTH

Y

INSTITUTE OF ARCHAEOLOGY
AND ANTHROPOLOGY
UNIVERSITY OF SOUTH CAROLINA

TIME

50 60 70 80 90 100 110
ATTRIBUTES (DEGREES)

296
PLOT OF OBSERVED AND PREDICTED VALUES FOR HEIGHT (Y2)

INSTITUTE OF ARCHEOLOGY AND ANTHROPOLOGY
UNIVERSITY OF SOUTH CAROLINA

FIGURE 4
PLOT OF OBSERVED AND PREDICTED VALUES FOR KICKUP (Y3)
PLOT OF OBSERVED AND PREDICTED VALUES FOR BASAL RING WIDTH (Y4)

INSTITUTE OF ARCHEOLOGY AND ANTHROPOLOGY UNIVERSITY OF SOUTH CAROLINA

FIGURE 6

OBSERVED
O PREDICTED
X BOTH

CLASS R-REGRESSION ANALYSIS
BM005R-POLYNOMIAL REGRESSION

1830
1810
1790
1770
1750
1730
1710
1690
1670
1650

10 20 30 40 50 60 70 80 90 100

ATTRIBUTES (DEGREES)

17 MAR 79

299
the site of Fort Moore. As a result, 29 bottles comprised of whole and restorable fragments were recovered. Fourteen bottles (38AI5) were found in a refuse pit and a cellar during the first excavation. The fifteen bottles (38AI4) obtained during the second excavation were found in a cellar.

Twenty-seven of the bottles found at Fort Moore displayed somewhat similar visual characteristics, including two half-size bottles. Two bottles had visual characteristics attributable to so-called "onion" bottles. Linear measurements were obtained for each sample and subjected to the previously described procedure of angle derivations (Fig. 7). The derived attributes were applied to their counterparts in the model by a comparison between the attributes of each sample and those of the model (Fig. 8; Appendix I). The resulting occurrence indicated that the bottles obtained during the first excavation (38AI5) ranged between 1731-1755. Forty-three percent of the bottles display attributes similar to the bottles dated 1740 in the model (Fig. 9). The derived median date for the bottles is 1741.64. The results of dates derived by use of ceramics and pipe stems are: 1741.7 produced by the mean ceramic date formula, and 1744.16 by use of pipe stem dating (South 1970: 91). The date range for the bottles found during the second excavation is between 1727-1750 (Appendix I). Thirty-eight percent had attributes approximating those of the model dating at 1740, and forty-three percent ranged between 1727-1737. Nine percent range between 1745-47. The derived median date is 1738.08. The ceramic and pipe stems dates are 1726.1 for ceramics (South 1970: 91), and range between 1737.87 - 1740.55 for pipe stems (Richard Polhemus, personal communication). Stanley South indicated that with regard to the ceramics, a considerable quantity of earlier sherds occurred in context with the later ones, causing the ceramic date to be somewhat earlier.

A comparison of attributes of the two "onion" type bottles, found in association with the above mentioned, and the model revealed dates approximating 1710 and 1713 (Appendix I). The bottles (38AK4-45B-2) having the assigned 1713 date resembles the bottle dated 1714 in the model (Fig. 10).

At Newington Plantation (c. 1680-1845) three bottles having "onion" type characteristics were recovered from the burned architectural remains of a timber and clay cellar which is believed to have been destroyed on July, 1715 during the Yamassee War by: 

... the Apalatchee and other Southern Indians (Cheves 1894: 316-355).

A comparison between the model attributes and those of the archeological samples evidences dates of 1708, 1713 and 1713.5 (Appendix I). The ceramics, wine glass stems, etc. recovered from this cellar all reveal characteristics attributable to the late seventeenth and early eighteenth century (Richard Polhemus, personal communication).
KEY: A-WIDTH
B-HEIGHT
C-KICKUP
D-BASE WIDTH

DIMENSIONS
A-100
B-311
C-62
D-92.5

CORRESPONDING ANGLES
1-86.5°
2-107°
3-146°
4-16°
### TABLE OF BOTTLE ATTRIBUTES<br>COMPRISING CHRONOLOGICAL MODEL<br>BASED ON DATED SAMPLES IN<br>AN GUIDE TO ARTIFACTS OF COLONIAL AMERICA

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$y^1$ = Width; $y^2$ = Height; $y^3$ = Kickup; $y^4$ = Base Ring Width.

1 Expressed in terms of degrees.
FIGURE 9

DRAWING FROM
A GUIDE TO ARTIFACTS OF
COLONIAL AMERICA
HUME, 1970 P. 65

303
DRAWING FROM
A GUIDE TO ARTIFACTS OF
COLONIAL AMERICA
HUME, 1970 P. 64

FIGURE 10
Thirty-nine bottle samples were obtained from Spanish Town, Jamaica (Appendix I) which were recovered from a midden deposit of a structure known as the Old King's House constructed in 1761 (Mathewson 1972: 4). Duncan Mathewson (personal communication) indicated that the midden contained a large assemblage of English ceramics, Chinese porcelain and glasswares which he dates between 1775-1800. Documentary evidence suggests that a considerable quantity of material in the form of refuse existed throughout the last quarter of the eighteenth century, all of which contributed to the large midden believed to have been deposited in about 1800 and used as foundation fill for a later servant's quarter (c. 1805-15/20).

The earliest bottle dated by attribute comparison is dated at 1731 and the latest at 1834. A median date of 1775.03 was derived for all of the bottles, which is the beginning date assigned to the midden deposit.

Five bottles from Brunswick Town, North Carolina (1734-1776) came from the Governors' Mansion known as Russellborough constructed in 1751 and burned in 1776 (South 1967: 360-372). The bottles recovered are dated between 1736-1767 by comparison with the model. The mean ceramic date for this structure is 1754.6 and the pipe stem date is 1756. The median date derived from the bottle dates is 1754.

Fort Michilimackinac has a historic occupation date of 1715-1781, with the French occupying the fort until 1760 when it was surrendered to the British (Maxwell and Binford 1961: 10-13). Five bottle dimensions were supplied by Margaret K. Brown from bottles which had been dated by context (Appendix I). Bottle A was recovered from a feature used between 1775 and 1781. The bottle date derived from the attribute comparison is 1770. Bottle B is from a feature dated between 1770-1774. The bottle date derived is 1772. Bottle D was found in a basement believed to have been filled after 1770. The derived bottle date is 1788. Two bottles (MS2-4414A and MS2-4414C) are from a latrine (Feature 397) dated between 1770-1774 (Margaret K. Brown, personal communication). The attributes for these two bottles are almost identical, both having a derived date of 1772. Bottle B, also dated at 1772, has attributes which are considerably similar (Appendix I). A median date of 1774.8 was derived for the five samples.

Thirteen bottle samples were supplied by Lee Hanson, Jr., from Fort Stanwix, New York (1758-1781). With the exception of one sample (20860-2/8064) which was recovered from a cellar, the samples are from a nineteenth century privy. The West Barracks cellar of the fort from which the single bottle was recovered was dated c. 1767-1781, by context, with the cut-off date possibly being 1774. The attribute comparison derived a date of 1772 (Appendix I). The twelve bottles found in a nineteenth century privy dated at between c. 1810-1825, and possibly of the first half of that period, ranged in date between 1765-1809, with sixty-seven percent ranging between 1800-1809. Three bottles were dated at 1765, and two at 1772 (Appendix I). The median date for the bottles is 1791.83.
The preceding data has been an attempt at demonstrating the feasibility of the use of a mathematically derived model which possesses the capability of allowing English wine bottles to be used as analysis tools.

The median bottle dates attributable to the sites from which they were derived, correspond closely with those obtained utilizing other sources independently of each other. It is thought that this cannot be attributable to mere coincidence, but rather should be considered as further evidence of the ability to isolate and make accessible to testing, a minute segment of culture process. This is the reason that the study was undertaken, as its purpose was not intended merely to produce another dating tool, but rather to further illustrate the validity of the theoretical constructs which serve to evidence the underlying cultural factors which account for its ability to serve as an analysis tool.

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MATTHEWSON, R. DUNCAN

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NOËL HUME, IVOR

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APPENDIX I

COMPARATIVE ATTRIBUTE CORRELATIONS ESTABLISHING TEMPORAL RELATIONSHIPS BETWEEN CHRONOLOGICAL MODEL AND APPROXIMATE MANUFACTURE DATE OF ARCHEOLOGICALLY RETRIEVED BOTTLES

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1Expressed in terms of degrees.

*Y* = Width; Y = Height; Y = Kicup; Y = Base Ring Width.
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313
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**Brunswick Town, North Carolina**

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          - 102.0
          - 144.0
          - 12.5
        - 1757
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          - 102.0
          - 140.0
          - 12.0

- **Ceramic Date** c. 1754.6
  - 1761
    - 87.0
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  - 1761
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    - 101.5
    - 146.5
    - 11.5

- **Pipe Stem Date** c. 1756
  - N50-3-2-4
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    - 105.5
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    - 14.5

- **Median Bottle Date** c. 1754
  - N50-3-2-2
    - 89.5
    - 102.0
    - 143.0
    - 11.5

314
### APPENDIX I (Continued)

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**Fort Michilimackinac, Michigan**

Historic Date
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British Occupation
1761-1781

Median Bottle Date
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**Fort Stanwix, New York**

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APPENDIX II

BOTTLE MEASUREMENT INSTRUCTIONS

1. Five measurements are essential; (1) Maximum Width, (2) Maximum Height, (3) Height of the maximum width from the base, (4) Kickup Height, (5) Maximum Base Ring Width.

2. Obtain height by placing bottle on flat surface and measuring with an engineer scale at 40 units.

3. Obtain the maximum width measurement for placement on the form by measuring the height from the base to the point of maximum width on the vertical axis. With calipers, obtain the maximum width. Take two or more measurements of each to arrive at a median measurement. Divide this figure by two.

4. Kickup is measured from the outside. Take a reading at the deepest portion. Dimension derived by its relation to the base.

5. Maximum base ring width is the visible ring on the base which indicates where the bottle rested. Take several measurements, obtain median and divide by two.

6. Once all measurements have been obtained, place compass on horizontal line of form and obtain angles. To obtain the maximum base ring width angle, take reading from attribute No. 2 (height), using the vertical line as the $0^\circ - 180^\circ$ axis.
PAWNEE POTTERY REVISITED:
FORMULA DATING OF A NON-EUROPEAN CERAMIC TRADITION

Roger T. Grange, Jr.

ABSTRACT

This paper reports an application of South's ceramic formula dating method to a non-European ceramic tradition. Ceramic formula dates are calculated for sites of the Pawnee-Lower Loup ceramic tradition of the proto-historic to historic periods in the Central Plains.

A good correlation between the calculated mean dates and expected median date values was obtained. The study demonstrates that the ceramic formula method can be applied to aboriginal ceramic traditions, thus suggesting that the extension of the method to fully prehistoric contexts should be possible in the future.

INTRODUCTION

A recent and highly significant contribution to historical archaeology has been the development of a ceramic dating formula by Stanley South (1972a). This method employs the multiplication of sherd counts by the median dates of known manufacture ranges of pottery types to calculate a mean ceramic date for the site or assemblage in question (South 1972a: 72, 75, 83). South has demonstrated that these dates correspond closely to the median dates of sites for which the occupational span is known. The method offers excellent prospects for estimating the dates of historic period site occupations containing British and other European ceramics of the sixteenth to nineteenth centuries.

Some commentators discussing South's method have pointed out that formula dating may not be needed by an archaeologist who can identify British pottery well enough to apply the formula (Stone 1972: 179; Walker 1972: 138), a point which South noted himself (1972a: 86). A more extreme view is that of Liggett who asserted that the calculation of dates by the formula method is useless (Liggett 1972: 196–7). My own reaction has been that South's formula is useful in the analysis of historic sites and, perhaps even more important, that the method may be applied to materials of other cultures and periods (Grange 1972: 191).

It is to this latter application of the ceramic dating formula that this paper is directed in the spirit suggested by Larrabee who pointed out that the controls of historical archaeology may be used to refine methods applied in other fields (Larrabee 1969: 72). If it can be shown that ceramic formula dating can be applied to the ceramic tradition of a non-European and non-industrial culture, the possibility of applying this analytical method to problems of prehistoric archaeology may be enhanced. It is the purpose of this paper to demonstrate that such an application is possible.
In order to determine the usefulness of South's method outside British or European ceramic traditions, a reasonably well-known and relatively complete ceramic tradition is needed as a test case. It must be a tradition for which there are sufficient dates to permit the calculation of the approximate periods of manufacture of the constituent types, and one including some sites sufficiently well dated to permit a test of results.

The Pawnee and Lower Loup ceramic tradition of the Central Plains offers a good case for study. With the exception of recently excavated sites, virtually all ceramic material from the historic Pawnee villages and their proto-historic Lower Loup forerunners has been studied in detail (Grange 1968). The historic Pawnee sites can be dated by historical documentation and a reasonable initial date for the origin of the ceramic tradition can be estimated on the basis of an early historical reference. It is thus possible to estimate the manufacture periods of Pawnee pottery types, to date sites by the ceramic formula method and to compare the calculated dates with expected median values determined from direct historical identification of some of the sites.

The available information also permits testing of the results by means of historical date brackets tentatively assigned to some proto-historic sites whose actual occupations cannot be confirmed by the direct historical approach. Still earlier Lower Loup sites have been dated by carbon-14 determinations and these data can be used to test the ceramic formula results if they are not employed in the initial dating of the pottery types used in the analysis.

The Pawnee-Lower Loup material offers another advantage: the data are readily available in a form which permits their immediate application to this problem. All of the type descriptions and sherd count data used in this analysis are taken from the monograph, Pawnee and Lower Loup Pottery (Grange 1968) and these details need not be reproduced in this paper. One theme of the earlier study was the demonstration that the Lower Loup focus represents the proto-historic phase of Pawnee culture and that this was a single, changing ceramic tradition (Grange 1968: 71). Historical identifications of the sites have also been reviewed, providing a basis for the type and site dates utilized here, thus simplifying the task. Readers may wish to refer to that source for additional information.

**DATING**

The first step in applying the ceramic formula method to the Pawnee ceramic tradition is to determine the periods of manufacture of the constituent ceramic types. Twenty types have been used for dating purposes. These are all rimsherd types; bodysherd data have not been used.

In applying South's method to this non-European ceramic tradition we lack the historical documentation of periods of manufacture of pottery
types which are available for British wares. Nevertheless, estimation of such ranges for Pawnee types can be based on historical dates. The terminal manufacture dates were easily determined for most types since the making of pottery by the Pawnee was a craft that died out in the period being considered (Grange 1968: 116). The terminal occupation date for the latest site in which a pottery type appeared was arbitrarily selected as representing the terminal manufacture date for the type. This is obviously not an exact terminal date but must be a close approximation. Such terminal dates range from 1825 to 1846.

The initial date for some types and for the tradition as a whole can also be estimated from European historical documentation. The earliest possible reference to the Pawnee is in 1541 and is derived from Coronado’s visit to the plains. The identification is an indirect one based on the inference that Harabey was a reference to the Pawnee (Grange 1968: 119), an inference which has some support in archaeological evidence (Grange 1968: 122-3).

Archaeological cross-dating can also be cited to support an estimate of the beginning of the Lower Loup period at about A.D. 1500 (Grange 1968: 123-6), close to the 1541 estimate based on the Coronado reference. In fact, the ultimate origin of the Pawnee-Lower Loup ceramic tradition may be earlier, as seen in the affinity of some ceramic types to fifteenth century materials from the Arzberger, Lynch and Campbell Creek complexes (Grange 1968: 125). Indeed, the implication of the seriation diagram of the major type Nance Flared Plain (Grange 1968: Fig. 6) is that there must be some earlier, as yet undiscovered, Lower Loup components.

In this study the base date estimated for the origin of the tradition, A.D. 1500 (Grange 1968: 126), has been used as a baseline. Manufacture ranges, median and peak popularity dates of the pottery types have been estimated on this base date. Some types had their origin at the A.D. 1500 baseline and remained in use long enough that their terminal date could be based on historic Pawnee site occupation data. Other types, however, either began or ended at intermediate points. The dates for these types were estimated on the basis of historical dates as outlined below.

One part of the study of the Pawnee and Lower Loup pottery was a seriation analysis and the excavation unit seriation (Grange 1968: Fig. 6) was used as the basis for type dating. Dates for historic Pawnee sites and the A.D. 1500 base date were entered at approximate points on a copy of the excavation unit seriation. One other historical date, 1777, was also inserted at the point of the ceramic demarkation between the historic and proto-historic phases. This point was earlier estimated at 1750 or 1775 (Grange 1968: 120) but 1777 was selected as a date more surely linked to identified sites (Grange 1968: 119).

Estimated type origin points or terminal dates falling between 1500 and 1777 were estimated on the basis of these two historically determined points. Two assumptions were necessary; that the excavation unit seriation afforded a reasonably accurate picture of the development
of the types in question and that the seriated units were more or less evenly distributed through the 1500 to 1777 period. On this basis the initial appearance point of a type was determined by its location between the 1500 and 1777 dated points. The distance from the 1500 point on the graph to the earliest appearance of a type in an excavation unit in the seriation was measured and a date assigned on the basis of the proportional distance between 1500 and 1777. Terminal dates and peak dates within this range were estimated in a similar manner. Such estimated dates were rounded to the nearest five year figure in contrast to specifically historical dates which were not rounded.

In this manner a manufacture span was estimated for each type and a mid-range (median) date was calculated. South's method of formula date calculation employs the median manufacture date (South 1972a: Fig. 1). Walker (1972: 130-2) has discussed the possibility that modal dates might produce more accurate results, but other studies have shown that it makes little difference if modal dates are used instead of median dates in British ceramic calculations (Grange 1973). However, the life-spans of types in the Pawnee ceramic tradition are longer and overlap one another to a greater degree than do the more restricted manufacture periods of British ceramic types. It is therefore likely that the use of peak or modal dates is more important for dating purposes in non-European ceramic traditions. Accordingly a peak date was estimated wherever possible on the basis of the standard excavation unit seriation diagram.

The seriation of Pawnee and Lower Loup sites was based for the most part on short term excavation units, obviating the need to use the cumulative seriation technique (Grange 1973) for estimating modal dates for the Lower Loup period types.

Several types do not exhibit a sufficient peak of popularity to estimate a modal date and in those cases the median or mid-range dates are used in ceramic formula calculations.

Table 1 summarizes the estimated date range, the mid-range and peak popularity dates for each type. Specific comments on some types will serve to clarify special problems and necessary adjustments.

Nance Flared Plain is the most common pottery type present in the collection. The type was plentiful in the earliest features in the series and thus originated at least as early as A.D. 1500. It last appears in the Clarks Site (25Pkl) and thus has an estimated terminal date of 1845 (Grange 1968: Table 4). The mid-range date is 1672 while the estimated peak of popularity is at 1570. These dates can be used in all ceramic formula date calculations involving this type, but examination of the seriation diagram (Grange 1968: Fig. 6) shows that this type was not very common in historic period Pawnee sites. The 1570 modal date for the type tends to distort the formula dates for historic Pawnee villages, and an adjustment was worked out empirically for this
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</tr>
<tr>
<td>Shell Tempered</td>
<td>1525-1700</td>
<td>1612.5</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Walnut Decorated Lip</td>
<td>1500-1614</td>
<td>1557.0</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Miscellaneous</td>
<td>1500-1845</td>
<td>1672.5</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Totals:

* = Use adjustment for historic period sites.
type. The historic Pawnee period is 1777 to 1845 and the mid-range date for that period is 1811. In those sites where the typical historic Pawnee pottery type, Webster Collar Braced, amounts to 10% or more of the site collection, the mid-range date of 1811 is used for the type Nance Flared Plain in formula calculations. When Webster Collar Brace accounts for less than 10% of the site total of dated types, the 1570 peak date is used for Nance Flared Plain. This adjustment follows a simple rule and, in effect, subdivides Nance Flared Plain into two chronological varieties for dating purposes. It should be noted that in his formula date calculations South offers alternative median dates for Decorated Delftware, a type of long duration, to be used in seventeenth or eighteenth century sites respectively (South 1972a: Fig. 1). The adjustment of the date for Nance Flared Plain outlined above is similar in its effect upon the resultant dates.

Another specific adjustment is the peak date of 1844 assigned to Webster Bowl Ware. This date is based on the fact that this is the only aboriginal ceramic type to survive in the Fullerton Site (25Nc7) of 1842-1846 (Grange 1968: Table 4). There are later historic Pawnee sites but native pottery was totally absent after 1846. The sherd count from 25Nc7 is only two sherds and in numerical terms the peak date of this type occurs later than its 1713 mid-range date but earlier than the 1844 date assigned. However, ceramic formula dates cannot be later than the latest mid-range or peak date assigned for dating purposes, and it would not be possible to date 25Nc7 without the adjustment noted above. Therefore, the percentile peak of this type was selected for dating purposes rather than its modal date as determined by cumulative seriation.

The 1825 peak date for Webster Collar Braced, the most common type in the historic period, was estimated on the basis of its peak presence in sites 14Rp1 and 25Nc2 and by means of the cumulative seriation method (Grange 1973). Both approaches to this estimate are consistent with the 1825 date. The 1830 peak for Webster Bowl Lids was also confirmed by the cumulative seriation method.

Two significant types found in early Lower Loup sites are Burkett Cord Roughened and Burkett Collared which may link the proto-historic phase with earlier prehistoric periods (Grange 1968: 126). The terminal date for these types was estimated by the position of its latest appearance on the seriation diagram. The initial date for the types appears to pre-date A.D. 1500 and an estimate of 1450 was used. That estimate was based on tree ring and carbon 14 dates for sites in which related ceramics were found (Grange 1968: 126) and 1450 may be closer to a modal than to an initial date. A reliable peak or modal date for these types is difficult to estimate and the median value of 1565 is used in formula calculations. In terms of the seriation diagram this appears too late a value and a date of 1505 could be utilized as an alternate figure. However, these are numerically infrequent wares, and the difference in resultant dates for sites is not great.
Trade ceramics containing shell tempering are also present in some Lower Loup sites. The date range 1525 to 1700 was employed for these materials (Grange 1968: Table 16). It is also possible to calculate formula dates omitting the sherds of these types from the calculations in the same manner that South eliminates Chinese Porcelains from dating (South 1972b: 217).

Walnut Decorated Lip was also present as a trade type and the date span of 1500-1614 for the White Rock Aspect (Grange 1968: Table 16) was used for this type. It, too, can be omitted from calculations if desired.

There were several miscellaneous types and miniature vessels represented in the Pawnee ceramic tradition. These can be eliminated from consideration as rare items, or, as done here, can be grouped together and treated as a "type" with a manufacture range of 1500 to 1845.

The initial, terminal and mid-range or peak dates for the other types listed in Table 1 were calculated by the process described above but did not require special adjustments and are, therefore, not discussed individually. Peak dates could not be estimated for several types and in those cases the mid-range dates are used for formula purposes.

It should be noted that in this study, the mid-range dates are calculated to the .5 year value rather than being rounded as South did in his dating procedure.

Table 1 not only summarizes the dates for the types but illustrates the work sheet used in this application of South's formula. Sherd counts from each site or excavation unit taken from the report on Pawnee ceramics (Grange 1968: Table 12) were entered on the work sheets and totaled. The counts for each type were multiplied by either the mid-range or peak date and the products summed. The sum is then divided by the number of sherds to calculate the mean ceramic date for the site (South 1972a: Fig. 1). Coding the date factors will reduce the size of numbers if necessary. The sherd counts used in the calculations of dates in this study are not repeated here because they are readily available in the monograph containing the original data. The calculated dates for 15 Pawnee and Lower Loup sites are listed in Table 2; these are the sites used in testing the results of the dating experiment. Ceramic formula dates for all excavation units listed in the original study (Grange 1968: Table 12) are recorded in Table 4.

The 15 sites used in testing the formula date results include 11 historic sites for which occupation periods and median dates can be calculated. Three of these, 25Nc5, 25Nc7 and 25Pkl are well-dated sites but ones containing very few aboriginal ceramic specimens. Four sites, 25Bul, 25Bu2 Pawnee component, 25Hwl and 25Nc2 are well-dated historic sites and ones from which a fair sample of aboriginal pottery was recovered. The collection from 25Hwl is surface material only. Four other historic Pawnee sites, 25Gal, 25Sdl, 14Rpl and 25Wtl, present
special problems. The Yutan Site (25Sdl) produced only a small sample of sherds and its dating and identification are open to question. The date range is approximate and culturally it appears to be an Oto site heavily influenced by the Pawnee rather than a real Pawnee site (Grange 1968: 23-4). Despite these problems the site was used in testing the method. The terminal date of 25Gal is well known (Grange 1968: 20) but the beginning of its occupation cannot be associated with the specific historical references. For the purpose of this test an initial date of 1797 has been selected since this is a reference to the Tappage band (Grange 1968: 119). Earlier mentions of this band in 1767 could also be used. There has long been controversy concerning the identification and relationships of the Kansas Monument Site (14Rpl) and the Hill Site (25Wt1), both occupied by the Republican band or sub-tribe of the Pawnee. For the purposes of this test, the late dating, 1821-1831, of the Kansas Monument Site and the early dating, 1775-1810, for the Hill Site have been used (Grange 1968: 119-20).

The preceding discussion shows that there are some problems associated with the historic period sites used to test the results of formula dating. These are shortcomings resulting from small sherd samples or imperfect dating or both. Nevertheless, these sites provide expected median dates which can be compared with the calculated ceramic formula mean dates.

Two Lower Loup sites have been dated by the carbon-14 method; these are 25Nc1 (A.D. 1630 ± 100) and 25Nc3 (A.D. 1680 ± 100) (Grange 1968: 129). The radio-carbon values have not been used in estimating the ranges, mid-range or peak dates of the pottery types and therefore can be used to test the results of ceramic dating. It is assumed that these carbon-14 dates represent mean dates for the sites and they are treated as comparable to the median dates of occupation calculated for historically dated sites. In fact, the carbon-14 samples were selected to date early and late features in the standard ceramic seriation and thus were not necessarily from mid-range occupation features of the sites.

Two other Lower Loup phase sites may also be used in testing the results of formula dating. These are 25Bu4 and the Lower Loup component of 25Bu2. These sites cannot be dated by the direct historical approach as the historic Pawnee villages have been dated. However, it has been inferred that some early historical references to the Pawnee must relate to the area in which these sites are located (Grange 1968: 120-1) and that these sites therefore fall within the period 1687-1777. This date bracket and its mid-range date have been used in comparison with the ceramic formula dates for these two sites.

Thus a total of 15 historic Pawnee and proto-historic Lower Loup sites with expected median date values are available for test purposes. These results are listed in Table 2.

Two series of dates have been calculated for each site. One series is based on the use of modal or peak date values for pottery types where
Table 2. Known and Calculated Dates: 15 Test Sites

<table>
<thead>
<tr>
<th>Site</th>
<th>Span of Occupation</th>
<th>Median Date (HD)</th>
<th>Mean Date (CD)</th>
<th>Standard Deviation (CD)</th>
<th>Median Date (Formula)</th>
<th>Mean Date (Formula)</th>
<th>Standard Deviation (Formula)</th>
<th>Comparison:</th>
</tr>
</thead>
<tbody>
<tr>
<td>25Nc7</td>
<td>1842-1846</td>
<td>1844.0</td>
<td>1844.0 ± 0</td>
<td>1844</td>
<td>1698.0 ± 0</td>
<td>1698</td>
<td>0</td>
<td>-146.0</td>
</tr>
<tr>
<td>25Nc5</td>
<td>1820-1845</td>
<td>1832.5</td>
<td>1826.7 ± 3</td>
<td>1824-1830</td>
<td>1771.7 ± 33</td>
<td>1739-1805</td>
<td>32.1</td>
<td>-60.8</td>
</tr>
<tr>
<td>25Nt1</td>
<td>1775-1810</td>
<td>1792.5</td>
<td>1824.6 ± 23</td>
<td>1822-1848</td>
<td>1729.6 ± 37</td>
<td>1693-1767</td>
<td>62.9</td>
<td>-115.6</td>
</tr>
<tr>
<td>25Pk1</td>
<td>1820-1845</td>
<td>1832.5</td>
<td>1815.5 ± 55</td>
<td>1760-1870</td>
<td>1716.9 ± 45</td>
<td>1672-1762</td>
<td>-84.9</td>
<td>-15.5</td>
</tr>
<tr>
<td>25Nc2</td>
<td>1809-1842</td>
<td>1825.0</td>
<td>1814.6 ± 39</td>
<td>1776-1854</td>
<td>1740.1 ± 32</td>
<td>1708-1772</td>
<td>-81.7</td>
<td>-10.4</td>
</tr>
<tr>
<td>14Rpl</td>
<td>1821-1831</td>
<td>1826.0</td>
<td>1810.5 ± 45</td>
<td>1765-1855</td>
<td>1744.3 ± 25</td>
<td>1719-1769</td>
<td>10.6</td>
<td>-69.2</td>
</tr>
<tr>
<td>25Bu2 (P)</td>
<td>1797-1800</td>
<td>1798.5</td>
<td>1809.1 ± 32</td>
<td>1777-1841</td>
<td>1729.3 ± 35</td>
<td>1694-1764</td>
<td>-69.7</td>
<td>-81.7</td>
</tr>
<tr>
<td>25 Bu1</td>
<td>1777-1809</td>
<td>1793.0</td>
<td>1806.2 ± 29</td>
<td>1777-1835</td>
<td>1723.3 ± 38</td>
<td>1685-1761</td>
<td>13.2</td>
<td>-69.7</td>
</tr>
<tr>
<td>25Sdl</td>
<td>1775-1835</td>
<td>1805.0</td>
<td>1793.9 ± 46</td>
<td>1748-1840</td>
<td>1713.6 ± 37</td>
<td>1677-1751</td>
<td>-11.1</td>
<td>-91.4</td>
</tr>
<tr>
<td>25Hw1</td>
<td>1804-1836</td>
<td>1820.0</td>
<td>1791.6 ± 57</td>
<td>1735-1849</td>
<td>1708.1 ± 36</td>
<td>1672-1744</td>
<td>-119.9</td>
<td>-28.4</td>
</tr>
<tr>
<td>25Gbl</td>
<td>1797-1825</td>
<td>1811.0</td>
<td>1783.5 ± 65</td>
<td>1718-1848</td>
<td>1708.5 ± 41</td>
<td>1667-1749</td>
<td>-102.5</td>
<td>-27.5</td>
</tr>
<tr>
<td>25Bu4</td>
<td>1687-1777</td>
<td>1732.0</td>
<td>1683.9 ± 94</td>
<td>1600-1788</td>
<td>1683.0 ± 25</td>
<td>1658-1708</td>
<td>-49.0</td>
<td>-67.3</td>
</tr>
<tr>
<td>25Bu2 (L)</td>
<td>1687-1777</td>
<td>1732.0</td>
<td>1664.7 ± 93</td>
<td>1572-1758</td>
<td>1677.8 ± 13</td>
<td>1665-1691</td>
<td>-54.2</td>
<td>-41.5</td>
</tr>
<tr>
<td>25Nc3</td>
<td>1580-1780</td>
<td>1680±100</td>
<td>1638.5 ± 86</td>
<td>1552-1724</td>
<td>1673.5 ± 26</td>
<td>1647-1699</td>
<td>6.5</td>
<td>-</td>
</tr>
<tr>
<td>25Nc1</td>
<td>1530-1730</td>
<td>1630±100</td>
<td>1603.9 ± 53</td>
<td>1551-1657</td>
<td>1667.0 ± 0</td>
<td>1667</td>
<td>37.0</td>
<td></td>
</tr>
</tbody>
</table>

Totals: -242.8 -1069.3
(N=15)  
\[
\bar{Y} = 16.186 \quad 71.286
\]
possible and the other represents dates based only on the mid-range values for all types. This makes possible a comparison of the effectiveness of mid-range and modal date results.

South tested his formula results in British ceramics by calculating the mean deviations of formula dates from the expected values (South 1972b: 216-8). A similar comparison of the Pawnee results is provided in Table 2. The mean deviation is -71.3 for dates calculated with the mid-range type values and -16.2 for dates based on modal estimates for the types. On this basis the modal dates appear to be the most effective for dating purposes, as expected. South uses similar data to support subtracting 1.0 years from formula results (South 1972b: 218). A +16.0 correction might improve these results but the calculation of more accurate modal date factors is a better approach for the future.

Another means of testing the calculated formula dates is to compare the date sequence with the known stratigraphic sequence of dated excavation units. There are 18 pairs of stratified excavation units in the original ceramic study (Grange 1968: Fig. 5). Each unit was dated by the ceramic formula method. In nine cases the formula dates were in agreement with the relative positions of the stratified units. In eight cases the dates were the reverse of known stratigraphic position and in one case the same date was found for each member of the stratified pair. These results are not as good as anticipated for the formula date as a technique of relative chronological ordering. However, several of the stratigraphic units had fewer than ten rimsherds in the pottery sample and are hardly reliable. Only nine pairs of stratified units had a sherd sample in excess of 50 sherds in one or both members. Of these six produced formula dates which were in agreement with the relative positions of the excavation units. A better case study, involving larger pottery samples, is needed to evaluate the formula dates in relation to stratigraphic sequence. In Table 4 all excavation units previously seriated (Grange 1968: Fig. 6) are re-ordered on the basis of formula dates.

The original study of Pawnee ceramics was an attempt to detect band (sub-tribe) units of Pawnee social organization in their ceramics. Some type frequency variations, and consequently some seriation positions of sites, are due to these cultural factors rather than to chronological differences. Local site sequences were therefore related to major subdivisions of the Pawnee tribe (Grange 1968: 131-45). In Table 3 the ceramic formula dates for sites are listed by band and local sequence in previously determined order (Grange 1968: Table 17). Here it is notable that the formula dates for only four of 24 site units are at variance with the earlier seriation and historical dating of these units. The discrepancies are underlined in the table. It may well be that these ceramic formula dates suggest better relative positions for 25Nc16 and 25Cxl than previously determined. With respect to the historic sites, 14Rpl and 25Wt1, the formula dates are "reversed" from the previously determined temporal placement of these sites. This could have some
Table 3. Modal Basis Formula Dates by Band and Local Sequence

<table>
<thead>
<tr>
<th>Site</th>
<th>History Date</th>
<th>Formula Date</th>
<th>Site</th>
<th>History Date</th>
<th>Formula Date</th>
<th>Site</th>
<th>History Date</th>
<th>Formula Date</th>
</tr>
</thead>
<tbody>
<tr>
<td>25Nc7</td>
<td>1844.0</td>
<td>1844.0±0</td>
<td>25Pk1</td>
<td>1832.5</td>
<td>1855.5±55</td>
<td>25Nc5</td>
<td>1832.5</td>
<td>1826.7±3</td>
</tr>
<tr>
<td>25Hw1</td>
<td>1820.0</td>
<td>1791.6±57</td>
<td>25Nc2</td>
<td>1825.0</td>
<td>1814.6±39</td>
<td>25Nt7</td>
<td>1825.0±0</td>
<td>1810.5±45</td>
</tr>
<tr>
<td>25Nc4</td>
<td>~1643.1±82</td>
<td>1638.5±86</td>
<td>25Bu2 (L)</td>
<td>1793.0</td>
<td>1806.2±29</td>
<td>25Nc2</td>
<td>1825.0</td>
<td>1814.6±39</td>
</tr>
<tr>
<td>25Nc3</td>
<td>1680±100</td>
<td>1644.8±74</td>
<td>25Bu1</td>
<td>1732.0</td>
<td>1683.9±94</td>
<td>25Nt1</td>
<td>1792.5</td>
<td>1824.6±23</td>
</tr>
<tr>
<td>25Nc16</td>
<td>~1644.8±74</td>
<td>1638.5±86</td>
<td>25Bu4</td>
<td>1732.0</td>
<td>1664.7±93</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>25Pt1</td>
<td>~1620.7±75</td>
<td>1617.8±77</td>
<td>25Cx1</td>
<td>~1600.5±66</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>25Pt13</td>
<td>~1630±100</td>
<td>1603.9±53</td>
<td>25Cx2</td>
<td>~1609.8±75</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>25NC1</td>
<td>~1630±100</td>
<td>1603.9±53</td>
<td>25Cx3</td>
<td>~1604.5±77</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Tappage River Sequence

<table>
<thead>
<tr>
<th>Site</th>
<th>History Date</th>
<th>Formula Date</th>
</tr>
</thead>
<tbody>
<tr>
<td>25Nc5</td>
<td>1832.5</td>
<td>1826.7±3</td>
</tr>
<tr>
<td>25Gal1</td>
<td>1811.0</td>
<td>1783.5±65</td>
</tr>
</tbody>
</table>

Skidi-Loup River Sequence

Grand Shell Creek & South Platte Sequence

Republican River Sequence

Blue River Sequence
bearing on the identification problems discussed earlier. However, a more likely explanation of the formula date discrepancy is that the late date for 25Wtl is due to the presence of Webster Bowl Ware. This is a late type absent from 14Rpl. The presence of this type in the 25Wtl collection could reflect a late re-occupation of the Hill Site at the same time as an adjacent site, 25Wtl, was occupied, a possibility which has been previously noted (Grange 1968: 143). If this is correct, the ceramic formula date discrepancy is the understandable result of mixing an early and late occupation at the site.

Thus, at the site-local sequence level the formula date method appears to be a reasonably effective seriation device. Since it has been shown that the rates of ceramic change were different for different Pawnee band-sequences (Grange 1968: 144-5), it could be argued that formula dating could be improved for each sequence by calculating modal type dates for each cultural sub-division of the tribe. This has not been tried.

Fitting has suggested that standard deviations should be calculated for ceramic formula dates (Fitting 1972: 161) and such calculations are provided for the 15 sites listed in Table 2 as well as in Table 4. The time range represented by each standard deviation is also listed in Table 2. The known span of occupation for these sites either falls within or partly overlaps the time range represented by the standard deviations of formula dates based on modal type dates. In contrast, only four partial overlaps are seen when known date ranges are compared with the standard deviation ranges based on the mid-range type date results. It is apparent in this sample that modal type dates are better than mid-range type dates for formula dating purposes. There may be some potential in the standard deviation as an estimate of an occupation span for sites (Fitting 1972: 161; South 1972b: 201). Whether or not this proves to be the case, the standard deviation is an important component of a ceramic formula date.

The ranges of the standard deviations of ceramic formula dates of the Lower Loup sites all fall within the same general time period. From this point-of-view the formula dates are all essentially the same and the results may be viewed as bracketing the period of the Lower Loup phase more effectively than they "date" individual sites with statistical validity. A similar pattern is seen in the historic Pawnee group of sites. Ceramic formula dates are estimates, not absolute dates, and should be used with caution as measures of relative chronology.

Another approach to the evaluation of the results of the formula dating experiment is through the calculation of a product-moment correlation coefficient 'r'. When the historic median dates are compared with the calculated ceramic formula dates (based on modal type date estimates), a correlation coefficient of .958 is obtained. Similar comparison of known dates calculated with mid-range type dates yields a correlation coefficient of .758. On this basis it can be asserted that there is a reasonably good correlation between the results of ceramic formula
dating and the expected date values. Further tests based on newly exca-
vated materials not utilized in the development of the type date estimates
would be desirable.

The results of formula dating are more closely correlated with the
expected or known values when the dates are based on modal type date
estimates. However, the correlation obtained for mid-range type dates
is good enough to permit the suggestion that dating with only mid-range
dates would be possible for aboriginal ceramic traditions just as it is
for European ones.

SUMMARY AND CONCLUSIONS

The problem undertaken in this project was the application of South's
ceramic dating formula to a non-European ceramic tradition. The Pawnee-
Lower Loup pottery tradition was selected for this purpose because the
data were readily available and because historical data could be utilized
in estimating the date ranges of pottery types and for testing the
results. Carbon-14 dates were also available for testing purposes.

Formula dates based on alternative type dates, mid-range vs modal
values, were calculated and compared. A good correlation between cal-
culated and expected dates was obtained on the modal type data basis.
Fair correspondence between stratigraphic position and formula dates
was found. Good results were achieved with date sequences compared with
local sequence site seriations. The method appears on this basis to be
potentially applicable to this non-European ceramic tradition. This
implies that it should be possible to develop applications of this method
to fully prehistoric contexts using tree-ring dates or carbon-14 dates
for type manufacture period and peak popularity estimates. It should
be possible in this manner to cross-date sites by the ceramic formula
method. The utility of the method for seriation purposes may become
greater in the future.

While mid-range type dates may be used, peak popularity or modal
dates for types will produce better results. It should be remembered
that, close as they may be to expected values, ceramic formula dates
are mean values and not absolute dates. It is recommended that standard
deviations be reported.

This attempt to apply South's formula dating method to an aboriginal
culture and its pottery tradition illustrates an important role of his-
torical archaeology as a laboratory in which techniques can be developed
and tested and then applied to other archaeological problems. It will
be interesting to see the results of attempts to apply the ceramic
formula method to fully prehistoric contexts.
Table 4. Chronological Ordering of Excavation Units by Mean Year
(Formula Date, Modal Basis).*

<table>
<thead>
<tr>
<th>Site/Excavation Unit</th>
<th>Ceramic Formula Date</th>
<th>Remarks</th>
</tr>
</thead>
<tbody>
<tr>
<td>25Nc7 Site Total</td>
<td>1844.0±0</td>
<td></td>
</tr>
<tr>
<td>25Nc2 H1 C2</td>
<td>1825.0±0</td>
<td>*Stratigraphic position</td>
</tr>
<tr>
<td>25Nc2 H1</td>
<td>1835.9±10</td>
<td></td>
</tr>
<tr>
<td>25Nc2 H2 C1</td>
<td>1825.0±0</td>
<td>Stratigraphic position</td>
</tr>
<tr>
<td>25Nc2 H2</td>
<td>1833.1±10</td>
<td></td>
</tr>
<tr>
<td>25Nc5 Site Total</td>
<td>1826.7±3</td>
<td></td>
</tr>
<tr>
<td>25Wt7 (P) Site Total</td>
<td>1825.0±1</td>
<td></td>
</tr>
<tr>
<td>25Bu1 H3</td>
<td>1788.3±32</td>
<td>Stratigraphic position</td>
</tr>
<tr>
<td>25Bu1 H3C4</td>
<td>1825.0±0</td>
<td></td>
</tr>
<tr>
<td>25Sd1 F7</td>
<td>1825.0±0</td>
<td></td>
</tr>
<tr>
<td>25Sd1 Pit</td>
<td>1825.0±0</td>
<td></td>
</tr>
<tr>
<td>25Wt1 Site Total</td>
<td>1824.6±23</td>
<td></td>
</tr>
<tr>
<td>25Ncl1 Site Total</td>
<td>1820.3±8</td>
<td></td>
</tr>
<tr>
<td>25Bu1 UN H2</td>
<td>1815.8±22</td>
<td></td>
</tr>
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* Stratigraphic units are in proper sequence despite ceramic formula date. Those units with "reversed" dates are designated "Stratigraphic position" in Remarks column. All other units are in sequence based on formula dates.
JOHN M. GOGGIN AWARD PAPERS - Grange

ACKNOWLEDGEMENTS

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