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The Conference on Historic Site Archaeology Papers 1975 - Volume 10

Stanley South
University of South Carolina - Columbia, stansouth@sc.edu

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The Conference on Historic Site Archaeology Papers 1975

Volume 10

PART 1 Presented Papers
PART 2-4 Historical Archaeology Forum
THE CONFERENCE ON HISTORIC SITE ARCHAEOLOGY PAPERS

1975

Volume 10

Part 1 - PRESENTED PAPERS at the Sixteenth Annual Conference
Gainesville, Florida

Part 2 - HISTORICAL ARCHAEOLOGY FORUM
Brer Rabbit, Skunks, and the Devil: The Dollar-Schuyler Debate

Part 3 - HISTORICAL ARCHAEOLOGY FORUM
The Methodological Frontier in Historical Archaeology

Part 4 - HISTORICAL ARCHAEOLOGY FORUM
Neighings, Brayings, and Quack, Quack, About A Man Called Horse:
The Dollar-Thurman Debate

Stanley South, Editor

Additional copies of this volume are available for $8.00 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
March 1977
THE CHAIRMAN'S REPORT

The Sixteenth Annual Conference on Historic Site Archaeology was held at the Gainesville Hilton, Gainesville, Florida, on November 6-7, 1975. Some of the papers presented at that time are published here as PRESENTED PAPERS. One group of papers was presented as a symposium chaired by Kenneth E. Lewis. These papers are published here as a FORUM.

Two papers by Clyde D. Dollar stimulated response from others, Robert L. Schuyler and Melburn D. Thurman. The resulting debates are presented here as separate FORA.

Papers submitted for consideration under the CONTRIBUTED PAPERS section of the Conference Papers for this volume will, instead, be published in Volume 11, in order to balance the size of volumes 10 and 11.

The John M. Goggin Award for Method and Theory in Historical Archaeology was offered in 1976, and the winning paper will be published in volume 11.

I would like to thank those at the Institute of Archeology and Anthropology at the University of South Carolina who have helped with the preparation of this volume. Typists assisting me in typing the manuscript are Sharon Howard and Nancy Goodyear. I would also like to thank Pam Stoops for assisting with proofreading. Particular thanks are due to Susan Jackson who helped with many editorial duties relating to this volume. I am also grateful to Maryjane Rhett, Executive Secretary of the Conference on Historic Site Archaeology for her handling of budgetary matters for the Conference, and for helping to see this volume to press. I would also like to thank Robert L. Stephenson, director of the Institute of Archeology and Anthropology for his continued support of the Conference Papers.

Stanley South, Chairman
Conference on Historic Site Archaeology
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PART 1

PRESENTED PAPERS

The following papers were presented at the Sixteenth Annual Conference on Historic Site Archaeology held at Gainesville, Florida on November 6-7, 1975.

Stanley South, Chairman
The Conference on Historic Site Archaeology
PARALLELS IN THE RISE OF THE 
VARIOUS SUBFIELDS OF HISTORICAL ARCHAEOLOGY

Robert L. Schuyler

During the late fourteenth and early fifteenth centuries a number of scholars in Italy made detailed architectural and epigraphical studies of a particular feature, the ruined but still standing Arch of the Roman emperor Trajan. Almost 400 years later in the 1890's a group of Anglo-American immigrants began preservation and restoration work on the site of New Helvetia in the Sacramento River Valley of California. Although this monument was in a state of extremely poor disrepair it had been standing within the memory of the people involved in its reconstruction. In fact, New Helvetia, or Sutter's Fort, was a major center for foreign activity in California during the late 1840's and early 1850's and so had barely been abandoned before it was again the focus of attention. Moving ahead another half century we find a group of aroused citizens in England waging a battle in the 1950's to preserve the Euston Station Portico, an early Doric style gateway dating from the opening era of the railroad age. Unfortunately their cause failed and the structure was demolished in 1962.1

What do these three events have in common? Historically they share few features. They are separated by geography, significant time spans, and in at least one case occur in qualitatively different cultural settings. Yet processually these events may be identical. What I am proposing in this brief and very tentative paper is that all the subfields of historical archaeology, irrespective of their specific historical setting, will tend in their development to pass through a series of five, broad predetermined levels or stages. This proposal is not completely speculative as it is based in part on a detailed study of one branch of historical archaeology, historic sites archaeology in North America, which I have just completed (Schuyler 1974). That review involved a detailed investigation of the rise of historic sites archaeology in the Far West and a more general national survey. However, because I have yet to do such a detailed investigation of any of the other branches of historical archaeology, my proposal has not been tested against independent case studies.

I will not attempt to summarize the development of historical archaeology in America, but based on that evolution I propose the following stages:

I The Study of Standing Monuments and Relics
II Excavation of Standing Monuments and Discovery of Associated Artifacts
III Broad Excavations and Use of Artifacts as Illustrators
IV Recognition of Cultural Context and Expansion of the Range of Sites
V Explanation of Recovered Cultural Context

1 I will not attempt to document many of the specific facts in this paper (see Schuyler 1974) as it is a tentative version of what will hopefully be a longer, detailed investigation.
Each historical archaeologist can consider this scheme and see if it fits their own experience; assuming they are old enough to have transcended several evolutionary stages. On the West Coast, for example, work at New Helvetia was soon followed by similar projects at the Franciscan missions. In fact, the opening stage of historic sites archaeology in several regions of America might be called 'Mission Archaeology' or 'Fort Archaeology.' Even the excavation of Johnny Ward’s Ranch was accidental, arising from the search for an early mission in Arizona. Such "standing monuments" are not always literally still on their feet. Yet even in those cases where they are buried, attention is usually brought to them via work at similar monuments that are still in part standing.

Paralleling this concentration on architecture may be an independent area of interest and expertise concerning relics. Such materials may be heirlooms or even be the product of random excavations, as seen in Renaissance antiquarianism, but they are removed from any meaningful archaeological context. In the West the early work of Arthur Woodward is a good example.

In Stage I two patterns appear, an interest in standing monuments and perhaps an interest in relics, but these themes are normally independent and disconnected.

Stage II - "Excavation of Standing Monuments and Discovery of Associated Artifacts" - is a product of the focus on standing monuments in Stage I. Restoration leads scholars in the direction of excavation as foundations are outlined. Such work may be done by the restoration committee itself or, if they are available, by professional archaeologists. Even attempts to merely "follow walls" cannot avoid encounters with contemporary artifacts. These may be ignored but usually are saved, especially if archaeologists are involved. An important nexus is thus created between the earlier separate themes of standing monuments and antiquarianism. Artifact experts are called in at this point, or arrive uninvited, and so add an element of expert knowledge.

In the West work at missions and forts, for example, soon drew the attention or helped to create the interest in those investigating "glazed ceramics," guns, beads, or roof tiles and other specific architectural items. Although very important accomplishments result from such contact, usually relating to individual features in restoration (such as tiles), the two themes of architecture and artifacts meet in Stage II but do not fuse in any meaningful way.

It is in the next or Third Stage - "Broad Excavation and Use of Artifacts as Illustrators" - that conflict first appears between the excavators and the restoration/preservation committees. This occurs because the professional archaeologists, as happened in the case of the National Park Service prehistorians in America, are not content with being limited to foundation trenching or because the discipline itself expands its horizons. Broad area excavations commence although these are still normally tied into building outlines. Recovered artifacts grow in
quantity and variety and those specifically related to architecture are put to analytical use. Total assemblages, nevertheless, are not used so much to interpret the site as to illustrate it. The museum case rather than the scholarly monograph is the benefactor.

Stage III produces a fusion of the themes of standing monuments and artifacts but this combination is technical and noncultural. Artifacts are viewed as secondary items appended to architecture and serving the goals of restoration.

Stage IV - "Recognition of Cultural Context and Expansion of the Range of Sites" - is the most crucial stage in the development of any type of historical archaeology. Indeed, it is only at this stage that a separate and autonomous area of scholarship appears. A significant break occurs in this phase because the study of artifacts recovered (with varying degrees of provenance) from a cultural setting eventually leads to the recognition that a total cultural context is preserved in the site. Architecture devolves to one of a wide range of aspects of culture, and as a result the types of sites investigated greatly expands. My survey of the rise of historic sites archaeology indicates that this change in America falls after 1960.

Resulting strains between the goals of restoration and scholarly research produce a crisis in this phase that may well lead to a rupture between various groups concerned with historic sites. On occasion the archeologists are able to reverse the relationship and slow down, control, or halt restoration, demanding that their interests take precedence on the site. If this is not possible then the mission or fortress may be abandoned for the company worker's house or the settler's cabin where few restorationists have ever tread. Since historic sites archaeology in America is primarily in this stage, rather than the succeeding one, we are all personally aware of the strains caused by such a transformation. Whether it is a Fortress of Louisbourg in Nova Scotia or a Hugo Reid Adobe in Los Angeles, the recognition of a true cultural context makes conflict inevitable just as it creates the possibility for the formation of a legitimate field of scholarly research.

Stage V - "Explanation of Recovered Cultural Context" - is only fore-shadowed in one subfield of historical archaeology and there it is a product of external rather than internal forces. It might be proposed that Stage V would be the natural and eventual development for each subfield of historical archaeology. Historically, however, this has not occurred, and the only subfield reflecting a complete evolution to Stage 5, historic sites archaeology, is obviously doing so because American archeologists are anthropologists. Perhaps one reason why such a development is not inevitable is that Stage IV, which could be equated in its fuller form with Binford's "reconstructing past lifeways," is a legitimate scholarly goal in itself and does not have to be processual in orientation.

Before we move to a discussion of how this proposed five-stage scheme appears in the rise of the other subfields of historical archaeology a number of complicating factors must be recognized. One of these, the problem of disciplinary context, we have just discussed. If a given subfield emerges in association with anthropology (a social science) or with the more humanistic disciplines can be crucial. Not only may Stage V be precluded or predetermined by such an association but the rate of movement from stage to stage can be retarded or accelerated along the entire scheme.
PRESENTED PAPERS - Schuyler

Trajan's Arch, the Acropolis, and the Forum found their equivalents in ecclesiastical sites dating from the Middle Ages. The study of armor and other specific categories are also newer forms of antiquarianism, although an emphasis on excavated ceramics is an exception. Medieval archaeology is hovering between Stages II and III. The medieval privy and pigsty has joined the cathedral, not without some violent reactions, but few studies reflect a recognition of a general cultural context (cf. The work of Lynn White 1964, who interestingly is quite anthropological in his approach).

Post-Medieval Archaeology

Excavations at medieval sites could not fail to frequently encounter superimposed, more recent, strata. This material and a parallel deeply rooted interest in artifacts and structures directly surviving from the post-medieval period, led in 1963 to the formation of the Post-Medieval Ceramic Research Group. Again based in England this group concentrated on British ceramics manufactured between 1450 and 1750. The former date is taken as a major transition between a medieval and a renaissance influenced ceramic tradition. The mass production of pottery began in 1750 (Barton 1968: 102-103). By 1966 it was apparent that many scholars with interests transcending ceramics were studying the same period. This led to the evolution of the Society for Post-Medieval Archaeology which issued its first journal number in 1968. Post-medieval archaeology is particularly interesting as it is the exact parallel of historic sites archaeology in America. Yet there are striking differences. Although the journal, Post-Medieval Archaeology, is one of the highest caliber, and excavation techniques are certainly equivalent to those in America, the concept of cultural context is just as clearly missing. Standing monuments are a primary focus but Stage I has been altered to some degree by the fact that the field had its roots in the study of ceramics and this has had positive effects. Most post-medieval archaeology would, I believe, fall into Stage III but tending more toward State IV than medieval archaeology.  

Industrial Archaeology

Between 1760 and 1830 much of the technological, social, political, and ideological aspects of English culture were altered in a manner that had no parallels in the immediate past. Thus England is considered by many scholars to be an industrial society after 1830, while other European nations were still totally preindustrial or just starting on the road to industrialization. It is not, therefore, surprising that the study of the material manifestations of this transformation should first make its appearance in the United Kingdom. By the 1950's it had become apparent to many concerned people in England (not necessarily professional archaeologists) that the continuing spread of industry and urbanism was inevitably obliterating earlier traces of the industrial revolution. In 1953 Donald Dudley, Professor of Latin at the

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2 I am presently undertaking a content analysis of journals such as Medieval Archaeology and Post-Medieval Archaeology. A major limitation on my interpretations, particularly in reference to classical and medieval archaeology, is that I have limited my review to English archaeology ignoring developments on the Continent.

5
Similar influential factors can be expressed as two questions:
1) Did a given subfield emerge before or after the rise of general (i.e. prehistoric) archaeology?
2) Did a given subfield emerge in the presence of another previously existing subfield of historical archaeology?

The second problem is similar to that faced by evolutionists in social anthropology. Certainly both factors would at least accelerate the rate of development and might even totally shortcircuit the system causing stages to be mixed or deleted.

Along with historic sites archaeology the other subfields of historical archaeology that are developed enough to merit recognition are: 1) classical archaeology, 2) medieval archaeology, 3) post-medieval archaeology, 4) industrial archaeology, and 5) a series of mainly unnamed areas of research such as the study of literate civilizations in India and the Islamic world.

Classical Archaeology

Classical archaeology is unique in that it was the first field in historical archaeology to appear and also predates prehistoric archaeology. Some scholars even see it as instrumental in the rise of prehistoric studies in Northwest Europe (Rowe 1965). I do not want to be unfair to our classical colleagues but I think, based on a superficial familiarity with the field, that much of classical archaeology falls in Stage II or at best Stage III. The study of standing monuments established the field as is seen in the publication of such 15th century works as Rome Restored and although the collecting of objects for their own sake has evolved into art history, the antiquarianism of the dilettanti is still very evident. Perhaps one of the reasons for the extremely slow rate in the evolution of classical archaeology is its temporal primacy which predates most modern scholarship in science and history and which has tended to some degree to isolate classical archaeologists from advances, especially on the theoretical level, by archaeologists in other fields.

Medieval Archaeology

Medieval archaeology has its roots in earlier developments in European archaeology. In England, the country that has seen the rise of other, more recent subfields of historical archaeology, antiquarian studies appeared early and developed into a local branch of classical archaeology. Since 1911 the Society for the Promotion of Roman Studies has continued this earlier tradition and directed research on the historical period of Roman occupation. Growing out of the formal structure of this society and related groups, as well as their field work, was an interest in archaeological remains postdating Roman Britain. In 1956 a Society for Medieval Archaeology was organized, initially under the honorary chairmanship of Sir Mortimer Wheeler, "to encourage the study of the archaeology of the period of the growth of the English nation" (Harden 1958: 1-2). Subsequent research has focused on the period of A.D. 400 to 1400 (or 1450) thus covering the "Dark Ages" as well as the full medieval time range.
University of Birmingham, proposed the term "industrial archaeology" (Hudson 1966: 11-14) which has since been used with many connotations all of which, however, can be covered in:

...a field of study concerned with investigating, surveying, recording, and, in some cases, with preserving industrial monuments (Buchanan 1970: 281).

Attempts at the preservation of items such as the Doric Portico mentioned at the start of this paper have been instrumental in the formation of over 40 local societies for industrial archaeology in Great Britain (Vogel 1969). By 1964 a national publication, The Journal of Industrial Archaeology, under the initial editorship of Kenneth Hudson, had been added to a long list of regional surveys and local newsletters dedicated to the subject. American scholars have been somewhat slow in following the British lead but in 1971 a national Society for Industrial Archaeology was established in a meeting at the Smithsonian Institution. Interestingly the English pattern was, however, repeated in that most of the society's members, including professionals, are not archaeologists (Vogel 1971).

A lack of professional archaeologists in this new discipline in both England and America is also reflected in the dearth of excavations at industrial sites. This situation has led to a series of debates on the nature of industrial archaeology between anthropological archaeologists (Foley 1968, 1969; Lee 1972; Schuyler 1972, 1975) and other scholars (Vogel 1969). The anthropologists have urged that the definition of the field be expanded to include both field excavation and the total range of material culture above and below ground. These discussions and the handful of general excavations at industrial sites, such as Edward Rutsch's work in Patterson and my project in Lowell, do not, however, alter the fact that industrial archaeology is still in the main in Stage I and many industrial archaeologists even take pride in this fact.

Conclusion

When the present levels of development of the various subfields of historical archaeology are compared (Fig. 1) it is evident that longevity is not correlated with the level or rate of evolution. Except for industrial archaeology, classical archaeology, the oldest subfield, is the most underdeveloped. An explanation for this situation may be found in the question posed in the text. Since the roots of classical archaeology extend back to a period predating the establishment of modern archaeological field techniques the impact of this methodology was delayed and indirect while other factors (especially the emphasis on architecture and "art objects") established the field's intrinsic orientation. This indirect relationship continues and may to some degree explain the embryonic state of classical archaeology. In contrast a field like post-medieval archaeology emerged long after the rise of modern archaeological methodology and thus directly took it over and as the field developed remained less isolated from current advances.
A COMPARISON OF THE EVOLUTION OF THE VARIOUS SUBFIELDS OF HISTORICAL ARCHAEOLOGY

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<th>STAGE</th>
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<th>Post-Medieval Archaeology</th>
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Figure 1
Another factor is the impact of disciplines external to archaeology. Certainly the influence of anthropology is one of the main catalysts in the rapid transformation of historic sites archaeology. An even more interesting case is that of industrial archaeology. As this field was the last to appear it entered a scholarly setting occupied by several "archaeologies" some of which, particularly post-medieval archaeology and historic sites archaeology, overlapped with its subject matter. The impact of anthropology on industrial archaeology is already recognized in America and it will probably cause a unique short-circuiting of the evolutionary process. There are some indications (as seen in recent meetings of the Society for Industrial Archaeology) that the acceleration may be so rapid that the field may fragment with a monument-preservation group splitting off to purposely limit their version of industrial archaeology to Stage I.

In England the conflict over industrial archaeology is not so intense or at least it is primarily concerned with the question of whether such a field is needed rather than the nature of the field. Nevertheless it is quite interesting that the one clear case of a similar impact to that of anthropology in America also derives from the social sciences. This exception involves the attempts of Robert A. Buchanan (1972), an economist and economic historian, to define the field more broadly and to put it into some meaningful social-cultural setting.

Without a scientific orientation deriving from fields like anthropology and economics, there would be no reason why the evolution of any subfield of historical archaeology would not terminate at Stage IV.
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FORT LOUDOUN EXCAVATIONS: 1975 SEASON

Carl Kuttruff and Beverly Bastian

Fort Loudoun is located approximately two miles northeast of the present town of Vonore, Tennessee, or approximately 45 miles southwest of Knoxville, Tennessee, on the Little Tennessee River. It was the westernmost fort of a series of fortifications extending from Charleston, South Carolina, including Fort Prince George and Ninety Six. After nearly ten years of consideration, construction of Fort Loudoun began in October of 1756, and was essentially complete by July 30, 1757, as indicated by a letter of that date from Raymond Demere, commander of the garrison, to Governor Lyttelton of South Carolina (McDowell 1970: 391). The Overhill Cherokees in the Little Tennessee River Valley wanted the fort to be constructed as a refuge as well as a constant source of supply for trade goods. The English needed the fort in that area to deter possible French encroachment from the south and to solidify their alliance with the Cherokees (Kelley 1958: 5-12; 1961 303).

Two companies of provincial militia and one company of British Regulars, commanded by Captain Raymond Demere, were engaged in the construction of the fort. John William Gerard DeBrahm was responsible for the final selection of the location and the supervision of the construction, although he abandoned the project in December of 1756, leaving instructions with Demere for the completion of the fort. Raymond Demere completed the fort with some modification and additions to the original plans set forth by DeBrahm (Figure 1 and De Vorsey 1971: 105). The final construction consisted of a diamond-shaped log palisade with four bastions, an embankment of dirt on the outside of the palisade line, and a surrounding ditch or dry moat (Figure 2). The status of the final construction of the outworks (Ravelin Lyttelton and Fort Glen, shown on both plans by DeBrahm) is uncertain, although they were at least begun as noted in a survey of the construction dated December 23, 1756 (McDowell 1970: 384-386). Other constructions within the fort, known by the contemporary documentation, include gun platforms in the four bastions, barracks for the officers and men, store houses, a blacksmith shop, powder magazine and a guardhouse. It is also inferred that there were other related housing and buildings within the fort, and in the vicinity of the fort for families of the men stationed there and possibly others (McDowell 1970; Kelley 1961: 303-304; 1958: 12-23).

Captain Paul Demere replaced Raymond Demere as commanding officer of the fort in August of 1757 (McDowell 1970: 396-404). The two companies of militia had left by that time, and the fort was then manned by only one company of British Regulars. Relations with the Cherokee remained relatively friendly through the fall of 1759, after which time the Cherokee began to pressure the garrisons at Fort Loudoun and Fort Prince George. During the winter of 1760 the pressure by the Indians gradually increased, and throughout the spring and summer the
Figure 1. Plan and Profile of Fort Loudoun by William G. DeBrahm
Reproduced by permission of the Huntington Library, San Marino, California.

Figure 2. Aerial view of Fort Loudoun showing excavations in progress. Top of photograph is to the southwest. Photograph courtesy of the Tennessee Valley Authority.
siege was tightened to the point that Demere decided to surrender the fort to the Cherokees in early August 1760. On August 9, 1760, the garrison began their march back to South Carolina and the following morning were ambushed by the Cherokees about 15 miles south of Fort Loudoun (Kelley 1958: 21, 28-33; 1961: 305-306).

The disposition of the fort after the British surrender is not as well documented as the period prior to its abandonment. To date, no documents have been located that describe the fort when it was abandoned, or during the period immediately following the takeover by the Cherokee. It was apparently occupied for some period by the Indians, and possibly destroyed by the British after it was surrendered by the Cherokee in 1761 (Hamer 1925: 38). Haywood in 1823 indicated that the fort was broken up by the British before they left, but provides no source for this information (Hawood 1823: 44). Henry Timberlake, who traveled through the area in 1762 passed the fort or its remains, and shows it on his "Draught of the Cherokee Country," but gives no other details on the condition of the fort (Williams 1927). In November 1762 the South Carolina Assembly used the existence of the fort in support of arguments for a more northern boundary between the Carolinas. Three years after the surrender of the fort, Lord Egremont referred to the existence of Fort Loudoun and others in a letter to Governor Dobbs of North Carolina. Other mentions of the fort are recorded without elaboration in 1797 and 1799 (Kelley 1961: 307-308). A version of the "I. Christie Account" printed in 1847 also notes the remains of the fort (Anonymous 1847).

The Cherokee held the land that the fort was situated on until 1819 when it was ceded as part of the Hiwassee Purchase. The land transfers since that time, as well as the events leading up to the preservation of the fort and its present day reconstruction, are detailed by Paul Kelley (1958; 1961).

Extensive excavations and other more limited tests have been carried out at Fort Loudoun since 1936, but there is a minimum of documentation on those excavations. This is due to the general lack of records for the WPA excavations, and some loss of notes, artifacts, and photographs in the fire that destroyed the Fort Loudoun Museum in 1972, as well as a scattering of the records over the years. Efforts are being made to assemble any extant information remaining from the previous excavations, and will continue for the duration of the project. The best source for this information at the present time is a summary report written by Peter Kunkel prior to the 1972 fire when the information on the earlier excavations was still available, and Kunkel's field notes and records from his excavations in 1958 (Kunkel 1960). The latter were located in the Fort Loudoun Association files in 1975 and are available for the project.

In 1936 and 1937 extensive excavations were carried out by WPA crews under the direction of Mr. Hobart Cooper. The extent and nature of the excavations is not entirely known, although they apparently consisted of a series of narrow trenches (see Figure 3) to locate and follow the palisade line(s), and other features (Richard Polhemus, personal communication, 1975; Kunkel 1960:21; Myers and Polhemus n.d.).
Kunkel, who had access to various maps, drawings, and photographs from the WPA excavation, which were on file with the Fort Loudoun Association, indicates that several features relating to the fortification were exposed by that project, including the main palisade line, the original earthwork, the inner palisade line, and the postmolds for the gate on the river side of the fort. A cross section of the earthwork on the south side of the fort, and an unspecified amount of the ditch and counterscarp were also excavated. Features excavated on the interior of the fort included nine fireplace bases, and other structural features in and around the enlisted men's barracks on the west side of the powder magazine in the King's Bastion, and the fireplace base of Paul Demere's house located on the northern and upper part of the fort near the north curtain (Kunkel 1960: 7-12; Kirkland n.d.).

During the period of 1957 and 1958 Ellsworth Brown, then Research Director of the Fort Loudoun Association, assisted by A.C. Grist, Jr. and Bennett Graham carried out a series of excavations aimed at solving certain specific problems. A trench in the King's (Northwest) Bastion defined the former parade level in the apex of the bastion and located one postmold that was probably part of a gun mount or gun platform. Another trench excavated during the summer of 1958 extended on a diagonal for 63 feet from the apex of the Prince of Wales (Northeast) Bastion towards the apex of the Duke of Cumberland (Southwest) Bastion. This excavation revealed the original parade level in the Prince of Wales Bastion, as well as an area of charcoal, ashes, and red clay which was probably structural debris. In the upper or northern part of the fort, Brown determined the probable dimensions of the Demere house. Brown reexcavated the postmolds of the river gate that had been located and mapped by Hobart Cooper in 1937, the guard house that had been partially excavated by the WPA crew, as well as the contiguous Officer's of the Day quarters (Kunkel 1960: 10-12).

In 1958 Mr. Peter Kunkel carried out a series of excavations in and outside the fort including the Queen's (Southeast) Bastion, the barracks area in the western part of the fort, the Duke of Cumberland Bastion, the King's Bastion, and the Fort Glen area on the riverside of the fort. Work in the Queen's Bastion area was aimed at the location and definition of the smith's shop. No firm structural evidence was found for this structure, except one postmold; several iron artifacts and concentrations of rocks were noted and may have been indicative of the smith shop (Kunkel 1960: 15-17; J. H. Polhemus 1966-1967: Field Notes). Another area tested was the barracks area, around the previously excavated chimney bases in the western part of the fort. Extensive excavations in this area revealed a number of structural features, including one postmold and several sills, stone lines and unlined drains, midden deposits, structural debris and some evidence of WPA trenches (Kunkel 1960: 17-21).

Several test trenches were placed in the area of Fort Glen on the river side of the fort. Two tests were dug in the swale between the bank of the Little Tennessee River and the slope of the first terrace, but no evidence of historic occupation was noted. The third test on the first terrace near the upstream property line revealed structural evidence in the form of a palisade line or timbered outwork and midden materials.
Figure 3.

View southwest across Queen's Bastion showing excavation and narrow WPA excavation trenches.

Figure 4.

Backhoe excavation of the moat on the south side of the Queen's Bastion.

Figure 5.

View south showing excavated parts of the moat on the east side of the Queen's Bastion.

Figure 6.

View north showing detail of the moat on the east side of the Queen's Bastion.
Two small tests in the Duke of Cumberland Bastion dug during the winter of 1958 revealed what was probably the parade level with evidence for a burned structure or gun platform. Finally, tests in the King's (Northwest) Bastion revealed three clusters of flat limestone slabs in clay matrices which have been interpreted as fragments of walkway (Kunkel 1960: 21-22).

In 1966 and 1967 Richard Myers and James H. Polhemus carried out some excavations in the Queen's Bastion area to supplement the work done by Kunkel, and to attempt to determine the location and structural details of the smith's shop. Several features were located and recorded including post molds, a hearth and two other features from the period of the fort's occupation. The important discovery of the excavation was that it showed that there were undisturbed structural features remaining in the area of the Queen's Bastion. In 1966 a grid of magnetic readings was made over the area of the guard house near the river gate. A small test pit was made immediately east of the chimney foundation of this structure to determine the reason for high magnetic readings at that location. A well preserved bullet mold and evidence for two floor levels below the present surface were located (Myers and Polhemus n.d.; Richard R. Polhemus, personal communication).

In September of 1973 the Division of Archaeology excavated two test trenches across the first and second terraces in the area of Fort Glen on the river side of the fort. The purpose of the tests was to determine whether there were any structural features located in this area. Both trenches revealed what was probably an earthwork or parapet in the approximate location corresponding to the east edge of the outwork on DeBrahm's plan of the fort (Figure 1 and De Vorsey 1971: 105). In the east end of the southermost trench, several burned logs were located which were presumably some of the structural timbers associated with this outwork.

In July of 1974 Richard R. Polhemus tested four corehole stations south of the present fort. The location nearest the river, southeast of the Queen's Bastion produced evidence of prehistoric and historic occupation in the form of Woodland and Hiwassee Island cultural materials and a ditch or palisade line parallel to the east face of the Queen's Bastion. The only other cultural material excavated was at the second location back from the river, and that consisted of flint chips in the plow zone (Richard R. Polhemus letter to Corydon W. Bell, Jr., January 14, 1975).

The Division of Archaeology conducted similar tests at several drill stations along the proposed location of the dike south and west of the fort. All were sterile with the exception of the station nearest the river where the recovery of several unmodified flint chips in the plow zone verified the results of Polhemus's test at this location.
The construction of the Tellico Dam on the Little Tennessee River near Loudoun, Tennessee, and the proposed January 1977 closing date for that dam will flood large sections of the Little Tennessee and Tellico Rivers and their tributaries. Salvage excavations are currently being carried out at Fort Loudoun by the Tennessee Division of Archaeology, financed by the Tennessee Valley Authority. While only part of the fort and its outworks will be inundated, the fort will be completely excavated. This will insure that all materials and information remaining at the fort will be recovered, since they will be endangered by construction activities in the area of the fort, which are going to consist of raising the existing contour of the fort approximately twenty feet with an earth fill.

The 1975 excavations began on the 28th of May and continued through December 19th. Approximately 3400 square meters of the fort and surrounding areas were excavated during that period. Artifact processing and cataloging ran concurrently with the excavations, at the Fort Loudoun laboratory in the Anthropology Department at Vanderbilt University. Excavations were begun in the area that was traditionally thought of as the cemetery, either for the three individuals of record that died at the fort, or for later historic periods. Several trenches excavated by Kunkel in the 1960's were located, as well as one small Cherokee cooking pit, and a sparse scatter of fort period artifacts.

The ditch and parapet between the King's Bastion and the cemetery were cross-sectioned, confirming that the existing cut bank, ditch and earthwork are the remains of the original construction of the fortification works in this area. The existing slope of the ridge was cut to form the ditch or dry moat, with the earth from the ditch used to add height to the slope against the main palisade line. A lens of fort period trash was located at the base of the ditch against the lower part of the counterscarp, attesting to the lack of modification of that ditch since its original excavation.

Similar cuts extending from the south and east curtains of the Queen's (Southwest) Bastion located the surrounding ditch on those sides, as well as the stratigraphy within the ditch and the adjacent parapet. Work with a backhoe continued the sectioning of the various facets of the parapet and adjacent moat on the east, south and west sides of the fort. Large sections of the moat were then excavated with the backhoe and a shovel crew, revealing the southeast and southwest corners of the moat and large sections of the moat on the east, south and west sides of the fort (Figures 1, 4, 5, and 6). The ditch fill on the several areas sectioned and excavated consisted of several zones of post-fort period fill, and several WPA trenches within the ditch and parapet, indicating that the WPA project had in fact located and defined large sections of the moat, although they apparently did not attempt to excavate any large areas of it.

Several prehistoric pit features were defined and excavated within and adjacent to the ditch on the south side of the fort. Stratigraphy within the existing parapet revealed a subsoil and original humus zone with several prehistoric features, mainly postmolds. Immediately above these zones were the remains of the original earthwork as it existed in 1936, followed by a zone of WPA backdirt used to heighten the parapet at that time, and a
subsequent addition of fill from the 1960's when the palisade was reconstructed. The cross-section trench on the east side of the Queen's Bastion was extended through the existing palisade line, but no evidence was found in that cut to reveal the precise location of the original palisade line. While it is felt that the existing reconstruction is substantially correct in its location, efforts will continue to document its authenticity.

The Fort Glen and Ravelin Lyttelton areas on the riverside of the fort were extensively tested, but the results of those excavations are presently inconclusive. It is possible that the work revealed what was the partial construction of a parapet or other earthen embankment for Fort Glen, but it does not have the appearance of ever having been completed. Work in those areas will be continued during the 1976 season to determine precisely what construction was done on Fort Glen.

Approximately 2000 square meters have been excavated within the interior of the fort, particularly in the southern half, but also on the slope in the northern part of the interior area. In the Queen's Bastion area the remains of at least four structures have been defined and excavated. Two are located along the east curtain of that bastion, one along the south curtain, and the fourth is situated in the apex of the southeast bastion of the inner palisade line. One partially defined structure has been located in the central part of the fort area adjacent to what was probably the parade ground. Several incompletely defined structures in the western part of the lower fort area have been located in the vicinity of several partially reconstructed chimney bases.

The first structure was a large rectangular building situated between the inner and outer palisade lines along the east side of the Queen's Bastion (Figures 7, 8, and 9). Within that structure was a root cellar or other similar subsurface feature (Figures 9 and 10), and the remains of a chimney base. The structure had apparently been divided into two rooms.

The second structure was of similar construction, was somewhat wider, and also parallel to the east side of the Queen's Bastion. This structure shared a common wall with structure one, and also had an interior partition, and a large chimney base situated at the northern end (Figures 8 and 11). The third building, located along the south wall of the Queen's Bastion was apparently constructed similarly to those of the French, with the walls consisting of vertical posts set in a trench. Within the structure was a large pit feature that had been filled with trash (Figure 12).

The inner palisade line shown on DeBrahm's map (Figure 1) has been defined in the Queen's Bastion area, along the south curtain of the fort, and in the southwest bastion area. This feature had been at least partially excavated by the WPA project in 1936. Figure 13 shows the southwest bastion of the inner palisade line in the Queen's Bastion, as well as the postmolds associated with the structure that was built within the apex of that interior bastion. That structure was probably a five-sided structure,
Figure 7. View northwest across Queen's Bastion showing excavations in progress.

Figure 8. View southwest across Queen's Bastion.
Figure 9. Structure 1 during excavation showing postmolds and unexcavated root cellar.

Figure 10. Detail of root cellar in Structure 1 after cross-sectioning.

Figure 11. Detail of chimney base at the north end of Structure 2.
Figure 12. View south showing Structure 3 and interior pit feature. Structure and pit are cut by a WPA exploratory trench.

Figure 13. View southwest across Queen's Bastion showing southwest bastion of inner palisade line and postmolds of the structure located in the apex of that bastion.
and the archaeological remains correspond very well with the same structure shown on DeBrahm's plat. The pointed end of this structure was most likely of horizontal timber construction, forming the apex of the interior palisade. According to DeBrahm's map this structure was a blacksmith's shop, but the major concentrations of slag recovered by the project centered around the large pit feature in the first structure noted above.

Additionally, the excavations in the Queen's Bastion area have revealed numerous postmolds and pit features associated with earlier prehistoric occupations of this site. In the western part of the fort numerous postmolds, sill molds, and one stone lined drain (previously excavated by Kunkel), relating to several structures in the area of several partially reconstructed chimney bases have been located. The excavations were terminated before these features could be completely defined and excavated.

Although the excavations are only partially completed, the work that has been accomplished so far has shown several important things. Despite the great amount of previous excavation at the fort, most, if not nearly all of the structural features and other subsurface features are virtually intact, with what amounts to a minimum of disturbance. How the prior work has affected the artifact recovery has not yet been ascertained, but is thought to be within tolerable limits. It will be possible to reconstruct to a large extent the WPA excavations, and elaborate on the findings reported by Peter Kunkel and other excavators. The excavations of Fort Loudoun will also provide some of the information necessary for a total reconstruction, much of which data is not in the extant contemporary documentation. Additionally, the salvage of Fort Loudoun will consist of something approaching a one hundred percent sample of the remains of a mid-18th century fort. This body of information will be important not only of itself, but also useful in studies of the contemporary Cherokee materials from the Little Tennessee River Valley that have been excavated over the past several years by the University of Tennessee Museum, as well as other contemporary forts which have been excavated in South Carolina and elsewhere.

ACKNOWLEDGEMENTS

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A PRELIMINARY STUDY*

Jean-François Blanchette

The aim of this paper is to show, contrary to previous examinations, that permanent French Canadian fishing settlements, (not only seasonal nor merely French occupations) existed in the Gaspé Region during the last century of the French Regime (1713-1758). My proof is principally based on archaeological evidence, but the framework for this study was established by the use of historic manuscripts and additional printed sources. This paper permits the generation of the following hypotheses for further archaeological researches:

1- The production of certain red earthenwares from sites in New France occurred in New France.

2- a) The ethnic identity of the people who lived on the sites discussed in this paper was French Canadian.

b) The inhabitants were of the lower class of the French Canadian society.

c) Their settlement was permanent.

d) Subsistence was primarily based upon fishing.

This paper is not designed to give a definitive view of the history of the XVIIIth century of Gaspé and Forillon. The study is part of an archaeological survey of the Forillon National Park and based upon selective rather than extensive excavation. The conclusions presented here are of a preliminary nature. Nevertheless, the paper should give some indication of the potential for a more intensive documentary and archaeological interdisciplinary study of this area.

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Geographical Location

The general region with which this study is concerned is located at the tip of Gaspé Peninsula (Québec) on the western limit of the Saint Lawrence Gulf. More specifically, the area to be discussed includes the region surrounding the Bay of Gaspé and the territory called Forillon, located to the North of the bay. Gaspé Bay is arranged in the form of a triangle. Its western point is located sixteen miles from the open ocean and its mouth is 7 1/4 miles wide. It is a hilly land, having few terraces and few slopes (Figures 1 and 2).

The History of the Region

As early as 1663, governor Dubois Davaugour wrote Louis XIV, recommending that a settlement be created at Gaspé. Reasons given were "la bonté de la rade et du port et l'abondance de la morue" (RAPQ 1922-23: 84). The area was already known by this time, because a seignory stretching from Forillon to the Baie des Chaleurs (3,810,240 arpents) had been conceded to eleven associates in March of 1652. However, there were no habitants in the region by 1663 and no indication that the governor's recommendations had any favorable effects for the future (Trudel 1973: 517-518, 524).

Prior to 1697, French fisheries were concentrated on the Newfoundland and Acadian coasts, and also along the interior Gulf of Saint Lawrence. However, although it supported a sizeable summer population, no permanent fishing settlement existed in the Gaspé Region. In 1688 for example, 200 Frenchmen were fishing in the region and settled for the summer between Grande Grave and Penouille (Lelievre 1973: 20). Similarly, Sir Hovenden Walker stopped at the Bay on his way to Quebec in 1711. He did not find any permanent settlement, but "...only one French fishing vessel which he burned" (Lee 1970: 30). The closest permanent settlements during the XVIIth century were at Percé and Bonaventure Island, located to the South of Gaspé Bay. The fishing rights for these settlements belonged to Nicholas Denys from 1653 to 1687 (DBC I: 265-6). They were destroyed by English privateers in 1690 and no further settlement was established for the subsequent 23 years, in fear of raids. The French preferred to send seasonal fishing expeditions, which arrived early in the spring and departed at the end of the summer.

In 1697 and 1713, the Ryswick and Utrecht treaties transferred Newfoundland and the territory of present-day Nova Scotia to England. Maritime resources hence became scarce for French fishermen who were constantly threatened by English privateers. As a result, plans for the construction of Fortress Louisbourg were projected for the purpose of protecting the Grand Bank fisheries and of serving as a port of call for the West Indies ships and as a permanent port for the ships of the Marine Royale which patrolled the Gulf (Larabbee 1971: 8).
FIGURE 1. Location of the Gaspé Region.

FIGURE 2. Map of the Gaspé-Forillon Region showing sites Penouille 1 and 3.
Only one document is explicit about the importance that Gaspé and Forillon had after the loss of the Nova Scotia fishing settlements by the Utrecht Treaty in 1713. This document, dated 1732, as issued by the Nova Scotia Council, states: "Ever since the French were drove out of Canso [1713]...They have settled a Great fishery at Cape Gaspy in his Majestys Dominions, where they have been unmolested for these several years past" (in Lee 1970: 30). This statement has been interpreted as applying only to seasonal fishing settlements (Ibid.) but the rise of small industries in Gaspé Bay and neighbouring areas seems to indicate that the fishing settlements of Gaspé were becoming permanent. In October 1726, a saw mill was built to cut pine and spruce for planks, sheatings, and masts. Elm was also processed for the construction of gun carriages and ship pumps (Lelièvre 1973: 20). A slate manufactory was set up at Grand Etang, some miles northwest of Forillon (Lee 1970: 35). Low quality roof slate, as are the pieces found in Penouille sites, was probably produced at Grand Etang from 1729 to 1734. In addition to the above industry, a forge was built in the Gaspé area along the Darmouth river (Bell in APC, Coll. Northcliffe, M.C. 28, M63, reel C-370).

Permanent settlements were developing in the Gaspé region, but at the same time, a sizeable number of Canadians were still going to Gaspé seasonally, between sowing and harvesting times, to fish cod. In 1734, the number of fishermen was so high that "the price of cod lowered to 8-9 livres a quintil." Populations other than Canadians fished seasonally in the Gaspé area. It seems that after 1713, the French Basque preferred this area to all others (Lelièvre 1973: 20) and that, if they did not settle permanently, they at least constantly went there. In 1724, there were eleven boats in the bay (Lee 1970: 47). By 1740, the number had increased to approximately 50 boats. At the latter date, Acadians asked that lands be given to them in Gaspé to be able to fish without fear of English molestation (Lelièvre 1973: 21). By 1745, there were 360 fishermen in the Bay (Ibid.) thus indicating the rapidly increased awareness of the richness of the fisheries in the Gaspé area.

When the War of the Austrian Succession erupted in 1744, it was decided to post sentinel at Cap des Rosiers. A Canadian named Arbour "was reported to be cultivating wheat, buckwheat, hay and various vegetables with some success. He is noted as a permanent resident of the bay..." (Lee 1970: 49). In 1745, "a ship and militia captain, Jean Barré, settled at Gaspé with a schooner and 23 boats. On July 2, 1747, at the head of 17 men, he drove off an English attack on Penouille [sic], killing 11 Englishmen and wounding 25 others" (Lelièvre 1973: 84). Barré was still in Gaspé in 1753; in a contract from Louisbourg, he was described as "habitant etably à Gaspe dans le golfe Saint-Laurent (ANO, G3, Vol. 2041-2, 1 août 1753). He settled in Louisbourg in 1754 as a resident merchant until his death in 1757 (Christopher Moore, personal communication, June 4, 1975). Some years after the war, the Gaspé fisheries witnesses new developments. "Fishermen descendants, coming from Bayonne and Sables d'Olonne, the Denys, the Hubert, the Lefebvre, migrated and settled in the region" (Lelièvre 1973: 21; translation by the author).
In 1752, the merchants Revol and Arnoux settled in the region of Gaspé Bay. Revol obtained fishing rights to the area for a low amount paid to the crown thereby becoming the lord of the Gaspé fisheries. From 1752 to 1755, these merchants settled some forty inhabitants in the region (Lelièvre 1973: 21). In 1758, there were some 60 fishermen under the Revol's tutelage. They were required to give him a predetermined quantity of fish annually. He, in return, distributed some money, but mostly gave the fishermen merchandise (APC in Lelièvre 1973: 93).

The economy of the Gaspé region is historically known to have been centralized on cod, a fish which has a high protein value (Lee 1970: 32). The fishermen's diet was not restricted to it however. In a wild region such as Gaspé and Forillon, hunting was probably always a complementary mode of subsistence.

Gardens probably provided the essential vegetables. In coves, small valleys, on the littoral, and in the rare locations where abrupt hills gave way to mild slopes or plateaus, portions of the land were cleared. Wheat, buckwheat and hay were sown (Lee 1970: 49). However, the growing season was quite short and agricultural production was certainly relatively modest. It is suspected that all that the fishermen could not produce themselves was acquired through the coastal lord who owned the fishing rights in the area. It was from his store that the settlers received wheat, peas and pork when these products were available. There too, fishing boats and equipment and household wares were bought.

The fisheries in Gaspé region were fruitful when General Wolfe took over the Gaspé settlement in September of 1758, after the surrender of Louisbourg. There were then three hundred permanent inhabitants. Two documents, a map, and a print, giving a very precise idea of the bay and the life of its settlers, at this time, have survived the centuries. They are abstracted and commented upon here.

The first manuscript is the journal of Captain Bell, General Wolfe's aide de camp. This journal contains a map which identifies various locations (Figure 3):

1- Isle Bonaventure, wood
2- Isle Percé, wood
3- Flat Island...
4- Grand Grave
5- A little cleared spot where Anstruter Regiment encamped. Fish used to be dried here.
6- Another cleared spot where General Wolfe and part of Bragg's (Regiment) encamped
7- Part of Sand bank dry at high water; fine mackreel on it
8- Not to be seen at high water
9- Peninsula or Gaspee
10- The two arms
11- A cleared spot with 3 hutts on it
FIGURE 3. Map of Gaspé-Forillon drawn on location by Captain Bell in September 1758. (Public Archives of Canada, Coll. Northcliffe, MG 28, M 63, roll C-370)

The text itself gives abundant information about the settlement.

To judge from the list of what was taken or destroyed by the English in Gaspé Bay, this settlement was quite prosperous. Revol's settlement consisted of a house, store and warehouses, located on the southern shore of the Gaspé Basin. These, and numerous fishermen's houses located around the bay, a forge at Darmouth River, a saw mill at York River, four schooners and about 200 small boats, fishing equipment, gun powder, provisions of brandy and pork, a few cattle and fowls and 3,000 quintals of fish (6,000 according to Lee 1970: 51) worth approximately 120,000 livres on the Quebec market were destroyed or taken by the English (Ibid).

Of considerable descriptive value is a print used for many years as a document in geographical orientation and in history for the Bay of Gaspé (ANQ, GH 272-70) (Figure 4). This print, published by T. Jefferys in London in 1760 was engraved by Peter Mazell from a drawing made at the conquest of Gaspé by Captain Hervey Smyth (of Wolfe's Army). Written at the bottom of the print is, "A view of Gaspé Bay in the Gulf of St.-Lawrence. This French settlement used to supply Quebec with fish until it was destroyed by General Wolfe after the surrender of Louisbourg in 1758." Located in the middle of this illustration is a two story house, double with "tambour," set on the beach. This was Revol's house and store. To the right, close to the forest, there are four small huts, probably fishermen's houses. On the point, there are three circular structures (possibly fish smoke houses) which may have contained 1,500 quintals of fish, according to the vignette (The Bell's journal indicates however 3,000 quintals).

This print was interpreted by previous historians to be of Penouille (drawn from point A on Figure 2). The topography of Gaspé Bay is not consistent however as the bay is quite narrow and the mountains are too close. The author does not believe that the peninsula represented is actually Penouille. Comparing this print to Bell's map and journal described above, it can be seen that in 1758 Gaspé (where Revol had his house) was also called Penisle (peninsula) (item 9) and was located on the southern shore of Gaspé Basin in the present-day town of Gaspé. It is hence suspected that this print is a reversed reproduction of Smyth's original drawing. It seems that such inversions often occurred in the XVIIIth century (see Noël Hume 1970: 42; Hind 1963: 1-18). Many of these prints from engravings were "popular views" having no other purpose than
to arouse public interest in the activities of the English Marine in North America. Were one to both reverse this print, to get the original positive view, and interpret the title "Bay of Gaspé" as Basin of Gaspé, the print fits very nicely with Bell's map and even the actual topography of the area (direction B, Figure 2). This is relevant to our archeological study since Revol's two story house and store was originally believed to have been in Penouille where two sites of the same period were found. However, as indicated by Bell's map and journal these two sites and Revol's settlement were located on the opposite side of the bay (beneath what is presently the Gaspé railway terminal).

The Archaeology of a Permanent French Canadian Fishing Settlement

Historical documentation indicates that seasonal fishing settlement existed in Gaspé Bay during the XVIIth century and the beginning of the XVIIIth century. It is probable that these settlements, due to their temporary nature, would leave minimal archaeological remains, and as expected none of these settlements have as yet been discovered.

However, two sites dating from c. 1720 to 1758 have been located in Penouille (Figure 5), and it is believed that they represent permanent French Canadian fishing settlements. A small building was discovered at the first site, while the second contained the remains of a fisherman's house (Figure 2: PEN-1 and PEN-3).

The first site (PEN-1) is located on the South-West shore of Penouille approximately fifty feet from the bay. The study of its stratigraphy and of the geological formation of Penouille suggests that the site was located close to the water 200 years ago (Blanchette 1975). One trench and several test pits dug in the center of the site revealed a 5 x 7 foot rock floor which had a pair of beams, arranged perpendicularly and containing forged nails, lying on top of it. This foundation, which was situated on the shore, probably held a small building which may have been located at the tip of a wharf (Figure 6).

The materials recovered from the test excavations at this site are scarce and will not be discussed in great detail. Artifacts found in the occupation level consisted mostly of small ceramic fragments which are presented here by the minimum number of vessels of the respective types:

- 2 faience bowls with blue-on-white decoration,
- 1 grey stoneware container,
- 1 red earthenware container with lead glazed interior,
- 1 slip decorated red earthenware kitchen bowl with lead glazed interior,
- 1 red earthenware mug with a green and black glaze on the exterior and a blackish glaze on the interior,
- 1 Whieldon tortoise-shell type fine earthenware teapot,
- 1 Staffordshire white salt-glazed stoneware plate with the dot, diaper, basket pattern,
FIGURE 5. Penouille, a low sand beach peninsula on Gaspé Bay.
FIGURE 6. Excavation and stratigraphy of Penouille 1 (PEN-1).

(Scales are in feet)
2 plain Staffordshire white salt-glazed stoneware containers, 
1 Chinese hard-paste porcelain small bowl decorated in blue.

(Types after Noël Hume 1970)

Also found at the site were nineteen fragments of clay pipes among which one bears the inscription R on one side of the spur and R or B on the other side, thirty-nine fragments of glass bottles, three window pane glass and numerous pieces of copper, lead, roof slate, brick and forged nails. Occupation of PEN-1 seems to have stopped during the 1750's as cream-colored fine earthenware was represented by just one vessel. The presence of Staffordshire white salt-glazed stoneware suggests that the site was probably occupied by the 1740's. The mean date of 1732 was obtained from the Harrington-Binford clay-tobacco pipe stem formula. On the basis of the archaeological data alone, one cannot be absolutely certain of the ethnicity of this site, but artifactual similarities with PEN-3, as discussed below, suggest that PEN-1 was a French-Canadian settlement.

The second site (PEN-3) is located 800 feet to the west of PEN-1 and a similar distance from the shore. A trench excavated through the center of the PEN-3 and an additional 18 small test pits yielded portions of three features ("structures" on Figure 7): a wooden footing for a building and two refuse pits. None of these features was completely excavated. Pieces of very friable red brick have been found scattered over the entire site. A concentration of the latter occurred in one area, possibly indicating the former presence of a chimney. This site was probably the location of a fisherman's house, as suggested by Bell's map of 1758 (Figure 3: item 11).

Numerous categories of artifacts, which give the site a character of permanence, were recovered from PEN-3. The ceramics are presented by the minimum number of vessels present at the site. It is to be noted that count by sherds and count by vessels give significant differences (see Table 1). Shapes are indicated whenever it is possible.

- Ceramics possibly produced in France are:
  - faïences (4 vessels, of which 2 are decorated in blue); grey or brown stoneware jars and pitchers (6 vessels) (see Lunn 1973: 186); one grey stoneware with green colored lead glaze; and one soft-paste porcelain decorated in blue.

- Ceramics possibly produced in New France are:
  - some slip decorated red earthenware bowls with lead glaze, possibly made by the Quebec potter, François Jacquet (5 vessels; Figure 9); additional slip decorated red earthenware with lead glaze (3 vessels, of which one is a plate); red earthenware with lead glaze (3 vessels, of which one is a small container); red earthenware with green glaze (1 vessel); red earthenware with orange glaze (2 vessels).
FIGURE 7. Test pits and stratigraphies of Penouille 3 (PEN-3).
(Scales are in feet)
- Ceramics possibly produced in New England are:
  red earthenware with brown mottled glaze (1 vessel) and red earthenware with black glaze (1 handled container).

- Ceramics produced in England are:
  Staffordshire white salt-glazed stoneware (6 vessels, of which 2 are plates of the dot, diaper, basket pattern, 3 are plain bowls and 1 is a small container with molded decoration); Nottingham brown lustered stoneware type (1 vessel); Whieldon tortoise-shell fine earthenware type (1 teapot with lid).

- Ceramics from other proveniences are:
  1 Iberian coarse earthenware jar and 1 Chinese porcelain small bowl or cup, decorated with a foliate design in red and yellow (see Miller et al. 1970: Figure 49c).

To summarize the possible provenience of the ceramic items, it is believed that 12 are from France (31.6%), 14 from New France (36.8%), 2 from New England (5.1%), 8 from England (21.1%), 1 from the Iberian Peninsula (2.6%) and 1 from China (2.6%).

Other artifacts from the site are: 92 fragments of clay pipe stems, one of which has the mark of Reuben Sidney; 16 clay pipe bowls fragments, 6 of which bear the inscription WM and 1 which has a milled rim (Figures 12, 13); 158 glass fragments; 2 spall gunflints; a fire-flint, a few flints from ballast (?) stones; 8 lead-shots; a jackknife; 4 brass buttons; a brass buckle; a tin spoon handle; a lead toy having the form of a rabbit; and finally a considerable amount of forged nails and brick fragments (Figure 11).
FIGURE 8: Ceramics from Penouille 3.

A: Faience bowl decorated in blue
B: Soft paste porcelain container decorated in blue over glaze
C, E: Whieldon tortoise-shell fine earthenware tea pot with lid
D: Nottingham brown lustered stoneware
F, G: Staffordshire white salt-glazed stoneware plate of the dot, diaper, basket pattern
H: Slip decorated red earthenware with lead glaze
I: Red earthenware with green glaze on the exterior
J: Red earthenware jar (?) with lead glaze
K: Staffordshire white salt-glazed stoneware bowl
L: Grey-brown French stoneware jar
M: Iberian coarse earthenware jar

(Scale: 5 cms.: 2 ins.)

FIGURE 9: Slip decorated red earthenware kitchen bowls attributed in this paper to François Jacquet, a Quebec potter; mid-XVIIIth century. Site Penouille 3.

A, B: The interior face of the bodies of these bowls was completely covered with a white slip which was then wiped to let the paste appear in curled and linear fashions. A lead glaze is added.
C: Bowl decorated merely with a rectilinear slip and lead glaze.
D: Bowl decorated with both techniques.
E: Interior face of a slip decorated bowl base; no footring is present.
F: Exterior face of a slip decorated bowl base with a footring. Lead glaze on the exterior face is present on this piece.

Usually, the exterior face of the bowl was left uncovered; more rarely, a lead glaze covered a portion of it.

(Scale: 5 cms.: 2 ins.)
FIGURE 10. Faïences decorated in blue-on-white.
Site Penouille 3.

A-K: Decorations on the exterior face of the containers
L: Decoration on the interior face of the container
M: Footring decorated with blue lines
N: Interior face of a base without footring.

(Scale: 5 cms.: 2 ins.)

FIGURE 11. Miscellanei.
Site Penouille 3.

A,B: Decorated brass buttons
C: Unidentified brass object
D: Lead shots
E,F: Plain brass buttons
G: Brass buckle
H: Lead toy having the form of a rabbit
I,J: Grey and grey-black spall gunflints
K,M: Grey-black and grey flints from ballast (?) stones
L: Fire-flint from local chert.

(Scale: 5 cms.: 2 ins.)
FIGURE 12. Fragments of clay pipe bowls from Penouille 3.

A: decorated by milling of edge of mouth
B, C: bearing the inscription WM

FIGURE 13. Fragments of clay pipe stems from Penouille 3.

A: roulette design
B: with the mark REU/ENSI/DNEY of Reuben Sidney, Southampton, England (Walker 1971: 86)

FIGURE 14: Profile of slip decorated earthenware bowl from Penouille 3 (see figure 9: A).

FIGURE 15: Profile of slip decorated earthenware bowl from Penouille 3. The marly is slightly concave (see figure 9: C).
<table>
<thead>
<tr>
<th>Ceramic Type</th>
<th>Minimum number of vessels</th>
<th>%</th>
<th>Sherd count</th>
<th>%</th>
</tr>
</thead>
<tbody>
<tr>
<td>Grey and brown French stoneware.</td>
<td>6</td>
<td>15.8</td>
<td>115</td>
<td>14.6</td>
</tr>
<tr>
<td>Faience</td>
<td>4</td>
<td>10.5</td>
<td>142</td>
<td>18.1</td>
</tr>
<tr>
<td>Grey stoneware with lead glazed interior face, and green colored</td>
<td>1</td>
<td>2.6</td>
<td>1</td>
<td>0.1</td>
</tr>
<tr>
<td>lead glazed exterior face.</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>French porcelain, soft paste</td>
<td>1</td>
<td>2.6</td>
<td>5</td>
<td>0.7</td>
</tr>
<tr>
<td>Slip decorated red earthenware bowls, lead glazed. Attributed to Jacquet.</td>
<td>5</td>
<td>13.2</td>
<td>167</td>
<td>21.2</td>
</tr>
<tr>
<td>Slip decorated red earthenware, lead glazed: bowls.</td>
<td>2</td>
<td>5.1</td>
<td>18</td>
<td>2.3</td>
</tr>
<tr>
<td>Slip decorated red earthenware, lead glazed: plate.</td>
<td>1</td>
<td>2.6</td>
<td>1</td>
<td>0.1</td>
</tr>
<tr>
<td>Red earthenware, lead glazed</td>
<td>3</td>
<td>7.9</td>
<td>128</td>
<td>16.3</td>
</tr>
<tr>
<td>Red earthenware with green glaze</td>
<td>1</td>
<td>2.6</td>
<td>12</td>
<td>1.5</td>
</tr>
<tr>
<td>Red earthenware with orange-colored glaze</td>
<td>2</td>
<td>5.1</td>
<td>8</td>
<td>1.0</td>
</tr>
<tr>
<td>Red earthenware with mottled brown glaze</td>
<td>1</td>
<td>2.6</td>
<td>11</td>
<td>1.4</td>
</tr>
<tr>
<td>Red earthenware with black glaze</td>
<td>1</td>
<td>2.6</td>
<td>2</td>
<td>0.3</td>
</tr>
<tr>
<td>Staffordshire white salt-glazed stoneware</td>
<td>6</td>
<td>15.8</td>
<td>61</td>
<td>7.8</td>
</tr>
<tr>
<td>Nottingham brown lustered stoneware</td>
<td>1</td>
<td>2.6</td>
<td>24</td>
<td>3.1</td>
</tr>
<tr>
<td>Fine earthenware, Whieldon tortoise-shell type</td>
<td>1</td>
<td>2.6</td>
<td>11</td>
<td>1.4</td>
</tr>
<tr>
<td>Coarse earthenware, Iberian jar.</td>
<td>1</td>
<td>2.6</td>
<td>79</td>
<td>10.0</td>
</tr>
<tr>
<td>Chinese hard-paste porcelain, floral decoration in yellow and red.</td>
<td>1</td>
<td>2.6</td>
<td>3</td>
<td>0.4</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>38</strong></td>
<td><strong>99.4</strong></td>
<td><strong>788</strong></td>
<td><strong>100.3</strong></td>
</tr>
</tbody>
</table>
This site, Penouille 3, is the most fruitful historic site found to date in the Gaspé region. Its importance lies as much in the quantity and variety of excavated artifacts as in the type of economic orientation that it represents (fishing) and the time at which it was occupied (last half century of the French Regime).

The material found at the site is remarkable since it came from only eighteen small test pits covering a very limited area. There is hence the suggestion of a domestic site of a long and permanent occupation. The presence and absence of certain ceramic types establish the probable occupation period. The absence of XVIIth century brown Rhenish stoneware and of red marbleized wares suggest that the site had not been occupied during the XVIIth century. On the other hand, the presence of French grey and brown stoneware (15.8%) and faience (10.5%), in significant proportions, possibly indicates that the site was occupied by the beginning of the XVIIIth century. However, as indicated by the entire ceramic assemblage, the main occupation certainly occurred around 1740. Finally, as Whieldon tortoise-shell fine earthenware is present (2.6%), the site must have been occupied after 1754. The low quantity of the latter suggests that the site had not been occupied at the end of the 1750's. By 1760, cream-colored fine earthenwares were becoming popular and appear on many archaeological sites in large quantity, but this is not the case for the Penouille sites.

In the analysis of another artifact category, clay pipes, a mean deposition date of 1740.9 has been obtained from the 92 clay pipe stem sample. If we claim that the site was not occupied after 1758, with the destruction of Gaspé settlements by Wolfe, the use of this mean date refers to a minimal date of 1723.8. In fact, it is known that a pipe stem bearing the mark of Reuben Sidney (from Southampton, England) was manufactured between 1714 and 1716 (Walker 1971: 86). Realizing the short longevity of clay pipe, it had probably been discarded some years later. Consequently, pipe deposition dates support a similar range of occupation as ceramic evidence. The site then would date from c.1720 to 1758.

Certain ceramic types of the period are absent: sgraffito earthenware, combed slipware, German grey and blue Westerwald stoneware and scratch-blue wares. These ceramic types have been found on the contemporary sites of Fortress Louisbourg and Fort Michillimackinac. Their absence on Penouille sites may be the results of different activities, or of a limited access to the particular varieties. Since these ceramics are present at both Fortress Louisbourg and Fort Michillimackinac, which were temporarily occupied by British, and not present in Penouille, which is considered to be a non-contaminated French site, these ceramics are perhaps the most sensible indicators of an English occupation.
It is believed that most of the red earthenwares were made in New France. These wares consisted of slip-decorated kitchen bowls, possibly made by François Jacquet between 1754 and 1758; other slip-decorated wares with lead-glaze; red earthenwares with lead glaze or green glaze; and others with orange glaze. It is generally assumed that these coarse earthenwares were produced elsewhere than in New France, but the contrary is argued here. There is documentary evidence that diversified wares were produced in New France as early as 1746-47. In a letter sent to Pierre Guy, merchant at Montréal, on July 22, 1746, the Québec merchants Francois Havy and Jean Lefebvre stated that "No earthenware bowl (terrine) is coming from France and apparently, as long as the war will last, it will be so" (Université de Montréal, Collection Baby, pers. translation. Marie Gérin-Lajoie, personal communication July 15, 1975). They add that a potter, hired by Mrs. Fornel, produced earthenware bowls (terrines). The following year, he produced earthenware large bowls, soup plates and small unidentified pieces (Idem, letter dated May 25, 1747). These wares were lead-glazed and sometimes green glazed, since Mrs. Fornel had ordered 500 to 600 pounds of red lead and 100 pounds of copper filings the previous year (Idem).

Mrs. Fornel's potter in 1746 and 1747 was not the first New France artisan to produce ceramics. The first potter in New France was probably named Aubain (or Urbain) Salomé who produced lead-glazed earthenware vessels in 1694 (Lambart 1970: 1). Others have been recorded after that date but none is well known; it is possible that they did not have significant role on the ceramic history of New France. However, the war which started in 1744 had a greater effect on imported products. The author believes that the production of ceramics in New France increased significantly at this time due to the rarity of import and to the cheapness of locally made items. The industry developed so well that "By the close of the French Regime there were probably 13 or 14 potteries in operation in the colony..." (Lambart 1970: 1). The ceramics produced by these potters must have gone somewhere and, among other places, some probably went to Gaspé area. If the regression in French import and the increase in local production occurred as described here, a related pattern should be manifested in the archaeological data. This aspect of ceramic inference can be developed further when a better knowledge of the actual ceramics produced in New France is attained.

Ceramics play an extremely important role in this study. It is to be remembered that presence of certain types depended upon the availability of these types, on their desirability, on the social status of those who used them, and on the specific function of the pieces (Deetz 1973: 19-20). For instance, many of the mentioned ceramic types were of English origin. Examples are the Staffordshire white salt-glazed stoneware, the fine earthenware Whieldon tortoise-shell type, and the Nottingham brown lustered stoneware type. The presence of these ceramics in Penouille sites does not necessarily indicate an English presence. Rather, they may be the product of trade with the exterior, the New France "fermiers" and merchants serving as intermediaries, or the products of war plunder from English ships attacked in the Gulf. Nor does this material necessarily infer that these settlers were
of a high social class. Rather, the fishermen who lived on Penouille may have had access to this external market, paying for these products with fishes and labor. If the economic status of an individual is related to his access to refined ceramic types, it is not from the presence of a few pieces of the same style that this status should be inferred but rather from the presence of complete ceramic sets which were discarded as sets, as new types were available on the market.

In speaking about social and economic inferences derived from the study of ceramics, I shall discuss here three varieties of slip decorated redwares which I have attributed to the master potter François Jacquet from Quebec (figures 9, 14, 15). These earthenwares are made of a very porous paste. Decorations on these three varieties are as follows:

1- for some, the interior face of the bowl was completely covered with a white slip which was then wiped to let the paste appear in curled and linear fashions. The slip appears yellow under the lead glaze (figure 9: A, B)

2- other bowls are decorated merely with a rectilinear slip (figure 9: c). The marly of this variety is slightly concave (figure 15)

3- a third variety was decorated with both techniques (figure 9: D).

The exterior face of the bowl was usually left unglazed but in certain rare cases a lead glaze covered a portion of it. These ceramics are kitchen bowls. Rims with large lips and two bases have been partially reconstituted. One of these bases has a footring (figure 9: F; 14). These three slip decorated redware varieties represent 13.2% of the whole ceramic assemblage from this site.

To my knowledge, the above ceramic varieties have never been found before. This suggests that they were not produced in a large distribution center. Evidence for their attribution to François Jacquet stems from a contract binding Pierre Revol, a Québec merchant and "fermier" on the Gaspé fisheries at the end of the French Regime, and François Jacquet, a potter in the same town. This document was signed in the office of notary Jean-Claude Panet on April 6, 1754 (ANQ). Revol provided the funds (5,190 livres, 2 sols) to Jacquet for buying a house, build an oven and produce ceramics. This amount was to be reimbursed at a minimum rate of 300 livres a year. Until whole payment was completed, all Jacquet's possessions were mortgaged (Idem). To pay Revol, Jacquet had to produce an enormous amount of ceramic, a part of which surely turned in Revol's hands. Since Revol was owner of the Gaspé fisheries and was merchant there, circumstances suggest that the above described slip decorated wares were turned by Jacquet after this agreement.
Further studies of documents and excavations of sites in Gaspé and Québec should shed more light on the economic transactions between Revol and the Gaspé fishermen to whom this ceramic was probably sold. By the same token, one could derive a lesson in social history by elaborating on the ramifications of the practice of fishing privileges in Gaspesia. Similar studies should provide us with pertinent information on the relationships between fishermen and coastal lords and on the ascendancy of the latter.
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EIGHTEENTH CENTURY FRENCH CONTACT SITES IN NORTH-CENTRAL OKLAHOMA

Byron Sudbury

There are at least three early 18th century French contact sites in Kay County, Oklahoma. They have been known to local collectors and professional archaeologists for some time, but little data about them has been available to archaeologists in other parts of the country. The purpose of this presentation is to disseminate information regarding these sites to those outside of the general Oklahoma region. Recently I completed a paper describing the history and artifact sample from one of these sites known as the Deer Creek site. This report, to be published elsewhere, is based on surface collections and thus should be considered as a preliminary report. Excavations at the Deer Creek site are to be conducted in the near future. The purpose of this brief communication is to inform the archaeological community of the existence of these three contact sites, and to briefly describe these sites' historical and cultural affiliations. The need for such interregional awareness and communication regarding significant sites was previously discussed by G. F. Fielder, Jr. at the 16th Conference on Historic Site Archaeology (Fielder 1975).

The three currently recognized 18th century contact sites in Kay County are the Deer Creek, Bryson, and Love sites. The Deer Creek site (Ka-3) is located on the west bank of the Arkansas River. This site was recognized as early as 1914, and limited excavations were conducted by Thoburn in 1917. The exact location of Thoburn's excavations and the nature of the materials recovered is not known. A representative sample of Deer Creek site trade goods from a private collection was submitted to Arthur Woodward who indicated that the European materials were of French origin and probably dated 1700-1750 (Steene 1953). Thoburn worked for the Oklahoma Historical Society and apparently identified the Deer Creek site as "Ferdinandina"—a name appearing on 19th century maps which may refer to the 18th century Deer Creek site occupation. The standard Oklahoma Historical Society position has been that the site represents the point of contact with the Paniassa (or at the very least, the group contacted) by DuTinsé in 1719. It has been further contended that the site became a French trading post. Excavations at the nearby Bryson site (Ka-5) were conducted by Thoburn in 1926. The Bryson site appears to be related to the Deer Creek site. A portion of the Bryson site collection has survived but the bulk of it is uncatalogued and thus of somewhat dubious value. Excerpts from a Thoburn manuscript summarizing his field activities at the Bryson and Deer Creek sites has been published (McRill 1963). The materials recovered by Thoburn have never been properly studied or reported.

The study presently being summarized was conducted on three private surface collections from the Deer Creek site. The possibility of several very minor prehistoric occupations was noted. A minor mid-19th century Indian occupation (possibly Osage or Cherokee?) was also indicated. The primary occupation was felt to have occurred from ca. 1735-1760. This date was based primarily on the almost exclusively French trade good sample present. Definite references to a contact site at this locale do not exist in the known records of this time although several possible references were noted.
In addition, maps show "Paniassa" in this general region, and one map indicates a "Paniassa Indian Village and Fort" at this locale (Mitchell 1755). These are thought to represent the occupation at the Deer Creek site. Trade materials are also present at the Bryson site, and it appears probable that these two sites were occupied by closely related peoples. It is thought that the Bryson site was established slightly earlier than the Deer Creek site although they were in part contemporaneous. The third contact site, the Love site (Ka-2), was reported by Wyckoff and also had trade goods (Wyckoff 1964). These three Kay County contact sites are within 6.5 kilometers of each other on the west bank of the Arkansas River.

The Deer Creek native artifact inventory as well as surface features at the site indicate that the Deer Creek site inhabitants were direct descendents of the Great Bend Aspect peoples who have been identified as the 16th and 17th century Wichita in Kansas (Wedel 1959). There are two defined foci in the Great Bend Aspect, and the material culture present at Deer Creek suggests a relationship to both with a probable coalescence of these two previously distinct groups apparently being represented. The actual relationship of the Deer Creek site to these two previously defined foci is not clearly delineated as artifact associations and relationships apparent in an excavated sample are not present in the Deer Creek site surface collections. The Deer Creek site occupation appears to be a new distinct protohistoric cultural manifestation of the Great Bend Aspect. The other two Kay County contact sites mentioned also appear to belong to this same complex. The need and appropriateness of a new focus designation and description is apparent. However, as only surface collections have been studied, I am hesitant to assign a name and detailed list of specific attributes to these sites as the possibility of multiple occupation phases cannot be totally excluded. Thus, although the groundwork has been laid, the actual definition is left to those who report on excavations at these three sites when the definite assemblage of traits assignable to this group can be adequately substantiated.

All of the evidence—historical references, maps, and native artifacts—suggest that the Deer Creek site occupants were the Wichita proper. This suggestion has been previously made although the author's current report is the first time actual data has been available (Bell and Baerreis 1951; Harper 1953; Wyckoff 1964; Bell and Bastian 1967; Richards 1971). No clear evidence of a trading post has ever been noted, and it is the opinion of this author that the site was an Indian village that served as a rendezvous point for French traders. In addition, contact with sites of the Norteno Focus of Texas was evidenced by several decorated trade sherds, and was also suggested by the polished surface finish and painting on some of the resident potsherds. Edwards Plateau (central Texas) flint was also found at the site, and the predominant clay pipe form was identical to that noted for the Norteno Focus. Contact with the Southwest was evidenced by the presence of obsidian flakes, and Southwest trade sherds (Wyckoff 1964).

The University of Oklahoma through the Oklahoma River Basin Survey has conducted excavations at the Bryson and Love sites during the past two seasons. Reports on this work should be available in one or two years. The Deer Creek site has been designated as a National Historic Landmark, and plans for excavations with some possible resulting interpretive displays and/or restoration are in the works, also by the University of Oklahoma.
These sites apparently are some of the earliest contact sites in this region. Although some Spanish contact occurred with one earlier focus of the Great Bend Aspect this was minimal, apparently resulting from 16th century explorers and not from trading enterprises. So, it is likely that these Kay County sites represent an early phase of extensive Wichita-European contact. As such, they are of great importance in our understanding of Plains Indian acculturation and French trading expansion and activity as well as presenting a trade good sample from a relatively restricted time period.

**Postscript**

Since submission of the above abstract, the site report under discussion has been published (Sudbury 1976a). [Maps of site location were deleted at the request of the National Park Service.] The report primarily examines the Deer Creek site although extensive comments on the Bryson site are included. The Love site was mentioned as a minor note for completeness. All the information available regarding a historic occupation at the Love site was ultimately based on an old site report filed at the University of Oklahoma. Recent limited testing at the Love site did not yield evidence of a historic occupation (Young 1976). It is possible that the site report erroneously superimposed the Deer Creek and/or Bryson site occupations and/or artifacts on the Love site location (Sudbury 1976b). Regardless, an error was made in assigning the Love site occupation to a historic period. Based on current knowledge, the comments on the Love site in the preceding note should be disregarded.

The author has personally observed and studied the Deer Creek and Bryson sites. The Deer Creek and Bryson sites are about 2.5 kilometers apart, and appear to represent related occupations. An Oklahoma River Basin Survey [1808 Newton Drive, Norman, Oklahoma 73069] site report on the Bryson site was recently published (Hartley 1975). This ORBS report, based on a sample from limited testing, was primarily descriptive as more extensive excavation was planned for the Bryson site. Additional studies on the Deer Creek and Bryson sites in the near future will help further elucidate their relationship.

This report is to be published in full under the title: **Ka-3, The Deer Creek Site: An Eighteenth Century French Contact Site in Kay County, Oklahoma. Bulletin of the Oklahoma Anthropological Society, Volume Number XXIV for 1975.** Edited by Don G. Wyckoff.
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THE LAST OF THE SQUARE-RIGGED WINDJAMMERS

Floyd Painter

Ships are artifacts too, you know, and no other artifact conjures up more visions of adventure, romance, and faraway places and climes, than does an old square-rigged windjammer. They were ships with wings, so to speak, and they served the most adventurous spirits of the world far longer than any present-day form of transportation. With these winged ships our ancestors circumnavigated the globe and explored the navigable far corners of the earth. They served adventurers and pilgrims, pirates and missionary priests, traders and slavers, whalers and seekers of gold, and they brought our European ancestors to the New World.

Many people today are striving to preserve the relics of our historic past, and we archaeologists are busily uncovering foundations of brick and stone, the earthworks of old forts, and other vestiges of days long gone. Whole communities are restoring old buildings that relate to their local history and in these they take great pride. All around the world archaeological research, preservation and restoration is taking place at an ever increasing pace for we realize that the bulldozers of modern progress and technology will soon destroy these symbols of our heritage.

Ruined temples, historic homes, old fortresses, even old iron foundries have an air of mystery and sometimes romance, but these structures, these artifacts of our ancestors all pale in comparison to the rarest and most romantic of all the symbols of our adventurous past, a square-rigged sailing ship. Less than thirty of these winged ships are still afloat in our modern world. The others lie sunken on the rocks of time.

A few replicas of historic ships have been built from original plans and these are tourist attractions at Jamestown in Virginia and at Plymouth, Massachusetts. The Jamestown ships are: the galleon Susan Constant, the caravel Godspeed, and the pinace Discovery. The ship at Plymouth, is of course, the caravel Mayflower. A reproduction of the galleon Santa Maria, flagship of the fleet of Columbus, can be seen in the harbor of Barcelona, Spain, another Santa Maria is on display in St. Louis, Missouri. A replica of the famed bark H.M.S. Bounty was built for the movie Mutiny on the Bounty and sailed to Tahiti some years ago. A reproduction is a mere soulless symbol and we mention them only in passing.

Still afloat but unseaworthy, the frigate Constitution (Old Ironsides) lies at pier in Boston, Massachusetts. Others in like condition are: the bark Charles W. Morgan (a whaler), at Old Mystic, Connecticut; the frigate Constellation at Baltimore; the clipper Star of India at San Diego; the bark Falls of Clyde at Baltimore; the barks Moshula and Wavertree in New York City; the bark Balclutha at San Francisco; Lord Nelson’s H.M.S. Victory at Portsmouth, England; the clipper Cutty Sark at Greenwich, England; the ancient bark Vasa and the steel-hulled bark Af Chapman at Stockholm, Sweden. These old ships are not dead things, they live, but are shorn of their great white wings.
Still seaworthy, and sailing the bounding main, are the following graceful coursers of the deep: the bark Eagle, schoolship of the U.S. Coast Guard; the barkentine Mercator, Belgian schoolship; the barkentine Goleta Esmeralda, Chilean schoolship; the bark Christian Radich, Norwegian schoolship; the bark Sagres, Portuguese schoolship; the bark Staatraad Lemkuhl, Norwegian schoolship; the brigantine St. Lawrence II, of Canada; the bark Danmark, of Denmark; the bark Libertad, Argentine schoolship; the bark Gorch Fock II, West German schoolship; the barkentine Dewarutji, of the Indonesian Navy; the bark Sorlandet, of Norway; the bark Joseph Conrad, of Old Mystic, Connecticut; the brig Vencateswaraloo, of India; and the barkentine Gazela Primeiro, oldest wood-hulled ship still under sail. The Gazela Primeiro is owned by the Philadelphia Maritime Museum, and she is ninety-two years old.

The Gazela Primeiro (First Gazelle) was built in Cacilhas, Portugal, in the year 1883, and though built as a whaler, she is indeed a work of art. Perfectly joined in every timber and frame, heavily built, but graceful in every line. Looking more like a yacht than a working vessel, she has a finely drawn stern and a clipper bow. Her hull, masts, booms, and spars are made of pine, Portuguese pine, cut from a national forest preserve planted in 1460 by Prince Henry the Navigator, and expressly for the building of ships. It would be difficult to find such timber, and the craftsmen to shape it, in the world today.

The present-day specifications of the Gazela Primeiro are as follows: Length overall - 177 feet 10 inches; Beam - 27 feet; Draught - 17 feet 6 inches; Foremast height from main deck - 93 feet 4 inches; Gross tonnage - 324; Frames, masts, booms, and yards - stone pine; Hull planking and decks - maritime pine; Hull - copper sheathed; Sails - 13: Sail area - 990 square yards; Crew - 30 to 37 men; Speed under power - 6 knots; Speed under sail - ?; Last sailed to Grand Banks in 1969; Last sailed (five day voyage) in 1974; Present home port - Philadelphia.

The Gazela Primeiro was converted to a cod-fishing vessel in 1938, minor changes were made, such as fitting her with a diesel auxiliary engine, two small diesel generators, and a pilot house. Still a wooden sailing ship, truly manned by "iron men", she sailed every spring from Lisbon for the fishing banks, the "Grand Banks" off Newfoundland, and the rich cod-grounds of Davis Strait, between Greenland and Baffin Island. Six months later, and with a hold filled with salted cod, she headed for home with all sails set and her auxiliary engine at full throttle. Even the sails on the fishing dories (nested on deck) were set to catch the wind. There was singing and dancing in the streets of Portugal when the fleet returned, sometimes led by the "First Gazelle".

First as a whaler, then as a cod-fisherman, the Gazela Primeiro helped feed the hungry mouths of Europe for eighty-six years. She's a great ship, a beautiful ship, one of the last of her kind, and she will quicken the pulse of an adventurous spirit. The sight of her great swelling sails, and the sound of the wind strumming her rigging, are never to be forgotten.
In a few more years these last tall square-riggers will furl their sails for the last time, and the world will never know their like again.

Underwater archaeologists are long familiar with the sunken remains of tall ships, Spanish galleons of the "Plate Fleet" in the waters of the Caribbean and Gulf of Mexico, Greek and Roman wrecks in the Mediterranean, and the famous "blockade runners" sunk during the American Civil War. Their knowledge is great concerning such imperishable objects as ships' anchors, cannons and cannonballs, ship fittings of iron and brass, glass bottles, oil and wine jars (amphoras), and many other items of ships' hardware and cargo. We must mention, of course, the major artifacts of their search, ingots of silver and gold, bejeweled crucifixes, exotic golden figurines from the civilizations of Mexico and Peru, Dutch doubloons and Spanish pieces of eight.

How familiar are these archaeologists and divers with the ships that carried their "treasures" of iron and gold? A great quantity of information is now available for such research and the above listed vessels are still afloat for their study. Do they pause in their dangerous endeavors to visualize the graceful clipper or the elegant high-pooped galleon among whose ribs they search? We are certain they do, for any person with imagination and a thirst for adventure would thrill at such retrospect. Are they acquainted with the life, adventures, and hardships of the sailors who manned these romantic ships of the past? Unless they themselves, are sailors of square-rigged ships they cannot know or appreciate such a life.

These sea dogs of old were for the most part, poorly paid, underfed, overworked, and brutally treated. Theirs was the most dangerous pursuit man has ever chosen to follow. Their everyday work took them aloft in the reeling rigging, and under the most trying conditions of wind and cold. One misstep on a footrope could hurl them screaming to the deck far below, or plunge them into the boiling sea. At times they were forced to fight for their ship and their lives in order to repel pirates, enemy sailors and soldiers, or hostile natives. They risked showers of arrows, enemy bullets and shells, they were impaled like a pig on a pike or mortally slashed with a cutlass. Above all else they risked losing their ship on rocks and shoals, from storms or enemy action. Worse yet, most old-time sailors could not swim.

Is it any wonder that many superstitious sailors had the letters H-O-I-D F-A-S-T tattooed on the backs of their eight fingers, or that one old salt made the following observation concerning yachtsmen, "A man who would go to sea for pleasure would go to hell for a pastime"?

What could possibly motivate a man to choose such a perilous career? Some had little or no choice in the matter. Some were drugged and "shanghaied" from the bars of San Francisco, some were given the choice of going to sea or rotting in jail for debts or crimes committed, some to escape the dull drudgery of life in Europe or elsewhere, and some were apprenticed at a tender age and knew no other life. Others, it seems, went to sea for profit, to become master or owner of his own ship, to seek fortunes in gold, jewels, and pearls in the
earth's remote corners, to take prizes and plunder, to find a better life in some earthly paradise such as Tahiti or Pago Pago.

Still others went to sea for love of adventure, some found the relatively dull lives of landsmen intolerable, they craved excitement, romance, danger. They loved the tall ships that took them to exotic places, they loved the challenge of wind and wave, these men would have chosen no other life. If today, one advertised for men to sail a fleet of bathtubs "round the world," he would have more volunteers than he could use, and they would be the same breed that "went down to the sea" in tall square-riggers.

Adventurous, daring men, we still have many, but beautiful, challenging, and exciting ships, we have but few. Present day adventurers choose to fly supersonic fighter planes, take voyages to the moon and planets as astronauts, sail small boats to odd and dangerous places, climb mountain peaks, explore deep caverns, and dive to the sea bottoms searching for ancient square-rigged ships. The romance is still with us, and we are the richer for it.

Pay a visit to a tall ship —— she will stir your blood.

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TRADE GOODS FROM THE TRIGG SITE, RADFORD, VIRGINIA

Howard A. MacCord

The Trigg Site (44 My 3) was located on the right (south) bank of the New River in the City of Radford, Virginia (Figure 1). New River originates in North Carolina, flows north across southwestern Virginia and northwest through West Virginia, joining the Ohio River at Point Pleasant. The site is on the flood plain, 150 feet from the river, and has suffered some damage from recurring floods. Complete excavation of the site took place during the fall of 1974 and spring of 1975. The site has now been destroyed by a construction project.

The site proved to be that of an oval, palisaded village, measuring 260 feet by 290 feet, and containing about 2.7 acres. Within the palisade, circular house sites, hearths, storage pits, and many human burials were found. Of the 308 burials, 42 (13.7%) contained copper or glass artifacts of European origin. Glass beads of 19 types totalling 348 individual beads were recovered, mostly as grave goods, with a few from refuse-filled storage pits.

Most of the glass beads were found in the neck regions of the burials, usually in alignments indicating that they had been strung as necklaces. Many of the strands were made up of a mixture of a few glass beads interspersed with beads made of marine shells, or with copper beads or pendants. In one instance, four large beads were with an adult male, two beads at each ear, apparently used as ear-bobs. One group found in the abdominal area of a child had probably adorned a pouch, sash, or apron. Another child had five beads near the right wrist, perhaps representing a bracelet or sleeve decoration.

Bead preservation was generally good, although many show a coating of corrosion products. The beads have been classified according to the Canadian classification established by Kidd and Kidd (1970). The following table lists the types, the number of each type found, and a brief description of each type.

Table 1: A List of the Bead Types Found (after Kidd and Kidd 1970)

<table>
<thead>
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<tr>
<td>NO.</td>
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<tr>
<td>17</td>
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<tr>
<td>2</td>
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<td>40</td>
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<td>1</td>
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<tr>
<td>8</td>
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FIGURE 1. Map showing location of Trigg Site at Radford, Virginia.
In an effort to define the distribution of the various types and to arrive at a date for the Trigg Site, a thorough search was made of the relevant literature for Virginia and other states of the mid-Atlantic area. Many of the Trigg Site beads seem thus far to be quite rare in Virginia. The following paragraphs summarize the data found in our search.

Bead Type IIAl3, an opaque white, oval bead was found in the fort area of Flowerdew Hundred, on James River, just west of Jamestown. The fort dates from the period 1618-1630.

Type IIAl3 a light aqua blue, spherical bead was also found at the Flowerdew Hundred fort and at the Littletown house site on Kingsmill Plantation near Williamsburg. This was the home of the Pettus family in the fourth quarter of the 17th century.

Type IVB13 is a two layered "seed" bead, with a light gray center and an outside opaque white layer, with six dull stripes. About 15,000 of this type were found at the Mt. Airy Site in Richmond County, Virginia, where several ossuaries were exposed in gravel diggings. These finds are dated (McCary 1950) to before 1650 and attributed to the historic Rappahannock Indians.

Type IVK4 is a star chevron bead with four layers. The center is opaque white, covered by an opaque red layer, then by another opaque white layer, and finally by an outer layer of bright navy blue. McCary reports this type from the Mt. Airy Site. Identical beads were found in Maryland in an ossuary

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### Table: Present Beads

<table>
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<tr>
<th>NO.</th>
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<td>IIA33</td>
<td>Clear light aqua blue</td>
<td>Round</td>
<td>7 mm.</td>
<td>4</td>
</tr>
<tr>
<td>1</td>
<td>IIA39</td>
<td>Clear aqua blue</td>
<td>Round</td>
<td>5 mm.</td>
<td></td>
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<tr>
<td>1</td>
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<td>Opaque robin's egg blue</td>
<td>Round</td>
<td>4 mm.</td>
<td></td>
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<tr>
<td>1</td>
<td>IIA43</td>
<td>Clear bright blue</td>
<td>Round</td>
<td>6 mm.</td>
<td></td>
</tr>
<tr>
<td>4</td>
<td>IIA44</td>
<td>Clear cerulean blue</td>
<td>Round</td>
<td>7 mm.</td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>IIA46</td>
<td>Clear purple blue</td>
<td>Round</td>
<td>6 mm.</td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>IIA48</td>
<td>Opaque dark shadow blue</td>
<td>Flattened on one side with groove</td>
<td>7 mm.</td>
<td>2</td>
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</table>

### Class IV

<table>
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<tr>
<th>NO.</th>
<th>TYPE</th>
<th>DESCRIPTION</th>
<th>SHAPE</th>
<th>SIZE</th>
<th>FIGURE</th>
</tr>
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<tr>
<td>1</td>
<td>IC10</td>
<td>3 layers-opaque navy blue, (Variant) opaque white, opaque navy blue</td>
<td>Round</td>
<td>13.3 mm.</td>
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<td>IVA11</td>
<td>3 layers-clear light gray, opaque white, clear light gray</td>
<td>Circular</td>
<td>1.5 mm.</td>
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<td>2</td>
<td>IVA12</td>
<td>3 layers-clear light gray opaque light blue, clear light gray</td>
<td>Circular</td>
<td>2 mm.</td>
<td>2</td>
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<td>4</td>
<td>IVB13</td>
<td>2 layers-clear light gray, opaque white, 6 red stripes</td>
<td>Circular</td>
<td>2 mm.</td>
<td>2</td>
</tr>
<tr>
<td>2</td>
<td>IVK4</td>
<td>Clear bright navy, white opaque red opaque, white opaque, glass 4 layers</td>
<td>Star</td>
<td>6 mm.</td>
<td>2,5</td>
</tr>
</tbody>
</table>
FIGURE 5. (above)

Beads from burial #18 are of three types: center bead is a star chevron IVK4; those on either side type IIB57; the remaining beads are IIB56.

FIGURE 6. (right)

Bead from burial #39 is type IV10 (variant).

FIGURE 7. (right)

Effigy claw of sheet copper found with burial #247.
on Piscataway Creek (Ferguson and Stewart 1940) along with specimens of bead Type IIB56, an opaque robin's egg blue with three opaque white stripes. This ossuary was dated to around 1680. Star chevron beads (Type IVK4) were found in quantity at several sites at Washington Borough, Lancaster County, Pennsylvania. The Ibaugh Site (Witthoft, Kinsey and Holzinger 1959; Kinsey 1960) yielded this as a minor bead type. The Ibaugh Site has been dated to 1600 to 1625. The Schultz-Blue Rock Site, dated to 1575-1595 (Heisey and Witmer 1962) yielded a higher percentage of this type. In New York state, several sites yielded this bead type. Among these are the Kleis Site, dated by White (1967) to about 1625-1640; and the Dutch Hollow Site, dated by Ritchie (1954) to about 1590-1615. In the Seneca sequence discussed by Wray and Schoff (1953) the star chevron bead type was placed at around 1590-1616.

Type IVA10 (variant) is a rather large bead with an opaque white layer sandwiched between an inner and outer layer of opaque navy blue. Only one of this type was found at the Trigg Site. A somewhat lighter colored bead of the same size is reported from the Goodnow Mound in Florida (Griffin and Smith 1948), and is dated to the 17th century.

The copper trade items are mainly tubular beads made from rolled thin sheets (Figure 8). Found mostly in graves these items seem to have been used in necklaces, as ear-bobs, pendants, and as hair ornaments. Some were found on the same strings with shell or glass beads. The tubular beads range in length from 4.9 mm. to 83.5 mm., with an average of 29.0 mm. Widths ranged from 2.1 mm. to 11.7 mm., with the average at 4.1 mm. Some of the rolled beads are quite small (2.8 by 2.4 mm.) and may be classed as "seed" beads.

Four thin, machine-rolled discs with central perforations were recovered (Figure 8). One was a small, flat disc, 12.9 mm. in diameter. Two others were slightly cone-shaped, with diameters of 15.9 mm. and 16.7 mm. The fourth was of special interest - a large, slightly oval disc, 131.4 mm. by 124.4 mm., with a central perforation 17 mm. in diameter (Figure 9). This latter disc was found on the rib cage of an adult male, buried in an extended position. That this was a high-status burial is demonstrated by the copper and the other objects interred with him. The other grave goods included many marginella beads, 40 tubular copper beads, and a large busycon shell trumpet.

Three triangle pendants of copper found had tiny perforations at the smaller end. The pendants were from 36.5 mm. to 38.0 mm. long and from 12.5 mm. to 15.6 mm. wide. One narrow, leaf-shaped strip of copper occurred in a storage pit. This object was 54.8 mm. long and only 5.8 mm. wide, with slightly pointed ends. The use or purpose of this item is unknown.

The back half of a large hawk bell, made from a copper alloy was found in a child's grave, at the top of the head, as if it had been worn in the hair. The bell back has a riveted loop for attachment instead of the usual cast eye. The diameter of the open end of the bell is 27.2 mm., and two moldmarks or incised lines encircle the open end. This specimen is similar to one found at the West Ferry Site in Rhode Island (Simmons 1970:82).
FIGURE 8. Copper Artifacts

Left, rolled beads.
Center, copper leaf (?).
Right, pendants, and small "seed" beads.

FIGURE 9. Copper disc from Burial #194.
One small effigy claw of sheet copper was found. It has a small hole in one end (Figure 7). It resembles similar effigy claws from the Trigg Site made from ground slate and also some made from cannel coal found on numerous Fort Ancient sites in West Virginia and along the Ohio River.

Numerous copper cones or tinklers were found. These vary in size from 18.8 mm. to 45.0 mm. in length. These were made by wrapping a rectangular sheet of copper around itself to form a cone.

CONCLUSIONS

Judging from the number of trade beads and other artifacts found, an extensive trade in copper and glass was taking place during part of the time the Trigg Site was occupied. The numerous finds of marine shell beads and ornaments at the Trigg Site and at many other sites nearby demonstrate that an extensive trade network extended from the coast to the interior and was well established, even in prehistoric times. As the coastal Indians received copper, glass and iron objects from the early settlers, they began immediately to include these in the trade moving inland from the coast. Iron objects, while valuable to the Indians, were also heavy and usually of a utilitarian nature. On the other hand, the copper and glass items were relatively light, and being ornamental, they would be compatible with the shell beads and ornaments already being traded. It seems logical that the iron objects would remain near the source, while the ornamental objects would move quickly into the hinterland. The trade items would thus have reached the Trigg Site through indirect trade and not through direct contact with Europeans. Since only about 14% of the graves at the Trigg Site had European trade items, we postulate that these items arrived in the area only during the last 3-5 years of the site’s occupancy.

While the beads do not pinpoint the date of the site, our comparisons with similar sites in other areas support a 17th century dating. The relative scarcity of trade items, the complete lack of iron and larger copper objects, and the absence of tubular glass beads known to be of late 17th century date, lead us to believe that the site was occupied during the first quarter of the 17th century, probably contemporary with the settling of Jamestown in 1607. Jamestown would have been a convenient source of such items and since there were pre-existing trade networks linking the areas, arrival of European trade items at the Trigg Site probably occurred within ten years after Jamestown was settled. Until the complex of beads found at the Trigg Site can be more specifically dated, it appears that this generalization is the best estimate we can make.
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McCARY, BEN C.

RITCHIE, WILLIAM A.

SIMMONS, WILLIAM SCRANTON

WHITE, MARIAN E.

WITTHOFT, JOHN, W. FRED KINSEY, III and CHARLES H. HOLZINGER

WRAY, CHARLES F. and HARRY L. SCHOFF
Though the title of this paper is directed to the study of a particular artifact type, it is not the author's purpose, nor that of the archaeological discipline, to primarily emphasize the artifact. Materials divorced from their cultural context are of little value, unless they can be studied and arranged in such a manner that they shed some light on the life and culture of the peoples who used them. However, this is not intended to underestimate the value of material culture studies. The study of a single artifact, in this case bells, can contribute much to our historical and anthropological knowledge.

With the exception of glass beads, small brass bells were probably the most popular item of the North American Indian trade, yet no one, to my knowledge, has extensively studied this particular artifact type over space and time. The author's distributional studies have revealed the existence of bells throughout an area stretching from Saskatchewan to Florida, and from Cape Breton Island to southern Texas. They were traded by the Spanish as early as 1492 (Morison 1942:301-302), by the English at Jamestown (Smith 1624: 38) and Hudson's Bay (Quimby 1966: 66), and by the French throughout the seventeenth and eighteenth centuries. The purpose of this paper is to examine the use of bells by both the donor and recipient cultures, to present a short summary of the known types and varieties of the seventeenth and eighteenth centuries, and to focus more intensively on three of the varieties to determine what contribution, if any, the study of bells may have to better understanding trade networks between the Indians and the various European powers.

The European use of small bells is somewhat obscure. Literature on church bells is abundant, but very little mention has ever been made of the production and role of the ubiquitous small bell. They have been called "hawk bells," "turkey bells," "sleigh bells," "morris bells," and a host of other terms, but no one has ever given a satisfactory definition as to what these terms mean. "Hawk bells" and "turkey bells" are presumably the small, light, sheet brass bells which were attached to the legs of the particular birds they were named after. The heavier, larger, cast brass bells were tied to sleighs and horses. Bells were apparently put to a number of other uses in Colonial America. In Williamsburg, the bell had an important function in the garden. An illustration in Audrey Noël Hume's work, "Archaeology and the Colonial Gardener" depicts a series of bells tied to a string which had been fastened between two poles. The tinkling bells, put into action by the power of the wind, served to frighten away unwanted birds (Noël Hume 1974:87-88). Contraptions such as the above were being made in England at least as early as 1659 (Ibid: 60, fig. 39).

The English traders in the Southeast had a somewhat different use for small bells, much to the annoyance of William Bartram. This late eighteenth century traveler described a large trading caravan, of between twenty and thirty horses, which he accompanied into Creek territory:
They seldom decamp until the sun is high and hot; each one having a whip made of the toughest cow-skin, they start all at once, the horses having ranged themselves in regular Indian file, the veteran in the van, and the younger in the rear; then the chief drives with the crack of his whip, and a whoop or shriek, which rings through the forests and plains, speaks in Indian, commanding them to proceed, which is repeated by all the company, when we start at once, keeping up a brisk and constant trot, which is incessantly urged and continued as long as the miserable creatures are able to move forward; and then come to camp, though frequently in the middle of the afternoon, which is the pleasantest time of the day for traveling: and every horse has a bell on, which being stopped when we start in the morning with a twist of grass or leaves, soon shakes out, and they are never stopped again during the day. The constant ringing and clattering of the bells, smacking of the whips, whooping and too frequent cursing these miserable quadrupeds, cause an incessant uproar and confusion, inexpressibly disagreeable (Van Doren 1928: 350-351).

These caravans undoubtedly created a stir in the backwoods of Georgia and Alabama. The resounding bells must have announced the traders' presence from a considerable distance. Not only were the Indians attracted to the many wares carried by the caravans, but they apparently desired the bells as well. In 1729, the Frenchman Regis du Roullet indicated that "very large bells such as mules wear" were some of the items traded by the English to the Chickasaw (Rowland and Sanders 1927: 53).

The manner in which these small bells were used by the Indians is alluded to in very few historic accounts. There is abundant evidence indicating that Indians wore bells (eg. Thwaites 1900: 251), but few authors described the manner in which they were worn. An exception was Le Page du Pratz, who recorded that bells were an integral part of a warrior's outfit:

All the attire of a warrior consists in the ear pendants which I just described, in a belt ornamented with rattles - and bells when they can get them from the French - so that when they walk they resemble rather mules than men (Swanton 1911: 127).

It might seem curious that one would adorn his person with bells when attempting to ambush a foe. However, Indians appear to have had an uncanny ability to move without noise when they so desired, even with bells as part of their apparel:
PRESENTED PAPERS - Brown

Presently in came fine Men dressed up with feathers, their faces being covered with Vizards made of Gourds; round their Ancles and Knees were hung Bells of several sorts; having Wooden Falchions in their Hands, (such as Stage Fencers commonly used); in this Dress they danced about an Hour, showing many strange Gestures, and brandishing their Wooden Weapons as if they were going to fight each other; oftentimes walking very nimbly round the Room, without making the least Noise with their Bells, (a thing I much admired at)... (John Lawson among Catawba in 1701 - Hudson 1970: 2).

The use of bells in personal adornment has perhaps been revealed best by archaeological investigations. At the Gros Cap Cemetery site in Michigan, certain burials had bells which were strung along with beads, perhaps as necklaces (Quimby 1966:125-126). Evidence for bell necklaces also occurred at the Haynes Bluff site in Mississippi, where one burial had a number of bells, grouped together in a mass, encircling the neck of the skeleton.* Bells were perhaps most commonly attached to clothing. At site 6, a Navajo component in the Gobernador District of New Mexico, a child burial was excavated which had three bells attached to a woolen garment encircling the individual's neck (Carlson 1965: 39-40). Bells were also found attached to clothing at Gros Cap Cemetery (Quimby 1966: 125-126), and at the Doniphan site in Kansas. At the latter site, the bells were found around the knees of the burial (Wedel 1959: 61). Similarly, two bells were found between the femurs of a skeleton at the Rock Island site in Wisconsin (Ronald Mason, pers. comm.); bells were found near the feet of two burials excavated at the Angola Farm site in Louisiana (Ford 1936: 136); and two bells were found in the leg area of a burial at the Cooper Farm site in Alabama. Another burial at the latter site had four bells, two at the knees and two at the wrists (Lindsey 1964). Late seventeenth century Micmac Indians were reported to have worn bells in their ears (Le Clercq 1691: 343), and archaeological evidence has suggested that this particular mode of bodily adornment was practiced by other North American aboriginal groups. At the Albert Ibaha site in Pennsylvania, two bells were found resting against the left side of a skull in a burial (Kinsey 1960: 89-90), and at the Oliver site in Mississippi (Brain 1975: 133) a bell was found in one burial near or in contact with the skull, and in another burial under the left ear.*

Much still remains to be learned about how historic trade bells were received and used by the North American Indians. An important step to attaining this knowledge is the establishment of a coherent and flexible classification for bells. Presented below is a typology of the various bell types and varieties distributed to the Indians during the seventeenth and eighteenth centuries. The following is an abridged presentation, but hopefully will give the reader an idea of the kinds of bells in existence during the above period. A more detailed discussion will be found in a forthcoming publication (Brown n.d.).

*Lower Mississippi Survey Files, Peabody Museum, Harvard University
The first class of bells is made of cast brass. They are generally large, thick, and heavy. Three types have been formulated on the basis of the attachment handle shape, two of which shall be discussed in this paper. The first type has an inverted 'V' shaped handle which has been flattened on the top. A small round hole has been punched through the handle. These have been classified as Key Bells. The Fishkey bell (Figure 1) is one variety of this type, so named because the surface decoration resembles fish scales. A bell of this variety has two holes in the upper hemisphere and four in the lower, two of which are connected by a wide slit. The height of the attachment handle is approximately 1/3 the height of the bell. Surface decoration consists of small semi-circular projections, 1 cm in diameter, which overlap and cover the entire surface of the bell. The apex of the semi-circles faces away from the equatorial seam in both the upper and lower hemispheres. Thus far this variety has only been found in the Tunica Treasure, where it had a considerable representation, and at the Cooper Farm site in Alabama (Marvin T. Smith, pers. comm.).

The Flowerkey bells (Figure 2) also have an inverted 'V' shaped attachment handle. Similarly, these bells have two holes in the upper hemisphere and four in the lower, with the height of the attachment handle being approximately 1/3 the height of the bell. Surface decoration consists of floral projections and protruding ridges which separate the floral arrangements into sections. This particular variety has a large representation in the Tunica Treasure from the Trudeau site, and has also been discovered at Taskigi, a Creek Indian site located near Fort Toulouse, Alabama.

The second type of cast brass bells has an attachment handle shaped like the inverted block letter 'U'. The hole in the attachment handle is in the shape of an arch rather than circular, and the bells of this type have been classified as Arch bells. Only one variety has been established under this type. This particular specimen is a Circarch bell (Figure 3). It has two holes in the upper hemisphere but, unlike the Key bells described earlier, has only two holes in the lower hemisphere. The latter are connected by a wide slit. Surface decoration which appears solely on the lower portion of the bell, consists of elongated circular loops which begin near the slit and attain their maximum height just beneath the equatorial seam. Circarch bells have an extremely wide distribution, as will be discussed later. They often have two initials stamped on their bases, one on each side of the slit. 'K.W.' and 'G.W.' are the most commonly found initials, and these are believed to be manufacturers' marks. The author has recorded eight sites which had Circarch bells with the initials 'K.W.', all of which were occupied in the early eighteenth century. Three additional sites with 'G.W.' Circarch bells had date ranges covering the entire eighteenth century. 'G.W.' or 'W.G.' bells are known to have been made in seventeenth century Wiltshire, England, and Ivor Noel Hume, in his book A Guide to the Artifacts of Colonial America, illustrated a late eighteenth -

*Moundville Museum Collections, Alabama.
Figure 1
Fishkey Bell
(Tunica Treasure)

Figure 2
Flowerkey Bells
(Tunica Treasure)
73
Figure 3
Circarch Bell
(Tunica Treasure)

Figure 4
Flushloop bell
(Tunica Treasure)
presented papers - brown

early nineteenth century bell of the circarch variety which he believes was made in aldourne, wiltshire, in a foundry belonging to robert wells. wells operated this foundry during the late eighteenth century until his death, and commonly applied his initials to the manufactured bells. the business had been established as early as 1694 by previous members of his family (noel hume 1970:58-59), and it is possible that they too applied their initials to the bells. if this was actually the case, it is possible that many of the eighteenth century north american circarch bells had their origin in a small wiltshire brass foundry.*

the second class of bells is made out of sheet brass. these bells are generally much smaller and lighter than the cast brass bells. three types have been established under this class. the first type are of flush-edge construction (jelks et al 1966: 87). the sheet brass or copper had been worked into two bowl-like hemispheres which were then placed together so that the edges joined flush. they were then brazed. two flush-edge varieties have been set up, only one of which shall be discussed here. the flushloop variety (figure 4) has an attachment handle which was made from a thin strip of brass. the two ends of the strip were bent together, forming a loop, and were then pushed through a small opening in the top of the bell. the ends were then separated and soldered to the inside of the bell. the solder, which was also used to join the two body halves of the flushloop bell (ray and jelks 1964: 131), was a silver-colored metal. flush loop bells usually have circumferential grooves on each side of the equatorial seam. they are apparently a product of the manufacturing process. only two holes were made in these bells, both of which were connected by a narrow slit in the lower hemisphere. marks often consisted of an asterik, an arrow, the letter 'd', the number '4', a crown, etc. stamped on the bottom of the bell. the flushloop bell has the largest spatial and longest temporal distribution of any historic trade bell. it has thus far been recorded for 23 sites. an interesting development appears to have occurred in the form of the flushloop bell during the eighteenth century. in the early 1700's there seems to have been a great diversity in the size of flushloop bells, with very small specimens being found in association with large ones. by the middle of the eighteenth century these large bells seem to have given way to the increasingly more popular smaller flushloop bells.

another type of sheet brass bell is the flanged-edge bell. the construction of this bell consisted in turning up the edges of the two bowl-like hemispheres and then soldering them together, resulting in a narrow flange around the equatorial seam. one variety of this type is the saturn bell (figure 5). this particular variety has two holes in the lower hemisphere which are connected by a slit. the attachment handles are loops

*there are several sites in the williamsburg area which produced circarch bells. it is possible that some of these bells, or ones like them were actually made in the colonies for it is known that at least one gunsmith and brass founder (james geddy) in williamsburg was making small cast brass bells in the second quarter of the eighteenth century (noel hume 1970b: 11,13).
made out of a wide strip of brass which are attached to the bell in a manner similar to the Flushloop variety. Bells of the Saturn variety lack surface decoration, but marks, like the letter 'D', are sometimes found stamped into the lower hemisphere. The Saturn bell was extremely popular in the eighteenth century, sharing a similar spatial distribution as the Flushloop variety. Also consistent with technological trends in the latter is the great size diversity of Saturn bells in the late seventeenth and early eighteenth centuries, with only small specimens surviving into the nineteenth.

The last sheet brass type to be considered is the lapped-edge bell. In this type one of the two bowl-like hemispheres is lapped over the other and is crimped and usually soldered to prevent the two halves from coming apart. The Clarksdale variety (Brain 1975: Fig. 1) of this type is crimped in such a manner that a square-like flange is created. The attachment handle is made out of a wide strip of sheet brass and is secured by pushing the two ends through the top of the bell and soldering them to the inside. Two holes in the lower hemisphere are connected by a narrow slit and surface decoration is lacking. There is some evidence that the bells of this variety are of sixteenth century Spanish make, and their discovery at sites located along the route of DeSoto's travels through the Southeast (Ibid) suggests that this particular variety may be the oldest bell dealt with in this paper.

Up to this point the discussion on bells has been essentially descriptive, but this was deemed necessary in order to establish a foundation for the following interpretive analysis. In examining the various eighteenth century sites in which the described bell varieties were found it has been observed that the aboriginal sites of the Lower Mississippi Valley generally have a wider varietal range than other regions. Angola Farm (Ford 1936: 29,40) and Bayou Goula (Quimby 1957), both located in Louisiana, the first a Tunica site and the second a site occupied by a number of historic groups, each had four bell varieties. The Fatherland site (Neitzel 1965), the principal village of the Natchez Indians, produced five varieties. And finally, the Trudeau site (Brain n.d.), from whence the Tunica Treasure was collected, yielded a total of six varieties. In contrast, aboriginal and European sites situated outside the Lower Mississippi Valley have generally yielded only one or two varieties. An examination of 136 specimens from Fort Michilimackinac revealed only three varieties, none of which were of the cast brass class. A similar situation has been observed at Fort de Chartres in Illinois (Margaret Kimball Brown, pers. comm.). The author has no knowledge as yet of any cast brass bells being found in the Upper Mississippi Valley or the Great Lakes region. This unusual bell distribution suggests that the Indians of the Lower Mississippi Valley may have been influenced by a different trade source comparable in strength to that of the French.

In order to better visualize this unusual situation, it was decided to compare the distribution of the Flushloop and Saturn bells to that of the Circarch (Figure 6). These first two varieties are the most representative in the sheet brass class while the Circarch bell is the most popular variety in the cast brass class. The Flushloop and Saturn bells follow a distribution
Figure 5

Saturn Bell
(Fort Michilimackinac)
conforming nicely to the eighteenth century French trade sphere. Exception to this are the Albert Ibaugh site in Pennsylvania (Kinsey 1960: 89, 90, Fig. 7), the Burr's Hill site in Rhode Island,* and the Trigg (MacCord 1975) and Patowomeke (Scmitt 1965: 20, pl. 3a,5,6,7) sites in Virginia. However, these four aboriginal sites were occupied in the seventeenth century, a time at which French influence was not unusual along the East Coast.** The only other exception is the Spanish Fleet Camp in Florida, but the actual context of the bell from this site is in question (Clausen 1967: 125). In comparing the Flushloop and Saturn bell distributions to that of the Circarch bells, a notable difference is readily apparent. Circarch bells are found in areas which were most heavily influenced by English traders radiating out of Charles Town and Virginia. This conforms nicely with the earlier suggestion that most Circarch bells were manufactured in England. The only heavy zone of overlap between the two varieties seems to have occurred in the Lower Mississippi Valley. The sites in this area which have produced Circarch bells were associated with the Natchez, Yazoo, and Chickasaw Indians, all of whom were generally, or in part, of pro-British sentiment. Of particular interest is the fact that the Tunica sites of Trudeau, Angola Farm, and Haynes Bluff, also yielded Circarch bells, as well as a very large sample of other cast brass bell varieties. If the cast brass bells were a product of the English traders, as hypothesized in this paper, it seems strange that the Tunica, the staunch allies of the French should have had such an extremely large amount of British goods. It is possible that the French could have been trading British goods, but if this was the case, one would expect Circarch bells and the other cast brass varieties to be found west of the Mississippi, as are the Flushloop and Saturn bells. Thus far they have failed to turn up in this region. We must question the historic accounts as to the actual status of the Tunica Indians and must also reevaluate the role of the English traders in this region. These men, whom little was written about, may perhaps have played a much more important role in the history of the Lower Mississippi Valley than is generally supposed.

In conclusion, this paper was designed to present a brief but hopefully informative discussion on the aboriginal use, design and derivation of the various historic trade bells. Trade networks may be reflected by the distribution of the bells, but before this can be said for sure it is necessary to perform similar classificatory and distributional studies on each artifact type involved in the Indian trade. We cannot hope to be able to contribute to the historic and anthropological record unless we first establish a firm foundation for the material culture.

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*Heye Foundation Collections, New York, New York.

**The Saturn bell seems to have a closer tie to the French interaction sphere than does the Flushloop. It is possible that the Flushloop bells found at Patowomeke, actually were traded by the English, because it is historically known that "Captain Argoll traded copper, bunches of beads, hatchets, knives, bunches of bells, and scissors at Patowomeke itself" (Smith 1624: 38).
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BROWN, IAN

CARLSON, ROY L.

CLAUSEN, CARL J.

CLEREQ, CHRESTIEN LE
1691 Nouvelle Relation de la Gaspésie, Paris.

FORD, JAMES A.

HUDSON, CHARLES M.

JELKS, EDWARD B. et al.

KINSEY, W. FRED III

LINDSEY, MRS. E. M.

MACCORD, HOWARD A.
MORISON, SAMUEL E.

NEITZEL, ROBERT S.

NOËL HUME, AUDREY

NOËL HUME, IVOR

QUIMBY, GEORGE I.

RAY, CYRUS N. AND EDWARD B. JELKS

ROWLAND, DUNBAR AND A. G. SANDERS
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SCHMITT, KARL JR.

SMITH, JOHN

SWANTON, JOHN R.

THWAITES, REUBEN GOLD (editor)
VAN DOREN, MARK

WEDEL, WALDO R.
PART 2

HISTORICAL ARCHAEOLOGY FORUM

Brer Rabbit, Skunks, and the Devil:

The Dollar-Schuyler Debate

In the following paper, presented at the Gainesville conference, Clyde Dollar discusses the published papers of several historical archaeologists. Of the several individuals involved only Robert Schuyler chose to reply to Dollar's paper. The result is the Dollar-Schuyler debate.

Stanley South
Chairman
Conference on Historic Site Archaeology
"THE DEVIL LOOSE AMONGST US," OR, SOME OBSERVATIONS ON THE HISTORICAL ACCURACY OF CERTAIN HISTORICAL ARCHAEOLOGY

Clyde Dollar

Of the numerous accomplishments of the American academic community in the past century, few have outpaced the development of anthropology. Aided by archaeological techniques, this discipline has evolved truly brilliant methods for determining information from the material remains of the past. As a tribute to its scholastic value, its findings have been used by a number of related disciplines, such as sociology, psychology, and political science.

During the 1950's, developments at Colonial Williamsburg and Jamestown focused anthropological attention on historic sites as new arenas for investigation. The potential for joint use of archaeological and historical techniques to illuminate the past proved intellectually exciting, and within a brief span of years, what began as "garbage can" archaeology developed into a recognized pursuit, wealthy in terms of numbers of researchers and projects, and respectably complete with two professional organizations.

In the past two decades, American historians have also vastly broadened their scope of inquiry. Such diverse methodologies as those of economics, sociology, psychology, medical pathology, and statistics have been brought to bear on historical data, and the results have been quietly revolutionary. One would expect the field of historical archaeology, with its techniques for shedding light on particular events and past culture, also would be of considerable interest to the historian.

Such has not been the case. A search through the major American historical periodical literature for the past ten years revealed not a single article incorporating the findings of historical archaeology, and with the exception of some references to the work at Colonial Williamsburg, I have been unable to find even a footnote citation to an article on the subject. At a time when many historians are actively seeking the insights of other disciplines, for ours to be so largely ignored should give us cause for concern. And if the scientific value of a profession can be measured by the degree to which related disciplines make use of its findings, then this ignoring contains a message worth our serious attention.

A number of minor factors bear on this situation, but a debate of these would only obscure the heart of the problem. In the following paragraphs, I will discuss what I consider to be the major reason why the findings of a research field oriented to the recovery of data about past events and culture has so far failed to win the attention of the discipline of American history. This will take the form of a critique of several articles published in The Conference on Historic Site Archaeology Papers for 1972. I will evaluate these as an historian and will remark on the relative merits of their content.
The article by Jack and Kay Hudson, "An Analysis of a Cache of Gunflints from a Site in New Orleans," is a good point of departure.\textsuperscript{2} The cache in question consisted of 124 gunflints found under a walkway near a door at the Gallier House Site. Their article contained five pages of text, three statistical tables, one page of photographs showing 52 of the artifacts, and a bibliography of four entries.

The Hudsons described the circumstances of the finding of the cache and presented a brief history of the Gallier House Site. According to the authors, these gunflints could not have been deposited later than 1857. The Hudsons then proceeded to type their finds according to the pattern devised by Lyle Stone for the gunflints of Fort Michilimackinac.\textsuperscript{3}

The statistical manipulations to which they subjected their 124 flints produced scattergrams, clusters, nice columns of figures, perambulations of percentages, and a good deal of cross referencing with Stone's stones. On the basis of all this, they concluded that a number of their specimens were similar to those found at that northern fort, hardly a conclusion startling enough to justify the effort. Such numerical gyrations are sport which, if not especially productive, are at least not particularly harmful.

In the course of their article, the Hudsons divided their gunflints into English and French types, presumably on the basis of stone color.\textsuperscript{4} At least in the text they equated honey-colored flint with French origins and dark-colored stone with English quarries. On the basis of this division they concluded that their cache contained 123 French gunflints and one of English manufacture, and therefore, if their Gallier House sample was representative of the period, then "French flints were ... preferred over English [ones]."\textsuperscript{5} To the authors, this indicated that the people of New Orleans, "for reasons of their own, [may have] preferred and specifically ordered French gunflints," suggesting this to be to the exclusion of the English market.\textsuperscript{6}

Aside from their logic being strained beyond Aristotle, neither the premise nor its conclusion is historically correct. The Hudsons' equating of stone color with national origins is based on generalities at best too frail to withstand such rigid classification. To be sure, the stone from the French and English quarries differed in color, but not universally so. For example, French knappers in the late 18th century worked with stone listed as "blond," "brown," and even "blackish."\textsuperscript{7} In the late 1950's, during a visit to a number of the older French quarry sites, Carlyle S. Smith noted the presence of "black flint" in the workshop debris, stock piles, and private collections of the area.\textsuperscript{8} With this being the situation, I am not prepared to accept the premise that all black stone identifies an English gunflint.

The simplistic typology of nationalistic origins based on stone color further breaks down under the impact of evidence from the Brandon Quarries, the major source of English gunflints. In 1837, a certain Dr. James Mitchell visited these quarries and there learned that, as of that date at least, the French no longer made gunflints.\textsuperscript{9} From about that data until 1848, when the French had another of their revolutions, the stone from their quarries had been sent to Brandon for the English
knappers to shape into gunflints. On the basis of this evidence, I am not prepared to accept the premise that all blond flint indicates French knapping.

Even if their premise of national origins were acceptable, the Hudsons' economic conclusion derived from the 124 gunflints runs counter to substantial historical evidence. For example, five times more money was spent by Americans in 1840 for imported English arms than for similar items from France. By 1850, this spread sharply increased as the English firearms industry attained a world standard position. And five years later (1855), the imported value of English firearms almost doubled again while that of French weapons fell to a new low for the century. In the face of this evidence, for the Hudsons to conclude, based only on their one sack of discarded blond gunflints, that trade with France in such items was substantial stretches the boundaries of credibility to an absurd limit.

Three excellent sources of information existed, almost at the doorstep of the Gallier House, which, if consulted, would possibly have kept the Hudsons from making some of their errors. The first of these are the files of the New Orleans Price Current and Commercial Intelligencer, a trade newspaper published from 1822 through 1884 specifically to provide its readers with information about ship arrivals, cargo content, and port of origins. An examination of these papers should have suggested to the Hudsons cause to doubt their economic conclusion. The files of the New Orleans Daily Picayune, published since 1836 (to 1914) also contains shipping and economic information which should have been consulted by the Hudsons. And finally, no statement about trade in New Orleans should have been made without a thorough search through the records of the U.S. Customs Office and Port Authority for that city.

That the Hudsons did not research these documents prior to making sweeping economic generalizations based on only one bag of discarded gunflints displays a gross lack of scholarship. I found the article to be so minuscule as to be meaningless and so incompletely researched as to be worthless.

The next article for review is "Sandy Ground: Archaeological Sampling in a Black Community in Metropolitan New York," by Robert L. Schuyler. The article described the investigation of a Black settlement on the western end of Staten Island, the purpose of which, according to the author, was to recover "a varied but total range of [documentary, archaeological and ethnographic] data for all the major periods in the history of the community." The article contains 15 pages of text, seven pages of photographs, two maps, and a bibliography of 34 entries, of which only five are primary sources. Of these, four are maps and one an 1843 description of agriculture in the area. Appended is a four page list of cemetery inscriptions.

Schuyler began his text with a description and history of Sandy Ground, the Black community being investigated. This he followed with a discussion of his general research design and a description of the archaeological procedure for excavating two house structures and a possible
root cellar. He added the results of surface collections made at two turn-
of-the-century dumps and an examination of a community cemetery. He
concluded the article with a summary of the growth patterns of the Black
community, based on the evidence obtained during the investigations.

This would be an acceptable research effort if it were not so
mechanically and historically flawed. Schuyler used citations, indicating
a certain intent to employ historical methods, but he confused historical
writings with historical records, resulting in an article that went no
deeper than secondary sources. In addition, his writing style produced
occasional spots of lucidity, but these were often marred by areas of
considerable awkwardness. For example, in a five-consecutive
paragraph section, he constructed as many linguistic aberrations and
misuses. These took the form of ambiguously constructed phrases, verbal tense
switching within sentences, and colloquialisms masquerading as academic
phraseology. These simple technical errors detracted considerably from
the scholastic credibility of what he attempted.

It was in his history that Schuyler went desperately astray. On
the basis of six early Sandy Ground families known to have come from
Maryland, he saw the 1820's origin of the settlement as the result of a
planned migration of Blacks from that area. However, a check of the
1830 census for the state of New York, county of Richmond, township of
Westfield, which included the area now known as Sandy Ground, presents
differing information. As of the date of that census, the total Black
population in the area, including children, numbered 134 persons. Of
this total, Schuyler's 'six families from Maryland' probably numbered no
more than 30 individuals, or in other words, approximately 22% of the
total Black population at that time. On this evidence, I find his state-
ment that this was a 'planned migration' strained, to say the least.

About the community's period of fluorescence and stability, which
he ascribed to the time between 1850 and 1900, Schuyler stated that

For over a half of a century the people of Sandy Ground
maintained a high standard of living based upon the
flourishing oyster industry.... There was no overt
discrimination against Blacks in the industry....
Economically most activity in the community centered
around oystering but some local crafts ... horticulture
... and specific local industries were also important. Beginning with the 1850 census, a considerable amount of population data
became part of the permanent historical record, and therefore it is
possible to check the accuracy of Schuyler's statements. About Sandy
Ground, or the area of Westfield, it is possible to determine the name
of every person who lived there, the age, sex, and race of those persons,
the profession or trade of every male over 16, the value of their real
estate, the place of birth, marriage and education information, and
whether they were deaf, dumb, blind, insane, idiotic, pauper, or a
convict.

These census records show that Schuyler's statements about Sandy
Ground are erroneous. In 1850, the total population of Westfield numbered
2943 persons, of whom only 152 were Blacks (male or female). Of this Black population, only nine worked in a sea-related trade, only five engaged in farming activities, and only three listed Maryland as place of birth. None of the families had real estate in amounts indicative of a high standard of living.20

In 1860, the Black population in the area rose to 253 (121 males and 132 females), while the total population increased in approximate proportion. Blacks engaged in sea trades still only numbered nine, while those involved in farming increased to 14. Sixteen Black families claimed Maryland as their birthplace. Groupings based on real estate values are visible as residential areas, but the Blacks are noticeably scattered and not coalesced into a community. Again, there is no evidence to justify the claim that Blacks had a high standard of living.21

The Black population decreased to 242 individuals in the ninth U.S. census for 1870, and these were divided into 50 families. The total population for the area, however, increased markedly. Blacks involved in sea trades increased to 28, or barely 12% of the total Black community, and all of these had only hired-hand status, hardly indicative of a high standard of living. Residential groupings based on economic factors are still visible, with patterns now coalescing around trades, i.e., oystermen tended to live in clusters with other oystering families. Blacks and Whites of the same economic status are now mixed within habitation areas, but still there is no indication that a Black community is beginning to form. Also of note is the relatively high percentage (36%) of Black families having five or more members.22

By no means does this exhaust the demographic information applicable to Sandy Ground contained in these census reports. However, as I have no intention of doing Mr. Schuyler's researches for him, I will go no further. Suffice to say that, as of 1870, I see little or no evidence to support his contention that Sandy Ground resulted from a planned migration from Maryland, nor do I find sufficient justification to accept his statement that Blacks in the community maintained a high standard of living after 1850. On the contrary, there is considerable evidence of lowering standards and economic discrimination, even in the oyster industry. And finally, his assertion that "economically most activity in the community centered around oystering"23 has no basis in the primary sources of the period.

If the primary purpose of Schuyler's research was to recover "a varied but total range" of data, then certainly in documentary information he fell far short, resulting in his whole research design being built on sandy ground. His article has a thin and erroneous beginning, a weak and erroneous ending, and much flaying and flapping in between. It is a classical case of 'much ado about nothing,' a gigantic answer in desperate search of a question. As "an archaeological sampling of a community,"25 Mr. Schuyler's work at Sandy Ground is a failure.

The article, "Ethnography, Archaeology, and Burial Practices among Coastal South Carolina Blacks," by John D. Combes also needs a critique.26 Written with a skill not quite equal that of a sophomore, the article contained a brief examination of trash-strewn graves of

*{Editor's note: I have corrected the misspelling of John Combes' name throughout the manuscript.}
Blacks in the sea coastal areas of South Carolina. The assistance of an ethnographer was called for, and soon a number of myths from the Black cultural closet were used to explain why such items as medicine bottles and broken crockery decorated graves of Black deceased. The article ended with instructions to other archaeologists so that they too can find trash-strewn graves—a point hardly in need of belaboring. Mr. Combes devoted six pages of text and four pages of photographs to his thesis, and his bibliography contained three entries.

Combes concluded that "many of these [burial] practices clearly have African origins,"27 and with that gave no further thought to the matter. He might be somewhat surprised to learn that this mortuary custom of Blacks has not gone unnoticed by historians.28 He may be even more surprised to know that, had he done his historical homework and asked the right questions of his data, he might have shed significant light on an interesting cultural situation resulting from a rather recent historical event.

In May, 1864, the Union armies of General William Tecumseh Sherman began their famous march through Georgia. In the process of this campaign, Sherman's men cut a swath of war-torn earth almost sixty miles wide from Chattanooga through Atlanta and on to Savannah. As Sherman's army fought its way east to the sea, freed Blacks and their families joined the progress of the conquering force. At one time or another, some 30,000 of these displaced Blacks accompanied Sherman's army. In an attempt to resettle these people, in January of 1865 Sherman set aside confiscated and abandoned lands along the coastal areas from Charleston, South Carolina, south to Jacksonville, Florida. By midsummer of that year, more than 40,000 Black people, mostly from inland areas, had been settled along the sea coast. Subsequent events produced a disruption in this settlement process, but the ninth census for 1870 showed a notable percentage of Georgia-born Blacks still residing in several of South Carolina's coastal areas.29

Now that Mr. Combes has called our attention to this Black mortuary custom in his area, and if he really wants to do contributive research, he can ask himself the question: Did this custom exist in South Carolina prior to the Civil War, or was it introduced to that area by inland Blacks migrating from Georgia? In his researches, I am sure he will carefully check the records of the Freedman's Bureau for both Georgia and South Carolina30 as well as the extensive materials collected by the Federal Writer's Project.31 He will also want to read the several eye-witness accounts of the Black migration from Georgia listed in Thomas D. Clark's bibliography of Travels in the New South,32 a valuable point of departure for any research on the postbellum South. He should then read Sherman's papers in the National Archives, the official reports published in the Annual Reports of the Secretary of War, and the applicable materials listed in the 11 volume National Union Catalogue of Manuscript Collections.33 Then, he will want to study Guion G. Johnson's work, A Social History of the Sea Islands,34 along with W.L. Rose's Rehearsal for Reconstruction,35 Guy B. Johnson's Folk Culture on St. Helena Island,36 and Mason Crum's thorough work on the folkways, culture, and languages of the coastal Blacks of South Carolina, titled Gullah: Negro Life in the Carolina Sea Islands.37 After that, he should look through all issues of Phylon, the scholarly quarterly of Black culture published by Atlanta University,38 and then peruse the output of the South Carolina University Press, on which campus Mr. Combes now works.
And finally, being a thorough researcher, he will be interested in the account given by Mrs. Telfair Hodgson about Black graves in her area being decorated with the last articles used by the departed, broken pitchers and bits of colored glass, rude wooden figures like images of idols, and sometimes even patchwork quilts. Mrs. Hodgson wrote of life during the 1850's on her family's plantation located just west of Savannah, along Sherman's path of march. After doing this research and combining it with his archaeological investigations, Mr. Combes' work will then be worth attention.

The examination of the Kingsley slave cabins in Duval County of Florida, written by Charles H. Fairbanks, also warrants a critique and discussion. Stating that "archaeology can supplement and extend the understanding offered by written history [as it] is constantly concerned with process, rather than events, with technology rather than politics," Fairbanks attempted to apply this maxim to the problem of defining slave life on a Florida plantation of the early 19th century. His site was that of a slave settlement consisting of 32 cabins, arranged in a double row facing a road, and headed by two houses somewhat larger than the others, doubtlessly one of which was the quarters of the overseer. Fairbanks attributed the period of the cabins to 1813-1843 on the basis that these were located about 1000 feet from the main plantation house occupied by Zephaniah Kingsley during that time.

The article consisted of 20 pages of text, two pages of artifact photographs, six maps and charts, and a bibliography listing 23 entries. It was well written and generally clear, except for an interesting but unfortunately somewhat muddled historical introduction.

The archaeological investigation concentrated on one of the two larger cabins. This structure was partially excavated, and the well associated with it found but not fully developed. The second of the two larger cabins, scheduled for reconstruction by the Florida Park Service, presumably as slave domestic quarters, was only briefly investigated. The article contained detailed descriptions of methodology employed and results produced. The artifacts were described and used as a basis for generalizations about life in a slave settlement. All in all, the effort was commendable and almost worthwhile.

Unfortunately, a major error in research judgement and an important omission in research procedure in the opening paragraphs seriously mar this article. The first deprived Fairbanks of a great deal of comparative information (which might have prevented him from making a mistake in interpretation), and the other brought into serious question the very identification of the site itself.

In the second paragraph of his article, Fairbanks noted the existence of contemporary accounts of Southern slavery, but stated that these "generally are lacking in specific information ... on how the slaves lived and the details of their housing, crafts, family life, and daily activities." He acknowledged knowing of the writings of Frederick Law Olmsted, but dismissed these as being too political and economic in nature to be of interest to his research effort.
How unfortunate that through the error of not reading these contemporary accounts, he deprived himself of this description of sea island slave quarters:

The huts stand in a row, like a street, each detached with a poultry-house of rude planks behind it...No attempt at any drainage or any convenience existed near them...Heaps of oyster shells, broken crockery, old shoes, rags, and feathers were found near each hut. The huts were all alike windowless, and the apertures, intended to be glazed some fine day, were generally filled up with a deal of board. The roofs were shingled, and the whitewash which had once given the settlement an air of cleanliness, was now only to be traced by patches, which had escaped the action of the rain.  

or this description of slave quarters on a plantation along the seacoast:

[The houses were] ranged in a row, sometimes in two rows facing each other. They are 16 feet by 12, each appropriated to a family, and in some cases divided with a partition. They numbered on the plantations visited 10 to 12, and on [this] plantation they are doubled, numbering 23 double houses intended for 46 families.

or this account of the largest Black settlement on a certain seacoastal plantation:

There was a street, or common, two hundred feet wide, on which the cabins of the Negroes fronted. Each cabin was a frame building, the walls boarded and whitewashed on the outside, lathed and plastered within, the roof shingled: forty-two feet long, twenty-one feet wide, divided into two family tenements, each twenty-one by twenty-one: each tenement divided into three rooms—one, the common household apartment, twenty-one by ten; each of the others (bedrooms) ten by ten...Each tenement is occupied on an average, by five persons...There were in them closets, with locks and keys, and a varying quantity of rude furniture...Each cabin has a front and back door, and each room a window, on hinges....

This traveler also noted chicken coops, pig pens, and the size of gardens associated with the cabins. He even made a telling comment on the personal lives of the slaves by observing that "internally, the cabins appeared dirty and disordered, which was rather a pleasant indication that their home-life was not much interfered with."

At another time, this same traveler lost his way during a journey and stumbled on a slave settlement, sought out the overseer, who lived in the larger cabin at the end of the two rows of slave huts, and requested lodging. This was granted, and that night the traveler penned this description of the overseer's house in which he stayed:
I think that they [the overseer and his wife] gave up their own bed to me, for it was double, and had been slept in since the sheets were last changed; the room was garnished with pistols and other arms and ammunition, rolls of Negro-cloth, shoes and hats, handcuffs, a large medicine chest, and several books on medical and surgical subjects and farriery: while articles of both men's and women's wearing apparel hung against the walls, which were also decorated with some large patent-medicine posters.  

The traveler even noted in his account not only the name of the patent medicine advertised in the decorative poster but also the full text of that advertisement. That traveler, by the way, was Frederick Law Olmsted, the same writer whom Fairbanks summarily dismissed as being too economic in his writings to have usable information about the daily life of slaves!

These are by no means the only narratives that contain, in varying detail, eye-witness accounts of slave life and dwellings. In fact, more than 1100 (eleven hundred) such narratives are listed in Thomas D. Clark's bibliography of Travels in the Old South, and at least 33 of those listed apply to the areas of Florida's sea coast. And, as if Clark's material were not enough, the Federal Writer's Project for slave narratives of the Florida area contains hundreds more accounts giving specific information about slaves and the life they led. For Fairbanks not to have tapped these vast resources makes rather awkward his attempts to use archaeology as a supplement to "extend the understanding offered by written history."

The cavalier attitude toward history with which Fairbanks approached his excavation led him into making several serious errors at this site. Apparently, it never occurred to him why there would be two larger cabins at the head of the housing rows. The answer, which he missed by not using comparative historical sources, was that one of those probably served as the nursery/hospital for the slaves, a structure often noted in their settlements. This situation brings into question the identity of just what the Florida Park Service is reconstructing.

His second error, one of omission, is far more telling. At the site he identified as the "Kingsley Slave Cabins," Fairbanks found 32 house remains. Assuming an average of five persons per family, this number of cabins would provide housing for a settlement population of about 160 slaves and their resident overseer and his family. Yet, a check of the fifth U.S. census for the area, taken in 1830, reveals that the Black population of Zephaniah Kingsley's plantation numbered only 46 Blacks. This plantation population is hardly enough to justify construction of 32 housing units. Going a bit further, I discovered that the 1840 census for the area does not list a Zephaniah Kingsley at all, nor is there any indication that his plantation was even occupied as of that date. It is incumbent on Fairbanks to explain this hiatus—and an overlooking of some 160 Blacks is not a reasonable explanation for a census taken at a time when the South, disturbed over its slipping political power in Congress, was actively seeking to increase that power through added representation based on population. Until such time as Fairbanks presents evidence sufficient to cause me to change my mind, I reject his dating of these cabins to the Kingsley period.
A number of other articles in the 1972 Conference on Historic Site Archaeology Papers deserve critique and evaluation, but for the time being, these four will suffice. In making these comments, the intention has been to give substance to the numerous statements previously made by others, as well as myself, that history is a vital part of the research process on historical sites. I trust the point has been made.

The tragedy of this past decade in our profession is not that techniques of anthropological archaeology have been applied to historic sites, but that historical research at those sites has been so neglected. To be blunt, this is a lack of scholarship, and until we improve that quality in our work, there is justifiable reason why other disciplines will continue to ignore the findings of our profession.

A folktale, apocryphal no doubt, will serve to illustrate my concluding point. In the hill country of the Arkansas Ozarks, there is a story about the chastening of Brer Rabbit. It seems that at one time this famous character of Southern folklore got rich, very rich, and before long, began to put on airs. He snubbed his less wealthy neighbors and former friends and associated only with those who fawned on him. He took to dressing overly fancy and effecting manners haughty and prideful. He feigned learning on obscure subjects, and grew expert at making oracular observations. In short, he became a snob.

The Devil, seeing all this and sensing an opportunity to have some good, clean, devilish fun, rose to earth, took human form, and chanced to meet the wealthy bunny one day. "Fine day, Brer Rabbit," greeted the Devil. "Humph," sniffed the haughty hare at this stranger who smelled of sweat and somewhat of brimstone.

The Devil pressed on: "Brer Rabbit, you look like a man of great wealth; what can you give me?" Replied the well-dressed cottontail, "I have nothing to give you that you would know how to use."

Undaunted, the Devil said: "Brer Rabbit, you look like a person of great knowledge; what can you tell me?" Returned his conceited companion, "I have nothing to tell you that you would understand."

The Devil tried again: "Brer Rabbit, you look like a man of great wisdom; what can you teach me?" Irritably retorted the haughty hare, "I have not the time to be bothered with such matters."

"Well then," said the Devil, "If you have nothing to give me that I could use, if you have nothing to tell me that I would understand, and if you won't bother to teach me any of this, then you are not wealthy but poor, not learned but haughty, and not wise but stupid."

Outraged at this impudence, Brer Rabbit swung his ebony and gold cane at the Devil, striking his head just where the horns were hidden. Instantly the human form changed to that of his satanic majesty, complete with billowing smokes, belching clouds of brimstone, and a great, ghastly voice that cried, "Aha! I've got you now, you bumptious rabbit!" Clawed hands, red and reeking of unholy odors, clutched at the fine clothes of the startled cottontail. But Brer Rabbit, moving with a speed born of panic,
shed his clothing, and running stark free of his finery, screeched for all his soul's worth, "The Devil loose amongst us! The Devil loose amongst us!!"

Old timers in the Arkansas hills say that on dark and windy nights, the Devil can still be heard in the valleys of the Ozarks, lurking there, just waiting for Brer Rabbit to put on airs again.
1. For example, the prestigious and scholarly *Journal of American History*, published by the Organization of American Historians, the largest professional organization of its type in North America, contained no such use of the findings of historical archaeology in any article published in that journal from Vol. LI (June, 1964) up to the present issue. This same situation appears to also hold true for the other national professional historical quarterlies.


4. Much of the Hudsons' analysis of their flint cache is based on information provided in Stone's article (see note 3 above), but Stone does not offer evidence for his distinguishing color characteristics either. Instead, he apparently relied heavily on various articles by Henry Hamilton and others (see Stone's bibliography for his article in CHSAP, V, pp. 32-34), but even these are vague on this point.

5. CHSAP, VII, p. 8.

6. Ibid., pp. 8-10.


10. Ibid., p. 31.


12. Ibid (1850), Serial Set #386. From England: $206,043; from France (both seas): $6,358.

15. Ibid., p. 22.
16. Ibid., pp. 26 and 31.
17. Ibid., pp. 18 and 37.
18. Fifth Census of the U. S., 1830, State of New York, County of Richmond, Township of Westfield; U. S. National Archives, MF 1911, M19, Reel 106.
23. CHSAP, VII, p. 20.
24. Ibid., p. 22.
25. Ibid., see title of Schuyler's article.
26. Ibid., pp. 52-61.
27. Ibid., p. 59.
28. For example, see Newbell Niles Puckett, Folk Beliefs of the Southern Negro, University of North Carolina Press, Chapel Hill, 1926, pp. 104-107. Puckett provided almost four pages of text describing the grave decorations about which Combes rhapsodized in vacuo. This author also included two photographs of such grave-goods as seen in Mississippi, as well as numerous citations for other sources of information. From among these, I cite: H.C. Bolton, "Decoration of Graves of Negroes in South Carolina," Journal of American Folk-Lore, Vol. IV, 1891. For a recent summary of historical awareness of this custom, see Bennet H. Wall, "An Epitaph for Slavery," Louisiana History, Vol. XVI, No. 3, Summer, 1975, pp. 247-248. I am aware that Wall's article appeared in print after Combes published.
amount of material can be found in *The American Slave; A Composite Autobiography*; Vol. I, *From Sundown to Sunup: The Making of the Black Community*, by George P. Rawick, Westport, Conn., 1972. Volumes II and III of this series contain condensed records from South Carolina; Volumes XII and XIII the Georgia narratives.


38. *Phylon: The Atlanta University Review of Race and Culture*, published since 1940 by the Atlanta University, Atlanta, Georgia.


40. Also of interest will be Charlotte Forten's "Life on the Sea Islands," in the *Atlantic Monthly*, May, 1864, pp. 587-596, and June, 1864, pp. 666-676. In addition, compare the descriptions of Black burial grounds in Charleston and Savannah as seen in 1854 by Frederick Law Olmsted, recorded in his *Journey in the Seaboard Slave States*, New York, 1904, Vol. II, pp. 31-35.

41. CHSAP, VII, pp. 62-93.

42. Ibid., p. 62.

43. For example, the reader is left totally in the dark about post-1843 events at the site, an omission by Fairbanks which, as will be seen, allows for an error of considerable magnitude.

44. CHSAP, VII, p. 62.

45. The full text of what Fairbanks wrote: "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 Kenble'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..." (and here Fairbanks cited Frederick Law Olmsted—with an erroneous citation, I might add).

Ibid.

46. Written by William Howard Russell, who visited the Trescot Plantation on Barnwell Island, off the coast of southern South Carolina in April, 1861, several months before the capture of the region by the Federal Army; see William Howard Russell, My Diary North and South, London, 1863, pp. 146-147.


48. Written by Frederick Law Olmsted in 1854 while visiting a plantation on the sea cost of Georgia; see Olmsted, Journey in the Seaboard Slave States, Vol. II, pp. 49-50.

49. Ibid., p. 50.


51. Ibid., pp. 175-176.

52. Thomas Dicynysius Clark, editor, Travels in the Old South; A Bibliography, in three volumes; University of Oklahoma Press, Norman, Oklahoma, 1956.

53. See note 31; Volume XVII of this series contains the condensed Florida narratives.

54. CHSAP, VII, p. 62.

55. For example, see Olmsted, Journey in the Seaboard Slave States, Vol. II, pp. 43 and 50-53.

56. Ibid., p. 50.

57. Fifth Census of the U. S., 1830, Territory of Florida, County of Duval; U. S. National Archives, MF 1911, M19, Reel 15.

58. Sixth Census of the U. S., 1840, Territory of Florida, County of Duval; U. S. National Archives, MF 1912, M704, Reel 36.
THE WRITTEN WORD, THE SPOKEN WORD, OBSERVED BEHAVIOR AND PRESERVED BEHAVIOR: THE VARIOUS CONTEXTS AVAILABLE TO THE ARCHAEOLOGIST

Robert L. Schuyler

In a paper presented at the Sixteenth Annual Conference on Historic Site Archaeology and published in this volume, Clyde Dollar has criticized the published research of several historical archaeologists. In general he proposes that historical archaeology has been artificially limited in its contribution to general scholarship because of the unwillingness or inability of its practitioners to undertake accurate, primary documentary research. More specifically he criticizes, among other projects, my research at Sandy Ground (Schuyler 1974) on the basis of what he conceives as a faulty outline of that community's history. He uses a specific source, the Federal Census (1830, 1850, 1860, and 1870) in an attempt to prove that most of the basic conclusions concerning the formation and nature of Sandy Ground are erroneous. In this reply I will take the opportunity to discuss some general points and then, in a second part, I will show that Dollar's criticisms are meaningless from the perspective of either a historian or scientist.

Part I

Scholars concerned with Man may approach their subject matter from one of two perspectives. They may base their investigations on direct or indirect observation of human behavior (an etic analysis) or they may concentrate on the views and beliefs that the subjects hold concerning their own behavior (an emic analysis). An individual study may be purely emic or purely etic or involve elements of both approaches. Access to emic or etic information, however, is strongly influenced by the context in which the data are found. If the researcher is contemporary with the situation he is studying, he may directly observe human beings, their behavior, artifacts (in the broadest sense), and the use of artifacts. He also has direct access to the beliefs of his subjects through participant observation or the more focused use of informants and questionnaires. If the researcher is investigating a past situation, he may gain indirect insight into human behavior as it is preserved in the archaeological record, documents, and human memory.

Although studies of past events may involve an ethnographic dimension, normally the data are limited to three contexts.†

Archaeological Context

Archaeological data present the researcher with one of the strongest lines of evidence for reconstructing past human behavior. Archaeology is the principal source when it stands alone as it does for the totality of prehistory and for the numerous prehistoric interludes within the historic period. Whether it is a "dark age," an undocumented frontier situation,
or a nonhistoric enclave as seen in the life of the slave and peasant, archaeology is a major bridge across the lacunae in the historical record. More importantly, archaeology continues to provide significant information even when its findings are paralleled by a complete written record. This fact has not been convincingly demonstrated on the substantive level until very recently. Projects, such as the study of the Mott farmstead (Brown 1976) and its work on faunal analysis (Bowen 1975) and family history, clearly answer the question so often asked of the historical archaeologist—Why dig things up when written sources are available?

For all its strengths, however, the archaeological record is uncommunicative in regard to an entire range of data. A purely prehistoric assemblage can provide direct and ample information on all aspects of culture provided the analysis is etic in orientation. Artifacts do not speak. Artifacts provide no information on the emic level. The emic aspect is present but unless there is documentation or a "direct historical/general ethnographic analogy" it is uninterpretable. Even a cursory examination of Olmec art, for example, reveals a combination of symbols (were-baby, clefted head, fangs, and snarling mouth) that are obviously part of the iconography of an archetypal Mesoamerican religion. What set of beliefs and values produced and gave meaning to these traits is lost unless some direct tie to the present, such as Furst (1972) assumes in his shaman-jaguar interpretation based on ethnographic analogy, is found. Psychoceramics (Emerson 1974) and speculations aside, totally prehistoric materials are mute.

Documentary Context

Documents can be the basis for either an etic or emic analysis, although they have almost never been used for the latter by historical archaeologists. Historical writings may record human behavior in the form of observations of human actions or statistical data. They also, of course, contain the values and beliefs of those writing the records and those written about thus giving direct access to the emic level. Nevertheless, this availability of the emic dimension is both the greatest strength and the greatest weakness of the document. As all documents were written by one or more persons, and not left inadvertently and directly as a result of human behavior, their emic aspect also erects a screen between the researcher and direct access to preserved human behavior. I once heard a colleague in historical archaeology say (he had been drinking) that the only thing a document proves is its own existence. Such a view is too extreme as there are methods of internal and comparative contextual analysis for evaluating documents, but it does emphasize the emic nature of written records.

Robert McC. Adams (1974) has lately offered an innovative interpretation of the nature of Mesopotamian society that relates to this problem. Adams proposes that the primary archives of Mesopotamia were produced by an urban elite with a strong commitment to and bias for city life. Thus the traditional reconstruction of the ancient Near East by historians is based on sources that distort what is a much more complex situation. The city was only one segment of an urban-village-nomad network within which even the city dweller did not necessarily lose his ability to return to a
nonurban orientation when events called for such a "devolution." Yet only one segment of the continuum appears in the records.

Even in the most focused and systematic form of document, such as the questionnaire that underlies a census tract, the emic aspect is all pervading and dominant.

Oral History Context

Oral history is a bridge between documentary history and ethnography. Human memory preserves both emic and etic information, but it also structures the data by placing a series of barriers between the past and the investigator. The emic aspect is intensified with oral sources because although beliefs about past behavior may survive, the behavior itself is not directly approachable. Just as the beliefs of a person recording events as they unfold always distort the past, consciously or unconsciously, so do the beliefs of a person undergoing an interview distort past behavior and concepts. In fact, there may be more than one screen erected; the original psychological and cultural blinders in force in the past and the present beliefs of the informant. However, the desire to rewrite one's past, along with the simple fallibility of human memory, is counterbalanced because oral history does present an opportunity to understand the cultural matrix of the informant and to cross-examine the sources to a degree that is usually impossible with written sources.

Oral history has been a part of general historiography since Herodotus and Thucydides interviewed survivors of the Persian and Peloponnesian Wars; but it has probably, at least until recently (cf. Montell 1970, especially Preface), seen a greater development in anthropology. Cultural anthropological techniques, particularly the "new ethnography," depend on living informants and so naturally relate to oral history. Indeed, much of our knowledge of traditional American Indian and African cultures is based on such "memory ethnography." Questions concerning the differences between documentary and oral sources and their analysis are crucial and cause oral history to overlap as much with ethnography as with historiography.

Part II

Differing accounts of the history of Sandy Ground as offered in my original article and by Dollar in his critique are the product of two factors. First, although Dollar seems unaware of it, each account is in the main drawn from two different contexts—oral history and documentary history. Second, Dollar's account is grounded on a failure to recognize the nature and complexity of primary, written sources.

As most readers will only have a passing interest in the details of the history of Sandy Ground, I have relegated some of the data pertinent to a complete evaluation of Dollar's interpretations to a series of footnotes. The more general points will be briefly covered in this section, and I ask the reader to keep in mind that my statements are in response to Dollar. I am not offering a documentary historical synthesis for Sandy
Ground and so will limit my critique to the sources used by Dollar and not use other census schedules, state or local censuses, or the many other archival records.

**Preliminary and Final Reports**

Unfortunately Dollar opens his review with a misunderstanding of the nature of my paper on Sandy Ground. He views it as a final report when it is obviously a preliminary, programmatic statement which discusses the research design for Sandy Ground, the potential range of data, and presents some initial findings on the origins of one artifact category (glass containers) from a single excavated feature. The historical outline presented is drawn from the work of Dr. Mina Wilkins (1943a,b,c,d,e, 1972) and is in the main based on oral history. Her study is the only historical synthesis presently available for Sandy Ground.

**Primary and Secondary Sources**

Dollar's major criticism is that my paper is based on historical "writings" (i.e., secondary sources) rather than historical "records" (i.e., primary sources). In his comments Dollar seems to limit primary sources to documents thus excluding oral history. There is clear reference in my paper not only to Wilkins' historical synthesis but also to her original field notes and a recent interview with her. Texts or notes based on interviews, although different from some types of documents, are also primary sources. In fact, the method of interviewing that Wilkins used usually produces more extensive and reliable data than the type of interview on which a census is grounded. I will return to this point in Part III of this paper.

**Use and Misuse of Primary Sources**

After misinterpreting the nature of my article and arbitrarily limiting "records" to written sources, Dollar then uses such a source to criticize my presentation of Wilkins' historical outline. Although it is clearly stated in my paper that documentary research was being developed (Schuyler 1974: 43), this fact is ignored as is the specific reference to the availability of census data (Schuyler 1974: 22). Local historians on Staten Island are examining the census schedules, as well as other archival sources, on Sandy Ground. They recognize the obvious fact, however, that specific historic facts cannot be taken at face value nor removed from context.

Dollar in his survey of the censuses does not seem to be aware of the complexity of written sources. He directly offers a "reading" of the census tracts from which he draws several conclusions. The questions around which these conclusions center are legitimate points of debate but Dollar's interpretations are meaningless. He does not realize that it is necessary to evaluate specific primary sources, that the data contained in such a source must be analysed not "read," and that historical facts cannot stand in isolation from some meaningful context.
Although Dollar discusses several minor points his major conclusions consist of four basic statements that contradict my presentation of Wilkins’ findings on Sandy Ground.

1) Sandy Ground is not a product of a planned migration from Maryland.²

   **Basis:** Dollar grounds this inference on a comparison of the proportion of Marylanders to the total Black population of southwestern Staten Island.

   **Fundamental Error:** Notwithstanding the fact that Dollar recognizes that Sandy Ground is located in Westfield, he ignores this relationship in his calculations and inadvertently shifts the focus from Sandy Ground to a larger, arbitrary unit (see Fig. 1). It is true that the census data are organized into the four traditional regions ("townships") that divided Staten Island during the 19th century. Westfield is one of these divisions. It is not possible or permissible, however, to use the Westfield data as a unit in reference to Sandy Ground. The majority of the Blacks in Westfield, with a few exceptions, were not involved in either the foundation or later history of Sandy Ground. To conclude that people giving Maryland as their state of origin only approximate "22% of the total Black population" in 1830 or that "barely 12% of the total Black community" were involved in oystering in 1870 are misleading and in fact meaningless statements. It would make as much sense to compare the proportions of Blacks of a Maryland origin with the total White population of Westfield, or all of Staten Island for that matter. There is no evidence of a Black "community" corresponding to the boundaries of Westfield and to assume that all Blacks in an arbitrary geographic section are equivalent units (which can be added, subtracted, or divided) is a blatant example of racist thinking. Many of the Blacks in Westfield had a different origin than the Sandy Grounders, a different social relationship with the general Staten Island population, a different economic orientation (e.g., servants resident with wealthy White families), different church affiliation, and different, if any, relationships to the Sandy Ground community itself.³

   **Evaluation and Conclusion:** Dollar may not be familiar with local Staten Island history but there is no excuse for this error. A perusal of any good 19th century map that carries the regional divisions shows Westfield covering most of southwestern Staten Island and including a number of towns (Rossville, Princes Bay, Tottenville), small settlements (Sandy Ground or Woodrow being one), and scattered farms. The problem of differentiating the residents of Sandy Ground from the total population is a difficult and crucial task.⁴ It cannot be ignored or glossed over. Documents must be analysed not "read."

2) Sandy Ground was not, at least as late as 1870, a discernible community. Although there are some occupational clusterings, Blacks were scattered and/or intermixed with Whites.

   **Fundamental Error:** Again the confusion between Sandy Ground and Westfield invalidates Dollar’s conclusion but he compounds this error with a second oversight of equal magnitude. In his inferences on the degree of concentration or scatter for Westfield Blacks he clearly assumes that he has direct access to such patternings in the census data. This is not true. The censuses do not present a verbal map of human settlement, rather they
Figure 1.

MAP SHOWING THE RELATIONSHIP BETWEEN WESTFIELD AND SANDY GROUND

Figure is based on the M. Dripps Map (1850) and shows the four 19th century townships of Staten Island (Richmond County). Within Westfield Sandy Ground and some of the more important towns are also designated.

(1) Sandy Ground
(2) Rossville
(3) Princes Bay
(4) Tottenville
contain an indirect and very nebulous presentation of residence patterns. Individual entries are organized on the basis of the order in which the individual dwellings and families were visited and it is not immediately clear how the census enumerator covered a given area. Using other sources (documents and oral accounts), in conjunction with the censuses, it is clear that Sandy Ground exists as a racially mixed but nucleated settlement in both the 1870 and 1860 censuses. However, the total settlement cluster of Sandy Ground is partially masked and artificially scattered by the manner in which the census was compiled. This factor is probably the most difficult to control in using the tracts but cross-reference to maps, other documents, and oral accounts partially solve it. Tentatively I suggest that what occurred was that the census taker did not cover a given settlement as a unit but rather ran several transects through an area. If, for example, the person was moving up Bloomingdale Road (N - S), a main thoroughfare for Sandy Ground, he might also cover Harris Lane, a dirt side street that was a deadend, but would not necessarily turn off on Woodrow Road (W - E) or Sharrots Road (E - W). Rather he or she would probably continue down Bloomingdale directly into Rossville thus passing outside of Sandy Ground proper. Such probable routes become evident only after examining local maps or by walking the area as I did frequently during the archaeological survey during 1971, 1972, and 1973.

Again, documents must be analyzed not recited.

Evaluation: It would seem self-evident that human settlement spread along roads is lineal in its pattern and not random or equidistant. This situation in turn should alert the researcher to the creation of a very complex and confusing arrangement if a house by house survey is the basis of recorded information. Which way did the person walk, when and why did he or she turn off on a new road, and what was a "natural unit" for completion? Dollar ignores all such complexities because he suffers from the blinders of a ruling hypothesis; that is, he is so committed to disproving Wilkins' conclusion that he starts to simplify and force the data into the direction of the "right" answer.

Conclusion: Settlement pattern as preserved in the censuses is the most complex and difficult problem encountered in the analysis of these data. Although there are maps dating from the 1850's these seem to be incomplete and later maps, at least until the appearance of detailed atlases after the turn of the century, are only slightly modified copies of these originals. Nevertheless using maps and other sources (e.g., gravestone inscriptions) it is clear that Sandy Ground existed and does exist today.

It has geographical and social integrity. It is clustered in a specific area and although, as I originally pointed out (Schuyler 1974: 20), it was always intermixed with White dwellings, there are visible breaks between Sandy Ground and adjacent nucleated areas in Rossville, Pleasant Plains-Princes Bay, the new suburban housing development, and Charleston (see Figures 1 and 2b in Schuyler 1974). Socially it did and still does center on the Black church. It is and was a small community and it is here that Dollar also errs. He does not have the prerogative of determining proper demographic size for a community or to use this device to "define" it out of existence.
Historically, if one analyzes the census data, the existence of Sandy Ground is demonstrated by the patternings of the 1870 and 1860 censuses. It is not clearly delineated in the 1850 census and there could be several reasons for this situation.

3) Sandy Ground was not economically focused on oystering; indeed most Blacks in the area were in other trades.

**Basis:** Dollar compares the occupational status of all Blacks for each decade between 1850 and 1870 and finds that only a minority are in oystering.

**Errors:** Again the fundamental error of confusing the units of study invalidates his conclusion. It is true that a detailed study of the economy of Sandy Ground in 1870, for example, might show a greater division of income sources than the oral history would imply. At the same time, such a diversity would not necessarily contradict the claim that the economic focus of the community was oystering. Before its economic diversity in the 19th century can be understood there are several problems with the census data that have to be recognized.

When the perspective is shifted from Westfield to Sandy Ground, there is a concentration on oystering although other occupations are also involved. One problem is that it is not clear if the terms "boatman," "oysterman," and "laborer" are mutually exclusive categories. If younger males from Sandy Ground worked on a White oyster boat, would they be "oystermen" or "laborers"? If sons worked, perhaps only part time, for a father or other relative on an oyster boat, how would they be classified in the census?

**Conclusion:** Oral history repeatedly pictures Sandy Ground as an "oystering town." It is possible that such an image would not correspond with a tabulation of the total sources of income at a given point in time. Such a finding would not mean that the oystering image is false; in fact, many of the specific opportunities in such economic diversity might be indirect spin-offs of oystering (e.g., the Bishop blacksmith shop). The documents do not at this stage of analysis contradict the reconstruction based on oral history.

4) The inhabitants of Sandy Ground did not have a high standard of living and their standard lowered through the 19th century.

**Basis:** Dollar primarily supports this inference with an examination of "real estate" figures from the various tracts.

**Error:** Residents in Sandy Ground, as I pointed out, owned small units of land because they were not farmers. Their specialized horticultural activity involved backyard gardening not farming. Therefore, it is not surprising that their "real estate" valuation would be less than that of Staten Islanders involved in farming and certain other occupations. The "real estate" figures in isolation mean nothing; they will only be given pertinence, if at all, by detailed comparative studies.

**Evaluation and Comment:** Part of this problem is created by my use of the vague phrase "high standard of living." I did not mean to imply, nor does the oral history suggest, that the inhabitants of Sandy Ground were
wealthy. In no sense are they to be classified with the minority of wealthy White oystermen who built mansions on Richmond Terrace on the northern end of the island. After the initial settling-in period, however, Sandy Ground was a stable and economically successful community until after the turn of the century. Its inhabitants owned their own houses and had steady, if varied, sources of income. In a few cases they owned boats and had manipulative amounts of capital.

* * * *

All points Dollar attempts but fails to make are legitimate questions, but questions for which answers based on documentary research are not yet available. Dollar's ersatz research only confuses the situation because of his naivete concerning historiography.

Part III

If one source enables a scholar to gain direct access to past events while another by its nature offers only indirect access to the same events, it would seem that a reconstruction based on the former would be stronger by definition. Most documents give us direct insight into emic phenomena and indirect views of behavior (etic), while oral history removes both types of phenomena a step further from the investigator. As has been shown, however, it is not only the intrinsic nature of the written account compared to the verbal account but also the individual features of the specific item and the manner in which it is analysed that determines its worth.

Both documentary and oral history are similar in that they, unlike the archaeological record, do not directly preserve the remains of human behavior. They are different in that the recording of past events in documents may be contemporary with the event (an "eyewitness account") while it is not when only the memory of events is preserved. Many documents, of course, also involve a memory factor in that they were written after the event, sometimes years later. The true contemporary document is a rarity among archival sources. A census is one of the exceptions and when properly utilized is a significant source of information.

An Isolated Document

The accuracy of primary, written sources is very difficult to evaluate except when independent sources of information, usually other documents, exist. Unless a document can be placed in a general historical context it must be used with extreme caution. Although Dollar compares censuses from different decades, he treats them as isolates removing them from any meaningful context. The only prepared context, that of oral history, he only uses in a negative manner. This approach forces him to accept the census "facts" as self-validating. Such an oversight is not allowable. In fact, the specific nature of a census more than counterbalances the fact that it is coeval with the information it contains. If such data are used for broad, regional studies, as the cliometric historians have recently done for slavery, certain sources of error may not be statistically significant, but if the focus is on a local sequence of events, these flaws are accentuated.
What is a census? Census schedules, at least the variety used by Dollar, are an attempt to collect quantified data by asking informants a set of standard questions. Anthropologists have learned that the questionnaire is probably the most misleading technique for collecting ethnographic data because the specificity and rigidity of the format distort the information obtained. Frequently the results have more to do with the people drawing up the questions than with the people being questioned. An additional factor concerns the conditions under which the questionnaire is administered. Who is the census enumerator? Today there is usually an attempt to use local people whenever possible, and it is known that one member of Sandy Ground did census tracts in the 20th century, but the 19th century circumstances are not clear. If a person was from Rossville or Tottenville or was White or Black could be important influences on the results obtained. Another problem is the possibility that the terms used in the censuses after 1850 did not carry the same meaning for enumerators separated by at least a decade. Historians working with censuses for the antebellum South have encountered similar problems (Bonner 1974: 36).

Dollar opened his discussion of the censuses with a rather amusing statement:

Beginning with the 1850 census ... it is possible to determine (for Sandy Ground and Westfield) the name of every person who lived there, the age, sex, and race of those persons, the profession or trade of every male over 16, the value of their real estate, the place of birth, marriage and education information, and whether they were deaf, dumb, blind, insane, idiotic, pauper, or a convict.

How many people are going to list the number of "insane" or "convict" members of their families for a census taker? Even if the enumerator is a local person and knows the real situation would he or she record it? Is it surprising to discover that these columns are almost uniformly blank on the census forms? All such categories prove is that they were of interest to the bureaucrats that composed the census.

Selecting a less obvious but perhaps more pertinent category, is it clear that people would give their correct place of birth? Why not? If in 1850, for example, some of the Marylanders then residing in Sandy Ground were squatting on land, as the land records seem to indicate, would they give their place of birth which might draw the attention of Staten Island officials or would they simply say New York? Indeed if they could avoid being covered by the census survey, which might not be too difficult if the enumerator was a White outsider, that might be an even simpler solution to this problem. Whether such behavior occurred or not is unknown but some of the Marylanders that oral history and other written sources would place at Sandy Ground in 1850 are not listed in the census.

As a final example, there is the category of "personal estate." This column is frequently left blank but when figures are given, such as $100 for John Holmes or $400 for R. Langdon in 1860, what do they mean? Is "personal estate" the value of a person's material possessions, a potential annual income, the amount of savings or cash on hand, or some combination of these sums? What did the term mean to the person being
questioned or the person asking the question? How would you answer it today? More importantly did this category, or the other phrases, consistently carry the same meaning from enumerator to enumerator or from census to census?

A Historical Synthesis

Dollar avoids problems of internal analysis by ignoring the need for a general documentary context which might alert the researcher to such distortions. Nowhere is this lack clearer than with the very foundation of his critique--primary versus secondary sources. He does not comprehend that Wilkins' synthesis for Sandy Ground, which is a secondary source, is stronger and more meaningful than an isolated series of "facts." Her image of Sandy Ground is based on a careful internal study and evaluation of primary oral sources and then an interpretation of these data.8

An examination of Wilkins' (1943c,d,e) original notes and an interview with her (1972) show that she used adequate field techniques. She did not enter the community via a formal institution; in fact, she avoided the church since the minister was an outsider. After another investigator had attempted a study of Sandy Ground and failed, Wilkins simply walked in, found a child hanging clothes and was thus passed from family to family. Although she was White, she was also a Virginian and her background created an excellent rapport which lasted long after her study. She interviewed over 30 people and had six key informants making it possible for her to cross-check statements and reevaluate nebulous or contradictory points. She also used written sources (censuses, land, probate, and church records) to check certain ranges of data.

A documentary synthesis equivalent to Wilkins' oral history might well produce a different and more detailed picture of Sandy Ground but Dollar does not offer such a synthesis and it does not yet exist. When it is produced by historians who understand primary documentation I predict that it may contain useful insights and data not available in the oral record but unless there are internal records such as diaries and letters, and these have not been found, it will not match the oral reconstruction of the culture history of the community.

Historical research must be based on primary sources but raw data cannot be substituted for historiography. It is the secondary, scholarly analysis ("historical writings") that gives meaning to primary sources, not the reverse.

Conclusion

In an earlier presentation at the Conference on Historic Site Archaeology, Dollar (1968) offered "Some Thoughts on Theory and Method in Historical Archaeology" and was severely criticized by several scholars for his misuse and misunderstanding of anthropological and archaeological concepts and terminology. Some commentators (Williams 1968) simply dismissed Dollar's paper because it displayed an acute lack of familiarity with their field. Nevertheless, when I reviewed that discussion, I
attempted to be fair by not dwelling on these feelings but rather by trying to understand and evaluate the basic issues being debated (Schuyler 1970: 229-231). I shall take the same approach here.

Dollar's principal point seems to be that historical archaeology is being ignored by other fields, especially by professional historians, because historical archaeologists have failed to undertake accurate or extensive documentary research. This interpretation is incorrect. It is quite evident, especially if a particularistic, fact-oriented Dollarian version of historiography is espoused, that historical archaeologists are aware of primary documents and use them extensively and successfully to answer specific historic questions. A number of writers (e.g., Griffin 1958; Schuyler 1972) have discussed the desirability of shifting the focus from specific to broader problems and cultural reconstructions. Even if such an expansion occurred and historical archaeologists were to produce historical syntheses ("culture history" or "reconstructions of past lifeways") this shift would not necessarily draw the attention of historians. Successful endeavors in documentary history would only result in the generation of good local history and there are hundreds, if not thousands, of academic and regional historians already producing similar monographs.

Historical archaeology, with a few outstanding exceptions, has fallen short of its potential because of a set of more complex problems. Archaeologists have not fully recognized that there are several ranges of data which exist in related but different contexts and they have failed to consider the differences of these contexts, their interrelationships, and their potential unification into final, more replete reconstructions. Because of this oversight, they have yet to even convincingly demonstrate the unique strengths of the context peculiar to their field--the archaeological record.

In Part I, I listed these contexts and reviewed their strengths and weaknesses; but when their interrelationship is seen in the form of a table (Fig. 2), the potential for historical archaeology is emphasized. It is only when the context of archaeological data is contrasted with other sources which deal with past events that its unique ability to serve as the basis for an etic analysis becomes clear. Several factors including the manner in which historical archaeology developed in America, limitations arising from an association with the restoration movement, and the short-term, limited nature of most historical excavations, have masked what should have been an obvious strength. However, if the etic-advantage of archaeology is recognized, such a realization would not differentiate historical from prehistoric archaeology. It is only when documents (written records or oral accounts) appear that a fuller and different type of archaeology is possible. Yet most historical archaeologists use documents and other nonarchaeological data to solve specific or at best etic-oriented problems ignoring the emic potential of archival, oral, or ethnographic contexts. As scholars who were formerly limited to etic data, archaeologists tend to approach documents in a fashion that abridges much of the "historical" aspect of historical archaeology. They interdigitate artifacts and documents to get a more complete picture of past behavior or use them separately to get two varieties of the same etic view. Historical archaeology will emerge as a significant field only when it is realized that two contexts, which in part are qualitatively different, may be combined to form a superior cultural reconstruction. This possibility has been discussed by a number of scholars,
Figure 2. Various Contexts within which Data are Available to the Historical Archaeologist.
although usually with an emphasis on the etic-etic potential, and I have recently (Schuyler and Mills 1976) explored a site by heuristically treating each context as a separate entity. Unfortunately the lack of extensive excavations and the specialized nature of the site, an 18th-20th century sawmill, limited the results.

Only one archaeologist, in the traditional sense of the word, has attempted the type of synthesis I am proposing. Deetz has combined archaeological data (gravestones, ceramics, and architecture) with an emic image provided by written sources to create a fascinating, if somewhat impressionistic, "cognitive historical model for American material culture" between 1620 and 1835 in New England (Deetz 1974).

What is an artifact? What is a document? Some consider the answers to these questions to be self-evident. I do not. When an understanding of the various available contexts and their interrelationships is achieved and when this achievement is placed into a comparative, cross-cultural perspective, then the field of historical archaeology will draw the attention of both historians and scientists.
FOOTNOTES

1. The boundaries between archaeological, documentary, oral historic, and ethnographic sources are not absolute. A document, for example, is also an artifact and may be treated as such with chemical, C-14, and other tests. Certain documents are also more closely associated with artifacts on an interpretative as well as a technical level. Photographs are a good example. Although most illustrations may be viewed as typical documents (i.e., strongly emic in origin and nature), the invention of the camera added a mechanical element which directly preserves human behavior. Yet it should not be forgotten that even a photograph has to be "taken" and this process involves an emic screening process (see Adams 1975; Schuyler and Mills 1976).

2. Sandy Ground did not have an 1820's origin, as Dollar states, although its ultimate roots may have that time depth in both the Delmarva and metropolitan New York areas. According to oral history, the migration started in the 1830's and 1840's; in fact, the known arrival dates for specific families are in the late 1840's. By 1852 the movement had been extensive enough for the establishment of the local Black church.

3. An examination of the 1820 and 1830 censuses, which bracket emancipation on Staten Island, show that a large population of Blacks, over a hundred, were already resident in Westfield before the formation of Sandy Ground. Many of these were slaves and after emancipation some set up their own homes or may have left the area, but a number seem to have continued as servants living with White families. With a few possible exceptions these Blacks had no connection with Sandy Ground. (See the map in Fogel and Engerman (1974: 45) for an impression of the place of Staten Island within what was in 1790 a heavy zone for slavery in the Northeast.)

4. There is strong evidence for migratory origin for Sandy Ground in the 1860 census. This movement involved not only the families from Snowhill but also, as oral history relates, a broader oystering zone on both sides of the Delmarva Peninsula. Snowhill is on the Atlantic side but oystering also extended into the Chesapeake Bay bordering on parts of Virginia as well as Maryland. Oral history refers to connections between Maryland and Virginia Blacks, such as with the Cooley family, and this relationship is also supported by the censuses. In 1860, 16 families (41 individuals) in Westfield (the separation of Sandy Ground and non-Sandy Ground families in the documents is still incomplete at this stage of research) had a Maryland origin and Virginia has the second highest number of nonmetropolitan origins (16 individuals).

Joseph Bishop, who is interestingly listed as a "laborer" although his presence at Sandy Ground was a product of his ties to the oyster trade, is married to a Virginia woman and three of his five children were born in Maryland. There are two other Maryland-Virginia marriages similar to that of the Bishops, and the Purnell family has a male member (brother?) from Virginia.
Another aspect of the formation of Sandy Ground is that even in the metropolitan area a migration is involved. Some Blacks moved from New York and New Jersey into the area probably as a result of their contacts through the oyster trade. Except for a few families, perhaps the Jackson and Henry, most Sandy Grounders were migrants and the initial core was from Maryland and adjacent areas.

5. In 1860 Sandy Ground existed as a community but is dispersed in the census probably for the reasons given in the text. For example, by order of visitation the enumerator is in Sandy Ground in the 1450's (1451, 1453, 1454) with the Bishop and Robbins families, which are probably on Woodrow, a side road. Again in the 1470's and 1480's (Stephens, Purnell, and Landin families), Sandy Ground families appear, but the main cluster is in the 1490's (1490, 1491, 1496, 1497, 1498, 1499, and 1500) with a number of Snowhill families prominent. These patterns are repeated in the 1870 census.

6. A few Maryland families (e.g., Stephens) and non-Maryland but later Sandy Ground residents (e.g., the Henry family) appear in the 1850 census but Sandy Ground is not discernible as a settlement. Oral history would place more Maryland families in the area by this period and this seeming conflict may be explained by a number of factors: (1) the Marylanders were not covered by the census (see the discussion on this possibility in the text; also quote under note 8), (2) the formation of the community is slightly later than oral history would imply, (3) the Marylanders were on Staten Island but outside of Westfield. Oral history, however, does not support this last proposition.

7. There is an association between Marylanders and the terms "oysterman" and "laborer" as against "farm laborer." In the 1860 census the Bishops (1451, 1454, 1498) are called laborers but they were tied into the oyster trade. Other families even more deeply involved in oystering (e.g., the Purnells) also fall under the "laborer" category. Another example of this problem is the Landin brothers. After Robert and Dawson Landin became owners of the Fannie Fern (1870's?), they employed ten men in oystering (Wilkins 1943e: 24). How would their "crew" be classified in a census? Although sons may well have been farmed out as general laborers or even agricultural workers, I suspect that the term "laborer" may also cover working on an oyster boat.

In the 1870 census the correlation between Marylanders, or other known Sandy Grounders, and oystering is more pronounced (especially the families listed under 724, 725, 727, 729, 730, 731, and 732).

8. Oral historical research is continuing at Sandy Ground but almost the entire generation that Wilkins interviewed, and which had at least indirect ties into the oystering period, is gone. However, there is another oral account of Sandy Ground that was collected in the period Wilkins was at work. In 1947 Joseph Mitchell followed Wilkins' lead into Sandy Ground to collect background materials for his book, The Bottom of the Harbor. His published account reads like a text; in fact, too much like a text as it is clearly a reconstruction of the actual interview. Recently part of Mitchell's (1973) original
notes have become available, and these give a firmer idea of his field methods. He briefly interviewed some of the people Wilkins worked with, but his main "text" is that of George H. Hunter who was 87 in 1947. He was an outsider having arrived in Sandy Ground in the 1880's. His statements (Mitchell 1959: 108-110) confirm those collected by Wilkins but tend to give greater emphasis to the early, hard years during the founding of Sandy Ground:

I wasn't born in Sandy Ground myself, 'he continued.' I came here when I was a boy. My mother and my stepfather brought me here. Two or three of the original men from Snow Hill were still around then, and I knew them. They were old, old men. They were as old as I am now. And the widows of several others were still around. Two of those old widows lived near us, and they used to come to see my mother and sit by the kitchen range and talk and talk and I used to like to listen to them. The main thing they talked about was the early days in Sandy Ground—how poor everybody had been, and how hard everybody had had to work, the men and the women. The men all worked by the day for the white oystermen in Prince's Bay. They went out in skiffs and anchored over the beds and stood up in the skiffs from sunup to sundown, raking oysters off the bottom with big old claw-toothed rakes that were made of iron and weighted fourteen pounds and had handles on them twenty-four feet long. The women all washed. They washed for white women in Prince's Bay and Rossville and Tottenville. And there wasn't a real house in the whole of Sandy Ground. Most of the families lived in one room shacks with lean-tos for the children. In the summer, they ate what they grew in their gardens. In the winter they ate oysters until they couldn't stand the sight of them.

When I came here, early in the eighteen-eighties, that had all changed. By that time, Sandy Ground was really quite a prosperous little place. Most of the men were still breaking their backs raking oysters by the day, but several of them had saved their money and worked up to where they owned and operated good-sized oyster sloops and didn't take orders from anybody. Old Mr. Dawson Landin was the first to own a sloop. He owned a forty-footer named the Pacific. He was the richest man in the settlement, and he took the lead in everything. Still and all, people liked him and looked up to him; most of us called him Uncle Daws. His brother, Robert Landin, owned a thirty-footer named the Independence, and Mr. Robert's son-in-law, Francis Henry, also owned a thirty-footer. His was named the Fanny Fern. And a few other owned sloops.... In those days, the oyster business used oak baskets by the thousands, and some of the Sandy Ground men had got to be good basket-makers.... Also, several of the men had become blacksmiths. They made oyster rakes and repaired them, and did all kinds of ironwork for the boats....
An interesting footnote in Hunter's recollection is that men used to appreciate the privacy of the cemetery lot for drinking. By the 1940's this lot was overgrown, implying it had been kept up previously, and the drinkers added broken bottles to the debris. Both these facts would mitigate against one of the explanations for the broken, artifact-grave pattern I originally reported (Schuyler 1974), although I do not recall broken bottle glass as being the main item around the graves. What is needed is a detailed study of the cemetery to see if different graves (sex, age, and status) are associated with particular items.

* * * *

Acknowledgements

I would like to thank Gail Schneider of the Staten Island Institute of Arts and Sciences for providing access to the original notes of Minna Wilkins and Joseph Mitchell. William Askins (CCNY) drew the map for Figure 1.
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Mr. Robert Schuyler’s rejoinder to my critique of his work at Sandy Ground successfully managed to present little that is new and much that is distorted. The considerable confusion he exhibited in distinguishing primary from secondary sources, and records as opposed to writings, is a dead giveaway that he has little grasp of the mechanisms of historical research. This, plus his use of the word "historiography" outside its accepted professional context, and his assigning to oral history credibility it does not have, further points out his amateurishness in the field of historical research.

About the census reports: I am aware of numerous limitations in the use of these documents, even some Schuyler failed to mention. However, they are not nearly so restrictive as he somewhat frantically contended. His overkill discussion of census limitations (during which he made some very unscaffolded assumptions) and the errors in use he attributed to me, stemmed from his desire to now discredit a documentary source he should have used in his initial research but did not. Limitations of data do not relieve the researcher from responsibility for analyzing such materials and placing their data in context within the research discussion. This Schuyler failed to do in his 1974 published work on Sandy Ground. It is encouraging, however, to note that finally, after three years of field surveys on the site and at least one published report, he has at last turned his attention to these very important primary sources.

For all its fuss and feathers, Schuyler’s rejoinder failed to respond to the central theme of my critique of his paper: that he inadequately searched for and evaluated the primary documentary evidence for his site. Accordingly, my critique analysis still stands: his work represents inferior scholarship. When he personally has researched the excellent documentary collections in archives almost within sight of the City College of the City University of New York, then his work at Sandy Ground will take on something more than an amateurish blend of hearsay, myths, superannuated secondary sources, pretentious academic jargon, and garbage.
FOOTNOTES

1. Clyde D. Dollar, "'The Devil Loose Amongst Us,' or, Some Observations on the Historical Accuracy of Certain Historical Archaeology," presented before the XVI Conference on Historic Site Archaeology held in Gainesville, Florida, November, 1975, and published in this volume.

2. Schuyler could correct these very basic deficiencies by reading Louis Gottschalk, Clyde Kluckhohn, and Robert Angell's The Use of Personal Documents in History, Anthropology, and Sociology; Bulletin 53 published by the Social Science Research Council (New York), 1945, especially pages 3 through 75.

3. At least as early as 1947, Walter W. Taylor, who was not an historian misused this word, and so it entered the vocabulary of nonhistorians effecting the guise of an historian (Taylor, as quoted in Gordon R. Willey and Jeremy A. Sabloff, A History of American Archaeology; W.H. Freeman & Co. (San Francisco), 1974, 138-139). Even a brief exposure to an historical research methods course would have brought Schuyler into contact with this word as defined in Webster's New International Dictionary, Second Edition, Unabridged; G. & C. Merriam Co. (Springfield, Mass.), 1937, 1183, "historiography," 2nd definition.

4. More than 120 years ago, a British historian, Sir George Cornwall Lewis, presented exhaustive evidence to show that, even in a literate society, traditions of past events or conditions are not transmitted orally from generation to generation with any appreciable degree of accuracy for more than a century, and in most instances for a considerably shorter period of time (Sir George Cornwall Lewis, Inquiry into the Credibility of the Early Roman History; J.W. Parker & Son (London), 1855, Vol. I, 98 ff.). Since that time, comparative historical methods have consistently demonstrated that Sir George's statements remain valid. And in February, 1972, in a paper titled "Oral History: Windfall or Deadfall?", presented before the Southern Anthropological Society meeting in Columbia, Missouri, I discussed the theoretical applications of oral history at some length, and arrived at five general limitations to the use of such orally derived information as historical data (this paper is included in a collection of essays presently being prepared for publication). Based on my reading of Schuyler's Sandy Ground report, he managed to exceed all five of these limitations in the use of his oral data.

5. For example, he asserted that the census enumerator (neither named nor dated) collected data from a certain street in Sandy Ground but not from another, for which statement Schuyler presented no evidence whatsoever. He further assumed that the streets over which he (Schuyler) "retraced" the census route existed at the time of the early censuses—again, an assumption made without evidence. Apparently, he is unaware that frequently the exact route of most census enumerators can be reconstructed through the use of land and tax records. These records are so complete for most areas of New York that an enterprising researcher should be able to virtually reconstruct the demography of most communities, including Schuyler's pet plot of very sandy ground.

7. Schuyler has a disturbing propensity for letting other people do his work for him. For example, he placed far too much reliance on the work of another person (Dr. Mina Wilkins, done in 1943) and apparently deluded himself into believing hers was the only worthwhile source on Sandy Ground history (as I have only a draft of Schuyler's rejoinder from which to work, it is not possible to reference his final pagination; however, this dependence he acknowledged in Part II, "Preliminary and Final Reports"). Furthermore, in another portion of his rejoinder, he admitted that "Local historians on Staten Island are examining the census schedules, as well as other archival sources, on Sandy Ground" (Part II, "Use and Misuse of Primary Sources"). All this raises the question of just how much of his 'research' he actually did.

8. Among these I would include the New York Historical Society, the New York Public Library (excellent collections!), and Columbia University Library (manuscript section)—as starters.
FINAL REPLY TO DOLLAR

Robert L. Schuyler

Clyde Dollar knows, and knew, that "Sandy Ground: Archaeological Sampling in a Black Community in Metropolitan New York" is a preliminary report. Clyde Dollar purposefully ignored this fact in order to establish an artificial basis for his criticisms. Clyde Dollar joined with me in proposing research on the pertinent censuses and other documents, but unfortunately he also went on to offer an immediate reading of these data. Clyde Dollar now knows that his conclusions, whether correct or incorrect, are meaningless because he ignored all problems inherent to the source he was using.

Rather than admitting the situation, Dollar has chosen to cover it up in his reply with a series of minor and misleading criticisms. I will answer him by referring to his footnote designations.

1) Points 2 and 3.

Clyde Dollar, myself, and probably W.W. Taylor all learned the "accepted professional" use of the word "historiography" in the same context; in my case an undergraduate course on "Historiography and Methods" at the University of Arizona. At the same time apparently Dollar was not taught, or did not learn from a reading of the works of historians, two facts. First if historiography is equated purely with the manner in which scholars have written history, its meaning is so limited as to remove the term from any serious discussion of historical methodology. Indeed, some historians do just that. A good example is seen in the reference mentioned by Dollar (Gottschalk, Kluckhohn, Angell 1945: 8-9) in which the word is introduced and subsequently ignored. The second fact is more significant; historians do not use the term consistently and many give it broader meaning, as I did, as a synonym for some or all aspects of historical research. If Dollar had read W.W. Taylor, rather than depending on Willey and Sabloff, he would have found that Taylor was not only aware of the "accepted professional" usage of historiography (in fact, he quotes the definition cited by Dollar), but that he also lists almost twenty examples of how historians and social scientists have differentially put it to use (see especially Footnote 45 in Taylor 1948: 207). On a more general level Dollar's criticism of my use of other basic "terms" is misdirected. Dollar and I, along with most historians, probably agree on the meaning of "primary source" and "secondary source" at least as these phrases apply to written records. The census tracts are primary as are oral accounts provided they represent an informant's own experiences. This was the case in almost all the life histories collected by Wilkins. The few exceptions are those involving the foundation of Sandy Ground when her subjects were relating stories told to them by their parents or grandparents. These statements are secondary as are any derived accounts including historical syntheses such as Wilkins' "Sandy Ground: a Tiny Racial Island."
Beyond the use of these basic terms we do not agree. Dollar referred to *The Use of Personal Documents in History, Anthropology and Sociology* (Gottschalk, Kluckhohn, Angell 1945), a work which unfortunately has been ignored by most anthropologists after its initial publication. This monograph is not significant, however, because of the section alluded to by Dollar. The first part is merely an attempt to discuss and to some degree define basic terms and concepts. The authors are no more successful than Willey and Phillips were in their endeavors to establish an agreed upon terminology for archaeologists. Dollar is wrong in believing, as he apparently does, that common English words and phrases can be given precise scholarly definitions. Terms such as "historical records," "historical writings," "human documents," or "personal documents" are not used in any consistent manner by either historians or social scientists. I am not sure what Dollar means by "historical records" and "historical writings" except that his contextual use implies that the former is primary and the latter a secondary source.

Enough on semantics and word games.

2) **Point 5.**

Dollar avoids the real issue concerning the census tracts; indeed, he could benefit by reading Gottschalk, Kluckhohn, and Angell (1945: 22-23; 66) on this subject. At no point did I attempt to "discredit" the census data or any other body of data. Such sources must be analyzed and their potential strengths and weaknesses recognized. Dollar did neither and that is the issue.

At no point did I state that a census enumerator "collected data from a certain street in Sandy Ground but not from another" (emphasis mine). My point was that we do not at this stage know how the enumerator covered the community. I also did not retrace the actual census route, which is unknown, but only suggested that the street pattern, which is well documented by maps as having considerable time depth in the area, and the boundaries of Sandy Ground make foot coverage of the settlement a complex affair. I also did not "assume" that a particular street, Woodrow, was covered. My statement was that Woodrow probably correlates with a small cluster of families in the 1860 Census. This conclusion is a specific hypothesis not an assumption and is based on the small number of families visited, Woodrow being a short street, and the fact that the Bishops, who later dwelled on Woodrow, are listed.

Of course I agree, and stated, that the route(s) of the enumerator(s) can probably be worked out in the main from other documentary sources. The fact that it needs to be established must be recognized first. Dollar displayed no such recognition until I discussed the problem.

3) **Point 4.**

Dollar's comments on oral history are inconsistent. Wilkins was dealing with a span of less than a century and most of her informants were relating events based on their own lives rather than those of past
generations. Except for the earliest founding of the community, their accounts are primary and serve as a vital and meaningful source of information.

4) Points 7 and 8.

Dollar's comments on 'doing one's own research' are, I suppose, meant as an insult. For those of use who do not consider scholarship a game but rather a cooperative venture, the more sharing of research and research tasks the better. The work of a historian, Minna Wilkins, helped to stimulate my own investigations at Sandy Ground, and my research in turn has helped to reactivate a long standing interest on the part of local historians in the oral and documentary history of the community. I am not insulted.

I would like to thank Dollar for listing the New York Historical Society, of which I am a member, and the New York Public Library, of which I am a supporting member. I was only surprised that he did not remind the reader of the existence of the Hayden Planetarium not to mention the Bronx Zoo. Although the major metropolitan archives are important, particularly the New York Historical Society and the American Geographical Society for cartographic information, the best resources on Sandy Ground are more local holdings and records on Staten Island.

A number of my colleagues urged me not to respond to Dollar's original comments because it would be a waste of time and because Dollar in the past has shown some strange inconsistencies in debates. I do not consider the exchange between Dollar and myself as unproductive. I am pleased to have such specific instances as my confusing use of the phrase "high standard of living" brought to my attention. The only real significant point that a superficial examination of the censuses delineates is an apparent conflict between the written and oral accounts on a specific question—the date for the establishment of Sandy Ground. It is possible that the founding of the community at its present location is slightly later than oral history claims. A difference of only a few months might explain why the 1850 Census failed to include many Sandy Ground families. On this one point the census data even in an unanalysed state are still clear and primary (although not without the possibility of omission) when compared to the oral accounts that are secondary (cross-generational). Nevertheless, alternate explanations are also possible and I discussed them in my original comments.

More importantly our exchange has highlighted several basic questions on the range and varieties of data available to scholars investigating historic societies. On a more personal level of colleague to colleague I cannot, unfortunately, consider the Dollar-Schuyler discussion as very fruitful. I am afraid I must conclude by recalling an old Southern adage (Brer Rabbit?).

"When you have a pissing contest with a skunk even when you win you lose."

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HISTORICAL ARCHAEOLOGY FORUM

The Methodological Frontier in Historical Archaeology

At the Gainesville meeting of the Conference on Historic Site Archaeology Kenneth Lewis chaired a symposium of several papers dealing with method in historical archaeology. This group of papers is presented here as a forum. Only abstracts of papers by Richard Carrillo and Stanley South are included since the full text is published elsewhere.

Stanley South, Chairman
The Conference on Historic Site Archaeology
The Methodological Frontier in Historical Archaeology

Kenneth E. Lewis

The following papers were presented in a symposium entitled "The Methodological Frontier in Historical Archaeology." The emphasis upon method expressed here reflects the orientation of these papers toward procedural aspects of research. A research methodology is especially significant with regard to archaeological studies because it forms the crucial link relating theoretical precepts to technical aspects of data recovery and observation. Archaeological methodology must be capable of building and testing hypotheses dealing with regularities of sociocultural organization and change as well as with the relationship between these behavioral phenomena and the form, content, and structure of the archaeological record. An adequate methodology will permit the collection and analysis of archaeological data in such a manner as to yield information concerning the sociocultural system that produced them.

Implicit in the following papers is the recognition that understanding human behavior is the goal of archaeological research. This belief is couched in the assumption that the data base with which the archaeologist deals is the direct result of, and therefore reflects patterning within, the past sociocultural system. Archaeological methodology must seek evidence of patterning in human behavior from its data. It is not primarily concerned with the particular results of behavior, but rather with those aspects of it which reflect general, adaptive responses inherent in the adjustments made by human populations to the conditions of their physical and social environments.

The emphasis on historical archaeology indicates that the papers deal with archaeological data of a specific type. Temporally it means that they are concerned with societies of the post-European contact period, with "late man" in North America. In terms of the nature of the data themselves, historical archaeology implies the presence of a source of information separate from the archaeological record, namely documentary evidence. Like the archaeological record, the documentary record is a byproduct of the systemic context of the society that produced it. While both forms of evidence reflect the same past reality, each is the result of separate transformational processes. Such processes produce data bases composed of elements that are not directly comparable unless the distinct nature of each process is recognized. Questions about the past must be addressed to the systemic context within which people, artifacts, and environmental variables once functioned rather than to the archaeological or documentary contexts in which their remains are differentially preserved.

Historical archaeology does not represent a different kind of archaeology or even a methodology distinct from that employed by archaeology in general. Rather, it signifies the presence of historical documentation as a separate source of data from which to derive analogy and with which to
test the conclusions of the archaeological research. Archaeological analysis must be able to stand by itself as a tool for interpreting the past whether our concern be prehistoric, protohistoric, or historic societies. Historical archaeology can play a significant role in this analysis through its ability to refine methodology. It is toward the attainment of this methodological goal that the papers in this symposium are presented.
The historical development of the European settlement of North America from the time of the earliest permanent settlement at least through the end of the nineteenth century has been characterized by historians as a process of constant expansion into new lands (Bartlett 1974), lands unoccupied, or those populated by groups possessing a lower level of sociocultural integration than that of the intrusive societies. Apart from the effects of contact, this expansion required a temporary adaptation by the intrusive cultures to the condition of remoteness they encountered on the frontier of settlement. The frontier is not seen here to represent a border, but rather a zone of transition in which a newly occupied territory is incorporated into the social, economic, and political system of the complex society. It constitutes the moving fringe of settlement where an attenuation of ties with the homeland requires a temporary breakdown of complex institutions which persists until the frontier becomes, in effect, an integral part of the parent state.

North American frontier development may be readily observed in that region where the earliest English settlement occurred, the Atlantic seaboard. In South Carolina, the frontier period encompassed the greater part of the eighteenth century and culminated with the transition of the former British colony into a segment of a newly emerged nation-state situated in the New World. This paper will center upon the use of archeological methodology to investigate aspects of frontier change through an analysis of the material remains left behind by a portion of the intrusive British society which settled that area.

The similar nature of adaptive changes made cross-culturally by intrusive societies in frontier situations implies the operation of patterned regularities of behavior. Such patterns have been noted by scholars in many disciplines studying the phenomenon of pioneer colonization cross-culturally (Turner 1893; Dawson 1934; Leyburn 1935; Webb 1952; Hallowell 1957; Allen 1959; Mikesell 1968; Wyman and Kroeber 1957; Kristof 1959; Casagrande, Thompson, and Young 1964; Thompson 1970, 1973; Wells 1973). Their work has formed the basis for the definition of an evolutionary process of sociocultural change upon which it has been possible to construct a "frontier model" (Lewis 1973, 1975). This model deals with change in terms of a systemic framework applicable in a general sense to all "settlement frontier" situations. Given a knowledge of the systemic organization of the intrusive society prior to colonization, it permits the investigator to predict changes within it and to observe these changes in the archeological record, or in any other form of data as long as he is aware of the formation processes by which this record is related to the past systemic context (see Schiffer 1975).

*Paper to be published in Research Strategies in Historical Archeology, edited by Stanley South.
Five characteristics associated with the process of frontier change form the distinguishing traits of the frontier model. First, prolonged contact must be continually maintained between the colonists and their parent society. Second, as a result of its relative isolation and the attenuation of trade and communications linkages with the homeland the intrusive culture exhibits a sudden loss of complexity. Third, the settlement pattern in the area of colonization becomes more geographically dispersed than that of the homeland unless temporarily impeded by conditions. The fourth characteristic is that the dispersed settlement pattern within the area of colonization is focused around central settlements, called "frontier towns." The frontier town serves as a nucleus of social, political, economic, and religious activities within a portion of the colony and as the terminus of the transportation network linking the area of colonization to the homeland through an entrepot. Because it serves as the primary link to the national culture, the frontier town forms as the nexus of the communications network within the colony. Finally, as the colony changes through time it also varies geographically. The pattern of temporal growth and change in a single community is replicated spatially with those settlements closest to the moving frontier always representing the earliest stages of frontier development. As the colony expands with the influx of new settlers areas of earliest settlement experience marked changes in population density and achieve a more complex level of internal integration. In effect, the older colonial areas begin to replicate the national culture of the homeland. As the frontier expands, settlements grow and take on new roles as they pass through a "colonization gradient" (Casagrande, et al. 1964: 311). With this change, the functions of the original frontier towns become decentralized and those that no longer occupy strategic positions in the trade and communications network decline and may be completely abandoned.

The frontier model is useful in the diachronic study of regions, such as the South Carolina Piedmont, which passed through a period of colonization. First, the model is broad enough to incorporate all the parts of a frontier system yet narrow enough to deal with each in regard to its own role. In terms of archeological investigation, the components of a frontier system may be visualized as sites, parts of sites, or groups of sites. Second, the frontier model does not confine research goals to the study of archeological patterning at a general or abstract theoretical level. Rather, it permits the consideration of a variety of questions simultaneously, making the model applicable to contract and salvage projects in which interpretive goals are sought in addition to questions of a broader nature.

To illustrate the utility of orienting archeological research around a model such as that described above, it was decided to organize the investigations of Camden, South Carolina, so as to explore the settlement's role as a component of a larger frontier system. Camden, an eighteenth century political and economic center in the South Carolina Piedmont, occupied a strategic position in the trade and communications network of the inland frontier of the colonial period (Fig. 1). Documentary sources suggested that it fulfilled the role of a frontier town in relation to pioneer settlement over much of the northern portion of the present State of South Carolina (Schulz 1972 and Ernst and Merrins 1973). Certainly the investigation of the site of Camden would be useful in demonstrating the ability of archeological methodology to recognize aspects of frontier change in this settlement and
Fig. 1. Camden in relation to the Major Overland Routes on the Frontier in Eighteenth Century South Carolina.

Fig. 2. The Heard Map of Camden as Surveyed in the early 1770's. The Grid Pattern and Central Square are retained in the present City Plan. (Source: South Carolina Statutes, 1798/no. 1702. South Carolina Archives, Columbia.)
Fig. 3. Spatial Distribution of Brick and Architectural Features — Structural Pattern.

Fig. 4. Greene Map of Camden and its Fortifications, 1781. Broad Street runs through the Center of the settlement in a North-South Direction and Meeting Street lies just inside the South Line of the Palisade Wall. (Source: Nathaniel Greene Papers, Papers of the Continental Congress, Library of Congress, microfilm.)
in providing new information concerning the nature of the frontier phenomenon in the Southeast.

In 1974 and 1975 archeological investigations were carried out at the site of the colonial settlement in conjunction with an interpretive study of the 1780 period town (Fig. 2). Because documentary sources revealed little information concerning the size and extent of the settlement that could be useful in interpretive exhibits or restorations, one task of the archeology was the discovery of structural remains as well as other patterns of past human activities. With this objective in mind it was possible to initiate excavations designed to examine the site in terms of the interpretive goals as well as that of eliciting aspects of the frontier model.

The immediate goals of the archeology included: 1) locating the Revolutionary War period palisade which delineated the limits of the contiguous 1780 settlement; 2) identifying structures within the settlement; and 3) determining dates for the town as well as for structures and other cultural features within it. With regard to the frontier model, objectives of archeological research centered around the identification of those sociocultural phenomena associated with the frontier town.

In order to approach these questions and thereby begin to analyze this portion of the frontier (or for that matter any other past phenomena), one must first determine the nature of the data base with which he is to deal. This may be accomplished in a "discovery phase" of archeological research intended to answer general interpretive questions about the site. The discovery phase is designed to elicit information concerning: 1) the general condition of the archeological remains at the site; 2) the form and spatial extent of past human occupations there; 3) the ethnic or cultural affiliation of the settlement; 4) its beginning and termination dates; and 5) the nature of intrasite variability and the distribution of behaviorally significant archeological materials.

The discovery phase of archeology at Camden has involved the use of a technique of investigation designed to gather a representative sample of the archeological materials distributed over the entire site. Such a technique requires, of course, that the limits of the site be defined prior to the sampling. This was accomplished at Camden by determining the location of the 1780 Revolutionary War palisade wall which surrounded the contiguous settlement. All noncontiguous structures were separately fortified.

Because statistical treatment of the archeological data is desirable, a technique for the random selection of sample units was chosen for this study. Random sampling offers the advantage of providing every unit defined within the sample area the same chance of being chosen (Dise 1952: 28) and eliminates the potential bias inherent in a sample based upon arbitrary measurements established by the investigator (Mueller 1974: 3). Redman and Watson (1970: 281-282) suggest that the stratified unaligned random sample provides the best method for examining artifact patterning because it prevents the clustering of sample units and assures that no areas are left unsampled. It accomplishes this by dividing the site into a series of large units based upon the coordinates of the site grid. Within each of these squares one unit of a smaller size is randomly chosen. The relative sizes of the units involved will determine the percentage of the site area sampled. Naturally, the
greater the size of the sample the more reliable will be the results; however, the difficulty of enlarging the magnitude of such a sample increases with the size of the site. For this reason, it becomes necessary to decrease the size of the individual sample units in order to maintain the degree of their dispersal over the site. This permits a maximum area to be investigated with a minimum of area sampled (Redman 1973: 63). Because the total accessible area of the Camden site was quite large, totalling over 487,500 square feet, the discovery phase of excavations here utilized a small sample comprising 1% of the entire site. The sampling was achieved by surveying the site in 50 x 50 foot squares and excavating one 5 x 5 foot unit randomly selected within each. In all, 186 sample squares were completed.

The excavations revealed that the entire site of Camden had been under cultivation, resulting in the vertical mixing of the historic component. It is assumed, however, that this has not greatly altered the horizontal distribution of the artifacts and the patterns of deposition should still be visible though discernible features may, in fact, be unrecognizable. The presence of only scattered posteighteenth century occupations suggests that the remains represent a nearly uncontaminated occupation which would include the Revolutionary War period settlement. The investigations revealed that the historic occupation covered most of the site with the greatest concentration occurring along a north-south strip paralleling the road bisecting the site. Posteighteenth century destruction of the site appears to have been confined to the construction of several public buildings in its northeast quadrant and a narrow strip removed during modern road construction.

In general, stratigraphy on the site consists of three layers: a grey loam lying at the surface, a pale brown sand, and sterile red sandy clay. The historic component is confined to the grey loam except in those places where the pale brown sand is exposed at the surface. In effect, the entire historic component utilized in the comparative analysis was recovered from a single zone throughout the site.

At present, the results of the sampling phase are far from complete but useful information is already emerging from the analysis of certain classes of artifacts. Ceramics, in particular, are significant in that they are capable of providing clues to the cultural affiliation of the site, the dates of its occupation, and, to some extent, its form and size. The Camden ceramic collection has yielded specimens representative of an eighteenth century British site, a great quantity of English wares together with smaller amounts of foreign products re-exported to its colonies through Britain's vast mercantile system. A mean ceramic date of 1789 was derived for the site as a whole utilizing South's (1972) method. It differs from the median historic date (1788) by one year. Documents indicate a temporal span from 1758 to 1819. Mean ceramic dates calculated for individual sample squares range from 1764 to 1819, closely approximating the limits of the historic time span. A frequency distribution of these dates forms a unimodal curve with a mode of 1791, suggesting that the greatest area was occupied at this time. General terminus post quem and terminus ante quem dates for the site as a unit have also been estimated utilizing the temporal use spans of the ceramic types represented. These are respectively 1775 and 1813 and fall within the historic range.
At this time it is possible to make a few inferences concerning the form and spatial extent of the site based on the portion of the data available. Utilizing the brick specimens recovered in the excavations at Camden, an attempt has been made to compare relative frequencies of weights per excavated unit in order to ascertain the patterned distribution of these artifacts on the site. It is assumed that there is a direct correlation between these artifacts and structure locations. The results of this comparison have been portrayed graphically utilizing a SYMAP (Synagraphic Computer Mapping) program. The maps illustrating the distribution of brick by weight indicates a great deal of variability in their distribution (Fig. 3). In the western half of the site 14 areas of concentration occur while three areas of concentration appear in that portion of the eastern half of the site. Several general characteristics are visible in the patterning. First, the highest concentrations are surrounded by concentric zones of progressively decreasing density, suggesting a thinning out of materials derived from definite central locations. Second, the concentrations appear to lie closest to Broad Street, the major road bisecting the settlement and the main street in the eighteenth century. Third, the locations of the concentrations found correspond to the general pattern of structures shown on a 1781 Revolutionary War military sketch map, the only known plan of eighteenth century structures in Camden (Fig. 4). Unfortunately documentation is very scanty for eighteenth century Camden (McCormick 1975) and it is not possible to identify structures on the basis of written sources alone. Therefore, it will be necessary to base the final recognition of structures and activity areas on an analysis of the archeological data. It will be necessary to complete the analysis of a number of other artifact classes to clarify the relationship between the brick density patterning and the distribution of activities at the site.

To approach the phenomena of activity patterning it is necessary to study those classes of artifacts which are functionally related to the activities considered. This may also involve the separation of certain classes, such as ceramics, into smaller categories possessing special temporal and/or functional significance. For example, the distinction of heavyware versus teaware discussed by Ferguson (1975: 49) in relation to status differentiation associated with the occurrence of the tea ceremony (Roth 1961) may be useful here in defining structure use and function. This distinction assumes temporal relevance when asked with regard to changing ceramic types of the eighteenth century.

In the search for patterning, artifacts may also be grouped by classes representing components of various sociocultural subsystems suspected to have operated at Camden as the result of the settlement's status as a frontier town. A subsystem might involve trade and communications, (certainly the paramount binding element on the frontier), subsistence, social organization, or any other segment of the frontier system in which Camden participated and within which it played a crucial role. Activities identified by artifact configurations may be arranged and associated chronologically through the use of datable items (e.g., ceramics) in order to demonstrate stability or change in functional patterns through time. In this manner it will be possible to view early Camden as a cultural entity both in a synchronic as well as a diachronic sense.
In summary, the recent archeological investigations at Camden are beginning to yield data which are intended to form the base upon which to launch a long-term program of interpretation at the site as well as to provide preliminary information designed to explore larger questions about Camden's role on the Carolina frontier. It is hoped that this work will emphasize not only the advantages gained through the use of a discovery sampling phase of archeological research, but also the compatibility of theoretical and interpretive goals in historical archeology.
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REGIONAL DATA IN HISTORIC ARCHEOLOGY:
EXAMPLES FROM ENVIRONMENTAL IMPACT SURVEYS

John H. House

Abstract

The emphasis on regional vs. single site data in prehistoric archeology is related to the assumption that culture is an adaptive behavioral system articulated with the natural and social environments. The testing of hypotheses and models about the nature of past societies in the historic period likewise requires regional data, not all of which may be available in the historical record. Archeological survey data—including those generated by Environmental Impact studies and other specific contract research—may be relevant to measuring cultural variables of historic period societies on a regional basis. Location, variability, and density of sites are some aspects of the historic archeological record which are especially amenable to measurement using survey data. Some examples of these aspects, derived from recent contract work in Arkansas and Missouri, are discussed.

Introduction

Within prehistoric archeology in North America there has been in recent years an increasing emphasis on regions rather than individual archeological sites as the major focus of research. This change in emphasis can be attributed to two independent developments. First, a change within social science of our view of patterning in human behavior and the operation of cultural processes; and, second, new environmental legislation which presents us, as archeologists, with responsibility for cultural resource management in the face of massive land modification projects in unprecedented numbers and on an unprecedented scale (cf. McGimsey, Davis and Griffin 1968; Gumerman 1973). These parallel developments at first gave rise to a conflict of values and a disparity between theory and practice in American archeology (King 1971). We have begun to realize, however, that the two sets of goals can be harmonized and that our greatly expanded responsibilities present us with hitherto nonexistent opportunities for exciting and relevant research as well as with new problems and headaches (Lipe 1974; Schiffer 1975; Goodyear 1975).

Current land modification projects—Corps of Engineer reservoirs, levee systems and stream channelization projects, Soil Conservation Service Watershed development plans, etc.—threaten archeological sites of the historic period on a region-wide scope as well as prehistoric sites. In this paper I shall briefly review the theoretics and methodology of the regional approach in prehistoric archeology and indicate ways in which this approach may be relevant to dealing with archeological remains from the historic period as well. To illustrate these points, I will present some data generated by recent Environmental Impact surveys in northeast Arkansas and southeast Missouri.
Archeological research strategy is closely related to an investigator's assumptions about the nature of human behavior. During the past decade or so, most prehistoric archeologists have come to see culture as an adaptive behavioral system—rather than as a set of learned, shared ideas governing human behavior—and have begun to attempt to operationalize this view in archeological research. We have come to emphasize the ways in which the behavioral repertoire of a society is participated in differentially by different social segments and the way in which various activities take place at different times and at different loci.

Turning to archeological applications, we accordingly expect variability in the archeological record to stem not only from changing ideas in time and space but from numerous other processes as well. We expect the archeological record produced by any given society at a single time in the past to exhibit considerable variability within and between sites; we attribute this variability to a complex set of behavioral processes including seasonality, performance of different tasks at different loci, division of labor by sex, and status differentiation within the society. Therefore, it is impossible to assume that data from a single site—or even a few sites in a region—can form a basis for typifying the cultural behavior of a past society during a given interval in time (cf. Binford 1964, 1965; Struever 1971).

The initiation of a program of archeological research guided by these assumptions would involve a survey of a region and an attempt to gather reliable data on the totality of archeological sites formed by the past society under consideration. Information on site location would be relevant to inference of the specific natural resources critical to the system and possibly other aspects such as communication and defense. Information on site variability would be relevant to inference of the total range of behavioral variability within the past society, and information on density of various classes of archeological phenomena would be a prerequisite for quantifying behavioral variables and testing hypotheses about past cultural processes.

The relevance of these three themes—location, variability, and density—in historical archeology will be developed below. It should be emphasized that probabilistic sampling in archeological survey is a prerequisite for obtaining truly reliable information on all three parameters of the archeological record (cf. Mueller 1974).

Is Survey Data Necessary In Historic Archeology?

The necessity of gathering survey data in prehistoric archeological research is obvious. It might be asked, however, to what extent does the existence of maps, land patents and other documentary records relied upon by social historians and cultural geographers make survey data on historic remains unnecessary and redundant for investigation of most problems. Until some comprehensive sets of corresponding documentary and archeological survey data have been collected and compared, this question will be impossible to answer with any certainty. I will only offer a few suggestions, based on my
own recent survey experience, as to ways in which archeological survey data might prove indispensable to investigation of historic problems.

In addition to the usual problems with reliance on documents (i.e., documents may be concerned with only specific things; they may be falsified, lost or destroyed, etc.; cf. Bloch 1953; Deetz 1971), several disparities between what is recorded in documents and what can be observed by inspection of the ground have been noted. First, historical records—census records, land patents, etc.—may not contain all of the information on human/land relationships of interest to a social scientist. Second, records from frontier situations may be especially incomplete. "Squatter" homesteads, for instance, are not recorded in land patents. Furthermore, as shall be demonstrated later on in this paper, some ephemeral settlements and other activity loci, while nonetheless economically important, may be very poorly documented, even within the relatively recent past.

The Management of Historic Archeological Resources

The sheer number and diversity of contract projects which we are becoming responsible for is threatening to overload the capacities of most archeological research institutions. In attempting to avert chaos or recourse to shoddy research—or both—some institutions are trying to streamline their contract programs and maintain a research orientation by formulating regional research designs on various topics into which specific contract projects, as they arise, can be integrated (King 1971; Goodyear 1975; Price et al. 1975). Environmental Impact Statements (the initial estimates of the extent and significance of the resources to be affected by a proposed project) are seen as the first stage of a de facto multistage research program involving both later "mitigation" stage work on the same project and research on future projects in the same locality. Carrying out these aims in regards to historic archeological resources will require, of course, considerable input of historic archeological expertise at all stages of contract research planning and execution.

This approach has barely begun to be applied to historic archeological problems thus far. Experience over the last year or so, however, strongly indicates that systematic gathering of historic site data during Environmental Impact surveys can yield kinds of region-wide data which have hitherto been unavailable in historic archeology.

Three Recent Surveys in the Central Mississippi Valley

The examples presented during the remainder of this paper are derived from three recent Environmental Impact surveys for land modification projects in northeast Arkansas and southeast Missouri. These projects are:

2. The Little Black River Watershed Project: a large-scale Soil Conservation Service flood control project spanning the Ozark Highlands/Mississippi Valley ecotone in southeast Missouri and northeast Arkansas (Price et al. 1975).

3. The Poinsett Watershed Project: a relatively small-scale Soil Conservation Service flood control project in northeast Arkansas on Crowley's Ridge, an upland remnant within the Mississippi Valley (House 1975).

It should be emphasized that only the survey of the Cache Basin involved any type of probabilistic sampling. The other two surveys were confined to specific zones in which direct project impacts are expected to occur. This nonprobabilistic sampling can be assumed to be biased and to provide less reliable estimates of regional parameters than would probabilistic sampling (cf. Mueller 1974). These biases, however, can be specified and can be taken into account and it is likely that the intensive on-the-ground investigation of a number of dispersed, areally-bounded impact zones in a region can provide a much more reliable basis for measurement of most variables than could hit-or-miss intuitive sampling.

The data generated by these three surveys are not adequate for testing any hypotheses about early historic occupation in the regions involved. They do, however, suggest patterning in historic site location, variability and density in various portions of the central Mississippi Valley area.

Site Location

One of the major research designs operationalized during the survey of the Little Black Watershed Project was an investigation of determinants of historic site location. In particular, the data gathered in the field and obtained from documentary sources were used to partially test some hypotheses about the location of nineteenth century homesteads in relation to such environmental variables as arable land, wild food resources for domestic animals, fresh water sources and access to communication routes (Price et al. 1975: 77-78).

In the Ozark Highlands portion of the Watershed, present evidence suggests that through the midnineteenth century, American homesteads tended to be located on high terraces or low hilltops overlooking rivers and major creeks. The settlement seems to be earliest in proximity to the Natchitoches Trace, a major communication route of the old Louisiana territory. In addition, there seems to be a strong association of early to midnineteenth century homesteads with permanent springs (Price et al. 1975: 146-160).

When the data on early historic site locations in the lowland portion of the Watershed are compared with data on thirteenth and fourteenth century A.D. Mississippi occupation, a particularly interesting pattern seems apparent. Sites of both occur on high, sandy terrace remnants, known locally as "sand ridges," rather than on the intervening lowlying flats (Figure 1) and the location of the early historic homesteads seems to correspond more with that of the Mississippi hamlet sites rather than with the sites of the larger villages (cf. Price 1974). This suggests that in both cultural systems--
FIGURE 1
SAND RIDGE SYSTEM OF THE LOWLAND PORTION OF THE LITTLE BLACK RIVER WATERSHED, MISSOURI AND ARKANSAS
the prehistoric Mississippi and the early nineteenth century frontier Anglo-American—the requirements of settlement location may have been similar. Both economies were based on maize agriculture and the requirements for arable land, a water source and elevation above seasonal flooding probably operated in both systems (Price et al. 1975; cf. Lewis 1974: 29-32).

**Site Variability**

Reliable data on variability among sites formed by a single past cultural system is a prerequisite for inference of the behavioral variability within the system and understanding of the articulation of that behavioral variability into a systemic whole. The survey of numerous proposed catchment basin sites and drainage channel routes throughout the Little Black Watershed brought many aspects of historic site variability in the watershed into much sharper focus than was possible previously.

The most common type of historic site located during the survey was, as might be expected, sites of houseplaces. One cabin probably dating to the 1850’s was still standing but many other were completely in ruins and only recognizable by the observation of scattered foundation stones and the subsequent use of a metal detector to locate buried metal artifacts.

Other types of sites were located, too. These include two probable barn sites such as the midnineteenth century example illustrated in Figure 2. We also relocated the site of the ephemeral logging town of King Bee, dating to the turn of the century. The site contained almost no standing structures. We were able to map the site only because an elderly local resident showed us the location and indicated the position of numerous structures he remembered from his childhood (Figure 3).

One quite important economic activity from the even more recent past is poorly documented—for obvious reasons. Sites of at least three moonshine stills, probably dating to the 1920’s and 30’s, were found within the basins surveyed. The site illustrated in Figure 4 represents a particularly large operation; numerous mash barrels are indicated by the piles of barrel hoops and two cookers are represented by the two hearths.

**Site Density**

Testing models and hypotheses of past cultural behavior will almost invariably require quantitative vs. presence-or-absence, or "trait," data. On a regional level, this may take the form of measurements of the density of various classes of archeological phenomena, though a number of other measures of locational structuring (cf. Haggett 1966) may also be relevant to the analysis of survey data. It is in measurement of this parameter that probabilistic sampling is particularly crucial. In the absence of probabilistic sampling, however, really marked differences in observed density may nonetheless indicate underlying patterning. Such marked differences in density are apparent from comparison of data on early to midnineteenth century occupation generated by the Cache River Archeological Project and the Poinset Watershed survey.
FIGURE 2
SKETCH MAP OF PROBABLY MID-NINETEENTH CENTURY BARN SITE (23RI-H42), LITTLE BLACK RIVER WATERSHED, MISSOURI
FIGURE 3

SKETCH MAP OF THE KING BEE LUMBER VILLAGE SITE (23RI-H3I) IN THE LITTLE BLACK RIVER WATERSHED, MISSOURI
FIGURE 4
SKETCH MAP OF A MOONSHINE STILL SITE (23BU-H13) IN THE LITTLE BLACK RIVER WATERSHED, MISSOURI
These two projects, as noted above, are in two highly contrasting environmental zones with the Mississippi Alluvial Valley in northeast Arkansas. The Cache Basin is a predominantly flat, lowlying, poorly-drained area which has very little land suitable for maize or cotton cultivation and which remained mostly wooded until the last 20 or 30 years. This type of environment is, in fact, typical of much of lowland northeast Arkansas. Historical sources (Goodspeed Brothers Publishing Co. 1889; Williams 1930) indicate that prior to the Civil War, nonaboriginal occupation of the region was especially concentrated on Crowley's Ridge, an extensive, hilly upland remnant which stands isolated in the heart of the Central Mississippi Valley.

Comparison of the archeological data gathered by the Cache Project in 1973-74 and the survey of the Poinsett Watershed Project in 1975, respectively, revealed differences in historic site density quite consistent with this suggested patterning. In the Cache survey, a total of more than 10 mi.² throughout the basin was intensively surveyed. In this area, only two sites produced recognizable evidence of early to midnineteenth century occupation. The Poinsett Watershed survey, on the other hand, covered a total of about 200 acres associated with twelve proposed floodwater retarding structures on Crowley's Ridge. During the latter survey, three early to midnineteenth century homestead sites were located within this limited area.

Conclusion

The surveys discussed above were carried out by persons whose primary research interests are in anthropology and North American prehistory. The research was a learning process and a challenge to all, both in that we had to strive to learn to recognize early historic artifacts and features in the field and to learn to use available documentary sources. We did, however, find our archeological survey skills and perspectives, developed in prehistoric research, to be quite useful and productive of insights into the cultural systems of the historic past as well.

I regard the inferences presented above as part of a cumulative process of inferring patterns and acquiring testable models and hypotheses relevant to historical archeology in the regions involved. Hopefully, in the future we will work with better formulated research questions and better definitions of the data classes--both archeological and documentary--relevant to their solution. I feel that these examples do indicate something of the potential of survey data in historic archeology. I would suggest that if our programs of contract research can be integrated into ongoing regional research designs concerning the archeology of the historic past, we can use these programs to make a meaningful contribution to the nomothetic study of human behavior.
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During 1974, archeological excavations were undertaken at the sites of King's Mountain National Military Park and Brattonsville. Both sites are located in the upper Piedmont area of South Carolina which was settled primarily by people of both British and German cultural traditions. The site of the Howser House, located at King's Mountain NMP was built and occupied by an individual whose sociocultural tradition was German. The Bratton House was built and occupied by an individual having a British sociocultural tradition.

The analysis of the recovered artifacts was aimed at defining differences within the archeological context having causal links to differing sociocultural traditions. The following hypotheses were proposed for testing.

1. Systematically controlled excavations at the Bratton and Howser Houses should reveal, through quantitative analysis, archeological variability reflecting specific behavioral activities.

2. General differences in refuse disposal patterns in contrasting sociocultural systems, i.e. British Colonial and German American, should be revealed in contrasting artifact relationships at the Bratton and Howser Houses.

3. Historical documentation and archeological evidence suggest that in the British Colonial system refuse will be consistently discarded adjacent to the structure (Bratton House), whereas in the German American system (Howser House) there would be little, if any, systematic discard of refuse adjacent to the structure. Therefore, a greater association among artifact classes was expected to occur at the Bratton House than at the Howser House.

The dispersion, density, and association of artifact classes within the archeological record were examined for the purpose of discovery of the kinds of refuse disposal patterns involved in producing that record. A contrast between such behavioral patterns was found at the Bratton House and the Howser House. In addition, contrasts were found in positive statistical associations of artifact classes at the Bratton House and lack of associations at the Howser House. These data are certainly suggestive of sociocultural variability resulting from the different cultural traditions represented at these houses. Considerable studies, explicitly defining archeological variability, are needed in order to more firmly establish causal links.

*Because this paper is being published in Research Strategies in Historical Archeology, edited by Stanley South, New York: Academic Press (1977), only an abstract is presented here.
The archeologist is concerned with understanding past lifeways, culture history, and culture process by examining the material remains of culture reflecting these processes. The conceptual framework for this understanding is that of evolutionary theory. The method whereby these phenomena of the past are examined pivots on the recognition of pattern in the archeological record. Once pattern is abstracted and synthesized with other patterns, these demonstrated regularities are often expressed as empirical laws. The explanation of why these lawlike regularities exist is the goal of archeology. The explanation is addressed to the causal processes in the past cultural system in the form of hypotheses to be tested with new data through research designs specifically constructed to fit the questions being asked. The understanding of culture process and how it works comes through this basic procedure of archeological science. This understanding provides a conceptual environment within which new theory is invented to explain the phenomena the archeologist has observed.

With this procedure as basic to archeological science, it follows that the use of ethnographic data and historical documentation by the archeologist does not result in a different kind of archeology merely because a wider data base is available. This viewpoint is not generally shared by archeologists, however. Many colleagues assume historical archeology is a particularistic involvement with details of history, cataloging, and classification. This is not enough! The archeologist has a responsibility to go further than this and to address the culture process by scientific procedures.

There is historical reason for the more limited approach in that historical archeology has so frequently been done by archeologists with a particularistic point of view. This historical development accompanied by the publication of a number of books emphasizing the particularistic approach has resulted in historical archeology having a particularistic image. Historical archeologists must come to a realization that we can, and in fact must, do more than this in an area of archeological research that offers great promise for the development of archeological science.

Unless there is an effort made to go beyond the particularistic approach to historical archeology there can be no concern for pattern recognition. Pattern recognition, however, is a basic step in any analysis. Judging from many recently published reports by historical archeologists as well as a number of doctoral dissertations, containing no attempt at pattern recognition, it is apparent to me that the training these people received did not prepare them to carry out scientific archeology. Pattern recognition is a basic methodological approach in archeology. Without

* Since this paper is being published in Method and Theory in Historical Archaeology, by Stanley South, New York: Academic Press, Inc. (1977), only an abstract is presented here.
quantification, however, there can be no explicit pattern recognition. Without pattern recognition there can be no archeological science. Without archeological science our ideas about man's past cannot be predictably tested, and this is the basic goal of archeology. Without predictability man's ideas about the past amount to antiquarianism. Therefore, pattern recognition and quantification are basic to the archeological process. These are, however, merely the first steps in that process, but archeologists must take them before they can ever hope to contribute, through their work, to a science of archeology.

The concepts we are concerned with here can be envisioned in terms of "Archeology and the Art of Weaving." The basic warp of the fabric is the process of evolution, interwoven with the weft of unique events trailed from the shuttle of history. The variable strands of the weft produce a pattern interlocked with the regularity of the warp. The resulting design, "Carolina Pride," has determined the relationship each strand of yarn has to every other in the woof and warp of the fabric. This design can be equated with culture process. The fabric is that creation of man known as culture.

The particularist is involved primarily with the description of the weft strands as they cross the warp, tracing each step of the way, over and under, with every row of yarn representing a single archeological site. At the end of the row he writes his report and he is done.

The archeological scientist searches for pattern not only within each row of weft yarn as it goes over two, under three, over two, under three, but he also notices that adjoining rows of weft (sites) have somewhat similar, yet varying patterns. With pattern recognized for a number of sites (weft rows), he makes a prediction as to what pattern the next row (site) will have. If his postulates are empirically verified, he then hypothesizes as to the design (culture process) that was the explanatory determinant for the pattern he has delineated from the empirical data. As his hypotheses are tested and found to be valid, he eventually is able to say "the explanatory phenomenon is a design I will call 'Carolina Pride.'" Having thus addressed himself to culture process, he is well on the way to understanding something about the fabric of culture.

It is hoped that the discussion here has made clear that historical archeology is archeology carried out on sites of the historic period. This fact does not make it a different kind of archeology than any other. David Clarke (1968:13) has emphasized that "archaeology is archaeology is archaeology," and Leslie White (1938) has stressed that "Science is Sciening." In the decades to come, as more archeologists come under the continuing influence of the "great pulsation" toward archeological science, there may come a time when it can be said that archeology is sciencing, and no one will seriously challenge the proposition. At that time archeology can indeed be spelled with a capitol "S" for science, as Flannery has suggested (1973:47).
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The archaeological interpretation of changing settlement patterns often rests implicitly or explicitly upon a particular view of social and cultural dynamics. As Kwang-chih Chang has shown in Rethinking Archaeology (1967: 30-35), some leading archaeologists and anthropologists argue that the social structure of human groups is relatively stable during periods in which they maintain an equilibrium with their natural and cultural environs. When the balance is upset, social structure is thought to change in response to the new situation and eventually regains the relative stability of another so-called "stationary state" (Chang 1967: 31). Social structure is supposed to represent uniformities underlying the innumerable choices, decisions, and interactions which make up social events. From the ethnographic point of view, a stationary state persists as long as a particular structural representation of society remains accurate, as long as social events exhibit the same sort of abstract regularities.

Archaeologists seek abstract regularities among the material remains of society rather than its social events. From the archaeological point of view, then, a stationary state persists as long as deposition occurs without upsetting the overall alignment of cultural elements (Chang 1967: 33). Alternating periods of stability and change evident from archaeological sites in the region occupied by a society presumably reflect the series of stationary states through which it passes. But deposition at any particular site may be quick to register fairly minor local events and slow to record the cumulative changes in a region more important to social organization. Chang thus expects small discrepancies to occur between archaeological and sociological representations of settlement succession, but also, that both should be recognizable as dealing with the same phenomena in cases of historically significant change (1967: 35-36).

The Daniels Village experiment was designed to critically examine and perhaps clarify the relation between sociological and archaeological models of changing settlement pattern, particularly the displacement of one community by a subsequent one. The research strategy was fairly straightforward. The archaeological features of a 100-acre locale were extensively sampled in order to model settlement replacement according to the stationary state framework. Surviving documents and oral history were intensively reviewed with respect to social organization and community development over the last 250 years, for precisely the same area. Separate models of settlement succession were then synthesized from the archaeological and documentary information. The two resulting constructs were compared as if they were independent reconstructions of the same empirical events, but no attempt had been made to develop either body of data in ignorance of the other. Uncertainty about the influence that one sort of data had upon the interpretation of the other is a definite weakness of this experiment. Even so, results offer some interesting implications about evolutionary mechanisms.
The Daniels Village site lies along a short stretch of the Five Mile River in northeastern Connecticut, about four miles south of Putnam. Those archaeological features used to identify alternating periods of stability and change were located in a ten-acre village and four smaller outlying sites. The outlyers consist of a farmstead, a schoolhouse, and two dwelling sites. Roughly comparable features concentrated in the village include a standing stone dwelling, a hydraulic system, and the ruins of a store, grist mill, barnyard, factory complex, and seven former dwelling sites together with a fairly substantial midden deposit (see Figure 1). The stationary state framework proved a convenient way to summarize temporal discontinuities and represent settlement succession at Daniels Village. Six periods of transition lasting ten or fifteen years are separated by five stationary state intervals from twenty to thirty-five years each (see Figure 2a).

The first period of transition archaeologically identified occurred with the initial occupation of the two outlying dwelling sites. Since these are the only defining events discovered, the interval has been fixed between the first around 1765 and the second about 1780. A considerable number of events make up the next period of transition between 1815 and 1830 when most of the village buildings and farmstead appear to have been erected and the midden lens containing apparent builders refuse deposited. Erection of the schoolhouse and abandonment of the outlying dwelling sites probably occurred during the interval. The massive hydraulic system and factory complex which may have obscured earlier remains near the river are probably attributable to this period but still need to have datable associations established. The third interval of transition occurred between 1850 and 1860 when the textile factory was totally destroyed by fire and the outlying schoolhouse was abandoned. Replacement of open hearths by stoves and Victorian style alterations in the stone house probably relate to this period but lack precise dating criteria.

The village began to contract during the twentieth century. Between 1890 and 1905 the hydraulic system was apparently redeveloped to operate a small electric generator at the former factory site, but the grist mill, store, and three dwelling sites adjacent to it were discontinued. Privies located beside three of the surviving dwellings were abandoned and a different style erected further behind them. The fifth period of transition dates between 1930 and 1940 with abandonment of the electric generator, barnyard, and the four remaining frame dwellings. Large quantities of refuse began to accumulate in two open cellars near the stone house, two others were intentionally pushed in, and the midden began to accumulate great amounts of stove ash containing nails and hardware from former buildings. The latest period of transition began after 1960 with the renovation of the stone house and continued into the present with recent remodeling of the farmstead dwelling and construction of a small studio on the site of the former electric generator.

In prehistoric studies covering much longer time spans, periods of change are often reduced to points along the temporal continuum. In Chang's treatment of the Feng-pi-t'ou site on Formosa, for example, the 2000-year Lungshanoid occupation was represented as four stationary states of 500 years each (1967: 26-29, 1969: 19-133). But this simplification would violate the Daniels Village data given the uncertainty with which some events must be dated and the variance apparent among others representing the same
transition. The periods identified as stationary states at Daniels Village are also considerably shorter than those usually described in prehistoric studies. William Longacre considers a comparably long occupation of the prehistoric Carter Ranch pueblo in Arizona to be essentially a single stationary state (1970: 26), although his analysis suffers because of his refusal to subdivide the occupation (Bartovics 1974: 203). There is little doubt, however, that the composition of each stationary state at Daniels Village is noticeably different from the next. They are comparable to prehistoric settlement units in structure, if not in scale.

However useful the stationary state concept may be for summarizing the archaeological evidence at Daniels Village, the historical information defies a similar treatment. Data gathered from recorded deeds, vital records, published genealogies, Federal census schedules, business records, and oral tradition do suggest at least five phases of site development, but described as fairly continuous cycles, not alternating periods of stability and change (see Figure 2a). Each cycle begins with the acquisition of existing premises, construction of new facilities, and experimentation in putting the facilities to use. A more or less productive interval follows in which certain adjustments or improvements occur. Finally, a variously long period of decreasing utility renders the site vulnerable to redevelopment. Transfers of title which initiate a subsequent cycle of development are usually between parties more distantly related by ties of business, kinship, and residence than those which occur within a cycle. This strongly suggests that a particular network of people with access to a certain set of resources is responsible for each adaptive stage in the development of Daniels Village.

The first cycle began with the original division of common lands making up the site between two of the earliest residents of Killingly in 1722. A single grist mill was erected on one parcel by 1725, followed by 11 years of unsettled ownership. William Robinson purchased the premises in 1736 and controlled the grist mill for the next 16 years. He moved the mill a few hundred yards down stream to the present dam site after acquiring the other parcel in 1743. By 1752 financial problems forced him to sell off the mill, a dwelling, and part of the surrounding land, losing the balance of the second parcel to his creditor within the next three years. This sale of the mill to a recent arrival from Rehobeth, Massachusetts, began a second cycle. By 1759 the newcomer had divided his sole right to the mill privilege making it possible for others to add a saw mill and fulling mill. His brother-in-law, Jared Talbut, purchased the grist mill parcel in 1761 and increasingly consolidated the surrounding land and facilities under his ownership after 1776. He sold a dwelling and about 25 acres to his eldest son in 1794 and a one-acre mill privilege to an outsider in 1804, mortgaging the rest of his estate later that year. Both he and his son sold all interest in the premises to a pair of local investors in 1808, thus ending the second developmental cycle.

These investors held most of the estate until 1813 when the Killingly Manufacturing Company consolidated the entire privilege, constructed a massive hydraulic system, and erected a cotton textile factory. Several new dwellings were built between 1820 and 1824 after successfully reorganizing the manufacturing system and introducing power looms. The owners incorporated the firm in 1829 according to newly enacted Federal statutes and continued to enjoy a fairly productive phase until the 1837 sale of
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their assets to a pair of related local manufacturers. They fell hopelessly into debt over the next eight years. The encumbered factory hamlet was sold to a different group of nonresident proprietors in 1845 beginning a fourth cycle of development. The new owners improved the hydraulic system and successfully manufactured print cloth in the factory until 1855, but had failed to clear the old debts. In 1856 the earliest notes against the factory were acquired and the debts foreclosed by Dan A. Daniels, a nephew of one owner who had run the operation but had no official share in ownership. He refinanced the factory and continued to make similar goods until the factory burned in about 1861. His creditors withdrew their capital from the insurance settlement and the factory was never rebuilt, but Daniels was able to successfully concentrate his efforts on the farm, grist mill, saw mill, and rental of his tenements to workers from nearby factories. He sold the entire estate to another local entrepreneur in 1880. The latter was apparently less successful ending the cycle within the next ten years.

The last complete cycle of development at Daniels Village began with the division of the site in 1888 when the hydraulic system and about half of the former village buildings were sold. An electric generator was erected and used to supply additional power to a factory immediately downstream until about 1925. Afterward, the system became simply a reservoir and flood control device. The dwellings on this parcel were briefly occupied before they came down between 1900 and 1910. Dwellings on the opposite parcel were acquired by the Kelly family in 1894. They were inhabited by low-income laborers paying small rents. Firewood was cut on the overgrown farmland for sale in neighboring hamlets. After 1938 only the stone house was occupied by a tenant family until Helen Kelly sold the premises to its present owner in 1962. He has initiated a new cycle of development by consolidating most of the former village parcels and a neighboring farmstead, renovating the stone house immediately and encouraging clients to rebuild the farmstead dwelling, erect a studio in the factory ruin, and exploit the historical potential of the site through academic research from about 1970 to the present.

The idea of using developmental cycles to represent social phenomena is not particularly new to history or anthropology. The notion has been invoked by many of the same anthropologists who subscribe to the stationary state concept to explain how impermanent units of social organization change in composition and structure through time but ultimately replace themselves with very similar entities (e.g., Fortes 1949: 55-56). A well known cross-cultural application of the concept appears in a volume entitled The Developmental Cycle in Domestic Groups edited by Jack Goody (1958). The cycles of settlement growth at Daniels Village represent evolutionary rather than repetitive processes, however. It is, of course, entirely hypothetical whether an ethnographer would represent irreversible changes of comparable magnitude in a community according to the stationary state or developmental cycle framework, but it may well be fruitful to consider whether an archaeologist might be able to model settlement succession according to the latter.

Comparing the two abstract representations of settlement succession at Daniels Village reveals that there are indeed small discrepancies as Chang had anticipated. Stationary state intervals generally lag slightly
behind the equivalent developmental cycle. Two phases of the last developmental cycle were represented as different stationary states while the earliest cycle is still not documented archaeologically. These differences are minor, however, and essential features of both models are readily attributable to the same case of development. The original proposal outlining this research challenged an assumption that significant change in social structure would be systematically recognizable among deposits relating to a specific archaeological settlement (Bartovics 1971). Allowing those adjustments necessary to accommodate documentary information, the challenge has empirically failed.

On the other hand, unforeseen failure of the stationary state to accommodate information derived from documents is a significant result in itself. It implies that functionalist assumptions regarding the mechanism of social change are not adequate to describe the shape of settlement succession and might profitably be replaced by something akin to the interactionist perspective taken by Fredrik Barth's so-called "generative" model of social organization (1966). Certain aspects of archaeological data from Daniels Village lend themselves to developmental interpretation, but information collected to date is far too incomplete to detail a mechanism. The stationary state interpretation provides a reasonable first approximation of site development using a minimum of excavated remains. Understanding those changes, however, will require thorough examination of the transition periods rather than simple comparison of relatively stable periods with one another. Excavation of well documented historic sites should be able to contribute a great deal to the emerging body of theory and method required, not by emulating prehistoric archaeology, but by experimentally challenging it.
[Figure 1] The Daniels Village Sites
Figure 2

a. Above: Stationary state model of settlement succession at Daniels Village.
b. Below: Developmental cycle model of settlement succession at Daniels Village.
Picture Postcard View of Daniels Village, Northerly

Anonymous
ca. 1880?
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Neighings, Brayings, and Quack, Quack, About *A Man Called Horse*:
The Dollar-Thurman Debate

In Volume 7 of *The Conference on Historic Site Archaeology Papers* the movie *A Man Called Horse* was discussed by James Howard and Melburn Thurman. A reply to this debate was made by Clyde D. Dollar, historical consultant for the movie at the Gainesville meeting of the Conference on Historic Site Archaeology. At that meeting Melburn Thurman asked that he be allowed to reply to the Dollar paper. The resulting debate is presented here, beginning with Dollar's comment on the debate between James Howard and Melburn Thurman.

Stanley South, Chairman
The Conference on Historic Site Archaeology
BRAYINGS ABOUT HORSE:
THE THURMAN-HOWARD DEBATE ON FILMED ETHNOHISTORY

Clyde D. Dollar

Not the least of the qualities of the Conference on Historic Site Archaeology is that one can always find within the pages of its journal a lively debate on one subject or another. Some quite famous verbal battles have raged between the covers of the conference publication, and the recent Melvin [sic]* D. Thurman versus James H. Howard debate on Clyde Dollar, the film, A MAN CALLED HORSE, and the supposed resurgence of antiquarianism in ethnohistory and archaeology joins a list of prominent predecessors.

Not until November, 1974, with the publication of Volume VII of the CONFERENCE papers, did I learn of the debate, and it was humbling to discover that two such great minds had busied themselves with certain of my handiwork. Furthermore, to witness the strident thrusts and parries issuing from the typewriters of these two verbal combatants proved more than a little awe-inspiring.

However, despite the semantical pyrotechniques and mental gymnastics displayed by the debate's two participants, certain minor errors and small gaps in their comprehension of the film are visible and need to be corrected. Stanley South, editor of the Conference Papers, generously offered the opportunity to fill these gaps, and I accepted this challenge.2

Following a somewhat thin synopsis of the film's scenario, Thurman as initial protagonist, began a blow by blow attack on the film's authenticity. About this, he stated that "there are a number of lapses in authenticity in...customs and in portrayal of material culture. For example,

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1Thurman opened the debate with "The Resurgence of Antiquarianism in Ethnohistory and Archeology: Clyde Dollar's 'Letter From Mexico'," to which Howard responded with "Comments on "A Man Called Horse," Etc." Then Thurman: "Comments on James Howard's Anthropology, and So On;" then Howard: "Rejoinder to Thurman;" then Thurman: "Reply to Howard's Rejoinder;" then Howard: "Howard's Final Comment;" and finally Thurman: "Problems in General Anthropology and Plains Ethnohistory: Thurman's Final Reply." All of these are in the Conference on Historic Site Archaeology Papers, 1972, Volume VII, edited by Stanley South, 202-229. These will be collectively cited as CHSAP, VII.

2I have requested Stanley South, editor of the Conference on Historic Site Archaeology Papers, to vary his fine publication's format somewhat and reproduce this paper with the footnotes at the bottom of the page where they will be available for the convenience of the reader.

*Editor.
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."3 To this point, Howard, in his first reply, agreed, and further stated that the "Tetons did not secure their first Navaho blankets until 1858-59."4

Perhaps I can clear up this problem by asking a simple question of the two debators: Gentlemen, WHAT Navaho blanket? No such article appeared in the film, and the only item having even a remote possibility of this interpretation was an early nineteenth century English coverlet,5 clearly shown to belong to the Englishman, and just as clearly shown to have been appropriated from him by his Sioux captors.6 One wonders about the quality of criticism coming from critics who cannot tell the difference between an English coverlet and a Navaho blanket!

As an aside necessary to set the record straight, it should be noted that Howard's date of introduction of Navaho blankets into the High Plains, specifically among the Sioux, is about fifty years in error. Such blankets, as Spanish trade items, appeared on the High Plains, even as far north as central North Dakota, as early as 1806, and again in 1807, 1820, and 1833.7

3 CHSAP, VII, 204.
4 Ibid., 211.
5 The item in question was in fact an English coverlet, or 'throw blanket,' dating from the 1830's. Ms. Charlsie Bryant, of Cinema Center Films, the production company, generously loaned the blanket, her personal property descended from the estate of her English great-great-grandmother, for use in the film.
6 The 'story' of this blanket, along with that of the red bathrobe and several other items introduced to the viewer at the beginning of the film, unfolded as the scenario progressed.
7 For the Sioux and Cheyenne obtaining these blankets in central North Dakota in 1806, see Elliot Coues (ed.), Manuscript Journals of Alexander Henry (Minneapolis, 1965), I, 377-378 and 383-384. For the other dates, see Elliot Coues (ed.), Expeditions of Zebulon Montgomery Pike (Minneapolis, 1965), II, 535-536; Edwin James Account of an Expedition from Pittsburgh to the Rocky Mountains, in Reuben G. Thwaites (ed.), Early Western Travels (New York, 1966), XVI, 205, and XVII, 156 (hereinafter: EWT); Maximilian, Prince of Wied, Travels in the Interior of North America, in EWT, XXII, 367, and XXIII, 96. It should be pointed out that the winter counts on which Howard obviously based his information do NOT record the 1858-59 obtaining of Spanish/Navaho blankets as the FIRST event of that nature; see the winter count of Cloud-Shield in Garrick Mallery, "Pictographs of the North American Indians," in Fourth Annual Report...Bureau of Ethnology, 1882-83 (Washington, D.C., 1886), 143 and Plate XLVII, and the winter count of Battiste Good in Garrick Mallery, "Picture-writing of the American Indians," in Tenth Annual Report...Bureau of Ethnology, 1888-89 (Washington, D.C., 1893), 325.
A description of these 'Spanish' blankets, as seen in the Southwest between 1826 and the mid-1830's, confirms their identification as Navaho in origin.8

Thurman followed the Navaho blanket faux pas by pointing out that horse corrals probably were of European origin and therefore one should not have been shown in the film.9 In support of this statement, he called on John C. Ewers' work, The Horse in Blackfoot Indian Culture.10 Howard agreed with the noting of this alleged error, and went further by issuing the utterance that "horses were herded near the village by boys, not corraled."11

Unfortunately, Thurman neither read Ewers' fine work carefully nor thought through the problem, and Howard agreed too quickly. Even European origins for these features would not preclude their use on the High Plains in the early nineteenth century. As a matter of fact, Lewis and Clark noted a corral used by the Assiniboine in North Dakota in 1805.12 Later, while visiting a large village of several thousand members of five different High Plains tribes gathered in eastern Colorado, Jacob Fowler in 1821 mentioned the many horse corrals associated with that camp.13 In 1833, Maximilian, Prince of Wied, while at Fort McKenzie, Montana, described the horse corrals built by the Blackfoot, and revealed that the use of such features was because "the Indians are so addicted to horse stealing that they do not trust each other."14 And in 1837, Count Francesco Arese found several horse corrals in an abandoned Omaha village along the Missouri River.15


9CHSAP, VII, 205-206.


11CHSAP, VII, 212.

12Clark interpreted this as an 'antelope pen,' a highly improbable designation if the description is read carefully; see Reuben G. Thwaites (ed.), Original Journals of the Lewis and Clark Expedition (New York, 1959), I, 313.

13Elliot Coues (ed.), Journal of Jacob Fowler, (Minneapolis 1965), 60.

14Maximilian, Travels, in EWT, XXIII, 123.

15Count Francesco Arese, A Trip to the Prairies (New York, 1943), 78.
Whether these features were a borrowed trait from the Europeans begs the point; their use by numerous High Plains tribes during the period shown in the film is documented. As these corrals were associated with both temporary camps of nomadic tribes as well as permanent villages of sedentary Indians, that a Sioux tribe in a semi-sedentary village situation could not have constructed horse corrals is ridiculous!

Lest this discussion about corrals create a distorted picture of their use in the film, let it be noted that one, and only one, of these features appeared as part of the village complex. Evidently, in their concentration on this supposed error, both Thurman and Howard missed seeing the large number of horses hobbled, staked, and otherwise secured in herds near the village and around the tipis of their owners.

Howard, in his first reply, added a list of additional 'errors' he noticed. Among these were:

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.16

REPLY: Indeed this was one of the errors in the film, but the only one noted by the two debators. Harris' headband came about as the result of a production difficulty, and the wearing of that item by him prompted a vigorous protest from me, unfortunately to no avail. The scene in question required him to appear with long hair, denoting a passage of time from the previous scene. On donning the wig for shooting the scene, however, difficulties immediately became apparent. Here stood this muscular figure, dressed only in his breechcloth, to be admired by millions for his striking masculinity, and on his head sat a wig of shoulder-length flowing golden blond hair parted in the middle! The suggestion of a male Veronica Lake proved too strong, and Harris immediately protested loudly and with considerable Irishness. To ward off this storm, a production assistant quickly produced a strip of leather and tied down the offending hair. At this point I protested as 'headband-less Indians' had been already established as the standard for the film. Unfortunately, the director, Elliot Silverstein, had to choose between his Leading Man and his historical advisor, and the pragmatics of the moment overruled the best laid plans of mice and men. It will be noted, however, that none of the Indians depicted in the film wore headbands—only the white man.

There is irony here, as my researches into this problem led me to the conclusion that the headband was introduced into the High Plains Indian cultures around 1916 as a direct result of the motion picture industry.

16 CHSAP, VII, 212.
Previous to this date, a number of Sioux people had worked in the Los Angeles area as extras for several western films produced by Thomas Ince. Because their hair had been cut short to conform to reservation acculturation policies of that time, Ince gave them wigs to wear so they would look 'more Indian' in his films. Long-haired wigs presented few problems for most filmed scenes, but riding a galloping horse — as in the classical chase scenes — created difficulties surmounted only by tying on the wigs with headbands. Within but a few months of the return of these Sioux people to their reservation, headbands made their first appearance in the photographs of the High Plains Indians.

HOWARD: 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.

REPLY: Harris, as John Morgan, the Englishman, was after all a White man, and he made his own clothing — as the film clearly showed. That he would resort to the 'thong' method, especially as the art of sewing was not included in the education of an upper class English gentleman is, I think, a reasonable assumption. By the way, all clothing worn by Indians in the film appeared as sinew-sewn.

HOWARD: The Indian dancing shown in the film was unlike any Indian dancing I have ever seen. It seemed to be aimless, mad, capering.

REPLY: The Indian dances in the film were prepared and staged by Lloyd One Star, a Rosebud Sioux knowledgeable of his people's genuine older culture. Perhaps Howard has danced too many grass dances to be able to recognize an older style.

HOWARD: The flute shown was not a Teton style flute.

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17 This man's impact on both the motion picture industry and America's concept of its historical West has been considerable; see Williams K. Everson, Pictorial History of the Western Film (New York, 1969), 23 ff.; Benjamin B. Hampton, History of the American Film Industry from Its Beginnings to 1931 (New York, 1970), passim.

18 For ca. 1910 use of headbands in motion pictures, see Hampton, History, Photograph 18 B. For Ince's use of headbands, see scenes from BLAZING THE TRAIL and THE INDIAN MASSACRE (both filmed in 1912) in Everson, Pictorial History, 28-29 and 32.

19 CHSAP, VII, 212.

20 Ibid.

21 Ibid.
REPLY: The flute in the film was copied from a specimen in the collections of the Father Eugene Buechel, S. J., Memorial Museum at St. Francis, South Dakota, on the Rosebud Reservation. The item is documented as having been collected on the Pine Ridge Reservation during the 1890's. Evidently it was quite old as of that date. To my knowledge, it is the oldest authentic Sioux (i.e., Teton) flute known. It is dissimilar to the later Omaha style flute on which Howard apparently based his statement.

HOWARD: 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.  

REPLY: ...or to me either. However, both the choice of this song and the staging of the singing was done by Lloyd One Star, who used as the basis for its authenticity his knowledge of its use by his grandparents in the manner depicted in the film.

HOWARD: 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.

REPLY: Mrs. Olive Prettybird, a Rosebud Sioux and fluent speaker of Lakota, not only wrote the Sioux script but also recorded it and coached the actors while on location during the filming. With the exception of not more than half-a-dozen word changes (older forms taken from nineteenth century word lists and dictionaries) substituted for the modern forms used by Mrs. Prettybird, her script remained unchanged throughout the film. No matter

22This instrument was similar to the one pictured as D-10, page 57, of the Buechel Museum holdings list published as Father Eugene Buechel, S. J., Memorial Museum (St. Francis, South Dakota, 1973). The actual flute itself is not pictured in this catalogue.

23CHSAP, VII, 212.

24Ibid.

25The word lists and dictionaries used to derive the older forms of certain Lakota words in the script included: the word list of Thomas Say, member of the Stephen H. Long 1819-20 Expedition, found in James, Account, as EWT, XVII, 289-308; the word list of George Catlin, collected by him in 1832 and found in his Letters and Notes on the Manners, Customs, and Condition of the North American Indians (Minneapolis, 1965), II, 262-265; the vocabulary collected by Maximilian and contained in his Travels, as EWT, XXIV, 223-226; and the Sioux dictionary (primarily but not exclusively eastern Sioux, or Dakota) containing words from as early as 1837 and published as Stephen Return Riggs (edited by James Owen Dorsey), A Dakota-English Dictionary, as Contributions to North American Ethnology (Washington, D.C. 1890), VII.
how "uncharacteristic" the film's Lakota script may have sounded to Howard and his Dakota friend, that script was indeed both Sioux and authentic.

HOWARD: The notion that the Teton of this period would name a man "šünkawakan" or "Horse" is absurd. He would have been termed Saglaska (Englishman) or Tōka (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. 

REPLY: Perhaps Howard forgot that some of the Teton men whose naming dated close to the period of the film were Crazy Horse, American Horse, Young-Man-Afraid-of-his-Horses, Red Horse Owner, Stranger Horse, and High Horse -- to name only a few. Even allowing for some inexactness in the English translation of these Lakota names, each of these Teton men carried the name of "Horse," (šünkawakan). As for the last sentence, in which Howard claimed that the Sioux did not use horses as beasts of burden, I would ask him for documentation.

HOWARD: The horsemen in the movie mounted their steeds from the left hand side. Teton Dakota would have mounted from the right.

REPLY: This point not only takes the Pneumatic Nit-Picker Prize but also the Order of the Green Banana Award for being the most persistent myth that has sucker the greatest number of naive ethnohistorians. Between 1736 and the 1860's, hundreds of travelers visited the High Plains Sioux and left narratives describing their experiences. Contained in these primary sources are a wealth of observations covering a myriad of cultural expressions, including extensive information on horses and horsemanship. All of these journals were written by individuals who mounted their own horses from the left side. Yet, in not a single instance that I have noted did any of these left-side-mounting journalists record that the Sioux -- or any other High Plains Indians -- mounted their steeds from the opposite side. I find this great wealth of negative evidence convincing, and in the future, when an 'Indian expert,' Indian or White, dogmatically proclaims that a particular tribe mounted only from the right side, I will expect

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26CHSAP, VII, 213.

27Howard exhibits an overly-glamorized concept of early Sioux horsemanship -- not unlike some of his Sioux contemporaries. In his search for documentation of this idyllic notion, he might find it instructive to read Catlin's description of Sioux use of horses as beasts of burden in 1832 (Catlin, Letters and Notes, I, 44-45 and Plate 21).

28CHSAP, VII, 213.

29The eighteenth and nineteenth century Spanish are reported to have mounted from the right side, the English, French, and Americans from the left; see Frank Gilbert Roe, The Indian and the Horse (Norman, Oklahoma, 1955), 63 ff.
primary source citations before giving the least attention to that 'expert.' For the information of Mr. Howard, and because it more likely reflects the historical situation, in the film horses are shown mounted from both the left and right sides.

After Howard finished his comments, Thurman replied, and in the ensuing trade of rejoinders, sparks flew and the debate became a series of increasingly more acrimonious attacks on each other. The film and its questioned authenticity soon became a background while center stage was filled with smoggy issues and heated attacks on each other. Rather than discuss all points raised in this swirling mass of verbiage (too counterproductive to justify the effort), I will instead comment on only two.

At one point in the debate, Thurman cited George Hyde as an authority on the High Plains Indians, seemingly to the exclusion of other researchers. This is most revealing of Thurman's ability to analyze the sources for High Plains Indian history. Hyde, it will be recalled, suffered from major physical impairments that placed serious limitations on his ability. At age 15, George Hyde became almost totally blind and the following year completely deaf. As a result, he never completed his formal education, never actually saw either the subjects or areas about which he later wrote, and always worked within severely limited objectivity. While Hyde's physical condition was tragic, and is reason for all the more applauding the works he produced, to elevate his writings to a place of scholastic

30 Notwithstanding the early twentieth century writings of Clark Wissler, the work of John C. Ewers (Horse/Blackfoot and Teton Dakota Ethnology and History Berkeley, Calif., 1938 , 33), and the writings of Roe (Indian and the Horse), and their collective sources, all of which are either undocumented or inapplicable to the specific problem of Sioux mounting in the early nineteenth century, my statement still stands: I want PRIMARY source documentation to persuade me that this right-side mounting notion is not a myth. I will also add that I think it particularly significant that W. P. Clark, in his rich source of Indian ethnography, Indian Sign Language With Brief Explanatory Notes (Philadelphia, 1885), 213-214 and 319, takes no notice of mounting customs differing from his own, i.e., the left side.

31 CHSAP, VII, 217. Thurman referred to Hyde as "the man largely responsible for initiating ethnohistorical studies of the Teton Dakota," and on page 226 stated that "in regard to the Teton themselves, Hyde's Red Cloud's Folk was the first serious attempt to deal with the Oglala/sic/ his book on the Brule 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."

32 Philip Gurney, "Indian Politics, Indian Corn Are Grist for George Hyde's Mill," in South Omaha Sun (Omaha, Nebraska), March 16, 1961, 12.

33 For example, he could read written materials only with the aid of thick glasses and a strong hand magnifier, and then only for short periods of time. His communication with the outside world was limited to notes and letters. His sister and his various editors did the necessary preparation of his books for publication.
authority above those of Francis Densmore, Helen Blish, John C. Ewers, and others who actually worked among the Sioux, betrays an inexcusable shallowness on Thurman's part.

The second point needing examination is Thurman's disjointed discussion of Sioux "earthlodge" architecture. Based on interpretations of several winter counts, he claimed that the large structures noted in those sources really meant log houses built by white traders.\(^{34}\) He went on to state, without equivocation, that "there is not one piece of evidence for earthlodes being built by the Dakota west of the Missouri River,"\(^{35}\) and on this basis, further criticized the authenticity of A MAN CALLED HORSE. Apparently, he never realized that the structure seen in the Teton village in the film was not an earthlodge.

Thurman's involved analysis of Sioux construction being mistaken for that of white traders may sound somewhat plausible, on the surface at least. However, on checking his cited sources, I found quite a different situation. Of the four winter counts used by Thurman as the basis for his theory, three clearly show evidence that undermines his ideas. These three, i.e., the winter counts of The Flame, Lone Dog, and The Swan,\(^ {36}\) consistently recorded two different mnemonic devices, one for Indian-built lodges and the other for log structures built by traders. In addition, these two pictographs are mutually exclusive, that is, the one could not be visually mistaken for the other.\(^ {37}\) The fourth winter count on which Thurman built his case, that of American Horse,\(^ {38}\) contained no reference to Indian-built structures at all but mentioned only traders' houses, which are not surprisingly -- designated by a symbol easily recognized as a log cabin.\(^ {39}\)

Thurman also failed to mention that two additional winter counts, those of Cloud-Shield (Oglala Sioux) and Battiste Good (known as Brown Hat, a Brule Sioux),\(^ {40}\) also clearly show two distinct pictographs, one for

\(^{34}\) CHSAP, VII, 205, 216-218, 221, and 229.

\(^{35}\) Ibid., 205.

\(^{36}\) All three of these winter counts are in Mallery's "Pictographs," 100-127.

\(^{37}\) These different devices appear three different times in each winter count; see Ibid., Plates XIV, XV, and XVI.

\(^{38}\) The winter count of American Horse is in Ibid., 130-146.

\(^{39}\) Ibid., Plate XL (Year of 1815-16) and Plate XLI (Years of 1818-19 and 1819-20).

\(^{40}\) The winter count for Cloud-Shield can be found on the very same pages Thurman cited for the American Horse count; see Mallery, "Pictographs," 130-146. The winter count of Battiste Good is in Mallery, "Picture-writing," 287-328. Thurman also cited this winter count in support of another argument (see CHSAP, VII, 217).
Indian-built lodges and the other for white traders' houses. And, on close examination of Thurman's statements, it would appear that he left out a certain phrase, the omission of which gave credibility to his theories. On restoring the phrase in question, his ideas on this subject collapse like a house of cards. Before examining any more High Plains winter counts, perhaps Mr. Thurman needs new glasses.

I accept the premise that the historic Sioux could tell the difference between a trader's log house and an earthlodge-type structure, and that the use of two distinct symbols in the several winter counts was intended to designate these two different architectural forms. On this basis, the data in the following paragraphs bears on the problem of what the Teton built, and where.

Sometime during 1792, the Sans Arc, or Itazipco, band of Teton Sioux made peace with the Arikara and lived with them in their village for about two years. Such an experience provided the Sans Arc with ample opportunity to observe the architecture of the earthlodges built by the Arikara.

In July, 1795, a large group of the Miniconjou band of the Teton arrived at an Arikara village, possibly the one visited by the Sans Arc a year or so earlier. They also remained in close contact with the

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4. Cloud-Shield's pictograph of a trader's log house, shown in Mallery, "Pictographs," Plate XXXIX (year of 1809-10), is quite different from the pictograph of the Indian-built structure in Plate XL (years of 1815-16 and 1816-17). Battiste Good's count makes the same distinction: see Mallery, "Picture-writing," 311 (Figure 349), 315 (Figure 366), 316 (Figures 372, 373, and 374), and 317 (Figure 376).

42. CHSAP, VII, 216, Thurman stated: "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." On the basis of this Thurman concluded that "The sign referred to obviously was a general sign used for any large structure." The missing phrase in Mallery, "Pictographs," 114 and Plate XVIII (specifically cited by Thurman) where The Swan's count recorded for the year 1828-29 that "Trading post opened in a dirt lodge on the Missouri" (underlining added for emphasis to indicate missing phrase), meaning that Thurman has no basis for either his arguments or conclusions.

43. Recorded in The Flame's winter count for the years 1792-93 and 1793-94; see Mallery, "Pictographs," 101 and Plate VII.

44. Doane Robinson (ed.), "Trudeau's Journal," in South Dakota Historical Collections (Pierre, South Dakota, 1914), VII, 473-474. Trudeau referred to this group as the "Ta Corupa," undoubtedly the same as the "Tacchiropapais" which Pierre-Antoine Tabeau a few years later identified as the "Minican-Hinyojoj." Sioux; see Annie Heloise Abel (ed.), Tabeau's Narrative of Louisel's Expedition to the Upper Missouri (Norman, Oklahoma, 1968), 104.
Arikara for as long as a year, thereby giving this band of Teton the opportunity to observe the construction details of the earthlodges of their allies.

During the winter of 1815-16, the Sans Arc band of Teton built their first 'dirt lodge' at Peoria Bottom, on the east bank of the Missouri River, in South Dakota. This event made such an impact on the Sioux that it appeared in a Miniconjou, a Yanktonais, and two Oglala (Teton) winter counts. The following year, the same Sans Arc Teton band lived in their new lodge, and again the event was recorded in the winter counts of several other bands.

In 1828-29, a Sans Arc winter count reported that a white trader (Francois Chardon) built an earthlodge-type structure at the fork of the Belle Fourche and Cheyenne Rivers in Meade County, South Dakota. Chardon was not known for his construction industriousness, nor is there evidence that he previously had close contacts with earthlodges. That he, a white trader familiar with log cabins would choose instead to build by himself an unfamiliar and more difficult form of housing for his trading post is inconceivable. A more probable answer is that the Sans Arc band of the Teton Sioux either built the structure for him, or allowed him the use of an already existing structure -- some two hundred miles west of the Missouri River.

Sometime during 1832, another band of Sioux, probably Hunkpapa Teton, built a large lodge in the area between Cherry Creek and Bear Butte, in the northwestern part of South Dakota. That area is at least one hundred miles west of the Missouri River.

In the summer of 1832, George Catlin, while at Fort Pierre, on the west bank of the Missouri River, saw a structure only recently built by the Teton Sioux living close to that fort. About this structure he wrote:

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45 This probably was the Sioux-Arikara alliance against the Mandan reported by Maximilian in his Travels, EWT, XXIII, 230.


47 Ibid., 136 and Plate XL; 316.

48 Ibid., 114 and Plate XVIII; 279.


The Sioux...had an awning of immense size erected on the prairie which is yet standing, made of willow bushes supported by posts, with poles and willow boughs laid over; under the centre of which there was a pole set firmly in the ground, from which many of the young men had suspended their bodies by splints run through the flesh...51

Clearly this structure was an atrophied form of the earthlodge, built for the performance of a Sun Vow ceremony by the Teton Sioux.

In 1838, two different Teton bands each constructed their own earthlodge-type houses. One of these, built by the Sans Arc on White Wood Creek, which runs through Lawrence County in the Black Hills of South Dakota, was some 300 miles west of the Missouri River.52 Another of these 'dirt lodges,' this one a ceremonial structure, was built by Iron Horn, a Miniconjou Teton chief, along the Moreau River in the northwestern part of South Dakota.53 This stream flows into the Missouri River from the west.

A pattern of architectural construction has emerged from these primary ethnohistorical sources, one not previously mentioned in the textbooks of High Plains Indian history. In the closing years of the eighteenth century, both the Sans Arc and Miniconjou bands of Teton Sioux lived for at least one year each in very close contact with, if not actually in, the earthlodge villages of the Arikara. Not too many years later, one of those bands, the Sans Arc, built for their own use a structure similar to those used by the Arikara. This activity made news among the rest of the Sioux bands, and over the next twenty years, not only did the Sans Arc build two more of these structures but also the Miniconjou and Hunkpapa bands joined them in constructing these permanent lodges. Altogether, by 1840 at least six of these had been built by the Teton, five of them located west of the Missouri River. Two of these are known to have been built for ceremonial usage and the suggestion is strong that at least two others were also constructed for that purpose.

Here some observations are in order. Even though these Teton lodges were clearly influenced by the Arikara earthlodges, were even referred to as 'dirt lodges' in the various translations of the winter counts, and seemed to retain their multi-faceted circular outline, evidence suggests that these Teton structures were not earthlodges per se but rather a variant form using logs, poles, and brush, but little if any, earthen wall covering.

51Catlin, Letters and Notes, I, 233.

52Mallery, "Pictographs," 117 and Plate XXI.

53Ibid. Or along Owl Creek, just north of the Black Hills in Butte County, South Dakota; Mallery's information is not clear.
One winter count pictograph for the Sans Arc 'dirt lodge' in Peoria Bottoms recorded its exterior as a mixture of vertical and horizontal logs, and showed no evidence of earthen covering.\textsuperscript{54} The winter count pictograph for the 1832 Hunkpapa structure depicted it as being of horizontal log construction, again with no earthen covering.\textsuperscript{55} Catlin's description of the Fort Pierre Sun Vow structure indicated that it also was of this variant type, perhaps with "posts" meaning, or in place of, logs.\textsuperscript{56}

Correlative evidence for this hypothesis can be found in an examination of other references to large structures seen in the western parts of the High Plains between 1796 and the late 1850's. Some of these were built by the Crow or Cheyenne, while others are not identifiable as to tribal origins. A number appear to have been associated with ceremonies, and one at least was the site of a Sun Vow rite. Eight of these references contain descriptive information sufficient to see that they were circular, or at least non-parallelogram in form, that they were built of either logs or hefty poles, and that they had no earthen wall covering. Their resemblance to the Teton Sioux structure described by Catlin is worth note.\textsuperscript{57}

Beginning in the early 1840's, a change in style of these Teton-built lodges can be detected from the sources. Long poles covered with brush, rather than logs, became the main building materials, thereby making the structure less difficult to build, and also less permanent. Possibly a realignment of life styles in response to both the disease-caused population decimation and the growing material culture influence of the white traders brought about this modification in building materials. However, the basic form of the lodge shifted only in response to these lighter materials and did not lose its identity as having descended from the earthlodge form.\textsuperscript{58}

\textsuperscript{54} Mallery, "Picture-writing," 316.

\textsuperscript{55} Praus, The Sioux, 14.

\textsuperscript{56} Catlin, Letters and Notes, I, 233.

\textsuperscript{57} I plan a future article on these structures as seen through ethnohistorical sources.

Once the trauma of the main Indian Wars Period passed and the Teton finally settled on their reservations, their more sedentary lifestyle resulted in several architectural developments. The 'pole-and-brush' form of the lodge continued to be built, evolving over the years into the brush arbor, or "squaw cooler," so frequently seen on their reservations today. At about the same time, the older forms of the lodge were revived. In 1883, only four years after becoming permanently settled on the Rosebud Reservation, Iron Shell, a Brule Sioux, recorded that one of that tribe's societies, or akicita, built a "Dance Hall." 59 Reservation tradition places this structure near the now vanished community of Grass Mountain. 60 Within a decade, several more of these structures were built on the Rosebud Reservation: one at Black Pipe (in the "Little Bad Lands" area of the reservation), 61 one on White Thunder Creek, 62 and one near Big Turkey's Camp on Rosebud Creek. 63 In addition, three similar structures were built during the 1890's on the neighboring Pine Ridge Reservation, 64 and at least one on the nearby Standing Rock Reservation. 65


60 Which may explain why some have mistakenly identified this and other similar structures with the "Grass Dance." The structure disappeared during the 1930's or early 40's.


62 Hamilton and Hamilton, Sioux, 162.

63 Ibid., 66.

64 Anonymous, A Drive Through the Black Hills (Battle Creek, Michigan, 1892), 83-85; photograph labeled "Dance Lodge," as #92 in the photographic collection of Frederick Weygold, in the Speed Museum, Louisville, Kentucky; photograph taken by Fr. Eugene Buechel, S.J. and published accompanying the month of January, 1974, Little Sioux Calendar, St. Francis Indian Mission, St. Francis, South Dakota; see also the log lodge architectural form reproduced as an octagonal (?) frame structure built of sawn lumber in Blish, Pictographic History, 497.

65 Francis Densmore, Teton Sioux Music, in Bureau of American Ethnology Bulletin 61 (Washington, D.C., 1918), 468 and Plate 76A.
Francis Densmore, after visiting one of these structures in 1912, wrote:

In every Sioux village there was a lodge of suitable size for social gatherings or dances. An old type of Sioux dance lodge /had/ walls... of logs and the roof of branches covered with earth, a large smoke-hole being left in the center.66

In addition to being used for dances and other social gatherings, these structures also served as repositories for items of importance to the band,67 council houses,68 and in one instance at least, the site of a Sun Dance held sometime near the turn of the century.69

Photographs of six of these structures exist, and a comparison reveals not only a remarkable uniformity of design and building materials in all six but also a striking resemblance to the structures built by the Teton on the west bank of the Missouri River a generation or so earlier. Accordingly, I reject as unfounded Howard's notion that the log medicine lodge was introduced among the Teton Sioux during the 1860's as part of the Omaha Grass Dance complex.70 It seems to me quite evident that these structures represented a revival of an older style of Teton lodge, one built of logs, large poles, and brush, and used as a place for Sun Vow/Sun Dance ceremonies. It was for this reason that the log medicine lodge shown in the film, which is in fact NOT an earthlodge (as mistakenly identified by Mr. Thurman), was copied directly from a photograph of the log medicine lodge constructed by the Teton Sioux (Brule) in the 1890's at White Thunder Creek on the Rosebud Sioux Reservation, some 150 miles west of the Missouri River.71

It is time to pass on to some concluding remarks. I found Thurman's discourses on culture amusing. Dancing gaily across his factually tottering stage, he verbally arabesqued and pirouetted before an awed imaginary audience. Weaving sinuously through a maze of sematica obscura, he thrust and parried mightily in the jousting match with the Windmills of Ignorance. Trumpeting

66Ibid.
67Hamilton and Hamilton, Sioux, 263.
68Ibid., 162.
69Ibid.; note Sun Dance pole.
70Howard, Dakota or Sioux, text for Section III, Plate 6; see also CHSAP, VII, 212.
71Hamilton and Hamilton, Sioux, 162. During research on the film, I found a copy of this photograph in the Metro-Goldwin-Mayer Studio Historical Collections, Los Angeles, California. It later appeared in the Hamilton work.
and preaching, he charged forward, the systemic savior of all that is Right, and the Dreaded Nemesis of all who dare disagree. And finally, when the day seemed darkest and the battle fiercest, off came the wraps of secrecy and out shone the processual excalibur; in a society already beset with superlatives (super powers, super bowls, superchargers, and supermarkets) Melvin [sic]* D. Thurman has now given us the 'super artifact!' 72

Plunging toward the end of the debate, he wielded this mighty weapon with great sophomoric ability, and his discussion of how these 'super artifacts' fit into 'cultural space' became more diatribic and pedantic as it developed. Before he mercifully finished, the reader was treated to a parade of references, pompous in their pointlessness, involving a Southern Baptist Church service, "spatial correlates /and/ systematic relationships of non-tangible symbols and behavior with concrete symbols," and "a Folsom fluted point is a Folsom fluted point... is a Folsom fluted point." 73 All of this Thurman used as the basis that I "precisely... missed /the/ obvious point." 74 To which I must honestly confess that I certainly did!

Both Thurman and Howard spoke ex cathedra on subjects they had not really researched. This led them into errors of a magnitude unbecoming to each. In Thurman's case, this reached absurdity as he attempted to build billowing piles of behavioral constructs on only the barest bones of actual data. It is regrettable that no small amount of processual thought tends to be self-deluding, and the greatest self-delusion of all is that one can be believably nomethetic [sic]* without first being thoroughly ideographic [sic]*.

Thurman no doubt will continue his babblings and incantations to the Great God Methodology. Likewise, Howard probably will make even more of his oracular utterances on subjects about which he actually knows precious little. But it would perhaps be better—assuredly quieter—if all such noises were over and done with. The spectable of two professionals cavorting about like "two-book experts" hardly gives credence to their own qualifications and only impedes others seriously and scholastically researching the past.

To depict an ethnohistorical culture on film, one must first see that culture as its contemporaries saw it, and then translate that image into visual reality. This necessitates the examination of a wide range of primary materials, the ferreting out of specific detail and minute information, the careful analysis of such data in relation to the circumstances of its creation, and finally, the use of the distilled information to construct

72CHSAP, VII, 207. Actually Thurman only resurrected Albert C. Spaulding's term, which I suspect everyone else has tried hard to forget.
73Ibid., 206-207.
74Ibid., 207.

*Editor.
the framework of the cultural range. This was the process used to create
the filmed ethnohistory of A MAN CALLED HORSE. If this process be a
resurgence of antiquarianism, then perhaps both Mr. Thurman and Mr. Howard
should become a bit more antiquated. It might improve their scholarship.

I submit to you, that even with the film's real faults, and despite
the brayings over the film's imagined errors, A MAN CALLED HORSE is still
the most realistic and authentic depiction of the early nineteenth century
Western Sioux yet filmed. It remains for Mr. Thurman, Mr. Howard, or
anyone else so inclined, to better this standard, and I invite them to
do so.
FILMED PLAINS ETHNOHISTORY REVISITED:  
THE DEVALUATION (PROPER EVALUATION) OF DOLLAR

Melburn D. Thurman

A blind man will not appreciate the gift of a mirror.  
Old English Proverb

The charlatans of the nineteenth-century went to surprising lengths to advance themselves, whether academically or simply monetarily. There is, for example, some linguistic evidence and strong circumstantial evidence that Rafinesque, who claimed to have "discovered" the "document," was himself the author of the Walam Olum "of the Delawares." Rafinesque's case, if indeed this was a case of charlatanry, was far from unique, although most charlatans of the period were more directly concerned with monetary return. One of the most interesting of the latter type of frauds was Edwin Eastman's book,¹ in which Eastman misrepresented himself as a former Indian captive to provide a basis for the claim of the curative power of Dr. Clark Johnson's Indian blood syrup.

If the study of nineteenth-century charlatanry shows anything, it is that the context of events and statements, not just the surface meaning of these events and statements, should be examined. I believe it significant that publication of Mr. Clyde Dollar's reply to the "Thurman-Howard debate" roughly coincides with the release of Mr. Dollar's second effort at the filmed interpretation of Plains Indian life. Following the great Hollywood tradition of The Return of the Creature from the Black Lagoon, we are given The Return of a Man Called Horse. Could it be that we can draw an analogy between the Horse films and Dr. Clark Johnson's Indian blood syrup?

This reply to Mr. Dollar's attack on Dr. James Howard and myself has been greatly hampered by Mr. Dollar's procrastination. Mr. Dollar's paper,² dated October, 1975, and delivered in November, 1975, was put into final form over a year after Mr. Dollar first learned of the debate. Mr. Dollar made only eleven minor changes in the paper as delivered, but for some reason he withheld submission of these changes until very late in January or early February, 1976. Mr. Dollar, it is true, thrust a copy of his delivered paper into my hands at the Gainesville meeting of the Conference, but no scholar with

¹[Edwin Eastman], Seven and Nine Years Among the Comanches and Apaches: An Autobiography. (Jersey City, New Jersey: Clark Johnson, 1874).

²Clyde D. Dollar, "Brayings About Horse; the Thurman-Howard Debate on Filmed Ethnohistory," this volume. This paper will be cited hereafter as "Dollar, this volume."
other obligations would undertake to write a rebuttal of a paper with such gross inaccuracies if there seemed to be a likelihood of major changes. I felt confident there would be major changes because of the absurdity of the greater part of the paper. Stan South's letter forwarding the changes made by Mr. Dollar did not arrive at Princeton until February 20, 1976, with a request that I provide a reply by March 1, 1976. Although the time element has been decidedly to Mr. Dollar's advantage, I believe the mirror for Mr. Dollar provided by my reply, if not appreciated by Mr. Dollar, will be appreciated by the readers of this journal who are familiar with Mr. Dollar's hyperbole, bombast, distortions, inaccuracies and lack of theoretical and methodological understanding.

Entering into debate with Mr. Dollar is like trying to wrestle with viscous (perhaps vicious would be more appropriate) molasses. A scientist must first understand his opponent's position, then counter the arguments put forward. Mr. Dollar's technique of argument is not that of a scientist or even that of the scholar in other fields; Mr. Dollar simply puts forward a statement that is a distortion of his opponent's position, then battles the distorted statement with ostensibly clever verbal jabs. In this paper I shall first illustrate Mr. Dollar's unscholarly attitudes, then turn to specific charges leveled against Dr. Howard and myself.

Mr. Dollar's Inaccuracy

Item: More than one place Mr. Dollar referred to me as Melvin D. Thurman, but never by my correct name. 5

Comment: Mr. Dollar is not even certain with whom he is debating.

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3This reply was finished at the end of April.

4In dealing with someone on the margins of scholarship, a scholar is faced with a severe problem, as these people do not "play" by the rules of scholarship. Hence, if one "plays" by the scholarly ground rules, one can either ignore the person or be sucked into an interminable debate. Many scholars have simply ignored Mr. Dollar's papers, which seem to be written with as much rattlesnake venom as ink. I have opted to answer Mr. Dollar, but I do not intend to be drawn into fruitless debate. One demonstration that Mr. Dollar's "arguments" can be demolished by scholarly techniques and that I, as well as he, can play at word games will be sufficient for my purposes.

5Dollar, this volume.
Mr. Dollar's Appropriation of Other's Work

Item: Mr. Dollar, in referring to Navaho blankets, stated:

It should be pointed out that the winter counts on which Howard obviously based his information do NOT record the 18585 [sic]-59 obtaining of Spanish/Navaho blankets as the FIRST event of that nature.  

Item: Earlier I published the following statement:

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 [sic] Winter count) says only that Navaho blankets were brought to the Tetons by traders, not that these were their first blankets.  

Comment: Other cases of similar appropriations by Mr. Dollar could be cited.

Mr. Dollar's Distortion of Opponent's Comments

Item: Mr. Dollar's citation and comments on a point made by Howard illustrate this point:

Howard: The notion that the Teton of this period would name a man "Sünkawakan" or "Horse" is absurd. He would have been termed Ságíšá (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.

Reply: Perhaps Howard forgot that some of the Teton men whose naming dated close to the period of the film were Crazy Horse, American Horse... [and so on]. Even allowing for some inexactness in the English translation of the Dakota names, each of these Teton men carried the name "Horse," (Sünkawakan). As far as the last sentence, in which Howard claimed that the Sioux did not use horses as beast of burden, I would ask him for documentation.  

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6 Ibid.  
7 Following the convention established by Mr. Dollar, this volume, I shall refer to the papers of "the Thurman-Howard debate" collectively as CHSAP, VII. CHSAP, VII, 217.  
8 Dollar, this volume.
Comment: Dr. Howard was certainly guilty of an anachronism here, but Mr. Dollar's main point is simply a gross distortion of Howard's argument. Mr. Dollar apparently overlooked the fact that this journal does not provide galley proofs.* Undoubtedly a key phrase has been omitted in the last sentence, which should read: "The notion of a [man as a] horse [as a horse] as a beast of burden, something less than a man, is a European, not an Indian idea."

It is inconceivable that anyone who has ever studied Plains Indians could seriously argue that the horse was not a beast of burden amongst them in the 1820s. Clearly, Howard was arguing that the Teton Dakotas would never have tried to dehumanize a captive by treating him as a beast of burden, by calling him "horse" in a way analogous to Nazi usage of "sub-human" or "lower race."

**Mr. Dollar's Distortion of Direct Quotations**

Item: Mr. Dollar cited me as writing "A Folsom fluted point, is a Folsom fluted point...is a Folsom fluted point."10

Comparative item: I actually wrote:

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 imbedded in a mammoth has different cultural meaning than a fluted point involved in recent Huichol ceremonialism.11

Comment: I would never write that "a Folsom fluted point is a Folsom fluted point...is a Folsom fluted point." Not only is this a total distortion of my argument, but I am sure that Mr. Dollar's sharp eyes would recognize the similarity with Gertrude Stein's "a rose is a rose is a rose," and he would immediately charge me with plagiarism.

The above points illustrate my view that Mr. Dollar does not adhere to the generally accepted canons of scholarship (in anthropology, history, or whatever other fields Mr. Dollar variously claims to be in), but more irritating is the constant pedantism of Mr. Dollar. In various papers Mr. Dollar has attacked a number of scholars for their spelling, grammar, and virtually all other failings noted by old maid high school freshman English teachers. I will not deal with Mr. Dollar's own cliché studded writing, but there are two comments I wish to make concerning Mr. Dollar's English. A

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9How, if the Teton band in the film had never seen a White man, could they differentiate amongst Whites and refer to their first White captive as "Englishman?"  
10Dollar, *this volume*.  
11CHSAP, VII, 207.  
*[Editor's note: the statement was published as submitted by Howard.]*
great English teacher at the University of Chicago pointed out that "when you attack someone with the juxtaposition of facts, make certain that your facts are right." Mr. Dollar, as I show later in dealing with Plains ethnography, might profit from this advice. The second point is that attacks on an opponent's English usually indicate an inability of the attacker to deal with issues of substance. There are exceptions to this rule, as in an example of Mr. Dollar's spelling which must be touched on, as it suggests that Mr. Dollar has more than orthographic difficulties. Mr. Dollar stated that "the greatest self-delusion of all is that one can be believably nomethetic [sic] without first being thoroughly ideographic [sic]." This would support the views one has after reading Mr. Dollar, that he has little familiarity with any nomothetic approach. Perhaps if Mr. Dollar had more familiarity he would spell the word correctly. It is more surprising that Mr. Dollar also misspelled idiographic.

It is obvious, from Mr. Dollar's statement about "nomethetic [sic]" and "ideographic [sic]" studies, that an elucidation of the role of theory in ethnohistory would be lost on him, so I turn to methodological points which he will hopefully absorb by some sort of osmosis. These points will be dealt with under two headings: Mr. Dollar's "My Indian told me" syndrome; 2) Mr. Dollar's confusion of ethnography with ethnohistory.

Mr. Dollar's "My Indian Told Me" Syndrome

Item: In reply to Professor Howard's charge that the "Indian dancing shown in the film was unlike any Indian dancing I have ever seen," Mr. Dollar stated that the dance was staged by "Lloyd One Star, a Rosebud Sioux knowledgeable in his people's genuine older culture (italics added)."

Item: Dr. Howard referred to "the ridiculous unison singing of Inkapata... (which) has no basis in any ethnographic source known to me." Mr. Dollar asserted the accuracy of this, citing "Lloyd One Star's... knowledge of its use by his grandparents in the manner depicted in the film.""13

Comment: Who is Lloyd One Star? How old is he? What basis is there for asserting that Mr. One Star knows his people's genuine old culture, in apparent contradistinction to a somehow less genuine one known by Professor Howard?

1 Dollar, this volume.

13 Ibid.
Item: Dr. Howard criticized "the endless repetition of words and phrases in Lakota by the 'Indians' [which] seemed...uncharacteristic. In fact, a Dakota friend of mine who saw the movie was highly incensed at this behavior." Mr. Dollar, in reply, claimed the authenticity of the work because it was written by "Mrs. Olive Prettybird, a Rosebud Sioux and fluent speaker of Lakota."  

Comment: What are the qualifications of Mrs. Prettybird for writing dialogue for 1820 Tetons? Mr. Dollar himself admitted that half a dozen older forms of words were substituted in Mrs. Prettybird's script after it left her hands. After all, most Hollywood writers are fluent in spoken English in the sense that Mr. Dollar attributes fluency in Lakota to Mrs. Prettybird. We are all aware of what abominations in dialogue occur in English language film scripts.

Further comment: If Lloyd One Star is sixty or seventy years old, his grandparents could hardly have reported to him a situation earlier than the period 1855-1875, a period for which we have extensive literature obtained by "memory ethnography" done by early anthropologists. If, as Howard argued, what Mr. One Star reported is contrary to what is known ethnographically, the qualifications of Mr. Dollar's informants need to be minutely examined. Anthropologists have developed canons for the assessment of the reliability of informants. Unless Mr. Dollar can demonstrate the bona fides of "his Indians," his argument is "'my Indians' know more than 'your [Howard's] Indians' and the sum total of accumulated ethnographic material on the Teton Dakotas." The burden of proof is clearly on Mr. Dollar.

Mr. Dollar's Confusion of Ethnography with Ethnohistory

Item: Mr. Dollar wrote: "At one point in the debate [with Howard] Thurman cited George Hyde as an authority on High Plains Indians, seemingly to the exclusion of other researchers. This is most revealing of Thurman's [lack of] ability...[in analyzing] the sources of High Plains Indian history...[Mr. Dollar then stated that by the time Hyde was 16 he was almost totally blind and deaf and went on to say that] while Hyde's physical condition was tragic, and is reason for all the more applauding the work he produced, to elevate his

\[\text{Ibid.}\]

\[\text{Although I have criticized several of Professor Howard's interpretations of data (CHSAP, VII, 220), I should like to point out that, with the possible exception of Alfred Bowers, he has probably published more ethnographic data on the northern Plains tribes than any living ethnographer. A service for which we are all in his debt. (Some ethnographers, it is true, such as William K. Powers for the Teton, may have gathered more data than Howard, but are just beginning to publish.)}\]
writing to a place of scholastic authority above those of Francis Densmore, Helen Blish, John C. Ewers, and others who actually worked among the Sioux,\textsuperscript{16} betrays an inexcusable shallowness on Thurman's part.\textsuperscript{17} [In a footnote Mr. Dollar then quoted what I actually wrote: Hyde was] 
...the man largely responsible for initiating ethnohistorical studies of the Teton Dakota [italic added here by me]."

Comment: Earlier I pointed out Mr. Dollar's ploy of distorting opponents arguments. In the above case it is not altogether clear if the distortion was intended or if Mr. Dollar is simply ignorant of the distinction between ethnohistory and ethnography. In light of Mr. Dollar's general ignorance of the methods and techniques of ethnohistory, I choose to be charitable and believe that his confusion of terms was not willful.\textsuperscript{17}

Ethnohistorical research is primarily concerned with the analysis of written records, whereas ethnography involves observation of societies and/or use of informants. Those skilled in one type study are not necessarily skilled in the other, as I have previously pointed out on several occasions.

Mr. Dollar's charges against Hyde are irrelevant to an assessment of Hyde's ethnohistory. Not only are they irrelevant, they are illogical. Mr. Dollar stated that Hyde, who was according to him virtually blind from the age of 15, "never actually saw the subjects or areas about which he later wrote [italics added by me]."\textsuperscript{18} As most of the subjects of whom Hyde wrote had long been dead, it would be impossible for anyone to see them, unless of course someone such as Mr. Dollar physically exhumed their remains (in those cases where remains remain). I feel confident that no one would level a similar charge against a historian. Would Mr. Dollar attack Gibbon because the Romans of which he wrote were never actually seen by him? And although Gibbon did visit Rome, there is no evidence he ever visited Constantinople or the Danube frontiers of Rome, which were important areas dealt with in his history of Rome.

\textsuperscript{16}This passage shows Mr. Dollar's propensity for lack of consistency. Previously Mr. Dollar rejected Professor Howard's objections which were based on "actual...work...among the Sioux."

\textsuperscript{17}There are a number of examples of Mr. Dollar's willful distortion of opponents' comments in his paper. For example, he charged that "Melvin [sic] D. Thurman has now given us the 'super artifacts'." Yet in the footnote the distortion is "corrected" by Mr. Dollar's "clarification" that "actually Thurman only resurrected Albert C. Spaulding's term, which I suspect everyone else has tried hard to forget." This technique of distorted statement which is "clarified" in a footnote, is precisely the same approach Mr. Dollar used in treating with my discussion of Hyde's contribution to plains ethnohistory.

\textsuperscript{18}Dollar, this volume.
Mr. Dollar's attack on Hyde is based on a neighborhood newspaper, the March 16, 1961 issue of the South Omaha Sun. Only one copy of this paper could be located; the Omaha Public Library throws the Sun out after three months. According to this paper, "Mr. Hyde's own schooling ended when he was 15, at the time when his sight began to fail. His hearing went the next year." This would be in 1898-9. From this, Mr. Dollar concluded that, Hyde's work was of necessity of "severely limited objectivity."

Savoie Lottinville, who was Hyde's editor and knew him for over thirty years, gave a significantly different picture of Hyde than that given by Philip Gurney in the Sun article. According to Lottinville, Hyde suffered from a "...total lack of hearing from about his twentieth year. Today [1966] his added handicap of limited vision further complicates the problem [of communication with him]." Hyde was employed by Grinnell "...as his research assistant, sometime after the turn of the century." His task was "...to seek out Cheyenne informants and collect from them the oral history and eyewitness testimony which Grinnell needed but couldn't always get." This work, among other things, brought about the Hyde-Bent correspondence of 1905 to 1918, which is of extraordinary interest to Plains ethnohistorians.

In scholarship it is results and not good intentions that count. I submit that Hyde's corpus, despite some obvious faults, is the most significant body of ethnohistorical literature for the plains yet authored by one man.

19 I tried without success to obtain a copy of Mr. Dollar's paper, "A Tribute to George Hyde," which was supposedly read before the Rosebud Sioux Tribe Historical Conference on May 3, 1968. This paper is said to have been published in the Journal of the Conference.

20 Mr. Dollar also claimed, this volume, that Hyde's blindness and deafness "placed serious limitation on his ability." I would say that ability can compensate for almost any impairment, but that unimpaired stupidity cannot be compensated for. I hope someone will gently explain this to Mr. Dollar.

21 Savoie Lottinville in George Hyde, Life of George Bent Written from His Letters (Norman, Oklahoma, 1968), xvi.

22 Savoie Lottinville in George Hyde, The Pawnee Indians (Norman, Oklahoma, 1974), v.

23 Ibid., vi.

24 Mr. Dollar, this volume, slurs the results of Hyde's work by proclaiming that Hyde's "...sister and his various editors did the necessary preparation of his books for publication." This argument is similar to the argument that Shakespeare did not write Shakespeare's plays or that another blind Greek by the name of Homer, not Homer wrote Homer's Iliad. More recently, most literary critics recognize Thomas Wolfe as a "genius," while also recognizing that his editor did the "necessary preparation of [Wolfe's] ...books for publication." The same comment of Mr. Dollar's could also apply to the work of the blind historians Prescott and Parkman.
Hyde's correspondence with George Bent brought forth an important source we would not otherwise have. His work on corn which he carried out in his backyard in Omaha, led to the first monograph on corn among the Missouri River Indians. The Ethnographic Bibliography of North America recognized Hyde's Pawnee Indians as the outstanding single publication on this group. What is perhaps even more interesting is that often, when Hyde made an error it was repeated by those who followed him, frequently without direct citation being made to his work. As I have shown, Gene Weltfish, perpetuated an error by following Hyde's account of the reputed 1833 Skidi-Pawnee Morning Star Sacrifice. Other examples of this kind could be cited.

Savoie Lottinville, summarizing Hyde's stature, wrote that "the stature of a historian is often measured by the extent to which his fellow craftsmen reveal their dependence upon him." George Hyde's stature can be assessed as Lottinville suggested, but a more telling assessment would be not the citations to Hyde's work but the similarity of later author's positions to positions first outlined by Hyde. My statement that Hyde was "the man largely responsible for initiating ethnohistorical studies of the Teton Dakota" does not need modification.

Mr. Dollar's Ignorance of Plains Indian Ethnohistory

In this portion of this paper I will try to outline the context of my objection to Mr. Dollar's "work," rather than deal with each of his absurd

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25 George F. Will and George Hyde, Corn Among the Indians of the Upper Missouri (Cedar Rapids, Iowa, 1917).


28 Savoie Lottinville, in George Hyde, Life of George Bent Written from His Letters (Norman, 1968), xviii. As Hyde pointed out in his Red Cloud's Folk (Norman, 1957), vii, first published in 1937, this is "A complete history of one of the Teton Sioux tribes...." Up to this time no such work has appeared in print.

29 CHSAP, VII, 217. See also 226.
statements individually. If I should fail to touch on any of these absurd statements, I hope Mr. Dollar will point it out so that I might show what is objectionable in it. I turn now to what Mr. Dollar believes are the "facts" of Plains ethnohistory.

(Thurman's Original Attack on Dollar and Dollar's General Failure to Reply)

My principal points in attacking the authenticity of Mr. Dollar's film concerned the film's failure to adequately deal with a number of aspects of the Teton Dakota subsistence-settlement system in the 1820's. The points distorted in the film include: 1) an overstatement of Teton sedentariness; 2) lack of concern for Teton subsistence patterns; 3) distortion of Teton horse utilization; 4) failure to understand Teton involvement in widespread logistics networks; 5) failure to consider "cultural space," leading to the misattribution of semi-sedentary "traits" to "nomadic" Plains Indians, such as the Tetons. Mr. Dollar failed to reply to most of these points.

1) The Film's Overstatement of Teton Sedentariness.

In my original discussion of "A Man Called Horse," I pointed out that the Tetons of the village in which "Horse" lived... led 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 unfold through a period of at least one and one-half years.31

Mr. Dollar did not include even one sentence in reply to this.

2) The Film's Lack of Concern for Teton Subsistence Patterns.

In my original discussion of the film, I pointed out that "...one would obtain from the film no more than a vague notion of subsistence patterns..."32 Mr. Dollar did not include a single word in reply. This was to be expected, as I will now show, in dealing with antelope hunting by Plains Indians, in that Mr. Dollar is grossly ignorant of Plains Indian subsistence techniques.

2a) Antelope Hunting by Plains Indians.

In discussing horse corrals, Mr. Dollar wrote that "Lewis and Clark noted a [horse] corral used by the Assiniboine in North Dakota in 1805."33

An especially rich example of absurdity is Mr. Dollar's paragraph beginning "It is time now to pass on to some concluding remarks," this volume. After I deal with Mr. Dollar's "facts," I am sure that the unbiased observer will agree that it is time for Mr. Dollar to "pass on."

30CHSAP, VII, 205.
31Ibid.
32Dollar, this volume.
He then mentioned, in a footnote, that "Clark interpreted this as an 'antelope pen,' a highly improbably [sic] designation if the description is read carefully [italics added by me]."

In the original source, Lewis, quoting Clark, wrote:

...I saw the remains of several camps of Assiniboins; near one of which is a small raven~. there was a park which they had formed of timber and brush, for the purpose of taking the cabrie or Antelope. it was constructed in the following manner: a strong pound was first made of timbers, on one side of which there was a small apparature, sufficiently large to admit an Antelope; from each side of this apparature, a curtain was extended to a considerable distance, widening as they receded from the pound [italics added by me].

Clark's description of an antelope pound can be precisely equated with antelope pounds among virtually all other Plains Indians. Mooney, for example, in writing of a Kiowa impoundment recorded on a Kiowa winter count for 1860-61, described the pound as being an

...open corral of upright logs, stripped of their branches, with an entrance, from which diverged two lines of posts set at short distance from one another and covered with blankets to resemble men. The antelopes were surrounded on the prairies and driven toward the corral until they came between the converging line of posts, when it was an easy matter to force them into the closed circle, when they were slaughtered.

For the group dealt with by the Horse film, the Teton Dakotas, antelope pens are mentioned in the winter counts for 1860-61, as well as for 1828-9.

So much for Mr. Dollar's "careful" reading of Lewis and Clark, an example of Mr. Dollar's analysis which will prepare the reader for other "careful" readings by Dollar discussed further on. Contrary to Mr. Dollar's wish, Clark's antelope pound was just that and not a horse corral. Were Mr. Dollar better informed regarding Plains Indian subsistence techniques he would never have doubted Clark's interpretation.

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3) The Film's Distortion of Teton Horse Utilization.

In my original discussion of the "Horse" film, I mentioned that "...one would obtain from the film no more than a vague notion of... the overwhelming importance of horses in Dakota society." Mr. Dollar, in reply to this charge, betrayed a gross lack of understanding of the Plains literature. I shall deal with horse utilization under two headings: a) horse mounting by Plains Indians; b) horse corrals of Plains Indians. These are the only two points of horse utilization to which Mr. Dollar addressed himself in his reply.

3a) Horse Mounting by Plains Indians.

The issue of horse mounting has been of considerable importance to scholars concerned with the origin and diffusion of the "horse complex" on the Plains. Motor habits are culturally determined, hence comparisons of similarities and differences in motor habits (such as side of mounting a horse) are potentially important in unravelling historical relationships and coming to grips with processual questions.

Professor Howard noted that in the film "horsemen...mounted their steeds from the left hand side," whereas "Teton Dakota would have mounted from the right." 37

In reply to this, Mr. Dollar wrote that Dr. Howard's:

...point not only takes the Pneumatic Nit-Picker Prize but also the Order of the Green Banana Award for being the most persistent myth that has suckered the greatest number of naive ethnohistorians. Between 1736 and the 1860's, hundreds of travelers visited the High Plains Sioux.... Yet, not in a single instance that I have noted did any of these left-side-mounting journalists record that the Sioux—or any other High Plains Indians—mounts their steeds from the opposite side [italics added by me]. 38

Mr. Dollar continued, in a footnote:

Notwithstanding the early 20th century writings of Clark Wissler, the work of John C. Ewers..., and the writing of Roe..., and their collective sources, all of which are either undocumented or inapplicable to the specific problem of Sioux mounting in the early 19th century, my statement still stands: I want PRIMARY source documentation to persuade me that this right-side mounting notion is not a myth.

37 CHSAP VII, 213.
38 Dollar, this volume.
39 Ibid.
Comment: This exchange between Mr. Dollar and Professor Howard illustrates Mr. Dollar's scholarly ineptitude in at least two counts: 1) again Mr. Dollar stated one thing in the text and put another thing in his footnote; 2) Mr. Dollar is ignorant of the meaning of "primary source."

3a 1 Textual and footnote differences in Mr. Dollar's statements on horse mounting.

In the text of the paper Mr. Dollar asked for any source on right-side-mounting High Plains Indians between 1736 and the 1860s. Ewers, for example, provided such a citation in an 1855 account of a Comanche woman mounting a horse from the right side, a fact which totally destroys Mr. Dollar's argument. In the footnote, Mr. Dollar tried to squirm out of an obvious untenable position by saying that the only thing that can really destroy his position is a reference from the early 19th century for the Teton Dakotas.

3a 2 Mr. Dollar's ignorance of the meaning of "primary source."

As Mr. Dollar will not accept the sources cited by Wissler, Ewers, or Roe as being primary sources, it is obvious that to him a primary source is a record of a particular observation at a particular place. Primary sources are certainly often of this kind, but a general statement by a first-hand observer also constitutes a primary source, and there are a number of such observations of right-side-mounting "High Plains Indians" for the period between "1736 and the 1860's."

Colonel Richard Irving Dodge was cited by both Wissler and Roe to the effect that Plains Indians mounted from the right side. Colonel Dodge spent about 30 years among Plains Indians and in his book, published in 1882, he stated unequivocally that "...Indians mount always from the right side." Further, there is no question that Dodge was familiar with Teton Dakotas. For example, he mentioned that in 1867, while commanding the fort, "a Sioux Indian came to Fort Sedgewick." This was certainly not an

40 John C. Ewers, "The Horse in Blackfoot Indian Culture, Bureau of American Ethnology, Bulletin 159, C8, n. 38. It is not surprising that Mr. Dollar, being so closely associated with Horse films, must often cavalierly dismiss data—as in his rejection of Wissler, Ewers and Roe, the authorities on horse utilization among Plains Indians.


43 Richard Irving Dodge, Our Wild Indians: Thirty-Three Years Personal Experience (Hartford, 1882), 339.

44 Ibid., 263.
uncommon occurrence, as Fort Sedgewick was in the northeastern corner of Colorado, an area which was in the heart of the Teton country of that period.

There are sources other than the ones cited that Plains Indians mounted from the right side of their horses. Further, Wissler cited Dobrichoffer that Paraguayan Indians mounted from the right side as well.

Further comment: The issue involved in the study of the side of mounting a horse has traditionally been connected with the study of the origin of the "horse complex" of the Plains Indians. Wissler, who formulated the problem, agreed that the Indians largely took over the "horse complex," from a Spanish model, but he felt that the Indian practice of mounting horses from the right side (which he thought differed from the Old Spanish practice) could be attributed to historical and instinctive factors—Indians, unlike Europeans did not wear swords, and right-handed people instinctively tend to mount horses from the right side.

Wissler wrote:

The Indian has shown no originality. He devised no important appliances for using horses. He manufactured his own saddles, bridles, etc., but followed precisely a few definite patterns. Though these patterns appear to us as Indian, that is because the European colonists brought with them the English saddle. The Indian model is fundamentally like that of Southern Europe and Asia during the period of American colonization and still survives among the tribes of Patagonia. In general, the complete data will show that the greater part of the horse complex of the North American Indian was borrowed first by the tribes in contact with the Spanish settlements and then diffused as far as the Plains of Canada without loss or essential modification of detail.

The one striking variation is the habit of mounting on the right side of the horse instead of the left as do Americans and Europeans. The comparative data on the period make it clear that if left to their inclinations right-handed people will mount from the right. Historical data show the European method to have been first introduced into cavalry-tactics by Vespasian and to have survived to this day because the sword is worn on the left side.

More recent work on Spanish horsemanship has contradicted Wissler's statement about side of horse mounting. Roe stated that:

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Wissler, "Riding Gear...," 35.

Ibid., 37-8.
While sundry details of Spanish horsemanship have been observed since fairly early (horse) times, the outstanding phenomenon is the Spanish practice of mounting from the right-hand side, an inheritance from the Moorish Arabs.  

In light of the divergent opinions on the Spanish technique of horse mounting, it is not surprising that Ewers concluded that "there is a need for a careful analysis of the Spanish-Mexican horse complex of the colonial period which will afford us a detailed, factual basis for comparison with the horse complex of the Plains Indians."47 Ewers' own contribution has been his detailed analysis of the Blackfoot Indian horse complex in which he showed, by distinguishing traits of probable European origin, traits of possible European origin, European traits which were rejected, and traits of Indian origin, that Wissler's statement of Indian "slavishness" was somewhat overdrawn.48

Although there is some difficulty about determining the mounting side used by colonial Spanish horsemen, all authorities agree that the core of the "horse complex" of the Americas diffused to the Indians from the Spanish. All specialists also agree that the Plains Indians uniformly mounted from the right side.50

3b) Horse Corrals of Plains Indians.

Mr. Dollar attacked my criticism of the horse corrals shown in the original film. He wrote:

Thurman... [stated] that horse corrals probably were of European origin and therefore should not have been shown in the film. In support of this statement, he called on John C. Ewers' work, The Horse in Blackfoot Indian Culture....Unfortunately, Thurman neither read Ewers' fine work carefully nor thought

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47 Roe, The Indian and the Horse, 63-4.
49 Ibid., 327-331.
50 Mr. Dollar argued that Indians probably mounted "...from both the left and right." The only support for this is a statement given by a single informant of Ewers who said that in the old days a left-handed man would mount from the left side because it was easier (Ewers, "The Horse in Blackfoot Indian Culture," 68). Mr. Dollar's pressing of this point of Indian mounting from either side requires that he simply ignore the now large literature on the cultural transmission of motor habits.
through the problem... Even European origins for these features would not preclude their use on the High Plains in the early 19th century.

Mr. Dollar then cited references to what he identified as horse corrals: for the Assiniboins in 1805; Jacob Fowler's reference which Mr. Dollar read as referring to horse corrals among "five different high plains tribes"; Maxmilian's 1833 data for the Blackfeet; and the only definite reference for corrals, an 1837 reference from the Omahas.

Mr. Dollar then continued:

Whether these features were a borrowed trait begs the point; their use by numerous High Plains tribes during the period shown in the film is documented. As these corrals were associated with both temporary camps of nomadic tribes as well as permanent villages of sedentary Indians, that a Sioux tribe in a semi-sedentary village situation could not have constructed horse corrals is ridiculous!

Comment: This portion of Mr. Dollar's paper shows such breadth of ignorance that I must deal with the points raised under three separate headings: 1) Mr. Dollar's misconstruction of my argument; 2) Mr. Dollar's misuse of sources; 3) Mr. Dollar's misunderstanding of comparative plains ethnology.

I shall show, under the various subheadings below, that the use of horse corrals among the nomadic plains tribes was a development following from direct association with whites; that the references Mr. Dollar cited as showing horse corrals among these nomads were either distorted by Mr. Dollar or at least very equivocal; and that Mr. Dollar's citation to real horse corrals among the Omahas has no bearing at all on the argument. This last point is particularly revealing of Mr. Dollar's ignorance--the Teton of the film should not have been in a sedentary village, such as the Omaha, who were in fact only semi-sedentary.

3b 1) Mr. Dollar's Misconstruction of My Argument.

Mr. Dollar's point that horse corrals might have diffused to the Tetons as part of the original horse complex is well taken and should be obvious to anyone who has read the section on side of horse mounting by Plains Indians.

51 In Dollar, this volume, footnote 10, Dollar mentioned that "Thurman cited page 328 [of Ewers' book]." This is a portion of the summary of Ewers' conclusions. The implication is a slur suggesting I have never read the book. I would be happy to invite Mr. Dollar to my home to see my annotated copy of Ewers, provided, of course, that he is accompanied by a responsible adult.

52 Dollar, this volume.

5 Dollar, this volume.
The point I wished to convey in my original paper was that there is no evidence of horse corrals among the "nomadic" Plains Indians until their association with whites. Ewers, who is the only scholar who has dealt with the problem, wrote that one of his informants,

Lazy Boy[,] believed the Blackfoot obtained the idea of horse corrals from Whites. Indeed the whole procedure of guarding these corrals is suggestive of white influence. Nevertheless the use of horse corrals by Plains Indians was widespread and can be traced back to the early years of the 19th century.54

All but three of Ewer's references referred to the semi-sedentary plains tribes which did, in fact, have horse corrals in the early 19th century. Two of the references of Ewers are two of those cited by Mr. Dollar for the Plains "nomads." The third reference by Ewers was to the "Northern Shoshoni," a marginal plains "nomadic" tribe. In the section below I shall discuss Ewer's three references to the "nomads."

3b 2) Mr. Dollar's Misuse of Source Material.

I have already shown, in dealing with antelope pounds, that one of Mr. Dollar's references to "nomads' horse corrals" referred to an antelope pound rather than to a horse corral. I shall now show that the references of Jacob Fowler and Maximilian almost certainly do not refer to horse corrals either.

Jacob Fowler was barely literate and any of his statements must be carefully analyzed. The editor of Fowler's diary, Elliott Coves, said that the owner of the manuscript called Fowler's writing "hieroglyphics." Coves mentioned that "the syntax is the sort which has been happily called 'dash dialect,'" and noted that Fowler's "...spelling speaks so well for itself in print that little need be said on that score....[for example he wrote] 'campe,' 'cape'd,' 'capped,' or 'capted' for camped."55

Fowler, in describing the location of horses in a large "nomad" village in the south plains, wrote:

...The Indeans manifest a more friendly disposition and Intimate an Intention of moving down the River In Consequence of the many Horses Stolen from them Heare--between 4 and 500 Horses Have [been] Stolen from them Since we arrived and mostly from Pens in the Center of the village surrounded by upwards of seven Hundred lodges of Wachful Indeans...56

54Ewers, "The Horse in Blackfoot Indian Culture," 209.


56Ibid., 60.
The crucial point here is what Fowler meant by "Pens"; did he mean "pens" or "pins"? It is well known that the best horses in a Plains Indian camp were picketed in front of the owner's tipi. Ewers, for example, noted of the Blackfeet picket pins that:

The preferred picket pin was a forked length of serviceberry about 2 inches in diameter and 22 inches long. One end was driven about a foot into the ground. The line was tied below the fork at the upper end to prevent its slipping off should the horse become restive or frightened. 57

I shall return to the point of "Pens" or "pins" after dealing with the Maximilian reference.

Mr. Dollar cited the English translation of Maximilian, rather than the German original, which presents some problems at precisely those points of crucial concern to those interested in horse corrals. The printed English translation reads:

We proceeded to a large circle in the middle of the [Blackfoot] camp, enclosed with a kind of fence of boughs of trees, which contained part of the tents, and was designed to confine the horses during the night, for the Indians are so addicted to horse stealing that they do not trust each other. 58

The German original reads:

Wir traten in der Mitte des Lagers in einen grossen runden, mit dicken und dunnen Baumzweigen zaunartig umgebenen Platz, welcher einen Theil der Zelte enthielt, und bestimmt war, während der Nacht die Pferde aufzunehmen; denn die Indianer sind so grosse Freunde des Pferdestehlens, dass sie in dieser Hinsicht einander wechselseitig nicht trauen. 59

My translation of the German reads:

We entered the middle of the camp into a large round plaza which contained part of the tents and was surrounded by large and small branches arranged like a fence and which was designed to contain the horses during the night because the Indians so love horse stealing that they do not trust each other in this regard. 59

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57 Ewers, "The Horse in Blackfoot Indian Culture," 40.


59 Maximilian Prinz zu Wied, Reise in Das Innere Norde-America in den Jahren 1832 bis 1834, (Coblenz: J. Hoelscher, 1839), I, 590.
The problem is in the translation of "enthieilt" and "welcher." "Welcher" ("which") refers to the plaza rather than to the fence, and "enthieilt" ("contained" or "incorporated") might thus be read to mean that some of the tents were in the plaza with a fence around them or that part of the tents were within the plaza and the rest were within the fence. In either case the situation would indicate that tents were within, or a possible component part of the "horse corrals." This puts an altogether different configuration on the reference cited by Mr. Dollar.

The Blackfoot reference of Maximilian seems to refer to an inner camp circle with the component tents of the circle linked together with cut tree branches, a situation which has probably been referred to, but not detailed, by a number of ethnographers of "nomadic" plains tribes. Ewers' Shoshoni reference, perhaps refers to similar situations, although Ewers thought the reference indicated a horse corral. Ewers wrote that "Lowie's statement that the Northern Shoshoni kept their horses inside their camp circles would imply some sort of corral." A number of writers, such as Donald Collier for the Kiowa, have suggested that the "nomads" camp circle developed after horses were obtained and were an adaptation to horse ownership. I subscribe to Collier's argument and suggest that these camp circles were of two kinds. On the one hand there was the circle enclosed by branches, as among the Blackfeet observed by Maximilian, while, on the other hand, there were "unenclosed" camp circles, where the horses were picketed on pins. There are no unequivocal data which show that early 19th century plains "nomads" used horse corrals and, unless unequivocal sources are found, we should reject the notion that horse corrals were used by the "nomads."

3b 3) Mr. Dollar's Misunderstanding of Comparative Plains Ethnology.

In my original paper I pointed out the greatest flaw in Mr. Dollar's approach—he's "definition of 'authenticity' [is apparently merely] ...the placement of cultural attributes to a particular area and time." This definition fails to distinguish "cultural space." Mr. Dollar's citation of the Omaha (one of the semi-sedentary tribes) as having horse corrals in the early 19th century is totally irrelevant to the problem of whether or not the "nomads" had horse corrals then. Mr. Dollar's argument is analogous to arguing that the band organized Bushmen must have automobiles now, because as is well known, South Africans of European descent have automobiles in 1976.

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60I am indebted to Ursula Gedra, Department of German, University of Maryland for comments on my translation.

61Ewers, "The Horse in Blackfoot Indian Culture," 209, n. 52.

62CHSAP VII, 205.

63Ibid., 206.
4) Mr. Dollar's Failure to Understand Teton Involvement in Widespread Logistics Networks.

In my original paper I pointed out certain problems in Mr. Dollar's treatment of Teton logistic networks:

...[O]ne of the members of the war party which captured the Englishman possessed a Navaho blanket of a style not known from the 1820's... 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 shows that the gun frontier (spreading from the east) was well to the west of the Black Hills [where Mr. Dollar's Sioux were located in the film] by 1790 (Secoy 1953:104-6). Further, in light of the nature of the middle-Missouri River trade (Jablow 1950) it would be strange indeed if a group of Dakotas in the 1820's had never seen a white man. 64

Mr. Dollar did not reply to the point raised in this paragraph. The only reply Mr. Dollar made was to question my identification of the Navaho blanket! Mr. Dollar stated:

Gentlemen, WHAT Navaho blanket? No such article appeared in the film, and the only item having even a remote possibility of this interpretation was an early 19th century English coverlet, clearly shown to belong to the Englishman, and just as clearly shown to have been appropriated from him by his Sioux captor. 65

As I recall, the blanket was a "saddle" blanket of one of the warriors. I definitely was not referring to Mr. Dollar's coverlet. The point is irrelevant to my argument about Mr. Dollar's failure to consider Teton Dakota logistics. I cannot find the film currently playing, so cannot refresh my memory. But, be that as it may, I certainly wouldn't want to sit through the film again. I might add that I collect Navaho blankets and have examined literally thousands of them. Although it is possible I might have erred in identifying the Navaho blanket, I believe it is highly improbable that I did so.


65Dollar, this volume.
5) Mr. Dollar’s Failure to Consider "Cultural Space."

There is little need to touch on this point as I referred to this when dealing with Mr. Dollar’s misunderstanding of comparative Plains ethnology. Nevertheless, I cannot resist quoting Mr. Dollar’s acknowledgement of this point:

[Thurman's]...discussion of how these "super artifacts" fit into 'cultural space' became more diatribic and pedantic as it developed. Before he mercifully finished the reader was treated to a parade of references, pompous in their pointlessness, involving a Southern Baptist Church service, "spatial correlates [and] systematic relationships of non-tangible symbols and behavior with concrete symbols," and [a discussion of Folsom and Huichol use of Folsom points]. All of this Thurman used as the basis that I [Dollar] "precisely...missed (the) obvious point." [T]o which I must honestly confess that I certainly did!66

(Mr. Dollar’s Other Points)

Mr. Dollar touched on three other points concerning Plains ethnohistory:
1) The interpretation of winter counts; 2) the "evolution" of the Teton earthlodge and the relation of the earthlodge to the dance hall; 3) Indian headbands. I shall deal with each point in turn.

1) The Interpretation of Winter Counts.

In my original statement on winter counts, I argued against particularistic interpretations of the counts, suggested that the count keepers had only a hazy idea of the meaning of many of the signs, and showed that a non-particularistic approach demonstrates that a number of "obvious" interpretations of the meaning of signs should be abandoned. I showed in one paragraph that a single sign in a single winter count is used in three different ways (to indicate an earthlodge, a trading post and a medicine lodge). I concluded, from these that "the sign referred to obviously was a general sign used for any large structure."67

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66Dollar, this volume. This paragraph of Mr. Dollar’s is a gem of innuendo and distortion; a treatise could be written on it. I will, however, direct the reader to only one point, my "quotation" pasted together by Mr. Dollar in the next to last sentence. My original statement, CHSAP, VII, 207, follows: "This is obvious in the case cited, but this is precisely the point missed by Dollar." Mr. Dollar, who would lead us to believe that his English usage is always precise, once again violated the rules of English quotation in this citation.

In regard to my theoretic statements, I should like to point out that by some error there was a failure to acknowledge the assistance of my former colleague William Stuart of the University of Maryland. After discussion with Dr. Stuart, five of the words in the original draft were changed. The modified text appears in CHSAP VII, 220, paragraph 3.

67CHSAP VII, 216.
In the paragraph following the above paragraph, I showed that the interpretation of a single sign for a single year (1820-1) was given three different interpretations by the three different count keepers, although all three counts had a common source. Further, the sign in all three cases was the same sign.  

In the two paragraphs referred to above I showed there was considerable variation in meaning when we consider a single sign; in the next paragraph I showed that different signs can have the same meaning.  

The aim of these three paragraphs was to demonstrate how we can proceed to systematic analysis, rather than rely simply on particularistic studies. Systematic study requires comprehensive comparisons. I pointed out that "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 interpretations are based on this corpus and not simply on the four which I cited. I believe that a little further comment will be sufficient to answer Mr. Dollar.  

The following points were the objections raised by Mr. Dollar: a) In the three winter counts drawn from the same source "...The Flame, Lone Dog, and the Swan...consistently recorded two different mnemonic devices, one for Indian-built lodges and the other for log structures built by traders"; b) the "two pictographs (used by the three count keepers) are visually mutually exclusive, that is, one could not be visually mistaken for the other"; c) the winter counts of Cloud Shield and Battiste Good (Brown Hat) "...also clearly show two distinct pictographs, one for Indian-built lodges and the other for white traders' houses"; d) American Horse's winter count "...contained no reference to Indian-built structures at all but mentioned only traders' houses, which are--not surprisingly--designated by a symbol easily recognized as a log cabin; e) Thurman "...left out a certain phrase the omission of which gave credibility to his theories"--"the missing phrase...[which is in Swan's 1828-29 records] is that a 'Trading post opened in a dirt lodge on the Missouri'" (underlining added [by Mr. Dollar] for emphasis to indicate missing phrase.)  

In order to deal with Mr. Dollar's comments, it is necessary to touch upon several of the statements given in the winter counts by the count keepers, the interpreter or the "ethnologist." I detail these three as it is usually

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68Ibid.  
69Ibid., 217.  
70Ibid., 218.  
71Mr. Dollar, this volume, refers to "...the four winter counts used by Thurman..." I have been working for some time on the winter counts and will not be drawn out by Mr. Dollar until I am ready to publish the material as it should be published.  
72Dollar, this volume.
impossible to distinguish precisely who is responsible for the various statements we have in the literature.

1817-1818
(Sign: Log cabin with a leafless tree.)

The Flame: "Trading store built at Fort Pierre."

Lone Dog: "La Framboise, a Canadian, built a store with dry timber. The dryness is shown by the dead tree. La Framboise was an old trader among the Dakotas. He once established himself in the Minnesota Valley. His name is mentioned by various travelers."

The Swan: "Trading post built on the Missouri River 10 miles above Fort Thompson."

(No signs for the following.)

"...Mato Sapa says: A trading house was built on the Missouri River 10 miles above Fort Thompson.

Major Bush says the same as last, but that it was built by Louis La Conte."73

(Sign: A long house with smoke coming from chimney, man with hat next to it.)

Battiste Good: "'Choze-built-a house-of-dead-logs winter.' The house was for trading purposes. The Frenchman's name is evidently a corruption."74

1819-1820
(Sign: A log house with smoke coming from chimney.)

The Flame: "Another trading store was built."

Lone Dog: "Another trading store was built; this time by Louis La Conte, at Fort Pierre, Dakota. His timber, as one of the Indians consulted specifically mentioned, was rotten."

The Swan: "Trading post built on the Missouri River above Farm Island (near Fort Pierre)."

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73 Mallery, "Pictographs..." 109, pt. xiv. Major Bush was the owner of a calendar similar to Lone Dog's count, obtained in 1870. Ibid., 208. For Lone Dog, see Garrick Mallery, "Picture-writing of the American Indians," Bureau of American Ethnology, 10th Annual Report for 1888-89, 277. For more on Major Bush's winter count, see Ibid., 94.

74 Ibid., 316.
"White-Cow-Killer calls it: 'Made-a-house-of-old-wood-winter!'"75

(Sign: A square apparently representing a log structure, with a man, apparently hatted, standing next to it.)

Battiste Good: "'Choze'-built-a-house-of-rotten-wood-winter.' Another trading house was built."76

Comparison of these two years shows how "non-historic" the counts are.

Comment: The Flame in 1817-8 mentioned the building of a "trading store... at Fort Pierre," while in 1819-20 he mentioned "another trading store" at an unspecified location. This order was reversed by The Swan, who mentioned, in 1817-18 a post "...on the Missouri River 10 miles above Fort Thompson,"77 while for 1819-20 he mentioned a fort being established near Fort Pierre. Battiste Good, unlike the other two count keepers, did not really differentiate 1817-18 and 1819-20, but only mentioned that the trader "Choze" in the former year used "dead logs," and "rotten wood" in the latter year, for the construction of his post at an unspecified location.

If the winter counts are problematical in assignation of fort location, time of building and name of builder, what justification do we have for assuming that we can learn anything about Teton history from the winter counts considered for 1817-1818 and 1819-1820? Mr. Dollar has told us that the signs of the kind for these years are obviously White structures as the signs look like log-cabins. It would seem then, that the most we can conclude from these counts is that "something happened at a White fur post." Can we, however, even conclude that? I shall return to these points, but will anticipate my conclusion by pointing out that winter counts are valuable if they can be checked against independent data.

1820-1821

(Sign: "Earthlodge" with plume and two arrows projecting from roof.)

The Flame: "Large dirt lodge made by Two-Arrow. The projection at the top extends downward from the left, giving the impression of red and black cloth streamers."

75Mallery, "Pictographs...", 110, pl. xv.

76Mallery, "Picture Writing...", 317. For Lone Dog, see Ibid., 277.

77This post would have been about 30 miles above Chamberlain, South Dakota. (Fort Thompson was about 20 miles above the town.)
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Lone Dog: "The trader, La Conte, gave Two-Arrow a war-dress for his bravery. So translated an interpreter, and the sign shows the two arrows as the warrior's totem; likewise the gable of a house, which brings in the trader; also a long strip of black tipped with red streaming from the roof, which possibly may be the piece of parti-colored material out of which the dress was fashioned. This strip is not intended for sparks and smoke, as at first sight suggested, as the red would in that case be nearest the roof, instead of farthest from it."

The Swan: "A Minneconjou Dakota, named Two Arrows, built himself a dirt medicine lodge. This the interpreter calls, rather inaccurately, a headquarters for dispensing medicine, charms, and nostrums to different bands of Dakotas. The black and red lines above the roof are not united and do not touch the roof."

Consideration of this year (in comparison with 1815-16) shows how distortion of sign interpretation occurs.

Comment: If we look at the sign for 1815-16 given in the three winter counts cited here, we see that the sign is an "earthlodge" with a projection above it. This projection was variously reported by Mallery as a bow or a crow feather. Using Lone Dog's count, MALLERY stated that "The San Arcs [without arrows Tetons] made their first attempt at a dirt lodge... Crow feather was their chief, which fact, in the absence of the other charts, seemed to explain the fairly-drawn feather of that bird protruding from the lodge top, but the figure must now be admitted to be a badly drawn bow, in allusion to the tribe San Arc, without, however, any sign of negation. As the interpreter explained, the figure to be a crow feature, and as Crow Feather actually was the chief, Lone Dog's chart with its interpretation may be independently correct."

This quotation shows quite well how the mnemonic devices were embellished by the count keepers or the interpreters used by the "ethnologists." The projection above the earthlodge in the 1815-6 counts was variously interpreted as a bow, referring the the Sans Arcs, or as a feather, referring to "Chief Crow Feather."

Are we justified then, to conclude that "earthlodge" signs represent earthlodges? The Flame and The Swan both supposedly mentioned earthlodges (the first apparently a dwelling, the second a medicine lodge). Yet statements by the interpreters of the Lone Dog and The Swan's accounts clearly imply some role for white traders. As when dealing with another year, I

78 MALLERY, "Pictographs...", 110-111, pt. xv.

79 Ibid., 109.
believe the 1820-1 count, strongly implies that the "earthlodge" sign was a sign for a large structure. The important question thus is: "Was there invariable systematic differences in the signs used to differentiate 'earthlodges' from other large structures?" Contrary to Mr. Dollar's statements, the answer is no.

Comparison of American Horse's 1818-19 and 1819-20 Signs with The 1819-20 and 1820-1 Signs of The Flame, Lone Dog and The Swan:

1818-19
(Sign: Rectangular: Structure with chimney, window and door)
American Horse: "A large House was built." 80

1819-20
(Sign: Same as above)
American Horse: "Another House was built. The Dakota made medicine in it." 81

Comment: There can be little doubt that 1818-19 and 1819-20 of American Horse correspond to the 1819-20 and 1820-1 of the other three count keepers. Although there are a number of reasons why I argue this, the fact that American Horse and The Swan both referred to making medicine in the house is support enough for the argument. Although the 1818-19 sign of American Horse probably referred to a trader's house, there is no doubt that the "house" referred to by American Horse for 1819-20, using a "log cabin" as a sign, was supposed to be an Indian built structure. Yet Mr. Dollar claimed that the count "...of American Horse, contained no reference to Indian built structures at all but mentioned only traders' houses, which are—not surprisingly—designated by a symbol easily recognized as a log cabin." 82

So much for Mr. Dollar's point d.

(Mr. Dollar's "Consistent" Differences)

If we continue to examine the "consistent" differences between the signs for "earthlodges" and "log houses," we find that Mr. Dollar's statements have been overdrawn. According to Mr. Dollar the "...counts of The Flame, Lone Dog, and The Swan...consistently recorded two different mnemonic devices, one for Indian-built lodges and the other for log structures built by traders [italics added by me]." 83 Nevertheless, in addition to the "log

80Mallery, "Pictographs...", 136, pl. xli.
81Ibid.
82Dollar, this volume. Under 1815-16, American Horse gave a man in a hat within a rectangle as the sign for a white trader.
83Ibid.
house" sign and the "earthlodge" sign, all three of these count keepers recorded a third sign for 1811-12, a circle which was supposed to represent an earthlodge and was clearly different from the standard "earthlodge" sign. So much for Mr. Dollar's points a and b.

Mr. Dollar is correct in point c, that Cloud Shield and Battiste Good did differentiate between white log houses and "earthlodges", however, these two cases are more than counterbalanced by the four cases where there was in fact no consistent distinction of two signs. In three counts, "earthlodges" were represented by two different signs. In the fourth count a "log cabin" was represented both a trading post and the following year a "medicine lodge." Obviously, where such data can be obtained, scholarly interpretation of the meaning of count signs depends on data independent of the counts.

Comparison of the signs and interpretations for the same year offered by Mallery in regard to counts of The Flame, The Swan and Lone Dog, show over and over that different "recollections" of signs or aspects of signs lead to very different statements of sign meaning. Apart from the winter counts themselves there are no sources which indicate that earthlodges were constructed by Dakota west of the Missouri River. I reject the argument that the sign sometimes used to indicate earthlodges (as in the case of the Arikara earthlodges) also, of necessity, referred to earthlodges when applied to the Dakota, just as the sign often used for a white structure did not necessarily refer to a white structure. Interpretation of the winter counts requires widespread comparison of the counts, but, more importantly, it requires detailed formulation of cultural and historic contexts. As I wrote in the "Thurman-Howard" debate: "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" [supposedly "earthlodges" according to Mr. Dollar] in the winter counts (if they were not references to trading posts), can best be interpreted as references to the Sun dance [italics added here]." 85

The problem to be dealt with then is a problem of ethnographic context—is there any evidence to support Mr. Dollar's statements that the Dakota built earthlodges west of the Missouri River?

84 Mallery, "Pictographs..." 107-8, pl. xii.

85 CHSAP, VII, 221. Hence I did not feel it necessary to refer to the fact that for 1828-9 that a trader had a post "in an earthlodge" he built. Mr. Dollar, this volume, footnote 42, falsely implied that I gave a direct quotation and deleted a phrase to twist the data to fit an argument. The truth of the matter is simply that I reject, on the basis of ethnographic and historical winter count contexts, the assumption that winter count references to Dakota "earthlodges" are historical documents providing evidence for the existence of earthlodges among the Dakota. I deal with the 1828-9 "earthlodge" in detail in the text and in the appendix of this paper. So much for Mr. Dollar's point e.
2) The Earthlodge and the "Evolution" of the Dance Hall.

No one will argue with the fact that the Teton Dakotas had a great deal of contact with the tribes of the Missouri who resided in earthlodges, but this is quite a different matter from saying they constructed earthlodge villages. It is only the winter counts that claim that in 1815-6 and 1817-8 that the Sans Arcs built their first earthlodges.  

1815-16

The Flame: "The Sans Arc made the first attempt at a dirt lodge."

Lone Dog: "The Sans Arcs made the first attempt at a dirt lodge."

The Swan: "Sans Arc Dakotas built dirt lodges at Peoria Bottom."  

Battiste Good: "The Sans-Arcs-made-large-houses-winter"  

Cloud Shield: "Some of the Dakotas built a large house and lived in it during the winter."

1816-17

Battiste Good: "Lived-again-in-their-large-houses winter"  

Cloud Shield: "They lived in the same house that they did last winter"

Comment: It is not clear whether there was one or more than one structure built by the Teton Dakotas. If only one was built it puts the matter in an altogether different light, and makes the interpretation of the structure as a Sun dance lodge much more likely.

The next "earthlodge" which Mr. Dollar claimed was constructed by the Teton Dakotas was supposedly built in 1828-9.

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86It is interesting to note that Mr. Dollar accepts my dating of these events, which various winter counts date from 1813-14 to 1819-20 (CHSAP, VII, 218).


88Mallery, "Picture Writing...", 316.

89Mallery, "Pictographs...", 136.

90Mallery, "Picture Writing...", 316.

91Mallery, "Pictographs...", 136.
1828-29

The Flame: "Chadran, a white man builds a house at forks of Cheyenne River. This name should probably be spelled Chadron [sic], with whom Catlin hunted in 1832, in the region mentioned."

Lone Dog: "A white man named Shadran, who lately (as reported in 1877) was still living in the same neighborhood, built a dirt lodge."

The Swan: "Trading post opened in a dirt lodge on the Missouri a little below the mouth of the Little Missouri River."\(^9\)\(^2\)

The career of Francis A. Chardon sheds some light on these winter counts. Sometime after 1825, Chardon was transferred by his St. Louis employer from the Osage country to the upper Missouri. At the consolidation of the Columbia Fur Company with Astor's American Fur Company, Chardon became a member of the Upper Missouri Outfit. Chardon was probably with Laidlaw at the Upper Missouri Outfit post at the forks of the Cheyenne in the winter of 1828-9, trading with the Saone Teton, and he apparently continued to winter there until the winter of 1830-1. In 1832 he was transferred out of the Saone Teton country. He died in 1848.\(^9\)\(^3\)

Wintering in the Missouri drainage by traders, who worked out of a principal post (Fort Tecumseh in Chardon's case) invariably was in a log structure unless the traders were in a village of earthlodes. Utilization of earthlodes for wintering (in earthlodge villages) was very uncommon among the American period traders on the upper and middle Missouri (as we see in the erection of Fort Clark at a Mandan village). Only among the Pawnees of the central Plains was such practice common during the American period. The federal Trade and Intercourse Acts required that the traders be licensed for specific locations. If we look at the list of places granted in the licenses for 1831-2, we find the following two sites, among others, listed: "...mouth of Le Cheyenne river; where the Fire Heart's band commonly procure lodge poles..."\(^9\)\(^4\) Fire Heart was a prominent Saone chief and in 1831 Chardon was among the Saones at the Forks of the Cheyenne River. There was, as shown in the appendix, another Saone post further upstream from Chardon's post. The two branches of the Cheyenne encircle the Black Hills. Fire Heart's band almost certainly obtained their lodgepoles in or adjacent to the Black Hills. What is significant about this identification is that it shows that the Saone's of the period on the Cheyenne River were in fact living in tipis rather than earthlodes. Hence, the winter count statements of 1828-9 almost certainly are erroneous in claiming that Chardon's trading post was in an earthlodge.

\(^9\)\(^2\)Mallery, "Pictographs..." 114.

\(^9\)\(^3\)Annie Heloise Abel (ed.), Chardon's Journal at Fort Clark, 1834-1839 (Pierre, South Dakota, 1932), xix-xxvii, xliii, 228-9, 233.

\(^9\)\(^4\)"Abstract of Licenses issued to persons to trade with the Indians during the year ending 30th Sept., 1832," House Executive Documents, 22 Congress, 2 Session (Vol. 2), U.S. Serial 234, document 104.
Mr. Dollar cannot establish that the 1815-6 and 1816-7 references in the winter count refer to a village of Teton earthlodges; the references might just as well refer to a single "earthlodge" or, as I suggest, a medicine (Sun dance) lodge. Yet he would have us believe that Chardon lived in an earthlodge built for him by the Tetons. Mr. Dollar wrote:

"...Chardon was not known for his construction industriousness, nor is there evidence that he had previously had close contact with earthlodges. That he, a white trader familiar with log cabins would chose instead to build by himself an unfamiliar and more difficult form of housing for his trading post is inconceivable."

Yes, Mr. Dollar it is inconceivable. It would be much more reasonable to see that the sign in the winter count does not refer to an earthlodge, but rather to the usual log trading post. After all, one is not certain where the various bits of information reported in the three counts for 1828-9 came from. Perhaps some came from the count keepers, some from the interpreters and some from Mallery.

If we look at the statements for 1828-9 we find a number of errors. Chardon, as was shown, died in 1848, yet Lone Dog, or the interpreter, or Mallery claimed that "...Shadron...[was] lately (as reported in 1877)...still living in the same neighborhood..." The Swan, or the interpreter, or Mallery, located the post "...a little below the mouth of the Little Missouri..." The Little Missouri is another name for the Bad River. Both Fort Tecumseh and Fort Pierre were near the Mouth of Bad River, and were, respectively, about 2 and 3 miles above the mouth. It would seem likely that the information records under The Swan's count, refers to either the establishment of Fort Pierre in 1832, or the establishment of Fort Tecumseh (which was referred to in the 1822-3 winter count). It is also possible that this is a reference to Papin's or Cerre's post probably established in 1823 south of the Bad River's mouth. But they (Papin and Cerre) were not Chardon and in fact were primarily "opposition" traders.

95Dollar, this volume.

96For the location of Papin's or Cerre's post see Charles E. Deland (abstractor) and Doane Robinson (annotator), "Fort Tecumseh and Fort Pierre Journal and Letter Books," South Dakota Historical Collections (1918), IX, 95. For a sketch of Pierre D. Papin, who formed an "opposition" company in the summer of 1829, see William A. Goff, "Pierre D. Papin," in Leroy R. Hafen (ed.), The Mountain Men and the Fur Trade of the Far West (1972), IX, 304-320.
Mr. Dollar's reference to Catlin's 1832 description of a Sun dance lodge is so straightforward that even Mr. Dollar had to admit that this was built "...for the performance of a Sun Vow ceremony..." Yet he had the gall to term this pole and willow construction "...an atrophied form of the earthlodge..." 97

The last "earthlodges" referred to by Mr. Dollar show his incredible lack of analytical ability. Over and over I have pointed out that the winter counts of The Flame, Lone Dog and The Swan are drawn from the same source. The signs are all virtually the same. Yet Mr. Dollar used Mallery's information for the year 1838-9 to claim that two different bands of Tetons built earthlodges that year, even though this reference certainly provided only two different placements for a single event:

1838-9

The Flame: "Indians built a lodge on White Wood Creek, in the Black Hills and wintered there."

Lone Dog: "A dirt lodge was built for Iron-Horn..."

The Swan: "A Minneconjou chief, named Iron-Horn, built dirt lodge (medicine lodge) on Moreau River (same as Owl River)." 98

Mr. Dollar drew the following conclusions from the data given above under 1838-9:

97 Dollar, this volume. Mr. Dollar, it will be recalled, claimed that my attack on the earthlodge shown in the film was misplaced as the structure was not an earthlodge. As an archaeologist with experience on the Plains, I can see no basis for Mr. Dollar's statement. Clearly there was dirt on top of the lodge. I cannot, however, say I personally ascertained this was dirt, perhaps it was sand, and Mr. Dollar would be correct in calling it a "Sandlodge." This would be appropriate for the school of scholarship to which Mr. Dollar belongs—that school which points out that "Buffalo Bill never killed a buffalo, although he killed many bison." This statement requires a clarification of my unequivocal statement that there is no evidence of Teton Dakota construction of earthlodges west of the Missouri River. The clear intention of my statement was to limit this to the "nomadic" period. I recognize the early dance halls as "earthlodges." These dance halls did not develop until reservation times.

98 Mallery, "Pictographs...", 117.
In 1838, two different Teton bands each constructed their own earthlodge-type houses. One of these, built by the Sans Arcs on White Wood Creek...was 300 meters west of the Missouri River. Another of these "dirt lodges," this one a ceremonial structure, was built by Iron Horn, a Minniconjou Teton chief, along the Moreau River...

In summary, there are no compelling reasons to believe the various "earthlodges" cited by Mr. Dollar west of the Missouri River were anything other than Sun dance lodges or trading posts: the 1815-6 and 1817-8 "earthlodges" were probably Sun dance lodges; the 1828-9 "earthlodge" was almost certainly a log trading house; Catlin's 1832 structure was definitely a Sun dance lodge; the 1838-9 "earthlodge" was probably either a trading post or a Sun dance lodge.

Mr. Dollar not only could not demonstrate the existence of earthlodges among the Teton, he admitted as much:

[In regard to the earthlodges] ... these Teton structures were not earthlodges per se but rather a variant form using logs, poles, and brush, but little if any, earthen wall covering [italics added by me].

Mr. Dollar then went on to give his ideas about the further evolution of these structures:

"Beginning in the early 1840's, a change in style of these Teton-built lodges can be detected...Long poles covered with brush, rather than logs, became the main building materials...."

Later these long pole and brush structures developed into "squaw coolers," while the old form of structure was revived. There are several points to observe, Catlin's description of the Teton Sun dance lodge, for example, dates from 1832 and is of the kind of construction Mr. Dollar claimed began "in the early 1840's."

The only way Mr. Dollar could "establish" the builders as San Arcs was to infer that since The Flame, who was probably a Two Kettles Dakota, lived with the Sans Arcs, the Sans Arcs must have built the "earthlodge." This conclusion, in light of the common origin of all three counts, is certainly one of Mr. Dollar's most surprising statements.

100 Dollar, this volume.
101 Ibid.
102 Ibid.
103 Ibid.
A second point of interest in regard to Mr. Dollar's sequence is that the development of the "revived" form of earthlodge (the dance hall) has previously been outlined by other workers. Hamilton and Hamilton wrote that "the earlier dance houses were of logs covered with dirt, then of logs alone, and finally of lumber."\textsuperscript{104} Hence, if one were to believe Mr. Dollar, the Teton Dakotas built earth-covered structures which ultimately developed into Sun dance lodges as the structures became less strongly built and lost their dirt covering. After moving onto the reservations the Teton Dakota again began the construction of dirt covered lodges, but gradually the dirt covering was lost and the structures were ultimately built purely of boards.

Mr. Dollar criticized Dr. Howard for arguing that the Teton Dakota dance house came into being with the spread of the Grass dance.\textsuperscript{105} On the basis of the evidence at hand, although there are several ethnohistoric problems unsolved, Dr. Howard's interpretation, which is probably the majority opinion, is certainly more satisfying than the far-fetched interpretation of Mr. Dollar.

3) Indian Headbands.

After pointing out that virtually every statement in the paper by Mr. Dollar is either misleading or simply wrong, it is a pleasure to be able to point to a contribution by Mr. Dollar. If Mr. Dollar's observation about director Ince's Indian actors taking the headband trait back to the reservation with them about 1916 is original to Mr. Dollar,\textsuperscript{106} I offer Mr. Dollar my compliments on carrying out an original piece of research.

Now that the various errors of Mr. Dollar have been exposed, some of them for the second or third time, a summation of the Horse film would seem to be in order. A paraphrase of the comments of Richard Harris, the star of this film (and the forthcoming sequel), is very appropriate. Richard Harris stated that:

We wrote a scene at the end [of the sequel] in which 'he's old and he dies.' Harris says of his character, 'One Horse is all right, but two is enough'.\textsuperscript{107}

It would have been more appropriate, from the point of view of those seriously concerned with Plains ethnohistory, to say that "one horse film is more than enough."


\textsuperscript{105}Dollar, this volume.

\textsuperscript{106}Dollar, this volume.

\textsuperscript{107}Time, February 9, 1976, 48.
APPENDIX

Trading Posts on the Cheyenne River, 1828-1832

There is a vast specialized literature in the fur trade which, unfortunately, to a large degree has been ignored by those anthropologists, ethnologists and historians who have been primarily concerned with Plains Indians. If we are to understand the early development of Plains Indian culture, those concerned with Plains Indians must master this fur trade literature as well as the literature "directly" concerned with Plains Indians. Mr. Dollar's insistence on arguing that Chardon traded out of an earthlodge is a glaring example of the failure to consider the fur trade literature.

No authority on the fur trade has ever accepted the winter count statements (The Flame, Lone Dog, The Swan) that Chardon's 1828-9 trade was carried on in an earthlodge. All authorities have taken Chardon's post to be a typical wintering post, a log post dependent on a principal post (in Chardon's case Ft. Tecumseh, which was later replaced by Ft. Pierre). The traders left the principal post in September for their wintering ground and returned to the principal post in April.

Chittenden, in the classic history of the western fur trade, wrote:

Scattered throughout the Sioux country on both sides of the Missouri there were many subordinate posts or houses of the American Fur company dependent upon Fort Pierre. There were no fewer than three in the valley of James river (Rivère à Jacques). There was one at the forks of the Cheyenne, another at its mouth, one at the Aricara villages and others on Cherry, White and Niobara rivers, and among the Brulé, Ogallala and other bands of the Sioux. In fact, wherever there was an inducement to trade these temporary houses were erected. [italics added by me].

The knowledgeable Doane Robinson wrote of the Cheyenne River posts:

Trade of the Cheyenne [River:] At least two winter posts were maintained by the American Fur Company upon the Cheyenne river. One, at the mouth of Cherry Creek, where the sub-agency now is, and the second at the Forks. It is probable that there was another still higher up on the south fork. The post at Little Bend, near

108 Some general comments on this can be found in Melburn D. Thurman, "Review of Joshua Pilcher, Fur Trader and Indian Agent by John E. Sunder", American Anthropologist, 72 #2 (April 1970), 411. An example of the integration of "Indian ethnography" and the fur trade literature is found in my "the Skidi-Pawnee Morning Star Sacrifice of 1827", Nebraska History 51 #3 (1970), 269-280.

the mouth of the Cheyenne, was also sometimes spoken of as a Cheyenne post. The valley of the Cheyenne contributed a vast deal of fur to the Tecumseh outfit annually. 110

The Cheyenne River Posts, from the Cherry Creek westward, were primarily Saone Teton111 posts. Abel’s claim that Laidlaw traded with the Yanctonais while Chardon traded with the Saones at the post at the forks of the Cheyenne112 seems somewhat doubtful.

The details of the time of occupation of the Saone posts have not been worked out. It is generally accepted that the post at the forks of the Cheyenne was established by 1828.113 It is not certain if the post at the forks of the Cheyenne was maintained after Chardon’s last wintering there in 1830-1.114 As both Laidlaw and Chardon were mentioned at the forks fort in 1830 and 1831, and as the forks fort was not mentioned in the Fort Tecumseh journal for 1832, while both Laidlaw and Laboue were mentioned at the Saone post on Cherry Creek in 1832, it seems likely that the forks fort was abandoned after the winter of 1830-1 and replaced by the fort on Cherry Creek. During the period 1830 through 1832, the other Saone post (for the Oglalas), beyond the forks of the Cheyenne was maintained. It is not certain when this post was built (but 1828 seems the most likely date) nor if it was maintained after the explosion in 1832, which killed the trader Sarpy and destroyed the store.115

110Deland (abstractor) and Robinson (annotator) "Fort Tecumseh and Fort Pierre Journal and Letter Books", 93, n.3. That Robinson had consulted the winter counts is demonstrated by his footnote 137 on page 154.

Although Robinson stated that the post at the mouth of the Cheyenne was sometime called a Cheyenne River post, as far as I have been able to determine, only the Cherry Creek post, Forks of the Cheyenne post, and Oglala post beyond the Forks were ever referred to as Saone posts or "the Sawons" by the American Fur Company.

111For a discussion of the early Saone group see Harry Anderson, "An Investigation of the Early Bands of the Saone Group of Teton Sioux", Journal of the Washington [D.C.] Academy of Sciences, XLVI (1956), 87-94. This fine paper points the way for similar research among the vast array of chief and band names found in the early material on all Plains tribes. Plains ethnologists would do well to closely study this paper.

112Abel (ed.), "Chardon's Journal...", XXIV.

113Ibid., 228, n. 83.

114Ibid., xxvii.

115Documentation for the statements above will be given below in the excerpts from the Ft. Tecumseh journal.
As no printed sources provide adequate information on the personnel involved with the Saone posts, I will summarize the personnel of these posts. Sixteen voyageurs were listed as being stationed at "Saouns." These were numbers 117, 122, 129, 130, 132, 133, 140, 141, 150, 151, 158, 163, 164, 169, 215, and 225 on the 1830 list. A single voyageur (111) was listed at "Cheyenne Post." The following personnel were listed in addition to the voyageurs:

<table>
<thead>
<tr>
<th>Number</th>
<th>Name</th>
<th>Position</th>
<th>Location</th>
</tr>
</thead>
<tbody>
<tr>
<td>240</td>
<td>Joseph Lacompte</td>
<td>Clerk and interpreter</td>
<td>Saouns</td>
</tr>
<tr>
<td>248</td>
<td>Francis A. Chardon</td>
<td>Clerk and trader</td>
<td>Saouns</td>
</tr>
<tr>
<td>253</td>
<td>Thomas L. Sarpy</td>
<td>Clerk and trader</td>
<td>Ogallallas</td>
</tr>
<tr>
<td>258</td>
<td>Baptiste Daurion</td>
<td>Interpreter</td>
<td>Saouns</td>
</tr>
<tr>
<td>274</td>
<td>Louis Menard</td>
<td>Clerk and interpreter</td>
<td>Saouns</td>
</tr>
<tr>
<td>275</td>
<td>Patrice Dauphin</td>
<td>Clerk and interpreter</td>
<td>Saouns</td>
</tr>
</tbody>
</table>

It is not known why one voyageur and Sarpy should be distinguished from all others at Saone posts by locations found nowhere else in the 1830 list.

The personnel roster suggests that the Saone posts were not nearly as small as one might first suppose. The two Saone posts had a complement of 17 voyageurs and 6 clerks and interpreters, for a total of 23 men. Further, as shown below (under date of March 16, 1830; see also February 10, 1831), some of the men at these posts certainly had families with them. The number of people involved, coupled with the need to store equipment, trade goods, and furs (almost 5000 buffalo robes were obtained by Chardon in the 1829-30 trade) make it a virtual certainty that Chardon's post at the Forks of the Cheyenne was composed of more than a single cabin.

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116 DeLand listed only one employee for the "Cheyenne Post" and one employee for the "Saouns" for 1830. For 1831, DeLand gave a roster of only three employees for the Saoons. (DeLand, "Fort Tecumseh and Fort Pierre Journal and Letter Book", 234-5.) That Deland's information is very incomplete has long been recognized. Abel gave an extract from the complete roster of the Upper Missouri Outfit for 1830 which included two names for Sawons (Abel, ed., "Chardon's Journal...," 227-8.)

117 This is based on the complete list which was extracted by Abel, "Persons Employed for the Upper Missouri Outfit for the Year 1830", in the P. Chouteau-Moffett Collection of the Missouri Historical Society, St. Louis. This list, among other documents, was microfilmed for me in December, 1970, through the kindness of Frances Stadler, archivist.


119 For a biographical sketch of Sarpy, see George M. Platt, "Thomas L. Sarpy", in Hafen (ed.) "The Mountain Men..." (1965), III, 279-283.
EXCERPTS FROM THE FORT TECUMSEH JOURNAL
(January 31, 1830 - April 6, 1831; January 27, 1832 - May 30, 1833)

Forks of the Cheyenne Post

February 14, 1830  "Joseph Bary and an Indian left here for the forks of the Cheyenne River with a mule loaded with merchandise." 120

February 16, 1830  "Francis Quevel and Louison, an Indian arrived from the forks of the Cheyenne with 9 horses." 121

February 21, 1830  "Three men left here for the forks of the Cheyenne River"

February 25, 1830  "At 4 p.m. Bary and Toin with two men and 21 horses and mules arrived from the forks of Cheyenne River." 122

March 1, 1830  "...Baptiste, Dourian left here for the forks of the Cheyenne River" 123

March 16, 1830  "Jos. Juett and family arrived from forks of Cheyenne River." 124

April 9, 1830  "F.A. Chardon arrived from forks of Cheyenne with seven skin canoes laden with 4,360 buffalo robes and a quantity of fur merchandise etc....Mr. Chardon unfortunately lost a canoe with 400 robes in descending the Cheyenne river." 125

Oglala Post

April 22, 1830  "Three men left here in search of Mr. Thomas Sarpy who we presume is descending the Cheyenne in skin canoes and in want of assistance, as it is now a long time since we suppose he must have left his wintering grounds." 126

121 Ibid., 97.
123 Ibid., 99.
124 Ibid., 101.
125 Ibid., 110.
126 Ibid., 112-3.
May 6, 1830  "At 4 pm the man whom we sent in quest of Mr. Sarpy on the 22 of last month, returned without any intelligence of him. They followed the river Cheyenne, as far as Mr. Chardon wintering ground when they found themselves destitute of provisions and nearly barefooted, and consequently returned. We now think that both himself and those who were with him have been killed by some war party." 127

May 8, 1830  "Louis Piton arrived from the Cheyenne River where he left Mr. T.L. Sarpy with his peltries. He has unfortunately lost a skin canoe loaded with robes. It is now about two months since he left his wintering ground, during which time the weather has been so unfavorable that he has not made more than 60 miles in two months. His canoes were rotten, and he has sent in Piton for a supply of horses to bring his returns." 128

May 9, 1830  "At noon Piton, Dickson, Degrey and Lachapele with one man left here with 52 horses, mules and Jackasses to bring in the remainder of Sarpy's packs." 129

May 21, 1830  "At 10 am Mr. T.L. Sarpy and party arrived with 50 odd horses loaded with 108 packs of buffalo robes, a little beaver, merchandise, tallow etc." 130

Saone Outfit

September 16, 1830  "Put (up) an equipment of goods for the Saons and Cheyennes." 131

September 17, 1830  "Chardon, Durion and Gonpieras left here for the Sauons camp with six horses loaded with merchandise." 132

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127Ibid., 115. This shows that Sarpy's post was beyond the Forks of the Cheyenne post.

128Ibid.

129Ibid., 117.

130Ibid., 119.


132Ibid.
September 19, 1830  "We are at present very anxious for party who left here on 8 uito. for the Sauons Camp; one of Mr. Cerre's men arrived day before yesterday and he says LaChapelle left the camp on the return three days before him, he had a good guide with him, therefore we do not think he could have got lost. The present presumption is that he has been killed by some war party."133

September 22, 1830  "Primeau met LaChapelle and party at the mouth of the Cheyenne River, descending in a skin canoe with 300 pounds of dry meat."134

September 24, 1830  "Lachapelle and Vasseau arrived from the Sioux camp on Cheyenne river in a skin canoe laden with dry meat."135

October 6, 1830  "Put up two equipments of goods, one for Sawons and Cheyennes and one for Ogallallas."136

October 9, 1830  "Sent off goods for the Ogallallas, Sauons, Cheyennes Outfits."137

October 10, 1830  "F.A. Chardon left here with three horses laden with merchandise for the Sawon and Cheyenne outfits."138

October 11, 1830  "Sent off a cart for Sawon and Cheyenne outfits with Mr. Laidlaw's property."139

October 15, 1830  "Mr. Laidlaw started early this morning for Forks of Cheyenne river."140

October 29, 1830  "Mr. Laidlaw arrived from the Sawons."141

November 7, 1830  "Mr. Laidlaw left here for his establishment on the Cheyenne."142

133 Ibid.
134 Ibid., 137.
135 Ibid.
136 Ibid., 138.
137 Ibid., 139.
138 Ibid.
139 Ibid. This use of carts was probably referred to in American Horse's winter count for 1830-1 (Mallery, "Pictographs...", 138, pl XLII).
140 Ibid.
141 Ibid., 141.
142 Ibid.
Fork of Cheyenne Post

February 10, 1831  "Mr. and Mrs. Gordon left here for Mr. Laidlaw's establishment on the Grand Cheyenne river."143

March 25, 1831  "Mr. Laidlaw arrived from forks of Cheyenne river. He left Gordon and other man behind with horses and mules."144

March 26, 1831  "Mr. Gordon arrived and in the evening the remainder of the party with 2 or 3 horses and mules."145

April 6, 1831:  "Joseph Vassure arrived from the forks of Cheyenne river...He states that Mr. Chardon left there on the 1st inst. with 11 skin canoes containing 44 packs of robes."146

Oglala

January 27, 1832  "James Parker, Pineau le Yancton and Louison Brule arrived from Ogallallahs post with the melancholy news of the death of Mr. Thomas L. Sarpy, the Cos trader at that station. (Particulars of blowing up of the pieces of powder &c.)...Mr. S. was one of the Cos most useful clerks, his loss will be felt and much regretted by his employers. The other men are much injured, but are now considered out of danger."147

April 3, 1832  "Last evening J. Jouett arrived from Ogallallahs post."148

April 9, 1832  See this date under Cherry River post.

143Ibid., 146.
144Ibid., 149.
145Ibid.
146Ibid., 150.
147Ibid., 150. The details of the incident were given in a letter of April 6, 1831 of Jacob Halsey to Pierre Chouteau. This letter was printed by Platt, "Thomas L. Sarpy", 281-2. The explosion occurred in the store, which was blown up.
148Ibid., 154. The explosion was probably referred to in American Horse's 1831-32 wintercount (Mallery, "Pictographs...", 138, pl. XLII).
Cherry River [Creek] Post

February 9, 1832  "Baptiste Defonde and Baptiste Gailleau arrived from the Sawon post."\textsuperscript{149}

February 14, 1832  "One of our men arrived from Roy's Island. He says that four of Leclerc's men are coming up the river with two sleighs loaded with goods. They are bound for the Sawon's post on Cherry river. In the evening Mr. Laidlaw and an Indian arrived from the Sauon post."\textsuperscript{150}

April 9, 1832  "Five skin canoes loaded with buffalo robes in charge of Colin Campbell arrived from the Ogallallahs post on Cheyenne river. They bring news of the murder of Francais Quenel by Frederich Laboue the company's trader at Cherry river. Laboue arrived in a canoe."\textsuperscript{151}

May 7, 1832  "Colin Campbell...arrived from Cherry river. Mr. Campbell while at Cherry river disinterred the body of the deceased F. Quenel and as 7 wounds were found on the body Frederic Laboue was put in irons immediately on the arrival of the canoe."\textsuperscript{152}

\textsuperscript{149}DeLand and Robinson, "Fort Tecumseh and Fort Pierre Journals and Letter Book", 151.

\textsuperscript{150}\textit{Ibid.}

\textsuperscript{151}\textit{Ibid.}, 154.

\textsuperscript{152}\textit{Ibid.}, 155. This murder was recorded in the 1831-2 winter counts of The Flame, Lone Dog, and Swan (Mallery, "Pictographs...", 115, pl. XIX). This was probably also the incident referred to in Cloud Shield's winter count (Mallery, "Pictographs...", 138, pl. XLII).
DOLLAR'S REPLY

Clyde D. Dollar

In this latest, Thurman attempts to drown the reader in a flood of superfluous information and extraneous diversions, hoping, no doubt, to dazzle the uncritical and cow the rest. I am impressed only with how long it took him (45+ pages) to say so little.

The reader will recall that, in his initial attack on the accuracy of the film, A MAN CALLED HORSE, Thurman:

1. mistook an early nineteenth century English coverlet for a Navaho blanket;
2. chided the film for depicting a horse corral when actually such features were a part of High Plains Indian life of the period;
3. 'overlooked' a key phrase in one of his quotes, on the omission of which he constructed an elaborate theory as to meanings in winter counts;
4. placed great dependence on the perception and correctness of George Hyde, an author whose physical handicaps from youth prevented him from being either very perceptive or very correct;
5. unequivocally declared that no sources existed for Teton Sioux log structures built west of the Missouri River, when in fact six such structures in that area are recorded, three of which appeared in the very sources Thurman claimed to have at hand when he made his statement.

Now, adding to this somewhat unglittering track record, Thurman:

1. self-fabricates an assumption assigning me a role in a forthcoming motion picture I have never had;

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1 For a discussion of this, and the following points, see my first reply, "Brayings about HORSE; the Thurman–Howard Debate on Filmed Ethnohistory," Conference on Historic Site Archaeology Papers, this volume.

2 Thurman's crediting me with involvement in the sequel to HORSE is a fabrication on his part. I have had no contact with that production, and neither has the director of the original HORSE, Elliot Silverstein, nor any of the other major production department heads of that first film to my knowledge. [Whether my absence from this second film will improve its accuracy, as Thurman suggests in a following rejoinder to this reply, is a matter to be seen.]
2. continues to see Navaho blankets (now as saddle blankets) in a film that had neither Navaho nor saddle blankets;³

3. accuses me of identifying the log structures built by the various bands of Teton Sioux as "earthlodges," even though one entire paragraph in my first paper presented evidence that these very likely were not structures of that type;⁴

4. insists on seeing an "earthlodge" in the film when the structure shown obviously was no such thing;⁵

5. both mislabels and mislocates (by about 350 miles) the Little Missouri River, thereby betraying a certain lack of High Plains geographical and ethnohistorical knowledge;⁶

³ Only two blankets appeared in the film. One of these was the English coverlet already discussed in my critique of Thurman's first criticism. This item was shown in a number of places during the unfolding of the film's plot. The other, a solid mustard-colored blanket (hardly Navaho), can be seen at a recognizable distance during only one scene. Furthermore, NO horse blankets, Indian or white, are to be found in the film. For those interested in checking this, a copy of the film can be obtained from either commercial sources or through a university audio-visual facility (it is available in 16mm).

⁴ Thurman insists on this point at least five times, each time in a depreciating manner. As I very clearly made an opposite point in my original paper, I can only assume that Thurman either misread that paper, does not know what an earthlodge looks like, or else deliberately misrepresented my statements.

⁵ Having been the person responsible for providing the architectural information for this set, and having monitored their construction (actually two of these structures -- exact duplicates -- were built; only one is visible at any given time in the film), and having worked in and around them both for more than four months, I can state with some positiveness that they were NOT earthlodges, no matter what Thurman thinks he saw in the film. Their walls were of exposed logs, and their roofs of timber covered with branches, not earth as insisted on by Thurman. Perhaps he did not notice the roof of this structure catch fire during one of the film's scenes.

⁶ Thurman stated that the "Little Missouri was another name for the Bad River" (Thurman, this volume), repeating the error made by Mallery (Mallery, "Pictographs," p. 111). However, as of Friday, April 12, 1805, the stream emptying into the Missouri River in the northeast corner of Dunn County, North Dakota (opposite the future site of Fort Berthold) has borne that name (Original Journals of Lewis & Clark, Thwaites edition, Vol. I, pp. 296-297). That stream is about 350 miles north of the river so called by Thurman. Apparently he is not aware of the several early historic name changes of the various rivers and streams in that area.
6. continues to trumpet George Hyde's work as "the most significant body of ethnohistorical literature for the plains yet authored by one man."\(^7\)

7. uses a very confused thirty-three year long personal reminiscence written in 1882,\(^8\) an oral statement obtained in the early 1940s,\(^9\) and a secondary reference to the Paraguayan Indians (not even located on the North American continent) to 'prove' that the Teton Dakota of the 1820s 'always' mounted on the right side of their horses;\(^10\)

8. presents James Mooney's 1895 description of a Kiowa (southern Plains) antelope pen taken from an 1860-61 winter count (not a reliable source, according to Thurman) as rebuttle for William Clark's 1805 eye-witness account of an Assiniboine (High Plains) log pen;\(^11\)

\(^7\) Thurman, this volume. Having lived in the Rosebud, Mission, and St. Francis, South Dakota, areas of the Rosebud Sioux Reservation for almost three continuous years, and having on-the-spot compared sections of Hyde's history of the Rosebud Sioux (Spotted Tail and His Folk, University of Nebraska, Lincoln, 1961) with the land as it actually is, I am convinced of Hyde's unreliability in both perception of the land and the events that transpired there. For example, Hyde's relation of the murder of Spotted Tail is a grotesque distortion of both the place and happening; there are others equally as distorted. It might be of some amusement to learn if, in his enthusiasm for Hyde, Thurman subscribes to the hypotheses in that author's Indians of the High Plains (University of Oklahoma, Norman, 1959).

\(^8\) Apparently Thurman knows so little about the various groups of middle and High Plains Indians that he cannot recognize Dodge's thoroughly confused tale of his life in the west. Incidentally, contrary to the impression Thurman created, Dodge's earliest experiences were among the Southern Comanche and Apaches, not the Sioux.

\(^9\) Thurman mangles Ewers' data. In reality, Ewers compared W.B. Parker's account of a southern Comanche woman's mounting customs with those described to him (Ewers) by a Blackfoot woman in the 1940s (this gives me an opportunity to correct one of Thurman's citations: see Ewers, Horse in Blackfoot Indian Culture, page 68, and notes 37 and 38). In 1970, I discussed this very point with Ewers. His answer cast quite a different light on the matter than what Thurman has.

\(^10\) All this even in the face of a large body of negative primary evidence to the contrary!

\(^11\) And in the process, cleverly (and I can only assume deliberately) substituting the account of Lewis (who did not see the feature) for the description of Clark (who did encounter the log pen). Clark's description should be consulted, with particular attention being paid to the topographical placement of the feature, for verification of my original statement.
9. and finally, mounts a blustery attack on a secretary's typographical errors in a draft manuscript.\textsuperscript{12}

Thurman's blunders, as enumerated above, are trivial, but by their frequency and type nevertheless form a pattern for judging his perception and knowledge. More of these can be detected in his other efforts at further argumentation.

For example, his discussion of Maximilian's German is irrelevant, in that the proof of my point about the use of horse corrals by the Blackfoot is contained, not in the phrase over which Thurman expended so much mental effort, but in the following ones, and Thurman's translation of these agrees with the wording of the 1904 Thwaites edition, which was the basis for my point in the first place. Furthermore, his involved exposition of Chardon is pointless. Whatever may have been the circumstances surrounding the equivocation of that colorful character's seemingly post-mortem location,\textsuperscript{13} such would not have prevented him from being where sources placed him twenty years before his known date of death. For Thurman to sweep aside the entire validity of a source based solely on the confusion in his own mind about Chardon hints at desperation.

But it was in Thurman's involved discourses on the winter counts that the smog level reached the gas mask stage. Never, in a professional publication, have I read such a convoluted, misconstrued, and patently inaccurate group of sentences.

First, he reiterates the 'correctness' of certain of his original statements (which I had already shown through use of data, not just differences in interpretation, are wholly incorrect),\textsuperscript{14} and then builds further hypotheses on those statements. Next he dumps a mass of information on the reader, and in the unloading of this slips in a number of dogmatic statements that are

\textsuperscript{12}The words "nomethetic" and "ideographic" resulted from a secretary's typographical errors and appeared only in the draft manuscript. I assume that the editor of this journal has already exercised his good judgement and corrected not only the several such typos in my own manuscript but also those in Thurman's. Let me add that Thurman was specifically requested not to quote from the manuscript without first obtaining the author's corrections, a professional courtesy usually adhered to by all but the most discourteous and unprofessional. [Editor's note: These misspelled words were not corrected on the errata sheet sent out by Dollar. Because of the nature of this exchange, the words, once pointed out by Thurman, were left as submitted by Dollar.]

\textsuperscript{13}Had Thurman recognized where Chardon's Little Missouri River was located, there would have been little problem in sorting out the data and resolving the matter.

\textsuperscript{14}Thurman claims that he "showed in one paragraph that a single sign in a single winter count is used in three different ways" (underlining his). That statement simply is not correct, and I invite the interested reader to check the data as presented in my original paper, this volume.
either blatant misconstructions or else deliberate fabrications. Then, he questions the accuracy of the winter counts, states that "scholarly interpretations of the meaning of count signs depends on data independent of the counts," and then proceeds to reinterpret them, not on data, but solely and entirely on the basis of what he thinks the count keeps should have meant -- in order to prove his own hypothesis! And throughout all this are his misrepresentations of my statements.

However, for the utmost in convolutions and irrationality, I point to his lengthy paragraph which begins "Wintering in the Missouri drainage" and closing with "...Chardon's trading post was in an earthlodge." Herein are strung together an array of unrelated information and astonishingly erroneous statements, at the end of which Thurman brings about a conclusion created only by his deft use of selective perception.

The interested reader should compare Mallery's "Pictographs," pp.107-108. Plate XII, year 1811-12, against the claims made by Thurman: "all three of these count keepers recorded a third sign for 1811-12, a circle which was supposed to represent an earthlodge and was clearly different from the standard 'earthlodge' sign" (Thurman, this volume). I also call into question Thurman's several statements in the next paragraph, beginning with "...however, these two cases..." and closing with "...the following year a 'medicine lodge'." After a thorough search through his references, I failed to find any evidence for these statements, let alone their correctness.

For example, Thurman, without taking the slightest notice of band affiliations, or indeed his much touted 'cultural space,' lumps together the information in the winter counts of The Flame (Sans Arc), Lone Dog (Yanktonai), and The Swan (Minneconjou), and claims that all three of their signs for the year 1838-39 refers to the building of the same structure. So intent is he in superimposing his hypothesis on the data that he neglects to notice that [sic] of them do refer to the same structure, but that that structure could not have been located any closer than some seventy miles from the other, and probably a good distance further!

For instance, Thurman states that "Mr. Dollar cannot establish that the 1815-16 and 1816-17 references in the winter count refer to a village of Teton earthlodges." I made no such claim that the Tetons built 'earthlodge villages'!!! Further, Thurman repeatedly mentions "the 'earthlodges' referred to by Mr. Dollar" in relation to Sioux architecture. Again, I made no such references or statement. Either the man cannot read or he is deliberately misrepresenting my statements.

Of the eleven sentences in this paragraph, eight are either erroneous, internally contradictory, or ambiguous.

The last three sentences in his paragraph constitute a true masterpiece of selective perception. In order to fully appreciate the magnitude of Thurman's deception in these sentences, the reader should first refresh the memory on who the Saone were. I would suggest a quick reference to Frederick Webb Hodge's Handbook of the American Indians North of Mexico, New York, 1965, Vol. II, p. 464, paragraph on "Saone."
Rounding out this act, Thurman adds a thoroughly pointless appendix. For several pages, the reader is treated to a parade of obscure names, unexplained events, and irrelevant discussion. One of my students identified this section -- I think correctly -- as a snow job.

All in all, Thurman's presentation is a caricature of High Plains history, liberally sprinkled with clever manipulations and even misrepresentations, and in it he simply goes beyond credibility. Let those so uncritical as to believe his fantasies do so. I, for one, reject these totally, and in the future will have nothing further to do either with the man or his monumental facades of educated idiocy.
Mr. Dollar, like the Phoenix constantly reborn from ashes, keeps coming back, but to no purpose. Mr. Dollar, who was concerned with the fictional film, "A Man Called Horse," has apparently been unable to move away from the genre of fiction. This "work" of his would be hilarious were it not so sad that he attempts to do scholarship; it is even sadder that there may be a few naive people who cannot see through his charade. I am pleased that Mr. Dollar will have nothing further to do with me hereafter. It would be very beneficial to science had he promised to have nothing further to do with scholarship.

I am indeed sorry that I erroneously assigned Mr. Dollar a role in the sequel to the original "Horse" film, but Mr. Dollar gave me that impression at Gainesville. Without Mr. Dollar's advice, it would seem that there might be a fighting chance qf authenticity in the sequel.

As far as Mr. Dollar's other remarks, Note 12 of "Dollar's Reply" is such a blatant fabrication that I invite interested parties to write the editor, Stanley South, to determine if my quotations are from the author's "corrected copy." To those who recall the context of my quotation from Adlai Stevenson in the debate with Dr. Howard, I should like to add that the same philosophy applies now, but in my opinion there is no hope for Mr. Dollar. Whether or not there is a Navajo blanket in the film, my remarks on the blanket served to introduce a major point which cannot be invalidated, even if there was no blanket in the film. The other points reiterated by Mr. Dollar (and largely answered in my previous comments on the papers by Dr. Howard and Mr. Dollar) are so wrong-headed I will not again reply. I strongly suggest that interested parties read our respective papers with the publications referred to in our respective citations immediately at hand. It is only by such a procedure that one can appreciate the inventiveness of Mr. Dollar's mind. Mr. Dollar is truly one of the most creative writers of fiction of this generation.

No better final statement about Mr. Dollar's creativeness can be found than a quotation from Conrad Hilton's autobiography, Be My Guest (Chapter 3), where Hilton wrote the following:

One thing I know. I personally have been able to do business with some pretty rough characters; but I have never been able to deal with a liar. It is, as my cadet friend at Roswell would have put it, like shadow boxing. It isn't worth the effort. You can't win.

For shadow boxing, one might equally well read "wrestling with molasses."

This ends the debate with Mr. Dollar, but I believe long-time readers of this journal might find a little piece of fiction of some interest.
A Fable

Once upon a time there was a duck who could only say "quack, quack." The duck decided he wanted to be a scholar so he bought a shovel and excavated some house foundations in the provinces. He was being paid by the aborigines of the provinces, but that was demeaning; he wanted to be a theorist. So the duck wrote an hilariously naive paper which a number of prominent scholars, out of friendship for the journal editor, commented upon. The duck was crucified. With his feathers ruffled, the duck wrote replies. Unfortunately, all the duck could say about these eminent scholars was "quack, quack." But, deep down, the duck realized that he was no archaeologist.

Years went by. The duck decided he wanted to be a scholar again. This time the duck thought he would try being an historian. He wrote some naive papers which ostensibly were critiques of various scholars' work, but all the duck could say was "quack, quack." Again the duck was crucified.

Unfortunately, the story is not ended. In a few years the duck will come back. I wonder what his field will be.

Reflections on a Nightmare*

Last night I saw A Man Called Horse again; this time on television. I made a list of a number of erroneous points that neither Howard nor I had previously mentioned, but there is no need to beat a dead Horse. There is, however, a need to reconsider certain of the picayune points (Navajo blankets and roofing of earthlodges) which my opponent chose to dwell on rather than discuss substantial issues.

Further viewing of the film showed how truly laughable is its treatment of logistic networks. For example, the Tetons of the film had never seen a white man and did not have guns, and although they had almost everything else carried by traders, they had apparently never inquired into the source of these things. In the film, women cut off fingers with metal knives, trade blankets and metal pots were offered in exchange for brides, and so on. The Navajo blanket was introduced as an example of the false picture of trade given in the film. And contrary to Dollar's assertions, there were many blankets in the film, mostly grey blankets covering saddles on the horses ridden by the Shoshone raiders. In addition to a yellow blanket taken from "Horse's" tent by a raider, and another yellow blanket offered to Yellowhand for his sister, there was a red, black, and white striped blanket taken from a horse which was used to cover the naked "Horse," which was later covered by Dollar's famous English coverlet. On the other hand, the roof of the earthlodge (it was certainly supposed to be an earthlodge as the "Sun Vow" of the film was a tableau from Catlin, who had painted the scene within an earthlodge) was, as Dollar said, covered with branches, not dirt or sand. No one ever replied to my question, "Who is Lloyd One Star?," the man who apparently supplied so much data contrary to that in the ethnographic literature. Rather than being an old man, Mr. One Star played one of the warriors.

* [Editor's note: This was received immediately before going to press.]