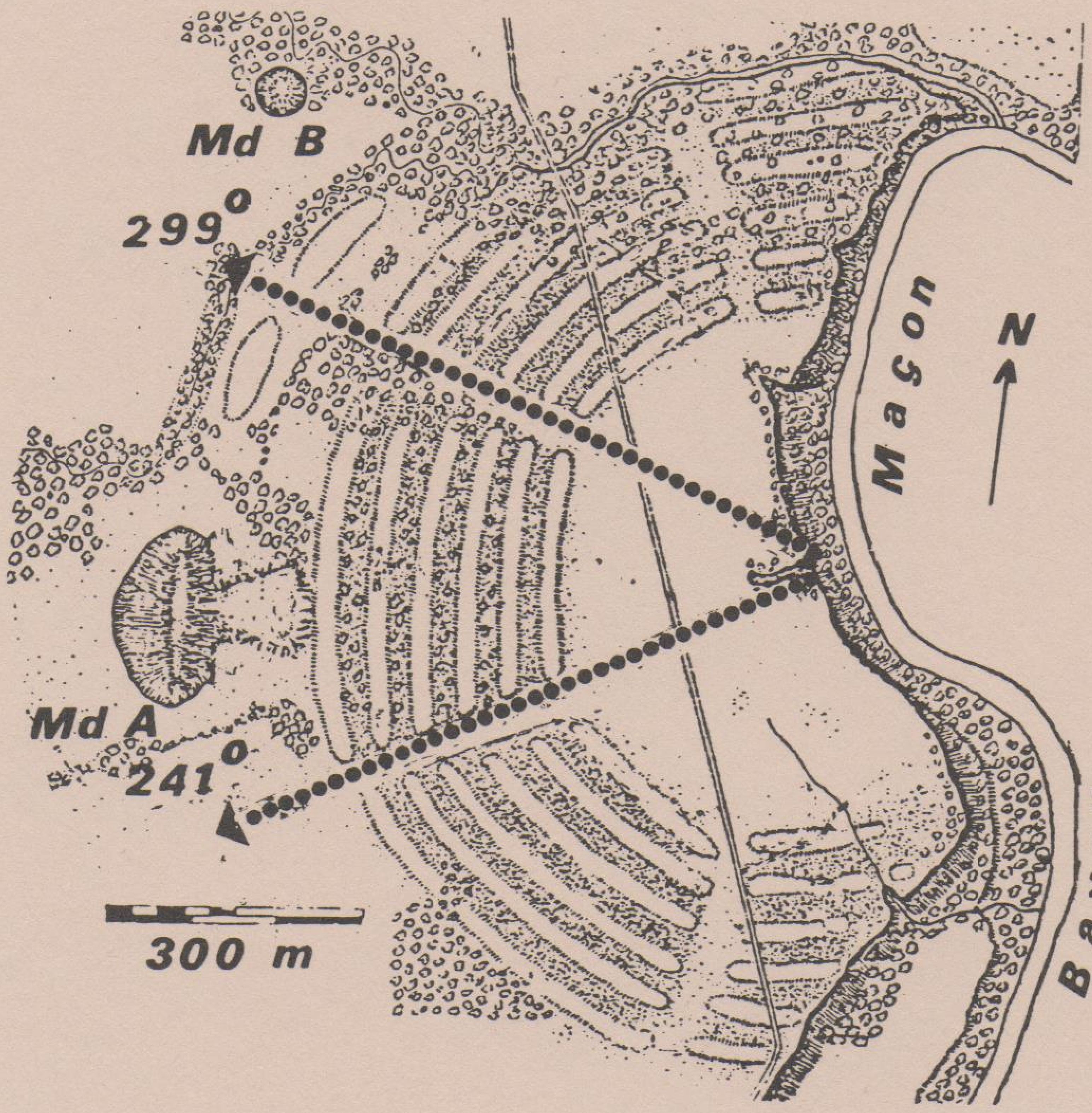
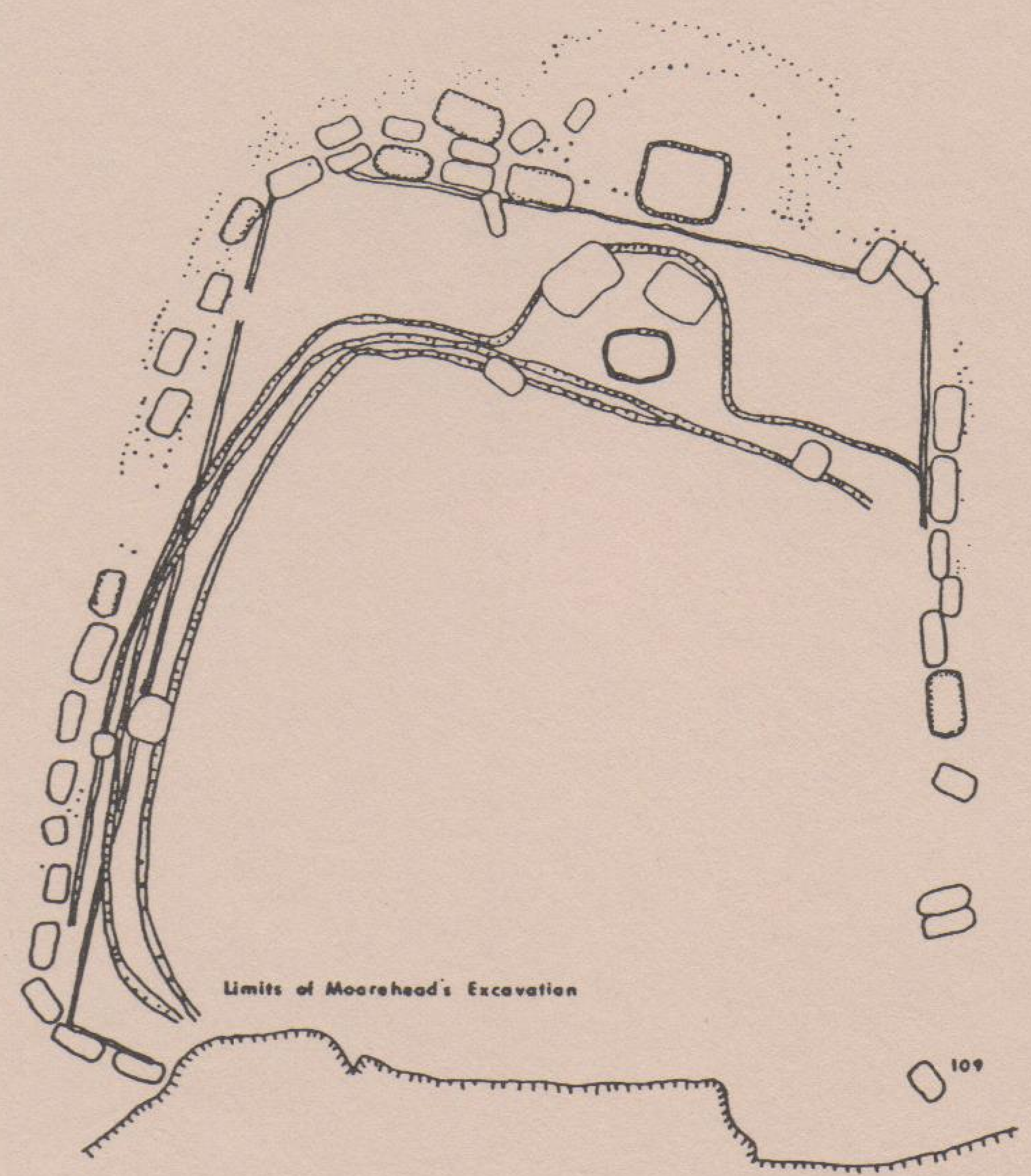
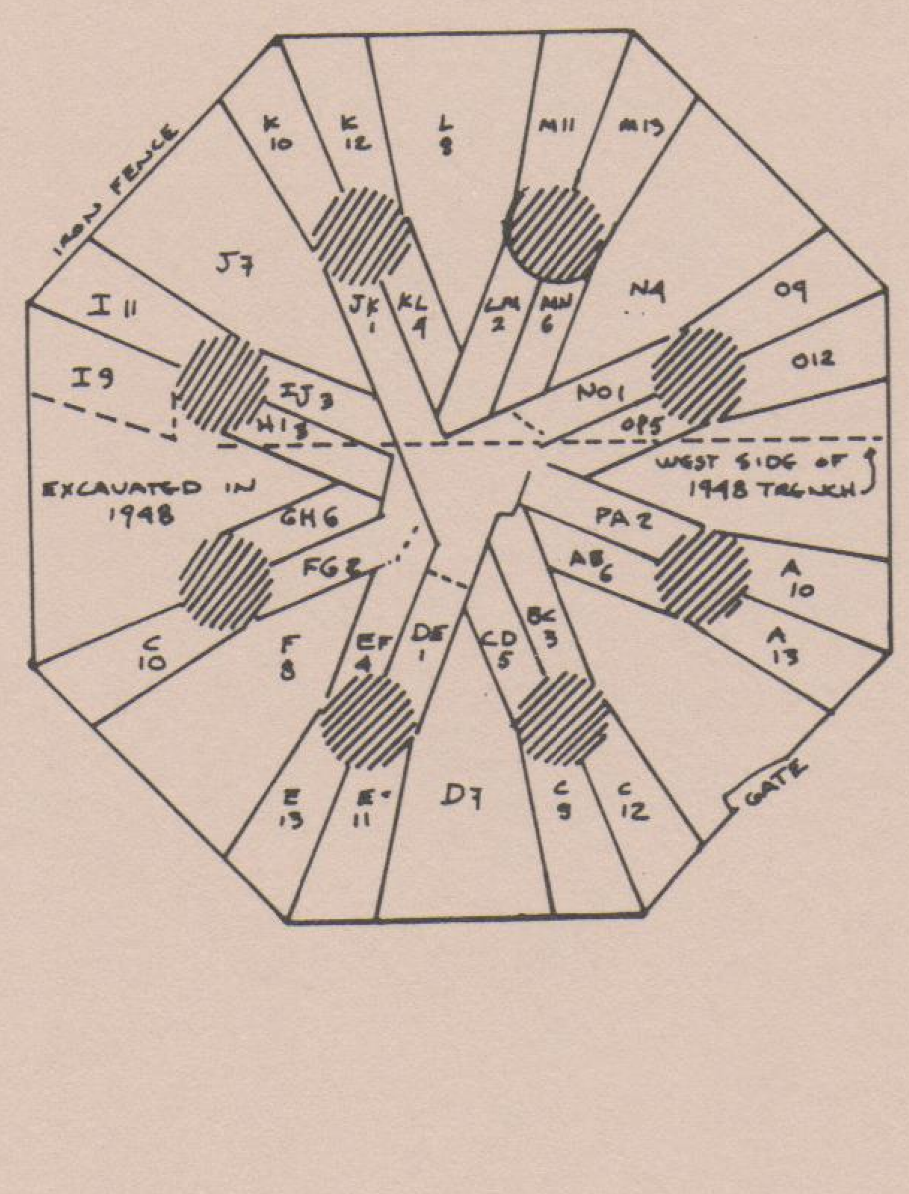
Archaeology of Eastern North America
Papers in Honor of Stephen Williams



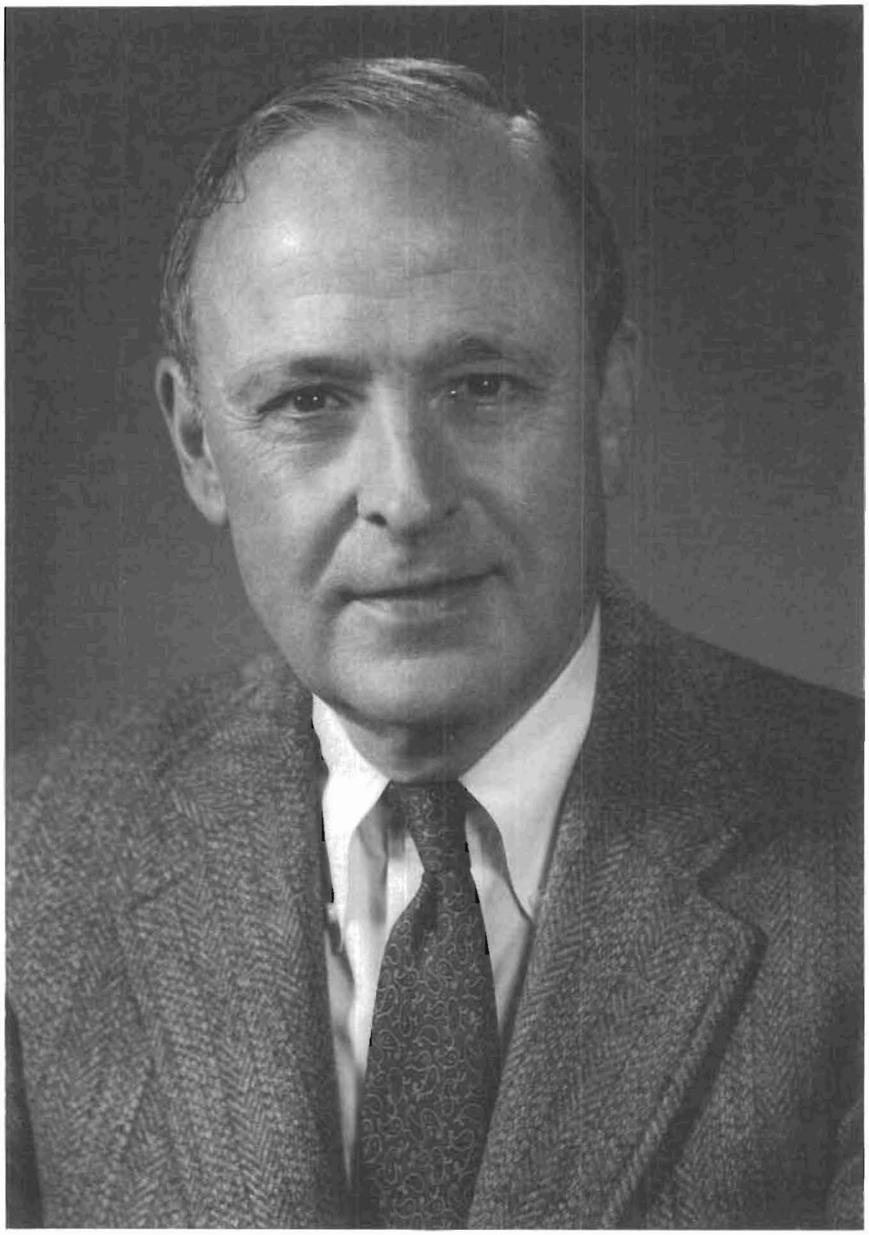
Edited by
James B. Stoltman





Archaeological Report No. 25
Mississippi Department of Archives and History
Jackson, Mississippi
1993

ARCHAEOLOGY OF EASTERN NORTH AMERICA
PAPERS IN HONOR OF STEPHEN WILLIAMS



Stephen Williams, 1991

Archaeology of Eastern North America
Papers in Honor of Stephen Williams

Edited by

James B. Stoltman

Archaeological Report No. 25

Mississippi Department of Archives and History

Jackson, Mississippi

1993

Mississippi Department of Archives and History
Archaeological Report No. 25

Patricia Kay Galloway
Series Editor

Elbert R. Hilliard
Director

Typeset by Altamese Wash

Library of Congress Catalog Card Number 93-620339

ISBN: 0-938896-62-8

Table of Contents

Figures

Tables

Foreword

This volume for Stephen Williams had its inception on an unidentified day in midyear 1989 when it became known to a few of us that Steve had reached the decision to retire from Harvard after the 1992-93 academic year. One day Bruce Smith mentioned that something should be done to produce a volume in honor of Steve, a thought that I also had in mind but had not voiced. Some amount of discussion between the two of us resulted in our contacting Jim Stoltman at the University of Wisconsin to ask him if he would take on the responsibility of organizing and editing such a commemorative volume. He immediately agreed.

Thanks to some behind-the-scenes intelligence work by Jeff Brain, a list of students who had received their doctorates at Harvard under Steve's tutelage was obtained. This list served as the starting point for identifying and notifying potential contributors of plans for the volume. As can be seen from the table of contents, there was an outstanding response from friends as well as former students. It should also be noted that prior commitments, in concert with the tight deadlines that we had to impose, prevented a number of others who had hoped to contribute to the volume from being able to do so.

In seeking a publisher for this volume we assumed that Harvard University, Steve's home institution, would be the ideal place, but, we learned that Harvard University Press has a policy against publishing festschrift volumes. Our next thought was then to seek a publisher close to the area where Steve had conducted so much of his field research, the Lower Mississippi Valley. For this reason, the Mississippi Department of Archives and History was approached, and they expressed interest, even enthusiasm, at the idea if a substantial subvention was provided to help defray the production costs of the volume. A number of prospective donors were approached in this regard, and they responded as enthusiastically as had the various authors.

The organizers of this volume wish to acknowledge, with thanks, the gracious gifts of Albert H. Gordon, L.B. Jones, Edward M. Simmons, Doris Z. Stone, and the Alfred Tozzer family. Their generosity has made this volume possible. We are also grateful to Elbert R. Hilliard, Director of the Mississippi Department of Archives and History, and to Patricia Kay Galloway, Archaeological Reports Series editor, for their interest in and labor on a volume dedicated to a man who has made major contributions to the archeology of the state of Mississippi, to the Southeast, and to Eastern North America generally, both through his scholarship and through his support of students and scholars pursuing archaoelogical research in the area.

James B. Griffin
January 1993

PART I

BIOGRAPHY

1 Stephen Williams, A Career Review

James B. Griffin
Smithsonian Institution
Washington, D.C.

Stephen Williams, Peabody Professor of American Archaeology in the Peabody Museum and the Department of Anthropology of Harvard University, was born in Minneapolis, Minnesota, to Lois (Simmons) Williams and Clyde Garfield Williams on August 28, 1926. His birth was just before or just after his twin brother, Philip. The twins had two older brothers. The twins had their secondary education at Blake School, where they both played forward on the basketball team. Steve records in his most recent book, *Fantastic Archaeology*, that "I can still remember that fall day nearly fifty years ago when I went into the familiar but still imposing oak-paneled library at my country day school and took down a copy of James Churchward's *Lost Continent of Mu* . . . ."

With that as a start, he inherited a large collection of the publications of Ignatius Donnelly on the assumed lost continent of Atlantis. Almost everyone in Minnesota had some knowledge of the Kensington Stone, which did not become a fraud until Hjalmar Holand made it his mission in life to transform what surely started as a gag into a travesty of historical research. However, with his undergraduate work at Yale and association with Irving Rouse, Wendel Bennett, and Cornelius Osgood, his work at Michigan for his M.A. with James B. Griffin, Volney Jones, Leslie White, and Albert Spaulding, and his final graduate work at Yale under the same mentors, he acquired his ability to question and require adequate documentation on archaeological and historical finds. In addition he had the unusual experience of rooming in the home of Ralph Linton, the last Stirling Professor of Anthropology at Yale.

His first formal archaeological experience was as a student assistant for four weeks in 1947 with a University of Minnesota group under the direction of Lloyd A. Wilford. This was during the summer between his sophomore and junior years at Yale, which he had entered in 1945 after spending the prior year in the U.S. Navy.

A now defunct Minneapolis *Morning Tribune* had in its Monday, September 26, 1949 issue a prominent writeup on page 17 as follows:

> Digger Traces Indian History. Youth Finds Bones 2,000 Years Old. He dug day after day in sun-baked Arizona earth and clay to discover scraps of information that may solve the mystery of a "lost civilization."
>
> A June graduate of Yale, Williams left Sunday to continue his archeological research work at University of Michigan graduate school . . . . They started to work under the direction of famed University of Arizona archeologist, Dr. Emil W. Haury . . .
> Williams' particular project was the restoration of pit house number three, which he located by digging in a known Mogollon village. In so doing, he turned up what may be significant bits of information. The house it-

> self was unique . . . . But Williams found a vestibule in front, which authorities say marks the first significant variation from Mogollon architectural pattern that scientists have ever discovered. Williams also found in the main room a rare hearth, dug into the yellow-clay floor. Age old ashes still remained in its depths. Dr. Haury says it is one of the first well-defined hearths ever uncovered in a Mogollon site . . . . Williams said he plans to send much of the bric-a-brac to Yale's Peabody Museum of Natural History (see Figure 1.1).

One might think that after that satisfying experience Williams would have continued to do Southwestern archaeology, but that was not the case. In mid-March of 1949 I received a letter with the printed letterhead "Stephen Williams," in which he referred to a letter Ben Rouse had written me that one of the senior students at Yale was thinking of doing graduate work at Ann Arbor and asked if he could show me a sherd collection he had made from a Middle Mississippi site in southern Missouri for his senior thesis. At the same time he "would like very much to look over the University before I decide definitely about it." I replied that I would be available on March 30th. He arrived and we went over his collection, and he was pleased with my interpretation of it. He returned in the fall with a new Pontiac station wagon, in which we drove in November to the Plains Conference in Lincoln, Nebraska in one day. It was the first of many long-distance drives to meetings that we have since shared, the most recent being in November 1991 from Washington to Jackson, Mississippi.

During his year at Michigan, plans were being made for excavations at Cahokia and a survey of the adjoining area as a part of the continuing Mississippi Valley archaeological survey. Our headquarters in the summer of 1950 were in the Old Court House in St. Louis, where the specimens from the excavations and surveys were temporarily stored. Williams wished to pursue his interests in Southeast Missouri, and Edward Scully was sent along with him. During the summer, they would return periodically to the

Figure 1.1. Photograph from the Minneapolis Morning Tribune, *September 26, 1949, showing Williams' participation in excavations at Point of Pines, Arizona, directed by Professor Emil Haury of the University of Arizona. Photograph courtesy of the Minnesota Historical Society.*

Old Court House, and we would go over and classify the collections they were making. Williams returned to Yale for his final graduate years and finished his thesis on his Missouri research in 1954. "An Archaeological Study of the Mississippian Culture in Southeast Missouri," unfortunately, was never published.

One ceramic complex that Williams identified included sherds that he called Varney Red Filmed. Its association was with "a variant of the general Mississippian tradition which was characterized almost entirely by plain shell tempered with handles rare to absent." He reported that the complex was identifiable by large jars and salt pans with heavy red filmed interiors and sometimes exteriors (1954:30). This complex he called the Malden Plain phase and wrote that it "is a regional variant of the Mississippian tradition which 'feels' early" because of the absence of decorated forms, few handles, and the vessel shapes. Later excavations (1967-1976) at the Zebree site in Mississippi County, Arkansas, a representative of the Big Lake phase in that area, has confirmed that this earliest Mississippian complex can be dated A.D. 900±, and Williams' belief that it was early has been substantiated (Morse and Morse 1983:217-222). Neither Williams in the early 1950s nor Phillips, Ford, and Griffin in the 1940s were expecting the Varney–Big Lake pottery to be the earliest true Mississippi forms.

In addition to his field research in Missouri he spent some time in the mid-1950s studying the collection of Dr. J.K. Hampson in Nodena, Arkansas. He was assisted in this work by Dixie Hampson and was subsequently heard to whistle the tune. But his archaeological interests were much broader than North America. Thus in the summer of 1955 he purchased a Volkswagen that was delivered in Paris and that he used during a three-month period to visit archaeological sites and museums in France, Spain, Italy, Austria, Holland, and England, providing first-hand experience for his future teaching of introductory archaeology as well as providing balance to complement his Southwestern and Southeastern experience.

In 1954 Steve went to Harvard because of the guidance and support given to the study of Lower Mississippi Valley archaeology by Philip Phillips. The recipient of an NSF post-doctoral fellowship, he spent 1955-56 pursuing his Lower Valley research interests, and then was appointed a Lecturer in 1956. He took his teaching responsibilities very seriously, which enabled him to gradually move to a tenured professorship in 1967 and then an appointment as Peabody Professor of American Archaeology and Ethnology two years later. Within the Museum, he progressed from his first position as Research Fellow to Assistant Curator to Curator to Director and then back to Curator. He will retire as Professor Emeritus. You can't retire as Curator Emeritus, the Harvard Corporation forbids.

During his years of formal teaching, Steve offered a wide range of courses from introductory anthropology to a beginning course in archaeology (which he created) to courses on American archaeology emphasizing the eastern United States, method and theory, and the history of American archaeology, which included Jeremy Sabloff and John Belmont in one year. He also taught North American archaeology and world prehistory in Harvard Extension. During his later years of teaching, he began to offer a course in "Fantastic Archaeology" to inform students on how to distinguish between the genuine and the spurious archaeological reports in newspapers, magazines, lectures, films, and books. This resulted in a volume published in 1991 by the University of Pennsylvania Press, *Fantastic Archaeology: The Wild Side of North American Prehistory*, which is a remarkable volume of scholarship. It ranks side by side with Robert Wauchope's *Lost Tribes and Sunken Continents: Myth and Method in the Study of the American Indian*, published some thirty years earlier, but covers more instances of frauds, misconceptions, and persistent stupidity. In addition he was an advisor and counselor for innumerable students, whether they were engaged in work in his major area of research or not, as certified by various Harvard students in letters excerpted in this chapter. He was the primary professor for at least 25 completed doctorates, most of whom have been significant contributors to archaeological knowledge from Iowa and Wisconsin to Florida and from Maine to Louisiana.

Williams was an able fundraiser for both the Museum and for the continuing Lower Mississippi Valley Survey. Major projects whose financing was obtained under Steve's leadership include the Tozzer Library and its building maintenance fund, indexing of the Tozzer Library holdings, and the Margaret Currier, Stephen Williams, Margaret C. Tozzer Endowment. A $450,000 grant from the National Science Foundation was obtained for the reorganization and preservation of the Photo Archives in the Peabody Museum. In addition he gave lectures for the Harvard Alumni Association in fifteen cities from Baltimore to San Diego, from Bangor to Houston. He led Alumni tours to Louisiana and Mississippi and three to Santa Fe and vicinity.

Williams was active on Harvard committees, including chairing the Undergraduate Admissions Committee for ten years, chairing the Task Force on College Life for two years, and chairing the Committee on Non-Departmental Instruction for six years. In addition he served as a member of the Harvard Foundation for Inter-Racial Affairs, was on the standing committee on expository writing and on athletics, and was a member of the Board of Advisors of the W.E.B. Du Bois Institute for nine years. He was Chairman of the University Museums Council for four years and served eight years on the Advisory Committee, Pre-Columbian of Dumbarton Oaks, where his views on the acquisition of specimens must have caused some discomfort.

For two years, 1954-55, he did historical and archaeological research on the Caddo for the Department of Justice. He was Treasurer of the Council for Museum Anthropology, 1974-1977; a member of the Archaeology Conservancy Board, 1984-1988; Editor of the Southeastern Archaeological Conference publications, 1959-1967; a member of its Executive Committee, 1982-1985; and an active member of the Society for American Archaeology and the Mid-South Archaeological Conference.

Shortly after his appointment as Assistant Professor in 1958, he obtained and furnished a vertical flat on Beacon Hill. I remember how proud he was of it as he showed it to me and my wife on one of our visits to Cambridge not long after it was essentially refitted. Steve was married to Eunice Ford on January 6, 1962, which began a long, fortunate, felicitous union. They have two children, Stephen John, who is in the Navy, and Timothy, who is a graduate student in the School of Architecture at the University of Washington. For a number of years while the boys were growing up, the Williams family had their summer vacation on Cape Cod. Among other activities, Steve was able to continue his sailing experiences and expertise that had begun in his boyhood around Minneapolis. One change of scenery was the year they spent in Santa Fe in 1977. How a man from the land of 10,000 lakes could become enamored of the Santa Fe environment is difficult to understand, but as a result, Steve and Eunice are planning to retire in Santa Fe.

Steve became Director of the Peabody Museum in 1967 and was anointed a Professor in the Harvard College. This was after some 13 years of exposure to the Harvard environment, which might have served as a brake on taking on such a time-filling task. He kept a diary of activities, as is his custom, and entered rather formal statements of his views of the proper function of the Peabody Museum and its interaction with the Department of Anthropology. He prepared a booklet, "From the Director's Desk," documenting his tenure as Director. An "Introduction" by the President of Harvard, Derek C. Bok, comments on the Peabody Museum as an important part of the instructional and research role of the university. His initial paragraph reads:

> I should like to take this opportunity to recognize the great progress that has been made in the last decade under the leadership of Professor Stephen Williams. These accomplishments represent the most eloquent testimony to the effort and imagination that Professor Williams has devoted to the maintenance and growth of the Peabody.

After commenting on several Peabody programs augmented in the ten-year tenure of Steve's directorship, President Bok concluded that, "These are noteworthy accomplishments. They provide an impressive reminder of all we owe to faculty members, such as Professor Williams, who are willing to sacrifice their scholarly work to devote their energies to enhancing Harvard's great museums and cultural resources." Such moments are rewarding, but Williams' ten year period as the administrative head of America's most famous anthropology museum was full of thorns as well as roses. He was not an absentee officer or one who delegated or neglected difficult decisions or managerial responsibility. He was engaged in, and oversaw, every facet of the functioning of that cultural treasure, including The Case of the Purloined Stela from Machaquila in Central Guatemala, the development of the University statement on the acquisition of antiquities, and an active participation in 1967 in Harvard's science funding drive, as a result of which the Peabody was to receive two million dollars. Plans were made for additional Peabody space of 177,000 square feet, but promised support was not forthcoming. That disappointment was tempered, however, by a million dollar gift for the Tozzer Memorial Library, whose construction, after much travail, was completed in late October, 1974. This was surely the most important addition to the Museum facilities since the Museum was started over 110 years earlier.

Active field research programs were conducted by Peabody Museum staff and students in the Near East, Peru, the Maya area, the Lower Mississippi Valley, Europe, Chiapas, Brazil, South Africa, Kenya, the Solomon Islands, and India. The fruits of these activities and earlier research resulted in an increase in the number and rate of publications that were prepared and issued by the Publications Department. Other phases of Museum activities, such as exhibits, lectures, conferences, American and foreign professional visitors, acquisitions, and donations, all increased under Steve's nurture. Funding for the Museum was always difficult, but in 1975-76 gifts and special receipts totaled nearly $520,000, the sixth straight year in excess of $400,000.

Writing of the fall of 1970, Williams recalled that the "term started with beautiful golden-warm afternoons and a bomb blast across the street." In the fall of 1972 "The summer doldrums, with gentle breezes and long afternoons, have bypassed Cambridge this season, too. Every year I look forward to the time when the building is really empty of both students and research staff, and each year I find that they are not what fill my days with activities. It's Buildings and Grounds, fund raising, and foundation chasing, and odds and ends."

Steve took pride in the continuing publications in the several Peabody series, a number of separate volumes, and a private-press series, "Antiquities of the New World: Early Explorations in Archaeology," with new introductions by Peabody staff members and a few by outsiders. In this latter series a volume of papers by that giant of early American anthropology, Frederick Ward Putnam, had an introduction by Williams emphasizing the important role his eminent predecessor had played, particularly in archaeology, even though he was wrong in some of his interpretations of archaeological data. While Steve was pleased at each publication, I believe he had more satisfaction with the issuance of studies in which he had some part, such as Phil Phillips' masterful study of Lower Yazoo Basin archaeology and its place in Lower Mississippi Valley prehistory and the magnificent study of the engraved marine shell funerary offerings from the Craig Mound of the Spiro site by Phillips and Brown.

In his last newsletter as Director, written in the winter of 1977, he referred to Gerald Ford leaving his Presidency for California as follows: "I can now sympathize with him in many ways I would never have imagined a few days ago. The decision to step down as Director was not taken lightly or quickly after a decade, but the withdrawal symptoms are evident." During the first two years of his directorship, he was also Chairman of the Department of Anthropology while the support for a much enlarged Anthropology building with a much enlarged ethnographic research facility seemed a distinct possibility. But this did not come about, primarily because such an expansion was frozen out of the Program for Science at Harvard when failure of the fund-raising drive shifted its title to "Finish the Job." It certainly finished the Museum expansion, with the final stages of the architectural plans having been completed since March 1971. This must have been one of the major disappointments of his career.

As museum director, Steve developed and propounded strong views on the illegal acquisition of antiquities. These views are given muted expression in his article in the *Saturday Review*, October 1972 Science number, where he commented on cases of vandalism in Guatemala, Mexico, and Peru, noting that such activity was an old industry in the Middle East and Egypt and not unknown in the United States. He wrote: "Legislation has proved to be quite futile in a climate of opinion that at worst, condones the looting of sites, or that, at best, only mildly condemns it. In the United States, with the exception of a few areas that have special legislation protecting historic districts, most private property remains exempt from existing federal or local laws that prohibit destruction of artifacts or that preserve monuments of antiquity. For example, a prehistoric Indian mound on any private farm in any state of the Union is fair game for the bulldozer or the looter if the owner agrees." Individuals who purchase artifacts excavated by untrained diggers with no prospect of producing a satisfactory report on their work are at least as guilty of unethical conduct as the digger, even if the purchaser places the specimens in a museum for his greater glory. The situation in 1972 that Williams castigated is not as bad now, with some recent federal and state legislation now on the books. However, some of these legislative actions were not wisely wrought and only serve to satisfy a strident minority of naive, half-learned individuals, and politicians.

On May 28, 1958 Steve wrote me that following the twenty-third annual meeting of the Society for American Archaeology, held earlier in May in Norman, Oklahoma, he had stopped to make a surface collection at the Knapp (Toltec) mounds in Arkansas. His comment on the pottery and projectile points was, "Looks pre-Gibson to me!!!!" At that time the George C. Davis site in northeast Texas was being touted as quite early and a point of entry for people and ideas from Mexico. In the second paragraph of the same letter he wrote: "I just wanted to remind you that this graduate student of ours: Kwang-Chi Chang, was going to be coming to your place around the 9th or 10th of June. He has a traveling fellowship to see museums and excavations. I am sure he would like to meet Kamer (Aga-Oglu) too." Mrs. Aga-Oglu was Curator of the Orient in the Museum of Anthropology who became an authority in Chinese export wares into Southeast Asia and the Philippines. Chang received his Ph.D. from Harvard in 1960, was at Yale University for a number of years, and returned to Harvard. In response to my request for a statement on his views of Williams as a colleague he wrote:

> I have known Steve for thirty-five years, and it is difficult to give you what I know and how I feel in a brief passage. I can say this: above all I favor Steve for his unwavering loyalty–loyalty to his friends and colleagues; to his institution; and to the highest standards of scholarship. All who know him know of numerous instances of Steve's loyalty to people he values and respects. The decade of his stewardship of Peabody

> Museum is marked, in every action he took and every word he uttered as Museum director, by his consideration for the Museum's best interest. His *Fantastic Archaeology*, which has just been published, is the best testimony to Steve's firm loyalty not to people, however famous or distinguished, but to the standards of scholarly quality by which their works–anyone's works–are to be measured. Steve is a rare gentleman, and I am privileged to be among his friends.

I know that Steve's appreciation of KC as a scholar, connoisseur of Chinese cuisine, and gracious gentleman was shared with me, particularly when Steve and KC were teaching extension courses at the same time and were able to have periods of relaxation together over evening meals and on their way to and from teaching sessions.

Antonio J. (Tono) Waring, Jr. was a native of Savannah, Georgia who received his college education at Yale and his M.D. from the Yale Medical School. As a young man, he became interested in the prehistory of the Savannah area and should have taken his doctorate in Anthropology. Instead, he became a pediatrician, but maintained an active participant role in Southeastern archaeology, both in field work and publication. Shortly after Waring's death, Williams became aware of the existence of a number of unpublished manuscripts at Georgia State College and in Savannah and felt they should be published in a single volume. The result was *The Waring Papers*, published jointly by the Peabody Museum and the University of Georgia Press (Williams 1968). This fine volume was a tribute both to the person and to archaeology, for Tono was gifted and probably equal in stature to the best in Georgia. The inspiration and perspiration that produced the volume is typical Williams in its compassion for an individual who lived a difficult life but had high standards of professional behavior.

In the collection of papers prepared to honor William G. Haag when he retired from Louisiana State University, Williams co-authored with John Belmont a fine study of the typology, distribution, cultural association, and temporal placement of painted pottery in three regions in the southern Mississippi Valley: the Lower Red, Tensas/Natchez, and Lower Yazoo. This seminal paper corrects earlier temporal and associational assignments and emphasizes the interrelatedness of northwest Florida and the Lower Mississippi Valley during the years from about 50 B.C. to the historic period.

In the same volume Williams has a short paper on the historical continuity of ceramic making in the Southeast up to quite recent times. Eighteen tribal societies were still making pottery, and the illustrations featured vessels found by Gregory Perino on Choctaw sites of about 1840 in McCurtain County, Oklahoma.

Williams was one of three archaeologists who formed a committee that produced "Research and Reflections in Archaeology and History: Essays in Honor of Doris Stone." In this festschrift volume he prepared a tribute to Doris on her accomplishments during a long career more or less contemporary with two other outstanding women archaeologists, Frederica de Laguna and Isabel Kelly. Steve also prepared her bibliography with the help of several librarians. This is another example of his willingness to participate and contribute to tasks to honor and support colleagues and students.

Williams' contributions to the Southeastern Archaeological Conference have been a continuing source of pleasure for him, for he has a strong commitment to its aims and to its changing personnel. At the 50th annual meeting "Golden Jubilee" he gave the banquet address that included reference to many of the participants in past meetings or who were influential in Southeastern archaeology. His mention of these people provides a brief glimpse of many stalwart workers from the mid 1930s to 1988.

Steve has always had compassion for his colleagues working in the Southeast. Robert Stuart Neitzel was, in many ways, the antithesis of Steve in dress (most of the time), in imbibing, in public behavior, and in seeming not to care what the future might bring. Steve, however, valued his friendship, saw through the facade that Neitzel portrayed, and recognized his commitment to Southeastern archaeology. Recognizing that Neitzel was well informed, an excellent excavator, and a willing instructor to individuals just beginning their careers, Steve incorporated him into many of the activities of the Lower Mississippi Valley survey, and both benefited from that collaboration.

One of Williams' strengths was his ability to interact effectively with collectors, land owners, tenant farmers, and others who furnished information on the archaeological resources of an area. His behavior and bearing inspired confidence and opened doors that provided work opportunities for students and colleagues alike. As Ian Brown has written me on April 11, 1991, ". . . one of Steve's fortes in archaeological field work is getting things started. Jeff [Brain], T.R. [Kidder], Rick [Fuller], and myself have numerous stories on the role Steve played in our projects, especially in getting the camp started. This was what he always enjoyed most." This statement was in a cover letter to me, along with copies of the letters involving the negotiations for the Petit Anse Project on Avery Island, a part of the Lower Mississippi Survey in 1977 under Steve's general direction. Those involved were Walter S. McIlhenny, Edward M. Simmons, Lanier Simmons, Jeffrey P. Brain, Nancy Lambert-Brown, Ian Brown, Sherwood Gagliano, and Jon Gibson. The correspondence covers some 36 pages. It was conducted with gentility, friendship, and, eventually, a feeling of satisfaction.

One of the undergraduate students at Harvard who became involved in the Lower Mississippi Survey was Vincas Steponaitis. In his sophomore year he took a course from Jeff Brain on North American archaeology and decided to sign up for the Natchez program for the summer of 1972. In answer to my request Vin replied:

> Having returned from a very interesting summer in Natchez, I spent the better part of my junior and senior years hanging around the Peabody Museum. Steve would often come up to the Putnam Lab or his 4th floor office (especially at 'tea time') and chat about Southeastern archaeology. I also remember being introduced to Steve's photographs of, and notes on, various museum and private collections from the Lower Mississippi Valley. That's how I became aware of the importance of these collections, including the Ford and Chambers material from the Big Black, on which I subsequently did some work.
>
> But my most vivid recollection is of a conversation I had with Steve in April of my senior year. I had been accepted to two graduate schools, Brown and Michigan, and was wrestling with the decision of where to go. I was more attracted to the Michigan 'program' but they had not offered me financial aid; Brown, on the other hand, had offered a generous fellowship. I remember Steve saying, "Go to Michigan; Griffin will take care of you." With that reassurance, I went to Michigan, and Steve turned out to be right.

Williams shared with me a great Christmas present that he had received recently from a former student at Harvard, which came as a most welcome antidote to certain unpleasant currents that had developed with some of his associates. Geoffrey Conrad, Director of the Mathers Museum at Indiana University, wrote on December 18, 1990:

> Dear Steve,
>
> I find myself thinking of you frequently these days, and I've realized that I should

> have written this letter years ago. So I beg your indulgence–I'm slow, but I get there.
>
> What's happened this year is that two of my junior faculty colleagues have been coming to me for advice–without ever saying so, they've made it obvious that they have settled on me as a mentor. Finding myself cast in this role causes me to reflect on how much *you* have filled that role for me, and how much you've meant to me over the years (25 now, if you stop to count them). While I've known all of this for a long time, I don't think I've ever acknowledged it openly or thanked you properly.
>
> So, from the bottom of my heart, thanks for all you've done for me over the time we've known each other. You've been a superb mentor and a true friend, and it's meant a lot to me, and what I understand now is that you not only steered me toward it when the opportunity arose, but that you'd been preparing me for it all along–sometimes through explicit advice, sometimes just by example. Some of it went over my head at the time, but I guess it all sunk in in the end.
>
> I've been very lucky to have known you, and I owe you a great debt. I don't think I can ever repay you directly, but I *will* try to pass things along to the next generation. Maybe that will count for something.

Jeremy Sabloff, a Harvard graduate whose research is primarily in the Maya area and a former colleague of Steve's on the Harvard faculty, prepared a brief comment for this chapter:

> Stephen Williams is one of the most dedicated teachers I have known. When I began my graduate studies at Harvard in the mid-1960's, Steve's commitment to students was readily evident. His introductory seminar for first-year grad students in archaeology was meticulously prepared, broadly based, and extremely enlightening as to the growth of archaeological theory, method and practice. He was readily accessible to students interested in continuing seminar discussions, and his enthusiasm for archaeology and archaeological fieldwork was contagious. Moreover, Steve's bibliographic interests stimulated students like myself to frequent the used bookstores of the Boston area in the hopes of discovering some rare, out-of-print (and hopefully inexpensive) archaeology books.

Another non-Southeasterner who benefited from the association with Williams is William Fitzhugh, head of the Arctic Studies Center of the National Museum of Natural History of the Smithsonian Institution. His statement is both longer and somewhat more formal than others that I obtained. Its distinctiveness is preserved below:

> *Around the Tea Table: Some Peabody Memories of a Putnam Labradorian*
>
> There was a time, back in the good 'ol days when archeologists were real persons, that Mississippians would regularly beat a trail north to Alaska. Those days, regrettably, had passed by the time I matriculated, close-cropped, at the Peabody in 1970. Coughed up from the engineroom of the *USS Peregrine, AG 176*, onto the pier at Boston Navy Base, the US Naval Reserve officer looked up, noted my destination across town, "Harvard, eh . . . we've got enough trouble down here with Viet Nam business," and in a flash I was delivered from four years of impending naval reserve duty to full-time graduate student status. On the other side of the revolving door was Steve

Williams. He didn't know it then, but I had just come from the Arctic and was planning to by-pass the Lower Yazoo Basin.

It's hard to imagine those first days and months–a sea of graduate students, incomprehensible genetics and statistics courses, social anthropology students speaking some kind of abstruse William James dialect, but as my hair grew out the tones began to sound vaguely atavistic, almost intelligible. If you couldn't understand your fellow students–the Renato Rosaldos and Rick Schweders–at least you felt you were on the same planet with Vogtie. And for Steve Williams (whose hair was also reassuring: by mid-semester mine was longer than his), my disguise was holding. More importantly, through the good offices of old hands like John Terrell and Cynthia Weber I found a seat around Steve's afternoon tea table. Here indeed was a haven for the oppressed and bewildered, provided you mustered up respectably on the North American Prehistory exam. That done, and as Vogtie's social anthropology seminar took off into the "SocRel" miasma, I discovered the Putnam lab and its director–by now my advisor–part of a reassuring new world. Slowly a sense of identity began to take root.

Steve's role as an academic advisor had an immense effect on me, and in later years I found that his impact left an indelible imprint also on all of the students he became associated with. The "became associated with" is the key phrase, because Steve's interests and energies were not universally available. His was not a large coterie, even in the halcyon days of the late 60s when an incoming class of Harvard graduate students numbered 16 in archeology alone–granted, most bound for Peru or Belize. And because Steve was not actively pursuing field studies but was ordering LMS potsherds, uncovering the roots of American archeology, assuming Directorship of the Peabody, and professionalizing the tired and musty field of museology, we were being offered a graduate career none of us had expected but which, in the end, produced a large number of curators and museum directors.

As the Putnam Lab, re-styled Williamsesque, brought the back end of the Peabody Museum out of its southwestern grit-and-cigar era, our group around the tea table grew closer even while we grew apart. Unlike so many other graduate experiences–none at Harvard of course!–that centered on a version of enslavement to a core program, we found ourselves hovering around the periphery of the LMS, regaled by old stories about the grand masters–Phillips, Ford (pre-formative), Griffin, and that turncoat Willey who migrated south. Because Steve was so involved with the museum in these days, because of geography (Harvard *is* in New England), and because many in the program had Ritchie roots, many of us found dissertation topics scattered around the periphery of the Lower Mississippi, like in New England, rather than in it. This had not been the case with our predecessors around the tea table–Dave Hally, Frank Schambach, Mike Hoffman, and others–who had already taken up the Southeastern banner. For one reason or another, our group soaked up those Southeastern and Archaic seminars but then headed for the hills rather than the swamps–John Terrell to Bougainville, Bruce Bourque to Penobscot Bay, Dan Ingersoll to the historical archeology of Puddle Duck in Portsmouth, N.H. (here Williams was really keen and way ahead of his field), Tom Layton somewhere into the Rocky Mountains, Bill Rathje to the garbage dump (via Mesoamerica and the New Archeology), Cynthia Weber to Iroquoia and her pipes, Vince Wilcox to the Heye Foundation via Fort Shantok, and Dave Browman, Drexel Peterson, and Kent Day to "higher civilizations" God-knows where. But throughout, as we made our way into our chosen fields, we carried with us Steve's tidbits and treasures on the history of American archeology and ideas about the foundations of North American archeology.

My course was not to be the pleasures of Plaquemine or the Tchefuncte trail, dangled so temptingly before me, but another. That difficult moment came when Steve asked me to consider a Southeastern dissertation. The winged nazguls passed overhead; the sky darkened momentarily. But fortunately there were precedents, and illustrious ones at that, ones who had, in saner, more reliable periods in their careers, participated in real archeology before abandoning the clay pits for Alaska permafrost. Henry Collins and his side-kick, that stringbean giant Jim Ford (who Collins liked to pose with a stadia rod and the shortest Eskimo on the crew for effect), and Elmer Harp (Harvard Ph.D., even!) had beaten a path to the Arctic before me; why couldn't I?

Steve was gracious, understood. The LMS could wait, and those paper bags of sherds and bones weren't going anywhere fast; quite the opposite with Harry Hornblower's architectural plans for the Tozzer Library where he and Steve were definitely on a roll. Thank goodness those young turks–Collins and Ford–had found their way north in time for me. With Henry's prize-winning work on the archeology of St. Lawrence Island, and Jim Ford's work at Barrow, that system worked out for Southeastern potsherds became a type system for harpoon heads and formed the basis for a scientific Arctic chronology and prehistory. Steve's only admonishment was about unpronounceable phase names. Please, no more Siberian Yupik and Inupiat phase names and harpoon head types: out with Seklowagyaget, Miyowagh, Ipiutak, Birnirk (. . . but what about Tchefuncte and Plaquemine, I protested?). In short order we were over the hump, and for the next two years I found my life enriched by the unending store of Americana archaeological that osmosed from those afternoon tea bags. And I found Steve to be an ardent supporter as my thesis work commenced, moved into the Canadian channels, and my interests broadened to musings about trans-Atlantic contacts and circumpolar visions. Perhaps there was life outside the Lower Mississippi afterall! Even better, near the end, when doubts overwhelmed me and I was ready to throw out a third of my dissertation final draft, he said "press on."

For a teacher and advisor, I could not have had better, and through the years I have continued to learn from Steve's vast store of knowledge. More gratifying, Steve served as advisor and friend for a slew of younger Arctic venturers, Steven Cox, Arthur Spiess, and most recently Anne Henshaws, all infected with northern fever. We, who did not choose the Mississippi Valley, continue in one way or another to sit at Steve's tea table and to absorb his knowledge of American archaeology, his insights about the history of our field, his concerns about the preservation of collections and a professional museology. I will have to leave to

> others to recount memorabilia of the Yazoo, Gagliano's meander chronology, those curious Poverty Point clay balls and damned "bird-shaped" mounds (SW: "Do ya see 'em? They're there in the drawing sure enough, but not in the dirt"); but I have benefited greatly from Steve's work as a teacher and a friend who introduced me to museum anthropology and made it matter. And still the Lower Mississippi pipeline to the North lives on!

John Terrell, who is Curator of Oceanic Archaeology and Ethnology at the Field Museum of Natural History, wrote me about his association with Williams while he was a student at Harvard, underscoring Fitzhugh's discussion of Williams' willingness to support good graduate or undergraduate students in Anthropology:

> I first encountered Steve Williams in 1961 toward the end of my freshman year at college. He had walked over to the Union from Peabody Museum to talk with students about majoring in Anthropology. After he had left us to digest his words, we all commented on how young Williams was. Yet he was a Harvard professor! We were impressed. And we all feared we would never rise so quickly to rank in the academic world.
>
> At that time, I was earning pocket money by working evenings at Harvard as a library assistant. Not long after meeting him, however, I heard he needed a student research assistant for the following academic year. I applied for the job. I got it. I kept it for the remaining three years of my undergraduate career.
>
> Steve often told me during those years that he was surprised how easily I understood his research instructions and work requests. He said people usually misunderstand him. He attributed my ease of understanding to the fact that we both had twins. It is quite true that, when we were young, my twin sister Jane and I communicated with each other intensely without need of words. So Steve's explanation, if one was needed, made sense to me.
>
> It never appeared to bother Steve that, as an undergraduate, I was more interested in European prehistory than in the Lower Mississippi Valley. (I hasten to record, however, that I did spend the summer of 1964 in Louisiana working with him on the Tensas Archaeological Survey.) Like many other people, I owe Steve a great deal. He took me seriously as an anthropologist long before I deserved such consideration. He helped personalize a university that was all too often cold and impersonal. And he kept a watchful eye over my progress as a graduate student at Pennsylvania, Auckland, and finally back at Harvard even though I abandoned both Europe and the Eastern United States for the palm trees and blue waters of the South Pacific. I will never be able to thank him enough.

Terrell also is an Adjunct Professor of Anthropology at Northwestern University, which apparently has a more tolerant atmosphere about interpretations of Oceania Anthropology than is currently possible on the Midway.

The final comment is by Derek Bok, who was President of Harvard during much of Williams' career. He observed and valued many of the personality and character traits that others have observed, but in a delightfully distinctive way:

> I first met Steve Williams in 1971. He was then Director of the Peabody Museum. I

was the new President of Harvard–and not a respectable Arts and Sciences scholar but merely a lowly law professor who knew next to nothing about museums, let alone the arcana of anthropology, archaeology and valuable artifacts. Educating me to understand the Peabody must have been a trying affair. I am grateful to Steve for not giving up.

In the years that followed, I gradually learned more about the Peabody under Steve's tutelage. Along the way, we were able to complete a number of ventures on behalf of the museum.

One of our first tasks was to build a new library for Anthropology. Our challenge was to squeeze a substantial new building into an old and stately courtyard without destroying the appearance of the museum. This was not an easy task. How to build something in a style acceptable to a self-respecting architect which would still be compatible with the quaint and venerable buildings of the Peabody? How to select a brick of reasonable hue that would somehow fit with the series of unfortunate colors that some feckless architect had chosen for the original structures? Somehow, Steve and the architect surmounted these challenges, and did so with enough distinction that the new building actually enhanced the old courtyard and attracted the attention and envy of those inhabiting other buildings in the neighborhood. Eventually, inspired by this example, the entire area was renovated to form a much more harmonious whole.

Another memorable undertaking was our pilgrimage to Mexico to give back a collection of jade objects which some loyal son of Harvard had taken from a *cenote* and given to the University around the turn of the century. Fortunately, the Supreme Court of Mexico had long since ruled that Harvard had a legal right to the collection. Nevertheless, Steve believed that the rightful place for the collection, at least for most of it, was in its country of origin. Somehow, he managed to arrange to give the objects back without awakening the wrath of the American museum establishment, which has a Pavlovian tendency to react ferociously against almost any effort by an institution to get rid of objects, however compelling the circumstances.

I quickly agreed that we should give the objects back, having long felt that opportunities often arise in which everyone can gain by imaginative arrangements to restore valuable objects to their place of origin. This appeared to be just such an occasion. True to his scholarly responsibilities, Steve retained enough of the objects to satisfy Harvard's remaining scholarly needs. At the same time, Mexico offered arrangements to house and exhibit the collection which seemed adequate to provide for their safety and to insure a greater audience than could ever see them in Cambridge.

The Mexicans seemed quite amazed at our offer to return the collection. They had grown accustomed over many years to expect what they regarded as acts of "cultural imperialism" on the part of their Yankee neighbors. Nothing in their experience had prepared them for an unsolicited offer to restore part of their cultural patrimony . . . with nothing asked in return. Enthusiasm in Mexico City ran high. We were met at the airport by curious reporters and their photographers. The President of Mexico was even moved to give a gala luncheon in our honor in his garden and to invite all Harvard alumni in the area. From beginning to end, it was a joyous experience.

Steve was not always so willing to part with objects from the Museum, especially when he thought that they might have something to teach future students and scholars. In the course of time, we came to differ on the sale of several "Wild West" paintings to finance badly-needed renovations in the museum. Although our curators found the pictures lacking in any artistic merit, Steve was not convinced that they were bereft of scholarly interest to those interested in the life and culture of Indian tribes in the last century. Characteristically, he was not shy about expressing his views. But just as characteristic was his complete and undiluted concern for the Museum and its educational mission. I always enjoyed such exchanges. They were often vehement, but always pursued with total honesty and passionate conviction without the slightest tinge of academic guile.

Alas, not all the dreams we shared came to fruition. In particular, we both agreed at an early point that it would be a splendid idea to build a Museum of Man in some impressive spot overlooking the Charles River where the public could come to see rotating exhibits supplied by the Peabody, leaving our buildings to be dedicated solely to education and research. Unfortunately, no munificent benefactor arrived on the scene to turn our dream into a reality. But I have not abandoned hope for the project–nor, I suspect, has Steve. Like World Peace or a World Series victory for the Red Sox, it is simply too good an idea not to come to fruition eventually. When it does, I hope that someone at the dedication ceremony will recall that it was Steve who conceived the idea long before its time.

Every university expects its professors to meet their classes regularly, teach conscientiously, and devote themselves to serious research and scholarship. But no institution can function if its faculty does no more than that. A healthy university cannot flourish, or even survive, unless it can find a substantial number of professors willing to go far beyond their formal job descriptions to take on the many other tasks that are needed, especially in difficult times. For me, Steve was always one of those professors, consistently willing to do more than what was required, whether it was to serve the museum, aid in the process of educational reform, or enrich the extracurricular life of the University and its students. Steve has been engaged in and committed to the total life of the institution. I hope that his special form of passionate engagement has enriched his life at Harvard. I know that it has enriched the University.

Williams' interest in American archaeology extended from the earliest arrival of the Asian immigrants to the historic period. Anyone working primarily in the Southeastern United States was more or less forced into knowledge of, and working with, the early historic interaction of European immigrants with people who had become "native" Americans. Living in Cambridge was enough to require knowledge of European arrivals and their destruction of the Indian way of life, as well as of the Indians themselves. Steve worked with numerous graduate and undergraduate students who had acquired an interest in this field, and for two summers he actively supervised excavations in the Harvard Yard where the Old College had been located. He was literally digging up the dirt of Harvard.

At the other end of the time scale from the historic period his article on the Island 35 Mastodon described a location in Tipton County, Tennessee, northwest of Richardson's Landing where, in July, 1900, Dr. James K. Hampson of Nodena, Arkansas had salvaged mastodon bones from compact, river-laid deposits along with a projectile point fragment and a chert scraper. The excavation operation was far from ideal, but the possibility of a meaningful relationship caused Williams to present the data and a comparative statement on other finds. It was an excellent review of the status of human and mastodon for the mid-1950s.

Another and more comprehensive examination of the early Indian occupation of the Southeast was his survey with J.B. Stoltman of the Paleoindian finds in the region. This was one of five chapters on Quaternary archaeology included in the admirable survey, *The Quaternary of the United States*, edited by Herbert E. Wright and David G. Frey (1965). This was the first intensive review of the early occupations of the Southeast and was a major insightful contribution. It correlated the fluted point distribution with the physiographic provinces and emphasized the essential similarity of chert technology as a "tradition" related to those of the western United States. There were proposals for temporal sequences for fluted point forms in several areas, the gradualness of the change from the fluted point to the early Archaic industries was recognized, and a meaningful relationship of the occurrences of mastodon finds with the distribution of fluted points was suggested.

Building on the relatively recent work of his colleagues and his own studies in southeast Missouri, Williams contributed a chapter "Settlement patterns in the Lower Mississippi Valley" to the Viking Fund volume, *Prehistoric Settlement Patterns in the New World*, edited by his associate at Harvard, Gordon R. Willey (1956). The emphasis of this paper was on the relationship between the physiographic areas identified by surficial geologists and cultural remains that were present or absent. One of the first warnings that archaeological data could contribute to a more accurate assessment of the age of some of the surfaces was made in the single footnote in the paper. A tentative correlation of the cultural sequence in the northern and southern divisions of the Lower Mississippi Valley was proposed. It has now been revised considerably by Williams and others. It is a pleasure to me to see that the equation in time of Early Mississippi in the north with Plaquemine in the south is now abandoned. This was one of Jim Ford's ideas that, since shell temper first becomes common in Plaquemine, its use to the north either came from the south or that Plaquemine and Early Mississippi were on the same time level.

As a result of his ethnohistorical work for the Department of Justice on the locations of the several societies of the Caddo linguistic group, Williams published an abbreviated paper on "the available data on the original location of the Kadohadacho and related tribes of the Red River region and to trace their movements during the period of recorded history. A brief archaeological section attempts to connect the prehistory with the findings of the historic period." (Williams 1964:545). This was published in the festschrift volume for George Peter Murdock, who had been one of Williams' professors at Yale. In this study it is apparent that there has been good agreement among historians and ethnographers in recording the several movements through time. Compelling evidence for the precise location of specific segments of the Caddo, identifying the site and complex, is much more difficult. One of the beneficial results of this historical foray was the consultation with the Shreveport pediatrician, Clarence H. Webb, who was, in my judgment, the best archaeologist in Louisiana, particularly of the northern and western parts of the state. In addition, investigative skills required in documenting activities of the historical period were honed in this research on the Caddo.

An interpretation of Southeastern archaeology that he has proposed and has continued to nurture is the Vacant Quarter hypothesis. He proposed that the late prehistoric to early historic Armorel phase occupations in northeast Arkansas and some other contemporary areas of the Southeast, such as the Cairo Lowland, southern Illinois, and northwestern Kentucky, was largely dispersed ca. A.D. 1450. There has been some disagreement about the Vacant Quarter, but certainly there was a decline in the intensity of occupation compared to that prior to the 1400s.

The major report on an excavation with which Williams was associated is Volume 74 of the *Peabody Museum Papers*, dealing with the Lake George site in Yazoo County, Mississippi, published in 1983. The report is the result of three seasons of field work at a multi-component site with good evidence of Coles Creek occupation followed by a full-blown Mississippian occupation. While Williams and Brain are identified as the authors, the report benefited from the work and ideas of many individuals who participated in the Lower Mississippi Survey. The report took a long time to appear because of a variety of delays, surely caused by the malevolent spirits. The volume is the nearest approach to a statement on Coles Creek that includes more than ceramic descriptions and discusses broader issues as well. As such, it will be the prime reference for Coles Creek for some time, for there is no other study in progress that can match or supersede it. It is unfortunate that a program of Federal Highway expansion, like that recently concluded in the American Bottom, did not take place in the Lower Mississippi Valley in order to make available funding for more extensive examination of large late prehistoric village sites and preceding occupations in this region. Neither private, state, nor foundation funding is adequate for such a program.

In the *Anthropology Newsletter* for November, 1991, Natalie Woodbury included in her "Past and Present" column a poor photograph that is captioned as follows: "Steve Williams, the fantastic author of Fantastic Archaeology." She goes on to say:

> Stephen Williams in *Fantastic Archaeology: The Wild Side of North American Prehistory* (1991) characterizes Gladwin's book (Men Out of Asia) as a "sort of hyper-diffusionist spoof" representing "the fundamental struggle between the amateur and the Establishment" (p. 228). The struggle may not be as fundamental today or even exist between the true avocational and the professional, but to the followers of a guru-like Barry Fell or the vicarious archaeologists who are fans of Indiana Jones or true believers in Erich von Daniken's visitors from outer space as creators of the Easter Island statues, there can well be a crusade to carry The Word. Williams' book is an antidote to the flood of books and projects presenting a fantastic archaeology. It leads us through the history of these imaginative interpretations to a recently revived approach, Psychic Archeology. The author does this in good style, providing us along the way and in an epilogue with a summary of "North American Prehistory–The Real Fantasy." Williams's is a fantastic book that supplements nicely Robert Wauchope's *Lost Tribes and Sunken Continents: Myth and Method in the Study of American Indians* published in 1962.

The lead review in the July, 1991 issue of *Scientific American* began:

> Sweetness and good humor grace this well-illustrated work by a senior Harvard archaeologist/anthropologist, teacher and museum curator. Without those traits in abundance, this questioning journey along the wild shores of wistfulness might leave the reader little but the sourness of disillusion. Williams, however, starts with a generous

> premise. "Without fantasy, science would have nothing to test." Curiosity and the imagination it kindles comes first, but it is stringent testing and veracious reporting that build a science.

My own brief review of *Fantastic Archaeology* is published in Anthro Notes, *National Museum of Natural History Bulletin for Teachers* 13:3, fall 1991:

> Williams, Stephen 1991. *Fantastic Archaeology: The Wild Side of North American Archaeology*. University of Pennsylvania Press.
>
> This volume by Stephen Williams, a distinguished archaeologist at Harvard University's Peabody Museum, is one result of some 45 years concentration on the study of American archaeology from the first migrant invaders into the North American continent. It has the broadest coverage and is the most intensively researched study of the multitude of demonstrably false interpretations, and contrived fakes made in the recent past for money, fame, or notoriety or to form an insecure, sandy foundation for an ethnic group or sect.
>
> Williams emphasizes how responsible professional archaeologists investigate new sites or artifacts to test the validity of statements about them by innocent finders or manipulative quacks, rogue professors, and downright scoundrels. Fakes or frauds have been found in at least thirteen states, in some of them their manufacture almost seemed to be an industry. In Canada, perhaps the most famous misinterpreted find was the Beardmore relics, which were genuine Norse items but not evidence of Norse presence in llth century America, a view which the uncritical curator of archaeology in the Royal Ontario Museum in Toronto had accepted. There are commentaries on many well-known and lesser known artifacts of non-Indian manufacture and on misinterpretations by laymen and professionals about the antiquity of man in the New World, or on intrusions of civilized groups from the Old World bringing real "culture" to the savage natives.
>
> Williams is particularly critical of some former Harvard professors, including Leo Wiener who was one of the early instigators of the idea that Africans had an important influence on prehistoric American cultures. This is simply not true. Non-Harvard professors from North Carolina and California are included in his presentation of individuals incapable of evaluating evidence.
>
> This is a book to cherish and enjoy. The book demonstrates once again how many ways there are for people to mislead other people, particularly in areas where emotions become involved in objective assessments of scientific evidence.

This chapter is, I hope, an adequate demonstration of the respect and admiration I have for a longtime colleague whose major area of field research is close to mine. Since 1949, Steve and I have had a continuing association unbroken by petty squabbles or attempts at upmanship or coup counting. Since my retirement, he has many times gone out of his way to involve me in contemporary affairs. These have served me well in keeping me alert and working on tasks that I can do. And so I conclude this contribution with these well-known words: he is a gentleman and a scholar.

2 Reminiscences of a Minneapolis Twin

Philip Williams
Northport, Michigan

The names of our parents were Clyde Garfield Williams, born in April 1879, and Lois Miriam Williams, born December 22, 1882. They were married in 1912. We had two older brothers, Daniel, born in 1913, and a second brother, Rodney, born in 1916. Stephen and I were born on August 28, 1926. Our mother and father were quite old when they were married–Father was thirty-three, Mother was twenty-eight–so when we were born, they were both well into their forties, and our names, Philip and Stephen, were always joked about as being a "postscript," or P.S.

Our parents came from quite different backgrounds. Father came from Redwing, Minnesota, and he was one of two children. He had a sister named Maud a couple of years older than he who became a teacher in the Minneapolis school system, where she taught in the elementary grades for some forty-five years. Our grandfather, Charles Williams, left our grandmother in approximately the middle 1880s, and our grandmother taught school at the Redwing Boys' Reformatory, we are told. Our father had less than an eighth grade education, as he went to Minneapolis, we are told, when he was approximately twelve years old to help support the family. He started out in a job working for a dentist's supply company, where he delivered dental pieces to various doctors around the downtown. Eventually Dad got into the grain brokerage business and eventually was an officer of a company called the Brown Grain Company. This was a commodity trading company, not like we have now, but on a much more minor scale, and Dad did quite well. He was basically self-educated because of his lack of schooling, and he loved to read.

Our mother came from a considerably different social world. Her father and mother were originally from St. Louis. Our grandfather was named Chester Simmons and he married Fanny Bemis. Fanny was the daughter of Stephen Allen Bemis, one of the founders of the Bemis Brother Bag Company in St. Louis. Chester Simmons was from a well-to-do family in Webster Groves, Missouri, and after they got married, they moved to Minneapolis around 1880, where Chester Simmons was the manager of the first branch of the Bemis Bag Company. The original plant was in St. Louis; Minneapolis was their first branch because of the new, growing flour milling industry there. Eventually, Chester and Fanny built a substantial house on Park Avenue, where all the other business leaders of Minneapolis lived, including the Pillsburys, etc. Mother was one of six children; three girls and three boys, and she was the third child.

Because our parents were so old when they married, Stephen and I knew none of our grandparents except one, Fanny Simmons, who died when we were approximately five years old. The family relationships within the Simmons family seemed to have been strained as several of the children, particularly Emily, the youngest, was a very free-spirited person who, back in the early 1900s, worked at the settlement house, Hull House, in Chicago, married a writer, and was considered very "Bohemian." She eventually lived in Washingtonville, New York and to her last days was a very involved person in civil rights activities, etc. Her sister, Ethel, who never married, was also a very bright and engaging person. She had a very questioning mind and I'm sure that both Ethel and Emily, whom Stephen and I both loved very much, had some deep impact on our lives. Unfortunately, our mother developed, from having a series of mild strokes in her sixties and seventies, a certain amount of memory loss and, although Stephen and I were never really fully aware of it until our father died, we realize that she had some significant mental problems in the last years of her life.

Our father was a very gentle person, and, as I've said, self-educated. Our mother probably was the stronger of the two personalities as long as she was healthy. She did not have Alzheimer's, because for the rest of her life she was always very, very vigorous in terms of planting gardens. Two things I think that our parents gave us very strongly was a love of gardening, which both Stephen and I still have, and from our father we got a deep interest in watching birds and animals.

Because our brothers were so many years older than we, Stephen and I really grew up together. Our oldest brother, who was some fourteen years older, was going to college when we became ready to go to grade school, and our second brother, Rodney, was mentally retarded from birth due to something called the "Williams Syndrome," similar to Down's Syndrome.

One subject I think worth mentioning is the closeness that Stephen and I have had for all these years. It might seem strange that in his recent book, *Fantastic Archaeology*, there's no mention of me. But this is not strange because we have a closeness that does not require any mention, and it's a very special relationship. There's never been any contest of wills, no fights in all of our life, much to my children's disbelief, because in their lives sibling rivalry was and is still a part of their growing up. Even though we are close, this does not mean that we are not our own person; we've both lived quite different lives. I was married in 1948, Stephen many years later, and we've always had a sense of self that we somehow developed. During our growing-up years we were dressed alike, and we obviously, being identical twins, looked very similar–it was difficult to tell us apart (Figure 2.1). Years later, after

Figure 2.1. Stephen and Philip Williams at about age two. Stephen on the left, Philip on the right.

having forty-three years of my wonderful wife's cooking, I am considerably heavier than my brother, but in many ways, particularly our voices, we are very similar. One of the things that might characterize this relationship is the fact that we started sailing in 1939 on Lake Minnetonka in an X-boat, appropriately called the "Postscript." We learned to sail, and in each race, one of us was the skipper and the other was the crew. Every other race we changed places. That may seem strange because the skipper and the crew are often known to have significant disagreements as to what to do. But Stephen and I never had those kinds of fights, and we saw this switching of places as a very ordinary sort of circumstance in our relationship. It was interesting that in the year 1941 we had a wonderfully successful year in sailing the X-boats. We won the best trophy for the X-boat class at Lake Minnetonka, which is a very large and very old yacht club, and we also won third place in the Inland Lakes Yachting Association Regatta. We shared these triumphs together because we had won them together.

We moved from a house on the Mississippi River in approximately 1932, about the time our Grandmother Simmons died. I believe Mother inherited some money then so they could move to another part of town, which was 4225 Fremont Avenue South, near Lake Harriet. We started our schooling at the Clara Barton School, a public school in the Minneapolis school system where we went through the fifth grade. In September 1937 we entered the Blake School at Hopkins, Minnesota, a boys' country day school, which I know affected my life much more than my Princeton education. The seven years that Stephen and I spent at Blake were very formative. We were lucky to have superior teachers and it was in all ways a very wonderful education as well as a place to grow up. Sports were taught in an organized way from the sixth grade, so Stephen and I played organized football, baseball, and skied in those early years between the sixth and the eighth grade. We also started to play basketball at Blake, but not under what would be considered the best conditions. We were coming out of the Depression, and building monies were scarce, so while Blake had a field house, it was a dirt-floored, unheated building. Knowing the kinds of winters we have in Minnesota, it was not exactly the most wonderful place to play basketball, yet that's where we started–in an unheated, dirt-floored field house. Eventually, particularly in our class, a number of us liked basketball, and so we talked the administration into letting us practice in some of the small church gymnasiums around the area. By the time of our senior year, we did have a basketball team, officially. It was at this time, as Stephen mentions in his book, *Fantastic Archaeology*, that our interest in archaeological phenomena developed. He specifically mentions reading the books on the Land of Mu. I read these books at the same time with Stephen and developed some of the same interests.

Figure 2.2. Stephen Williams during his navy year, 1944-45.

We both joined the Navy in May of 1944 and were discharged in 1945 (Figure 2.2). I was discharged a little earlier than Stephen, and we both selected Yale as the college we were going to attend. Of course, during the war years we had not toured the colleges, so we really had no first-hand experience with any of these colleges, and we simply chose Yale because some of our classmates had also made that same selection. When I came back from the Navy, I talked to one of my teachers at Blake, Mr. Bill Bryan. He had graduated from Princeton and suggested that I go there. So I changed my application and got into Princeton immediately for a November start. Stephen came back from the Navy a little later than I did, and one of our close friends, Gordon Ritz, had gotten out of the Canadian navy at the same time, so Stephen decided to go to Yale with Gordy. We had become separated during our navy careers, and so going to different colleges was not a particularly wrenching experience for us. As I've said, we were raised as individuals and felt very close to one another, but could go in different directions without great difficulty. I'm sure these kinds of statements seem strange to people who are not twins, particularly the kind of closeness that Stephen and I had, and yet we did this freely without any great trauma.

I don't recall exactly when Stephen started on his direction to getting a degree in archaeology, but I know that at Princeton I started out looking for courses in archaeology. I found them lacking since there was no department of anthropology. Eventually I majored in philosophy and minored in geology because that was the closest I could get to archaeology at Princeton. Stephen got into the department of anthropology at Yale, I know, well into his sophomore year.

I would like to relate our first digging experience. It was in the summer of 1947. Stephen and I took a course from Lloyd Wilford of the University of Minnesota that involved the excavation of Indian mounds around Lake Minnetonka. This was our first field experience, and this was the first time that Stephen ever actually did an Indian dig. This was about a four-week course. We moved an awful lot of dirt, but we didn't find many wonderful pieces of pottery or anything else, but it was an enjoyable experience, and I'm sure helped direct Stephen in his move toward becoming an archaeologist.

In a letter from Dr. Griffin I was asked about some reasons Stephen might have chosen the path of being a teacher. Perhaps a overall view of the family educational background might be instructive. As you can see from some of his history, his grandmother and aunt had been teachers. Our mother, coming from a well-to-do family, was the first one to ever go to college. She went to Carleton College in Northfield, Minnesota in about 1905, but she only stayed one year. I don't believe either Stephen or I know why she decided to come back to Minneapolis after that one year and did not complete her college education. Our brother Dan got the first family degree; he went to Lehigh University and graduated in 1937 with a degree in business. Interestingly, after Dan was in business for a number of years, at the age of fifty or so, he left the business world, went back to school, got a master's degree, and ended his career as a teacher and assistant superintendent of schools in Whittier, California. Dan's change of career occurred after Stephen had already set his sights on getting a Ph.D. I had also some feelings of moving that direction–considered perhaps being a teacher in philosophy–but I got married my senior year at Princeton and have been involved in business the rest of my life, so I never got to go into the teaching world. Stephen was the first one in the family ever to get a doctoral degree (Figure 2.3). In our children's generation I have a son who has a Ph.D. in theatre with an M.F.A. from

Figure 2.3. Dr. Stephen Williams with his father at completion of Yale graduate studies, June, 1954.

the University of Minnesota and an undergraduate degree from the University of Miami. He has tried to get into teaching and has taught for one year at the college level, but currently is now working in the window business because he could not find a job in the academic world, but that's where he wants to end up, if possible. My oldest son, Jud, is in the business world, selling commercial real estate in Baltimore, Maryland, but it is interesting that he feels that he would like to eventually get a masters degree and teach, as he feels that he is not giving enough of himself through his commercial business world. We'll see if this develops or not in the years to come. My daughter Lucy got a B.A. degree from the University of Denver in Elementary Education, and son Christopher has taught computer courses at Toledo University. It seems to run in the family.

I've searched my memory to try to determine why both of us felt that teaching might be a career. I can remember no encouragement that either of us got from our parents. There certainly was no stigma on teaching, but I do know that it has always frustrated Stephen a little bit that our dear father always felt that my going into business was a more appropriate direction than Stephen's, which involved getting a Ph.D. from one of the finest colleges in this country as well as a master's from the great University of Michigan, and then becoming a professor at Harvard, certainly one of the better universities in the United States. I know that Dad felt proud of Stephen's success, but his values put more importance on business than teaching. This is not to say that Dad didn't strongly support Stephen's directions, and there certainly was no hint that teaching wasn't a good occupation; it just wasn't as good a direction as business.

In trying to look back at the interest in education, I've tried to express in my earlier sentences the importance of the Blake School experience. This was one where the classes were small, the contact with the teachers close, and even though we were going to high school in the war years, it didn't diminish the satisfactory relationship we had with education. I know Stephen enjoyed his years at Yale and had some excellent professors who obviously stimulated in him his interest in archaeology as an undergraduate, so that moving on to a masters and Ph.D. seemed like a reasonable continuation of direction. I also think that, beyond Blake, the fact that there were personalities in the family like Aunt Ethel and Aunt Emily, who valued intellectual stimulation and discussion, were strong factors, too.

Perhaps another word on our "social environment" might be of interest. I've indicated that our father was basically self-educated, and he continually wanted to learn about new areas of thought. He read many, many biographies and also seemed to be very interested, as I've indicated earlier, in birds and animals. He learned to identify all the trees and birds that were indigenous to Minnesota. Because Dad had no formal education, one of the figures in his life that was important was a great uncle who had been a doctor in the Civil War. Dad had a great interest in and respect for doctors, so while he was in the grain business, his closest friends during the rest of his life were not from his business world, but were doctors. He spent many of his years with them after he retired, which was at sixty-two when he had a major heart attack. Many of them were surgeons, and he relished his relationships with them. In his retirement years, he and his doctor friends would seek out places where there were rare trees or birds nesting. This inquiring attitude is something I'm sure both Stephen and I learned from our father. He was, as I said, a gentle person, and we learned by watching rather than being told; this was the thing to do.

To sum up, if I was trying to list the reasons that Stephen went into the academic world, they would include especially the influences we got from the Blake School and from our father and our other relatives who had been in education and who put value on that kind of service. I use the word "service" here only to emphasize the kinds of social responsibility which I think both Stephen and I learned from our parents as well as our aunts Ethel and Emily.

As a twin brother, I have basked in my brother's success in the academic world. I proudly learned of his positions at Harvard, as head of the Peabody, and those kinds of things. I am very proud of his career and am excited about the publishing of his first book for the general public.

Our lives have gone in two rather diverse directions since I went off to Princeton and he off to Yale; I marrying young and having four children, Stephen marrying later with two children, but while the distances have been always great from where we ended up living, we still have been very close in many ways. I think it's difficult for one to understand how special the twin relationship is; I know that for the first few years of my marriage to Nancy, she couldn't understand me the way Stephen had understood me, and I know I made it difficult on her by not verbally explaining how I felt–because I'd never had to with Stephen. Even today, we do not call each other on a regular basis. But this does not mean that we aren't close or don't feel close to one another; it's simply a matter of such a degree of closeness that the phone calls aren't necessary to keep us in a very special close relationship.

3 Constructions in Form, Space and Time: The Other Career of Stephen Williams

J. Cynthia Weber
Weber Works - Capital Projects and Development
New York City

Archeologists usually consider their evidence to have three dimensions: Form, Space and Time.
Stephen Williams (1991a:18)

Stephen Williams' teaching and research as Peabody Professor of American Archaeology and Ethnology and Curator of North American Archaeology for the Peabody Museum at Harvard have been the foundation of the other papers in this tribute volume. But for ten years, 1967-1977, turbulent times for Harvard, academia, and American society in general, Stephen Williams was also the Director of the Peabody Museum. As the first Director in the Peabody's second century, he planned and brought about the revitalization of the Peabody as a university teaching museum.

As important as are the intellectual constructions Williams has made as an archaeologist using the dimensions of form, space, and time, it is as a master planner-architect-builder, as the originating force behind the Putnam Laboratory and the Tozzer Library, those constructions in form and space, that he made, arguably, his greatest contributions to anthropology and certainly to Harvard. They are the evidence of the achievements of his other career as Director of the Peabody Museum.

THE FIRST CENTURY

With the George Peabody gift in 1866, Peabody Museum, the first museum of anthropology in America, was established. In the original instrument of trust and letter of gift, as summarized by Williams (1967:1-2), "Peabody specifically allocated the original funds for collecting and preserving anthropology specimens, for a working library, for a professorship in the subject and for a museum building. He further suggested immediate research in the fast-disappearing remains of the American Indians."

It was the concern of the first directors to gather specimens, and the Annual Reports reflect their pride in the sheer numbers of accessions. Certain directors, notably Jeffries Wyman (1866-1874), the first director, and Frederick Ward Putnam (1875-1915, successively Curator, Honorary Curator, then Honorary Director to his death in 1915), also reported scientific data.

As Williams further commented, "it was an educated sense of collecting and exhibition of the material that began the Museum and actually set the physical requirements of the original building and its additions" (Williams 1977:2). The Peabody, "a factory-style, commodious and lovable building of red brick" (Williams 1969:7), was begun in 1877 and completed in three stages by 1915.

As the collections grew, students were attracted to the Museum. Under Frederick Ward Putnam, who also was the Peabody Professor of Archaeology and Ethnology (1886-1909), the first instruction in anthropology at Harvard began. As teaching became more important in the Museum's functions, a separate university Division was established in 1902 in conjunction with the Museum, the forerunner of today's Department of Anthropology at Harvard. By 1967 the Peabody had sheltered anthropology for nearly ninety years and was long overdue for renovation.

APPRENTICESHIP

Stephen Williams came to Harvard in January, 1954 and, after receiving the Ph.D. in Anthropology from Yale in June of that year, began a National Science Foundation post-doctoral fellowship working under Philip Phillips, Curator of Southeastern Archaeology (1949-1967).

In 1970, to mark Phillips' seventieth birthday and the publication of the "Archaeological Survey in the Lower Yazoo Basin, Mississippi, 1949-1955," Williams wrote:

> It was a warm spring day, as I recall it, late in May of 1951. I'd been in correspondence with Phil about the work I was doing in Southeast Missouri under the auspices of both Yale and Michigan. He invited me up for a visit and I pulled into the Bolton driveway in mid-morning after the trip from New Haven. Little did I realize that the direction of my life would be bent irreversibly in the next few moments, for Harvard and the Peabody Museum were not what was to bring me to Cambridge three years later. It was the opportunity to be part of the Lower Mississippi Survey under Phil's tutelage that made it so attractive to me . . . . What the following nearly twenty years have meant to me both professionally and personally can hardly be expressed (Williams 1970).

Phillips' tutelage extended beyond archaeology. He had trained originally as an architect at the Harvard School of Design and was in practice prior to a return to Harvard for a Ph.D. in Anthropology. He would become a sounding board for the ideas and plans Williams would later develop.

Williams filled in for Phillips in 1957, teaching North American archaeology. When in 1958 the Harvard faculty mandated the sophomore tutorial, it fell to the younger staff in the Department to carry out the mandate. Edward Hunt taught what would now be called biological anthropology, while Williams taught social and physical anthropology and archaeology. He became an Assistant Professor in 1958.

By the 1950s decades of collection by expedition or purchase or gift filled the Peabody from basement to attic with the treasures and the commonplace from around the world, which encroached upon exhibition space and crowded into the spaces begrudged for classrooms. Under Director J.O. Brew (1948-1967) the maintenance of the museum collections began to be addressed through the "Storage Committee," and a complete inventory of the collections was undertaken, with Phillips responsible for the Eastern North American material and Watson Smith for the Western. Brew saw Williams' interest, and he participated in the rediscovery of the Peabody collections, in particular the clearing out and cleaning out of the attics. Around 1956, Dr. Brew requested him to do the first space and function analysis of the Peabody.

Working and thinking like an archaeologist, Williams measured, scaled, and mapped the "site," identified the uses to which spaces were put, calculated the area devoted to teaching, exhibits, storage, halls, departmental administration, and museum operations. His analysis recorded exhibits as taking up 40-50% of the space, while the Department and teaching occupied only about 10%. This disproportion could have been greater, since public exhibit space, such as the (infamous) Second Floor, had already been closed to provide student research space.

> There was obvious pressure to increase facilities for teaching. I and they [Dr. Brew

> and staff] felt it [the museum] was full, but with the reorganizing of collections, storing them carefully, imposing some order, we could gain space.
>
> I also realized that the museum had corners, with materials stored that had never been mixed in context. Any re-storage had to preserve the contextual association and information (Williams 1991b).

And further,

> I saw the fortunate resiliency that anthropological materials have, at least at Peabody, in spite of how long they have been neglected and perhaps because of such neglect. Being left alone, undisturbed in sealed cases almost impenetrable except for light, materials held up extraordinarily well. Even the dim gloom of the exhibit halls was a benefit. The materials in cases were astonishingly O.K. The sealed cases buffered environmental changes. We did hygrometer readings showing very little fluctuations of humidity. Faded, yes.
>
> In my visits to other old collections such as at the AMNH (American Museum of American History), the condition of the material in the old cases was similar.
>
> Our storage now has to balance conservation and use (Williams 1991b).

In a follow-up to the space and function analysis, he began to design storage space for some of the Peabody material. While at Yale, he had done similar storage design for Cornelius Osgood for skeletal material and some whole pots–a smaller diversity of material and an easier task than he would later have to deal with–but also one approached with preliminary measurement and subsequent practical work with a carpenter. "After all, skulls are pretty much the same size" (Williams 1991b).

Work with Osgood also developed Williams' exhibition eye, a sense of design and information balance, an integration of object, label, and visual flow, as well as an attention-grabbing focus. Osgood understood and encouraged these exhibition qualities when he told his students to look at department store window displays. This foreshadowed current museum exhibition concerns where "design is absolutely critical in shaping the relationship of modern day visitors to the historical and anthropological subjects being interpreted" (Rabinowitz 1991:37; also see Volkert 1991).

Work with Robert N. Hottvedt, a planner/architect with the firm Johnson, Hottvedt, undertaken in Williams' first years as Director would complete his apprenticeship. With Bob Hottvedt, he entered a professional and personal relationship that, until Hottvedt's death in 1972, would structure and hone his own instincts and experience. The objectives developed from that planning partnership provided a framework for his directorship for the next decade.

Williams commented in 1991 on the planning approach that developed from the synergy of his anthropological background with that of the MIT-educated Hottvedt:

> Museums need help with planning, very few are single purpose, single function, but combine research and outreach. They are interactive.
>
> You must take a holistic and long term view. Once you've done something, other options are foreclosed. You can't do room-to-room; you must think through the long range implications.
>
> You must think through the functions–not just collection storage–but preparation

> rooms, conservation facilities, workmen's lockers, access, HVAC and maintenance as well as space.
>
> You must consider the heterogeneity of activity. Space must be as multifunctional as possible.
>
> In anthropological storage, there are problems of heterogeneity of size and materials with combinations of organics and inorganics with differing stabilities.
>
> Have to ask what kind of access do you want? Do you want long things? Or all of a kind things, like baskets? These aren't modern questions. But many museums–old and new–still store that way.
>
> Anthropologically you want it in context, the grave lot, the site, by tribe or group, by area. But the materials are heterogeneous, so material conservation needs to take over. Shelves and drawers are the standard answers. Some museums have adapted art museum's vertical screens for certain material. But it hangs, bangs and puts strain on the objects.
>
> Bob Hottvedt suggested that an auto parts store, a microfiched, computerized VW parts storage and retrieval system, would be a useful model for museum storage (Williams 1991b).

PUTNAM LABORATORY

The mid-50s space and function analysis undertaken by Williams documented the need for more teaching facilities at Peabody. And in the 1960s, when the National Science Foundation responded to a post-Sputnik national sense of educational crisis by instituting a program for upgrading undergraduate teaching facilities, specifically laboratories, Williams took advantage of the opportunity. The NSF had physics, chemistry, and biology in mind, but Williams envisioned the funding of an undergraduate teaching/research laboratory for the scientific analysis of the material objects of archaeology and ethnology. The Putnam Laboratory was certainly the first, and is likely the only, use of that NSF program for an anthropological facility.

Williams began to think about a research facility for archaeology in the early 60s as part of an emerging view in the profession that the totality of past life was capable of being investigated. "It was always a desired end but now you had the belief, the confidence that it could be done. Science and technology were the tools to reconstruct more than time and space. Remember that time itself, through C14, was a recent definable" (Williams 1991b).

As a post-doc, Williams had been sent by Brew to an NSF conference on the identification of non-artifactual remains. Participants there rejected the "black box" concept of a research center–where archaeologists would put artifacts in and get data out–in favor of regional centers with scientists, anthropologists, and students involved in the examination and analysis of the material.

> If you don't know the capabilities or limitations of the techniques, it leads to unwarranted reliance on or expectation of the methods. Archeologists also must know what methods and techniques are available or they will not collect the category of material or collect it in the proper fashion (Williams 1991b).

The Putnam Laboratory was constructed during the summer of 1966. It had reference collections of faunal, lithic, and ceramic materials: it had microscopes and facilities for micro-photography, aerial photo interpretation, and a dark room; it had a safety hood, equipment, and the chemicals to undertake simple inorganic analyses; it had sinks, racks, and shelves to allow processing of excavated materials. It was immediately used for undergraduate courses in archeological methods and for honors thesis research. Graduate students used it as well.

> The Putnam Laboratory was, in a way, taking a step back to the breadth of mind of Frederic Ward Putnam and a tribute to it. In my explorations of the museum I kept coming in contact with what Putnam had done. He had made faunal collections, even bringing back the bones of an in town duck dinner, and lithic collections, devoting much attention to the discernment of the artifact from the 'naturfact'–the result of natural processes or chance. He had interest in teaching and in archeological and anthropological research in the broadest context (Williams 1991b).

Putnam also had astonishingly modern methods of excavation and record keeping: "actually keeping broken sherds and animal bones . . . not a common practice in the 1870s, even in Europe" (Williams 1991:69). Under his direction, "(e)ven his somewhat illiterate digger, Edwin Curtis, kept notes on every burial, drew maps of his sites, and carefully packed the materials for shipment back to Cambridge. Those artifacts can still be put back in their original burial context through the use of catalogues and field notes" (Williams 1991:69).

The Putnam Lab facilitated the interdisciplinary scientific approach to anthropological materials at Peabody. It became the model for the establishment of other interdisciplinary laboratories and collaborations such as the current work between biological anthropology and archaeology, where data from human and animal osteology leads to reconstruction of diet and the tracing of the early development of animal domestication and agriculture in the Near East.

The Putnam and other archeological research facilities at Peabody underwent a major upgrade in capabilities through the generosity of Landon T. Clay as part of the establishment of the Landon T. Clay Professorship of Scientific Archaeology in 1983.

Peabody has become the locus of increasingly sophisticated archeometry and materials research using inductively coupled plasma/mass spectroscopy, stable isotope spectroscopy, and other state of the art techniques. Interdisciplinary collaboration in technique development and application has extended to other Harvard departments, for example, studies in petrography with Geology and metallography with Metallurgy. Particularly fruitful interaction and sharing of equipment exists with the Department of Earth and Planetary Sciences, conveniently located in the adjacent Hoffman Building, now physically connected to Peabody by a bridge, where joint ownership and operation of spectrometers avoids duplication of expensive resources and technical support.

Harvard courses in scientific archaeology are available at the undergraduate and graduate levels, and knowledge of scientific archaeology is expected of candidates for graduate degrees. The Peabody attracts archaeology students and researchers from throughout the U.S. to utilize its facilities and influences international research development as well through the current Landon T. Clay Professor of Scientific Archaeology, Nikolaas van der Merwe. This began, however, with Williams and the Putnam Lab.

THE TOZZER LIBRARY

In 1967 Williams became simultaneously the Director of the Museum and the Chairman of the Department of Anthropology. He had the opportunity and the authority to address the problems of the Museum and the Department together. He could make and formalize space and function allocations. And Harvard's Program for Science in Harvard College (the Science Drive), when completed in June, 1969, was to allocate some two million dollars toward the needs of anthropology at Harvard.

Williams ordered a space and function study of his own. It was then he engaged Bob Hottvedt to assist him in broad gauge and long-range planning (Williams 1968:5) to focus on long-standing problems and future objectives and needs of the Museum and the Department (Williams 1967:1). As background for that planning, Williams worked with staff and faculty to define goals for the Peabody and the Department in the three basic functions of the institutions: Curatorial ("to act as the permanent repository for a collection of priceless anthropological materials . . . as well as to maintain a comprehensive library of anthropological literature"), Teaching ("students on all levels in the major fields of anthropology"), and Research ("in all fields of anthropology") (Williams 1967:1).

In discussing the Curatorial aspect he noted the concerns were of equal strength for the artifact collections and the library, then a one-room reading room with cramped stacks and shelves.

> The Museum's collections, material and library, remain the essential cornerstone and *raison d'etre* for the form of the Museum and Department. First there was the Museum–then a Department of "Science" to utilize the Museum's holdings . . . . One can underline three major areas of continuing need: Preservation, Accession and Accessibility. We must conserve the materials we have, we must add judiciously to these holdings, and we must provide access to them (Williams 1967:2).

In the discussion of Teaching and the space to be given to it he considered the problem of the function of the Museum's exhibits.

> To be candid, the question of how much exhibition space and the level of approach–instructional vs. public display–is the major "bone of contention" among the staff.
>
> I myself consider that priorities have been established by: (1) the original deed of gift, (2) the fact that this is a University museum, and (3) the preservation needs of the Museum's collections. Thus a modest exhibit program, not carried out at the expense of our major functions of research, teaching and curatorial activity, would seem to be the most rational approach to what, I must confess, is a serious and far reaching dilemma (Williams 1967:3).

As for Research, noting the historical shift from private to government sources for the support of research and the richness of such funding in the late 1960s, he commented, "How long this opulence will continue is anyone's guess" but came down on the side of confidence, "thus, financial support for 'research funds' does not loom as a major problem" (Williams 1967:4).

Williams and Hottvedt undertook an extensive survey of activities and space-use in anthropology, incorporating Peabody-specific information gathered from interviews with the Peabody staff and broadening their perspective through a cross-country museum tour looking at major anthropological museums in Milwaukee, Chicago, Berkeley, Washington, and Philadelphia. They produced a 177,000 sq ft architectural Master Plan for the museum that called for renovation of the existing building (100,000 sq ft), and construction of a library (18,000 sq ft), an ethnographic research facility (some 35,000 sq ft), and an exhibition facility (30,000 sq ft), to be accomplished in that order (Williams 1967:5).

> A need for the complete rethinking of the priorities and the sequence of planning came to the fore in November, 1968, with the magnificent million dollar gift for the

> Tozzer Memorial Library. With its receipt the second phase of the program was assured even before the Science Drive goals were partially reached and the immediate reaction was one of relief to know that one could postpone the renovation schedule so that the books could be moved directly from the old building into the new structure at its completion (Williams 1967:5).

The next year was spent in exciting and satisfying planning of the structure that would become the Alfred Marston Tozzer Memorial Library, consideration of whether the ethnographic facility could be included, siting studies for optimum use of the quadrangle to the north of the Peabody (where the Tozzer is now located), negotiation with the Museum of Comparative Zoology for use of the quadrangle, separately or jointly, and proposals to the Harvard administration to reallocate Science Drive funds from renovation to new construction to finance the addition of the ethnographic facility to the library.

But the "winds of change" to which Williams had alluded as he started his directorship and began to lay out the goals, his goals, for the Peabody and the Department (Williams 1967:1), began to blow chill even for Harvard. The Science Drive would not reach its goal. And Williams reported,

> during August 1970 design development (for the combined library and ethnographic structure) . . . was completed . . . . In September working drawings, the final stage of design, were started, and by March 1971 they were completed, ready to go to bid to a contractor. The date was a bit later than originally scheduled, since by December it had become apparent that funds would not be available to go ahead as planned.
>
> The Program for Science in Harvard College . . . was reoriented in December to new goals, . . . but the Museum was not included . . . . The Museum still hopes to build its much needed wing . . . although it is now unknown when the new construction will begin (Williams 1971:7-8).
>
> The next year saw consultation with then new President of Harvard, Derek Bok, and other members of the administration which made it obvious that we could not build the larger structure on the quadrangle site, both for reasons of finances and for reasons of changed criteria in the use of space. These exigencies made total reevaluation necessary, but, as a result of funds which became available through a generous bequest to the Museum from the late Francis Boyer, the year closed with a real hope for the future. These funds will enable the Museum to build the Tozzer Library alone (Williams 1972:8).

In 1972-73, plans were drawn up for a smaller building (Williams 1973a:8). Ground breaking ceremonies for the new library took place in May, 1973 and on October 21, 1974 the Alfred Marston Tozzer Memorial Library was formally opened to scholars, researchers, and students. It was and is one of the world's foremost anthropological libraries.

Was the Tozzer, was being Director of the Peabody, worth the sacrifice of his own scholarly work? President Bok (1977) noted it as such in a tribute to Williams at the conclusion of his decade-long directorship. Read the pride and fulfillment in the humor with which Williams wrote during the summer of 1973, while construction of the Tozzer was underway:

> During other summers I have remained the frustrated archaeologist with no excavations to call my own. This season I have an exposure of Cambridge alluvial sands that ex-

> ceeds most archaeologists' wildest dreams (Williams 1973b:23).

OTHER CONSTRUCTIONS IN TIME

Stephen Williams shares with Frederick Ward Putnam the teaching of a generation of North American archaeologists at Harvard, broad intellectual inquiry in anthropology and archaeology, and the Peabody Museum. Both men were Peabody Professors of American Archaeology and Ethnology. Both brought rigor to the consideration of what constitutes archaeological evidence–Putnam to the recognition of what indeed was an artifact or not, to distinguishing the man-made from the "naturfact" and the use of both as teaching tools to develop his students' critical abilities, Williams to documentary and theoretical investigations leading to the gentle but unflinching exposure of the pseudoscientific logic and constructs of the practitioners of what Williams (1991a:5) calls "fantastic archaeology," and the use of their tracts to sharpen his students' critical abilities. Both led the Peabody Museum into professional eminence, Putnam turning a collection storehouse into a teaching museum, Williams revitalizing it, providing space and facilities for teaching and the research that fires it.

They share more. An account of the life and work of Putnam, published shortly after his death in 1915, concludes

> But of him as a personality how shall one speak? . . .
>
> But best of all was it of a Sunday afternoon, late, to "stop by" and find him with his family and friends before the fire. He would always welcome with the words, "How are you, my dear fellow? What's the good word?" That was the "good word" he was so eager and able to give (Peabody 1915).

Speaking as one who benefited from Williams' "good word" as a graduate student to become the first Keeper of the Putnam Laboratory, and again and again, best of all was it at "tea."

Tea, the daily break at four, that saw a varying number of Dr. Williams' graduate students, an undergraduate advisee or two, a distinguished visitor, a visiting professor, at times Bob Hottvedt, museum artists, staffers, gather in his office–for tea and talk. He, himself, was often too busy to attend. Tea was an oasis of intellectual and social warmth among the cold and long hours spent doing thesis analysis in gray alcoves formed by the dust-rimed glass cases of the closed exhibition hall on the Second Floor or on trestle tables tucked behind storage stacks in dimly lit corners of the museum, the wastelands.

It was a place to discuss what you did that day with people who understood why it was important to spend 12-14 hours a day coding the attributes of space, form, and time of Iroquois pottery pipes, to comment on the migratory patterns of caribou, to mention obscure references that helped complete a bibliography, to be scathingly critical of weak logic or sloppy technique, to debate the "new" archaeology, to attempt to quiet the anticipatory terror of orals, to learn the interpersonal lore of archaeologists, to discuss summer digs and real jobs, to become enculturated to archaeology as a profession. It was a place to receive an introduction to the complementary world of the museum with its plans, budgets and exhibitions.

Tea was also a refuge in unsettled times. Some of us in the late 60s, most of us, marched in the protests against the Vietnam War, debated participation in The Harvard Strike, and together attended the student demonstration that filled Harvard Stadium, and then came back to tea. We listened to the first broadcast of a Harvard faculty meeting during tea and quickly grew as bored as Williams, who heard it in person.

Throughout, a gently mocking amusement of us all with tea–well aware of an archaic ritual echoing privileged and leisured times, evoking the ghosts of the gentlemen scholars of Harvard who would have disdained to consort with the motley lot we were–but always an appreciation of the friendships forming then, and of the mentoring and guidance, of the "good word" of Stephen Williams.

ACKNOWLEDGMENTS

I thank Dr. Williams for a day-long discussion about the Peabody and his accomplishments as Director. I asked to speak with him with this volume in mind but presented to him a pretense of my need for information on the application of science and technology to archaeology in a teaching museum, using the Peabody as a case study. I apologize now for the deception. I thank him for the candor and good humor with which he let me query his intellectual and practical preparation for being Director, his approach to museum planning and operation, the challenges of the curation of the Peabody collections, the origins of the Putnam, the saga of the Tozzer, and his personal evaluation of his achievements as Director, and his one major disappointment–the unbuilt separate exhibition facility.

I also thank Nikolaas van der Merwe, Landon T. Clay Professor of Scientific Archaeology, and graduate students Robert H. Tykot, a recent Keeper of the Putnam Laboratory, and David J. Killick, now at the University of Arizona, for their information on the current state of scientific archaeology at Harvard and for their co-conspiratorship in keeping my real reason for wanting to know from Dr. Williams.

REFERENCES

Bok, D.
1977 Introduction. *From The Director's Desk: 1967-1977*. Peabody Museum of Archaeology and Ethnology, Harvard University, Cambridge.

Peabody, C.
Frederic Ward Putnam. *The Journal of American Folklore* XXVII(CIX):301-306.

Rabinowitz, R.
1991 Exhibit as Canvas. *Museum News*, March/April:34-38.

Volkert, J.W.
1991 Monologue to Dialogue. *Museum News*, March/April:46-48.

Williams, Stephen
1967 *Structure And Function–1967: A Consideration of the Peabody Museum and Department of Anthropology*. Reprinted in Williams 1977.
1968 *Director's Annual Report 1967-68*. Excerpted in Williams 1977.
1969 *Director's Annual Report 1968-69*. Excerpted in Williams 1977.
1970 Preface. In *Philip Phillips, Lower Mississippi Survey, 1940-1970*, assembled by Stephen Williams. Peabody Museum of Archaeology and Ethnology, Harvard University, Cambridge.
1971 *Director's Annual Report 1969-70*. Excerpted in Williams 1977.
1972 *Director's Annual Report 1971-72*. Excerpted in Williams 1977.
1973a *Director's Annual Report 1972-73*. Excerpted in Williams 1977.
1973b The Editor's Scrapbook. *The Peabody Museum Newsletter*, Summer. Excerpted in Williams 1977.
1977 *From The Director's Desk: 1967-1977*. Peabody Museum of Archaeology and Ethnology, Harvard University, Cambridge.
1991a *Fantastic Archaeology: The Wild Side of North American Prehistory*. University of Pennsylvania Press, Philadelphia.
1991b Personal Communication to J. Cynthia Weber (JCW notes of interview, February 19, 1991, at the Peabody Museum, Cambridge).

4 Publications of Stephen Williams

compiled by James B. Griffin

SINGLE-AUTHORED WORKS

1954 An Archaeological Study of the Mississippian Culture in Southeast Missouri. Yale University, Department of Anthropology, Ph.D dissertation. University Microfilms, Ann Arbor.

1956 Some Engraved Shell Buttons. *Southeastern Archaeological Conference Newsletter* 5(1):31-32.

1956 Settlement Patterns in the Lower Mississippi Valley. In *Prehistoric Settlement Patterns in the New World*, edited by G.R. Willey. Wenner-Gren Foundation Publications in Anthropology 23:52-62. New York.

1957 The Island 35 Mastodon: Its Bearing on the Age of Archaic Cultures in the East. *American Antiquity* 22:359-372.

1957 Obituary: James Kelly Hampson, 1877-1956. *American Antiquity* 22(4):398-400.

1957 The Problems of the Large Research Institution. In *The Identification of Non-Artifactual Archaeological Materials*, edited by Walter W. Taylor. National Research Council Publication 565:18-19.

1961 Discussion of "Relationships Between the Caddoan Area and the Mississippi Valley" edited by James B. Griffin. *Texas Archeological Society Bulletin* 31:40-43.

1961 A Short History of the Southeastern Archaeological Conference. *Southeastern Archaeological Conference Newsletter* 7(1):2-4.

1961 *A Paperback Bibliography of Anthropology and Related Subjects.* Peabody Museum Publication. Cambridge, Massachusetts.

1961 Historic Sites in the Caddoan Area and Documents Thereon. Proceedings of the Fifth Caddoan Conference. *Texas Archeological Society Bulletin* 31:122-130.

1962 Roundtable on Ceramic Classification. Proceedings of the 16th Conference. *Southeastern Archaeological Conference Newsletter* 8:44-61.

1962 Historic Archaeology in the Lower Mississippi Valley. *Southeastern Archaeological Conference Newsletter* 9(1):53-63.

1963 Report on the 20th Southeastern Conference. *Southeastern Archaeological Conference Newsletter* 9(2):15-18.

1963 The Eastern United States. In *Early Indian Farmers and Villages and Communities*, edited by W.G. Haag, 267-325. National Survey of Historic Sites and Buildings, National Park Service, U.S. Department of the Interior, Washington, D.C.

1964 The Aboriginal Location of the Kadohadacho and Related Tribes. In *Explorations in Cultural Anthropology*, edited by W.H. Goodenough, 545-570. McGraw-Hill Inc., New York.

1966a Historic Archaeology: Past and Present. *Annual Report of the School of American Research for 1966*, 23-25.

1966b Obituary: Antonio J. Waring, Jr. 1915-1964, *American Antiquity* 31(4):552-554.

1967 On the Location of the Historic Taensa Villages. *Papers of the Fifth Conference on Historic Site Archaeology* 1:2-13.

1968 (editor) *The Waring Papers: The Collected Works of Antonio J. Waring*. Papers of the Peabody Museum of American Archaeology and Ethnology 58. Harvard University, Cambridge.

1972 Ripping Off the Past. *Saturday Review of Science*, October, 44-53.

1973 (editor) *The Selected Archaeological Papers of Frederick Ward Putnam*. Antiquities of the New World 5. AMS Press.

1976 Reflections From the Lower Mississippi Valley. *Midcontinental Journal of Archaeology* 1:101-103.

1980 Armorel: A Very Late Phase in the Lower Mississippi Valley. *Southeastern Archaeological Conference Bulletin* 22:105-110.

1981 Some Reflections on the Long, Happy, and Eventful Life of Robert Stuart Neitzel. *Southeastern Archaeological Conference Bulletin* 24:7-9.

1981 Some Historical Perspectives on Southeastern Ceramic Traditions. In *Traces of Prehistory: Papers in Honor of William G. Haag*. Geoscience and Man 22:115-122.

1983 Some Ruminations on the Current Strategy of Research in the Southeast. *Southeastern Archaeological Conference Bulletin* 21:72-81.

1986a Doris Stone: The Pathways of a Middle American Scholar. In *Research and Reflections in Archaeology and History: Essays in Honor of Doris Stone*, edited by E. Wyllis Andrews, V. Middle American Research Institute Publication 57:199-202. Tulane University, New Orleans.

1986b Bibliography of Doris Stone: Her Works from 1930 to 1984. In *Research and Reflections in Archaeology and History: Essays in Honor of Doris Stone*, edited by E. Wyllis Andrews, V. Middle American Research Institute Publication 57:203-209. Tulane University, New Orleans.

1987 Fantastic Archaeology: What Should We Do About It. In *Cult Archaeology and Creationism*, edited by F.B. Harold and R.E. Eve, 124-133. University of Iowa Press, Iowa City.

1988 Some Fantastic Messages from the Past. *Archaeology* 41(5):62-70.

1990 The Vacant Quarter and Other Late Events in the Lower Valley. In *Towns and Temples Along the Mississippi*, edited by David H. Dye and Cheryl Anne Cox, 170-180. University of Alabama Press, Tuscaloosa.

1991 *Fantastic Archaeology: The Wild Side of North American Prehistory*. University of Pennsylvania Press, Philadelphia.

CO-AUTHORED WORKS

1956 Williams, Stephen, and John M. Goggin The Long Nosed God Mask in Eastern United States. *The Missouri Archaeologist* 18(3):1-72.

1965 Williams, Stephen, and James B. Stoltman An Outline of Southeastern United States Prehistory, with Particular Emphasis on the Paleo-Indian Era. In *The Quaternary of the United States*, edited by H.E. Wright and D.G. Frey, 669-683, Princeton University Press, Princeton.

1966 Williams, Stephen, William Kean, and Alan Toth *The Archaeology of the Upper Tensas Basin*. Lower Mississippi Valley Survey, Peabody Museum, Harvard University, Bulletin 1. Cambridge.

1970 Williams, Stephen, and Jeffrey P. Brain *Philip Phillips, Lower Mississippi Survey - 1940-1970*. Peabody Museum of Archaeology and Ethnology, Cambridge.

1981 Belmont, John S., and Stephen Williams Painted Pottery Horizons in the Southern Mississippi Valley. In *Traces of Prehistory: Papers in Honor of William G. Haag*. Geoscience and Man 22:19-42.

1983 Williams, Stephen, and Jeffrey P. Brain *Excavations at the Lake George Site, Yazoo County, Mississippi, 1958-1960*. Papers of the Peabody Museum of Archaeology and Ethnology 74. Harvard University, Cambridge.

BOOK REVIEWS

1952 *Archaeological Survey in the Lower Mississippi Alluvial Valley, 1940-1947*, by Philip Phillips, James A. Ford and James B. Griffin. *Archaeology* 5:2:124.

1955a *The Ruins of Zacaleu, Guatemala*, by Richard Woodbury and Audrey S. Trik. *Antiquity* 29(113):62-64.

1955b *Preliminary Salvage Archaeology in the Pomme de Terre Reservoir Area, Missouri*, by Carl H. Chapman. *American Antiquity* 21(2):192-193.

1957 *Excavations on Cape Haze Peninsula, Florida*, by Ripley P. and Adelaide Bullen. *American Antiquity* 23(2):198.

1958 *Excavations at Kolomoki, Final Report*, by William H. Sears. *American Antiquity* 23(3):321-323.

1958 *Modoc Rock Shelter: A Preliminary Report*, by Melvin L. Fowler and Howard D. Winters. *American Anthropologist* 60:196.

1958 *Archaeology of the Funeral Mound, Ocmulgee National Monument, Georgia*, by Charles H. Fairbanks. *American Journal of Archaeology* 64:304-305.

1960 *The Belcher Mound. A Stratified Caddoan Site in Caddo Parish, Louisiana,* by Clarence H. Webb. *American Journal of Archaeology* 64:304-305.

1965 *Indians and Archaeology of Missouri,* by Carl H. Chapman and Eleanor Chapman. *American Anthropologist* 67(4):1065-1066.

1966 *Archaeological Implications of the Role of Salt as an Element of Cultural Diffusion,* by Richard O. Keslin. *American Anthropologist* 68(2):579-580.

ABSTRACTS

1960 In *Abstracts of New World Archaeology*: 1959, Volume 1.
No. 367, Charles R. McGimsey, An Archaeological Survey of the Greers Ferry Reservoir in North-Central Arkansas.
No. 376, Robert Rands, The Willis Site: Poverty Point–Woodland Remains on the Pearl River, Mississippi.
No. 388, John Witthoft, The Transition Between Archaic and Woodland Cultures.

1961 In *Abstracts of New World Archaeology*: 1960, Volume 2.
No. 115, John C. Ives, The Type Concept: A Methodological Inquiry into Archaeological Classification.
No. 255, Douglas F. Jordan, The Bull Brook Site in Relation to 'Fluted Point' Manifestations in Eastern North America.
No. 537, William R. Bullard, Pithouse Archaeology in Southwestern United States Prior to A.D. 900, with a Description of the Excavations at Cerro Colorado, West-Central New Mexico and a Review and Critique of Southwestern Chronology.
No. 543, David L. DeHarport, An Archaeological Survey of Canyon de Chelly, Northeastern Arizona: A Puebloan Community Through Time.
No. 638, M.R. Harrington, The Ozark Bluff-Dwellers.

PART II

PALEOINDIAN AND ARCHAIC ARCHAEOLOGY

5 Pioneering in the Pleistocene: Large Paleoindian Sites in the Northeast

Dena F. Dincauze
University of Massachusetts
Amherst, Massachusetts

Beginnings have their own dynamics. First efforts at anything are characterized by risks and errors, are defined by contingencies, and are dominated by strategizing. Prehistoric beginnings, such as immigrations and colonizations, have a contentious history in the literature (Ammerman and Cavalli-Sforza 1973; Anthony 1990; Rouse 1986) because each is necessarily unique and archaeological remains of unique events and processes are elusive in the record. Archaeologists rarely identify innovative events in the archaeological record because of poor chronological resolution and underdeveloped criteria for recognizing "firsts." Archaeologists tend to assume that the record essentially represents examples of normal behaviors, instances of widespread classes of remains.

PROBLEM STATEMENT

Even the study of Paleoindians, likely candidates for a number of cultural "firsts" in North America, has been bedeviled by the pervasive normalization of the record by archaeologists. Paleoindian colonization of uninhabited terrain occurred only once in every part of the American continents. Just when that happened is an unresolved issue. For this present argument, I propose to consider that the colonization of northeastern North America south of the Canadian Shield and St. Lawrence lowland occurred within the first half of the eleventh millennium B.P. In that millennium, a time incomparable to the present or recent past in almost every respect, Paleoindian behaviors should have been outside of modern norms. In addition, the Younger Dryas climatic reversal early in the millennium likely presented special challenges to which pioneering people could have responded with risk-mitigating social innovations. I argue here that the abnormal, large residential sites of the northeastern Late Pleistocene could have resulted from unique events of colonial aggregation, the experimental initial steps in the creation of cultural landscapes.

The late Pleistocene centuries were times of high variability in climate as well as in the flora and fauna, both partly dependent on climate. Given the unprecedented environmental uncertainty, we should not expect that interpretations of human social and strategic behaviors can be based on late-Holocene ethnographic analogies. Every attempt at analogical interpretation (e.g., Gramly and Funk 1990; Peers 1986; Storck 1984), even the most sophisticated, is necessarily inadequate to understand this set of initial conditions and behaviors (Kelly and Todd 1988). As Shott states (1990:10), "Paleoindians faced not only a daunting range of rapid and problematic environmental change, but did so lacking the structural support[s]–decades if not generations of accumulated material knowledge and lore, and preexisting land-use patterns–that are taken for granted by modern foragers and which can spell the difference between survival and doom in unforgiving habitats . . . ."

Despite awareness of the exigencies of existence and the unpredictable environments of the latest Pleistocene, archaeologists have normalized Paleoindian behaviors continent-wide, but especially within the Northeast, during the entire unknown duration of Paleoindian sites. The normalization is a relict of the assumptions that similar tool kits indicated closely similar adaptations, and that "big-game hunters" could evade local environmental constraints and maintain a singular adaptation continent-wide (Kelly and Todd 1988; see Lepper and Meltzer [1991] for this intellectual history). A different longstanding kind of normalization is a widespread assumption that whatever the sociotechnical dynamics of Paleoindians might have been, they were environmentally determined. More likely, however, behaviors were environmentally constrained, with risk-minimization strategies salient (Dincauze and Curran 1983). The study of pioneering and colonization demands that the concept of "Paleoindian" be subdivided scrupulously and that the chronological and spatial sub-units be discriminated rigorously. Research in the Northeast is teasing out some perceptions of Paleoindian spatial variation, with regional and subregional differences being exposed (Ellis and Deller 1988; Gramly 1982; Lepper 1988; Storck 1983). Variation in time has only recently become visible, with the recognition of stylistic sequences among the Early Paleoindian fluted points.

Because radiocarbon ages are insufficiently specific about relative site ages within the eleventh millennium (Levine 1990), chronology is currently based on stylistic subdivisions of fluted points. A binary division of earlier and later fluted points is accepted by many researchers, and in some areas it has been extended to a tripartite sequence. Fluted points with parallel or slightly convex sides, resembling generic Clovis styles, are considered earliest; these include the Bull Brook and Shoop assemblages and the north-central Gainey style (Deller and Ellis 1988; Ellis and Deller 1988; Simons *et al.* 1984). The points with deeply concave bases from the Debert and Vail sites in the far Northeast are judged to belong to the early set, but to represent a late modification of technique or style. The Barnes style (Roosa 1977), with a long flute and "waisted" or fishtail base is considered the successor style, apparently coeval with, if not equivalent to, the Cumberland style of the greater Ohio and Tennessee valleys. Considered later than all these is the rounded Crowfield ("pumpkinseed") style, represented also at the Reagan and Plenge sites, which seems to be the final form of fluted point in the Northeast (Deller and Ellis 1988). The sequence is not contradicted by information currently in hand, although the distributions and associations of the several styles make the situation appear more complex than a straightforward succession at the regional scale.

In the area under consideration, east from central Michigan and north from Pennsylvania, Paleoindian finds are numerous, ranging in size from the Bull Brook I site to more typical smaller sites and isolated point finds (Anderson 1990; Gramly and Funk 1990); nevertheless, details remain scarce. Sites occur at or near the modern ground surface where they are subject to erosion, plowing, and quarrying; they are heavily collected, and artifacts are sold and traded widely, so that provenience information is lost. The region's biologically and chemically active soils take their toll on organic materials and spatial relationships. No site known is without significant damage from these agencies, at a minimum. Among the sites excavated, few have been fully reported. The reports are unstandardized as to data included and interpretive terminology employed, and remain incomplete. For example, such information as the area of sites is difficult to find and, when reported, is typically based on assumptions unique to the reporter.

THE LARGE SITE ISSUE

. . . not all Paleoindian sites are alike.

(Gardner 1977)

Several very large, productive sites such as Bull Brook I, Debert, Gainey, Nobles Pond, Shoop, and Vail are major features of the northeastern Paleoindian record. Equivalents to these large sites are not known in the herd-hunting areas of the Plains; Lindenmeier, a possible equivalent, seems different in many significant structural and social parameters (Wilmsen and Roberts 1978). Equally distinct are the large fluted point sites in the Southeast, typically quarry and lithic-workshop sites, which have different accumulation patterns and interior structure. The large non-quarry sites of the Northeast, apparently residential in function, stand in sharp contrast to the background of many smaller Paleoindian sites throughout eastern North America (Anderson 1990; Meltzer 1984).

Here I will consider the big northeastern residential sites only, exploring the idea that they may represent discrete *events* related to pioneering populations–in other words, strongly contingent phenomena. Those with enough data available to support preliminary discussion are Bull Brook I, Debert, Gainey, Nobles Pond, Shoop, and Vail (Figure 5.1). The very large Fisher site in Ontario, although at present only summarily published, seems to be later in time and different in kind (Storck 1983, 1991). The site contrasts in significant respects with the six large sites discussed here in (1) being characterized by a later style of fluted point, the Barnes type; (2) having significant lithic workshop activity based on quarry blocks (cf. the Williamson site in Virginia [Peck 1985]); and (3) having artifact concentrations devoted to special activities (Debert admittedly has a few such). For these reasons, and because Storck (1983) makes a good case for the site being a recurrently occupied anchor of a seasonal round in a band territory, Fisher is not included in the model presented here. With the exception of Shoop, all the large non-quarry sites are within the glaciated area of the Northeast. More details on all the sites are needed before I can be entirely confident about the membership and the usefulness of this postulated set.

The Bull Brook I site, in northeastern Massachusetts, was excavated over a period of years by amateur archaeologists who kept ahead of gravel quarrying operations. Subsequently the collections have been examined by several archaeologists (Byers 1954; Grimes 1979; Jordan 1960); they are currently under investigation at the Peabody Museum of Salem. Forty-two discrete artifact concentrations were found in a roughly circular configuration over an area estimated at 20 acres (acres are used here because they are the only unit reported for some of the sites). The number of artifacts reputedly exceeds 10,000, although no final count has been published. No plots of the individual clusters exist (Grimes 1979). Radiocarbon samples yielded ages younger than anyone today can accept for a Paleoindian occupation (Haynes *et al.* 1984; Levine 1990). The source or sources of the lithic raw materials represented at the Bull Brook site have been variously identified over the years, and the matter remains unsettled. On the basis of his extensive familiarity with the artifacts, John Grimes is leaning toward origins in the Hathaway formation in northwestern Vermont, 240-300 km to the northwest, and the Munsungan Lake silicates of north-central Maine, ca. 400 km north-northwest (Figure 5.1; Curran and Grimes 1989:68; Spiess and Wilson 1989). In the context of the present argument resolution of this issue is of primary importance. Bull Brook occupied by people from two northern areas supports the seasonal-camp model of Curran and Grimes (1989); Bull Brook settled from the west (or northwest) is the expectation of the interpretation developed here.

The Shoop site in central Pennsylvania was the first of the set to be reported (Witthoft 1952); nevertheless it remains the only one lacking any professional attention in the field. The area of the site is estimated at over 20 acres. Within the bounds of the site, minor elevated areas are the locations of eleven discrete clusters of artifacts (Cox 1986; Witthoft 1952). Originally presented as "Enterline" fluted points, unique forerunners of others, the style is now accepted within the normal variation of the earliest (Bull Brook-Gainey) cluster of eastern Paleoindian armaments (Callahan 1979; Cox 1986), possibly slightly earlier than Bull Brook. The Shoop site has been heavily collected and the contents widely dispersed. The overwhelmingly dominant lithic material was derived from the Onondaga outcrops in western New York, about 320 km to the northwest (Figure 5.1).

Debert, in central Nova Scotia, was the first of the big northeastern sites to be professionally excavated and reported (MacDonald 1968). It has served as the archetype ever since. The site had been damaged by use as an airbase by the Canadian army, and parts of it were subsequently bulldozed for parking. The total area is estimated at about 20 acres, comparable to several others of the set (but Keenlyside cites "8-9 acres," perhaps for the central core [1991:164]). Within the area, eleven discrete artifact clusters were examined; more could have been present prior to the extensive damage to the site. The 140 fluted points recovered exhibited a distinctive deep basal concavity. The dominant raw material is considered to have been acquired from bedrock

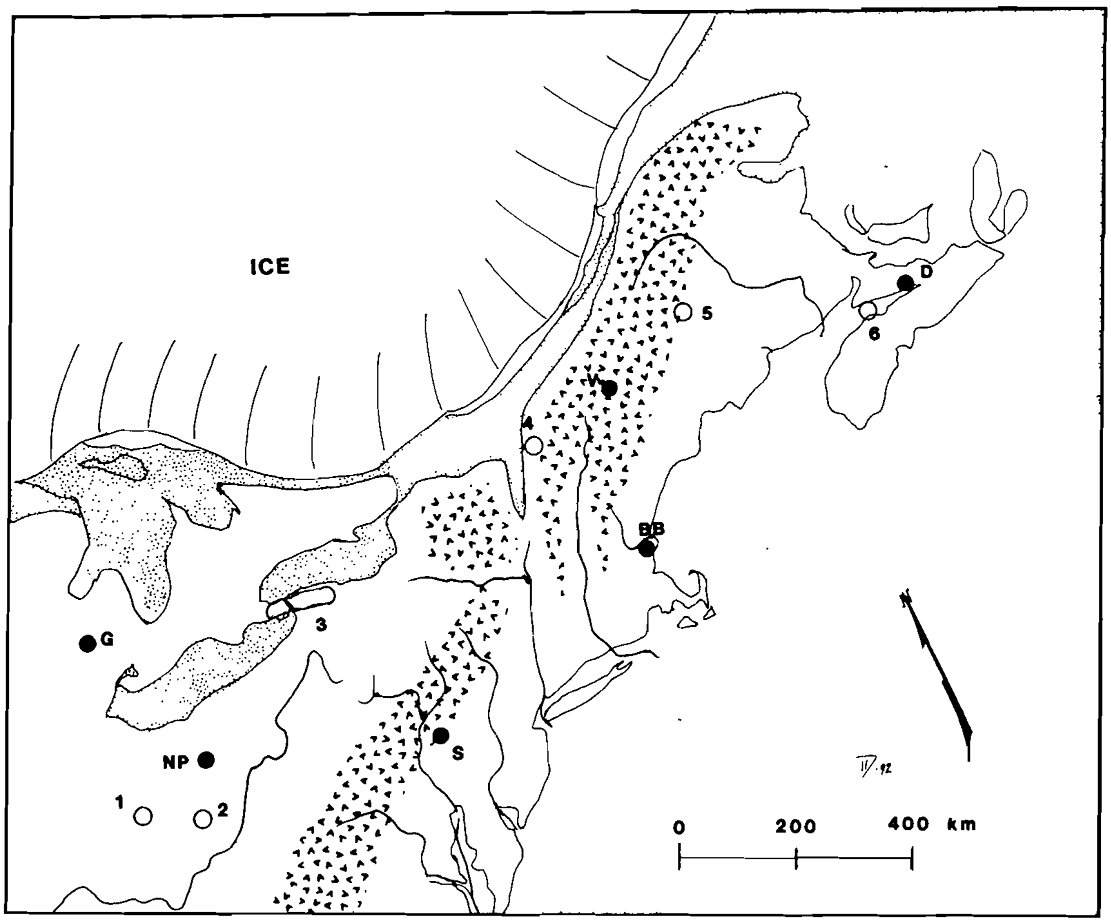


Figure 5.1. Map of the Late Pleistocene Northeastern Peninsula. The dots represent large sites: BB, Bull Brook; D, Debert; V, Vail; S, Shoop; NP, Nobles Pond; and G, Gainey. The numbered circles are bedrock outcrops of silicates: 1, Vanport (Flint Ridge); 2, Upper Mercer; 3, Western Onondaga; 4, Hathaway formation; 5, Munsungan Lake; 6, Minas Basin chalcedonies. The Ledge Ridge silicates are due north of Vail, too close to show at this scale. The landforms and ice limits are approximations averaged around an age of 11,000 ± 250 years B.P. The continental shelf is shown exposed south of the Gaspé, and the Champlain Sea transgression fills the St. Lawrence lowland. The random "v" symbol marks highlands.

outcrops now underwater in the Minas Basin of the Bay of Fundy, 67.5 km ("42 miles") west-southwest of the site (Figure 5.1; MacDonald 1968). The assemblages from the discrete loci include diverse artifact classes that are interpreted as domestic debris for nine of the loci and specialized manufacturing or processing for two.

The Vail site in west-central Maine (Figure 5.1) produced a lower number but essentially the same style of basally concave fluted points as Debert. Located in a river valley among mountains, the site as mapped has a maximum area of about 3 acres (1.25 hectares judging from the scale of Figure 5 in Gramly [1982]). Paralleling the valley wall, eight discrete clusters of artifacts were recovered from an erosionally truncated surface along the shore of an artificial lake. One very large and productive cluster might represent originally two (Spiess and Wilson 1987). The erosionally reduced old surface and the resultant scatter of artifacts lowers the precision that can be achieved in any estimates of site area or configuration; it is undemonstrated that the excavated clusters maintain any cultural integrity or that the count or any single instance is complete. Gramly thinks that the raw material comes from the "Ledge Ridge" outcrops 30 km to the north of the site (Gramly 1985); however, Spiess claims that significant amounts of raw materials have been derived from the Hathaway formation in Vermont, 180 km to the southwest (Spiess and Wilson 1987).

In Ohio, the Nobles Pond site is under investigation on a glacial outwash plain near a kettle pond in the northeastern part of the state. Estimates of the area approach 22 acres. Plowing and collecting have reduced the site's integrity, but after one season of fieldwork investigators mapped over 11 discrete loci of clustered artifacts (Gramly and Sommers 1986; Seeman 1991). The lithic materials at the site derive from the Vanport and Upper Mercer formations, respectively 115 and 75 km south-southwest of the site (Figure 5.1). The site and the two quarries are all linked by the Muskingum drainage network.

The Gainey site in central Michigan has been under investigation since 1978. Although initially described as "exceptionally large" (Simons *et al.* 1984:266), in the present company it is a small site, with an area estimated at three acres, comparable to Vail. Within those bounds, six or more discrete clusters have been recorded; one area may represent a palimpsest of perhaps two periods of use (Area 2; Simons *et al.* 1984:270). The lithics are overwhelmingly from the Upper Mercer formation of Ohio, 400 km southeast of the site (Figure 5.1; Shott 1989).

Interpretations of these notably large sites have conventionally favored versions of a modular model in which the big sites are considered as multiples of the more familiar small sites in the region. When cluster assemblages have been studied and interpreted, they appear to represent typical domestic debris resulting from diverse processing, manufacturing, maintenance and repair activities. The productive sites with their repetitive clusters are considered to have accumulated because of some special attractions of the locales, which brought people together as large groups on a few occasions, or as smaller groups on many occasions. These interpretations embed the large sites into the known universe of smaller Paleoindian sites, normalizing all together.

If, on the other hand, we separate the large sites analytically from the smaller, some potentially significant characteristics become visible. In addition to their uniquely large sizes and high artifact numbers, the big sites share other attributes that may be informative about their functions. In short, they

(1) are *widely dispersed* in the Northeast, with never more than one in an area the size of a state or province;
(2) are *rare*, in contrast to small sites, despite their high archaeological visibility and the prestige conferred on finders;
(3) all have the *earliest fluted point style* in their respective areas–none has Barnes, Cumberland, or later styles;

(4) display *assemblages dominated by one or two lithic materials*, typically from bedrock sources 30 to 400 km distant;
(5) all include *discrete artifact clusters* that do not overlap (with possible exceptions at Vail and Gainey, the smallest in area);
(6) have *notable richness of artifactual debris* in each cluster, with more and more diverse items than are characteristic of the small sites; and
(7) have *artifact styles that are consistent* in techniques and materials within the site.

PALEOENVIRONMENTS

These six unusual Late Pleistocene sites in the northeastern region of the continent must be evaluated with awareness of their unique environments. All but Shoop were in deglaciated areas, although only Debert and Vail were at all close to synchronous tundra environments. The environments of the Northeast during Late Glacial times were unlike anything currently observable. The period of ice melt, as the climate system changed from full-glacial to interglacial conditions, was one of exaggerated seasonal contrasts and unpredictable climate (Kutzbach 1987). Because of the orbital geometry of the earth at that time, winters became more severe: the northern hemisphere was farthest from the sun during that season. With the sun closest during the summer, increased solar radiation tempered the chilling effects of the continental glaciers that were melting away near the international boundary at the time when Paleoindians appeared in the Northeast (Figure 5.1). Weather patterns were erratic as the jet stream shifted northward. Habitats were stressed by rapid changes in living conditions for flora and fauna. Megafauna were on the verge of extinction, or were changing their ranges (Graham 1986, 1990). Sea level was rising along the Atlantic coast, and inland seas and proglacial lakes were draining (Curran and Dincauze 1977).

In the early eleventh millennium B.P., pioneering people in the Northeast faced additional sources of environmental uncertainty. The Younger Dryas climatic reversal, strongly manifested in the North Atlantic, intensified the instability of late Pleistocene biota nearby (Jacobson *et al.* 1987; Peteet *et al.* 1990). Vegetation range expansions that occurred earlier in the wake of warming climates and developing soils were reversed during the early eleventh millennium. Tree lines retreated from higher altitudes and latitudes and spruce replaced incoming hardwoods in some areas (Gaudreau and Webb 1985), triggering changes in animal ranges and behavior.

The caribou hunted by northeastern Paleoindians likely manifested the woodland adaptation of small herds with relatively short seasonal moves, which were often mainly altitudinal shifts between winter and summer grounds. The subspecific Barren Ground adaptation, with its large herds and major latitudinal seasonal range changes, was necessarily dependent upon the development of the extensive Barren Grounds, a Holocene high-latitude phenomenon. It seems also reasonably well established that the northeastern fluted-point users were not accustomed to tundra hunting; their sites do not extend north into the coeval tundra immediately south of the Champlain Sea (Dincauze 1988).

Nevertheless, we see the Paleoindians moving into the Northeast during the Younger Dryas millennium, into the teeth (so to speak) of the climate reversal. If we assume that they were moving northeast and northwest (Gainey) from the "staging areas" in the Ohio valley and its central tributaries (Anderson 1990:190), which seems to be the case on the basis of lithic raw materials carried north and east, the move must have entailed special adaptations by the human groups involved, or at least return to adaptations not practiced since arrival south of the Laurentide ice. Summer occupations likely posed few special challenges, but the Younger Dryas winter was not the time to try anything new or risky.

Coming as they had across the expanse of the continent, Paleoindians may have been a bit chary of seacoast environments as well, even though those might have offered some tempering of winter extremes (Curran 1987). The northwestern Atlantic Ocean had withdrawn from its postglacial maximum transgression by the time the Paleoindians reached its shores. Oldale (1985) estimates that the shore was 10-15 km east from Bull Brook at the time that site was occupied, although a salt marsh surrounds the location today. Debert also was farther inland during occupation than it is today (Figure 5.1). Seacoast coeval with Paleoindian occupations may be exposed in Vermont, where the raised beaches of the Champlain Sea have yielded many artifacts (Loring 1980) but no direct evidence of marine or littoral resource use.

INTERPRETATIONS OF LARGE SITES

It is safe to predict that [the record of Palaeo-Indian occupation in eastern North America] will, at the very least, cause us to pose new questions and to look beyond the pan-continental elements of "the" Palaeo-Indian lifeway for evidence of adaptability and a "multiplicity" of lifeways.
(Storck 1983:35)

Early interpretations of the large northeastern sites tended to favor accumulation over aggregation. The Debert and Shoop sites were so interpreted at first publication (MacDonald 1968; Witthoft 1952). More recently, the Vail site was argued to be a compilation of many visits by small groups of caribou hunters (Gramly 1982). However, as anthropological analogies replaced inductive speculations, and as the discreteness of artifact clusters on all the sites and the circular arrangement of clusters at Bull Brook demanded attention, investigators began to recognize additional reasons for aggregation by hunting bands, and to apply those insights.

The Episodic Reuse (Accumulation) Interpretation

Observers at many eastern Paleoindian sites, both large and small, emphasized the relative elevation of the artifact scatters above surrounding terrain. In combination with the assumption of a treeless tundra environment, these observations dominated explanations for site function: elevated lookouts and camps for big game hunters. Influenced by this convention, many archaeologists interpreted the big sites as accumulations at places favored for intercepting migrating caribou, repeatedly visited through years of use. This line of argument lost much of its force with the recognition that treeless tundra was not the immediate habitat of any northeastern Paleoindian site, although tundra may have been in the neighborhood of Debert and Vail (Dincauze 1981, 1988). Shoop, especially, should have been well forested by the time the fluted point users peered after game there.

If the big sites were in fact episodic accumulations, then characteristic #4, the dominance of one or two lithic materials at each site, should not be definitive of them all. It is unlikely to the point of strain to imagine people importing major amounts of lithic raw materials several hundreds of kilometers from the same direction every time they arrived to hunt. Instead, there should be significant amounts of materials indicative of arrival from several directions, as would be likely for episodic reuse of the location in an unstable environment. Debert and Vail apparently show use of raw materials from less than 100 km distant, bringing them closer than any others to meeting the criteria for episodically used camps within a single band territory.

The discreteness of the artifact clusters at all the sites (characteristic #5) has been a major problem for this interpretation from the beginning. Why should there be perfect avoidance of all previous campsites if an area was used over a period of years? In contrast to the large residential sites at issue here, eastern quarry and workshop sites such as West Athens Hill (Funk 1973), Thunderbird (Gardner 1989), and Williamson (Peck 1985) seem to be true palimpsests, with few discrete clusters and obvious constant economic attractions.

The Seasonal Hunting Aggregation Interpretation

The first variant of the aggregation models was the concept of a seasonal aggregation for herd hunting (Curran and Grimes 1989; Grimes 1979; Storck 1984). This interpretation gains support from the increasing evidence for caribou prey. Caribou bone has been identified at Bull Brook and smaller sites (Spiess, Curran, and Grimes 1985). (Recent indirect, and therefore anecdotal, reports of analytical work cite the identification of caribou blood on an endscraper from Shoop [Gramly and Funk 1990:24] and on one or more artifacts from Debert [Keenlyside 1991:164]). Ethnographic analogies are frequently cited in support of this hunting interpretation, often based on seasonal caribou hunters in the subarctic and arctic Barren Grounds. The absence of archaeological analogs for the large sites has been no deterrent; there are no comparable residential sites in the western plains and prairies, where herds of large game were hunted throughout prehistory. The apparently coeval Lindenmeier site in Colorado appears to differ in lacking both the discrete artifact clusters and the predominance of a single lithic source. Wilmsen (Wilmsen and Roberts 1978:146) interprets the site as having been reoccupied "on more than one occasion," showing "a great deal of areal overlap among the majority of the units." Moreover, the occupants appeared to have had ready access to bedrock quarries, since all stages of lithic reduction were represented at the site.

Nor are there archaeological analogs in caribou-hunting camps on the Barren Grounds or subarctic Labrador. New research in Labrador indicates that large interior caribou hunts developed only after the introduction of firearms and the institution of trade with Europeans (Loring 1992); the interior caribou-hunt sites were inhabitated for brief periods of time and are not comparable in artifact richness to the large residential sites of the northeastern Paleoindians.

Aggregations of otherwise small dispersed bands at single special places should leave archaeological traces of derivation from more than one direction–discrete band hunting ranges. The debris should include lithics from many directions, as well as many exhausted tools made from exotic lithics. The exhausted tools at these big sites, however, are typically made from the dominant lithic material, in stupefying conformity. Moreover, one would expect a range of technical and stylistic variation among the tools accumulated at an aggregation of dispersed bands (Conkey 1980). Characteristics #3 and #7 oppose that expectation with stylistic and technical consistency within each site (so far as is reported). Moreover, if we are observing the remains of a summer camp at Debert that had a southerly winter counterpart, we should see Debert-style points with deeply indented bases in higher numbers than we do to the south. In fact, the hunting camp model implies the expectation for far higher numbers of such sites than are evident anywhere.

In a variation on this theme, Shott (1989) presents a sophisticated, theoretically informed analysis of the technological organization of tool kits at the Gainey site, which supports his argument for a logistical settlement strategy and limited mobility in the seasonal round. This interpretation reads Gainey as a site occupied by caribou hunters whose seasonal mobility was necessarily limited while the spruce-parkland dominated their habitat. As he readily admits, however, Shott has neither direct data nor solid analogs for either the natural or the social environment at the site.

The Macroband Camp Interpretation

Impressed by the reported densities of fluted points in the East, and inspired by the discovery of the Vail site, MacDonald (1982:xi) suggested that the large sites could be macro-band camps, evidence of population growth "in eastern North America where environmental factors were more amenable to greater group size than on the Plains." Fitting (1977) had earlier argued for large populations and "tribal" social complexity, and the initial investigations at the Gainey site led to thoughts about a base camp (Simons *et al.* 1984:270). Although the densities of reported fluted points in the East increase apace (Brennan 1982; Anderson 1990), they remain well below the densities of any later style of weapon tip, so that their numbers cannot support claims for high population densities for their makers. Moreover, if macroband camps are to be taken for evidence of an established settlement pattern of a large population, they should appear at territorial intervals on the regional landscape (Hayden 1980:623). Characterisics #1 and #2 refute that expectation; the large sites are absolutely rare and widely separated.

The Social Aggregation Interpretation

The most anthropologically informed interpretations of the large sites see them as aggregation areas for the seasonal reunion of otherwise dispersed groups gathering for information sharing, mate selection, and exploitation of seasonally abundant resources (Curran 1987; Curran and Grimes 1989). This interpretation goes well beyond the aggregated hunting camp model, to include the satisfaction of a range of basic human needs. Periodic aggregations can be used by dispersed social groups to facilitate information exchange, scheduling and locating decisions, and mate selection (Moore 1981). Planning for such aggregations might include considerations of intercepting migrating game, but would not uniquely require them. The aggregations could continue as long as the local resources could support the high density of humans. This model of site formation is supported by characteristics #5 and #6 of the large sites, but not by #1-4 or #7, which imply not regularly scheduled activities serving regionally resident populations, but rather activities uniquely of the first tentative colonizing social groups.

With so many contending interpretations, it is clear that none is securely established. There are many reasons for this, but the overriding ones are that none of these large sites was investigated prior to being seriously damaged, none has been fully excavated, and none fully analyzed and published. The absence of close ethnographical or archaeological analogs should give pause as well, since so many of the extant interpretations are based on purported analogies to late-Holocene, high-latitude hunters.

THE PIONEERING MODEL

What is the significance of larger sites with internal cultural unity?

(Fitting 1977:372)

In this state of affairs, I offer yet another perspective on the situation, yet another interpretation: the large sites were marshalling areas for people who had crossed their perceived frontier, camps from whence they scouted good habitats before dispersing into them (Dincauze n.d.). "Marshalling area" in this context indicates the use of a place as a focus for the gathering, arranging, and allocating of resources and information, preparatory to dispersing in smaller groups. As such, these large sites are each the remains of unique circumstances, representing the first human groups considering settlement in their respective areas.

The first colonizing pioneers moving into terrain uninhabited by other humans are a very special class of human explorers. Information constraints are likely to be their greatest stressors–nothing is so fearsome as the unknown. Communication nets are stretched thin by low population densities and the distances and areas involved. Risks are exaggerated by lack of information and by distance to social support, and proliferate in unfamiliar space (Kelly and Todd 1988). Absolute newcomers in a place even lack the referential vocabulary to discuss spatial relationships and distance to resources or other people. Behind the pioneers lay the territories of their birth, their families of origin, the familiar terrain of their myths. Ahead lay lands known only from adventurous forays, uninhabited by people and thus unmapped except for the information scouts had established in anticipation of the move.

The uniquely dynamic environments of the Northeast in the eleventh millennium, with their strong seasonality, Younger Dryas climatic reversal, and ecotonal shifts, should have evoked unique adaptive strategies from pioneers. Thus, the absence from other parts of the continent of sites comparable to these may reflect lower levels of environmental contrast and uncertainty for pioneers expanding their ranges in those areas. We might expect some colonial aggregation sites near the ecotone between prairie and forest; I know of none as I write.

Although we are not dealing here with migration in the conventional sense of people moving into the socioeconomic space of other people, some of the migration criteria presented by David Anthony (1990) are helpful. For instance, the importance of scouting target areas, crucial for normal migrants, becomes more important when scouts are the only source of information. It is also important to keep in mind that "cultures do not migrate. It is often only a very narrowly defined, goal-oriented subgroup that migrates" (Anthony 1990:98). In the case of Paleoindians, it was likely to have been a subgroup dominated by, or entirely composed of, young adults, burdened with few children, who were in the best position to move into the unknown. They have the most to gain by leaving an established society and the fewest impeding obligations.

In discussing aggregations in European Upper Paleolithic societies Conkey cautions that aggregation behavior is not normative, that "the duration, location, cyclicity, extent, personnel, and activities of any aggregation may vary greatly" (Conkey 1980:609). Among their many advantages, aggregations of people establish conditions in which high-risk activities are buffered by the support available from other members of the group, who in turn benefit from the information gained by risk-takers. In the context of pioneering, aggregations would provide many of the benefits of risk-reducing information enhancement that colonial nesting provides Brewer's blackbirds (Horn [1968] quoted in Wilmsen [1973]). Conkey's criteria for risk abatement in aggregations suggest how appropriate such behavior would be for pioneers, even if they never again in their lifetimes congregated in such high densities. The duration of such aggregations would be limited ultimately by available resources, but there would be a premium on relatively long-term residence in one place while the hinterlands were scouted and evaluated. Long-term, in the late Pleistocene, might mean only a few months; the duration could be extended by initiating the settlement in late spring, as bird and fish migrations peak, and continuing into the summer and even later in hospitable environments. Such relatively long duration of residence, for a group of foragers, would mean verging toward a "logistical" strategy of resource collection (Binford 1980), which would require a high internal diversity of activities, and thus of archaeological remains, at the settlement. In the absence of evidence for storage facilities at the big sites, there is nothing to indicate that such logistical strategies were of more than seasonal duration.

The interpretation proposed here can be exemplified in a scenario. For the sake of argument, we can begin with Paleoindian people settled in the Ohio-Kentucky area among excellent chert sources and diverse cool-temperate flora and fauna. The populations were not dense in any modern sense, and resource stress in the sense of Keeley (1988) is unlikely. Nevertheless, at some point there is impetus for a subset of the population to move out beyond the established ranges. Young people decide to explore opportunities; scouts go out, collect information, and report back. Leaders enlist personnel from several family sets and plan a move. Noting the scouts' concerns about the relative scarcity of good lithic sources to the east and north, the volunteers first provision themselves with several months' supply of raw material in portable forms (Ellis 1989; Goodyear 1989; Meltzer 1989). In the spring they move out to the campsite selected by scouts, probably chosen for its diverse and dependable resources. They travel relatively lightly burdened, carrying basic equipment and essential lithic raw materials, intending to spend time and effort equipping themselves more fully during the warm months to come. Their mobility is unhindered by either pre-existing social construction of space or people. They encounter no circumscription, no other groups to object to usurpation or to demand compensation for crowding (Moore 1981). They settle into the base camp and send out scouting parties in all directions to evaluate resources and habitats. During the summer months of relative abundance they maintain themselves, collect resources and prepare equipment for winter family camps, and establish a referential vocabulary for mental maps of the region around them. By the end of the summer they are ready to partition family ranges to which they disperse for winter and the following years. The family ranges are chosen to be large and diverse enough to support small groups who exploit the resources within them and share information with contiguous neighbors during regular resource-collecting moves. The expedient group of pioneers need never again aggregate on the original terms, although cooperative co-residence may be undertaken for special purposes.

Here we can return briefly to Shott's explanation for the Gainey site (Shott 1989). Building on concepts of technological organization, he argues that the site was occupied by logistical hunters and gatherers who remained for a significant span of time. The evidence that supports his argument for limited logistical mobility at the site fits this colonist interpretation very well.

The colonist scenario is not dependent on any particular view of Paleoindian demographics, nor does it require specialization on large game as does Kelly and Todd's model of rapidly moving explorers (Kelly and Todd 1988); the two models are not mutually competitive. I personally favor the likelihood that Paleoindian mobility was the only significant constraint on birth rate, although I don't know how that translates into numbers in any particular area of the continent. I accept that populations derived from Eurasia benefitted from reduced morbidity after successfully passing the arctic filter, losing thereby many parasitic and endemic diseases. Paleoindian technology was demonstrably adequate to the demands of North American resources. Many modelers have assumed a relatively rapid population expansion, rather than resource scarcity, driving people quickly across the continent (e.g., Beaton 1991; Mosimann and Martin 1975), and I have no problem with that expectation. The potential for rapid growth of a thin, dispersed population unconstrained by resource competition, territorial limitations, or infectious diseases leaves open the possibility of a very short chronology for Paleoindian dispersal across the continent, well within a millennium of first entry.

A preliminary seriation of the large sites, based on inferred direction of movement and geographical distance from the continental center, suggests that Nobles Pond and Gainey might represent the first forays out of Ohio to the northeast and northwest. From western New York, ideally after the move to Nobles Pond, a group might loop around the mountains, follow the Susquehanna drainage southeast, and take a first look at the Atlantic Slope from Shoop. This only works if the tentative-looking fluting on the Shoop points is a product of qualities of the Onondaga chert; otherwise, Shoop might be older than Nobles Pond. A marshalling site in eastern New York is expectable to establish populations there and in western New England. Bull Brook, Debert, and Vail are harder to call because the actual sources of the cryptocrystalline rocks at those sites remain to be firmly demonstrated. If the northern sites are in fact both later than Bull Brook, which seems reasonable on technostylistic grounds (cf. Cox 1986:136; Shott 1990), then they may be the products of people who had already scouted the territories near the tundra border over a few tens of years, and knew where to find suitable rock. They could be parts of cyclical settlement systems like that suggested for the Parkhill Complex in Ontario (Roosa and Deller 1982).

IMPLICATIONS OF THE PIONEERING MODEL

> *Arnold Pilling, on the basis of his own research, has suggested the possibility of expedient residential groups formed around leaders with particular skills, usually shamans or hunters (or both) . . . . Such an organizational mode would certainly present a challenge for archaeological recognition.*
>
> (Fitting 1977:372)

Each large site resulting from behavior as modeled here should have assemblages dominated by rock from the direction of origin, transported in biface form, as a result of intentional provisioning. Lithics should be from one direction or source nearer the older occupation area, generally west or south (Figure 5.1). This condition is met in the Northeast, but not exclusively in the largest sites; some of the small sites also show the same provisioning behavior (Ellis 1989; Lothrop 1989). A marshalling site should be located on or near a major biological or physiographic ecotone, if we assume that the leaders were maximizing resource quantity and diversity for the long stay. The published record is incomplete for some of the sites considered here, but Bull Brook, Debert, Vail, and probably Gainey are so located, and the others may be. Marshalling sites should be located at significantly large distances from any other such sites, as these seem to be. Tool refuse should display high diversity, such as would result from an extended stay. Each site should have been used collectively only once, so that palimpsests are rare to absent. The activity areas in each site should be not only spatially distinct, but also mostly duplicative (redundant) in inventory and functions. They should represent many residential activities. The most highly styled artifacts (the fluted points) should be the earliest in each area, stylistically consistent within the site, and contrastive in some particulars with those normal in other areas.

The seven characteristics of the large sites presented at the beginning of this chapter to justify the integrity of the set can be shown to meet the implications of this model. However, it would be tautological to claim that they support the model, since they have partly defined it. Nevertheless, they can be used for a preliminary evaluation of the model's reality, and to indicate the kinds of analyses and data that will be needed to test it.

(1) The requirement that marshalling sites will be *widely spaced* is met in this set, which shows only one for each state-sized area. We may be missing one in New York, but discoveries at much closer intervals will weaken the case made here.
(2) Furthermore, it is necessary for the integrity of the argument that the *number of large sites* known should not increase dramatically as data accumulate. This asserts that the information at hand now is not seriously underrepresented, with the exception noted in (1).
(3) For me, it is especially telling that these largest sites each have uniquely the *earliest point style*

in their respective areas. Only in Ontario do large sites show the succeeding Barnes style, and those large sites may indeed be seasonal aggregations of different kinds (Storck 1982). Small sites in the several areas have a variety of styles, early and later, although the different styles are rarely found together. To test these suggestions, thorough analyses of techno-stylistic variation in northeastern Paleoindian artifacts are needed.

(4) Marshalling sites of pioneers, as distinguished from aggregation for other reasons, will necessarily have a *restricted variety of lithic materials predominantly from distant sources in single directions*, most likely radial directions from, e.g., Ohio. This is because the exploration that revealed local lithic resources was undertaken only after the marshalling site was occupied, by people who brought provisions with them. On the basis of this criterion, the Vail site may ultimately fall out of this set, since it seems to have major amounts of lithic materials from both west and north. Clearly, further progress on this issue will require more precise lithic sourcing studies, of the sort exemplified by Tankersley (1990).

(5) Because the activity loci within each site were contemporary, each *artifact cluster will be spatially discrete* on marshalling sites. This condition is met on all the sites except possibly the smallest, Gainey and Vail. Cross-matches of broken artifacts show at least pairs of loci to be contemporary at Vail (Gramly 1982), Nobles Pond (Seeman 1991), and Bull Brook (Grimes *et al.* 1984:178); more such studies are needed.

(6) The *artifact diversity* within marshalling sites should be greater than that of smaller sites, because of the long duration of occupation (Spiess 1984), the logistical strategies employed (Shott 1989), the high local population density, and the risk-reducing behaviors characteristic of them (Conkey 1980; Wilmsen 1973). Although Meltzer's "richness index" as published in 1988 fails to support this expectation, he properly cautions about the inadequacy and unrepresentativeness of the data used to compile the index. If reasonably comparable information becomes available for both large and small northeastern sites, this criterion should prove a strong test of expections (cf. Curran 1984).

(7) *Artifact styles* within large marshalling sites should be markedly *less diverse* than in equivalently sized samples compiled from beyond those sites. This is because of the expectations that the founding group was relatively homogeneous socially and that the site was occupied for a single span of time, probably less than a full year. To the extent that site reports and briefer studies address this issue for the sites considered, the criterion holds. We await eagerly more thorough analysis and reporting of sites already excavated. This model implies a further useful expectation: each exploring pioneer group of the sort postulated here would produce a technostylistic "founder effect." The episodic founder effect, each time in new territory, could explain the proliferation of sub-regional differences in fluted point styles that is being noticed in the record (Meltzer 1988).

The logistical strategies posited for the marshalling sites could not likely be maintained as the norm in the dynamic, uncertain environments of the latest Pleistocene. The basic economic unit for Paleoindians was apparently the family band with affiliates. After the initial aggregation episode, the colonists likely dispersed to family ranges, moving thenceforth from small site to small site in forager mode (Binford 1980). Family ranges were likely to be finite, so that they abutted others as required by the need to maintain effective contacts with social support networks, but ranges were probably very weakly bounded, not territories in the sense of defended space. Forager mobility patterns seem more appropriate as norms for the family bands, and suit well the typical small Paleoindian sites that are seen all over the Northeast (Gramly and Funk 1990; Jackson and McKillop 1991; Lepper and Meltzer 1991:178-180). Some of the small sites were apparently repeatedly visited (e.g., Spiess and Wilson 1987); others may have seen more than one family group involved at a time. Aggregations for information exchange might be expected, because thin population densities require considerable effort from people to maintain information and mating networks (Wobst 1976). But if so, such aggregation sites should be fairly numerous (annual or semi-annual gatherings), increasingly closely spaced, and located near crucial raw materials (quarries, food, water). They should also show some diversity of artifact technostylistic attributes, be of different sizes (varying personnel), and occur at landscape nodes to facilitate planning (as Lindenmeier obviously is). There is too little information about the structure or numbers of northeastern Paleoindian sites to indicate how many people, or how many years, were involved in creating the record. If we can distinguish functionally different site classes with any accuracy, the interpretation should become "not many people, and not a great many years."

SUMMARY AND RECOMMENDATIONS

The interpretation of the Shoop site has challenged prehistorians since the first report. On the basis of the flaking technology and the long-distance transport of chert, Witthoft championed it as the earliest fluted point site in the East, perhaps on the continent; his enthusiasm was rejected by most of his colleagues. "In recent years, Witthoft's (1952) explanation has fallen into almost total disrepute" (Ellis 1989:148-9) because of the recognition that long-distance transport of high-quality stone was a normal pattern in eastern fluted point sites. The transport distance at Shoop (320 km) falls between those of Gainey and Bull Brook, but is comparable; Debert, Vail, and Nobles Pond show shorter distances. Shoop is strange also for the NW-SE direction represented by the Onondaga chert, for being the only large residential non-quarry site outside of the glaciated regions, and for a location that has no obvious attractions over others in its area. In 1989 Roger Moeller published some musings on the "conundrum" of the site, with a number of interpretive scenarios, none of which quite satisfied him. In my view, he came close to the answer. "If the typological arguments for Shoop being a very early Paleo-Indian site are accurate, this was the first wave of people coming into an unknown territory. [V]isits to Shoop . . . would have ceased when the people had the opportunity to explore the diversity and advantages of other locales" (Moeller 1989:75).

All of Shoop makes sense if Shoop is the result of people exploring east of the Appalachian peaks for the first time, having originally gone northeast into New York along the easiest routes following the Ohio and Allegheny rivers to the Great Lakes plains. Provisioned with Onondaga chert, they then moved cautiously southeast along the Susquehanna drainage into the highland valleys and set up a marshalling camp. From there, scouts brought back native lithics (Pennsylvania jasper and others) from sources farther east. With fuller knowledge of the eastern terrain and resources, people could abandon Shoop for better bases. Shoop is not typical of the normative eastern fluted point site, but it may have been a very typical marshalling site of Paleoindian pioneers.

Once analysis moves confidently beyond normative interpretations, considerable diversity in Paleoindian settlement patterns and economic strategies should be discernible in the Northeast in both spatial and temporal dimensions and at several scales. The spatial concepts of site, range, region, and frontier should be employed analytically with more imagination than has been the case. We should try to overcome the constraints of thinking only in secular time or radiocarbon centuries; temporal units such as seasons (Curran and Grimes 1989; Spiess 1984) and generations (Dincauze n.d.) should be employed in interpretations because they were the spans of time experienced by the Paleoindians themselves. Differences between the first explorers and pioneers and their successors should be especially well marked in the range of site types and the specialization of tool kits because instantaneous establishment of full-blown adaptive strategies is highly unlikely. If archaeologists are ever to find evidence of innovation, risk-taking, and short-lived, imperfectly successful adaptive strategies, they should learn to seek them among the Paleoindian sites of North America where, once upon a time, everything was new.

ACKNOWLEDGEMENTS

This contribution grows out of my longstanding fascination with the Pleistocene, which Stephen Williams shared and encouraged. The speculations are grounded in the unprecedented innovative activity in Paleoindian research of the past decade. I am deeply indebted to all the researchers in eastern North America who helped keep me informed about new research on Paleoindians and their habitats through publications and other communications. I am especially grateful to David Anderson, Mary Lou Curran, Christopher Ellis, Albert Goodyear, Russell Graham, Bradley Lepper, Mary Ann Levine, David Meltzer, Michael Shott, Peter Storck, and Kenneth Tankersley, who have been notably generous with information and reprints. The skills of Maureen Manning-Bernatzky improved Figure 5.1. I accept full responsibility for what I have written, and hope that time and colleagues will test the results.

REFERENCES

Anderson, D.G.
1990 The Paleoindian Colonization of Eastern North America: A View from the Southeastern United States. In *Early Paleoindian Economies of Eastern North America,* edited by K.B. Tankersley and B.L. Isaac, 163-216. Research in Economic Anthropology, Supplement 5. JAI Press, Inc., Greenwich.

Ammerman, A.J., and L.L. Cavalli-Sforza
1973 A Population Model for the Diffusion of Early Farming in Europe. In *The Explanation of Culture Change: Models in Prehistory,* edited by C. Renfrew, 343-358. Duckworth, London.

Anthony, D.W.
1990 Migration in Archaeology: The Baby and the Bathwater. *American Anthropologist* 92:895-914.

Beaton, J.M.
1991 Colonizing Continents: Some Problems from Australia and the Americas. In *The First Americans: Search and Research,* edited by T.D. Dillehay and D.J. Meltzer, 209-230. CRC Press, Boca Raton.

Binford, L.R.
1980 Willow Smoke and Dog's Tails: Hunter-Gatherer Settlement Systems and Archaeological Site Formation. *American Antiquity* 45:4-20.

Brennan, L.A. (compiler)
1982 A Compilation of Fluted Points of Eastern North America by Count and Distribution: An AENA Project. *Archaeology of Eastern North America* 10:27-46.

Byers, Douglas S.
1954 Bull Brook–A Fluted Point Site in Ipswich, Massachusetts. *American Antiquity* 19:343-351.

Callahan, E.
1979 The Basics of Biface Knapping in the Eastern Fluted Point Tradition: A Manual for Flintknappers and Lithic Analysts. *Archaeology of Eastern North America* 7:1-180.

Conkey, M.W.
1980 The Identification of Prehistoric Hunter-Gatherer Aggregation Sites: The Case of Altamira. *Current Anthropology* 21:609-630.

Cox, S.L.
1986 A Re-analysis of the Shoop Site. *Archaeology of Eastern North America* 14:101-170.

Curran, M.L.
1984 The Whipple Site and Paleoindian Tool Assemblage Variation: A Comparison of Intrasite Structuring. *Archaeology of Eastern North America* 12:5-40.
1987 The Spatial Organization of Paleoindian Populations in the Late Pleistocene of the Northeast. Ph.D. dissertation, University of Massachusetts. University Microfilms, Ann Arbor.

Curran, M.L., and D.F. Dincauze
1977 Paleoindians and Paleo-Lakes: New Data from the Connecticut Drainage. In *Amerinds and their Paleoenvironments in Northeastern North America,* edited by W.S. Newman and B. Salwen, 369-374. Annals of the New York Academy of Sciences, Vol. 288. New York.

Curran, M.L., and J.R. Grimes
1989 Ecological Implications for Paleoindian Lithic Procurement Economy in New England. In *Eastern Paleoindian Lithic Resource Use*, edited by C.J. Ellis and J.C. Lothrop, 41-74. Westview Press, Boulder.

Deller, D. Brian, and C.J. Ellis
1988 Early Palaeo-Indian Complexes in Southwestern Ontario. In *Late Pleistocene and Early Holocene Paleoecology and Archeology of the Eastern Great Lakes Region*, edited by R.S. Laub, N.G. Miller, and D.W. Steadman, 251-263. Bulletin of the Buffalo Society of Natural Sciences, Vol. 33. Buffalo.

Dincauze, D.F.
1981 Paleoenvironmental Reconstruction in the Northeast: The Art of Multidisciplinary Science. In *Foundations of Northeast Archaeology*, edited by D.R. Snow, 51-96. Academic Press, New York.
1988 Tundra and Enlightenment: Landscapes for Paleoindians. *Quarterly Review of Archaeology* 9(2):6-8.
n.d. Fluted Points in the Eastern Forests. In *From Kostenki to Clovis: Problems in Late Paleolithic Adaptations*, edited by O. Soffer and N.D. Praslov. Plenum, New York.

Dincauze, D.F., and M.L. Curran
1983 Paleoindians as Generalists: An Ecological Perspective. Paper presented at the 48th Annual Meeting, Society for American Archaeology, Pittsburgh.

Ellis, C.J.
1989 The Explanation of Northeastern Paleoindian Lithic Procurement Patterns. In *Eastern Paleo-Indian Lithic Resource Use*, edited by C.J. Ellis and J.C. Lothrop, 139-164. Westview Press, Boulder.

Ellis, C.J., and D.B. Deller
1988 Some Distinctive Paleo-Indian Tool Types from the Lower Great Lakes Area. *Midcontinental Journal of Archaeology* 13:111-158.

Fitting, J.E.
1977 Social Dimensions of the Paleoindian Adaptation in the Northeast. In *Amerinds and their Paleoenvironments in Northeastern North America*, edited by W.S. Newman and B. Salwen, 369-374. Annals of the New York Academy of Sciences, Vol. 288. New York.

Funk, R.E.
1973 The West Athens Hill Site (Cox 7). In *Aboriginal Settlement Patterns in the Northeast*, edited by W.A. Ritchie and R.E. Funk, 9-36. New York State Museum and Science Service Memoir 20, Albany.

Gardner, W.M.
1977 Flint Run Paleoindian Complex and its Implications for Eastern North American Prehistory. In *Amerinds and their Paleoenvironments in Northeastern North America*, edited by W.S. Newman and B. Salwen, 257-263. Annals of the New York Academy of Sciences, Vol. 288. New York.
1989 An Examination of Cultural Change in the Late Pleistocene and Early Holocene (circa 9200 to 6800 B.C.). In *Paleoindian Research in Virginia: A Synthesis*, edited by J.M. Wittkofski and T.R. Reinhart, 5-51. Archeological Society of Virginia, Special Publication No. 19.

Gaudreau, D.C., and T. Webb III
1985 Late Quaternary Pollen Stratigraphy and Isochrone Maps for the Northeastern United States. In *Pollen Records of Late-Quaternary North American Sediments*, edited by V.M. Bryant, Jr. and R. Holloway, 247-280. American Association of Stratigraphic Palynologists.

Goodyear, A.C.
1989 A Hypothesis for the Use of Cryptocrystalline Raw Materials Among Paleoindian Groups of North America. In *Eastern Paleoindian Lithic Resource Use*, edited by C.J. Ellis and J.C. Lothrop, 1-9. Westview Press, Boulder.

Graham, R.W.
1986 Plant-Animal Interactions and Pleistocene Extinctions. In *Dynamics of Extinction*, edited by D.K. Elliott, 131-154. Wiley, New York.
1990 Evolution of New Ecosystems at the End of the Pleistocene. In *Megafauna & Man: Discovery of American's Heartland*, edited by L.D. Agenbroad, J.I. Mead, and L.W. Nelson, 54-60. Scientific Papers Vol. 1, The Mammoth Site of Hot Springs, South Dakota, Inc.

Gramly, R.M.
1982 *The Vail Site: A Palaeo-Indian Encampment in Maine.* Bulletin of the Buffalo Society of Natural Sciences 30.
1985 Recherches archéologiques au site Paléoindien de Vail, dans le Nord-ouest du Maine, 1980-1983. *Recherches Amérindiennes au Québec* 15:57-118.

Gramly, R.M., and R.E. Funk
1990 What is Known and Not Known About the Human Occupation of the Northeastern United States Until 10,000 B.P. *Archaeology of Eastern North America* 18:5-31.

Gramly, R.M., and G. Summers
1986 Nobles Pond: A Fluted Point Site in Northeastern Ohio. *Midcontinental Journal of Archaeology* 11:97-123.

Grimes, J.R.
1979 A New Look at Bull Brook. *Anthropology* 3:109-130.

Grimes, J.R., W. Eldridge, B.G. Grimes, A. Vaccaro, F. Vacccaro, J. Vaccaro, N. Vaccaro, and A. Orsini
1984 Bull Brook II. *Archaeology of Eastern North America* 12:159-183.

Hayden, B.
1980 Comment on Conkey's Aggregation Sites. *Current Anthropology* 21:623-624.

Haynes, C. Vance, D.J. Donahue, A.J.T. Jull, and T.H. Zabel
1984 Application of Accelerator Dating to Fluted Point Paleoindian Sites. *Archaeology of Eastern North America* 12:184-191.

Horn, H.
1968 The Adaptive Significance of Colonial Nesting in the Brewer's Blackbird (*Euphagus cyanocephalus*). *Ecology* 49:682-694.

Jackson, L.J., and H. McKillop
1991 Approaches to Paleo-Indian Economy: An Ontario and Great Lakes Perspective. *Midcontinental Journal of Archaeology* 16:34-68.

Jacobson, G.L., Jr., T. Webb III and E.C. Grimm
1987 Patterns and Rates of Vegetation Change During the Deglaciation of Eastern North America. In *North America and Adjacent Oceans During the Last Deglaciation*, edited by W.F. Ruddiman and H.E. Wright, Jr., 277-288 and end maps. The Geology of North America, DNAG vol. K-3. Geological Society of America, Boulder.

Jordan, D.
1960 The Bull Brook Site in Relation to "Fluted Point" Manifestations in Eastern North America. Ph.D. dissertation, Department of Anthropology, Harvard University, Cambridge.

Keeley, L.H.
1988 Hunter-Gatherer Economic Complexity and "Population Pressure": A Cross-Cultural Analysis. *Journal of Anthropological Archaeology* 7:373-411.

Keenlyside, D.
1991 Paleoindian Occupations of the Maritimes Region of Canada. In *Clovis: Origins and Adaptations*, edited by R. Bonnichsen and K.L. Turnmire, 163-173. Center for the Study of the First Americans, Corvallis.

Kelly, R.L., and L.C. Todd
1988 Coming into the Country: Early Paleoindian Hunting and Mobility. *American Antiquity* 53:231-244.

Kutzbach, J.E.
1987 Model Simulations of the Climatic Patterns during the Deglaciation of North America. In *North America and Adjacent Oceans During the Last Deglaciation*, edited by W.F. Ruddiman and H.E. Wright, Jr., 425-446. The Geology of North America, DNAG vol. K-3. Geological Society of America, Boulder.

Lepper, B.T.
1988 Early Paleo-Indian Foragers of Midcontinental North America. *North American Archaeologist* 9:31-51.

Lepper, B.T., and D.L. Meltzer
1991 Late Pleistocene Human Occupation of the Eastern United States. In *Clovis: Origins and Adaptations*, edited by R. Bonnichsen and K.L. Turnmire, 175-184. Center for the Study of the First Americans, Corvallis.

Levine, M.A.
1990 Accommodating Age: Radiocarbon Results and Fluted Point Sites in Northeastern North America. *Archaeology of Eastern North America* 18:33-63.

Loring, S.
1980 Paleo-Indian Hunters and the Champlain Sea: A Presumed Association. *Man in the Northeast* 19:15-42.
1992 Princes and Princesses of Ragged Fame: Innu Archaeology and Ethnohistory in Labrador. Ph.D. dissertation, Department of Anthropology, University of Massachusetts at Amherst.

Lothrop, J.C.
1989 The Organization of Paleoindian Lithic Technology at the Potts Site. In *Eastern Paleoindian Lithic Resource Use*, edited by C.J. Ellis and J.C. Lothrop, 99-137. Westview Press, Boulder.

MacDonald, George F.
1968 *Debert: A Paleo-Indian Site in Central Nova Scotia.* National Museum of Canada Anthropology Papers 16. Ottawa.
1982 Foreword. In *The Vail Site: A Palaeo-Indian Encampment in Maine*, by R.M. Gramly, x-xi. Bulletin of the Buffalo Society of Natural Sciences, Buffalo.

Meltzer, D.J.
1984 Late Pleistocene Human Adaptations in Eastern North America. Ph.D. dissertation, Department of Anthropology, University of Washington. University Microfilms, Ann Arbor.
1988 Late Pleistocene Human Adaptations in Eastern North America. *Journal of World Prehistory* 2(1):1-52.
1989 Was Stone Exchanged Among Eastern North American Paleoindians? In *Eastern Paleoindian Lithic Resource Use*, edited by C.J. Ellis and J.C. Lothrop, 11-39. Westview Press, Boulder.

Moeller, R.
1989 The Shoop Conundrum. *Pennsylvania Archaeologist* 59:70-77.

Moore, J.A.
1981 The Effects of Information Networks in Hunter-Gatherer Societies. In *Hunter-Gatherer Foraging Strategies*, edited by B. Winterhalder and E.A. Smith, 194-217. University of Chicago Press, Chicago.

Mosimann, J.E., and P.S. Martin
1975 Simulating Overkill by Palaeoindians. *American Scientist* 63:304-313.

Oldale, R.N.
1985 Rapid Postglacial Shoreline Changes in the Western Gulf of Maine and the Paleoindian Environment. *American Antiquity* 50:145-150.

Peck, R. (editor)
1985 *The Williamson Site, Dinwiddie County, Virginia.* Harrisburg, North Carolina.

Peers, L.
1986 Ontario Paleo-Indians and Caribou Predation. *Ontario Archaeology* 43:31-40.

Peteet, D.M., J.S. Vogel, D.E. Nelson, J.R. Southon, R. Nickmann, and L.E. Heusser
1990 Younger Dryas Climatic Reversal in Northeastern USA? AMS Ages for an Old Problem. *Quaternary Research* 33:219-230.

Roosa, William B., and D. Brian Deller
1982 The Parkhill Complex and Eastern Great Lakes Paleo-Indian. *Ontario Archaeology* 37:3-15.

Rouse, I.
1986 *Migrations in Prehistory: Inferring Population Movement from Cultural Remains*. Yale University Press, New Haven.

Seeman, M.F.
1991 Inter-Cluster Lithic Patterning at Nobles Pond: A Case for "Disembedded" Procurement Among Early Paleo-Indian Societies. Paper presented at 56th Annual Meeting of the Society for American Archaeology, New Orleans.

Shott, M.J.
1989 Technological Organization in Great Lakes Paleoindian Assemblages. In *Eastern Paleoindian Lithic Resource Use*, edited by C.J. Ellis and J.C. Lothrop, 221-237. Westview Press, Boulder.
1990 Stone Tools and Economics: Great Lakes Paleoindian Examples. In *Early Paleoindian Economies of Eastern North America*, edited by K.B. Tankersley and B.L. Isaac, 3-44. Research in Economic Anthropology, Supplement 5. JAI Press, Inc., Greenwich.

Simons, Donald B., Michael J. Shott, and Henry T. Wright
1984 The Gainey Site: Variability in a Great Lakes Paleo-Indian Assemblage. *Archaeology of Eastern North America* 12:266-279.

Spiess, Arthur E.
1984 Arctic Garbage and New England Paleo-Indians: The Single Occupation Option. *Archaeology of Eastern North America* 12:280-285.

Spiess, Arthur E., M.L. Curran, and J.R. Grimes
1985 Caribou (*Rangifer tarandus* L.) Bones from New England Paleoindian Sites. *North American Archaeologist* 6(2):145-159.

Spiess, Arthur E., and D.B. Wilson
1987 *Michaud: A Paleoindian Site in the New England-Maritimes Region*. Occasional Publications in Maine Archaeology 6. The Maine Historic Preservation Commission and the Maine Archaeological Society, Inc., Augusta.
1989 Paleoindian Lithic Distribution in the New England-Maritimes Region. In *Eastern Paleoindian Lithic Resource Use*, edited by C.J. Ellis and J.C. Lothrop, 75-97. Westview Press, Boulder.

Storck, Peter L.
1982 Palaeo-Indian Settlement Patterns Associated with the Strandline of Glacial Lake Algonquin in Southcentral Ontario. *Canadian Journal of Archaeology* 6:1-31.
1983 Commentary on AENA's Compilation of Fluted Points in Eastern North America. *Archaeology of Eastern North America* 11:34-36.
1984 Glacial Lake Algonquin and Early Palaeo-Indian Settlement Patterns in Southcentral Ontario. *Archaeology of Eastern North America* 12:286-298.
1991 Imperialists Without a State: The Cultural Dynamics of early Paleoindian Colonization as seen from the Great Lakes Region. In *Clovis: Origins and Adaptations*, edited by R. Bonnichsen and K.L. Turnmire, 153-162. Center for the Study of the First Americans, Corvallis.

Tankersley, K.B.
1990 A Geoarchaeological Investigation of Distribution and Exchange in the Raw Material Economies of Clovis Groups in Eastern North America. In *Early Paleoindian Economies of Eastern North America*, edited by K.B. Tankersley and B.L. Isaac, 259-299. Research in Economic Anthropology, Supplement 5. JAI Press, Inc., Greenwich.

Wilmsen, E.N.
1973 Interaction, Spacing Behavior, and the Organization of Hunting Bands. *Journal of Anthropological Research* 29:1-31.

Wilmsen, E.N., and F.H.H. Roberts
1978 *Lindenmeier, 1934-1974: Concluding Report on Investigations*. Smithsonian Contributions to Anthropology No. 24, Washington, D.C.

Witthoft, John
1952 A Paleo-Indian Site in Eastern Pennsylvania: An Early Hunting Culture. *Proceedings of the American Philosophical Society* 96(4):1-32.

Wobst, H.M.
1976 Locational Relationships in Paleolithic Society. *Journal of Human Evolution* 5:49-58.

6 A Reconsideration of Fluted Point Diversity in Wisconsin

James B. Stoltman
University of Wisconsin
Madison, Wisconsin

Unstemmed, fluted bifaces are widely accepted as the earliest unambiguous evidence of human occupation in most parts of North America (e.g., Haynes 1964; Anderson 1990; Gramly and Funk 1990). Initially assigned to late Pleistocene times in the Eastern Woodlands on the basis of typological similarities to spear points recovered from well-dated western sites (e.g., Mason 1962), a substantial number of radiocarbon-dated sites in the East now confirms that the fluted point tradition in this area does, indeed, date to Late Glacial times (e.g., Levine 1990).

Accepting, then, the general interval from ca. 10,000 B.C. to 8000 B.C. as encompassing most fluted point manifestations in the Eastern Woodlands, the enormous typological diversity of eastern fluted points suggests, further, that considerable temporal and/or spatial cultural diversity is encapsulated in the archaeological record. The major goal of this paper is to reconsider the extent to which the typological diversity of fluted points in Wisconsin can be interpreted as a reflection of age differences in light of recent data both from Wisconsin and surrounding regions.

In 1969 I co-authored an overview of fluted points within the state of Wisconsin that was based primarily upon an analysis of unpublished surface finds housed in the collections of three institutions, the State Historical Society of Wisconsin, the Milwaukee Public Museum, and the University of Wisconsin-Madison (Stoltman and Workman 1969). Because the 65 fluted points described in that paper all had to be treated as isolated finds, as opposed to viewed in the context of full site assemblages, assignment to specific types was frequently tentative and inconclusive. Indeed, nearly 40% of the sample (25 of 65) was left "Untyped." The remainder were assigned to four types, Clovis, Folsom, Quad, and Cumberland, and to a combined Enterline-Bull Brook class. In this paper I reassess this typology, and its implications for understanding the initial colonization of Wisconsin, in light of (1) recent research in southern Michigan and Ontario that has led to the recognition of three sequential fluted point complexes (e.g., Deller and Ellis 1988) and (2) observations made on three fluted point sites from Wisconsin, the Boaz mastodon and two assemblages, Aebischer and Withington, that are not yet fully published.

RECENT RESEARCH IN MICHIGAN AND ONTARIO AND ITS IMPLICATIONS

In the 1960s William Roosa was a leader in refining observation of formal properties of Great Lakes fluted points and using these observations to recognize explicit subtypes within what previously had been generally treated as an undivided fluted point class (Roosa 1965; Wright and Roosa 1966). Building upon these insights, the discovery of a number of fluted point sites in southern Michigan and Ontario in the 1970s and 80s saw the establishment of a sequence of fluted point styles based upon a combination of typological and geoarchaeological evidence unaided by direct radiocarbon dates (e.g., Roosa 1977; Roosa and Deller 1982; Storck 1983; Simons, Shott, and Wright 1984; Deller and Ellis 1988). The basic threefold sequence that emerged from this research, which is presumed to span the interval from roughly 9000 B.C. to 8000 B.C., is as follows (from oldest to youngest): Gainey/Parkhill/Crowfield (Deller and Ellis 1988). Before discussing this sequence, it should be pointed out that the absence of the familiar Clovis and Folsom complexes is intentional under the supposition that neither is represented in southern Michigan and Ontario. Thus the type names used and the times allotted can be seen to reflect the view that the fluted point complexes of this region postdate Clovis and parallel Folsom while formally belonging to neither.

The hallmark of the Gainey complex, named for a site in southeastern Michigan (Simons, Shott, and Wright 1984), is a fluted point type whose overall size and shape closely resembles the classic Clovis type of the Great Plains (e.g., Haury, Sayles, and Wasley 1959). Stereotypically, Gainey points have parallel lateral edges from base to mid-body, i.e., are plano-convex in form (Figure 6.1b), but convex (Figure 6.1a) and concavo-convex (Figure 6.1c) edges also occur (Deller and Ellis 1988). Fluting may be single or multiple on one or both faces, but stereotypically is manifest by a single, long flute on each face that extends well beyond the midpoint along the long axis of the point. It differs from Clovis in that fluting is accomplished through bi-beveled basal preparation and the isolation of a central basal striking platform (Simons, Shott, and Wright 1984:268-9). It is noteworthy that not all points within Gainey assemblages are fully fluted (presumably due to lack of success on the part of the flint knappers) and that, isolated from their assemblage, such points can be virtually indistinguishable from Clovis points (Deller and Ellis 1988:255). The age of the Gainey complex is estimated to be ca. 9000-8700 B.C. (Deller and Ellis 1988:255).

The Parkhill complex, named for a site in southwestern Ontario (Roosa 1977; Roosa and Deller 1982), is likewise characterized by fully-fluted points, i.e., what are referred to as Barnes points with Folsom-style fluting associated with the distinctive concavo-convex or "fishtail" form (Figure 6.1c). The distinction between the Gainey and Parkhill complexes depends primarily upon fluted point typology: both have fully-fluted forms that are

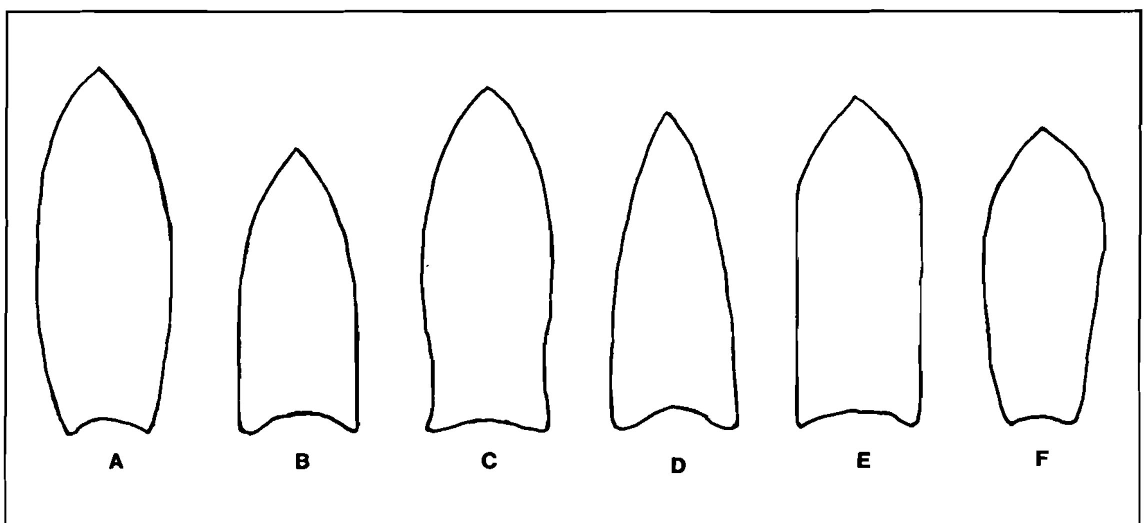


Figure 6.1. Common blade forms of fluted points. A, convex-sided; B, plano-convex; C, concavo-convex; D, convergent; E, parallel; F, divergent.

primarily plano-convex in the case of Gainey, in contrast to the generally smaller, thinner, fish-tailed (i.e., concavo-convex) forms of Parkhill. When assemblages are available for analysis, the two complexes seem distinctive, but at the level of the individual artifact there is considerable overlap since some concavo-convex forms occur in Gainey contexts (although not as extreme as many Parkhill forms; e.g., Deller and Ellis 1988:255-257), while plano-convex- to convex-sided forms occur in Parkhill contexts (e.g., Roosa and Deller 1982:5 and Storck 1983:96). Thus individual Gainey and Barnes fluted points may be difficult to distinguish when dealt with outside the context of an assemblage, as is the case with so many surface finds. The age of the Parkhill complex is estimated to be about 8600 B.C. (Deller and Ellis 1988:258).

The Crowfield complex is characterized by small (i.e., usually less than 6 cm long), thin, broad, well-fluted points (Deller and Ellis 1984; 1988). Besides size, the most distinctive feature of this point type is form (Figure 6.1f): the lateral edges diverge from the base to a position of maximum breadth that is closer to the tip than to the base, producing a form that is sometimes referred to as "shouldered" (Deller and Ellis 1984:44-45). It is believed to post-date 8400 B.C. (Deller and Ellis 1988:258). As with the other two types, the relative

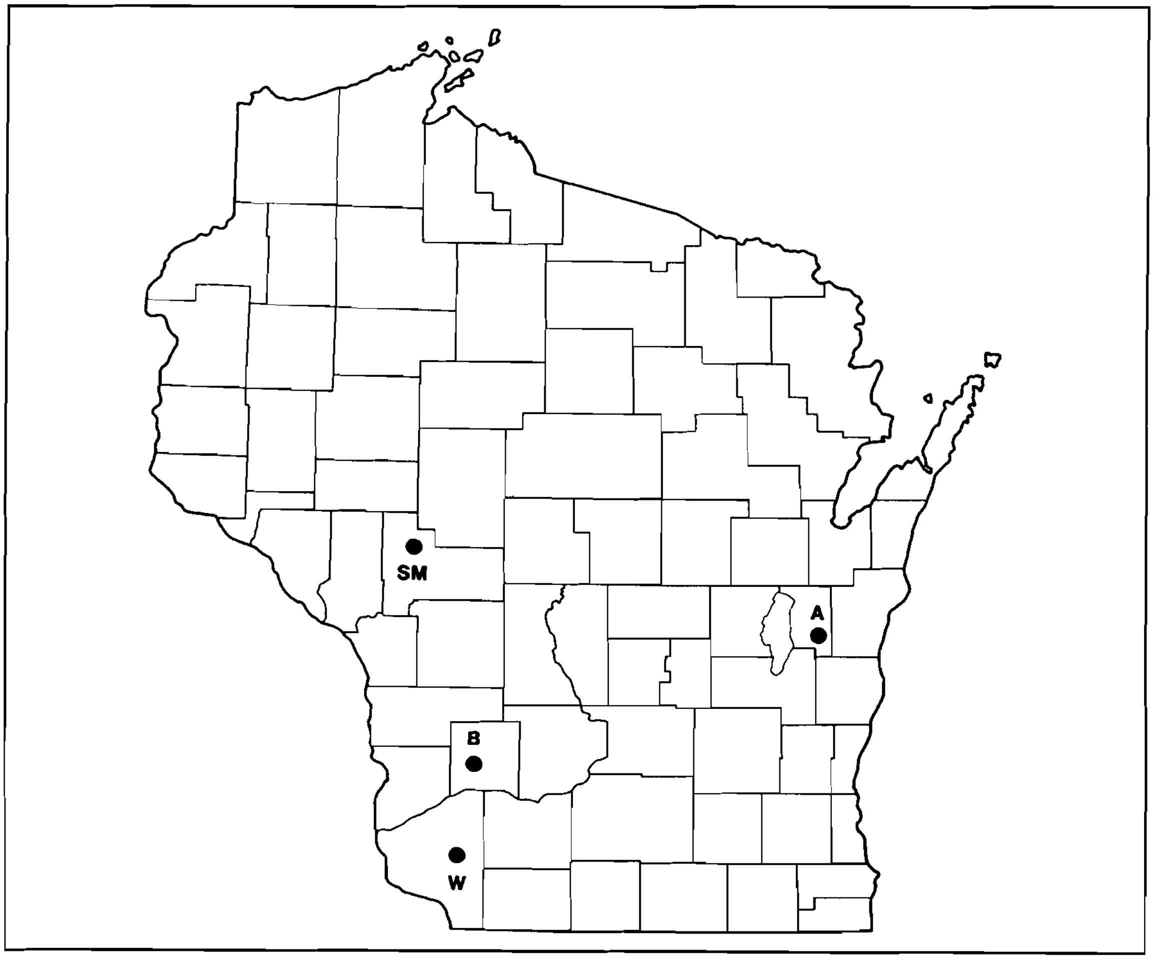


Figure 6.2. Map of Wisconsin showing main sites discussed in the text. SM=Silver Mound; A=Aebischer; B=Boaz; W=Withington.

age of this type is based primarily upon perceived typological trends, i.e., smaller, thinner, better-fluted forms are considered younger (e.g., Deller and Ellis 1988:255).

RECONSIDERING FLUTED POINT DIVERSITY IN WISCONSIN

A reconsideration of the typology of the sample of 65 Wisconsin fluted points originally analyzed in the mid-1960s (Stoltman and Workman 1969) in light of the recent research in Michigan and Ontario reveals a number both of differences and similarities. Unlike Michigan and Ontario, however, Wisconsin has a good representation of true Clovis and Folsom types.

Twenty-two of the 65 points in the 1969 study were classified as Clovis, an assessment that was made on conservative grounds that I still believe to be valid (Stoltman and Workman 1969:207; cf. Roosa 1965:93). The temporal implications of this are that Wisconsin was initially colonized by Paleoindian peoples prior to 9000 B.C., the generally accepted end date for the Clovis complex on the Great Plains (e.g., Haynes 1964). So far, the Clovis complex in Wisconsin is identified solely from isolated surface finds of the diagnostic Clovis points (i.e., plano-convex- or convex-sided points with flute lengths normally less than half, but never greater than three-fifths, of the total point length; Stoltman and Workman 1969:207), with no site presently known that has produced more than a single point of this type. Not discussed in the 1969 study were the lithic materials from which the points were made. In the case of the Clovis points, at least four were made from Hixton quartzite, a distinctive orthoquartzite whose outcrop near the town of Hixton in Jackson County, Wisconsin is locally referred to as Silver Mound (Porter 1961; Figure 6.2), while at least one was made from Moline chert, which outcrops widely near the mouth of the Rock River in Rock Island County, Illinois. Thus the combined evidence suggests that the earliest known occupants of Wisconsin were sparse in numbers, had highly mobile subsistence-settlement systems, and were intimately aware of, and capable of procuring, high-quality lithic resources from both local and distant sources.

Wisconsin is unusual in being one of the two easternmost states (along with Illinois) in which classic Folsom points have been recovered in some numbers–7 of 65 in the 1969 study were of this type (Stoltman and Workman 1969). As with the Clovis type, all were isolated surface finds, so no Folsom assemblage has yet been isolated in the state. All of these points, plus the one illustrated in Figure 6.3, are made from unidentified cherts; none is Hixton quartzite.

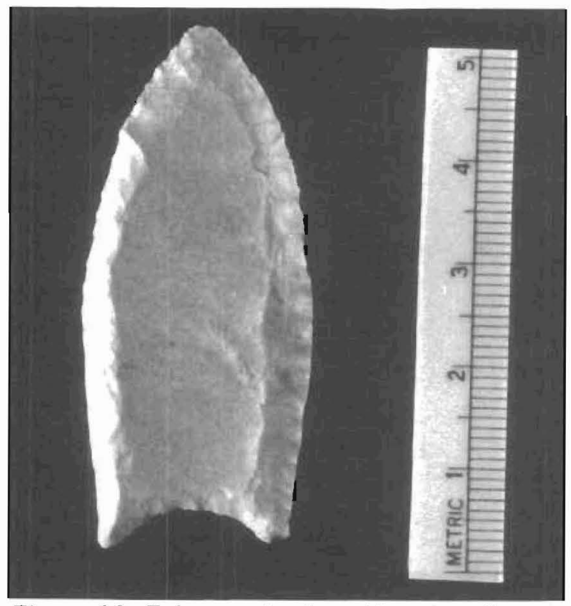


Figure 6.3. Folsom point from Marathon County, Wisconsin.

Folsom points are typically shorter and thinner than Clovis points and are commonly parallel- or divergent-sided in form (Figure 6.1e, f). Flute scars, which characteristically are extremely long and broad and terminate in feather edges rather than step fractures, were produced through indirect percussion or pressure upon isolated basal striking platforms specially prepared for flute removal on each face (Crabtree 1966; Roosa 1965). Following fluting, the base was commonly finished with fine, pressure retouch, a feature not observed on Clovis points (Roosa 1965). Based upon well-dated sites on the Great Plains and the supposition of rough contemporaneity of this highly distinctive point type in Wisconsin, these points should reflect a human occupation of southern Wisconsin sometime during the interval 8800-8200 B.C. (Haynes *et al.* 1984). Figure 6.3 illustrates a recent surface find of a classic Folsom point from Marathon County, Wisconsin. I include it here partly because it has never been published, but more to give substance to the claim, not widely appreciated until recently (e.g., Munson 1990), that Folsom points do, indeed, occur east of the Mississippi River.

Turning now from the Clovis and Folsom complexes, which have no analogues in Michigan and Ontario, I should like to consider the remaining fluted point diversity in Wisconsin in light of the newly established typology for these neighboring provinces to the east. It is clear that the fluted point type presumed to be oldest in Michigan and Ontario, namely, the plano-convex- to convex-sided form (some may also be faintly fishtailed, or concavo-convex) with Folsom-style fluting, is well-represented in Wisconsin. In the 1969 paper Gainey-like points were assigned either to the composite Enterline-Bull Brook category (at least 3, and possibly as many as 5, of the 8) or were left Untyped (at least 7, but possibly as many as 18, of the 25). These assessments are here reconsidered in light of the concept of the Gainey complex of Michigan and Ontario combined with an examination of the only two major fluted point assemblages currently known for the state of Wisconsin.

The Aebischer Site (47 Ct 30)

The Aebischer site has produced a unique fluted point assemblage from the formerly-glaciated terrain on top of the Niagara cuesta east of Lake Winnebago in Calumet County, Wisconsin (Figure 6.2). The site, which definitely has multiple prehistoric components, was owned, cultivated, and collected for many years by Gordon Aebischer. The site was first brought to the attention of professionals in 1966 when Mr. Aebischer informed Robert Hruska of the Oshkosh Public Museum of it. Hruska, and subsequently Alaric Faulkner of UW-Oshkosh, collected from the site on a number of occasions, with the latter also excavating some test pits (without recovering any artifacts *in situ*) in the early 1970s. The preponderance of the materials recovered from the site is currently in the custody of Mr. Aebischer's daughter. A number of scholars have seen at least some of the artifacts from the site over the years, but no description or analysis of these materials had appeared in print until Richard Mason published a preliminary report of his observations in 1988. I had the opportunity to examine these materials on two occasions, once in 1978 when Mr. Aebischer was alive, and again in 1989 after his death. The ensuing discussion and Figures 6.4 and 6.5 derive from these observations. The artifact frequencies reported below were recorded with the assistance of E. Steve Cassells, a graduate student who accompanied me on the 1989 visit and used this opportunity to write a term paper for one of my classes (Cassells 1989).

The first problem in dealing with the Aebischer collection is defining valid assemblages from among hundreds, possibly thousands, of artifacts, most of which lack specific provenience information. Projectile points from virtually all prehistoric periods are present, but the diagnostic fluted points (five whole, or nearly whole, and 11 fragments) can be readily isolated. To these can probably be added the 11 endscrapers and eight gravers in the collection. In addition, all fragments of Moline chert can almost certainly be assigned to the fluted point component because the only diagnostic artifact types of this distinctive material in the collection (e.g., 11 fluted points or fluted point fragments, six gravers, and nine endscrapers) are unambiguously Paleoindian types. Following this line of reasoning, there is a minimum total of 224 artifacts in the Aebischer

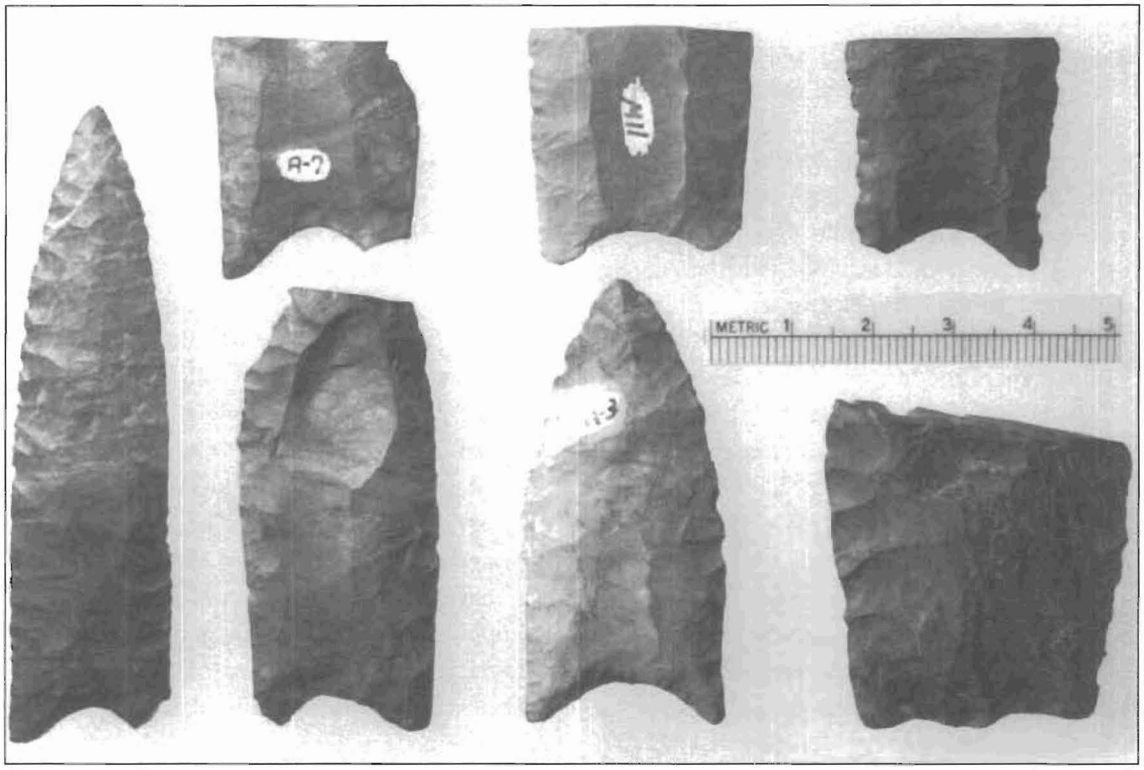


Figure 6.4. Six fluted points and one unfluted preform (lower right) from the Aebischer site; all are Moline chert.

collection that can be assumed to derive from the fluted point component. This total includes 215 artifacts of Moline chert–15 bifaces or biface fragments, 44 retouched flakes, and 130 unretouched flakes–along with the 26 diagnostics (i.e., fluted points, gravers, and endscrapers) mentioned previously. This total is impressive, for it is the greatest number of artifacts of Moline chert from any Wisconsin site of which I am aware, and it occurs over 200 linear miles from the presumed source of this material near the mouth of the Rock River. The nine non-Moline artifacts in the fluted point assemblage include three points or point fragments of Burlington chert (probably from central or southern Illinois) and one of Prairie du Chien chert. At least five flakes of Hixton quartzite were observed in the larger collection, but all lacked diagnostic properties that would permit a reliable assessment of their cultural affiliation.

Two other aspects of this assemblage are noteworthy. First, a number of cortical flakes of Moline chert are present, suggesting that on-site knapping of this material occurred. Second, numerous flakes and bifaces show pot-lid fractures and/or crazing, indicative of thermal alteration (but whether or not this was intentional is uncertain because clear luster differences were not observed).

In the context of this unique assemblage, it is now possible to draw some typological inferences beyond what was possible in the 1969 study when the fluted points analyzed were all isolated surface finds. A cursory examination of the five nearly complete fluted points from the site (Figures 6.4 and 6.5) quickly reveals two salient facts: (1) although differences in outline form are subtle, the forms represented are, nonetheless, quite variable, with plano-convex-, convex-, convergent-, and even slightly concavo-convex-sided forms represented; all but

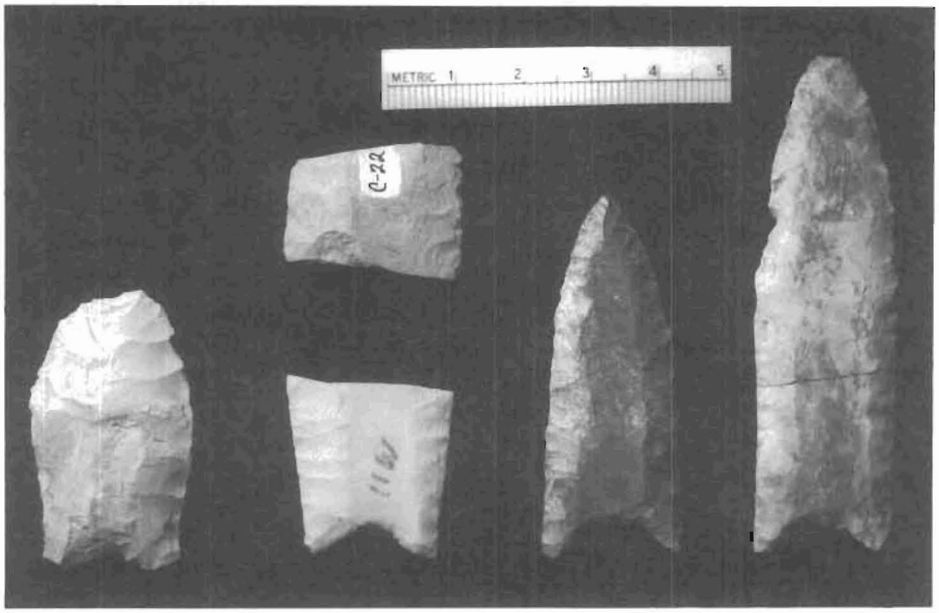


Figure 6.5. Five fluted points from the Aebischer site; all are of cherts other than Moline.

the convergent form (Figure 6.4, left and Figure 6.5, second from right) are typical Clovis forms; (2) the flute length on two of the complete points (and probably a number of the broken ones as well) exceeds three-fifths of the total point length, i.e., suggests a Folsom-type rather than a Clovis-type fluting technology. The latter observation is confirmed by an unfluted preform that retains a partially completed, but never utilized, centrally isolated striking platform (Figure 6.4, lower right). In other words the Aebischer fluted point assemblage conforms closely to the typological criteria used to define the Gainey complex, a suggestion made earlier by Roosa and Deller (1982:4) with which I agree.

This may be pushing typological inference a bit too far, but the presence of convergent-sided forms with Folsom-quality fluting in the Aebischer assemblage, which are not reported, so far as I am aware, from the Gainey complex in Michigan and Ontario, might simply reflect minor regional diversity in Wisconsin, or might possibly be viewed as indicative of a late or even slightly post-Gainey age for the Aebischer site. This form (Figure 6.1d) is common in what are presumably late fluted point complexes in the Northeast, such as Bull Brook and Debert (Byers 1954; MacDonald 1968), but it is possible that some points have this form as a byproduct of reworking after tip breakage (Grimes 1979).

The Withington Site (47 Gt 158)

The Withington site is located on a ridge overlooking the Platte River in the heart of the rolling uplands of the Driftless Area of southwestern Wisconsin (Figure 6.2). The site was first brought to the attention of Harris A. Palmer, a geologist with strong archaeological interests, by a student in one of his classes at UW-Platteville in 1962. Responding to one of Palmer's lectures on the initial colonization of the New World, the student brought to class one day a fluted point made of Hixton quartzite that he had found on his family farm. Following up on this lead, Palmer conducted an archaeological field school at the site in 1964. He excavated 38 five-foot squares on a plowed, loess-covered ridge whose surface had yielded not only the Hixton fluted point, but also a substantial amount of Hixton debitage. Despite the relative richness of the surface collections (over 425 artifacts), the excavations were disappointing, for they recovered only a few artifacts, all in plow zone contexts.

The materials collected from the site were in Palmer's custody for a number of years and then were donated to the University of Wisconsin-Madison, where they are presently housed. They have not yet been analyzed nor published. I have subsequently visited the site on a number of occasions, but it appears now to have been largely "picked dry": the last three visits to the site have netted a total of but a single flake of Hixton quartzite.

The site appears to have been a relatively small, probably seasonal camp that was occupied almost exclusively by fluted point makers. The present collection of 441 lithic artifacts includes five chert projectile points, or point fragments, that are of Early and Middle Archaic types, the only items in the collection that unequivocally postdate the fluted point component. However, most of the remaining 177 chert artifacts, mostly flakes, could be of any age. By contrast, the 256 Hixton quartzite artifacts, along with a lone rhyolite flake and a number of diagnostic scrapers and gravers of chert, jasper, and chalcedony, almost certainly are all attributable to the fluted point component. The case for assigning all Hixton artifacts to the fluted point component is strong, for there are several diagnostic Paleoindian artifacts made of this material, but not one identifiable post-Paleoindian diagnostic.

The most striking feature of the Withington collection is the large amount of Hixton quartzite present. This is especially noteworthy in light of the distance (approximately 105 air miles) between the site and Silver Mound (Figure 6.2). I know of no other site in southwestern Wisconsin, of any age, that has produced this much Hixton quartzite. Included in the inventory of Hixton items at Withington are four or five fluted points (one tip fragment may not be from a separate point), one fluted preform and 15 other bifaces, 12 endscrapers,

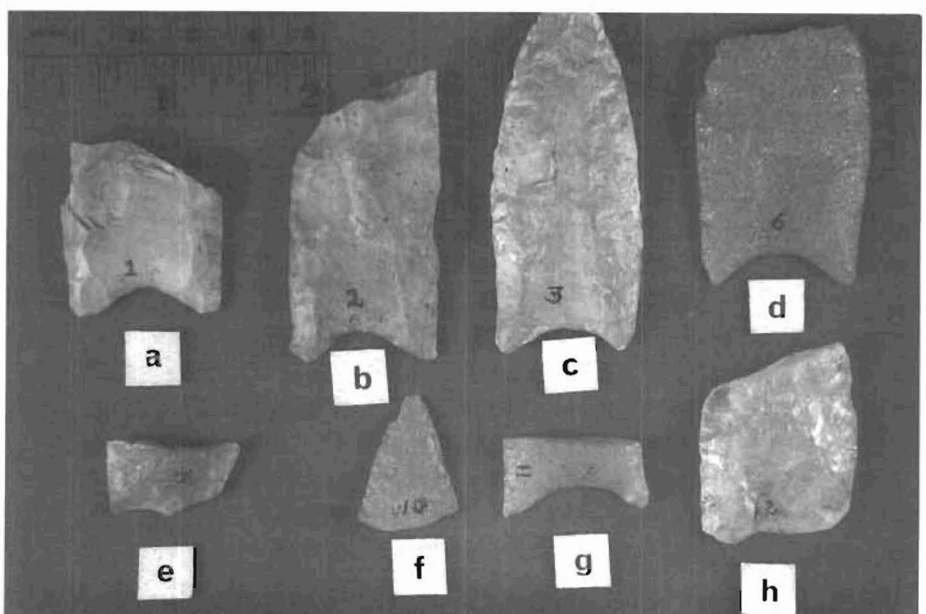


Figure 6.6. Fluted points from the Withington site.

3 gravers, 87 retouched flakes, and 133 fragments of unretouched debitage. To this inventory of Hixton artifacts I would also add two chert fluted points, a unique quartz crystal fluted point base, seven chert endscrapers, eight jasper and six gray chalcedony flakes (most retouched), the one rhyolite flake, and two hammerstones, bringing the artifact total for the fluted component, minimally, to 283 of the site's 441 artifacts.

In assessing the cultural affiliation of the Withington fluted point component the eight fluted points and point fragments are of primary importance. Of these, only one is complete, one is an undiagnostic tip fragment, and six are basal fragments (Figure 6.6). Unfortunately, the one complete point, which was retained by the Withington family, has disappeared. A photograph of the original was taken by Palmer, and a plaster cast was also made of it (Figure 6.6c). The basic edge configuration can be ascertained for six of the seven points (excluding the tip). With the exception of one base, which is simply too small for reliable observation (Figure 6.6g), it can be seen that the edges of six of the points range from plano-convex to convex in form. In short the size and outline form of the Withington fluted points are well within the range of classic Clovis points.

While the fragmentary nature of the point sample makes observation of fluting characteristics difficult, there is ample evidence to indicate that Folsom-like rather than Clovis-like fluting was utilized. Most important is the basal fragment, one of the few artifacts recovered during Palmer's 1964 excavations, that retains evidence of a prepared striking platform in the center of the base (Figure 6.6e). This base, along with the presence of a flute on the complete point that exceeds three-fifths of total point length and the unusually broad flutes on both faces of a second basal fragment (Figure 6.6a), are all consistent with the view that Folsom-style fluting was practiced by the Withington occupants.

Several additional observations on the Withington fluted point component merit brief mention. A basal fragment of a quartz crystal fluted point (Figure 6.6h) is unique, so far as I am aware, in Wisconsin. The use of quartz crystal for fluted point manufacture, while not especially common, nonetheless does recur widely in North America (Reher and Frison 1991). Some noteworthy examples include a probable Gainey point from Newcastle, Ontario (Roberts 1984:264), as well as similar forms from Pennsylvania (Dickson 1967) and Virginia (McCary 1951). Much farther afield, three Clovis points of quartz crystal have been reported from each of two Western sites, Lehner in Arizona (Haury *et al.* 1959: Fig. 12, a-c) and the Fenn Cache in the Idaho/Wyoming border area (Reher and Frison 1991:388-392).

Another noteworthy feature of the Withington assemblage is the relative abundance and diversity of non-local materials present. Besides the Hixton quartzite and quartz crystal, other exotic materials almost certainly associated with the fluted point component include jasper, gray chalcedony, rhyolite, and Burlington chert. It is currently impossible to calculate the percentages of local versus exotic materials in the fluted point assemblage because of the impossibility of reliably assigning the chert debitage to any component. Overall, however, it is likely that fully half of the lithic inventory of the fluted point component at Withington is comprised of exotic lithic materials.

The fluted point component at the Withington site, like that of the Aebischer site, is clearly neither Clovis nor Folsom. Even more than Aebischer (with its more variable fluted point forms, including convergent-sided types), the Withington assemblage finds its closest analogues in the Gainey complex of Michigan and Ontario. Indeed, considering the generally close spatial proximity and similar environmental context, I find it reasonable to consider Withington a local manifestation of the Gainey complex. But see Frison 1991 for new views on the Goshen complex as a candidate for a Clovis/Folsom intermediary on the northern plains.

The Boaz Mastodon

Before concluding, a brief reconsideration of the Boaz mastodon site is in order in light of the foregoing discussion. As described in an earlier paper (Palmer and Stoltman 1976), there is a reasonable possibility that a mastodon discovered in 1897 in Richland County, Wisconsin (Figure 6.2), was associated with a fluted point (Figure 6.7).

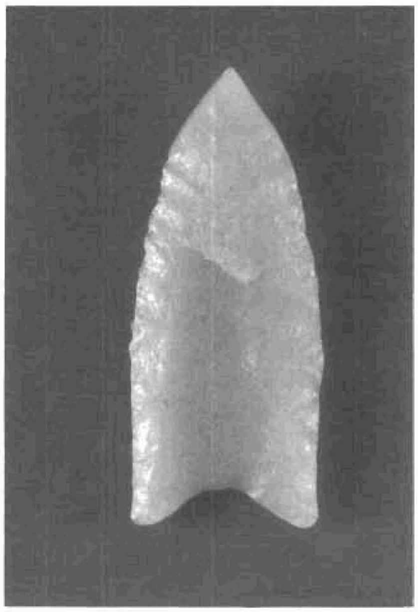


Figure 6.7. Gainey fluted point of Hixton quartzite reportedly found with the Boaz Mastodon; actual size.

The fluted point in question, which is made of Hixton quartzite, was included as point number 42 in the 1969 study by Stoltman and Workman. At that time we classed it as "Untyped" primarily because the length of fluting on one face exceeded three-fifths of the total point length, i.e., was too long for inclusion in the Clovis type despite the otherwise Clovis-like qualities of the point. In light of the discussion above it now seems reasonable to assign this point to the Gainey type.

CONCLUSIONS

Recent research in southern Michigan and Ontario has led to the recognition of a presumably post-Clovis complex characterized by fluted points with Clovis forms (i.e., plano-convex- to convex-sided) that were fluted, however, by the Folsom technique. Unlike the simpler Clovis technique (e.g., see Crabtree 1966), the Folsom technique involved extensive pre-flute basal preparation, including the careful isolation of a central basal striking platform, from which long, broad flutes were detached from both faces by either indirect percussion or pressure. A reconsideration of fluted point typology in Wisconsin in conjunction with an appraisal of the three most important fluted point sites in the state, Aebischer, Withington, and Boaz, has revealed that a non-Clovis and non-Folsom fluted point manifestation is well-represented.

The closest analogue for this complex, typologically, spatially, and presumably temporally as well, is the Gainey complex of southern Michigan and Ontario. Accordingly, it seems reasonable to recognize formally a Gainey-like complex within Wisconsin that is distinct from the Clovis and Folsom complexes that are also represented in the state. Withington and Boaz (at least the fluted point) can be unambiguously assigned to this complex, while Aebischer, which is unique in many ways, should perhaps be considered either a late or idiosyncratic variant of this complex. As in Michigan and Ontario, the precise age of this complex remains to be determined accurately, but a position within the ninth millennium B.C., probably the first half, seems likely. Almost certainly it was a post-Clovis phenomenon, but its relationship with the Folsom complex remains to be determined. On purely typological grounds Gainey points could be viewed as intermediate between Clovis and Folsom, raising the interesting possibility that the Gainey complex represents the missing transitional stage between the two better known fluted point complexes. In raising this possibility I am not suggesting that the origins of the Folsom complex are to be found in Wisconsin, but rather that a more widespread but not yet recognized Gainey-like fluted point stage, intermediate in age between Clovis and Folsom, may also exist on the Great Plains. (But see Frison 1991 for new views on the Goshen complex as a candidate for a Clovis/Folsom intermediary on the northern plains.)

Since neither the Parkhill nor the Crowfield complexes seem to be represented in Wisconsin, the possibility that they rather than Gainey were Folsom contemporaries (with Crowfield possibly being post-Folsom in age) is the view favored here. Possible candidates for post-Folsom fluted points in Wisconsin could be the few Quad-like and Cumberland-like points that have so far been reported (Stoltman and Workman 1969), but their precise age and typological affinities remain to be confirmed. In sum, then, this reconsideration of fluted points in Wisconsin has led to the recognition of at least three complexes, Clovis, Gainey, and Folsom, with Gainey being the only one so far represented by true assemblages as opposed to isolated finds of diagnostic artifact types. The suggestion has been offered, tentatively to be sure, that the Gainey complex be considered intermediate in age between Clovis and Folsom and that something akin to it will eventually be recognized on the Great Plains as the immediate ancestor of the Folsom complex.

REFERENCES

Anderson, David G.
1990 The Paleoindian Colonization of Eastern North America: A View from the Southeastern United States. In *Early Paleoindian Economies of Eastern North America*, edited by Kenneth B. Tankersley and Barry L. Isaac, 163-216. Research in Economic Anthropology, Supplement 5. JAI Press Inc., Greenwich.

Byers, Douglas S.
1954 Bull Brook–A Fluted Point Site in Ipswich, Massachusetts. *American Antiquity* 19:343-351.

Cassells, E. Steve
1989 The Aebischer Site (47 CT 30): An Analysis of the Fluted Point Stage Assemblage. Term paper for Anthropology 310, Department of Anthropology, University of Wisconsin-Madison.

Crabtree, Don E.
1966 A Stoneworker's Approach to Analyzing and Replicating the Lindenmeier Folsom. *Tebiwa* 9:3-39.

Deller, D. Brian, and C.J. Ellis
1984 Crowfield: A Preliminary Report on a Probable Paleo-Indian Cremation in Southwestern Ontario. *Archaeology of Eastern North America* 12:41-71.
1988 Early Palaeo-Indian Complexes in Southwestern Ontario. In *Late Pleistocene and Early Holocene Paleoecology and Archeology of the Eastern Great Lakes Region*, edited by R.S. Laub, N.G. Miller, and D.W. Steadman, 251-263. Bulletin of the Buffalo Society of Natural Sciences, Vol. 33. Buffalo.

Dickson, James A.
1967 A Rock Crystal Fluted Point from the Cumberland Valley, Pennsylvania. *Pennsylvania Archaeologist* 37:1-4.

Frison, George C.
1991 The Goshen Paleoindian Complex: New Data for Paleoindian Research. In *Clovis: Origins and Adaptations*, edited by Robson Bonnichsen and Karen L. Turnmire, 133-151. Center for the Study of the First Americans, Corvallis.

Gramly, Richard Michael, and Robert E. Funk
1990 What is Known and Not Known About the Human Occupation of the Northeastern United States Until 10,000 B.P. *Archaeology of Eastern North America* 18:5-31.

Grimes, John R.
1979 A New Look at Bull Brook. *Anthropology* 3(1-2):109-130.

Haury, Emil W., E.B. Sayles, and William W. Wasley
1959 The Lehner Mammoth Site, Southeastern Arizona. *American Antiquity* 25:2-42.

Haynes, C. Vance, Jr.
1964 Fluted Projectile Points: Their Age and Dispersion. *Science* 145:1408-1413.

Haynes, C. Vance, D.J. Donahue, A.J.T. Jull, and T.H. Zabel
1984 Application of Accelerator Dating to Fluted Point Paleoindian Sites. *Archaeology of Eastern North America* 12:184-191.

Levine, Mary Ann
1990 Accommodating Age: Radiocarbon Results and Fluted Point Sites in Northeastern North America. *Archaeology of Eastern North America* 18:33-63.

MacDonald, George F.
1968 *Debert: A Palaeo-Indian Site in Central Nova Scotia.* National Museum of Canada Anthropology Papers 16. Ottawa.

Mason, Richard P.
1988 Preliminary Report on the Fluted Point Component at the Aebischer Site (47Ct30) in Calumet County, Wisconsin. *The Wisconsin Archeologist* 69:211-226.

Mason, Ronald J.
1962 The Paleo-Indian Tradition in Eastern North America. *Current Anthropology* 3:227-278.

McCary, Ben C.
1951 A Workshop Site of Early Man in Dinwiddie County, Virginia. *American Antiquity* 17:9-17.

Munson, Patrick J.
1990 Folsom Fluted Projectile Points East of the Great Plains and Their Biogeographical Correlates. *North American Archaeologist* 11:255-272.

Palmer, Harris A., and James B. Stoltman
1976 The Boaz Mastodon: A Possible Association of Man and Mastodon in Wisconsin. *Midcontinental Journal of Archaeology* 1:163-177.

Porter, James
1961 Hixton Silicified Sandstone: A Unique Lithic Material Used by Prehistoric Cultures. *The Wisconsin Archeologist* 42:78-85.

Reher, Charles A., and George C. Frison
1991 Rarity, Clarity, Symmetry: Quartz Crystal Utilization in Hunter-Gatherer Stone Tool Assemblages. In *Raw Material Economies Among Prehistoric Hunter-Gatherers*, edited by A. Montet-White and S. Holen, 375-397. University of Kansas Publications in Anthropology 19. Lawrence.

Roberts, Arthur
1984 Paleo Indian on the North Shore of Lake Ontario. *Archaeology of Eastern North America* 12:248-265.

Roosa, William B.
1965 Some Great Lakes Fluted Point Types. *The Michigan Archaeologist* 11:89-102.
1977 Great Lakes Paleoindian: The Parkhill Site, Ontario. In *Amerinds and Their Paleoenvironments in Northeastern North America*, edited by Walter S. Newman and Bert Salwen, 349-354. Annals of the New York Academy of Sciences, Vol. 288. New York.

Roosa, William B., and D. Brian Deller
1982 The Parkhill Complex and Eastern Great Lakes Palaeo-Indian. *Ontario Archaeology* 37:3-15.

Simons, Donald B., Michael J. Shott, and Henry T. Wright
1984 The Gainey Site: Variability in a Great Lakes Paleo-Indian Assemblage. *Archaeology of Eastern North America* 12:266-279.

Stoltman, James B., and Karen Workman
1969 A Preliminary Study of Wisconsin Fluted Points. *The Wisconsin Archeologist* 50:189-214.

Storck, Peter L.
1983 The Fisher Site, Fluting Techniques, and Early Palaeo-Indian Cultural Relationships. *Archaeology of Eastern North America* 11:80-97.

Wright, Henry T., and William B. Roosa
1966 The Barnes Site: A Fluted Point Assemblage from the Great Lakes Region. *American Antiquity* 31:850-860.

7 Caribou, Walrus and Seals: Maritime Archaic Subsistence in Labrador and Newfoundland

Arthur E. Spiess
Maine Historic Preservation Commission
Augusta, Maine

INTRODUCTION

An extensive Archaic Period Indian coastal adaptation around the Gulf of St. Lawrence and Newfoundland, extending into arctic environments along the north coast of Labrador, has been recognized in the last twenty years (Cox 1977; Fitzhugh 1972, 1978; McGhee and Tuck 1975; Tuck 1976): the Maritime Archaic, circa 9000 to 3500 B.P. Faunal data constitute the sole record of subsistence in this region, where plants contributed little to diet. Faunal analysis, interdependent with settlement pattern analysis, can contribute to understanding three aspects of Maritime Archaic culture history. First is the question of cultural evolution from a postulated Late Paleoindian progenitor and possible characterization of the general nature of the Maritime Archaic adaptation. The amount and intensity of terrestrial hunting (primarily caribou hunting) versus sea mammal hunting in Maritime Archaic origins and adaptations are crucial issues in addressing this question. Secondly, such evidence can help to explain the cause of Late Maritime Archaic range expansion onto the north Labrador Coast, expansion of social group size and residence units along the entire Labrador coast, and late intensification of mortuary behavior in the whole Maritime Archaic range. If one cause of this cultural success were ecological, we would expect some shifts in subsistence patterns as the cultural patterns intensified. Thirdly, the nature of Maritime Archaic "influence" southward into the Gulf of Maine circa 4000 B.P., including specific aspects of mortuary behavior and transport of items of material culture, has been a focus of research. At one time a caribou and seal hunting adaptation was postulated for the Red Paint or Moorehead Phase group living along the Gulf of Maine as a partial explanation of this phenomenon.

Enough archaeological faunal and paleoenvironmental data have now accumulated to test and refine the initial hypotheses concerning these three questions. I shall dispose of the third question first. The Moorehead Phase of the Gulf of Maine (Bourque 1969, 1975; Sanger 1973, 1975) was once subsumed under a geographically comprehensive definition of Maritime Archaic (Tuck 1975). A caribou and seal subsistence base, like that of their northern contemporaries (Snow 1974), was at first hypothesized for the Moorehead Phase people along the Gulf of Maine coast. Comparatively large faunal samples now falsify this subsistence hypothesis. Moorehead Phase subsistence was based on intensive summer cod fishing and swordfish hunting combined with fall-winter-spring terrestrial white-tailed deer hunting (Spiess *et al.* 1983; Spiess and Lewis 1990). Seals played a minor role in Moorehead Phase subsistence. Moreover, the pattern of intensive cod fishing and swordfish hunting has earlier Archaic antecedents around the Gulf of Maine that predate demonstrable Maritime Archaic influences. Thus, the Moorehead Phase of the Gulf of Maine does not demonstrate a close parallel developmental history with the Maritime Archaic farther north. Moorehead Phase subsistence patterns are part of a complex mosaic of interior, riverine, and coastal Archaic subsistence patterns (Spiess n.d.) characteristic of northern New England. The causes of the cultural influence or convergence between the Moorehead Phase and Maritime Archaic circa 4000 B.P. must be sought outside the concepts of common culture history and similar subsistence base, and outside the scope of this essay. (Geographic place names for the area discussed in this paper are presented in Figure 7.1.)

The rest of this paper focuses on the first two questions above: the characterization of Maritime Archaic subsistence and its development, and possible subsistence bases for Late Maritime Archaic expansion and efflorescence. In an initial characterization of Late Maritime Archaic subsistence patterns Fitzhugh (1972 et seq.) emphasized a seasonal maritime orientation, particularly small seal hunting, but always included a seasonal interior hunting component necessarily focused on caribou hunting. Hypotheses of the origin of Maritime Archaic culture from a Late Paleoindian ancestral culture in the Gulf of St. Lawrence (McGhee and Tuck 1975) postulated a caribou hunting origin with increasing additions of marine mammal (primarily seal) hunting capability during the Early and Middle Maritime Archaic. In contrast, this review promotes a different view of the development of Maritime Archaic subsistence. I hypothesize that Maritime Archaic adaptation was primarily coastal from its Early Archaic beginnings, initially focused on seals and walrus in the lower Gulf of St. Lawrence. It is possible also that regular hunting of large whales developed as an important economic activity. A major interior caribou hunting adaptation, even a seasonal one, was not possible in Labrador until well after the demise of Maritime Archaic. Maritime Archaic furbearer trapping and caribou hunting in Labrador, and possibly also Newfoundland, involved relatively short absences from the coast, usually going no farther than the near-coastal forest.

The Late Maritime Archaic population concentration at Nulliak (Fitzhugh 1984, 1985), on the north Labrador coast, was supported in part by caribou hunting. This Nulliak occupation provided access to a lithic source (Ramah Chert, an outcrop located 70 km farther north) that was a cultural marker of Late Maritime Archaic lithic technology, a fact recognized as far south as Maine, where Ramah Chert artifacts made in Labrador were valued objects interred as grave goods. Moreover, the Late Maritime Archaic occupations of the Nulliak area were maintained in the face of pressure from immigrant Paleoeskimo groups. Occurrence of substantial numbers of caribou in northeast Labrador became possible only during the late Holocene, before which time caribou were confined to the near-coastal region of north Labrador in relatively small herds. In this sense, the Nulliak Maritime Archaic efflorescence may in part have been a response to a shift in caribou range or a localized caribou population peak. The importance of maritime hunting versus terrestrial hunting to the development of Maritime Archaic is thus reversed in my view from that presented by earlier authors: sea mammal hunting defines the tradition, while increased caribou hunting certainly aided the survival of Late Maritime Archaic.

In the sections that follow, I present a review of the regional environment and paleoenvironmental change; a summary of previous hypotheses of Maritime Archaic subsistence and settlement; summary biological data on major Maritime Archaic prey species including several seal species, walrus, and caribou; a review of Maritime Archaic faunal data; and a synthesis.

My interests in Labrador began as an archaeological field crew member under the direction of William Fitzhugh and Steven Cox (both Steve Williams' students) during 1973, 1974, 1976, and 1977. A synthesis of the archaeology of caribou hunters (Spiess 1979) followed, an interest I developed in part in graduate seminars with Williams and with H.L. Movius, Jr. Most of my

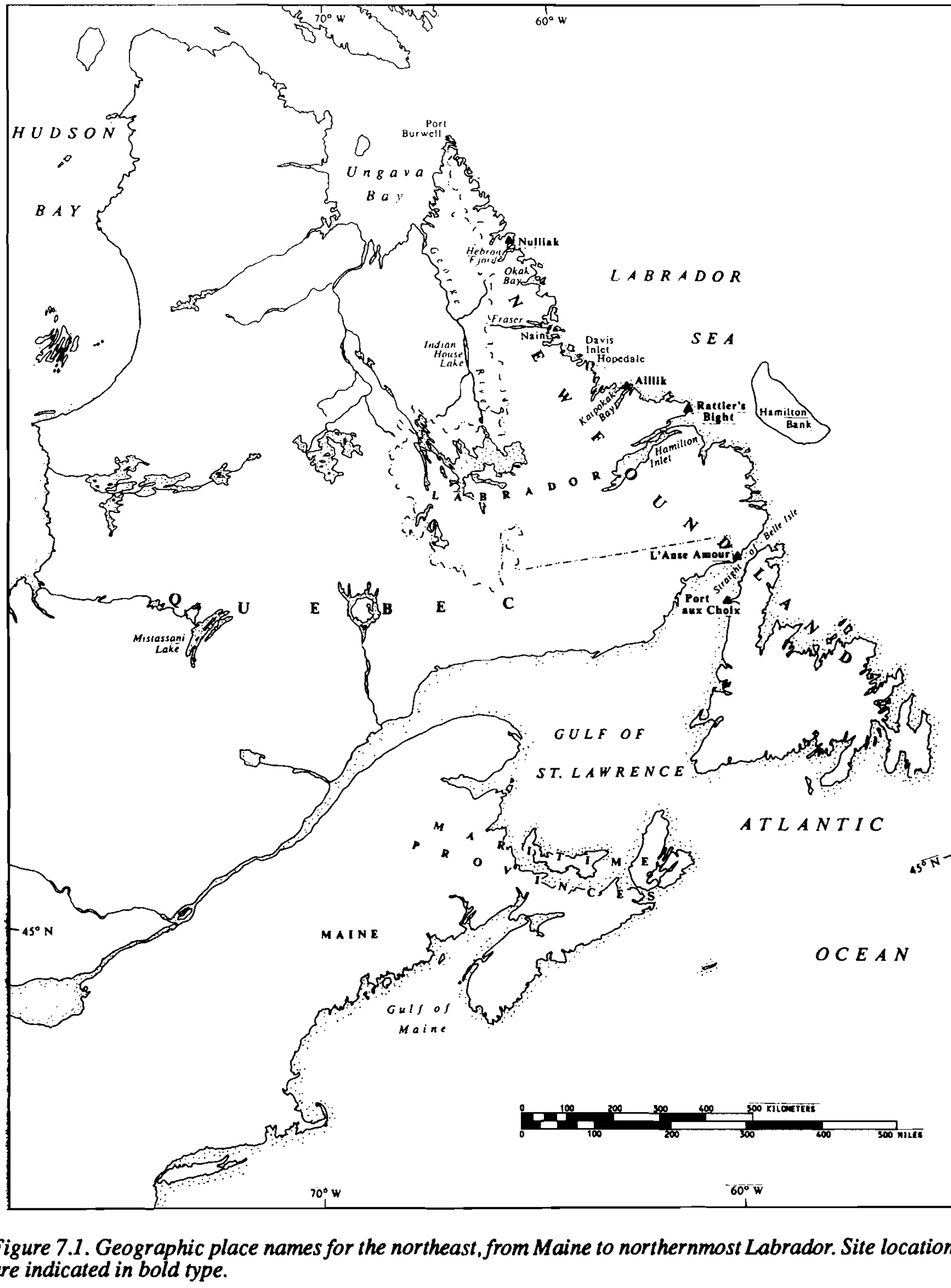


Figure 7.1. Geographic place names for the northeast, from Maine to northernmost Labrador. Site locations are indicated in bold type.

Labrador faunal research has been based on Neo-Eskimo (Inuit) and Paleo-Eskimo collections (Spiess 1976, 1977, 1978, 1991; Cox and Spiess 1980), primarily because of the enormity and high quality of the Eskimo faunal record. My original research contribution for this paper consists of a reanalysis of Maritime Archaic calcined bone faunal collections from the Hamilton Inlet area, originally reported by Savage in Fitzhugh (1972:212-215), and a revision of the synthesis (Spiess 1979) of the interaction of humans and caribou in Labrador. The recent (1980s) Labrador and Greenland caribou biology research that forms the basis of that synthesis, other archaeological faunal data, reviews of the paleoenvironment, and summaries of seal, walrus, and whale biology, are based on published literature as cited. My faunal identifications will be presented in detail in Labrador archaeology volumes under the editorship of William Fitzhugh for the Smithsonian Institution.

GEOGRAPHY AND PALEOENVIRONMENT

Pre-industrial age Native occupation of the Labrador-Newfoundland region could have been based upon only two economic strategies: sea mammal hunting (for several seal species, walrus, large and small whales) or caribou hunting. Sea mammal hunting was necessarily confined to a narrow coastal zone of islands, fjords, and bays. Caribou hunting could be pursued either near the coast or in the vast interior. Interior subsistence could be supplemented with seasonally nesting waterfowl and furbearer trapping. Coastal subsistence could be supplemented by hunting polar bear, seasonally available waterfowl, or fishing for cod or salmonidae. *The most successful economic strategies, i.e., those that supported the largest population concentrations or survived for centuries, involved combinations of sea mammal hunting and caribou hunting.* The exact nature of this combination involved caribou availability near the coast and/or a transportation system capable of extending the economic reach of coastal groups up to several hundred kilometers inland (e.g., Inuit dog sleds). The exact seasonal scheduling of the combination was dependent upon various factors of technology and local seasonal species availability.

The Inuit (Labrador Eskimo) subsistence adaptation and settlement pattern circa 1800 A.D. supported local groups of between 30 and 300 persons (Taylor 1974). Circa 1780 the annual average catch calculated from mission records for 300 Inuit and about 350 dogs living in the Okak Bay area approximated 700 harp seals, 700 ringed seals, 25 walrus, 2 baleen whales, 300 caribou, plus assorted other seals, small whales, bears, birds, and fish. Brice-Bennett (1977) described the modern Inuit and Labrador settler hunting and fishing adaptation on the north Labrador coast. The historic Innu (Naskapi) adaptation is less completely described (Speck 1935; Henrickson 1973), but consisted primarily of interior caribou hunting, with periodic famines. The Native American subsistence adaptation of Newfoundland and the north shore of the Gulf of St. Lawrence is much less well known because of early European contact and Native extinction (Newfoundland) or acculturation (St. Lawrence) before extensive written records.

The geographic focus of this paper is the north shore of the Gulf of St. Lawrence (eastern Quebec and southern Labrador), the island of Newfoundland, the central and north coast of Labrador, and peripherally the interior Quebec-Labrador peninsula (Figure 7.1). This region has been characterized as rock, sea, and ice that "God gave to Cain." The sea is now cooled by a southward flowing Labrador current, which maintains coastal tundra in southern Labrador and northern Newfoundland. The interior of Labrador-Quebec, and most of central Newfoundland, is a boreal forest of varying openness comprised primarily of spruce, larch, birch, lichen ground cover, sedge, and willow. Bedrock is mostly Cambrian granites, gneisses, and related rocks of the Canadian Shield. The interior Quebec-

Labrador peninsula is a raised surface with average elevation of roughly 500 meters and relief of several hundred meters. The northern peninsula of Newfoundland and the north coast of Labrador comprise mountain ranges rising up to a thousand meters out of the sea in places. Coastal fjords, bays, offshore islands, and rocks are common features. Recently deglaciated, the interior is a maze of lakes and wet tundra. A few large and many small rivers drain precipitously to the coast.

Freshwater puddles begin to freeze along the north Labrador coast in late August. By December, a stable fringe of landfast sea ice (marked at its outward edge by the *sina*, comprising leads and broken ice) has begun to form in north Labrador. By mid-winter all shorelines in the study area are fringed with stable ice of varying widths dependent upon prevailing winds and current, with broken pan ice in the Gulf of St. Lawrence and Labrador sea. The ice breaks up in May and June and, depending upon prevailing winds and currents, may hug portions of the coast until early July as broken pan ice.

Winter snowfall accumulation in the interior forest is often very deep (1000 cm). Summer conditions can be wet, windy, and just above freezing, or hot, dry and desperately loaded with mosquitoes and black flies, depending upon prevailing winds and nearness to the coast.

Nearly the entire Labrador-Quebec peninsula was covered with a remnant of the Laurentide ice sheet as recently as 9000 B.P. Deglaciation of the central Labrador coast did not occur until approximately 8500 and 7600 B.P. at Nain and Hopedale respectively. An ice mass in Ungava Bay maintained huge proglacial lakes in the Georges River Valley until circa 7000 B.P. (Clark and Fitzhugh 1990). The final ice sheet remnant in the center of the peninsula became stagnant circa 6200 B.P. and finally disappeared circa 5500 B.P. (Richard *et al.* 1982). As the ice in central interior Quebec-Labrador melted and uncovered mineral soil, it was almost immediately replaced by a relatively thick woodland composed of larch, paper birch, black spruce, and poplar, with some willow, alder, lichen, and herbaceous plants. Between 4700 and 4400 B.P. this forest cover was more dense than it is today. Only after 1600 B.P. does the pollen record reflect a substantial opening of the dense boreal forest in the center of the Quebec-Labrador peninsula to the more open conditions of today.

In southeastern interior Labrador tundra conditions or shrub tundra survived from between 10,000 and 9000 B.P. (as the ground was deglaciated) until between 8000 and 6000 (Engstrom and Hansen 1984) or circa 6000 B.P. (Lamb 1980). An open forest of white spruce and fir covered the landscape (circa 8000 to 6500 B.P., Engstrom and Hansen [1984]; 6000 to 4000 B.P., Lamb [1980]), giving way to a closed black spruce forest after 6500 to 6000, or 4000 B.P. (*ibid.*). The forest in southeasternmost Labrador opened slightly after circa 2000 B.P., but the immediate coast has never been covered by trees. The maximum northward extent of treeline along the north Labrador coast, spruce and larch trees in protected bays, occurred circa 4500 B.P. with trees in fjords just north of Okak Bay. Since 4500 B.P. there has been a slight retreat, and the northernmost substantial tree cover in protected coastal bays now occurs in Okak Bay (Fitzhugh and Lamb 1981).

Sea core data (Fillon 1976) indicate that during the early and mid-Holocene the cold Labrador current may have been diverted eastward at Hamilton Bank (east of Hamilton Inlet). This diversion removed much of the cold water influence from the Gulf of St. Lawrence and allowed warm water to penetrate from the south. This warmer water influence would have delayed sea ice formation in the fall along southern Labrador and in the Gulf of St. Lawrence compared with today, and caused earlier ice breakup. Fillon (1976) postulates that this warming effect was in full force until circa 6000 B.P., cooling as the Labrador current moved closer inshore until near-modern conditions were reached circa 3500 B.P.

The earliest recognizable Maritime Archaic culture is found on the north shore of the Gulf of St. Lawrence (southern Labrador) circa 9000 B.P. (McGhee and Tuck 1975). This coastline, which has never been heavily forested, had been deglaciated a millennium or so earlier. Maritime Archaic occupation of the central Labrador coast around Hamilton Inlet began circa 7500 B.P., approximately 1500 years before the local arrival of trees inland from the strip of coastal tundra (Fitzhugh and Lamb 1981). On the north Labrador coast, shrub, birch, and willow invaded the coastal tundra circa 6500 B.P., coincident with the arrival of Maritime Archaic inhabitants. Thus, Maritime Archaic occupation along the coast was apparently independent of tree cover, but was dependent (for fuel) upon shrub willow and birch. There is very little evidence of Maritime Archaic use of interior Labrador. For example, there is only a light occupation of Indian House Lake circa 6000-4000 B.P. (Samson 1983).

PRIOR MODELS OF MARITIME ARCHAIC SUBSISTENCE AND SETTLEMENT

Based on several seasons of survey and excavation around Hamilton Inlet, Labrador, as well as reliance on ethnographic analogy, Fitzhugh (1972:158-161) proposed four possible subsistence settlement systems for the area:

(1) The "Interior System" consisting primarily of winter interior caribou hunting and summer fishing at the mouths of rivers that empty into Hamilton Inlet.

(2) The "Modified Interior" system consisting of winter interior caribou hunting and summer adaptation to the coast. Coastal exploitation is characterized by "use of a rich environment without specialized techniques for utilizing its full range of resources." In other words, seal hunting might be attempted, but without any specialized equipment.

(3) The "Interior-Maritime" system includes an interior winter adaptation and coastal adaptation, but the latter is dominant in terms of time expenditure and resources procured. There is a clear preference for coastal habitation with greater possible population congregation size supported by a well adapted seasonal maritime harvesting adaptation.

(4) The "Modified Maritime" system is characterized by a coastal settlement pattern and year-round adaptation to marine fauna. Winter ice hunting techniques were used. Caribou hunting is important for clothing and other raw materials, but this hunting is done in the near-coastal zone and not in the interior.

The Maritime Archaic tradition of Labrador was initially assigned an Interior-Maritime subsistence system. The Rattlers Bight adaptation, in particular (Fitzhugh 1972:160), represents the "[c]ulmination of Indian marine specialization on the Northeast Coast" (Fitzhugh 1972:165).

An interior based, caribou hunting component of the Maritime Archaic tradition settlement system was originally postulated on the bases of negative evidence for substantial coastal (winter) houses and ethnographic analogy. The summer settlement pattern on the coast was clearly characterized by large and socially complex gatherings. Fitzhugh stated that a year-round coastal adaptation could have been achieved easily with the development of ice-hunting technology, but that "there is no evidence that this occurred" (Fitzhugh 1972:160-161). Thus, the suggestion that the northern Maritime Archaic tradition adaptation included an interior caribou hunting component seems to have been based on interpretation of the culture as "Indian" and the lack of positive evidence for year-round coastal residence. At about the same time, Tuck emphasized the maritime hunting and fishing capabilities of related peoples from Labrador, Newfoundland and Maine. He postulated a seasonal coastal residence for Maritime Archaic tradition populations on the west coast of

Newfoundland, specifically from late winter (February) through the following fall. An emphasis on coastal residence was implied (Tuck 1975:262).

After the discovery of a Middle Archaic period (7500 B.P.) burial mound at L'Anse Amour along with other early Maritime Archaic sites in Labrador, a developmental subsistence-settlement pattern model for the Maritime Archaic tradition in the Strait of Belle Isle was presented (McGhee and Tuck 1975:118-129). Late Paleoindian/Early Maritime Archaic groups had moved northeastward along the north shore of the St. Lawrence River maintaining a caribou hunting adaptation, perhaps seasonally supplemented with coastal sea mammal hunting and fishing. When they reached the Strait of Belle Isle area, they encountered spring whelping harp seal populations which could be harvested with comparatively unsophisticated technology (e.g., clubs). Scant evidence from the L'Anse Amour burial mound suggests that "rather early in the local sequence, these people had extended their maritime adaptation to ice-edge or open-water hunting of marine mammals as large and dangerous as the walrus" (McGhee and Tuck 1975:119-120). The seasonal nature of local resources and the apparent absence of substantial (permanent) structures was used to argue against year-round coastal habitation. McGhee and Tuck suggested that Maritime Archaic tradition populations spent the winter in the interior where they maintained an "older, caribou hunting" seasonal adaptation.

After preliminary results of the Nain area survey in 1975 became available, Fitzhugh (1976) remarked that Maritime Archaic tradition sites were abundant on the outer islands east of Nain and that there was a concentration of 20 Maritime Archaic sites in one square mile on an inner island. The outer island sites were larger than other known sites of the period. An intensive, probably seasonal, coastal adaptation was postulated on the basis of this evidence.

In Okak Bay Cox (1977) identified a more detailed Maritime Archaic tradition subsistence-settlement pattern based on site location and the content of lithic assemblages. Unfortunately, no faunal remains were recovered from these sites (Spiess 1977). The Okak Maritime Archaic sites were divided into three types: (1) exploitation camps covering a large area, but with low tool density; (2) base camps covering a large area and with greater tool density; and (3) activity stations, which are small sites with low tool density. Exploitation camps were found back in the bay, as well as on the eastern margins of the inner island zone near the *sina*, or the edge of the landfast winter ice. Base camps were found in the inner island zone. Inner island sites could have been used for hunting spring and fall harp seal migrations just before or after ice breakup or formation. A speculative reconstruction of the seasonal round (Cox 1977:305-307) was based on assumed caribou migration patterns and timing similar to those known today, postulating that people moved into the Labrador interior in December. Although not precluding a year-round coastal presence, Cox (1977:304) reconstructed coastal occupation during summer, fall, and spring, stating that the Maritime Archaic tradition exhibits "no specialized technology for spending winter on an arctic coast." He concluded that the Maritime Archaic tradition in Okak Bay exhibited an Interior-Maritime adaptation type, with a strong but not completely coastal adaptation.

In the Hopedale-Davis Inlet area the largest Maritime Archaic sites are located on islands and at the mouths of bays (Fitzhugh 1978); both settings would have been good areas for open water sea mammal hunting. Around Nain, the inner bays and fjords were lightly used, with the largest sites being present on outer islands. At Okak the compression of east-west ecological zone and deep water channels permits marine mammals to live closer to the inner islands. This geographic difference accounts for the different foci of Maritime Archaic tradition settlements in the Okak Bay and the Nain areas.

Maritime Archaic tradition settlement extended up the Torngat coast north of Okak to include

Hebron, Saglek and Ramah Bay. For earlier north coast remains of the Naksak complex, ca. 6000 B.P., Fitzhugh postulated a seasonal cycle including spring sea mammal hunts, summer travels and dispersals including the transport of Ramah chert, September caribou hunts near the coast, and a fall harp seal hunt. Winter settlement patterns and site locations are unknown. For Rattler's Bight phase settlements and those of similar age on the north coast (i.e., the Gull Arm group), an emphasis on fall harp seal hunting from large coastal camps was postulated. Evidence from the deep interior region around Indian House Lake allows the suggestion that those areas were used only sporadically, possibly only if near-coastal caribou hunts were unsuccessful. Fitzhugh (1978:79) conceded that there was no separate interior group with a wholly interior seasonal cycle during the time of the Maritime Archaic tradition.

Later, recognition of longhouse structures at Aillik and Nulliak began to revolutionize the conception of Maritime Archaic social structure and its evolution (Fitzhugh 1980, 1981, 1985). Rattler's Bight phase (Late Maritime Archaic) settlement patterns, including the northern limit at Nulliak, "are substantially different from those of earlier periods: large base camps are occupied throughout much of the year (probably not during winter) and contain one or more longhouses" (Fitzhugh 1981:26). Some of these segmented longhouses are 90-100 meters long and may have housed as many as 100 persons. Rattler's Bight phase sites are not spread evenly along the coast; "rather, a given settlement region tends to have a single large site located in a key outer coast resource zone. These sites can best be seen as semi-permanent base camps occupied by one or more longhouse groups 6-8 months each year, and occupying the inner coast or near interior during the winter months" (Fitzhugh 1981:28). The presence of these large, nucleated settlements and the absence of smaller stations scattered about the coast differentiates the Rattler's Bight phase from earlier Maritime Archaic tradition and later Indian and Eskimo settlement patterns. Settlement nucleation was accompanied by increased complexity in exchange systems and in mortuary sites.

Thus, early interpretations of Maritime Archaic tradition subsistence relied heavily on ethnographic analogy and historic period distributions of caribou herds. Excavations over the past twenty years (Fitzhugh 1976, 1978; Cox 1977) have failed to document a systematic interior hunting pattern for the Maritime Archaic tradition, however. Instead, additional settlement pattern evidence has been accumulated to document maritime adaptations. To the degree that it is appropriate to correlate a specific subsistence system with a broad cultural tradition, it is now apparent that a separate interior caribou-hunting seasonal adaptation played little or no part in the Maritime Archaic tradition and that the principal focus of this tradition was likely always coastal marine mammal hunting.

The discussion to this point has focussed on the use of settlement pattern data and ethnographic analogy. Direct evidence from faunal remains and early maritime technologies supports this model of Maritime subsistence. Data for hypothesis testing of these ideas originate in both mortuary and habitation contexts, to which we will return after a review of prey species biology.

SUMMARY BIOLOGY OF MAJOR MAMMAL PREY SPECIES

In this section I present baseline biological data such as body size and seasonal habits that are necessary to understand the part played by a prey species in a Labrador-Newfoundland hunting adaptation. The possible effects of paleoenvironmental differences between modern conditions and the Maritime Archaic period on these species are also discussed. The order of discussion proceeds through four species of seals to walrus, large whales, and caribou.

Harbor Seal (*Phoca vitulina*) and Grey Seal (*Halichoerus gryphus*)

Harbor seals are small pinnipeds, males and females averaging 87 kg and 65 kg respectively. Their diet is composed principally of fish (Bigg 1981). This species is sparsely and locally distributed throughout its arctic range, from Baffin Island southward (Davis *et al.* 1980:48). It inhabits coastal waters to a maximum of ten miles offshore, concentrating in bays, harbors, and river mouths during open-water season. They overwinter at locations where currents keep water open. In the Hamilton Inlet area (Ames 1977b:286, 287) harbor seals are found both in fresh and salt water areas. There are no studies of population size, reproductive biology, age-sex structure of populations, or population dynamics of harbor seals in the Canadian arctic (Davis *et al.* 1980:48). Harvest information and sustainable yield information is also missing (*ibid.*:49). Their localized populations are generally small and subject to extermination with heavy localized hunting pressure.

Grey seals are sexually dimorphic, with males weighing 230 kg and females 155 kg (Bonner 1981). They feed primarily on bottom fish, including cod, pollack, flounder, and to a lesser degree on cephalopods, pelagic crustacea, and schooling fish such as herring and salmon. Grey seals are distributed from the Gulf of Maine northward to approximately Okak Bay in Labrador. They are gregarious seals, which forage in groups and haul out to breed in closely packed colonies. They tend to prefer the same places as harbor seals and will displace harbor seals with agressive behavior (Banfield 1974:368). During summer in Labrador they are distributed in widely separated, small geographic areas, usually around inner islands or back in bays (Brice-Bennett 1977: Map 27), as far north as southern Okak Bay. Whether they move out to the *sina* and broken ice or move southward during winter is unknown.

Both harbor seals and grey seals are more common today in the Gulf of St. Lawrence and Gulf of Maine than they are in central and northern Labrador. Neither species can tolerate solid winter landfast ice cover. However, we postulate warmer climatic conditions and less extensive ice cover than today during Early and Middle Maritime Archaic occupation of the central Labrador coast and the Gulf of St. Lawrence. Small populations of harbor and/or grey seals might have been localized, summer resources for some Maritime Archaic populations.

Harp Seal (*Phoca groenlandica*)

The average recorded weight for harp seals is 120-135 kg (Ronald and Healey 1981). Thus, they are perhaps 30% larger than harbor seals. Harp seals feed opportunistically at several trophic levels. At their summer feeding grounds polar cod (*Boreogadus saida*) forms the bulk of their food. Farther south (Labrador) and on the breeding grounds, they feed on capelin, other pelagic fish, and crustaceans. Young of the year apparently continue to feed on crustaceans during the summer (Ronald and Dougan 1982).

Harp seals are highly migratory, with local availability corresponding with the seasonal movement of the population. The breeding season (midwinter to early spring) occurs on the sea ice on "The Front" off Newfoundland and on sea ice in the Gulf of St. Lawrence. Adults begin to move north in April. The young of the year follow separately about a month later (Davis *et al.* 1980:50). The population moves up the coast of Labrador quickly, reaching south and southwest Greenland in May. Thus the largest concentration of adults is available along the Labrador coast for a short period of time in April and May before the shore ice begins to break up, followed by stragglers and juveniles during the early weeks of ice breakup in May and June. The bulk of the population summers along the Greenland coast and in the Canadian high arctic, which some individuals reach in July and August. They begin to leave the Canadian high arctic in September, although a few are known to remain in Lancaster Sound into October. Some remain in Cumberland Sound, Baffin Island, until mid-January, and a considerable number remain in broken ice along the west coast of Greenland until February. However, most adults migrate south, close to the Labrador coast in October through December. These southward migratory seals move in among islands and into bays on the Labrador coast (Brice-Bennett 1977).

Considerable study has been focused on harp seal population size and sustainable yield because of the commercial nature of the fishery on the ice in late winter off Newfoundland. The population is, however, hunted through its whole migratory cycle today. Following a period of slack harvest during World War II, the northwest Atlantic harp seal population was estimated at 3.3 million individuals, producing 645,000 pups annually (Davis *et al.* 1980). That estimate is now thought to have been about 20% low, making peak modern population about 4 million animals. Circa 1980 estimates are 1 to 1.5 million animals. Harp seal reproductive success appears to be inversely dependent upon population, with the age at physiological maturity in females rising with increased population concentration (Davis *et al.* 1980:52). There does not appear to be any density-dependent change in migratory habit, meaning that the behavior today may be essentially similar to that of the whole postglacial period. The sustainable yield of several hundred thousand harp seals is certainly more than the precontact aboriginal population of the eastern arctic could have harvested. In fact, today's harvest by hunters in the Canadian arctic and Greenland, exclusive of the commercial hunt off Newfoundland, may approach only 10,000 animals (Davis *et al.* 1980:54-55).

The buoyancy of harp seals is very low when they first appear in the arctic during the spring; in fact, at least two-thirds of a sample of seals shot in Cumberland Sound at ice breakup sank before retrieval. Sinking losses decline into the fall as the seals' fat layers grow. The spring hunt is much less numerically important than the fall hunt because of the tendency for the spring seals to sink, although this differential would have been minimized before white contact when all seal hunting was done with a harpoon. Along the central and northern Labrador coast, harp seals are a seasonality marker: they are present only for a short period in spring (late April through early June) and then again for several months during the fall. The appearance of harp seals in the fall might be several weeks to a month earlier at the northern tip of Labrador (late September-early October) than at Hamilton Inlet (October).

The seals reach the breeding grounds of the Gulf of St. Lawrence and The Front off Newfoundland between late November and January. By early March females begin to haul out on the ice before whelping. Deliveries often take less than one minute, suckling commences within two hours, and neonate weight increases by 1.9 kg per day (Ronald and Dougan 1982:929). Females eat little or nothing during lactation. Pups are weaned in their second week, then begin to molt. They may begin to feed in the water on pelagic crustaceans as early as four weeks of age. Breeding begins, usually in the water, in late March as pups are weaned. As soon as mating ends, the adults move to more northerly ice for the annual molt (about the end of March, early April). Adult molting, for about two weeks, is again a time of fasting. Throughout the period of whelping, molting and mating, the portions of the St. Lawrence seal herd closest to shore are, and would have been, available to hunters walking out on the ice from the west coast of Newfoundland or south coast of Labrador.

Ringed Seal (*Phoca hispida*)

The ringed seal is the smallest of the Phocidae in the arctic, averaging 1.5 meters in length, 65 to 70 kg in adult males, with females slightly smaller

(Davis *et al.* 1980:66). Ringed seal blubber content varies from a high of 40% of body weight in late autumn to a low of around 23% during the spring fast (May and June). Ringed seals feed upon small crustacea and small fish. In deep water they feed on pelagic crustacea and in shallow water on crabs, benthic crustaceans (prawns), and benthic fish such as polar cod. They are reported to fast between April and late June or July, coinciding with the reproductive and molting seasons.

The ringed seal is the most widespread arctic marine mammal, occurring in all waters from middle subarctic latitudes northward. The distribution of ringed seals is highly dependent on the availability of landfast ice, or in the polar basin, on heavy pack ice. Adults maintain breathing holes in these stable ice conditions through the winter. Juveniles and young tend to be found in the water at the edge of the landfast ice and in broken pack or leads (Davis *et al.* 1980). These animals appear to be sedentary, undergoing only local movements in response to changing ice conditions (Davis *et al.* 1980:59). Smith (1973), however, documented movements of juvenile seals from an area of relative overpopulation due to light hunting pressure to an area of relative underpopulation due to heavy hunting pressure. Thus, it appears that juvenile seals are more likely to move modest distances (50-200 km), while the adults remain resident in familiar territory.

Because severely wounded or dead animals with low fat content will tend to sink, one factor of ringed seal availability to human hunters is the annual fat cycle. Ringed seal blubber content is at its lowest during the annual mating and molting period in May and June. Melting freshwater in June and July also tends to decrease surface water density, increasing the tendency of a dead ringed seal to sink. Seals in the water are thus least likely to sink in fall, winter or early spring. During the period of stable landfast ice conditions (December through May in central Labrador), adult ringed seals are accessible with special technology at their breathing holes, a technique which has been well described by many ethnographers of Inuit. During May and June, adults and juveniles alike haul out onto the spring ice to bask in the sun. They can be approached by careful stalking. Mothers with foetal/newborn pups can be found during spring (late March, April and May) in dens built in the snowcover above their breathing holes. These latter two techniques do not require extremely specialized technology.

Seasonality estimates based on age demographies of the kill (Smith 1973) can be quite useful in detecting hunting patterns using archaeological assemblages of ringed seals. Assemblages dominated by juveniles and adolescents, no matter what season of death, should come from open-water kills, either in leads or along the *sina* during cold seasons, or during ice-free conditions during the summer. Assemblages dominated by adult animals would represent harvesting of seals by some method at their breathing holes, with the seasonality determinable by relative width of the annulus. Assemblages dominated by female adult and foetal seals document April-May hunting on the ice, most often locating and taking mother seals and their infants in or near dens.

With warmer climatic conditions during the past along the Labrador coast, especially during Maritime Archaic occupancy, we would expect smaller areas of stable landfast ice and shorter seasons of landfast ice persistence. These environmental conditions would decrease the overall ringed seal population compared with modern conditions, thus decreasing the opportunity for ringed seal hunting at breathing holes (given the available technology) or basking on the spring ice.

Walrus (*Odobenus rosmarus*)

Atlantic walrus are large: males average 900 kg, females 570 kg (Davis *et al.* 1980:47). Walrus are primarily feeders on bivalve molluscs: clams and mussels of several genera (Davis *et al.* 1980:15), although at least 60 genera of 10 phyla, including coelenterata, annelida, bivalve and univalve molluscs, and cephalopods, crustaceans, and others (Fay 1982:145-157) have been found in their stomachs. Fish form a very small part of the diet, while a few "rogue" walruses attack and feed on other pinnepeds (primarily small seals) and some small whales (Davis *et al.* 1980:15; Fay 1982:156-157). Walrus feed primarily by sucking the soft parts of bivalve molluscs out from between their shells, using nearly prehensile mouth parts and considerable suction (Fay 1982). Full walrus stomachs have been reported to weigh 25-43 kg (Fay 1982:157) and are considered a delicacy by Inuit hunters. Walrus have not been observed feeding on benthic organisms in depths greater than 80 meters (Fay 1982:161-163), so shallow water is an essential determinant of walrus distribution. Especially when migrating, walrus can be found in deeper water, although their food is unknown in this situation (Fay 1982).

During the late 18th through 20th centuries, walrus have essentially existed only on the central and north Labrador coasts. Bulls come no farther south during winter along the Labrador coast than Okak Bay, while the females and young reached southern Labrador (Freuchen 1935 in Reeves 1978). Hebron was an important wintering area. By 1959 Hebron was the southward limit for all walrus (Loughrey in Reeves 1978:19). There appears to be substantial east-west migration in Hudson Strait. The eastward movement in October apparently splits, with some of the population moving south along the north Labrador coast (Davis *et al.* 1980:41). Walrus winter along the *sina* on the north Labrador and Baffin Island coasts and along the edge of the pack ice in Davis Strait.

Historic accounts primarily mention walrus along the *sina* in February and March off the north Labrador coast (Kaplan 1983). Brice-Bennett (1977:173, 179, 185, 189) reports that walrus were occasionally seen on drift ice in February along the ice edge north of Hopedale; along the *sina* east of Nain into late spring; along the *sina* and after breakup around outer islands off Okak Bay; and along the *sina* and around outer islands during open water off northern Labrador fjords. Walrus may have been present year round in the Button Islands (Port Burwell region).

In summary, during the historic period, walrus were commonly hunted along the *sina* during late winter in shoal areas in the mouths of major bays and fjords from Hopedale northward. Farther north, they remained available into the spring in some areas. In northernmost Labrador they may have been available nearly year-round, with a population peak in October.

Walrus were plentiful in the Gulf of St. Lawrence and at Sable Island off Nova Scotia before substantial exploitation in the 16th century (Reeves 1978:16-19). A single ship's crew killed 1,500 walrus at Sable Island in 1591 (Allen 1880, cited in Reeves 1978). By the mid-19th century walrus had been effectively exterminated south of Labrador. The prehistoric seasonal presence and movement patterns of walrus around central and southern Labrador thus cannot be estimated from historic sources. If a portion of the Gulf of St. Lawrence walrus population was migratory northward during the summer, then walrus might have been plentiful along the southern and central Labrador coast during the open water season. Reconstructing walrus seasonal presence during Maritime Archaic occupation of the Labrador coast would seem impossible based on modern biological data, but the summer presence of walrus on the central and southern Labrador coasts in Archaic times is suspected.

Reports of extensive walrus populations in the Gulf of St. Lawrence in the 16th and 17th centuries mean that many individuals of this population might have dispersed northward during the summer. The southern margin of the winter distribution of the main Hudson Strait-Foxe Basin-Northern Labrador population (roughly Hopedale during the late 18th century) might have shifted northward slightly. Thus, for Maritime Archaic occupants of the coast between southern Labrador and Nain or Okak, walrus may have been most frequent during the summer, rather than during the late winter/early spring as we would expect based strictly on the 18th/19th century reports. Alternatively, walrus might have been present multi-seasonally, at least in small numbers, especially between Hamilton Inlet and Okak.

Caribou (*Rangifer tarandus*)

Spiess (1979:19-139) has reviewed various aspects of caribou biology of use to archaeological interpretation of faunal assemblages. Three points from that review need re-emphasis here. (1) The oft-used dichotomy between "barren-ground" and "woodland" caribou as a basis for reconstructing a wide range of past behavior and biology is a falsehood. While it is true that the behavior and biology of some woodland and some barren-ground caribou groups contrast in many attributes, there is in fact a continuum between barren-ground and woodland caribou. To ignore this continuum of behavior is to invite spurious reconstructions of the past, with Labrador being one of the prime areas for such possible problems. (2) Human hunters cannot hope to "follow" long distance migrating caribou herds. Humans must rely upon interception of bands of caribou on migrations of variable seasonal and geographical predictability. (3) Caribou population levels change dramatically over a cycle of approximately 100 years' duration. Their migration patterns can change drastically as range quality and population levels change. Even more radical departures from current patterns of movement and population size occurred during the course of major post-glacial phytogeographic change in the study area, a point which bears directly on Maritime Archaic adaptation.

Medium-size males from Labrador weigh between 110 and 130 kg, while medium-size females weigh 70 to 85 kg. At the height of fat content in males (fall, pre-rut) approximately 20% of body weight is fat and approximately 55% is edible meat (Spiess 1979:28). Behaviorally, caribou are browsers. Even when feeding on preferred plant food, caribou are delicate feeders, picking their food with their mouths rather than tearing at it as do true grazers. Lichens are not caribou preferred food. They prefer willow and birch shoots, growing forbs, grass and sedge shoots, and fungi, but will harvest the plant with the highest digestible protein content available at a given season. Their ability to digest lichen, which other cervidae apparently do not have, gives them an adaptive advantage in certain biomes because lichen does retain a higher nutrient content (low nonetheless) than dormant vascular plants during the arctic winter.

In fact, all caribou that inhabit the Quebec-Labrador Peninsula are skeletally and (otherwise) morphologically classifiable as *Rangifer tarandus caribou*, the North American continental subspecies which includes all woodland populations, and which are differentiable from *R.t. groenlandicus* and *R.t. granti*, the central Canadian and Alaskan barren-ground subspecies respectively (Banfield 1961). Northern Quebec-Labrador is the home to two herds of long-distance migratory caribou, as defined by separate calving grounds: the George River and Leaf River herds (Figure 7.2). Their behavior parallels that of *R.t. granti* and *R.t. groenlandicus*, the barren-ground migratory herds west of Hudson Bay and in Alaska. These two Labrador-Ungava herds migrate up to 800 km from spruce woodland (taiga) out onto tundra and back. Since Labrador has lost its remnant continental icecap within the last 6,000 years, these migration patterns must have developed from more localized migratory behavioral patterns within that span of time. In Labrador too, there are (were) many smaller herds (500 to 2,000 individuals) that migrate over less distance (50 to 150 km), spend more of their time in the taiga, and exhibit more attributes of the paradigmatic "woodland" caribou.

Perhaps the most important of these smaller herds was the Mealy Mountains herd south of Hamilton Inlet, but there are (were) other herds northwest and southwest of Hamilton Inlet (Spiess 1979:48). At least one of these populations was "augmented occasionally" by animals from the George River herd moving south across Kaipokok

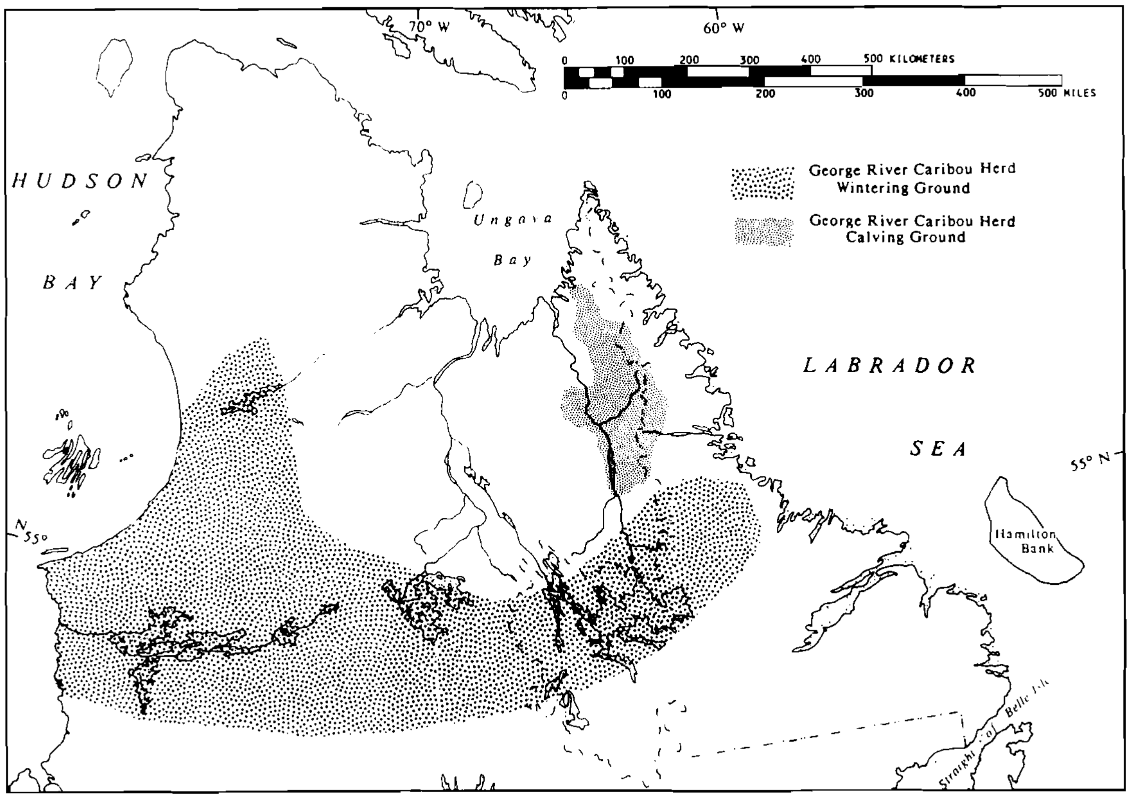


Figure 7.2. Extent of range of the modern George River caribou herd (redrawn after Couturier et al. 1990). Such an extensive range would not have been possible during Maritime Archaic occupation of Labrador.

Bay (Schwartz 1977:245). Apparently this group was a smaller satellite herd that might increase in size as population maximum in the George River herd was approached. The George River herd core range (Figure 7.2), however, has represented the center of habitation (Spiess 1979:62-63) for northern Labrador caribou, at least for the last few thousand years. A similar distribution of small, relatively localized migratory herds apparently characterized southern Labrador at times past.

Labrador caribou migration patterns were understood circa 1975 as follows (Spiess 1979:49-51; Stuart Luttich, pers. comm.). In early fall, caribou from the George River and Leaf River herds congregate in the Upper George River area near Indian House Lake. In late September and October they migrate to four separate wintering areas: the Koksoak River, the lower George River and Tunulic River, the Fraser River, and the Notakwanon River. There they winter until March or April. In April and May the females lead a movement to the two calving grounds that define the herds. Cows from the Fraser River wintering area move north and calve southwest of Hebron Fjord. Following calving, the females and newborns disperse and are joined by males and non-breeding females. Some of the population comes to the coast and fjord heads along the north Labrador coast. The herd is back in the George River/Indian House Lake area by early September. The early fall gathering around Indian House Lake is not related to the fall rut, but does correspond with an extensive sedge-willow-scrub plant cover area high in sedge and grass percentage. Sedges and grasses retain high protein content later in the fall than other plants, indicating that this fall gathering is probably caused by a unique phytogeographic factor. In addition to the above movements, occasional bands of wintering caribou from the Fraser River valley will wander out to the coast, especially around Nain.

The seasonal availability of caribou to Labrador Inuit and Innu (Indian) before the advent of motorized transport was related primarily to the geographic location of settlements relative to the seasonal movements of the George River and Leaf River herds. People from the Port Burwell area (north tip of Labrador) hunted caribou between 50 and 250 km south of Port Burwell on both coasts of the Labrador-Quebec peninsula (Val 1976). The core caribou hunting area, called Senikijuak, along the Ungava Bay side of the height of land, was used both summer and winter. Farther south, inhabitants of Hebron and Saglek would presumably also have had this multi-seasonal access to caribou in the same areas. Okak Bay residents traditionally made an inland trip in late April to intercept northward moving pre-calving bands of cows. Hunters from Okak and Nain would travel inland during winter or early spring to find caribou as near to the coast as the Fraser River valley west of Nain or as far inland as the George River (Brice-Bennett 1977:155, Maps 46, 47). Hopedale hunters generally found wintering caribou southwest from Hopedale but north of the Kanairiktok River. Postville hunters would take caribou within 10-20 km of Kaipokok Bay from January through early April. Makkovik hunters hunted the locally ranging caribou herd found between the Benedict Mountains and Double Mer (Schwartz 1977), apparently in winter. Hunters from the Hamilton Inlet area harvested this Benedict Mountains herd both in summer and winter, and harvested the Mealy Mountain herd south of Hamilton Inlet in fall and winter (Ames 1977a: Map 107). In sum, northernmost Labrador residents had multi-seasonal access to caribou if they used the wintering areas along the Ungava Bay coast for hunting, while residents of the central Labrador coast had multi-seasonal access to locally migratory caribou herds, mostly taking advantage of the opportunity during the winter. Residents of the Hebron and Saglek fjords area would have had access to caribou during late spring and summer, while residents of Okak and Nain mostly had access to caribou during winter and early spring migrations.

Range condition, size studies, and population censuses (Harrington 1988; Couturier *et al.* 1990; Crete *et al.* 1990) of the north Labrador caribou herds since the late 1970s have provided a new, long-term, dynamic perspective. Population growth of the George River herd continued positive from circa 1958, when the population was estimated at 15,000 individuals, to 1984, when it was estimated at 472,000 ± 15%. Since 1984, herd growth has been negative. Forage quality on the calving grounds has deteriorated, and the area used as calving ground has increased. Lower percentages of females are using the traditional calving ground, and calf mortality may have increased. The herd is using a larger range as a whole, and density of animals has decreased. The Leaf River and George River caribou herds both calve in the northern Labrador-Quebec peninsula, in the northeastern and northwestern portion of the peninsula respectively. The winter range of these herds overlaps along the east shore of Hudson Bay. The maximum distance of travel from winter range to spring calving ground along the height of land east of the George River for the George River herd is 1000 km. The timing and pattern of migrations has changed recently, with females leaving wintering grounds for the calving grounds later (late May) and most animals leaving the tundra plateaus around Indian House Lake on the George River in late July or August rather than September or October. Traditional hunting patterns have been upset, including those of communities along the Labrador coast. The 1990 spring commercial caribou hunt from Nain failed because of late and fewer arrivals (personal communication, Gary Baikie to the author). The number of animals coming to the north Labrador coast during the summer, and accessible to central

Labrador coast residents at various seasons, has fallen dramatically.

Historic data from northern Labrador document a roughly 100-year cycle from population high to population high. Historic and archaeological data from Greenland (Meldgaard 1986), where smaller herds (n=50 to 10,000 individuals) are confined to peninsular areas by fjord topography and inland ice, document caribou herd population cycles of 65 to 115 years between population highs. The population maximum may last from 10 to 25 years, and the population minimum may last from 35 to 75 years (*ibid.*:59). A logical model of predator-herbivore interaction, termed the "predator-pit" model (Harrington 1988), explains this cyclical pattern as follows. Population crash is initiated by poor range conditions and/or bad weather (e.g., ice storms), often preceeded by range expansion into marginal environments and poor calving success. Predator population levels (primarily wolves, bears, and humans), decrease but not enough to release pressure on the herbivore population. Eventually some event, such as disease in the predator population or an in-migration of a substantial number of caribou from a contiguous herd, releases the pressure and the caribou herd begins exponential growth. The predator population does also, but its collective appetite cannot slow caribou growth until herd growth is impeded by some deterioration of its food supply.

These data from Labrador-Quebec and Greenland have the following implications for understanding Maritime Archaic use of caribou. First, the entire Quebec-Labrador peninsula north of 52° latitude is necessary to sustain the large George River and Leaf River caribou herds, at least at their population maxima. The presence of ice before 6000 B.P. in the center of the Labrador-Quebec peninsula (Clark and Fitzhugh 1990), and denser forest cover in southern and central Labrador-Quebec between 6000 and 2000 B.P., would have severely limited the maximum caribou population size. The total maximum caribou population of Labrador-Quebec would have been less than today, and the population would have been divided into more (smaller) herds (more calving grounds), with shorter annual migration routes. During the Early Maritime Archaic, circa 9000 to 6000 B.P., as the remnant ice wasted away in central Labrador, the Greenland model (Meldgaard 1986) of multiple smaller herds, confined to a relatively narrow coastal and near-coastal area, is attractive. Caribou migration patterns in Greenland tend to be perpendicular to the ice: toward the coast in winter and further inland and up in altitude (toward the ice sheet) in summer. How much this seasonal pattern dominated the Labrador situation is unclear, but the caribou population would certainly have been present as more, smaller, more locally migratory, more near-coastal herds, and therefore more accessible to a coastal-dwelling human population.

The fall caribou hunt at Indian House Lake, which characterized the historic-period Innu (Naskapi) adaptation, classic "interior" caribou-hunting, was dependent upon a large herd in northern Labrador coupled with caribou attraction to a large area of sedge on the Labrador plateau during the fall. Pollen cores (McAndrews 1976) show a sharp rise in sedge pollen near Indian House Lake circa 1200 B.P. Therefore, the large caribou herd and specific fall attraction to Indian House Lake that made the classic Innu interior caribou hunting adaptation possible postdates Maritime Archaic occupation of Labrador by nearly 3,000 years.

During the span of Maritime Archaic occupation, caribou herds would have been relatively small in size (500 to 10,000 animals) and localized in migration pattern (50 to 150 km). Often, they would have been accessible multi-seasonally from a habitation base on the coast with travel distances of only a day or two. In the largest caribou range region in Greenland today there are currently 3 herds numbering 600, 2300, and 3000 animals, with maximum migration distances (one-way) of 70 kilimeters. We use this "Greenland model" as a basis for reconstructing Maritime Archaic use of caribou. Each localized herd would have been in a "population expansion" phase roughly one-third of the time. The Maritime Archaic adaptation in Labrador and Newfoundland must have taken advantage of such occurrences. Perhaps the synergistic effect of near-coastal access to a local caribou population high, coupled with a marine resource base, helps explain seemingly localized and short-lived Late Maritime Archaic population concentrations.

Other Species

Three species of large whales were hunted by Inuit and Europeans along the Labrador coast and Gulf of St. Lawrence: the humpback (*Megaptera novaengliae*), the bowhead (*Balaena mysticetus*), and the right whale (*Eubalaena glacialis*). Adults of these species weigh 20,000 to 28,000 kg (Allen 1916), and 10% to 20% of that weight can be blubber. All three species are north-south migrants along the Labrador coast, although the timing and details of migratory pattern differ from species to species. Because the populations of these large whales have been drastically reduced by European hunting beginning in the 16th century (Stevens 1985; Cumbaa, personal communication 1985), we are reliant upon historic (e.g., Taylor 1974:25) and archaeological sources to some degree to reconstruct their seasonal presence. Today humpback whales move northward from the Caribbean, reaching the Grand Banks of Newfoundland in June and July, and continue northward (Whitehead *et al.* 1983, Winn and Winn 1985). Fitzhugh (1985:54) reports humpback whales in Hamilton Inlet in July, and Spiess has seen this species moving southward inshore near Nain in early September. During the 19th century, bowhead whale presence inshore along the Labrador coast (southward bound) was highly seasonal, during November and December. During the 18th century, the southern limit of bowhead whales was approximately Hopedale, but bones of this species are reported from 16th-century contexts in the Red Bay, Labrador area (Cumbaa, personal communication). Today the northern (summer and early fall) range of the right whale (*Eubalaena*) includes the Labrador coast, perhaps as far north as Hudson Strait (Reeves *et al.* 1978). During the slightly warmer sea conditions of the mid-Holocene, one or more of these three species of large whales would have been available inshore in the Gulf of St. Lawrence and along the length of the Labrador coast at almost any time of year.

Furbearers associated with forest cover (fisher, marten, black bear), and those associated with aquatic habitats with wooded banks (otter, muskrat, beaver) would have been commonly available in southern and central Labrador (Nain south) and Newfoundland in forested environments away from the exposed coastal tundra during the latter half of Maritime Archaic occupation, if not earlier. We expect most of the bird species present today to have been present during Maritime Archaic occupancy, except that those species with a southern (winter) limit in northern Labrador, such as the ivory gull, would have been absent.

With warmer climate, codfish and salmon may have been as common or more common than they were before recent over-fishing. The southern limit of char distribution would presumably have been farther north than it is today and thus not significant for Maritime Archaic occupation.

Summary: Expectations for Maritime Archaic Economy

These general resource availability patterns allow us to hypothesize a limited series of focal points for Maritime Archaic economy to test against the available archaeological data. Most importantly, we hypothesize the lack of a late fall caribou concentration in interior Labrador, and the presence of more, smaller, shorter-distance migratory herds of caribou more confined to Labrador coastal areas than at present. Because of this behavior pattern, caribou should have been available to coastal and near coastal areas of Labrador (within 50-100 km) nearly year round. Maritime Archiac economies, therefore, would have failed to develop a major seasonal interior component characterized by habitation of the Labrador plateau for several months. Caribou should have been available within several days' journey at all seasons to coastal dwellers on the Island of Newfoundland.

Given the lack of a major interior attraction in the form of caribou, Maritime Archaic people in Labrador would have been dependent upon a multi-seasonal coastal and near-coastal adaptation (which included some local caribou hunting). Several coastal seasonal foci of resource procurement can be hypothesized. Perhaps the most obvious would be a fall (October-November) extended harp seal hunting season around inner islands and bay mouths. Winter could have included a time of generalized sea mammal hunting at the *sina* (ice edge), or a focus on hunting wintering caribou and hunting/trapping other animals of the near-coastal forested river valleys: black bear, marten, fisher, mink, otter, beaver, muskrat. These alternatives seem mutually exclusive. Late winter/early spring possibly focussed on hunting seals basking on the ice and hunting walrus along the *sina* (ice edge). As the sea ice broke up and summer came, we postulate a generalized open water sea mammal hunting economy (seals, walrus, perhaps a whale occasionally), supplemented by bird hunting. There may have been a specialized late summer codfish and/or salmon fishery.

On the Island of Newfoundland the open water multi-species sea mammal hunting season would have extended slightly later in the fall. Harp seals would have been a focus of a late winter and spring hunt on sea ice.

That leaves the question of Maritime Archaic technological ability to take the largest sea mammals: the bowhead, right, and humpback whales. If these species were not a focus for specialized hunting activity using technology available for walrus hunting, it is still possible that they were occasionally attacked as part of general open-water sea-mammal hunting activity.

MARITIME ARCHAIC SETTLEMENT AND FAUNAL DATA

Substantive faunal data of Maritime Archaic age are derived from two contexts: burials and habitation contexts. The latter have produced mostly calcined bone fragments from hearths.

Each of these contexts presents its own special problems for interpretation of subsistence patterns. Burial assemblages are likely to contain curated items, items obtained through long-distance trade, or items with special meaning to the deceased or his relatives. Most faunal items included in Maritime Archaic graves appear to have been either modified as tools or used for personal adornment or in ritual activity. Burial faunal assemblages certainly do not reflect quantitatively the cultures' subsistence base, nor can they be used for seasonal reconstruction of settlement patterns.

Calcined bone, the preservation state of most of the habitation faunal material, is much different from "fresh" Eskimo midden bone. Bone is calcined by undergoing structural and chemical changes between 400° and 600° C (Shipman *et al.* 1984). Microscopic change in hydroxyapatite crystal size and hydration results in some shrinkage (up to 15% at 900° C), loss of tensile and compressional strength (Knight 1985), much better resistance to soil acid than fresh bone (Knight 1985), and color change to chalky white. Spiess *et al.* (1985) have documented that the same bone can experience different rates of shrinkage on different surfaces when exposed to a hardwood fire, up to 30% in one linear dimension. Calcined bone represents a sample "selected" by site occupants for preservation by discard into a fire. Subsequent soil and microbial action may remove non-calcined bone from the archaeological sample entirely. Human passage and soil mechanical forces break the calcined bone into smaller and smaller pieces as time passes, with differential rates of breakage between body parts in one species or between species or classes (Knight 1985). Spiess (n.d.) has documented that fishbone is much less resistant to crushing than mammal bone, but that it does survive in quantity in calcined bone assemblages of Early and Middle Archaic age around the Gulf of Maine. Identifiability of calcined bone pieces to the faunal analyst is another source of data transformation because the smaller bones of smaller species tend to yield more recognizable fragments when reduced to a common size (e.g., 1 cm average) than do the bones of a large species. Small bones of the largest species, such as carpus and manus bones, tend to be the ones that remain identifiable in a calcined assemblage. A calcined bone assemblage dominated by identified seal flipper and caribou hoof bones does not, therefore, mean that some strange dietary practice was occurring. Such assemblages are simply a product of our ability to recognize species and elements in the size range usually represented by calcined assemblages. Finally, calcination is particularly hard on teeth, consequently the vast majority of season-of-death information usually available in arctic faunal assemblages is destroyed. With these caveats in mind, we turn our attention to the Maritime Archaic faunal assemblages.

Burial Assemblages

The early Maritime Archaic L'Anse Amour burial crypt was located on the north shore of the Gulf of St. Lawrence (McGhee and Tuck 1975:87-92). It yielded faunal remains and tools from identifiable species, plus a human subadult (early teenage and of indeterminate sex) radiocarbon dated 7530 ± 140 B.P. (I-8099). In a charcoal layer deposited just above slab-form stones forming a burial cist were found "burned" (calcined?), unidentifiable (i.e., to species; body part also not described) fishbones about the "size of a cod or salmon." The human skeleton was found 30 cm underneath the stones. A walrus tusk lay in front of its face. A bone flute or whistle made from a longbone of a large bird and a few "small fragments of birdbone" were also recovered. Three socketed bone points were made of caribou ulnae, and a 162 cm long curved handle-like object with a central line-hole was made of walrus ivory (McGhee and Tuck 1975:88; reported as antler in photo caption p. 244, but probably ivory from the photograph). A toggling harpoon was made of unreported material. Although artifacts may be curated and thus not indicative of any particular seasonality, the presence of cod or salmon-sized fishbone is suggestive of open-water season fishing. Other artifacts indicate the hunting of large birds, walrus, and caribou in the cultural repertoire. The presence of a toggling harpoon is strong evidence of marine mammal hunting. Thus, the L'Anse Amour mound is an early Maritime Archiac assemblage demonstrating competence with marine mammal hunting, including species as large and dangerous as walrus, plus fishing and caribou hunting.

The late Maritime Archaic cemetery at Port au Choix, Newfoundland (Tuck 1976) has preserved a vast array of animal bones from burial contexts. The Port au Choix cemetery is not precisely dated, but must date between 4000 and 3500 B.P. on stylistic grounds (Late Maritime Archaic). Often some attribute of the bone grave inclusions indicated that they were attached to clothing or included in a pouch or bag, perhaps as personal charms or amulets. Such attributes include perforation or other working, the anatomical parts recovered (seal claw bone cores in anatomical group, or groups of 200 great auk mandibles), or simply the context and location within the grave. A wide variety of tools, often weapon parts, were made from caribou antler, caribou bone, whale rib or jaw fragments, walrus ivory (adzes, dagger), and bird bone (needles).

The whalebone tools must have been made from the bone of large whales. This identification is based solely on the size of trabeculae visible in the whalebone illustrated by Tuck (1976) and the author's experience with Labrador Eskimo whale bone identification. The whales involved were much larger than white whale and small Delphinidae. In my experience the frequency of whalebone use in tool manufacture at Port au Choix is matched only by the frequency of whalebone use in whale-hunting Thule and Thule-descendant Inuit material culture. On this basis alone we hypothesize that Late Maritime Archaic people had the technology to hunt and recover large whales systematically.

The mammal bone non-tool assemblage from the graves was dominated by seal claw cores (n=97 from 18 graves) and beaver bones and teeth. These seal claw cores are "indistinguishable" (Tuck 1976:61) from claw cores of the harp seal, *Phoca groenlandica*, which means they came from one of the two large species of genus *Phoca* (probably also harbor seal, *P. vitulina*.). Tuck makes a persuasive case on the basis of species frequency that they are mostly or all harp seal. Other seal grave inclusions are limited to teeth: a postcanine and a "small canine from an immature seal" (Tuck 1976:61). The latter is likely to be from a foetal/newborn individual, since the permanent dentition erupts at about the time of birth in genus *Phoca*. These data suggest the Maritime Archaic harvesting of the whelping harp seal population on the ice off Newfoundland in March.

The beaver assemblage from the graves is composed of 118 unworked beaver incisors, two beaver forepaw skeletons, and a maxilla and mandible from one individual. Beaver bones were found in 20 different burials. Beaver could have been hunted at any season of the year, in fresh water near or far from the coast. Other furbearers' remains in the Port au Choix graves include red fox (5 maxillae, 10 mandibles), otter canines, pine marten (6 mandibles and one maxilla from 6 burials), and a wolf. The canids could have been trapped or shot at any time of the year, including on the sea ice during winter. The pine marten and otter, however, indicate trapping activity in a heavily wooded environment, perhaps coincidentally with beaver trapping.

Bear bones are present in the grave assemblages. Tuck (1976) cites the identification of polar bear incisors and black bear canines. Presumably these identifications were made on the basis of size (large = polar bear, small = black bear), and the assumption that grizzly bear was rare or absent (see Spiess 1976; Spiess and Cox 1976). Caribou are represented in five graves (rib, dew claw phalanges, perforated styliform or metapodial bones, two phalanges, and five sets of incisors) in addition to the common occurrence of caribou bone as utilitarian artifacts. Other mammal remains in the graves include two adult male dog skeletons from Burial 50 and a whale's tooth, probably a killer whale judging by the size and shape.

Birds are represented in the graves by pieces of over 278 individuals from 30 species. The great auk comprises 77% of the individual birds, with over 200 maxillae from one grave. In addition to the frequent occurrence of bird bone needles, swan and eagle ulnae have been modified into tools, some for flutes or whistles. Other genera/species represented include: common loon, red-throated loon, greater shearwater, gannet, cormorant, swan, canada goose, teal, harlequin duck, eider duck, merganser, bald eagle, marsh hawk, falcon, ptarmigan, whimbrel or curlew, godwit, gull, tern, great auk, common murre, dovekie, black guillemot, puffin, and snowy owl.

Fish are represented in the Port aux Choix graves only by 32 shark teeth in one grave and five codfish otoliths (ear ossicles) from five graves. There were no codfish (or any other bony fish) vertebrae or skull parts other than ear ossicles from the burials. The grave evidence, therefore, does not support a hypothesis of heavy dietary reliance on fish. Shellfish (softshell clam, sea scallop, and whelk) are present in the graves, although the shells have been reworked into gorgets or other items of adornment. There is no evidence, such as a nearby shell midden, that shellfish were an important dietary contributor.

In sum, the Port aux Choix grave inclusions provide strong evidence of a competent maritime adaptation, possibly including the hunting of whales and definitely including the hunting of walrus. This maritime hunting adaptation included at least some level of competence with ice hunting, focusing on harp seals, and may have included a very high level of competence based on the presence of toggling harpoons. Supplementing the maritime hunting was a strong forest and freshwater trapping/hunting competence and some level of reliance on caribou hunting. In Fitzhugh's (1972) terms, these data seem to indicate a modified maritime adaptation, but one with the capability to handle the hunting of large whales.

Two grave sites from the central and north coast of Labrador have yielded non-human bone, but the samples are much smaller. One or more graves in the Rattler's Bight cemetery, Hamilton Inlet, contained a walrus skull and a piece of walrus tusk (Fitzhugh 1976:125). Nulliak Cove, Mound 2, on the north coast of Labrador, has yielded a walrus tusk from another grave (Fitzhugh 1981).

Habitation Assemblages

The Fowler site on the Strait of Belle Isle yielded a sample of approximately 2000 calcined bone fragments from a hearth dated circa 6300 to 6800 B.P. (McGhee and Tuck 1975:45-49). Identifications by Howard Savage, who later worked on the Rattler's Bight sample presented below, include seal (genus *Phoca*, species indeterminate, n=18), caribou (n=2), and birds (n=9, including dovekie, unidentified duck, surf scoter, razorbill auk, and gull of indeterminate species).

Site EiBg-7, located on a 22m elevated sand beach west of Rivière Blanc Sablon on the northern shore of the Gulf of St. Lawrence, is an Early Maritime Archaic occupation dated circa 7000 B.P. on charcoal (Groison 1985:127-128). Associated projectile points are the small triangular style found at the Pinware and Cowpath sites (McGhee and Tuck 1985), where they are thought to date earlier than 7000 B.P. Faunal remains from the site include unidentified calcined bone as well as uncalcined walrus rib and tibia fragments (poorly preserved). Whether dating to circa 7000 or earlier, this site again records the hunting of walrus by Early Maritime Archaic people.

Calcined faunal samples have been recovered and analyzed from two Labrador Maritime Archaic sites, both located around Groswater Bay, Hamilton Inlet: Rattler's Bight 1 and Hound Pond 2. Both are Late Maritime Archaic sites dating circa 3800 B.P. Three faunal analysts have worked with various samples from these sites: Dr. Howard Savage and R. Robin Dods of the University of Toronto, and this author. Savage prepared three separate identification reports in manuscript form (Savage 1970, 1972a, 1972b, see Fitzhugh 1972: Appendix 2), as well as a summary discussion concerning the seasonality of occupation based on his and Dods' work (Savage 1978). Dods prepared a single manuscript report on her work (Dods 1977). In 1986 Spiess reviewed these reports and, with Savage's permission, reanalyzed all available specimens. During this process, several identifications (primarily of seal species) were changed, and faunal material was identified from 20 squares at Rattler's Bight that had never been identified previously. It is axiomatic that identification of the same faunal assemblage by two or more archaeozoologists will result in slight differences of opinion, due primarily to the biases, interest, and experience of the investigators. The potential for disagreement is even greater with calcined assemblages because the difficulties of identification are extreme due to fragmentation, distortion, and surficial erosion of the surviving bone.

The Rattler's Bight 1 sample is characterized by small size (1.14 cm average maximum dimension on each fragment, 0.11 grams average weight) (Dods 1977), and low rates of identifiability (about 2043 specimens identified to class [as bird, mammal] or to genus/species out of a total of 26,297 fragments in the Savage and Dods samples). The simple recognition of diagnostic shape on a bone fragment depends crucially upon investigator experience and bias in these circumstances. Dods had produced a much higher proportion of identifications of seal specimens to species than did either Savage or Spiess, and her report was the source of *Phoca* *vitulina* identifications subsequently questioned by this author on comparative grounds. Inspection of her primary data reveal identifications to seal species based on fragmentary phalanges and metapodials, often with epiphyses missing or unfused. My experience with seal bone identifications, including obtaining extensive measurement series from genus *Phoca* and reconstructing epiphyseal closure sequences, forces me to be much more conservative in identifying fragmentary genus *Phoca* bones to species. Only certain bones (teeth, jaw portions, auditory bulla portions) can separate all three of the common eastern arctic *Phoca* (the ringed, harp, and harbor seals) in fragmentary collections, and no species identifications can be made on fragmentary immature specimens. Thus, in the summaries below, I have taken the conservative approach and reduced all of Dods' specific seal identifications to the generic level where I feel that specific identification was unwarranted. Spiess' analysis of the previously unidentified Rattler's Bight faunal samples did add one new species to the identified list: walrus (*Odobenus rosmarus*). These identifications were facilitated by recent experience with a much better preserved (less fragmented and eroded) but calcined faunal collection from Point Revenge sites near Postville, that were full of walrus bone. Being from a very large animal, walrus bones tend to lose their identifiability when reduced to fragments averaging 1.0 cm maximum length. We suspect that some of the unidentified mammal bone in Savage's and Dods' reports is also walrus, simply on the basis of its difficult identification and the near ubiquitousness of walrus in other Maritime Archaic samples. Thus, the relative frequency of this species at Rattler's Bight should be judged only on the basis of Spiess' identifications in the limited 20 square sample.

The identified Rattler's Bight 1 calcined bone sample is dominated by mammal bone (n=181) fragments with a lesser frequency of bird bone (n=125). Bone counted as "mammal or avian" could not be identified to class, except that it was definitely not fish bone. The data presented below are based upon the sample identified by Spiess in 1986.

The mammal fauna are dominated by seals (n=124), most bones of which cannot be identified to species. However, there are several bones identified to *P. groenlandica*, the harp seal, and one ear bone piece (broken in two fragments) from a harbor seal, *P. vitulina*. Also present in the sample are walrus (n=14), caribou (n=7), hare (n=1), bear (n=1), and fisher or otter (n=2).

The seal identifications are dominated by manus or pes (hand-foot or flipper) bones. These bones are among the smallest in the seal body, are dense, and therefore resist breakage and survive to identifiability in calcined assemblages. Other identified seal bones include jaw parts, auditory bulla parts, cranial vault fragments, an occasional rib fragment, a pelvic fragment, and occasional appendicular longbone fragments. The bird bone identifications include a great many appendicular longbone fragments, phalanges, coracoids and scapulas, plus an occasional sternal or pelvic fragment. Again, the factors of preservation in a calcined bone assemblage seem to determine body element composition in the sample. Spiess' walrus identifications are based on cranial vault fragments, a tusk fragment (ivory), a rib fragment, and a proximal four-fifths fragment of a foot terminal phalange. Spiess' caribou identifications are based on antler fragments, metatarsal shaft fragments with a distinctive vascular groove, and hoof parts, particularly auxiliary (dew claw) parts and sesamoid bones.

Seasonality evidence is meager but present. The identification of dovekie in large numbers would indicate a late fall or winter occupation; however, only one bone of this species is identified. Since the species is present in small numbers at other seasons, it cannot be considered a seasonality marker. Most of the duck species identified by Savage are summer breeders or common summer non-breeding visitors. The exception is the king eider, which today can seldom be found breeding south of 65 degrees north. It is an uncommon summer visitor, a common winter resident off the south Labrador coast, and a common spring and fall migrant. A seal assemblage dominated by harp seal and harbor seal is indicative of open water hunting, or at least hunting among broken pack ice. Harp seals are seasonal migrants, with the greatest concentration available inshore during the late fall southward migration. We have elsewhere commented that walrus might have been present off the south Labrador coast during multiple seasons before the St. Lawrence walrus herd was drastically reduced by European hunting in the 16th and 17th centuries. Surprisingly, in view of the calcination of the sample, one (calcined) seal tooth yielded a season-of-death estimate. The tooth was a *Phoca* species canine tip with a very broad dentine cavity, indicating a young individual. If this specimen is a harp seal, it represents a juvenile taken on its northward (spring) migration. If it is a harbor seal, it represents a summer kill.

In sum, the Rattler's Bight economy appears to have been dominated by seal and walrus hunting, probably with dominant seal hunting during breakup (spring) and late fall harp seal migrations. The timing of walrus hunting is unknown, but it could have been multiseasonal. Spring, summer and fall bird hunting in open water or broken pack ice was also practiced commonly. The exact timing of terrestrial hunting for caribou, which appears to have been a modest part of the economy, and terrestrial fur trapping, is unknown.

A small faunal collection (n=33) from Hound Pond 2 contains a diversity of species that matches the diversity found at Rattler's Bight 1, including seal (n=2), duck (n=3), caribou (n=1), a large sea mammal, probably walrus (n=1), and a fisher or otter (n=1). In sum, the Hound Pond 2 sample records boreal forest hunting and trapping, caribou hunting, walrus and seal hunting, and summer (?) duck hunting.

The Rattler's Bight 1 and Hound Pond 2 data may indeed be used to support a Modified Maritime type of economy for Late Maritime Archaic in the mouth of Hamilton Inlet. Use of the wooded environment of the near interior to hunt caribou and trap furbearers was apparently accomplished from these sites, or the proceeds were habitually brought back to these sites. These data argue against any separate substantial interior seasonal hunting period with habitation sites well removed from the coast.

The coastal economy is confirmed to include walrus, which seems ubiquitous in Maritime Archaic subsistence. There is no evidence for the hunting of larger whales from these sites, but the calcined and broken state of preservation of the faunal sample would select against the identifiability of pieces of whalebone. Apparently the staple of Maritime Archaic subsistence, if it was not whale and walrus hunting, was a multiseasonal harp seal hunt. The possiblility of an extended fall hunt, relative to modern climatic conditions, would have allowed accumulation of a substantial surplus. The presence of king eider and possibly dovekie may indicate hunting in broken pack ice into late fall or winter.

CONCLUSIONS: THE MARITIME ARCHAIC SUBSISTENCE BASE

The earliest Maritime Archaic sites to preserve faunal remains are EiGb-7 and the L'Anse Amour mound, both dating before 7000 B.P. The Fowler site dates circa 6500 B.P. A maritime hunting economy based upon walrus, seals, and birds is indicated, while caribou were definitely a secondary resource. Other faunal samples dating to the late Maritime Archaic occur at Port-aux-Choix, Rattler's Bight 1 and Hound Pond 2. Seals, walrus, sea birds, and waterfowl dominate these assemblages. Caribou and terrestrial furbearers are present but secondary. On the central and northern Labrador coast, where early, middle, and late Maritime Archaic sites can all be found, general settlement location tended to remain the same over time: focused on the outer mainland coast and larger offshore islands. However, the late Maritime Archaic adaptation included much larger, probably multiseasonal, groups of people living in unique, linear, longhouse (or longtent) structures. The faunal evidence indicates that this intensification of settlement was accomplished without a dramatic change in basic subsistence strategy.

The Maritime Archaic culture was adapted to different climatic, phytogeographic, and maritime conditions than characterize the area today. During middle Maritime Archaic occupation, the last remnant of the Laurentide ice sheet was disintegrating in the middle of the Quebec-Labrador peninsula. A mixed open forest of white spruce, fir, birch, and poplar was expanding northward at the margins of the ice, but it was excluded from the central and north Labrador coast by tundra. Maritime Archaic culture was present along the Labrador coast as far north as shrub tundra had become established. The Labrador current was forced eastward at Hamilton Bank, allowing warmer sea surface conditions in the gulf of St. Lawrence and along the south Labrador coast than exist today, and general climatic conditions were slightly warmer. These conditions combined, especially along the south coast of Labrador and in the Gulf of St. Lawrence, to reduce sea ice conditions compared with today. By late Maritime Archaic times the interior Labrador-Quebec forest was becoming dominated by a closed black-spruce forest, and the Labrador current was beginning to make its influence felt along the southern Labrador coast and in the Gulf of St. Lawrence.

The generally lessened sea ice cover during Maritime Archaic occupancy would have affected sea mammal species differentially. Ringed seals, which are dependent upon stable, landfast sea ice for breeding, would probably have been less common. They may have been an insignificant population south of Hamilton Inlet. Harp seals, which are dependent upon pack ice in the Gulf of St. Lawrence for breeding, perhaps maintained a lower population level than the pre-20th-century maximum (although still in the millions). Their breeding ice may have been less accessible from shore, especially before the influence of the cold Labrador current increased in the Gulf of St. Lawrence after circa 6000-4000 B.P. Harp seal migrations along the central and northern Labrador coast could have begun earlier in the spring and lasted later in the fall. There would have been less exclusion of harp seals from bays and inner island shores by early fall formation of landfast ice. Populations of grey seals and harbor seals, which are localized but are severely limited by ice formation, might have been higher.

Walrus are primarily dependent upon the coincidence of open water (either the *sina* or broken pan ice during winter, or ice-free conditions) and shallow water to reach benthic feeding grounds. Lessened landfast ice cover would have meant greater inshore access to walrus for human hunters, and/or larger overall walrus populations. We know that the Gulf of St. Lawrence supported a massive walrus population before European and Euroamerican overkill during the last few centuries. Assuming north-south seasonal movements of walrus from population centers in the Gulf of St. Lawrence, the central Labrador coast, and Hudson Strait, walrus from various populations should have been present during most seasons of the year throughout the Maritime Archaic range. The three species of large whales (humpback, bowhead, and right whale) were likewise present in much higher population levels. One or the other species was present just about anywhere within the Maritime Archaic range during most seasons.

Most species of seabirds and waterfowl, particularly alcids, ducks and geese, would have been favored by slightly longer ice-free conditions. Slightly warmer waters, particularly south of Hamilton Inlet, should have favored codfish populations.

Caribou populations would have been much more localized in distribution and present in herds of much smaller absolute size than the modern George River herd. It may not have been until Late Maritime Archaic times that a substantial herd developed in northeastern Labrador. Even then, the migration pattern would not have been as extensive as that of the modern George River herd. I have proposed a model of relatively localized, near-coastal caribou herds for most of Labrador during Maritime Archaic occupancy, based on the pattern in Greenland. Caribou herds on the island of Newfoundland, of course, are even today modest in size, relatively localized in migration pattern (compared with long-distance migrators such as the George River herd), and relatively accessible from the coast.

Was there a subsistence pattern that characterized Maritime Archaic as a cultural tradition, and is there any evidence of its origin or developmental change? The answer to the first part of the question, at least, is affirmative. Maritime Archaic faunal collections, from the earliest to the latest, demonstrate a primary focus on sea hunting. Seals, walrus, and sea birds were primary prey. The addition of walrus to this complex, as a consistent and potentially major contributor, is a new feature added by my identifications from Rattler's Bight 1 and this review. Unfortunately, the nature of the faunal samples (grave inclusions and calcined hearth debris) limits our ability to make quantitative statements about the contribution of walrus. However, the average walrus weighs 10 times what the average *Phoca* (seal) weighs. For the Gulf of St. Lawrence, perhaps walrus were a primary resource early in the Maritime Archaic sequence, especially if access to the pupping harp seal herd was limited until later by sea ice conditions.

Caribou have played a consistent but auxiliary role in Maritime Archaic subsistence since the earliest faunal samples. Their part seems to have been allowed by the synergy of geographic proximity and easy access to caribou herds near the coast. We postulate that Late Maritime Archaic population concentrations in northernmost Labrador were in part fueled by a population peak in a near-coastal caribou population, one that may not have been present earlier in time.

Thus, we postulate a Maritime Archaic seal and walrus hunting adaptation, supplemented by caribou hunting and furbearer trapping in near-shore woodlands. There is no evidence in the record for an initial specialization on caribou, as postulated by McGhee and Tuck (1975). Various Late Maritime Archaic groups may have been favored by localized subsistence specializations added to this economic base, such as caribou migrating right through camp at Nulliak, and easier access to harp seals whelping on sea ice off the west coast of Newfoundland. Perhaps some or all Maritime Archaic groups added large whale hunting to the cultural repertoire at some time. As the data have accumulated during nearly 20 years of research, the concept of Maritime Archaic as an Interior-Maritime adaptation type has slowly shifted toward a concept akin to a Modified Maritime adaptation type, complete with nucleated settlements in the later stages of the develpmental sequence and accompanied by exchange and mortuary complexity.

In our view the concept of initial immigration into the Strait of Belle Isle area by people with an interior caribou-hunting adaptation is probably untenable because caribou would have been confined to near-coastal areas in smaller herds for much of the early and mid-Holocene in Labrador. The concept of a slowly developing maritime competence and retention of the seasonal interior caribou-hunting economic subsystem as a sort of cultural conservatism is a poor model. Rather, we would say that a geographically separate interior caribou-hunting seasonal adaptation has played no part in Maritime Archaic prehistory and that the principal focus of this tradition has always been coastal marine mammal hunting. In fact, walrus were an important part of the maritime hunting economy by 7500 B.P., and probably remained so for the whole Maritime Archaic sequence. Although uncertain, we speculate that large whale hunting of some form was also regularly practiced. Caribou hunting was locally important where caribou came into near-coastal areas on a seasonal basis, or as at Nulliak, where they walked right into the settlment.

We here hypothesize that inner and outer island and bay mouth settlement locatons were used year round by central and north Labrador Maritime Archaic groups. By the Late Maritime Archaic

(Rattler's Bight Phase), large longhouse-based villages had become year-round central places, from which there might have been short-term movements to other locations by a greater or lesser proportion of the population. The population concentrations were fueled by seasonal successes in marine mammal harvesting: fall and spring harp seal hunting and multi-seasonal walrus hunting. Occasional success killing a large whale in the summer or late fall may have been included in the economic mix. Thus, at least the Late Maritime Archaic longhouse villages take on an economic aspect similar to the large, multi-seasonal Neo-Eskimo villages, with a central-place aspect perhaps as described in ethnohistorical accounts of Labrador Inuit (Taylor 1974).

REFERENCES

Allen, Glover M.

1916 *The Whalebone Whales of New England.* Boston Society of Natural History Memoirs 8:2.

Ames, Randy

1977a Land Use in the Postville Region. In *Our Footprints Are Everywhere*, edited by C. Brice-Bennett, 205-237. Labrador Inuit Association, Nain.

1977b Land Use in the Rigolet Area. In *Our Footprints Are Everywhere*, edited by C. Brice-Bennett, 275-308. Labrador Inuit Association, Nain.

Banfield, A.W.F.

1961 *A Revision of the Reindeer and Caribou, Genus Rangifer*. National Museum of Canada Bulletin 177.

Bigg, Michael

1981 Harbour Seal: *Phoca vitulina* and *P. largha*. In *Handbook of Marine Mammals*: Vol. 2, edited by Sam H. Ridgeway and Richard J. Harrison, 1-28. Academic Press, London.

Bonner, W. Nigel

1981 Grey seal - *Halichoerus grypus*. In *Handbook of Marine Mammals*: Vol. 2, edited by Sam H. Ridgeway and Richard J. Harrison, 111-114. Academic Press, London.

Bourque, Bruce J.

1971 Prehistory of the Central Maine Coast. Ph.D. dissertation, Department of Anthropology, Harvard University, Cambridge.

1975 Comments on the Late Archaic Population of the Central Maine Coast: The View from the Turner Farm. *Arctic Anthropology* 12(2):35-45.

Brice-Bennett, Carol

1977 Land Use in the Nain and Hopedale Regions. In *Our Footprints are Everywhere*, edited by C. Brice-Bennett, 97-204. Labrador Inuit Association, Nain.

Clark, Peter, and William W. Fitzhugh

1990 Late Deglaciation of the Central Labrador Coast and its Implications for the Age of Glacial Lakes Naskaupi and McLean and for Prehistory. *Quaternary Research* 34:296-305.

Couturier, Serge, Josée Brunelle, Denis Vandal, and Guy St-Martin

1990 Changes in the Population Dynamics of the George River Caribou Herd, 1976-1987. *Arctic* 43:9-20.

Cox, Steven L.

1977 Prehistoric Settlement and Culture Change at Okak, Labrador. Ph.D. dissertation, Department of Anthropology, Harvard University, Cambridge.

Cox, Steven L., and Arthur E. Spiess

1980 Dorset Settlement and Subsistence in Northern Labrador. *Arctic* 33:659-669.

Crete, Michel, Jean Huot, and Line Gauthier

1990 Food Selection During Early Lactation by Caribou Calving on the Tundra in Quebec. *Arctic* 43:60-65.

Davis, Rolph, Kerwin Finley, and W. John Richardson

1980 *The Present Status and Future Management of Arctic Marine Mammals in Canada.* Science Advisory Board of the Northwest Territories, Yellowknife.

Dods, R. Robin

1977 Untitled manuscript report to William Fitzhugh on identification of 1972 and 1973 Rattler's Bight faunal samples.

Engstrom, D.R., and B.C.S. Hansen

1984 Postglacial Vegetational Change and Soil Development in Southeastern Labrador as Inferred from Pollen and Chemical Stratigraphy. *Canadian Journal of Botany* 63:543-561.

Fay, Francis H.

1982 *Ecology and Biology of the Pacific Walrus, Odobenus rosmarus Illiger*. North American Fauna, 74. U.S. Fish and Wildlife Service. Washington, D.C.

Fillon, Richard H.

1976 Hamilton Bank, Labrador Shelf: Postglacial Sediment Dynamics and Paleo-oceanography. *Maine Geology* 20:7-25.

Fitzhugh, William W.

1972 *Environmental Archaeology and Cultural Systems in Hamilton Inlet, Labrador*. Smithsonian Contributions to Anthropology 16.

1975 A Comparative Approach to Northern Maritime Adaptation. In *Prehistoric Maritime Adaptations of the Circumpolar Zone*, edited by W.W. Fitzhugh, 339-386. Mouton, The Hague.

1976 Preliminary Culture History of Nain, Labrador: Smithsonian Fieldwork, 1975. *Journal of Field Archaeology* 3:123-142.

1978 Maritime Archaic Cultures of the Central and Northern Labrador Coast. *Arctic Anthropology* 15(2):61-95.

1980 Preliminary Report on the Torngat Archaeological Project. *Arctic* 33(3):585-606.

1981a Boulder Pits to Long Houses: Settlement and Community Pattern Development in the Labrador Maritime Archaic. Paper presented to the Canadian Anthropological Association.

1981b A Prehistoric Caribou Fence from Williams Harbor, Northern Labrador. Paper presented to the Chacmool Conference, Calgary.

1984 Residence Pattern Development in the Labrador Maritime Archaic: Longhouse Models and 1983 Field Surveys. In *Archaeology in Newfoundland and Labrador 1983*, edited by Jane Sproull Thomson, 6-47. Heritage Resources, St. John's.

1985a The Nulliak Pendants and Their Relation to Spiritual Traditions in Northeast Prehistory. *Arctic Anthropology* 22(2):87-110.

1985b Early Maritime Archiac Settlement Studies and Central Coast Surveys. In *Archaeology in Newfoundland and Labrador 1984*, edited by Jane Sproull Thomson and Callum Thomson, 48-85. Annual Report No. 5, Historic Resources Division, Government of Newfoundland and Labrador, St. John's.

Fitzhugh, William W., and Henry F. Lamb

1981 Vegetation History and Culture Change in Labrador Prehistory. *Arctic and Alpine Research* 17:357-370.

Groison, Dominique

1985 Blanc-Sablon et le Paléo-Indien au Détroit de Belle-Isle. *Recherches Amérindiennes au Québec* 15:127-132.

Harrington, Fred H.

1988 The George River Caribou Population: Its History and Management, with special Reference to Low-level Military Training in Labrador. Manuscript on file with the Labrador Inuit Association, Nain.

Henriksen, Georg

1973 *Hunters in the Barrens: The Naskapi on the Edge of the White Man's World.* Newfoundland Social and Economic Series No. 12. Memorial University, St. John's.

Kaplan, Susan

1983 Economic and Social Change in Labrador Neo-Eskimo Culture. Ph.D. dissertation, Bryn Mawr College, Bryn Mawr.

Knight, James A.

1985 Differential Preservation of Calcined Bone at the Hirundo Site, Alton, Maine. Master of Science Dissertation in Quaternary Studies, University of Maine at Orono.

Lamb, Henry F.

1980 Late Quaternary Vegetational History of Southeastern Labrador. *Arctic and Alpine Research* 12:117-135.

McAndrews, J.H.

1976 Holocene Pollen Analysis from Northern Quebec. *American Quaternary Association Abstract*, Tempe.

McGhee, Robert, and James A. Tuck

1975 *An Archaic Sequence from the Strait of Belle Isle, Labrador.* National Museum of Man, Mercury Series, Archaeological Survey of Canada Paper 34.

Meldgaard, Morten

1986 *The Greenland Caribou-Zoogeography, Taxonomy and Population Dynamics.* Medelelser om Grønland, Bioscience 20. Copenhagen.

Reeves, Randall

1978 *Atlantic Walrus (Odobenus rosmarus rosmarus): A Literature Survey and Status Report.* Wildlife Research Report 10, U.S. Fish and Wildlife Service, Washington, D.C.

Reeves, Randall, James G. Mead, and Steven Katona

1978 The Right Whale (*Eubalaena glacialis*) in the Western North Atlantic. *Reports International Whaling Commission* 28:303-312.

Richard, Pierre J.H., Alayn LaRouche, and Michael Bouchard

1982 Age de la déglaciation finale et histoire postglaciaire de la végétation dans la partie centrale du Nouveau-Québec. *Géographie Physique et Quaternaire* 36(1-2)63-90.

Ronald, K., and J.L. Dougan

1982 The Ice Lover: Biology of the Harp Seal (*Pagophilus groenlandicus*). *Science* 215:928-933.

Ronald, K., and P.J. Healey

1981 Harp seal - *Phoca groenlandica.* In *Handbook of Marine Mammals*: Vol. 2, edited by Sam H. Ridgeway and Richard J. Harrison, 55-88. Academic Press, London.

Samson, Gilles

1983 Préhistoire du Mushau Nipi Nouveau - Québec: étude du mode d'adaptation à l'intérieur des terres hémi-arctiques. Ph.D. dissertation, Department of Anthropology, University of Toronto, Toronto.

Sanger, David

1973 *Cow Point: An Archaic Cemetery in New Brunswick.* Archaeological Survey of Canada Paper No. 12. National Museum of Man, Ottawa.

1975 Culture Change as an Adaptive Process in the Maine-Maritimes Region. *Arctic Anthropology* 12(2):60-75.

Savage, Howard

1970 Untitled report to William Fitzhugh on fauna.

1972a Untitled report to William Fitzhugh on fauna.

1972b Untitled report to William Fitzhugh on fauna.

1978 Untitled report to William Fitzhugh on fauna.

Schwartz, Fred

1977 Land Use in the Makkovik Region. In *Our Footprints Are Everywhere*, edited by C. Brice-Bennett, 239-278. Labrador Inuit Association, Nain.

Shipman, Pat, Girard Foster, and Margaret Schoeninger

1984 Burnt Bones and Teeth: an Experimental Study of Color, Morphology, Crystal Structure, and Shrinkage. *Journal of Archaeological Science* 11:307-325.

Smith, Thomas G.

1973 *Population Dynamics of the Ringed Seal in the Canadian Eastern Arctic*. Fisheries Research Board of Canada Bulletin 181. Ottawa.

Snow, Dean R.

1974 The Changing Prey of Maine's Early Hunters. *Natural History*, November 1974:15-24.

Speck, Frank

1935 *Naskapi, The Savage Hunters of the Labrador Peninsula*. University of Oklahoma Press, Norman.

Spiess, Arthur E.

1976 Labrador Grizzly (*Ursus arctos* L): First Skeletal Evidence. *Journal of Mammalogy* 57:787-790.

1977 Okak Survey: Archaeological Faunal Analysis and Comparison with Ethnographic Data. In Prehistoric Settlement and Culture Change at Okak, Labrador by Steven L. Cox, 398-445. Ph.D. dissertation, Department of Anthropology, Harvard University, Cambridge.

1978 Zooarchaeological Evidence Bearing on the Nain Area Middle Dorset Subsistence-settlement Cycle. *Arctic Anthropology* 15(2):48-60.

1979 *Reindeer and Caribou Hunters: An Archaeological Study*. Academic Press, New York.

1991 Avayalik Revisited: Faunal Data Report. Manuscript report on file, Arctic Studies Program, Smithsonian Institution.

n.d. Archaic Period Subsistence in New England and the Atlantic Provinces. In *Early Holocene Occupation in Northern New England*, edited by Brian Robinson, James B. Petersen, and Ann S. Robinson. Occasional Publications in Maine Archaeology, No. 9. Maine Historic Preservation Commission and Maine Archaeological Society, Augusta.

Spiess, Arthur E., Bruce J. Bourque, and Steven L. Cox

1983 Cultural Complexity in Maritime Cultures: Evidence from Penobscot Bay, Maine. In *The Evolution of Maritime Cultures on the Northeast and Northwest Coasts of America*, edited by Ronald J. Nash, 98-111. Department of Archaeology, Simon Fraser University, Publication No. 11. Vancouver.

Spiess, Arthur E., and Steven L. Cox

1976 Discovery of the Skull of a Grizzly Bear in Labrador. *Arctic* 29:194-200.

Spiess, Arthur E., Mary Lou Curran, and John R. Grimes

1985 Caribou (*Rangifer tarandus* L.) Bones from New England Paleoindian Sites. *North American Archaeologist* 6(2):145-159.

Spiess, Arthur E., and Robert Lewis

1990 The Turner Farm: Five Thousand Years of Hunting and Fishing in Penobscot Bay, Maine. Manuscript prepared for publication of the Turner Farm report, edited by Bruce J. Bourque. Smithsonian Institution Press, Washington, D.C.

Stevens, Willis

1985 Progress Report on the Marine Excavation at Red Bay, Labrador: A Summary of the 1983 Season. In *Archaeology in Newfoundland and Labrador 1984*, edited by Jane Sproull Thomson and Callum Thomson. Annual Report No. 5. Historic Resources Division, St. John's.

Taylor, J. Garth

1974 *Labrador Eskimo Settlements of the Early Contact Period*. National Museum of Man, Publication in Ethnology 9. Ottawa.

Tuck, James A.

1975 Maritime Adaptations on the Northwestern Atlantic Coast. In *Prehistoric Maritime Adaptations of the Circumpolar Zone*, edited by W.W. Fitzhugh, 255-268. Mouton, The Hague.

1976 *Ancient People of Port au Choix Newfoundland*. Social and Economic Series 17. Memorial University, St. John's.

Val, Erik

1976 Inuit Land Use in the Port Burwell Area. In *Inuit Land Use and Occupancy Project*, Vol. 1, edited by Milton Freeman, 121-123. Ottawa.

Whitehead, Hal, Kevin Chu, Judith Perkins, Peter Bryant, and George Nichols

1983 Population Size, Stock Identity, and the Distribution of the Humpback Whale off West Greenland-Summer 1981. *Report of the International Whaling Commission* 33:497-501.

Winn, Lois King, and Howard E. Winn

1985 *Wings in the Sea: The Humpback Whale*. University Press of New England, Hanover.

PART III

SOUTHEASTERN PREHISTORY

8 Archaeoastronomy in the Southeast

William G. Haag
Louisiana State University
Baton Rouge, Louisiana

The possibility of intentional alignments between archaeological structures and various astronomical objects has attracted the attention of Southeastern archaeologists for the last three decades. A search of the literature for articles pertinent to the Southeast is largely a fruitless effort, for little has been done in this area. However, so much revealing work has been noted elsewhere in the Americas that any suggestion has been explored. The finding of what purport to be several woodhenges at Cahokia in Illinois and the interpretation of the Poverty Point octagon as a solstice monument have further stimulated such efforts. Even a measure of respectability has been brought to these endeavors.

Archaeoastronomy is the term usually applied to the examination and elucidation of possible relationships between the patterns of archaeological structures and various celestial objects. Hawkins (1975) has documented the unwritten evidence that we must deal with in preliterate Europe and elsewhere in the Old World. In Mesoamerica, of course, we have the luxury of carved-in-stone records, so we have unequivocal proof of solar, lunar, and stellar alignments with building details. In eastern North America we have no written records, so we are relegated to much speculation and inference. In archaeoastronomy one must refrain from oversimplification while establishing a probable relationship. We must always remember that any two points on the face of the Earth will line up with a third somewhere. Whether there is a causal or purposeful relationship in that alignment is the problem of archaeoastronomy.

There is virtually unanimous agreement that the "decoding" of the Stonehenge monument led to general acceptance that earthly structures were built to call attention to celestial phenomena (Hawkins 1965). More than fifty henge sites are now recognized in the British Isles and adjacent northwestern Europe. The sheer number of sites and the very nature of dealing with sun, moon, and stars makes the subject attractive to pseudoscientists. As a result, there has been considerable fabrication of fraudulent evidence, which in turn has resulted in popular disparagement of archaeologists and astronomers. Any scholar who readily supports such ill-founded nonsense is likely to be classed with the lunatic fringe in American archaeology. In the last decade that has not been a lonely existence, as several respectable archaeologists have written about possible astronomical recognitions among prehistoric southeastern Indians.

That archaeoastronomy has come to archaeology by way of astronomers has lent prestige to it. At the same time the astronomers have been quite conservative in general theory construction, because it is realized that an astronomer cannot successfully operate in the subdiscipline without having a reasonably intimate knowledge of the culture involved. By the same token, many archaeologists have been dismayed or repelled by some basic formulations in astronomy.

In the southeastern United States, there are several factors that make it difficult to discern archaeoastronomical manifestations. The near total absence of any prehistoric construction in stone means that we are involved with earth mounds in most cases. Mounds are most often largely amorphous, with badly eroded sides, so that the planes of the mounds cannot be established with any degree of certainty. In some large mound groups it has been possible to align mound centers, but results have not been impressively successful. We are never sure what evidence on a site that has alignments may have astronomical significance. We cannot be certain what to look for, and it is probable that many examples have been overlooked.

Another hiatus in the southeastern record has been the lack of ethnohistorical examples to suggest long-continued practices among aborigines. There are some documented Caddo cosmologies (Wedel 1977), and the French explorers made abbreviated references to Natchez sun rituals (Swanton 1911). Most rituals that have been described by early ethnologists or travelers are concerned with hunting or planting (Hudson 1976).

The few southeastern examples of prehistoric archaeoastronomy show more dissimilarities than shared traits until we reach the plateau of Mississippian times. When that period is reached, we find enough shared traits to begin to make some generalizations (Daniel-Hartung 1981). In contrast to the Southeast, the archaeological history of the Southwest shows numerous rather patent examples of concern with celestial alignments. One of the more recent discoveries has been the so-called "Dagger of Light" on Fajada Butte in Chaco Canyon (Sofaer *et al.* 1971). The "medicine wheels" of the High Plains, of which more than fifty are now known, seem to have general acceptance as genuine prehistoric celestial observing structures (e.g., Eddy 1974; Kehoe and Kehoe 1979). The virtually universal concern of man with the sky in Mesoamerica from Olmec to historic times has been noted above.

In the seventeenth century, when the great English architect, Inigo Jones, was directed by James I to do a definitive study of Stonehenge, there were many explanations for its having been built. Most thought the Romans constructed it, others credited the Druids, but even in the eighteenth century it was noted that the principal axis of the whole work was aligned to midsummer sunrise. Two and a half centuries later, there are still many prominent English archaeologists who dismiss the complex multirelationships of Stonehenge to the sun and moon as pure "moonshine." It is no surprise that we have proceeded with caution in evaluating archaeoastronomical sites in the southeastern United States.

One of the best preserved and most obvious sites in the eastern United States is Crystal River, in Citrus County, Florida. It was first investigated and mapped by Clarence B. Moore in the first decade of this century (Moore 1903 and 1907). Moore was digging to acquire specimens for the Philadelphia Academy of Natural Sciences. His concern was never with the configuration of the mounds and other structures of the site. Several archaeologists from Florida have worked at the site, most notably Ripley Bullen, who had a number of seasons there including the restoration of the site when it became a state park. It was during this last episode of investigations that two stone monuments or stelae were found. Presently, these are the only two carved stone pillars known in the prehistory of the eastern United States (Bullen 1961 and 1966).

The site is not extensive. It consists of two large pyramidal or temple mounds with ramps, two burial mounds, one small house (?) mound, a shell midden, and the two stelae. A most detailed study of the site was undertaken by a local resident and completed in 1970.

Working over a period of eight months, Clark Hardman, Jr. took observations and photographs of various positions of the sun with reference to the stelae and mounds. He could discern that it was possible to fix solar positions at equinox and at the solstices. The two stelae are in alignment for noting the solstices, while an observer at one of the stone pillars would see the equinoctial sun rise behind the large mound of the burial complex. There seems to be little doubt that, except for one large mound, the entire group of mounds and stelae is essentially a solstice and equinox marker system. Hardman calculates that the site was first occupied about 30 B.C. and abandoned near A.D. 1200 (Hardman 1971).

The Angel Site in Vanderburg County, Indiana, is a large Mississippian mound group in which most houses, mounds, and some other structures are oriented to the cardinal directions either by having corners so directed or facing sides. The largest central mound (A) has its main axis along a line that bears 25° east of True North. No other mound alignments seem to be obvious (Black 1967; Daniel-Hartung 1981:108). It seems that all large mound groups are not necessarily of readily apparent celestial orientation.

An inspection of a map of the large central Alabama site of Moundville gives one an instant impression of the orderly arrangement of two dozen or more mounds. It appears that most are oriented on their mound faces or planes to the cardinal directions, except for Mound A in the center of the plaza. One is impressed by the deviation of this large flat-topped mound in terms of its orientation and its dimensions. A line drawn from the center of the top of Mound A through the center of Mound B has a N-S bearing. Hence, a projection at 90 degrees to this axis is an E-W azimuth, i.e., the equinoctial line of site. Hardman (1971, Figure 43) finds that lines drawn from the center of Mound A through, or along, one of the mound planes of several of the marginal mounds have significant solar alignments.

Moundville is one of the largest mound groups of the Late Mississippi period. It was either occupied by such a large population in A.D. 1542 that De Soto avoided any contact with it, or it was already in such a state of decline that it had no attraction to that early European visitor, much more likely since by 1541 De Soto was desperately seeking large food resources only obtainable at large population centers (Steponaitis 1983:169; Brain 1985:1i, footnote 24). Several other large mound sites of Late Mississippian times may have had configurations similar to Moundville. Etowah may fall into this category, although no meticulous survey has been done to prove its alignments. Etowah is very well preserved. Brain (*ibid.*) considers Etowah to have passed its heyday before the mid-sixteenth century.

The Fatherland site, or the Grand Village of the Natchez, is another of the late prehistoric mound groups that has three pyramidal earth mounds. French explorers of the early eighteenth century visited this site at a time near its cultural zenith. These visitors documented several interesting ceremonies. Neither these rituals nor the social relations of the resident Natchez Indians were understood by the French.

The site today consists of three large pyramidal mounds in close alignment. The centers of the three mounds have "an axis running approximately 30° to 35° E of N, to 30° to 35° W of S" (Aveni 1983:176). A line perpendicular to this axis would pass close to the direction along which the winter solstice sun rises and the summer solstice sun sets (Aveni 1983:176).

Approximately 12 miles north of the Fatherland site is Emerald, a pyramidal mound that measures 770 feet E-W, 435 feet N-S, with a 7.7 acre plaza on top that is 640 feet E-W and 345 feet N-S (Cotter 1951). At each end of the plaza there is an earth mound about 30 feet in height. A line of sight across the centers of the tops of the two mounds is true E-W. It is difficult to class this as another coincidence.

The first exhaustive study of the archaeoastronomy of a large Southeastern mound group is the Toltec site report (Sherrod and Rolingson 1987). This study goes beyond a simple determination of solar and stellar alignments, but analyzes the distances among mounds and other features of the site. The authors found that a standardized distance measurement was indicated in the preconstruction planning of the major features of the site. New mounds were not randomly added to the site plan.

The study revealed that a number of other large mound groups had been built in accordance with preconceived ideas of spacing. Most prominent in these lower Mississippi Valley mounds was conformity to primary alignments with winter solstice sunset.

The Cahokia site is a Late Mississippian site that is estimated to have numbered more than 200 mounds as recently as the last century. The group is dominated by the largest prehistoric monument north of Mexico. It has been judged that the density of population was greater here than at any other place in prehistoric North America. The largest mound, called Monks Mound because it was once surmounted by a Trappist monastery, is a multilevel pyramid whose sides are oriented to the cardinal directions. Many of the extant mounds in the vicinity are similarly oriented. Thus, Monks Mound and the Cahokia group in general coincide with other Late Mississippian sites in this orientation (Wittry 1980).

Sherrod and Rolingson (1987) made an exhaustive study of Cahokia, looking for astronomical alignments as well as evidence of modular spacing among the nearly 100 mounds still in existence. Alignments from Monks Mound to dozens of the visible smaller mounds were plotted. The complete analysis of the Cahokia site is too complex to describe here, but there undoubtedly seem to be at least three (and possibly as many as five) partial circles of postmolds. In all examples, these patterns of posts could have functioned as solstice and equinox markers. Sherrod and Rolingson expressed some reservation about these woodhenges, but recent disclosures and studies have strengthened the conclusion that these are solstice and equinoctial markers (Smith 1992).

All of the above sites have been large mound groups of late prehistoric age. A few other sites, however, can be shown to possess certain orientations with reference to the sun.

An interesting site with demonstrable postmold alignments that may be equinoctial markers is the Incinerator site in Dayton, Ohio. At the time of reporting, the site had not been completely excavated so that possible solstice marker areas were still unexplored. Digging there has disclosed numerous specimens of burned maize. Hence it is possible that the seasonal markers would have been a crucial feature in this corn-raising community. That no other Fort Ancient sites have shown solar alignments may be a reflection of the unfortunate fact that none was sought. Future research or even re-examination of field plots of completed excavations may produce some postmold patterns that fall into the category of celestial alignments (Heilman and Hoefer 1981).

Another site type worthy of perusal is the so-called "sacred circle," examples of which are quite numerous in Ohio and Indiana. Each consists of an excavated ditch in the form of a nearly complete circle. The unexcavated portion forms a causeway down which certain solar events could be viewed. The Mt. Horeb earthworks (Webb 1941) is such a site. The setting sun at winter solstice may be seen down its causeway. That it could be so viewed at the time of its construction has not been determined. Others of these circular structures have not been analyzed. Mt. Horeb was probably built in Adena-Hopewell times, thus antedating the large pyramidal mound groups by more than one thousand years.

The Poverty Point site is unique, at this writing. Its outstanding features are three large effigy mounds, one conical mound, and a central structure consisting of six concentric earth ridges that form about half of an octagon. The configuration of this central earthworks is not symmetrical nor is it irregular. There is some variation among the ridges as to distance from crest to crest and from one sector of the structure to another. Before there was much concern with the function of the aisleways, it was judged that the ridges were rather regular in their distance apart (Ford and Webb 1956:16 and Plate 1).

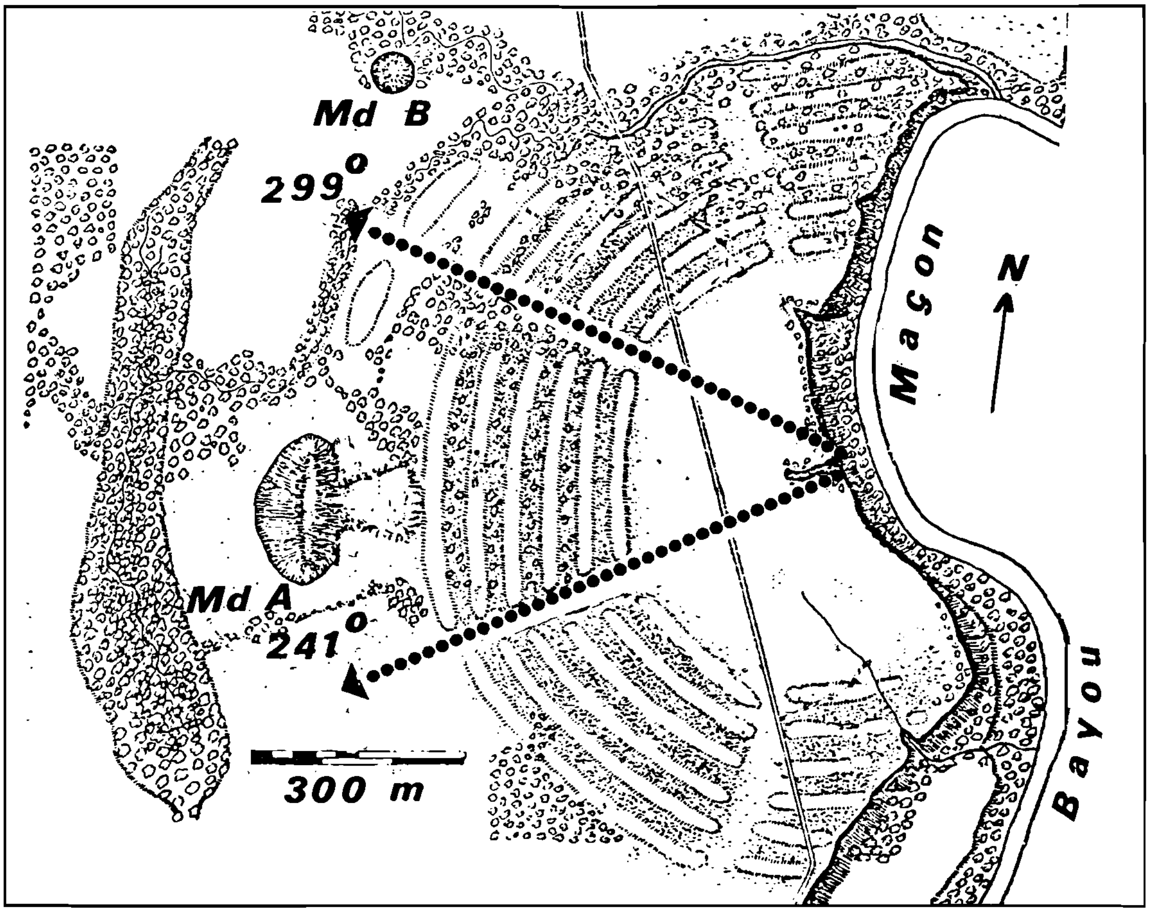


Figure 8.1. Azimuths of sightings down the northwest aisleway (299°) and the southwest aisleway (241°). These bearings are less than one degree from those of the summer and winter solstices, respectively, for this latitude ca. 1000 B.C. (See Brecher and Haag 1983:162).

The aisleways are quite straight, which leads some archaeologists to the conclusion that the ridges were built to produce the aisleways (Haag 1986:29).

It is not readily apparent whether the octagon was originally complete or not. Each scholar studying the site has made a judgment according to his needs. There has been serious slumping along meandering Bayou Maçon and there seems justification for judging that much of the original site has been lost in the last two or more thousand years of erosion.

If the octagon were formerly complete, the eastern half would probably have an aisleway to the northeast and another to the southeast. It would seem that these aisleways could have been solstice markers for summer sunrise and for winter sunrise. The ridge upon which the site was built is everywhere 20 to 30 feet (6 to 9 m) above the land surface that lies east of Bayou Maçon. From the site to the present Mississippi River, a distance of ten to fifteen miles, the land has been worked over by the meandering Mississippi and its various tributaries. We will never know for certain if the octagon was complete.

Figure 8.1 indicates the relationship of the central semioctagon to outlying mounds and other features. The site is partially wooded at present, and it presents only restricted viewing along the aisleways (Figure 8.1). The use of aerial photos gives better results. The large Motley mound is 7° W of

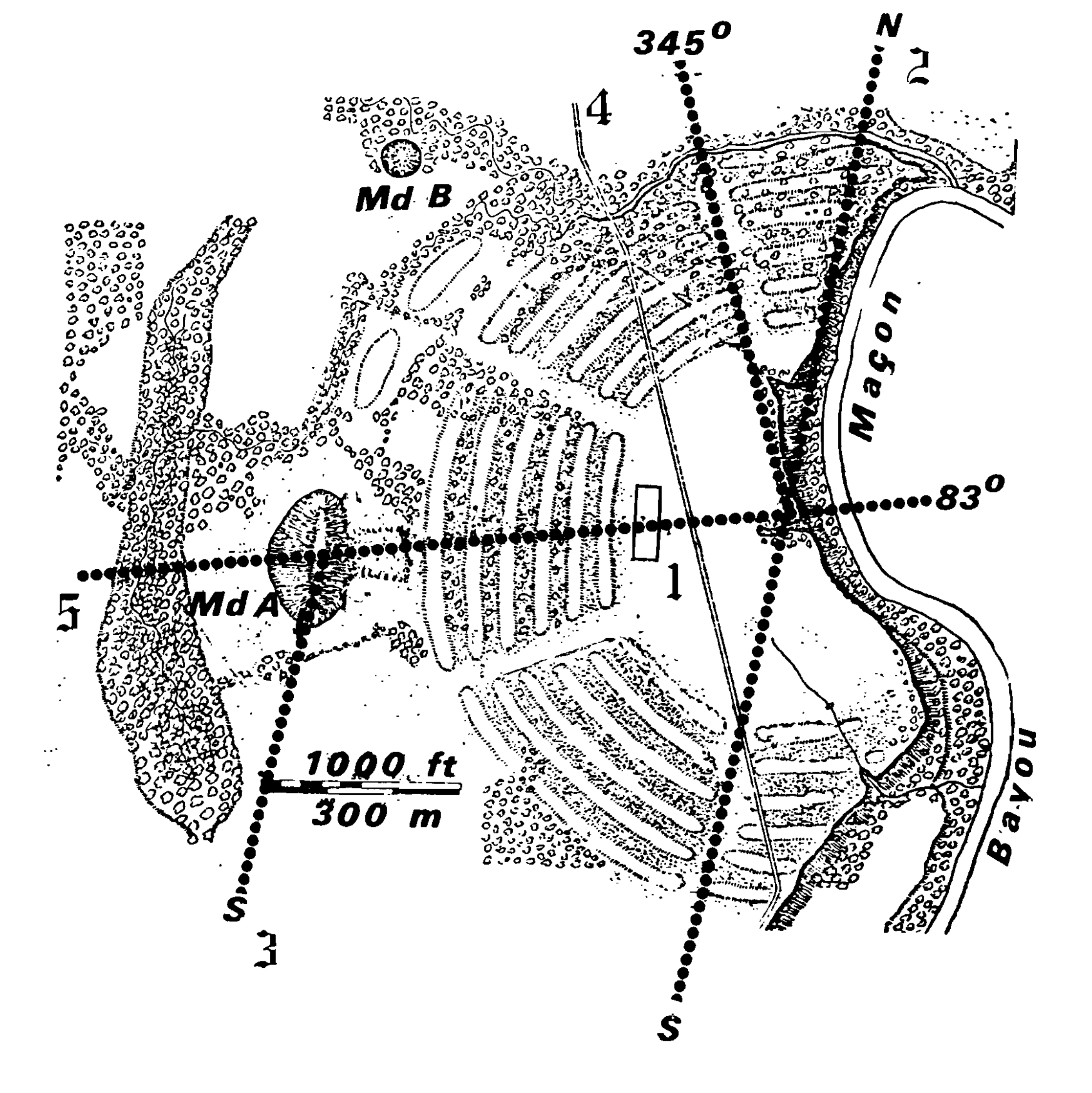


Figure 8.2. Sketch of the Poverty Point earthworks. Modified from Haag (1986: Figure 3). Not drawn to scale.

1. Area of large postmolds.
2. True north direction. On a bearing 7° W of true North at a distance of 1.5 miles (2.4 km) is the Motley Mound, perhaps another bird-shaped mound.
3. True South. At a distance of 1.6 miles (2.6 km) is the Lower Jackson Mound, an indeterminate-shaped pile of earth that may also be in the form of a bird.
4. Louisiana Highway 577, constructed in 1915. Much earth was removed from the margins of the "tail" of the bird, Mound A.
5. A line with an Azimuth of 83° running through the center of Mound A, through the center of the concentric ridges, and approximately bisecting the area of large postmolds.
345° Azimuth line extending along the west edge of the north aisleway. The archaeoastronomy of this potentially significant line remains to be determined.

North as viewed from the center of the earthworks. The azimuth of 83° for a line from Mound A through the center of the semioctagon has been noted by several scholars. The zone of large postmolds (1 in Figure 8.2) is neatly bisected by this line. This led the author to conclude that their presence, as well as the fact that there are many of them in an isolated and constricted area, was in some way related to the astronomical W-E sightings possible at the site (Haag 1986; Brecher and Haag 1983).

Meggers (1972:115), Ford (1969), and Webb (1968:318) have suggested that the Poverty Point culture is derived from Mesoamerica, possibly Olmec. The axis orientation for the site is shared with some structures at La Venta. Astronomers observed that "[t]he Poverty Point earthworks in Louisiana also possess this rare west-of-north orientation possibly indicating an infusion of Olmec cultural elements" (Aveni 1980:236). Olmec sites and Poverty Point were contemporary.

It is obvious that much additional study is necessary for further elucidation of the history of Poverty Point. This certainly encompasses much archaeological excavation data, and further explorations astronomical. Why the Poverty Point population of ca. 1000 B.C. wanted to fix solstice and equinox times is yet an unanswered question, but knowledge of the site remains quite limited despite a number of excavation programs. We can be certain that the ridges were not orderly foundations for house sites. Only four or five fire basins have been found in all our investigations to date. The ridges were not a planned "city." We can only conclude that hard established facts will "prove" that Poverty Point was an archaeoastronomical theater. That proof is yet in the future.

We may conclude that in the Southeast, there was widespread concern with fixing solar positions in relation to archaeological structures. There is little similarity of approach to solving the problem until late prehistoric times. It would seem that the practice may be recognized from Archaic to Historic Contact periods.

REFERENCES

Aveni, Anthony F.
1983 Appendix VI. Astronomical Orientations at the Fatherland Site. In *The Grand Village of the Natchez Revisited*, by R.S. Neitzel, 176. Mississippi Department Archives and History, Archaeological Report 12. Jackson.

Black, Glenn
1967 *Angel Site*. 2 Vols. Indiana Historical Society, Indianapolis.

Brain, Jeffery P.
1985 Introduction. In *Final Report of the United States De Soto Expedition Commission*, by John R. Swanton. Reprinted in the series Smithsonian Classics in Anthropology from 1939 original.

Brecher, Kenneth, and William G. Haag
1983 Astronomical Alignments at Poverty Point. *American Antiquity* 48:161-163.

Bullen, Ripley P.
1961 Radiocarbon Dates of South Florida Fiber-tempered Pottery. *American Antiquity* 27:104-106.
1966 Stelae at the Crystal River Site, Florida. *American Antiquity* 31:861-865.

Cotter, John L.
1951 Stratigraphic and Area Tests at the Emerald and Anna Mound Sites. *American Antiquity* 17(1) Part 1:18-32.

Daniel-Hartung, Ann L.
1981 Archaeoastronomy at a Selection of Mississippian Sites in the Southeastern United States. In *Archaeoastronomy in the Americas*, edited by Ray A. Williamson, 101-110. Ballena, Los Altos, California, and Center for Archaeoastronomy, College Park, Maryland.

Eddy, John A.
1974 Astronomical Alignment of the Big Horn Medicine Wheel. *Science* 184:1035-1043.

Ford, James A.
1969 *A Comparison of Formative Cultures in the Americas*. Smithsonian Contributions to Anthropology 11. Washington, D.C.

Ford, James A., and Clarence H. Webb
1956 *Poverty Point, A Late Archaic Site in Louisiana*. Anthropological Papers of the American Museum of Natural History 46(1). New York.

Haag, William G.
1986 Excavations at the Poverty Point Site: 1972-1975. In *Recent Research at the Poverty Point Site*, edited by Kathryn M. Byrd, 1-36. Louisiana Archaeology 13. Louisiana Archaeological Society, Lafayette.

Hardman, Clark
1971 The Primitive Solar Observatory at Crystal River and its Implications. *Florida Anthropologist* 24:135-167.

Hawkins, Gerald S.
1965 *Stonehenge Decoded.* Dell, New York.
1975 Archaeoastronomy: The Unwritten Evidence. In *Archaeoastronomy in Pre-Columbian America,* edited by Anthony F. Aveni, 131-162. University of Texas, Austin.

Heilman, James M., and Roger R. Hoefer
1981 Possible Astronomical Alignments in a Fort Ancient Settlement at the Incinerator Site in Dayton, Ohio. In *Archaeoastronomy in the Americas,* edited by Ray A. Williamson, 157-171. Ballena, Los Altos, California, and Center for Archaeoastronomy, College Park, Maryland.

Hudson, Charles
1976 *The Southeastern Indians.* University of Tennessee Press, Knoxville.

Kehoe, Alice B., and Thomas F. Kehoe
1979 *Solstice-Aligned Boulder Configurations in Saskatchewan.* National Museum of Man, Mercury Series, Canadian Ethnology Service, Paper No. 48.

Meggers, Betty J.
1972 *Prehistoric America.* Aldine-Atherton, Chicago.

Moore, Clarence B.
1903 Certain Aboriginal Mounds of the Florida West Coast. *Journal of the Academy of Natural Sciences of Philadelphia* 12. Philadelphia.
1907 Crystal River Revisited. *Journal of the Academy of Natural Sciences of Philadelphia* 13(3). Philadelphia.

Sherrod, P. Clay, and Martha Ann Rolingson
1987 *Surveyors of the Ancient Mississippi Valley.* Arkansas Archeological Survey Research Series No. 28. Fayetteville.

Smith, Bruce D.
1992 Mississippi Elites and Solar Alignments–A Reflection of Managerial Necessity, or Levers of Social Inequality? In *Lords of the Southeast: Social Inequality and the Native Elites of Southeastern North America,* edited by Alex Barker and Tim Pauketat, 11-30. Archaeological Papers of the American Anthropological Association 3. Washington, D.C.

Sofaer, Anna, Volker Zinser, and Rolf Sinclair
1979 A Unique Solar Marking Construct. *Science* 206:283-291.

Steponaitis, Vincas P.
1983 *Ceramics, Chronology, and Community Patterns.* Academic Press. New York.

Swanton, John R.
1911 *Indian Tribes of the Lower Mississippi Valley and Adjacent Coast of the Gulf of Mexico.* Bureau of American Ethnology Bulletin 43. Smithsonian Institution. Washington, D.C.

Webb, Clarence H.
1968 The Extent and Content of Poverty Point Culture. *American Antiquity* 33:297-321.

Webb, William S.
1941 The Mt. Horeb Earthworks Site I, and the Drake Mound, Site II, Fayette County, Kentucky. *Reports in Anthropology and Archaeology, University of Kentucky,* V(2):135-218.

Wedel, Waldo
1977 Native Astronomy and The Plains Caddoans. In *Native American Astronomy,* edited by Anthony F. Aveni, 131-145. University of Texas Press, Austin.

Wittry, Warren
1980 Cahokia Woodhenge Update. *Archaeoastronomy* 3(1):12-14.

9 Reconciling the Gender-Credit Critique and the Floodplain Weed Theory of Plant Domestication

Bruce D. Smith
Smithsonian Institution
Washington, D.C.

INTRODUCTION

To the extent that we are able to gain an understanding of the past, we do so through an open-ended, ongoing process of structured debate and disagreement. This ever-unfolding scholarly dialogue usually takes the form of hypotheses, speculations, or theories that claim to explain different aspects of the past being offered up for consideration and critical review, along with whatever kinds of supporting arguments may accompany them. Inherent in the presentation of any potential archaeological explanation of one sort or another, then, is a request for critical analysis, for debate. This was certainly the case with the initial presentation of what in 1987 I termed the Floodplain Weed Theory of Plant Domestication. Critical scrutiny and analysis was explicitly invited by requesting "the addition of supplemental layers of interpretation, including transformational or social supplementation" (Smith 1987:37).

Responses to such embedded or explicit invitations to find fault can take many forms and come from many different perspectives. They can propose alternative readings of the past based on different political or theoretical viewpoints, point out errors of logic, method, or fact, or present new information that contradicts aspects of a prior explanation. Certainly the most interesting and provocative response offered so far to the Floodplain Weed Theory is that of Patty Jo Watson and Mary Kennedy (Watson and Kennedy 1991), which takes the form of what for ease of reference I will call the Gender-Credit Critique. Responses such as that of Watson and Kennedy to extant theories in turn invite consideration and comment, both from the proponents of the previously proposed explanations, and from new directions, as new protagonists and new ideas enter the ring of scholarly debate. This paper represents such a second round response to the Gender-Credit Critique of the Floodplain Weed Theory. The mutual scrutiny of ideas, theories and interpretations represented by the Floodplain Weed Theory, The Gender-Credit Critique of that Theory, and the present consideration of the Gender-Credit Critique, is thus part of an ongoing scholarly process. Such continual mutual criticism and analysis of alternative interpretive frameworks compels scholars to attempt to successfully modify, extend, or supplement their theories in order to accommodate new information. This open and ongoing process produces, at any particular point in time, the best available approximations of past objective reality. Achieving the best possible approximations of what happened in the past is a goal that is commonly held by the vast majority of scholars in the discipline, who endorse and eagerly participate in the shared scholarly debate, the open and exhilarating clash of ideas. The consideration of the Gender-Credit Critique offered here is an effort to approach more closely an understanding of initial plant domestication in eastern North America and carries with it the explicit invitation for, and expectation of, future rounds of debate.

As new theoretical and political approaches emerge in archaeology, they invariably hold the promise of directing light on research problems from new angles of interpretation, sometimes providing valuable cross-illumination of information that was hidden in the shadows of previous explanations and missed. This is certainly the case with the various feminist approaches in archaeological inference recently summarized by Alison Wylie (Wylie 1992), which are casting interpretative light from new angles on a wide variety of research questions. One of the feminist case studies discussed by Wylie is the Gender-Credit Critique of Watson and Kennedy.

Wylie considers the Watson and Kennedy critique to have exposed "pervasive androcentrism [male centeredness] in explanations of the emergence of agriculture in the eastern United States." (Wylie 1992:22). Wylie further concludes that the Floodplain Weed Theory, and indeed "all the proponents of coevolutionary models that postulate a local, independent domestication . . . read women out of any active, innovative role in developing cultigens." (Wylie 1992:22). Drawing on the analysis of Watson and Kennedy, Wylie goes on to characterize the Floodplain Weed Theory as accounting for the initial development of domesticates in eastern North America in terms of unintentional and automatic selection pressures associated with permanent settlements and associated soil disturbance (Wylie 1992:22). In Wylie's judgement (1992:22-23):

> Watson and Kennedy make a strong case against the presumption, central to the coevolution model, that cultural change as extensive as adopting or developing domesticates could plausibly have been an "automatic process" (Watson and Kennedy 1991:266-267), and observe that they are "leary [*sic*] of explanations that remove women from the one realm that is traditionally granted them, as soon as innovation or intention enters the picture" (Watson and Kennedy 1991:264). Their assessment is that both theories [the Floodplain Weed Theory and an alternative theory proposed by Guy Prentice] share a set of underlying assumptions, uncritically appropriated from popular culture and traditional anthropology, to the effect that women could not have been responsible for any major culture-transforming exercise of human agency (Watson and Kennedy 1991:263-264).

In a more recent publication Watson extends this theme, stating that the Floodplain Weed Theory "trivializes" the role of women by "emphasizing the ease and naturalness of the proposed coevolutionary trajectory to domestication" (Watson 1992:24). Watson goes on to conclude that:

> Though Prentice and Smith *probably* did not deliberately choose to assume androcentric positions with regard to this issue, their accounts nonetheless reveal some aspects of gender bias in archaeological interpretation that should be examined more closely (Watson 1992:24, emphasis added).

Responding to Watson's closing suggestion in the above quotation, I will in subsequent sections of this paper examine more closely those aspects of the floodplain weed theory that have been characterized as being androcentric or exhibiting gender bias. As you might expect, I do not agree with the pejorative and politically engendered assessments of the Floodplain Weed Theory outlined above. The theory is not androcentric. It does not exhibit gender bias. It does not trivialize the role of women in plant domestication. It does not uncritically appropriate, deliberately or otherwise, any assumptions from popular culture regarding the limited capacity of women for creativity or cultural innovation.

Wylie's very interesting article makes two other important points that are relevant to the present critical comparison of the Floodplain Weed Theory and the Gender-Credit Critique. First, Wylie reaffirms the essential distinction to be made between political motivations and the actual strength of feminist interpretations and their supporting arguments:

> Social and political factors are crucial in directing attention to questions about gender but . . . these do not account for the successes of the research they inspire or inform. It is the substantive results of the research that makes it a serious challenge to extant practices, and these results are to a large degree autonomous of the political motivations and other circumstances responsible for the research that produced them (Wylie 1992:22).

Thus, while the claims that the Floodplain Weed Theory is androcentric and exhibits gender bias reflect back on the source of initial inspiration for the Gender-Credit Critique and attract attention because of their politically charged and personal nature, they do not provide any substantive support for the arguments that Watson, Kennedy, and Wylie present. Those must stand on their own.

The second observation made by Wylie that is pertinent to the present discussion comes in the concluding paragraph of her article and has to do with how interpretations of past reality, such as the Floodplain Weed Theory and the Gender-Credit Critique, should be compared and judged:

> I suggest then, that the question of what epistemic stance is appropriate–whether we must be relativists or objectivists, processualists or postprocessualists–should be settled locally, in light of what we have come to know about the nature of specific subject matters and about the resources we have for their investigation (Wylie 1992:30).

Based on Wylie's criteria, then, the Floodplain Weed Theory and the Gender-Credit Critique should be critically compared not only in isolation from political overtones and origins, but also within the specific context of plant domestication in the East–what kinds of information are available, and given the nature of the evidence, what forms of interpretative frameworks appear appropriate and compatible.

Interestingly, when these two observations by Wylie are used to frame the discussion, and the politically engendered discussions of androcentrism and gender-bias are set aside, the Floodplain Weed Theory and the Gender-Credit Critique do not appear to be incompatible, competing perspectives on the past. On the contrary, I argue that it is much more accurate and constructive to view them in terms of their considerable areas of commonality and compatibility. The Gender-Credit Critique clearly illuminates a number of areas where the Floodplain Weed Theory could benefit from modification and supplementation, while also highlighting promising areas for further research. At the same time the Gender-Credit Critique exhibits a number of interpretive and factual areas of weakness that can be improved and strengthened. In general, the actual differences between the two interpretive frameworks are based more on angles of emphasis than disagreements of substance. In effect, these two perspectives on plant domestication in the East are similar enough to suggest combining them in a manner that retains interesting and profitable planes of interpretive tension, while also resolving and setting aside issues of less import.

The best way to show the level of compatibility of the two perspectives, and how easily and profitably they can be merged, is by focusing on the four general areas in which they do not at present agree.

EARLY *CUCURBITA* GOURDS IN EASTERN NORTH AMERICA

The first of these four areas of disagreement has to do with the introduced domesticate versus indigenous wild status of early *Cucurbita* gourds in eastern North America.

The identity of these early, pre-4000 B.P. *Cucurbita* gourds in the East is crucial to understanding the domestication of indigenous North American plants in the region between about 4500 and 3500 years ago. Are these early *Cucurbita* gourds introduced domesticates or indigenous wild gourds? If they represent the introduction of an already domesticated plant into the East, along with the concept of agriculture, prior to the domestication of local plants, then they relegate the East to a secondary center status (Wilson 1990:452) and allow the subsequent domestication of local plants to be characterized as a derivative copy-cat or coat-tail process.

Although exhibiting some measure of equivocation on this question, Watson and Kennedy eventually adopt the mid-1980s secondary center consensus involving the early introduction into the East of an already domesticated *Cucurbita* gourd:

> The domestication of native cultigens described by Smith was apparently preceded by introduction of another type of domesticate, *Cucurbita* gourd and bottle gourd, in various parts of the Eastern US beginning about 7000 years ago (Watson and Kennedy 1991:263).

This position on early *Cucurbita* materials is in direct opposition to the Floodplain Weed Theory (Smith 1987, 1989, 1991, 1992a, 1992b), which identifies these early gourds as indigenous wild plants.

Based on research largely carried out since the initial development of the Gender-Credit Critique, substantial strong support now exists for the present-day and prehistoric existence of wild *Cucurbita* gourds in eastern North America. Lee Newsom has recently documented the presence of late Pleistocene wild *Cucurbita* gourds in Florida long before a domesticate would have been available for introduction from Mesoamerica (Newsom *et al.* 1992). A recent search of herbarium collections has yielded numerous collection records of a free-living *Cucurbita* gourd in the St. Louis area in the 1840s-1860s (Smith *et al.* 1992; Cowan and Smith 1991, 1992), less than 10 years after wild *Cucurbita texana* gourds were described in Texas.

Documented in the late Pleistocene in Florida, recovered from Mid-Holocene and later archaeological contexts across the East, and collected by prominent botanists in Texas and the St. Louis area as early as the 1830s–1860s, these wild *Cucurbita* gourds have now also been discovered, and described in detail, growing in natural floodplain habitats along the Buffalo and other rivers and streams of the Arkansas and Missouri Ozarks (Smith *et al.* 1992; Cowan and Smith 1991, 1992). Finally, allozyme analysis of these present-day wild Ozark *Cucurbita* gourds (*C. pepo* ssp. *ovifera* var. *ozarkana*) and comparison with other wild and domesticated *Cucurbita* taxa, carried out by Deena Decker-Walters and Terrence Walters (Decker-Walters *et al.* 1992), has established their considerable time depth in the East and their ancestral progenitor role for the *C. pepo* lineage independently domesticated in the East (*C. pepo* ssp. *ovifera*).

Taken together, all of these lines of evidence constitute a compelling argument for the rationally decisive rejection of the early-introduced-domesticate consensus of the mid-1980s, and suggest a necessary modification of the Watson and Kennedy position on this issue. The adoption of an indigenous wild gourd/local domestication of *Cucurbita* could be easily accomplished with no adverse impact on the main elements of the Gender-Credit Critique, while bringing it into close agreement with the Floodplain Weed Theory. Interestingly, it would also actually remove an internal logical inconsistency from Watson and Kennedy's general argument, and close off a vulnerable opening for a counter political critique. At present, by having domesticated gourds brought into the East, Watson and Kennedy leave themselves open to charges of androcentrism and gender-bias. A carefully conducted argument by analogy employing appropriately targeted and properly partitioned ethnographic analogs, similar to the one Watson and Kennedy themselves employ, could provide a convincing case that diffusion of new ideas and new crops into eastern North America was likely largely or entirely accomplished by men in the role of inter-societal traders and emissaries. It might be argued that, from a political perspective, Meso-centric, introduced-domesticate-gourd explanations, including the Gender-Credit Critique, are androcentric or male-centered.

Just as it seems appropriate to modify one aspect of the Gender-Credit Critique and in so doing strengthen its factual basis and internal logical structure, so too can the Floodplain Weed Theory be modified to acknowledge the likely major role played by women in the domestication of plants in eastern North America. This supplementation of the Floodplain Weed Theory is all the easier because it is in fact a gender-neutral approach.

GENDER-NEUTRAL APPROACHES IN ARCHAEOLOGICAL INFERENCE

Watson and Kennedy are correct in characterizing the Floodplain Weed Theory of Plant Domestication as gender-neutral (Watson and Kennedy 1991:259). It occupies a previously safe and heavily populated neutral zone of archaeological interpretation. It in effect favors neither men nor women, taking a position of neutrality, essentially no position, on the question of men's versus women's roles in plant domestication. Instead, it considers plant domestication on a different level of analysis. By acknowledging that a large network of small local populations across the Late Archaic landscape of the East operated the laboratories of domestication (Smith 1987:36-37), the Floodplain Weed Theory focuses on family groups forming closely cooperating social and economic units. Inherent in the Floodplain Weed Theory is the assumption that small close-knit kin groups–households and groups of households occupying discrete settlements or domestilocalities–constitute an appropriate unit and level of analysis, given the small scale, transitional to food production position, and egalitarian nature of the Archaic populations under consideration. This is not to suggest that these domestilocality kin groups did not witness shifts in group affiliation and varying patterns of division of labor by age and gender. But it does assume that the inhabitants of domestilocalities can be reasonably characterized as forming cooperative entities.

It is this seemingly straightforward selection of the small kinship-based social groupings that occupied domestilocalities as constituting an appropriate level and unit of analysis, however, that forms the basis for the accusation of gender bias. By focusing on entire family units rather than reserving the exclusive role of domesticator for the women members of such family groups, the Floodplain Weed Theory is judged by Wylie, Watson, and Kennedy to deny women their accomplishment and to trivialize their role in the process of plant domestication.

Interestingly, if this gender-neutral focus on entire social sub-units occupying domestilocalities constitutes androcentrism, then much of archaeological inference suffers from the same form of gender bias. By logical extension one could also conclude that in similarly failing to specifically and explicitly identify women as the gender responsible for plant domestication in the East, everyone who has written on the topic prior to Watson and Kennedy are also guilty of gender bias.

If a logical field approach is taken to assigning gender credit to human accomplishments observed in the archaeological record, and an initial assumption of two possible gender categories is made (but with no admission of hetero-centric bias), there would be only three logical categories of credit assignment. On the left, men get the credit. On the right, women get the credit. In the middle, a large neutral category would include all approaches to the question of plant domestication that do not include a gender-specific assignment of credit. These neutral-zone approaches, including the Floodplain Weed Theory, are targeted at non-gender-specific levels of analysis. They do not assign the credit to men, nor do they explicitly deny women the credit for plant domestication. They do not preclude a recognition of a major role for women in the process; they simply don't address the issue.

The Gender-Credit Critique, however, and Wylie's interpretive perspective, would appear to carry an embedded and engendered political simplification of the landscape of possible approaches, reducing it to two categories–one for those who explicitly acknowledge the role of women in the domestication of plants, and one for those who don't, with the latter category being labeled androcentric and gender biased. Under this political rule of archaeological interpretation, the middle ground neutral zone is effectively erased. Assuming that the proposed new "no neutral zone" rule applies retroactively to previous approaches to plant domestication in the East, as Wylie appears to indicate (Wylie 1992:22), it would necessitate a reclassification of all other previously neutral-zone, non-gender-specific accounts as androcentric and gender biased.

It is not yet clear, however, whether this gender-credit rule is seen as applying only to plant domestication in the East, more broadly to all developments or innovations in which women may have played a leading role, or to archaeological inference in general. Must archaeologists attempt to assign gender credit, male or female, in all situations of past innovation in order to avoid the gender bias label?

This political litmus test would appear to dictate that, in order to avoid the possible label of androcentric gender bias, it is necessary to operate within a restricted gender-specific frame of reference. Credit must be assigned. But this erasure of the neutral zone is basically unnecessary. It assumes that neutral-zone approaches, such as the Floodplain Weed Theory, which operate on a different level of analysis, are not gender specific but gender neutral and do not in fact address the issue of gender credit at all, represent a mutually exclusive alternative to gender-credit approaches. Since many of these gender-neutral approaches in fact neither deny nor preclude a gender-specific approach, however, and operate on different levels, they can easily accommodate, when appropriate, gender-credit recognition such as acknowledging that women likely played a major role in plant domestication in the East. The Floodplain Weed Theory of Plant Domestication, for example, was specifically corrected and expanded in response to the Gender-Credit Critique (Smith 1991, 1992a, 1992b) to acknowledge more specifically the likely role of women in the domestication of plants in eastern North America.

As is often the case with newly emerging approaches in archaeology, political rhetoric appears, in my opinion, to have both obscured and distracted attention from the main theme of the analysis by Watson and Kennedy. By specifically pointing out the likely major role of women in plant domestication in the East, they draw attention to a variety of gender-related research questions that otherwise would not come under consideration. These issues, and potentially productive lines of inquiry, some of which are raised later in this paper, have been hidden in the shadows of gender-neutral approaches and largely missed. But this does not mean that gender-neutral approaches are either gender-biased or no longer tenable.

HUMAN INTENTIONALITY AND PLANT DOMESTICATION

The question of human intentionality seemingly represents the most serious area of disagreement between the Floodplain Weed Theory and the Gender-Credit Critique. The apparent substantial degree of disagreement between these two interpretive perspectives regarding human intentionality, however, has been largely resolved through clarification and modification of the Floodplain Weed Theory subsequent to its first detailed presentation in 1987 (Smith 1987). This clarification, which primarily involved an important and necessary change in emphasis rather than any major substantive change or structural reconstruction, is evident in more recent presentations of the Floodplain Weed Theory (Smith 1989, 1991, 1992a, 1992b), as well as in earlier briefer discussions (Smith 1985a, 1985b). Watson and Kennedy are correct in stressing how human intentionality is downplayed or de-emphasized in the 1987 presentation of the Floodplain Weed Theory. This is not because the Floodplain Weed Theory fails to consider or incorporate human intentionality, but rather because the clear and central causal role it assigns to intentional human action in plant domestication could have been better emphasized and highlighted in the 1987 publication. The Floodplain Weed Theory has been presented numerous times over the past eight years, in various places and in varying levels of detail, and in regard to the morphological changes associated with domestication of eastern seed plants, the proposed explanation has consistently been the same. Contrary to the characterization presented by Wylie (Wylie 1992:22), these morphological changes were explained as resulting from a specific, deliberate, premeditated, intentional human activity–the planting of stored seed stock.

The archaeological evidence for plant domestication in eastern North America consists of morphological changes in the reproductive propagules (seeds) of squash/gourd (*Cucurbita pepo*), marshelder (*Iva annua*), chenopod (*Chenopodium berlandieri*), and sunflower (*Helianthus annuus*). These morphological changes–an increase in seed size in *C. pepo*, marshelder, and sunflower, and a reduction in seed coat (testa) thickness in chenopod, serve to differentiate domesticated taxa of these species from wild plants.

Since domestication of eastern North American seed plants is defined in terms of these morphological changes, finding their cause is central to providing an interpretive framework for explaining domestication. The cause is intentional and sustained planting. It is only with deliberate storage of seed stock and the intentional planting of seeds in prepared seed beds, activities clearly charged with human innovation and intentionality, that a new set of human rather than natural selective pressures are brought to bear, leading to intense seedling competition and strong selection for plants that will sprout quickly (reduced germination dormancy, sometimes reflected in thinner seed coats) and grow quickly (increased food reserves, reflected by larger seed size), thereby shading out their neighbors and contributing more to the fall harvest seed stock for next year's planting.

Thus, in the Floodplain Weed Theory a cause-and-effect relationship with human intentionality playing the central role in plant domestication is explicitly spelled out, if unfortunately not given adequate emphasis in the 1987 presentation of the theory. Planting, a substantial intervention in the life cycle of the plants involved, directly establishes a new set of selective pressures which result in the specific morphological changes recognized as defining the initial domestication of eastern seed plants. This cause-and-effect relationship between deliberate planting and the specific morphological changes under consideration is not restricted to eastern North America or to the few plants under discussion. Rather, eastern North America provides one case study of a general framework of explanation for a wide variety of morphological changes in seed plants associated with the adaptive syndrome of domestication. This general explanatory framework was first outlined by Jack Harlan and his associates almost 20 years ago (Harlan *et al.* 1973). The research by Harlan and his co-workers on the morphological consequences of various intentional, manipulative kinds of human intervention in the life cycle of plants has been widely accepted and broadly and successfully applied in many different regions of the world. This rich and widely recognized body of research provides a solid basis for the central "planting produces morphological change" component of the Floodplain Weed Theory.

In the 1987 presentation of the theory, this critically important and significant step of deliberate planting was placed at the end of a logical sequence of escalating levels of human encouragement and intervention in the life cycle of plants within disturbed soil contexts of domestilocalities, from initial benign neglect or tolerance, up through active encouragement, to eventual planting. Watson and Kennedy accept and adopt these human activities leading up to eventual planting in building their case for women's having a major role in plant domestication, and in this regard adopt major elements of the Floodplain Weed Theory. They take issue, however, and rightly so, with my characterization of the final step in the process, planting, as being just another small and simple step in the overall developmental process. I agree with their proposed change in emphasis at this point. The deliberate and sustained planting of stored seed stock and the associated necessary context of conceptual, social, and economic innovation represents a major cultural landmark in human history. This explicit and important modification in emphasis in the Floodplain Weed Theory brings it into close agreement with the position advocated by Watson and Kennedy.

I disagree, however, with Watson and Kennedy's suggestion that more direct human action, over and above deliberate planting of stored seed stock, was necessarily involved in causing the morphological changes associated with domestication (Watson and Kennedy 1991:267). Watson and Kennedy appear to object to planting alone as the cause of such changes, since the changes themselves would not have been deliberately, knowingly selected for by the women domesticators. While the domesticators can be seen as producing the changes through deliberate planting, the Floodplain Weed Theory does not require their active and sustained scrutiny and selection of desired attributes.

In contrast, Watson and Kennedy suggest that domestication would have required "special, self conscious, and deliberate treatment . . . to cause the very significant and progressive changes in seed size that at least two of them (sumpweed [marshelder] and sunflower) exhibit" (Watson and Kennedy 1991:267). While Watson and Kennedy discuss sunflower and marshelder, they do not mention *C. berlandieri*. In this small-seeded species evidence of domestication takes the form of a reduction in seed coat thickness of perhaps 10 microns–measurable with a scanning electron microscope, discernible with a light microscope, but beyond observation by the naked eye. As a result, it would be difficult to make a case for directed, self-conscious scrutiny and selection as causing the change. What then produced this reduction in seed coat thickness? The most reasonable answer, and one backed by robust and widely accepted theory, would be that it represents the plant's adaptive response to deliberate planting and the selective pressures of seed bed competition. Thus, while deliberate planting can account for this morphological change, any more direct scrutiny and selection cannot.

Extending this comparison of the "deliberate planting" versus "special visual scrutiny and selection" explanations to marshelder and sunflower, the same standard applies–are the changes in seed size that mark initial domestication large enough to consider up-close visual selection as a possible factor? The available evidence does not support such a position. The temporal curve of size change in marshelder, for example, shows a dramatic rise after about A.D. 200 (Smith 1987, 1991, 1992b), perhaps indicating direct selection of seed stock. But from initial evidence of domestication at 4000 B.P. up to A.D. 200, the marshelder achene size curve stays flat, just .7 mm above the wild population baseline. This 2200 year long flat part of the *Iva annua* achene size curve, along with the small increase in achene size of marshelder on initial domestication, argues against early visual scrutiny and seed stock selection, particularly when compared to the later post-A.D. 200 dramatic size increase that occurs under possible direct selection. Sunflower in contrast appears to exhibit a large increase in seed size from wild to domesticated at 2800 B.P. But this has long been considered as likely indicative of the simple absence of the beginning of the transition to domestication (and smaller size increases) prior to 3000 B.P. The recent documentation of pre-4000 B.P. domesticated sunflower in Tennessee provides support for this position (Crites 1991).

In summary, I conclude that the available archaeobotanical evidence directly contradicts the "direct scrutiny and selection" position in the case of *C. berlandieri*, while the small initial increase in achene size and flat curve for *Iva annua* argues against it, and sunflower provides no support. At the same time, deliberate planting can account for size changes in all three domesticates, as well as in *C. pepo*, while deriving strong support from Harlan's general interpretive framework. It is possible, however, that deliberate and direct human selection of other attributes of these four plants did occur during the process of domestication. If deliberate and direct human selection was operating during domestication, however, it was focused on morphological attributes that are not recognized at present in the archaeobotanical record of eastern North America.

Recent research also calls into question Watson and Kennedy's suggestion that the "natural history, natural habitat and distribution, and ecology" of the floodplain weed progenitors of eastern domesticates is not well understood, and that closer inspection might perhaps show them to have "required special, self conscious, and deliberate treatment to convert them to garden crops" (Watson and Kennedy 1991:267). As a result of considerable field research carried out in the river valleys of the East (much of which was either not yet reported or not yet undertaken when the Gender-Credit Critique was first developed), the natural history, natural habitats and distribution, and ecology of marshelder, wild *Cucurbita* gourd, and *C. berlandieri* is now well understood. This research firmly documents their floodplain weed niche and their resultant propensity for invading, uninvited, into agricultural fields and other anthropogenic settings (Smith 1987:30; see chapters 2, 4, 7, 8 in Smith 1992a). These studies provide solid support for another core element of the Floodplain Weed Theory–that three of the four progenitor species in question (*C. pepo*, *C. berlandieri*, and *I. annua*) were excellent candidates for domestication because as floodplain weeds their specific and successful long-term adaptation to disturbed-soil, open-space habitats in river valleys preadapted them for transformation into garden crops (see Smith 1987:29-31). While the "planting causes thinner seed coats and larger seeds" element of the Floodplain Weed Theory can be traced to the research of Jack Harlan, the "floodplain weeds are pre-adapted for transformation into garden crops" component is solidly tethered to the research and writings of Edgar Anderson, Jonathan Sauer, and other botanists in the late 1940s and early 1950s.

The arboretum of the Missouri Botanical Garden where Anderson was on the staff bordered on the Meramac River valley, and by the late 1940s he had published on the geomorphology of flood episodes along the river. By the early 1950s his interests had expanded to include Ozark floodplain weeds, including winter cress, a kind of wild mustard, as well as the sycamore, and the propensity of such "weeds" to invade anthropogenic open habitats (Smith 1992a:22-25). He also stimulated and guided Jonathan Sauer's classic study of Meramac River valley pokeweed populations. Sauer's study provided impressive documentation for Anderson's key proposal–that because floodplain weeds are adapted to colonizing open-habitat, disturbed-soil situations newly cleared by river flood episodes, they will also colonize similar disturbed soil habitat settings created by humans, and thus would be excellent candidates for transformation into garden crops.

Ironically, while these field case studies of Ozark floodplain weeds clearly provided the stimulus for Anderson's general Dump Heap Theory of plant domestication, he never attempted to apply his briefly presented theory specifically to eastern North America, nor did he directly consider any of the three floodplain weed progenitors of the East (see Smith 1992a: Chapter 2).

In some interesting respects it is possible today to observe the extent to which these floodplain weeds were pre-adapted to become garden crops, simply by walking the floodplain landscapes of the Lower Mississippi River in the fall of the year. Wild *Cucurbita* gourds, *C. berlandieri*, and marshelder thrive today in their very distinct and predictable natural floodplain habitats. Additionally, in the absence of a determined application of modern herbicides, these three floodplain weeds can also be found growing in large stands in agricultural fields, where they have overpowered the cash crops planted the previous spring. Farmers in such situations are confronted with bumper harvests of uninvited ancient crop plants rather than the soybeans or cotton they had hoped for.

In summary, I don't think that a very strong case can be made for these floodplain weeds' being difficult to transform into garden crops. Nor do I think their pre-adapted profiles detract in any way from the level of importance that should be assigned to the process of deliberate and sustained planting of seed stock that directly caused the initial domestication of seed plants in eastern North America. Similarly, I do not think that the long temporal gap that separates the initial domestication of seed plants in the East from the subsequent emergence of a substantial reliance on food production detracts in any way from the significance and importance of the process of domestication.

THE INITIAL ECONOMIC IMPORTANCE OF INDIGENOUS DOMESTICATES

A final area of disagreement between the Floodplain Weed Theory and the Gender-Credit Critique centers on the relative initial economic importance of these newly domesticated plants.

As outlined in the Floodplain Weed Theory (Smith 1987a, 1991, 1992a, 1992b), the archaeobotanical evidence available in eastern North America strongly supports the existence of a substantial lag period between the initial domestication of plants and the subsequent initial appearance of apparent farming economies in the region. The evidence cited in the Floodplain Weed Theory for the initial emergence of farming economies in the East is drawn from the detailed and long temporal span archaeobotanical sequences available for West-Central Illinois, the American Bottom, and eastern and central Tennessee (Smith 1991, 1992a, 1992b), and consists of clear and dramatic increases in the representation of seeds of domesticated and cultivated plants at about 250 B.C. to A.D. 1. This increase in the representation of garden and field crops appears to represent a broad geographical pattern of increasing dependence on food production. The substantial representation of quasi-cultigens, cultigens, and domesticates in the vestibule deposits of Salts Cave that may predate this broad geographical pattern of intensified food production, perhaps by a century or so, was also taken into account, and the period from 2500 to 2000 B.P. was identified as bracketing the transition:

> It is not until 2500 to 2000 B.P. that these indigenous domesticates, along with a whole host of other quasi-cultigens and cultigens, became economically important and archaeologically abundant (Smith 1987:37).

Watson and Kennedy argue that this placement of the emergence of farming economies at 2500-2000 B.P. and the associated temporal lag between initial domestication and a substantial economic role for domesticates diminishes the significance of initial domestication and the associated role of women. An effort is made to narrow this temporal gap that separates initial domestication from the subsequent significant dietary shift to food production. On the early end of the temporal gap the leading edge of plant domestication is shifted forward in time from 4000 to 3500 B.P., in what would appear to be direct contradiction of extant evidence. Interestingly, while the Gender-Credit Critique pushes the temporal context of initial plant domestication forward in time by 500 years, recent empirical information–early domesticated sunflower from Tennessee–appears, in contrast, to push it back in time, perhaps by an equal span of five centuries (Crites 1991). Thus, at the early end of the temporal gap an expanding base of empirical evidence stands in the way of any efforts to ratchet plant domestication forward in time, closer to the evidence for increased reliance on food production.

On the near side of the temporal gap, Watson and Kennedy propose pushing the evidence for increased reliance on domesticated plants from 2500-2000 B.P. to 2800-2500 B.P., narrowing the temporal span at most by only three centuries. Empirical evidence offered in support of an increased reliance on crop plants prior to 2500 B.P. consists of both paleofecal and flotation-derived remains from Salts Cave and Mammoth Cave:

> The best and most comprehensive dietary evidence for the early horticultural period comes from the long series of human paleofecal and flotation derived remains in Salts Cave and Mammoth Cave, west-central Kentucky (Watson and Kennedy 1991:267).

The archaeobotanical evidence from Salts and Mammoth caves, however, does not appear to provide much support for the suggestion that the transition to an increased reliance on crop plants can be pushed back prior to 2500 B.P. Paul Gardner's recent impressive and detailed consideration of the archaeobotanical sequence of the square KII vestibule deposits at Salts Cave reaffirm Richard Yarnell's earlier analysis of square JIV; both excavation units bracket a clear shift to greater dependence on cultivated and domesticated plants, with this shift occurring in levels 5 and 6 (Yarnell 1974, Figure 16.1; Gardner 1987:362; see also Watson 1974:237-238). In the archaeobotanical sequences of both excavation units seeds of cultivated and domesticated plants are present in low frequencies until level 6, when they substantially increase in abundance. In square KII, level 6 is dated to 250 B.C. ± 60, placing the shift to greater reliance on these plants in close agreement with the broad regional pattern of increased food production documented in eastern and central Tennessee, west-central Illinois, and the American Bottom. Underlying level 6 in unit KII (which yielded 5307 seeds of cultigens and domesticates), levels 11 and 14 in the same square produced dates of 430 and 460 B.C. ± 60, respectively, along with a total of only 194 seeds of possible domesticated and cultivated crop plants (Gardner 1987:362). A somewhat earlier date of 390 B.C. ± 150 for the level 6 increase was obtained from the nearby JIV excavation unit analyzed earlier by Yarnell (Gardner 1987:359). This clear and compelling evidence from the Salts Cave vestibule archaeobotanical sequences for a shift to increased reliance on crop plants and an associated pattern of possible land clearance (Watson 1974:235-238) by 250 B.C. does not, however, enter into Watson and Kennedy's analysis.

Watson and Kennedy's argument for an earlier strong reliance on domesticates rests not on the well stratified and meticulously analyzed vestibule deposits, which provide little support for their position, but on the more than 100 paleofecal samples collected from a wide variety of locations throughout the caves:

> The fecal evidence dates to 2800-2500 B.P. and is quite clear and consistent. Over 60 per cent of the plant foods consumed were seeds of indigenous domesticates and cultigens: sunflower, sumpweed [marshelder] and chenopod . . . (Watson and Kennedy 1991:267).

I disagree with this characterization of the temporal context of the paleofecal data base. The human fecal material collected and analyzed from Salts and

Mammoth Caves represents over a hundred individual depositional episodes made by an unknown number of Native American miners and other explorers over a long period of time and along miles of passageways (Watson 1974:235). As a result, I would argue that it is not possible to be definite regarding the temporal assignment of any particular fecal sample without dating it directly. Only five of these paleofecal samples have been dated, and of those only two predate 2500 B.P. The five dates on paleofecal samples from Salts Cave are: 2660 ± 140 B.P., 2570 ± 140 B.P., 2350 ± 140 B.P., 2270 ± 140 B.P., 2240 ± 140 B.P. (Yarnell 1969:50-51). The blanket statement that the fecal evidence dates to 2800-2500 B.P. would thus appear doubly open to question. First, since only 5 of 100 have been dated, the definite temporal context of 95% of the paleofecal specimens remains unknown. Secondly, since only two of the five dated samples fall into the 2800-2500 B.P. time frame, there is also extant contradictory evidence to the generalization offered in the Gender-Credit Critique. Finally, to reach 2800 B.P., the older of the two samples is pushed back in time to the early end of its first standard deviation. Pushing it one standard deviation in the other temporal direction brings it up to 2520 B.P., only 20 years outside of the transition period specified in the Floodplain Weed Theory.

In summary, the Gender-Credit Critique's case for the strong dietary importance of domesticated and cultivated plants in the eastern United States prior to 2500 B.P. consists of two paleofecal samples dating to 2660 and 2570 B.P. ± 140, both containing substantial amounts of cultigen seeds. The older of these was deposited in the Blue Arrow passage, about a mile from the entrance to the cave, while the more recent one was encountered about 1200 feet from daylight (Watson 1974:236). Interestingly, because of the 140 year standard deviations associated with both of these dates, neither specimen is statistically very far removed from 2500 B.P.

As corroborating evidence for these two paleofecal samples, Watson and Kennedy cite reports on three eastern Kentucky rockshelters--Newt Kash, Cloudsplitter, and Cold Oak. While these shelters all provide clear and important evidence of the utilization of indigenous domesticates prior to 2500 B.P., the plants in question are represented only in small amounts in pre-2500 B.P. contexts at the three sites. They thus offer no support for the proposition that domesticated plants were a major dietary component prior to 2500-2000 B.P.

Based on these two paleofecal samples and reference to Newt Kash, Cloudsplitter, and Cold Oak, Watson and Kennedy draw the following conclusion:

> This single well-established datum for a period relatively early in the history of the indigenous domesticates *might* be taken to cast *some* doubt on the generalization that the addition of the domesticate species had only a slight dietary impact (Watson and Kennedy 1991:267)(emphasis added).

In summary, I suggest that the argument in support of premaize crops contributing a sizable portion of annual diets prior to 2500 B.P. is very weak. The empirical evidence employed is limited, localized, may not be representative of larger social groups, and is not far removed from 2500 B.P.

Even if these two paleofecal samples are accepted as evidence of a broad scale and substantial dietary role for premaize crop plants as early as 2800 B.P., however, and even if the earliest indication of plant domestication dates to 3500 B.P. rather than 4000 B.P., a gap of 700 years still separates initial domestication from a substantial dietary role for premaize crop plants.

If the generally acknowledged date of 4000 B.P. is substituted for 3500 B.P. as marking the first morphological changes associated with domestication, this gap quickly widens from seven to twelve centuries. If the oldest Salts Cave paleofecal sample is pushed from the far to the near end of its first standard deviation, this temporal gap then easily expands to fifteen centuries. Finally, if the deep cave paleofeces are dropped in favor of the 250 B.C. transition to increased reliance on premaize crops recorded in the vestibule deposits at Salts Cave as well as in other regional archaeobotanical sequences across the interior mid-latitudes, and the new sunflower dates from Tennessee are factored in, the temporal gap widens again, this time to two millennia or more.

In my opinion the archaeobotanical information available today provides strong empirical support for a temporal separation of at least 1500 years and more likely 2000 years between the leading edge of domestication in the East and a subsequent marked increase in dietary dependence on food production.

I argue that the length of this gap between initial domestication and dietary dependence does not diminish either the developmental importance of plant domestication or the role of women as innovators. It makes little difference if the full significance of the accomplishment is delayed in realization. Whether or not these crops made a major caloric contribution almost immediately or only after a lag of 2000 years seems less relevant when the present-day scale of world-wide cultivation and consumption of summer squash and sunflower, both eastern North America domesticates, is used as an appropriate point of reference.

Of much greater importance, I argue, is the extent to which, in addressing the early role of domesticated crops, Watson and Kennedy draw attention to a number of interesting issues and research questions that reside, largely unaddressed, in this 2000 year long period from about 2250 to 250 B.C. This period, which spans the Late Archaic to Early Woodland florescence in eastern North America, encompasses a rich variety of dramatic changes in technology and material culture, community and settlement patterns, and regional exchange, along with innovations in food procurement, processing and storage. What role did domesticates play within this broader context of increasing social and cultural elaboration? Given their low representation in archaeobotanical assemblages, premaize crops did not provide a major caloric contribution to the diet of Late Archaic populations in the region. But the economic and social significance of these crops should not be viewed narrowly in terms of their total caloric contribution or their relative abundance in fecal samples. It would be a mistake to relegate them to the shadows and characterize them as simply "domesticates in the presence of hunter-gatherers" because they have low archaeological visibility.

At the present time the consensus Late Archaic–Early Woodland role that has been cast for premaize crop plants is that of storable food sources that could well have been of critical importance during the late winter and early spring lean season (Cowan 1984). It seems clear at this point, however, that any better understanding of the role of premaize cultigens in the Late Archaic will come not from analysis of archaeobotanical assemblages, but from careful consideration of their archaeological context of storage, preparation, and use. Any such consideration will in turn engender a rich variety of gender-related research questions. Do the contexts of Late Archaic–Early Woodland storage and preparation suggest seasonal as opposed to longer term storage? Can we assume that storage of both seed stock and food reserves was the responsibility of women? What concepts of proprietary claim applied to garden plots and their annual harvest? Was the seed stock of crop plants from individual garden plots the property of individual women? What social mechanisms might have constrained an expanded, more important early role for these crop plants? These and other interesting issues of gender associated with early plant cultivation in the East come into clearer focus as a result of Watson and Kennedy's gender-specific perspective on the Late Archaic–Early Woodland gap between initial plant domestication and the appearance of farming economies.

This 2000-year-long period of separation also serves to highlight the major role that human intentionality played in the Early to Middle Woodland period initial emergence of farming economies in eastern North America. The substantial temporal separation between plant domestication and the initial development of farming economies underscores the human intentionality inherent in the origin of farming in the region. The clear conclusion drawn from this temporal separation is that farming does not automatically follow from plant domestication. The presence of crop plants does not immediately and invariably lead to agriculture. While domesticates are obviously a prerequisite to agriculture, humans and human intentionality rather than a domesticate-driven destiny plays the critical central role in the transition to farming.

DISCUSSION AND CONCLUSIONS

In the preceding sections of this paper I have addressed the four main areas of apparent disagreement between the Floodplain Weed Theory of Plant Domestication and the Gender-Credit Critique. In each of these four areas the apparent differences separating the two perspectives on plant domestication in the East are considered and found, at least in my opinion, to be largely open to resolution through either clarification or relatively minor modification of one or both of the interpretive positions.

It is suggested that based on recent research results the Gender-Credit Critique's position on early eastern gourds is untenable. If this position were changed, from introduced domesticate to wild indigenous, the Gender-Credit Critique would come into close agreement with the Floodplain Weed Theory and an internal logical dilemma would be resolved.

The gender-neutral Floodplain Weed Theory in turn has been modified through an interpretive extension to accommodate and acknowledge the likely role played by women in plant domestication in eastern North America. In addition, a number of questions are raised regarding the advisability and application of the Wylie, Watson and Kennedy "no neutral zone rule" in archaeological inference, and it is suggested that political rhetoric obscures the very important main thrust of Watson and Kennedy's new perspective–that gender specific approaches can open up new areas of research inquiry.

In addition, in response to Watson and Kennedy's critique the central role assigned in the Floodplain Weed Theory to human intentionality in the form of deliberate and sustained planting of stored seed stock is clarified and given appropriate causal emphasis. This brings the Floodplain Weed Theory into closer agreement with Watson and Kennedy.

At the same time it is also suggested that two positions taken by Watson and Kennedy regarding human intentionality have little empirical support–the suggestion that direct visual scrutiny and selection played a role in producing the morphological changes associated with domestication and the proposition that as-yet-undocumented barriers to domestication may have made it difficult to transform floodplain weeds into garden crops. These two areas of disagreement between the Floodplain Weed Theory and the Gender-Credit Critique remain the major and most interesting sources of interpretive tension between the two perspectives. Interestingly, when considered in terms of Wylie's "local context" criteria, the Floodplain Weed Theory seems solidly supported by both substantial empirical data and robust theory, while the Gender-Credit Critique, in contrast, looks to possible future developments as providing supporting arguments.

Finally, it was suggested that efforts to reduce the temporal gap that separates the leading edge of domestication from the initial emergence of a significant role for food production in eastern North America had little likelihood of success, given the currently available archaeological and archaeobotanical data base. Watson and Kennedy's attention to the role of crops in this long lag period was also recognized as illuminating the potential rich variety of interesting research problems involving issues of gender and early domesticates that remain to be addressed. Their attention to this lag period also served to highlight the central role of human intentionality in the initial emergence of farming economies in eastern North America. This initial shift to greater importance for seed crops in the economy of Middle Woodland populations marks an important and widespread social transformation in eastern North America. It was a transformation, I should add, in which women likely played a major role.

ACKNOWLEDGEMENTS

I would like to thank C. Wesley Cowan, Chris Gremillion, James B. Griffin, James Stoltman, Richard Yarnell, and Melinda Zeder, who read and commented on early versions of this chapter.

REFERENCES

Cowan, C. Wesley
1984 From Foraging to Incipient Food Production: Subsistence Change and Continuity on the Cumberland Plateau of Eastern Kentucky. Ph.D. dissertation, Department of Anthropology, University of Michigan, Ann Arbor.

Cowan, C.W., and Bruce D. Smith
1991 New Perspectives on a Free-Living Gourd in Eastern North America. Paper presented at the 14th Annual Conference of the Society of Ethnobiology, St. Louis, Missouri.
1992 Habits and Historical Records of Wild *Cucurbita* Gourds in Eastern North America. Paper presented at the 15th Annual Conference of the Society of Ethnobiology, Washington, D.C.

Crites, Gary
1991 Indigenous Domesticated Plants and Independent Origins of Food Production: New Evidence from Tennessee. Paper presented at the 48th Annual Meeting of the Southeastern Archaeological Conference, November 6-9, Jackson, Mississippi.

Decker-Walters, Deena, Terrance Walters, C. Wesley Cowan, and Bruce D. Smith
1992 Isozymic Characterization of Wild Populations of *Cucurbita pepo*. Paper presented at the 15th Annual Meeting of the Society of Ethnobiology, Washington, D.C.

Gardner, Paul
1987 Plant Food Subsistence at Salts Cave, Kentucky: New Evidence. *American Antiquity* 52:358-364.

Harlan, Jack, J.M.J. de Wet, and E.G. Price
1973 Comparative Evolution of Cereals. *Evolution* 27:311-325.

Newsom, Lee, S. David Webb, and James S. Dunbar
1992 Early *Cucurbita pepo* from a Florida Wet Site. Paper presented at the 15th Annual Meeting of the Society of Ethnobiology, Washington, D.C.

Smith, Bruce D.
1987 The Independent Domestication of Indigenous Seed-Bearing Plants in Eastern North America. In *Emergent Horticultural Economies of The Eastern Woodlands*, edited by William Keegan. Southern Illinois University at Carbondale Center for Archaeological Investigations, 3-48. Occasional Paper No. 7. Carbondale.
1989 Origins of Agriculture in Eastern North America. *Science* 246:1566-1571.
1991 The Development of Food Producing Economies in Eastern North America. Paper presented at the Annual Meeting of the Society for American Archaeology, New Orleans.
1992a (editor) *Rivers of Change*. Smithsonian Institution Press.
1992b Prehistoric Plant Husbandry in Eastern North America. In *The Origins of Agriculture in International Perspective*, edited by C.W. Cowan and Patty Jo Watson. Smithsonian Institution Press, Washington, D.C.

Smith, Bruce D., C. Wesley Cowan, and Michael P. Hoffmann
1992 Is it an Indigene or a Foreigner? In *Rivers of Change*, edited by Bruce D. Smith, Chapter 4. Smithsonian Institution Press, Washington, D.C.

Watson, Patty Jo (editor)
1974 *Archaeology of the Mammoth Cave Area*. Academic Press, New York.

Watson, Patty Jo
1992 Origins of Food Production in Western Asia and Eastern North America. In *Quaternary Landscapes*, edited by Linda Shane and Edward Cushing. University of Minnesota Press, Minneapolis.

Watson, Patty Jo, and Mary Kennedy
1991 The Development of Horticulture in the Eastern Woodlands of North America: Women's Role. In *Engendering Archaeology*, edited by J. Gero and M. Conkey, 255-275. Basil Blackwell, Oxford.

Wilson, Hugh G.
1990 Gene Flow in Squash Species. *Bioscience* 40:449-455.

Wylie, Alison
1992 The Interplay of Evidential Constraints and Political Interests: Recent Archaeological Research on Gender. *American Antiquity* 57:15-36.

10 Lower Ohio Valley Mississippian Revisted: An Autocritique of "The Kincaid System"

Jon Muller
Southern Illinois University
Carbondale, Illinois

INTRODUCTION

"The Kincaid System: Mississippian Settlement in the Environs of a Large Site" was published in Smith's *Mississippian Settlement Patterns* in 1978. In that paper, I gave a summary of work that had begun in the late 1960s. Since 1978, our project has done much additional survey and excavation in the region. This newer work has been described in my *Archaeology of the Lower Ohio River Valley* (1986) and elsewhere. The work reported in the 1978 paper was the result of the combined efforts of many, especially Blakeman (1974), Butler (1977), Lafferty (1973, 1977), and Riordan (1975). Since 1978, many others have contributed directly or indirectly to the project, including Ahler *et al.* (1980), Avery (1983 and others), Butler *et al.* (1979), Canouts *et al.* (1983), Cobb (1988), Davy (1982), Martin (1991), Penny (1983), Rudolph (1981), and Santeford (1982). Indeed, in one way or another, most of the work done in the region by the Center for Archaeological Investigations of Southern Illinois University at Carbondale is relevant to the discussion here.

The goal of work in the Kincaid locality was to test perceived "truths" about Mississippian in this region (e.g., Cole *et al.* 1951). It was not so much that my colleagues and I thought these views were completely wrong. Rather, we felt that many aspects of the traditional models of Mississippian politics and economy were reasonable but were not founded on solid evidence. I am by nature and nurture a skeptic, and Stephen Williams's teaching at Harvard had further educated me to a reluctance to accept ideas uncritically. Some of his published reviews had illustrated how important it was not simply to accept received interpretations uncritically, however reasonable they might appear.

Now, as our work in the Lower Ohio Valley passes its 25th year, I have seen my own 1978 paper cited uncritically and treated as a kind of "received truth." For that reason, I offer an autocritique reassessing the approaches and conclusions from our work as summarized by me in 1978. I shall briefly discuss each of the sections of the 1978 paper and then criticize the assumptions and conclusions considering more recent experience.

WHAT IS THE "KINCAID SYSTEM"?

Kincaid is a large mound center located in the Black Bottom of the Ohio River (37° N, 89° W) near present-day Metropolis, Illinois (Figure 10.1). It is one of the larger Mississippian sites in area (compare in Morgan 1980) and had over 90,000 m^3 of mound construction in the years between A.D. 900 and 1400. The central site is surrounded by smaller Mississippian farmsteads and hamlets in a fairly linear distribution along the river valley. Larger sites with a mound or two occur at various locations in the Valley, but the only mounds in the Black Bottom are at Kincaid itself.

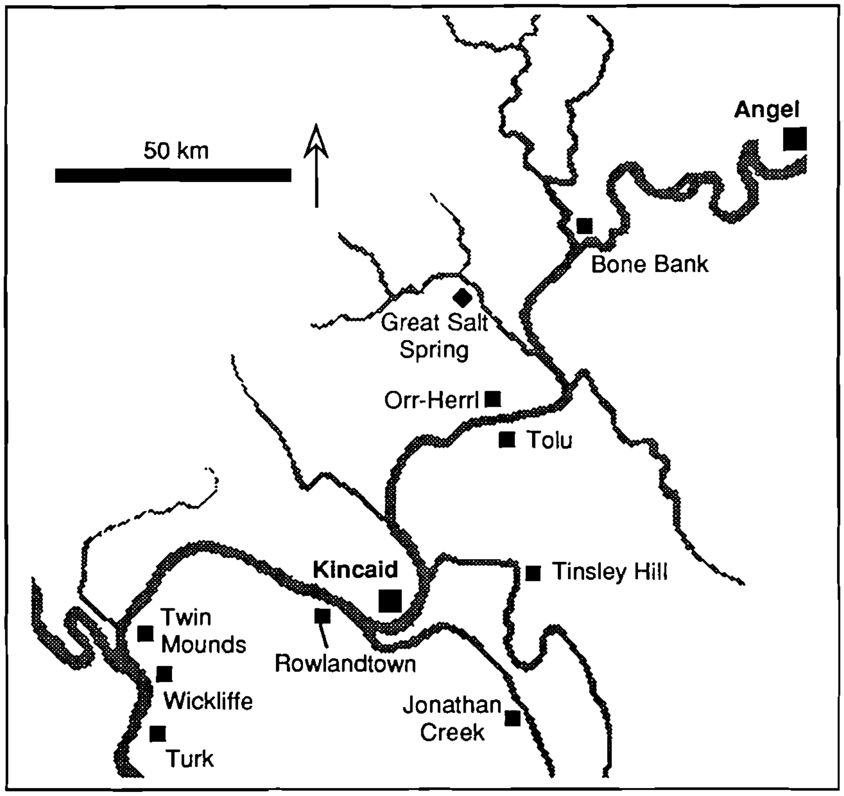


Figure 10.1. Lower Ohio Valley with major Mississippian archaeological sites.

By 1978, we had fair control of the environmental data on the Lower Ohio Valley. Kincaid's environment is and was southern in character, with cypress and tupelo swamps. Kincaid was on rich agricultural land and near a broad range of natural resources.

As survey and other work extended into areas outside the Black Bottom, our data on Mississippian environments in the Valley have largely confirmed what we thought we knew in 1978. The Lower Ohio Valley is a marginal environment in southeastern terms, with greater risk of frost and shorter growing seasons than faced by many late prehistoric peoples. On the other hand it is less marginal in the same terms than are some "Mississippian" societies to the north in the American Bottom and the Illinois Valley. However, after our 25 years of research on the Kincaid and Angel societies, I now choose to emphasize the marginality of the environment at the northern limits of the Gulf Coastal Plain, as discussed below, in terms of the implications of possible horticultural practices.

We have recently presented revisionist interpretations of Mississippian in terms of environmental limits (Muller and Stephens 1991). The question is whether some complexes that are commonly called Mississippian might better be thought of in other ways. I feel that a so-called core Mississippian (*sensu strictu*) is clearly related to environmental conditions, but there can be no doubt that flexibility in responses, even within Ohio drainage Mississippian, includes settlement in such diverse environments as those occupied by the Tennessee-Cumberland Mississippian societies. Tennessee-Cumberland Mississippian societies are closely related in both organization and form to Kincaid and Angel, but are embedded in strikingly different environments. For example, the Mound Bottom and Pack sites (e.g., Morgan 1980:108-109; O'Brien 1977) in Tennessee are similar to Kincaid in structure, but their local environment is not very much like that of the Black Bottom.

Even within the Lower Ohio Valley, there are exceptions to the fairly uniform picture generated by focus on the dominant (and, to be sure, the most common) bottomland sites. Blufftop and small stream valley farmstead sites exist in the Cache and its tributaries (e.g., Canouts *et al.* 1983). In upland contexts, sites such as the Bridges site (e.g., Hargrave *et al.* 1983) connect in many ways to Mississippian "core areas." Even in the strictest definition, "Mississippian" may show much more flexibility in economic and environmental organization than allowed for in my earlier discussions. In the end, I did not allow enough for human flexibility in retaining basic organizational unity under diverse conditions.

RESEARCH ORIENTATION

The original project goals were to assess the evidence for a series of views about Mississippian societies that had been widely held. Many of these perspectives involved the relative dependency of "hinterland" Mississippian in relation to the "core area" of Cahokia. Smith (1984) gives a nice summary of the theoretical views involved in this perspective (see also Muller 1986:170-173). That these views are not simply a matter of history may be seen in the editor-chosen title of a recent volume, *Cahokia and the Hinterlands* (Emerson and Lewis 1991). What Stephen Williams called the "view from Monks Mound" still has much life in it.

Work in the Black Bottom was also motivated by concern about the conservation of an archaeological resource that was suffering increasing degradation from modern activities. At the beginning of the project, most of the danger was from agricultural exploitation, and the Kincaid site and its surrounding locality had less industrial and urban intrusion than most other large Mississippian sites. We became especially interested in the relative roles of small sites and large sites, since even our earliest surveys showed that small Mississippian sites were underrepresented to a surprising degree in older surveys. One of our important tasks, then, was to obtain a representative sample of Mississippian settlement to understand the nature of the system as a whole.

Even at the time, it was clear that a suggestion that Kincaid was a "militaristic state" (Sears 1968) was based on very loose metaphors from Mesoamerican "Classic" and "Postclassic." Rejecting such claims was, of course, not difficult. In fairness, Sears's statement of the developmental level of Mississippian was outspoken, but not out of character for the time of writing. I would suppose that he had not visited sites, such as Wickliffe, that he described as "militaristic states."

Of the things that the Black Bottom Project did, one of the most important steps was to concentrate on small sites rather than larger ones. This allowed us to proceed much farther toward a balanced view of Mississippian, even when samples were small. The view of Mississippian that was generated out of farmstead-level work in the Black Bottom has subsequently been corroborated in the American Bottom (e.g., Milner 1990 and the extensive bibliography given in that paper) and in other localities (e.g., Kowalewski and Hatch 1991).

However, in correcting for a bias toward large sites in Mississippian archaeology, our work may have come to overemphasize small sites. Lack of attention to the central site, after all, does isolate the project from information about the critical emergence of elites. Our work at small sites has shown little in the way of social differentiation, but it can be argued that this might be expected for small sites in simple hierarchical societies. It would have been premature to go to Kincaid before relative completion of the small-scale site work, but a weakness in the model developed in the Black Bottom is that attention largely to small sites has its own biasing effect. It is too easy to believe in *states* or large-scale chiefdoms if one looks only at the large-scale phenomena; but it is too easy to focus on the *domestic* side of the political economy in the small-scale sites. In the end, you have to look at it all to have it all. Future work in the region simply has to look at the major centers from the new perspectives gained in the smaller sites. I believe that the large sites will look very much like clusters of small homesteads, especially in economic terms, but we shall have to prove this in detail.

SURVEY METHODOLOGY

At the time of survey, nearly the entire arable area of the Black Bottom and much of its hinterland (in the dictionary-defined meaning) was open and cultivated. This allowed relatively simple walkover survey to be highly effective. As survey was extended outside the Kincaid locality, other techniques such as shovel testing were employed (e.g., Butler *et al.* 1979; Ahler *et al.* 1980). As it happened, unusual and fortuitous local conditions in the bottomlands meant that the site areas defined in survey corresponded very closely to actual habitation zones. Alluviation typically covered areas of midden spread around the central inhabited portions of bottomland sites. This phenomenon means that some adjustments must be made in using the habitation/land ratios from the bottomland in comparison to other areas, however. We detected the buried midden zones only because we used stratified random sampling in our tests, with control units excavated outside areas of known surface distribution.

In retrospect, it now seems that supplementary shovel testing in the Black bottom survey would have been helpful in identifying some sites in alluviated areas. On the other hand, the evidence from the Smithland Survey (Butler *et al.* 1979 and Ahler *et al.* 1980) as well as from other surveys suggests that we got a fair representation of the Black Bottom. This was only partly because of the methods employed. The completeness of our survey was grounded in what may be described as the advantage of blind luck. The degree of clearing of the Black Bottom and the lack of development made it much easier for us than for archaeologists working in other localities. We appreciated how fortunate we were and missed these conditions when we extended our survey into more diverse zones. The Black Bottom survey is exceptionally valuable, precisely because we were lucky enough to survey the locality when everything was fairly easily visible but before development had degraded the resource substantially.

SETTLEMENT PATTERN

General

Our information from the Kincaid locality proper has not altered substantially since 1978. It is hard to improve on near 100% coverage, after all. We now have much better coverage upstream, however; and clearly the known Kincaid and, probably, Angel patterns represent only one kind of Mississippian settlement–that in major floodplains. These floodplain patterns clearly are the major part of the complete Mississippian patterns, of course. Since 1978, we have substantially expanded our regional coverage with surveys that have covered from the mouth of the Wabash to some kilometers below the Black Bottom (Ahler *et al.* 1980; Butler *et al.* 1978; and other work). A complete view of Mississippian in the region will involve still more work at upland stream valley "farmsteads" (e.g., Canouts *et al.* 1983; Lynch and Jefferies in Jefferies and Butler 1982) and even in an apparently unique, perhaps even fortified, blufftop Mississippian settlement away from the major stream valleys. Such sites may provide clues to both temporal shifts in settlement and diversity in political organization within the region.

Dating

The general chronology discussed in my 1978 summary still holds true. The majority of dates from the region still support the tenth- to fourteenth-century time span for the Mississippian settlement of the region. Maximum settlement was in the thirteenth century. Mississippian in the strict sense began as early as the tenth century in the floodplain areas, and Caborn-Welborn complexes north of the Saline confluence continued into early historic times.

One problem in dating complexes in the region is the fair degree of overlap between dates in the period from the ninth to the eleventh centuries for the Terminal Late Woodland (a.k.a "Emergent Mississippian" in some areas) and so-called "Middle Mississippian." There is not the slightest evidence that Mississippian was intrusive into the region from Cahokia or elsewhere. There is a smooth transition from bottomland Late Woodland into Mississippian (e.g., at the Mx109 site) in terms of both ceramics and other artifact classes. At the same time, no one has come up with a very good explanation for the apparent lag in ceramic (and other) changes in upland areas, but such a lag is present. It seems that we may have to accept that some upland populations continued to follow older artifact styles at times when those styles had been supplanted in the main valleys.

As shown in Figure 10.2, the dates for the Black Bottom and the Lower Ohio Valley Mississippian have been very consistent. It seems clear that whatever happened before and after A.D. 1250, there were contemporary Mississippian settlements throughout the locality and region then. The chart of radiocarbon dates indicates the midpoints and the 95% confidence ranges for the dates according to Klein *et al.* 1982. These correction tables are used because the earlier dates in the region do not have C13/C12 ratio corrections. Note that the ranges given are not one standard deviation, but 95% confidence intervals. These ranges, even including what are very likely outliers, with one exception fall at least partly within the A.D. 850 to A.D. 1450 range of "Emergent" to "Mature" Mississippian in the region.

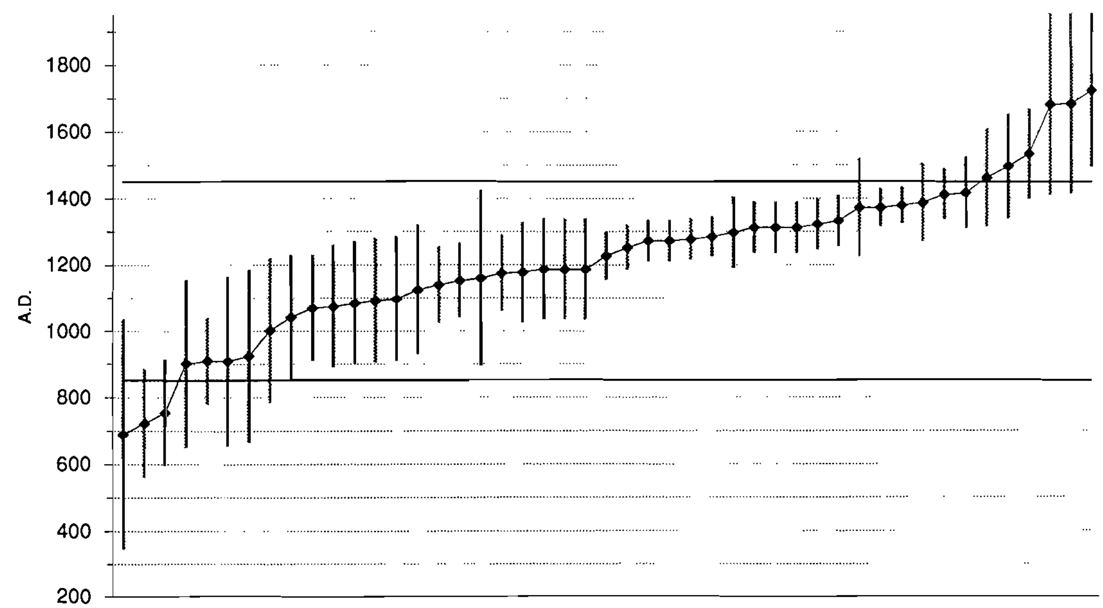


Figure 10.2. Radiocarbon date midpoints and ranges (95% confidence intervals) for the Lower Ohio Valley (corrected per Klein et al. 1982).

The area seems to have been largely abandoned after A.D. 1350 or perhaps a little later. Recent discussions have again raised the question of whether Angel (near present-day Evansville, Indiana) might be somewhat later than Kincaid, perhaps running from the twelfth to mid-fifteenth century (Hilgeman 1991b). While this is not impossible, more data are needed to support the view of persistence of Mississippian settlement in the Angel locality. It is certainly the case that the distinctive shell gorget styles and motifs of the fifteenth century are *not* now known from the Angel area in association with Angel phase sites. Some of these artifacts, such as Nashville shell gorgets and others, are reported to occur in Caborn-Welborn, however (Green and Munson 1978:303), and that complex does appear to fall into the period of circa A.D. 1450 to A.D. 1600 or so. In any case, I have never believed that the "end" of either Kincaid or Angel was a dramatic exodus of 100% of the remaining residents. While the Angel site *may* have had slightly later persistence of occupation and *may* have been founded slightly later, there can be little doubt that occupations at the two sites were largely contemporary. In any case, we seem to be talking about the difference between my earlier estimate of Tinsley Hill ending at circa A.D. 1400 (Muller 1986:185); and a "revised" chronology in which the end of the equivalent phase at Angel is seen as coming around A.D. 1450! We are a little closer to an understanding of these population changes in Mississippian times than we were in 1978, but not much so. An inability to explain the phenomenon and meaning of "abandonment" sometimes makes it tempting for many to deny the "Vacant Quarter" idea proposed by Stephen Williams (e.g., 1977, 1980, 1984). Even so, we know that the region was largely uninhabited at the time of the first intensive European contacts. Protohistoric depopulation may explain some of this, but even the post-A.D. 1400 complexes in the area that are thought *really* to exist (as opposed to being illustrations of the statistical nature of C14 determinations) are indisputably at a smaller scale than those of the thirteenth century.

Our difficulty in developing a fine-grained chronology has been a real disappointment to us and a focus of legitimate criticism. Among other things, we sought out hearths for archaeomagnetic dating for fine-grain comparison of adjacent settlements, but too few of these have proved to survive the increasing depth of plow zone–if they were common in the first place. The ceramic and lithic materials from our small sites are so few and so simple that it is difficult to do more than hope that we shall be able to improve on Riordan's efforts (in 1975; compare Muller 1986 and Butler 1991) in sorting out phases on *ceramic* grounds (as opposed to defining them from radiocarbon-dated assemblages). Richer, larger sites provide higher numbers of chronologically significant markers. Future work will need to look at these, but work suggests that the *percentages* of such goods are essentially uniform across the locality regardless of site size (see Martin 1991). Radiocarbon dating remains our best source of chronological investigation, and improvements in the technique since 1978 will eventually allow the finer-grain analysis that we need to deal with issues of contemporaneity of small and large settlements.

Site Location

Site location data from elsewhere in the Lower Ohio region have extended the range of settlements without negating the summary of conditions for Mississippian bottomland settlement that I described in 1978. Bottomland sites throughout the valley are usually in cane bottoms and on either Armiesburg silty clay loam or on a similar soil known as Huntington silty clay loam (Figure 10.3). Most sites are at about 4 m above normal river pool on ridges with more than 2 to 3 ha of contiguous area. Swampy and wet environments were major attractors for Mississippian settlement, even in upland areas.

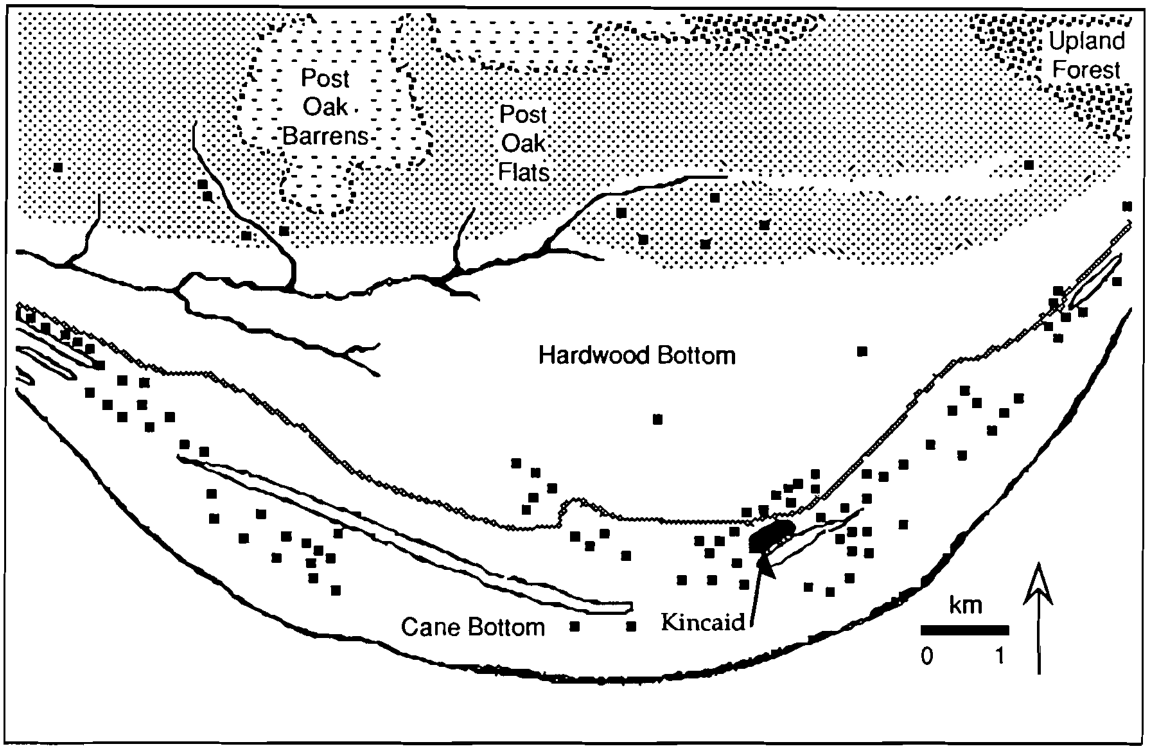


Figure 10.3. Central portion of the Black Bottom indicating Mississippian site locations in relation to vegetation zones.

The variables of soil fertility, drainage and elevation, and sufficiency of support area still hold, as in 1978–but really only for the special circumstances of bottomland Mississippian residences. The sites on terraces, much less those elsewhere, were located in terms of other criteria directly related to the range of activities taking place at the locations. The basic idea that horticultural and wild food resources were primary foci of attention still seems good, and the emphasis of Mississippian on plant foods (wild and domestic) is largely true. However, the evidence from Great Salt Spring raises the possibility that considerable quantities of meat and fat could have come into bottomland sites in forms, as a result of preprocessing elsewhere, that would leave little or no direct trace archaeologically. Davy (1982) programmed a simulation of settlement in the Black Bottom and showed that a small number of persons could easily generate the kind and number of settlements found in our survey. Such results and other evidence still support low absolute population densities for the bottomlands.

Site Hierarchy

In "The Kincaid System," in parallel with many other papers in Smith (1978), I suggested a roughly trimodal classification of smaller site size–(1) possible small, special-purpose sites, (2) 0.3 ha "homesteads," (3) 0.9 to 1.0 ha "nodal" sites. Kincaid itself was unique in the locality, with an area enclosed within palisades of more than 70 ha. The smallest class of sites was postulated at least partly because of the economic model proposed by Service for developing chiefdoms (1975) involving the concept of "redistribution."

The use of a "trimodal" model of Mississippian settlement–the old hamlet–village–town pattern borrowed from Feudal and Mesoamerican models–was an error in the 1978 paper. Although it is possible to categorize the sites into groups based on size, this ignores the essential "household" orientation of Mississippian bottomland settlement. I did get the modality of the farmstead-homestead right, but I failed to appreciate how this negates many of the implications of the "ranked" settlement kind of model.

The concept of "special purpose sites" as suggested in the paper, at any rate, was an error. While there is nothing outrageous in the idea that some Mississippian sites saw more or less of one kind of use or another, the truth is that a rigid typology of site types in Lower Ohio Valley Mississippian is mostly garbage. People live, even when fishing, making salt, or getting chert. How they live, what they consume, and what they produce are situational matters that involve many factors and alter dramatically over time and according to circumstances. Binford has made this point for the Nunamiut (1978 and elsewhere), but it has still not been appreciated sufficiently in archaeological interpretation. Of course, we may identify major, even primary, activities at sites, but it is misleading to assume that they can be typologized into site classes in any simple fashion. Some short-term sites do have important special uses. Wood identified a "hunting camp" (Wood 1968), and our own work at the Great Salt Spring suggests that a major activity at that site was processing the harvest from hunting as well as salt production. What we need to do is to describe the variability in sites in ways that emphasize human activities rather than site "types."

In terms of other kinds of sites, mounds, after all, are rather visible archaeological monuments, so it is not surprising that they were known early and well in terms of distribution. As in 1978, it seems clear that only a few small mound centers such as Rowlandtown, Tolu, or Orr-Herrl can be suggested to have been "secondary centers" in the main valley. The relationship of such sites as Tinsley Hill and Jonathan Creek to Kincaid is a difficult, and unresolved, question. Seen in the broader perspective of low populations and a general domestic economy, the lack of mounds in many areas is probably not so significant as thought in the early years. Construction of so-called "secondary" mounds may be as much linked to fluctuations in the scale of regional political organization as it is an indication of a developed hierarchical structure involving "centers" and "dependencies." It is a mistake to assume that small mound centers are necessarily in "tributary" relationships with larger sites (as also suggested in slightly different terms by Clay 1991 for a "secondary" site near Angel). The Western Kentucky Project of Barry Lewis and his associates has shown how autonomous communities can exist within a region, and these sites provide a contrast to aid in understanding the Lower Ohio Valley (e.g., Kreisa 1988; Lewis 1986).

INTERNAL SITE PATTERNING

As early as 1978, our work had led us to the idea that the small farmsteads or homesteads were the building blocks out of which hamlets, towns, and larger settlements were constructed. One of the major discoveries was that the majority of the population of the Black Bottom lived in these small settlements that were so small that they were apparently not considered worthy of a site form when they were located in the 1930s survey in the locality. At any rate, the original 15′ quadrangle sheets had light pencil markings at a few of the locations where we found homestead sites in our survey. These sites truly are small–typically around 0.3 ha in area–and would have been recorded as larger in unalluviated areas because the actual occupied area of each site is just above the modern annual flood elevation. Thus, alluviation below that elevation has buried refuse and borrow pit areas and made site "edges" appear very exact. It is important to note that this is in comparison to sites from other areas. It is also important to note that we tested low areas at all sites as parts of control statistical strata. In none of the Black or Upper Bottoms test excavations were residential use areas found below the modern annual flood line. Typically, the edge of the site was also defined by the presence of large numbers of irregular borrow pits along the edges of what were probably sloughs with some seasonally standing water. These pits had been filled in with refuse. It is quite clear that these features were not storage facilities at any stage in their use.

We had also recognized by 1978 that, despite the attractiveness of the concept of "activity areas" up to that time, the primary activity at Mississippian sites could only be described as domestic. Site use areas were not easily distinguished from the general round of daily life. As we thought in 1978, the main distinguishable "activities" were those related to rank and ritual. In general, what have been called "activity areas" are the result of patterned, habit-governed behavior, not the designation of "functional areas" within a site. Tasks were completed where it was convenient. Noxious and other activities were located in terms of personal interactions with others and prevailing conditions at the time of the activity. The overwhelming pattern at the sites is for domestic activities to take place around the residence. Over time, a statistical pattern emerged at a site as persons tended to settle into behavioral patterns.

Kincaid, of course, is a large site with complex features. However, as discussed in 1978, it remains the case that it is made up of smaller internal units. These appear, of themselves, indistinguishable from other homesteads and hamlets throughout the Bottom. Intermediate sites such as so-called hamlets often have associated burial areas; but, like the small mound sites, such local expressions can be understood without necessary reference to dominance hierarchies and multiple organizational levels. Burials at domestic sites probably reflect density of settlement (and size of support area) more than social ranking. The presence of what might be called "grave fields" in some areas across the Ohio River from Kincaid remains a mystery; but one that is not likely to be resolved, given the destruction of these graves by looting.

Data from other kinds of sites in the Lower Ohio Valley have not much changed the view that we had of the more or less permanent settlements. These data, however, have demonstrated that a considerable range of activities took place outside the residential context of the bottomlands. The modular character of the homestead is true but is a reflection of a domestic production unit at a small scale, rather than something imposed by a hierarchical structure of domination.

It is possible that our aerial survey data underestimates the numbers of structures at Kincaid for earlier periods. Of course, in 1978 the more common problem was overestimation of the size of central sites. However, there were many fewer contemporary structures than most large-site archaeologists tend to believe. The careful sorting out of overlapping, hence non-contemporary, structures in the Angel or Kincaid excavation maps alone would suggest this. The more recent work in the American Bottom has further shown how large "villages" dissolve into fairly small settlements under tight chronological and stratigraphic controls.

The mound construction estimates given in 1978 were based on solid contractor estimator guide figures for the amount of work possible under these conditions. While some constants might be tweaked somewhat for better allowances for stone hoes as opposed to other kinds of tools, the general scale remains intact. More detailed discussion of these estimates can be found elsewhere (Muller 1986).

Almost all areas of the Black Bottom except for the tops of the mounds are flooded at irregular but certain intervals. Figure 10.4 shows the relative elevations of Kincaid and the Great Salt Spring floodplain area in relation to annual and historical floods. The tops of the highest mounds at Kincaid are as much as 9 m above the normal annual flooding and would have escaped even the massive flooding in 1937. The refugial function of the high mounds is echoed in early 20th century, European-built mounds in the Black Bottom used as foundations for corn cribs and houses.

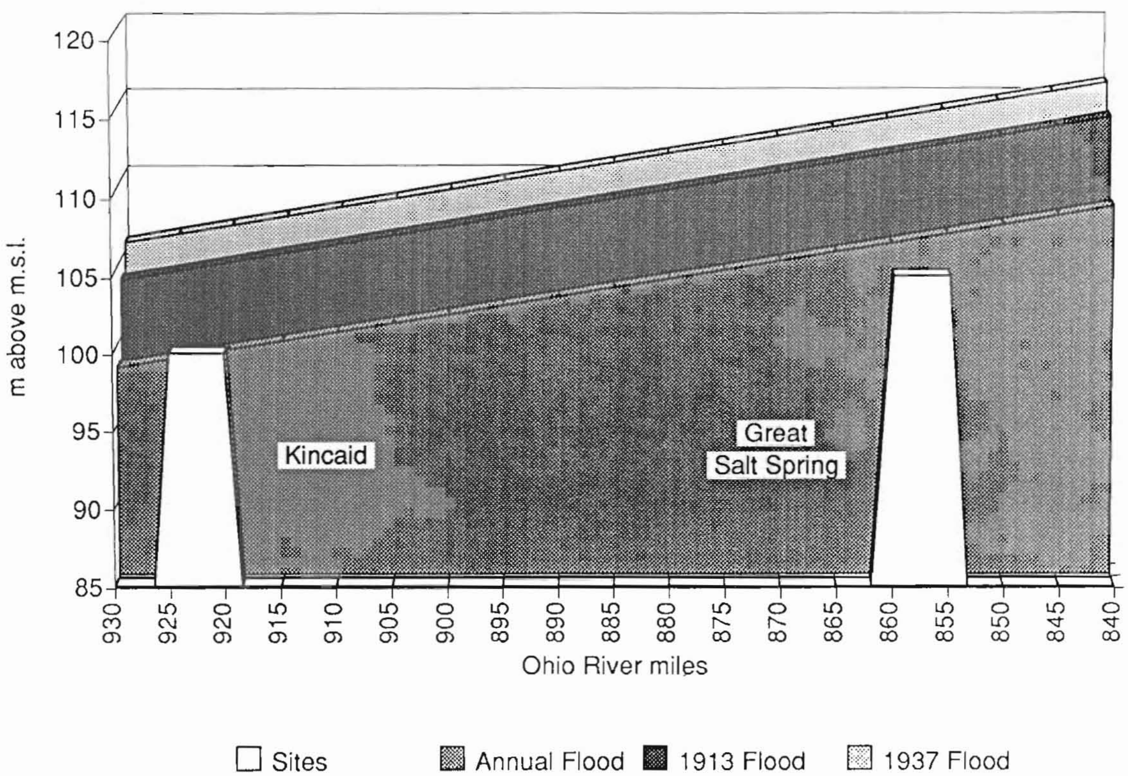


Figure 10.4. Kincaid and Great Salt Spring site locations in relation to flood levels. (Data from Corps of Engineers 1976).

The evidence from upstream surveys (e.g., Butler *et al.* 1978; Ahler *et al.* 1980) suggests that there may have been planting on poorly drained areas as well as on the ridge tops. This extension to the 1978 data on site organization is important in assessing how settlement might have been affected by other factors. Planting on low ground would have been feasible only if the plantings were done after the end of major flood threat in the spring. Peak flooding occurs typically from January to June, with the most common month of peak flooding being March (28%).

In the recorded data from 1937 to 1974, however, substantial numbers of flood peaks occurred in April and May, and some 5% of the peaks occurred in June (Figure 10.5). Ridge-top planting would be relatively secure after March, but lowlying fields

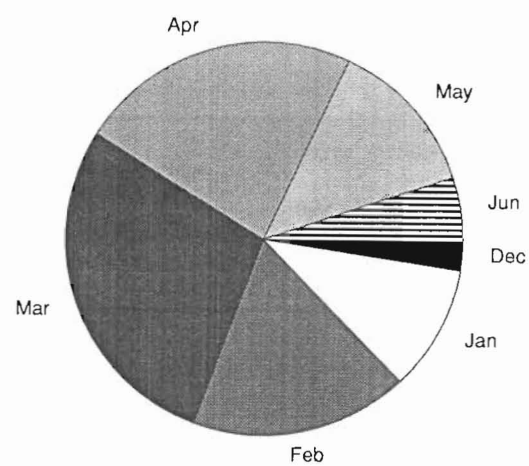


Figure 10.5. Month of peak flooding, 1937-1974. (Source: Corps of Engineers 1976).

would be at considerable risk until late into the planting season. Although 100 to 120 days are required for maturation of various varieties of maize, yields drop off on crops planted after mid May. The success of late-planted, lowlying fields would depend on the length of time between the last spring flood and the first frosts in the fall. This is not so much double cropping, as it is staggered field planting. I suspect that the window of opportunity in the Black Bottom and Angel areas for successfully planting both the tops of the ridges and the lower sides was fairly close to the limits, and that minor reductions in the length of the growing season could have been significant because of increased risk in the second fields. Historical data from Europe for the same period indicate that temperature and crop yields are often related and that even relatively stable economies are subject to perturbation by a congeries of natural causes.

The charts (Figure 10.6) show the number of radiocarbon dates from the Black Bottom compared to the number of Middle European towns founded, estimated North Atlantic temperatures, and some European wheat prices. These are given simply as an illustration of the very widespread problems northern hemisphere agriculturalists had with the climate during the period after A.D. 1250. It is not meant as an argument for climatic determinism in the Black Bottom, but merely as a suggestion that environmental stresses that are roughly contemporary with the "decline" of Kincaid created significant local problems on a widespread basis (e.g., Ladurie 1971, also see similar data on prices of various grains, etc. in Postan 1972 and Braudel 1981). Bryson and Murray (1977) develop an extensive argument for climatic influences in the Mill Creek complex of Iowa. However, one does not need to postulate climate as the main cause of change in order to see that climatic factors such as cooling had important effects between the thirteenth and the eighteenth centuries. Other factors such as the establishment of endemic diseases in communities that had been settled for three or four hundred years might have combined with effects of climate changes to make a bad situation even worse.

SETTLEMENT SYSTEM ANALYSIS

The supposed settlement hierarchy of "three or four types of settlements" doesn't make nearly as much sense today as it seemed to in 1978. The "special purpose sites" have not revealed themselves to intensive investigation except for the Great Salt Spring and some lithics source areas. The known "special purpose sites" are not the small "camps" of less than 0.1 ha that we thought they might be. The many small sites generally are homesteads or larger sites that are more deeply buried under alluvium. We had begun to see that in 1978, but later data definitely support the 1978 rejection of a general idea of "specialized extractive" sites in the bottomland. Such "specialized" sites as there are, are large, repeatedly visited locations where localized resources such as chert or salt were exploited transiently.

The *almost* self-sufficient homestead still remains the primary Mississippian settlement. The terrace sites definitely do present the same range of activities as other sites, and so do the few examples of upland residential sites known to us from elsewhere in the region. Clearly, even though our data rejected the model of economic specialization based on geographically differentiated resources, some of that idea remained as a residual from the Service model of chiefdoms. It is probable that terrace sites may have served as refuges in major flood times. Residential settlements do not appear to have been used only on a seasonal basis, but it is now clear from the evidence at the Great Salt Spring that some number of people did make forays to localized resources, leaving their usual homes for short periods.

The self-sufficiency of the local domestic unit is even more established now than in 1978, but we still lack a clear understanding how central authority at sites like Kincaid emerged. The idea of population

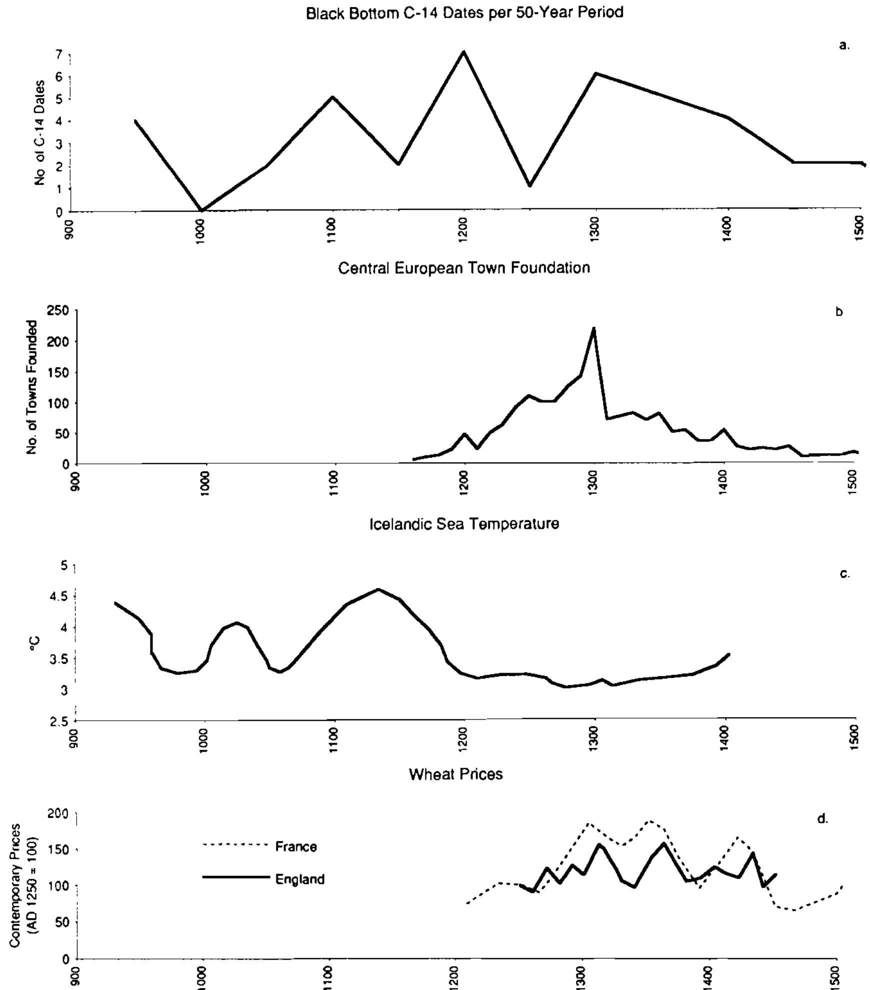


Figure 10.6a. Numbers of radiocarbon dates from the Black Bottom by 50-year periods; b. Numbers of towns founded per year in Central Europe from A.D. 1170-1500, data from Stoob 1970:21, Abb. 2; c. Estimated Icelandic sea temperatures (30-year running average), data from Bergthórsson 1969:98; d. Wheat prices in France and England, A.D. 1200-1500, data from Abel 1980:2, Fig. 1, and Abel 1976:105, Abb. 5).

pressure introduced in the 1978 paper is not pure nonsense, but the difficulties of defining carrying capacity in an operational way make it a difficult argument to test. It is not a contradiction, of course, to speak of low population density in an absolute sense while at the same time suggesting that population density was high relative to the resources and technology available. Today, the concept of risk avoidance and management can be operationalized in more precise terms–especially in the area of biological stresses in Mississippian populations. Unfortunately, almost no human skeletal material is available in the Black Bottom for comparison with the human populations from the Illinois Valley or such sites as Moundville. Bone is simply not well preserved in these environments. Risks, of course, can be "anthropogenic," and other factors such as mere thuggery may have come into play in the local development of hierarchies.

I now strongly suspect that the "hamlet" level probably did not exist as a "local governmental unit" or distinct organizational class. It seems quite possible, based upon historical analogies, that the entire Black Bottom was considered to be the "town" of

Kincaid. "Hamlet-like" residential clusters were thus probably the result of the combined effects of the linear and noncontinuous terrain of the locality and residential patterns that were predominantly kin-based. To the extent that the hamlet existed, it was not a geographical entity, but more probably a descent segment.

As already indicated, it now seems much more risky than it did formerly to assume that outlying small mound centers were necessarily "secondary centers" standing in some subordinate relation to the "paramount" center. There can be no doubt that conditions of dominance and subordination were developing in these societies, but such small mound sites may be expressions of local autonomy, rather than subordination to Kincaid. Mounds are fairly hard to miss in survey, and it can be reaffirmed that aboriginal mounds are lacking in the Black Bottom except at Kincaid.

Efforts at finer chronology are still needed, although the C14 dates are very consistent for a more or less synchronous cross-section at circa A.D. 1250. Our hope for finer chronology using archaeomagnetic dating as a relative technique was, as noted above, frustrated by poor survival of hearths. Refinements of radiocarbon dating may, in the end, make it possible to look at the very small time intervals needed to assess contemporaneity of these small settlements.

Despite the extensive work done in the 1930s at the main site, Kincaid remains a major problem in interpretation. The University of Chicago work at Kincaid was better than the final report suggests, but it still was carried out in the social and archaeological climate of the 1930s. Kincaid was called a prehistoric "Metropolis," to be sure, but probably by analogy to the nearby town of Metropolis, Illinois (population around 7,000). It is still the case that the support area and population at the site are at the same ratios as at other locations in the bottomland. A problem is that we have no new data on elite contexts. We shall not be able to make much progress on the questions of relationships between so-called elites and the ordinary domestic economy until we have better quality data on the elites. The Black Bottom Project has fleshed out the "commons," but we still have little information on elite production activities and roles. I still subscribe to the statement that I made in 1978 expressing skepticism about excessively rigid, typological use of terms such as *chiefdom*.

RESEARCH GOALS FOR THE FUTURE–1978 AND 1991

In 1978 I suggested that we needed site-location survey up and down stream. Since 1978, extensive survey and some testing have been carried out in low areas for the Smithland Dam (Butler *et al.* 1978; Ahler *et al.* 1980). We have also had surveys in upland areas (e.g., Canouts *et al.* 1983) and at various other locations in the Valley. In addition, we have had six seasons of work over ten calendar years at a complex salt production site that was used throughout the Mississippian period (Muller 1984, n.d.). All of these data have allowed the reevaluations and refinements discussed above.

I called for more data on small sites as a class. We now have much more data on small site character, and these seem to be either larger sites that are buried in areas of greater alluviation, or else–in a few cases–they are refuse disposal zones in low areas near larger sites. In any case, there is little evidence for any substantial number of small, "specialized extractive" sites away from homesteads. This is not to say that such things as fishing camps or the like did not exist, but they are not major components of a settlement system. Most use of nearby special resources now appears to have been carried out from the homestead base. We do know that distant resources were exploited by persons who came for the resources and camped out nearby during the time necessary for recovery or production of the resource. In none of these cases do we have evidence for the Service kind of model of local specialization followed by administrative control of redistribution. The best estimate that I can make today for the functions served by developing elites is that they were reservoirs of supplies in times of domestic resource failure. Fortunate individuals, in this sense, may have been able to exploit periodic failures of domestic, kin-based economies to enhance their own prestige and patronage. Certainly from the earliest Mississippian times, the relative freedom of the Kincaid site from the worst flooding would have given residents of that site less risky conditions, perhaps sowing the seeds of social inequality and subordination.

I have already discussed the disappointments of not being able to determine the contemporaneity of individual structures. We are no closer to that than we were in 1978, unfortunately. Economic and social relationships of exactly contemporary settlements are still obscured by the coarseness of the archaeological record. Thus it becomes difficult to be sure whether a larger and a smaller settlement are successive stages of growth or are "central" and "dependent" communities.

I think that we really did surprisingly well in meeting our 1978 goals, but those goals themselves are now quite "long in the tooth." I have, perhaps, been too much influenced by the vulgar materialists and the "Candide" school of political theory in placing emphasis on the positive benefits of elite activities. Yet a crucial question is why anyone would have stayed in areas where "chiefs" ruled. There seems to have been no shortage of different leaders to whom one could ally oneself or alternative locations where independent Mississippian pioneers could have moved. Indeed, it is possible that some upland Mississippian complexes may represent just such withdrawals from a "Mico's" overzealous demands. A major problem in any early development of social hierarchies is how egalitarian, communal societies come to tolerate the personal excesses of central elites. Qui bono? What positive forces, as well as what negative sanctions, allow the development of relations of domination and subordination?

The Lower Ohio Valley Project (née Black Bottom Project) is not completed. The major kinds of sites that remain to be investigated are the upland Mississippian sites and the central, elite locations themselves. The research questions we are asking now have moved beyond the basics of site location and economy into the realm of political economy and the development of hierarchical society.

ACKNOWLEDGMENTS

Support for work in the Lower Ohio Valley Project has been provided by many agencies. Portions of the work were supported by the National Science Foundation, the National Forest Service, the United States Geological Survey, the U.S. Army Corps of Engineers, the Center for Archaeological Investigations, the Department of Anthropology, and the Graduate School and College of Liberal Arts of Southern Illinois University at Carbondale.

REFERENCES

Abel, Wilhelm

1976 *Die Wüstungen des ausgehenden Mittelalters.* Quellen und Forschungen zur Agrargeschichte, Band I. Gustav Fischer Verlag, Stuttgart.

1980 *Agricultural Fluctuations in Europe from the Thirteenth to the Twentieth Centuries,* trans. Olive Ordish. Methuen & Co., London.

Ahler, Steven, Jon Muller, and Joel Rabinowitz

1980 *Archaeological Testing for the Smithland Pool, Illinois.* Southern Illinois University at Carbondale, Center for Archaeological Investigations, Research Paper 13.

Avery, George

1983 Salt, Pots, and Diet: Replication Studies of Late Prehistoric Shell-tempered Ceramics. M.A. thesis, Department of Anthropology, Southern Illinois University at Carbondale.

1985 Salt and Shell-tempered Pottery: An Examination through Replicative Experiments and X-Ray Diffraction Analysis. In *The Emergent Mississippian: Proceedings of the Sixth Mid-South Archaeological Conference, June 6-9,* edited by R.A. Marshall, 22-31. Cobb Institute of Archaeology, Mississippi State University, Occasional Papers 87-01.

1988a An Analysis of a Clay Sample from the Great Salt Spring Site (25B3-1), Gallatin County, Southern Illinois. Ms.

1988b An Analysis of 30 Fabric-Impressed and Plain Salt Pan Sherds from the Great Salt Spring Site (25B3-1), Gallatin County, Southern Illinois. Ms.

Bergthórsson, P.
1969 An Estimate of Drift Ice and Temperature in Iceland in 1,000 Years. *Jökull* (Journal of the Icelandic Geological Society) 19:94-101.

Binford, Lewis R.
1978 *Nunamiut Ethnoarchaeology*. Academic Press, New York.

Blakeman, Crawford
1974 The Late Prehistoric Paleo-ethnobotany of the Black Bottom, Pope and Massac Counties, Illinois. Ph.D. dissertation, Department of Anthropology, Southern Illinois University at Carbondale.

Braudel, Fernand
1981 *The Structures of Everyday Life: The Limits of the Possible. Civilization and Capitalism: 15th–18th Century*, Vol. I. New York, Harper and Row.
1984 *The Perspective of the World. Civilization and Capitalism: 15th–18th Century*, Vol. III. New York, Harper and Row.

Bryson, Reid A., and T.J. Murray
1977 *Climates of Hunger: Mankind and the World's Changing Weather*. University of Wisconsin Press, Madison.

Butler, Brian M.
1977 Mississippian Settlement in the Black Bottom, Pope and Massac Counties, Illinois. Ph.D. dissertation, Department of Anthropology, Southern Illinois University at Carbondale.
1991 Kincaid Revisited: The Mississippian Sequence in the Lower Ohio Valley. In *Cahokia and the Hinterlands*, edited by T. Emerson and R.B. Lewis, 264-273. University of Illinois Press, Urbana.

Butler, Brian M., Glen P. Harrell, and Mary C. Hamilton
1979 *An Archaeological Reconnaissance of the Illinois Portions of the Smithland Pool of the Ohio River*. Center for Archaeological Investigations, Research Paper No. 5. Southern Illinois University at Carbondale.

Canouts, V., E. May, N. Lopinot, and J. Muller
1983 *Cultural Frontiers in the Upper Cache Valley, Illinois*. Center for Archaeological Investigations, Research Paper 16.

Clay, R. Berle
1991 Dynamics of Mound Building at Andalex Village (15HK22). Paper presented at the 36th Midwest Archaeological Conference, La Crosse, Wisconsin.

Cobb, Charles
1988 Mill Creek Chert Biface Production: Mississippian Political Economy in Illinois. Ph.D. dissertation, Department of Anthropology. Southern Illinois University at Carbondale.

Corps of Engineers, Department of the Army
1976 *Flood Plain Information, Ohio River, Pope and Hardin Counties, Illinois*. Louisville District.

Davy, Douglas M.
1982 Proximity and Human Behavior: Settlement Locational Pattern Change in Prehistoric Illinois. Ph.D. dissertation, Department of Anthropology, Southern Illinois University at Carbondale.

Hargrave, M., G. Oetelaar, N. Lopinot, B. Butler, and D. Billings
1983 *The Bridges site (11-Mr-11): A Late Prehistoric Settlement in the Central Kaskaskia Valley*. Southern Illinois University Center for Archaeological Investigations, Research Paper 38.

Hilgeman, Sheri
1991a Angel Negative Painted Design Structure. *Midcontinental Journal of Archaeology* 16(1):3-33.
1991b The Chronology of the Middle Mississippian Occupation of the Angel Site. Paper presented at the 36th Midwest Conference, LaCrosse, Wisconsin.

Jefferies, R., and B. Butler, eds.
1982 *The Carrier Mills Archaeological Project: Human Adaptation in the Saline Valley, Illinois*. Southern Illinois University Center for Archaeological Investigations, Research Paper 33.

Klein, Jeffrey, J.C. Lerman, P.E. Damon, and E.K. Ralph
1982 Calibration of Radiocarbon Dates: Tables Based on the Consensus Data of the Workshop on Calibrating the Radiocarbon Time Scale. *Radiocarbon* 24(2):103-150.

Kowalewski, Stephen A., and James W. Hatch
1991 The Sixteenth-Century Expansion of Settlement in the Upper Oconee Watershed, Georgia. *Southeastern Archaeology* 10:1-17.

Kreisa, Paul P.
1988 *Second-Order Communities in Western Kentucky: Site Survey and Excavations at Late Woodland and Mississippian Period Sites*. Western Kentucky Project, Report 7. Department of Anthropology, University of Illinois, Urbana.

Ladurie, Emmanuel Le Roy
1971 *Times of Feast, Times of Famine: A History of the Climate since the Year 1000*, trans. Barbara Bray. Ferrar, Straus and Giroux, New York.

Lafferty, Robert
1973 An Analysis of Prehistoric Southeastern Fortifications. M.A. thesis, Department of Anthropology, Southern Illinois University at Carbondale.
1977 The Evolution of Mississippian Settlement Patterns and Exploitative Technology in the Black Bottom of Southern Illinois. Ph.D. dissertation, Department of Anthropology, Southern Illinois University at Carbondale.

Lewis, R. Barry (editor)
1986 *Mississippian Towns of the Western Kentucky Border: The Adams, Wickliffe, and Sassafras Ridge Sites.* Kentucky Heritage Council, Frankfort.

Martin, William
1991 A Functional Analysis and Comparison of Three Mississippian Period Ceramic Assemblages from the Black Bottom of Southern Illinois, Pope and Massac Counties. M.A. thesis, Department of Anthropology, Southern Illinois University at Carbondale.

Milner, George R.
1990 The Late Prehistoric Cahokia Cultural System of the Mississippi River Valley: Foundations, Florescence, and Fragmentation. *Journal of World Prehistory* 4(1):1-43.

Morgan, W.N.
1980 *Prehistoric Architecture in the Eastern United States.* MIT Press, Cambridge.

Muller, Jon
1978 The Kincaid System: Mississippian Settlement in the Environs of a Large Site. In *Mississippian Settlement Patterns*, edited by B.D. Smith, 269-92. Academic Press, New York.
1984 Mississippian Specialization and Salt. *American Antiquity* 49(3):489-507.
1986 *Archaeology of the Lower Ohio River Valley.* Academic Press, Orlando.
1987 Salt, Chert, and Shell: Mississippian Exchange and Economy. In *Specialization, Exchange, and Complex Societies*, edited by E. Brumfiel and T. Earle, 10-21. Cambridge University Press, Cambridge.
n.d. (editor) *The Great Salt Spring: Mississippian Production and Specialization.*

Muller, Jon, and Lisa Renken
1989 Radiocarbon Dates for the Great Salt Spring Site: Dating Saltpan Variation. *Illinois Archaeology* 1(2):150-160.

Muller, Jon, and Jeanette E. Stephens
1991 Mississippian Sociocultural Adaptation. In *Cahokia and the Hinterlands*, edited by T. Emerson and R.B. Lewis, 297-310. University of Illinois Press, Urbana.

O'Brien, Michael J.
1977 Intrasite Variability in a Middle Mississippian Community. Ph.D. dissertation, Department of Anthropology, University of Texas at Austin. University Microfilms, Ann Arbor.

Penny, JoAnne
1983 The Raw and the Cooked: Nutritional Evaluation of a Prehistoric Mississippian Diet. M.A. thesis, Department of Anthropology, Southern Illinois University at Carbondale.

Postan, M.M.
1972 *The Medieval Economy and Society: An Economic History of Britain in the Middle Ages.* London, Penguin Books.

Riordan, Robert
1975 Ceramics and Chronology: Mississippian Settlement in the Black Bottom, Southern Illinois. Ph.D. dissertation, Department of Anthropology, Southern Illinois University at Carbondale.

Rudolph, Teresa Perry
1981 The Distribution of Late Woodland Sites in the Black Bottom Area, Pope and Massac Counties, Illinois. M.A. thesis, Department of Anthropology, Southern Illinois University at Carbondale.

Santeford, Lawrence
1982 Mississippian Political Organization and Chipped Stone Artifacts: A Typological Model for the Study of a Prehistoric Society in Southern Illinois. Ph.D. dissertation, Department of Anthropology, Southern Illinois University at Carbondale.

Sears, William H.
1968 The State and Settlement Patterns in the New World. In *Settlement Archaeology*, edited by K.C. Chang, 134-153. National Press Books, Palo Alto.

Smith, Bruce
1978 (editor) *Mississippian Settlement Patterns.* Academic Press, New York.
1984 Mississippian Expansion: Tracing the Historical Development of an Explanatory Model. *Southeastern Archaeology* 3(1):13-32.

Stoob, Heinz
1970 Kartographische Möglichkeiten zur Darstellung der Stadtenstehung in Mitteleuropa, besonders zwischen 1450 und 1800. In *Forschungen zum Städtewesen in Europa*, von Heinz Stoob, Band I.S. 15-42. Böhlau Verlag, Köln.

Williams, Stephen
1977 Some Ruminations on the Current Strategy of Archaeology in the Southeast. Paper presented at the Southeastern Archaeological Conference, Lafayette, Louisiana.
1980 Armorel: A Very Late Phase in the Lower Mississippi Valley. *Southeastern Archaeological Conference Bulletin* 22:105-110.
1984 Some Negative Painted Pottery in the Southeast. Paper presented at the Cottonlandia Conference, Greenville, Mississippi.

Wood, W. Raymond
1968 Mississippian Hunting and Butchering Patterns: Bone from the Vista Shelter, 23SR-20, Missouri. *American Antiquity* 33:170-179.

11 The Territorial Size of Mississippian Chiefdoms

David J. Hally
University of Georgia
Athens, Georgia

How large were Mississippian chiefdoms? How much territory did they control? Published estimates vary considerably. Muller (1986:187) proposes that a single Mississippian polity may have extended more than 120 km along the Lower Ohio River Valley during Kincaid and Angel phases. Scarry and Payne (1986) argue that the Rood's Landing site in southwest Georgia and the Lake Jackson site in northwest Florida were the regional centers for two complex chiefdoms that each measured approximately 90 km in diameter. Smith and Kowalewski (1980) argue that six mound sites, distributed along a 90 km stretch of the Oconee River in central Georgia, constituted a single "sociopolitical unit" during the 15th and 16th centuries. According to Fowler's (1978: Figure 15.7) reconstruction of the American Bottom settlement system, the Cahokia polity at its height of development extended for only approximately 40 km along the Mississippi River. Moundville in Alabama has received perhaps the most attention in this regard with at least three different size estimates appearing in print (Bozeman 1982: Figure 1; Peebles 1971; Steponaitis 1978: Figure 14.5). That these range between 200 km and 28 km is a clear indication of how little is known about the size of Mississippi chiefdoms and about the way in which their size is to be determined.

I suspect that the wide variation among these estimates reflects the view of most archaeologists that Mississippian chiefdoms did in fact vary considerably in size. After all, sites such as Cahokia, Moundville, and Etowah, with their massive mound construction and elaborate burial ceremonialism, must have controlled territories and populations that were considerably larger than those controlled by sites with only one or two mounds. Or did they?

In this paper, I argue that the territories utilized and controlled by chiefdoms seldom measured more than 40 km in maximum dimension and usually considerably less. This observation is based upon the spatial distribution of Mississippian sites with platform mounds in northern Georgia and the distribution of Mississippian mound and habitation sites in the Valley and Ridge section of northwest Georgia and adjacent portions of Tennessee and Alabama. My argument is based on the premise that platform mounds were a necessary and invariant feature of Mississippian administrative centers and on the assumption that the locations of all, or nearly all, Mississippian mound sites in the region are known. The validity of these points is discussed elsewhere (Hally 1992).

THE SITE DISTRIBUTION DATA

Forty-seven sites with Mississippian components and platform mounds are known to exist in northern Georgia (Figure 11.1, Table 11.1). The great majority of these sites have multiple Mississippian components identifiable from pottery collections. Figure 11.2 depicts the time period(s)

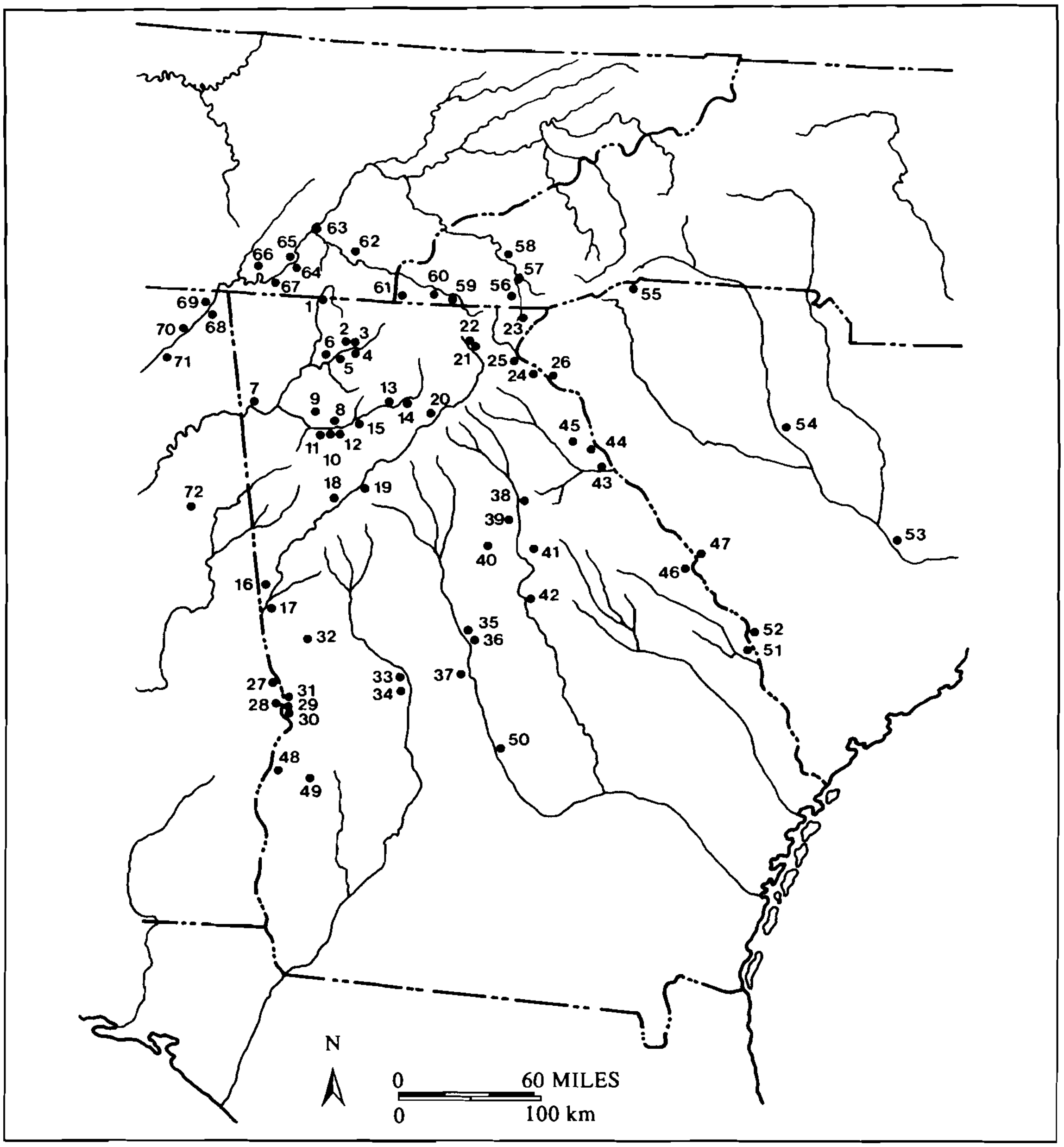


Figure 11.1. Known Mississippi period mound sites in northern Georgia and adjacent portions of Alabama, Tennessee, North Carolina and South Carolina.

1. 40PK16
2. 9MU100
3. 9MU101
4. 9MU102
5. 9GO4
6. 9GO8
7. 9FL3
8. 9BR1
9. 9BR3
10. 9BR6
11. 9BR26
12. 9BR40
13. 9CK1
14. 9CK4
15. 9CK5
16. 9TP41
17. 9TP64
18. 9DO1
19. 9FU10
20. 9FO4
21. 9WH2
22. 9WH3
23. 9RA3
24. 9ST1
25. 9ST3
26. 38OC47
27. 1LE1
28. 1RU61
29. 9CE3
30. 9CE5
31. 9ME3
32. 9HS2
33. 9TR1
34. 9TR12
35. 9BI1
36. 9BI2
37. 9BI12
38. 9GE4
39. 9GE5
40. 9MG46
41. 9HK1
42. 9BL1
43. 9EB1
44. 9EB85
45. 9EB86
46. 9RI1
47. Mason's Plantation
48. 9SW1
49. 9SW2
50. 9PU10
51. 9SN4
52. 9AL11
53. Scott's Lake
54. Blair
55. Lindsay
56. 31MA34
57. 31MA1
58. 31MA5
59. 31CY1
60. 31CE1
61. 31CE5
62. 40PK3
63. 40MG31
64. 40HA1
65. 40HA3
66. 40HA60
67. 40HA65
68. 1JA176
69. 1JA180
70. 1JA101
71. 1JA9
72. 1CA196

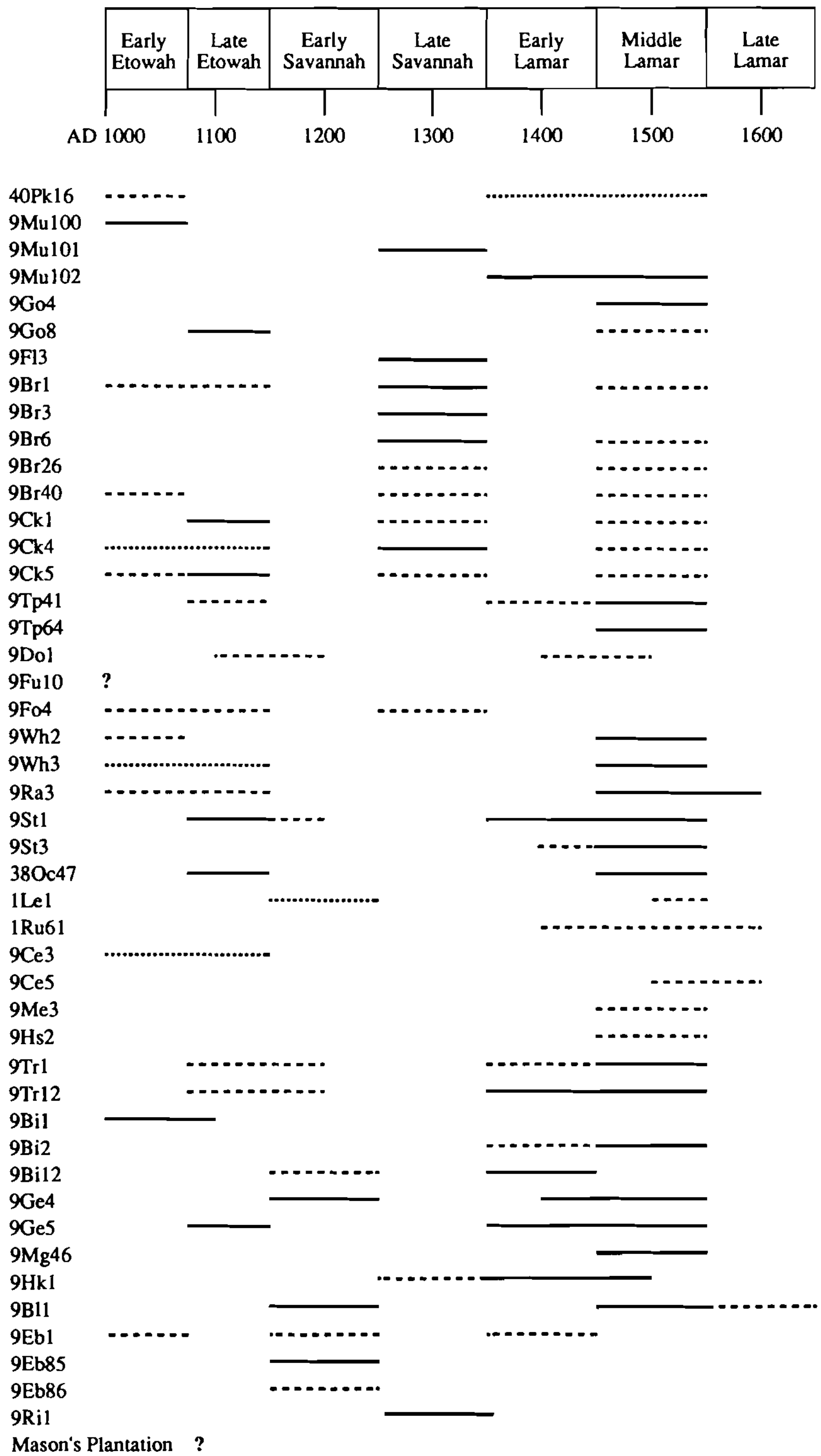


Figure 11.2. Duration of site occupancy and mound construction episodes.

Table 11.1. Components and stratigraphically documented mound construction stages at Mississippian sites in northern Georgia.

Site	Number of Mounds	Components Present	Documented Mound Construction Episodes	Sources
40PK16	1	Early Etowah		UGA colls.
		Early Lamar*		
		Middle Lamar*		
9MU100	1	Early Etowah	Early Etowah	Kelly *et al.*, 1965
9MU101	1	Late Savannah	Late Savannah	Kelly 1972
9MU102	3	Little Egypt	Little Egypt	Hally 1980
		Barnett	Barnett	
9GO4	1	Barnett	Barnett	Hally and Langford 1988
9GO8	1	Late Etowah	Late Etowah	Hally and Langford 1988
		Barnett		
9FL3	1	Wilbanks	Wilbanks	Hally and Langford 1988
9BR1	6	Early Etowah		Larson 1971; King 1991
		Lake Etowah		
		Wilbanks	Wilbanks	
		Brewster		
9BR3	1	Wilbanks	Wilbanks	Wauchope 1966: Fig. 195
9BR6	1	Wilbanks	Wilbanks	Wauchope 1966
		Brewster		
9BR26	1	Wilbanks		Wauchope 1966
		Brewster		
9BR40	1	Early Etowah		Wauchope 1966
		Wilbanks		
		Brewster		
9CK1	1	Late Etowah	Late Etowah	Wauchope 1966; UGA Colls.
		Wilbanks		
		Brewster		
9CK4	1	Early Etowah*		Wauchope 1966; UGA Colls.
		Late Etowah*		
		Wilbanks	Wilbanks	
		Middle Lamar		
9CK5	1	Early Etowah		Sears 1968; Wauchope 1966
		Late Etowah	Late Etowah	
		Wilbanks		
		Brewster		
9TP41	1	Late Etowah		Hally and Oertel 1977
		Early Lamar		
		Bull Creek	Bull Creek	
9TP64	2	Bull Creek	Bull Creek	Huscher 1972
9DO1	1	Late Etowah/		Wauchope 1966; UGA Colls.
		Early Savannah		
		Early Lamar/		
		Middle Lamar		
9FU10	1	unknown		Wauchope 1966
9FO4	1	Early Etowah		Wauchope 1966; UGA Colls.
		Late Etowah		
		Late Savannah		
9WH2	1	Early Etowah		Wauchope 1966; UGA Colls.
		Middle Lamar	Middle Lamar	

** Component identification uncertain due to inadequate pottery collections.*

Table 11.1. Continued.

Site	Number of Mounds	Components Present	Documented Mound Construction Episodes	Sources
9WH3	1	Early Etowah*		Heye *et al.*, 1918
		Late Etowah*		
		Middle Lamar	Middle Lamar	
9RA3	1	Early Etowah		UGA Colls.
		Late Etowah		
		Middle Lamar	Middle Lamar	
		Late Lamar	Late Lamar	
9ST1	1	Jarrett	Jarrett	Williams and Branch 1978; UGA Colls.
		Rembert	Rembert	
		Tugalo	Tugalo	
9ST3	1	Tugalo	Tugalo	Kelly and DeBaillou 1960
38OC47	1	Jarrett	Jarrett	Kelly and Neitzel 1961
		Tugalo	Tugalo	
1LE1	1	Early Savannah*		UGA Colls.
		Bull Creek		
1RU61	1	Bull Creek		DeJarnette 1975
		Late Lamar		
9CE3	2	Averette**		UGA Colls.
9CE5	1	Bull Creek		UGA Colls.
9ME3	1	Bull Creek		UGA Colls.
9HS2	1	Bull Creek		UGA Colls.
9TR1	1	Brunson		Worth 1988
		Thorton		
		Lockett	Lockett	
9TR12	1	Brunson		Worth 1988
		Thorton	Thorton	
		Lockett	Lockett	
9BI1 (all Mounds)	8	Macon Plateau	Macon Plateau	Fairbanks 1946; Ingmanson 1964a, 1964b
9BI2	2	Stubbs		H. Smith 1973
		Cowarts	Cowarts	
9BI12	1	Early Savannah		Williams 1975
		Stubbs	Stubbs	
9GE4	2	Scull Shoals	Scull Shoals	Williams 1988
		Dyar	Dyar	
9GE5	1	Stillhouse	Stillhouse	M. Smith 1981
		Duvall	Duvall	
		Dyar	Dyar	
9MG46	1	Dyar	Dyar	Williams and Shapiro 1990
9HK1	3	Late Savannah		Williams 1990a
		Duval	Duval	
		Iron Horse	Iron Horse	
		Dyar	Dyar	
9BL1	2	Scull Shoals	Scull Shoals	Williams 1990b
		Dyar	Dyar	
		Bell		
9EB1	5	Early Etowah		Rudolph and Hally 1985
		Beaverdam		
		Rembert		
9EB85	1	Beaverdam	Beaverdam	Rudolph and Hally 1985
9EB86	1	Beaverdam		Hutto 1970
9RI1	2	Hollywood	Hollywood	DeBaillou 1965
Mason'sPlantation	6	unknown		Anderson 1990

* *Component identification uncertain due to inadequate pottery collections.*
** *Period identification of Averette phase is uncertain.*

during which occupation occurred at each of the 47 sites and the time period(s) when stratigraphic evidence indicates that mound construction took place.

It is important for what follows to consider how Table 11.1 and Figure 11.2 were constructed and what inferences can and cannot be drawn from them. The temporal framework is based on sequences of ceramic phases that have been developed for different areas of northern Georgia over the past several decades (Fairbanks 1950; Hally 1979; Hally and Langford 1988; Hally and Rudolph 1986; Wauchope 1948; Williams and Shapiro 1987; Worth 1988). These sequences have been merged into a single set of seven time periods, and the latter have been assigned temporal durations based on a combination of radiocarbon dates (Hally and Langford 1988) and best guesses. Site components are listed in Table 11.1 by phase or time period, depending upon the adequacy of documentation for the particular pottery collection and whether phases have been formally defined for a particular area.

Episodes of mound construction are dated by means of pottery collections excavated from refuse strata that accumulated on the summit or flank of mounds during their use. These episodes, each of which consists of one or more earth mantles and accompanying summit buildings added during a single period or during two sequential periods, are depicted in Figure 11.2 as solid lines. Components that cannot be stratigraphically identified with mound construction are indicated by dashed lines. Dotted lines indicate those components that cannot be identified with absolute certainty due to inadequate ceramic evidence. With few exceptions, lines have been drawn across entire periods, implying that components endured for entire periods and that mound construction and use were continuous throughout entire periods. In very few cases, however, is it possible to document that components and mound construction episodes were coterminous with time periods. Nor is it likely that "contemporaneous" components and mound construction episodes at different sites began or ended at precisely the same point in time. It is unlikely, for example, that the mound construction episodes at 9MU101, 9FL3, 9BR1, and 1RI1 all ended at A.D. 1350. Rather, the information presented in Table 11.1 and Figure 11.2 can only be interpreted as indicating that a particular component or mound construction episode at one site may have been at least partially contemporaneous with that at another site, or that it was not contemporaneous.

Altogether, thirty-seven episodes of mound construction can be documented stratigraphically among the northern Georgia sites. As can be seen in Figure 11.2, most mound construction episodes lasted no longer than a single time period, although some spanned two periods. In five cases, mound construction can be shown to have occurred during two non-sequential periods, a phenomenon that is probably much more common than the available stratigraphic evidence reveals.

Table 11.2 lists the straight-line distances between sites having contemporaneous episodes of mound construction. For the sake of simplicity, only distances less than 60 km have been listed. The decision to measure distances between sites as straight lines rather than along river courses was based on three factors: the historically documented importance of trails for travel between towns and polities in the Southeast (Carleton 1989; Goff 1953; Hudson 1991; Hudson *et al.* 1984; Meyer 1928); the lack of historical documentation for the importance of water transportation in the region; and the fact that most contemporaneous, neighboring sites are located on different rivers.

The straight-line distances between sites with contemporaneous mound construction episodes are graphed in Figure 11.3a. As is clear from this figure, the distribution of inter-site distances is bimodal; with only two exceptions, distances are either less than 18 km or more than 32 km. The exceptions, 9CK1 and 9MG46, are located 26.5 km and 28.8 km respectively from neighboring sites (9CK5 and 9GE4) having supposedly contemporary mound construction episodes. As will be discussed in a later section, both "exceptions" can be readily accounted for.

There were doubtless many more mound construction and use episodes at northern Georgia sites than are listed in Table 11.1. For one thing, twelve of the listed sites have no stratigraphically identified mound construction episode, yet mound construction must have taken place during one or more of the periods represented by the components identified at each site. Furthermore, most (30 out of 47) of the sites listed in Table 11.1 have multiple, temporally discrete occupations or occupations that span two consecutive periods. Five sites have stratigraphic evidence for two or more temporally discrete episodes of mound construction and seven sites have stratigraphic evidence for mound construction episodes that span two consecutive periods. Certainly, other sites had multiple construction episodes or multi-period episodes as well. The Etowah site (9BR1) is a case in point. Given the abundance of Early and Late Etowah period pottery at the site (King 1991) and the large size of Mound A, mound construction must have occurred during more than one phase.

On the assumption that mound construction may have occurred during any Mississippian occupation, distances separating all sites with contemporaneous components are listed in Table 11.3 and depicted in Figure 11.3b. One hundred forty-one pairs of contemporaneous components are represented.

Contemporaneous components occur at 117 pairs of sites separated by less than 18 km or more than 32 km. In 24 cases, however, sites with contemporaneous occupations are separated by distances ranging between 18 and 32 km. To the extent that mound construction occurred during these components, the case for a bimodal distribution of distances separating sites with contemporaneous mound construction episodes is considerably weakened. A review of the exceptional cases, however, reveals that the validity of most is questionable for one reason or another.

- The major component at 9GO8 (located 18.2 km from 9MU102) dates to the Late Etowah Period, and there is stratigraphic evidence for a mound construction episode at that time as well. The surface concentration of Barnett phase pottery at the site, however, is physically separated from the mound.

- The mound at 9EB86 (located 20.4 km from 9EB1) may not be a Mississippian mound. The site has a Middle Woodland Swift Creek occupation, and its location, far up a small stream where there is little alluvial bottomland, is more typical of sites with Woodland mounds than sites with Mississippian mounds.

- Four sites, 1RU61, 9CE5, 9ME3, and 9HS2, are located between 26 and 29 km from 1LE1 and have occupations that are contemporaneous with the Middle Lamar component at that site. The mound at 1LE1, however, may date to the Middle Woodland period. The site has a Swift Creek component, and the mound, which is now destroyed, was described early in the century as containing large rocks (Brannon 1909). Mississippian mounds containing layers of stone are a common feature in northeastern Georgia and adjacent areas of North and South Carolina, but have not been reported elsewhere in the state. Middle Woodland mounds constructed entirely of stone or having stone mantles are more widely distributed, being known from the Tunacunnhee (9DD25) and Shaw (9BR17) sites in northwestern Georgia and the Plant Sherer (9MO180) and Rock Eagle (9PM80) sites in middle Georgia.

- 9CK4 and 9FO4 have contemporaneous components sometime during the Early or Late Etowah

*Table 11.2. Distances between sites separated by less than 60 km and having contemporaneous, stratigraphically documented episodes of mound construction.**

Site	Mound Construction Episode	Contemporaneous Site	Distance
9MU100	Early Etowah	none	
9MU101	Late Savannah	9BR1	53.9
		9BR3	44.5
		9BR6	55.1
		9CK4	52.2
9MU102	Early Lamar	none	
	Middle Lamar	9GO4	8.6
9GO4	Middle Lamar	----**	
9GO8	Late Etowah	9CK1	50.7
		9CK5	46.6
9FL3	Late Savannah	9BR1	52.8
		9BR3	42.9
		9BR6	51.8
9BR1	Late Savannah	9BR3	15.4
		9BR6	1.5
		9CK4	54.1
9BR3	Late Savannah	9BR6	14.9
		9CK4	58.7
9BR6	Late Savannah	9CK4	55.6
9CK1	Late Etowah	9CK5	26.5
9CK4	Late Savannah	----	
9CK5	Late Etowah	----	
9TP41	Middle Lamar	9TP64	15.0
9TP64	Middle Lamar	----	
9WH2	Middle Lamar	9WH3	2.7
		9RA3	43.3
		9ST1	39.4
		9ST3	34.3
		38OC47	47.6
9WH3	Middle Lamar	9RA3	44.7
		9ST1	41.8
		9ST3	36.7
		38OC47	50.1

** Distances between pairs of sites with contemporaneous mound construction episodes are presented only one time–for the first site listed.*
*** Dashed line indicates that distances to all sites with contemporaneous mound construction episodes have been listed earlier in the table.*

periods and during the Late Savannah period. The distance separating these two sites, 31.1 km, is barely outside the bimodal distribution.

- The Wilbanks site (9CK5) is involved in the remaining 16 "exceptional" distance measurements. The site has four distinguishable com-

Table 11.2. Continued.

Site	Mound Construction Episode	Contemporaneous Site	Distance
9WH3	Middle Lamar	9RA3	44.7
		9ST1	41.8
		9ST3	36.7
		38OC47	50.1
9RA3	Middle Lamar	9ST1	40.1
		9ST3	35.1
		38OC47	44.9
	Late Lamar	none	
9ST1	Late Etowah	38OC47	8.2
	Middle Lamar	9ST3	6.3
		38OC47	8.2
9ST3	Middle Lamar	38OC47	14.1
38OC47	Middle Lamar	----	
9TR1	Middle Lamar	9TR12	4.7
		9BI2	49.1
9TR12	Early Lamar	9BI12	46.1
	Middle Lamar	9BI2	50.2
9BI1	Early/Late Etowah	none	
9BI2	Middle Lamar	98BL1	50.0
9BI12	Early Lamar	----	
9GE4	Early Savannah	none	
	Early Lamar	9GE5	17.0
		9HK1	47.6
	Middle Lamar	9GE5	17.0
		9HK1	47.6
		9MG46	40.1
9GE5	Late Etowah	----	
	Early Lamar	9HK1	32.9
	Middle Lamar	9HK1	32.9
		9MG46	28.8
9HK1	Early Lamar	----	
	Middle Lamar	9BL1	46.8
9BL1	Early Savannah	none	
	Middle Lamar	----	
9EB85	Early Savannah	none	
9RI1	Late Savannah	none	

ponents dating to the Early and Late Etowah, Late Savannah, and Middle Lamar periods. According to Sears (1958), mound construction began in the Late Etowah period. This being the case, there can be no mound at Wilbanks contemporaneous with the Early Etowah components at 9BR1 (22.0 km) and 9BR40 (22.0 km). Seven Wilbanks distance measurements are to single mound sites (9BR3, 9BR6, 9BR26, and 9BR40) which were occupied at the same time as Etowah (9BR1). As will be discussed later, it is probable that these sites were minor administrative centers in the Late Savannah period chiefdom centered on the Etowah site. They may have had the same status during the other three periods when Etowah was occupied. In this kind of situation, the critical distance would seem to be between 9CK5 and Etowah, the primary administrative center; the distances to the smaller sites being in a sense redundant.

- Wilbanks does have components that are contemporaneous with the Late Etowah, Late Savannah, and Middle Lamar occupations at Etowah (9BR1). The two sites, which are separated by 22.0 km, could have had contemporaneous mound building episodes at any or all of these times. As noted earlier, Wilbanks also has components that are contemporaneous with the Late Savannah and Middle Lamar occupations at 9CK1 (26.5 km). Mound construction and use could have occurred at both sites during either or both of these periods. The "exceptional" status of all cases involving 9BR1, 9CK1, and 9CK5 will be considered in a later section.

Following the preceding arguments, only nine of the 141 pairs of contemporaneous components (including the four marginally exceptional cases involving components at 9CK4 and 9FO4) are separated by distances falling between 17 and 33 km. The site component evidence then tends to conform to the bimodal distribution pattern suggested by sites with stratigraphically documented mound construction episodes.

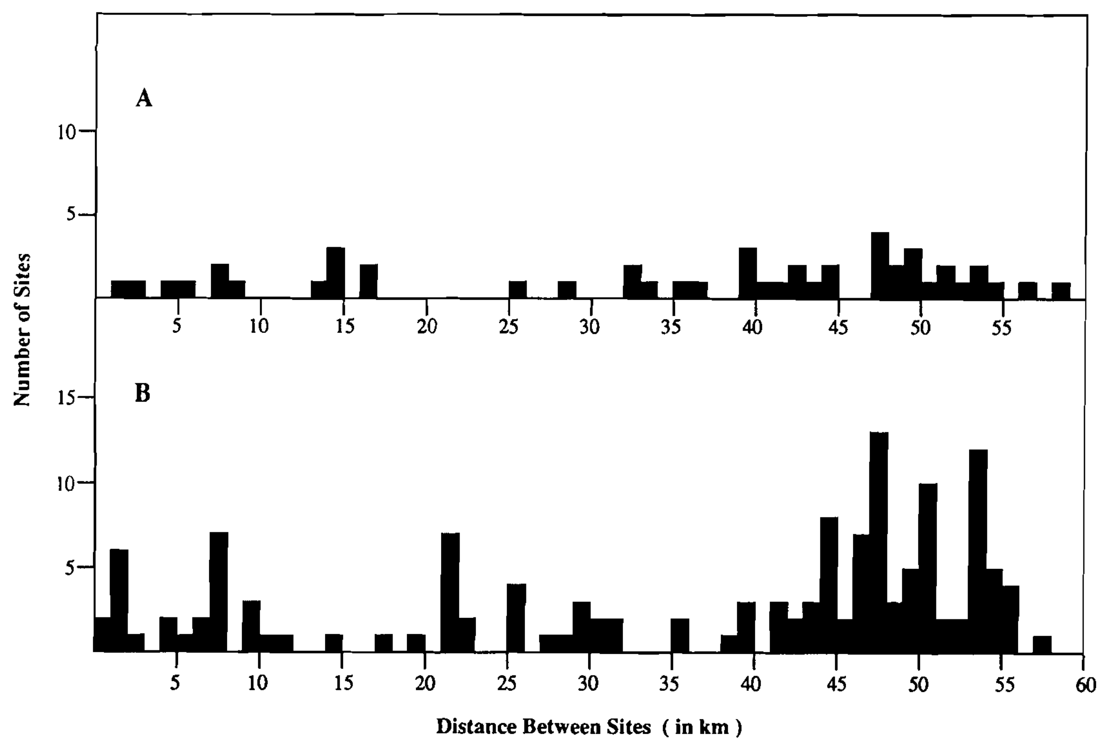


Figure 11.3. Distances separating Mississippian mound sites with contemporaneous construction episodes (A) and site components (B).

*Table 11.3. Distances between mound sites separated by less than 60 km and having contemporaneous components (excluding sites with contemporaneous mound construction episodes listed in Table 11.2).**

Site	Component	Contemporaneous Site	Distance
40PK16	Early Etowah	9MU100	43.2
	Early/Middle Lamar	9MU102	44.1
		9GO4	45.5
		9GO8	50.8
9GO8	Middle Lamar	9MU102	18.2
		9GO4	9.8
		9BR1	47.3
		9BR6	48.1
		9BR26	47.4
		9BR40	47.8
		9CK1	50.7
		9CK4	58.1
		9CK5	46.6
9BR1	Early Etowah	9MU100	55.0
		9BR40	0.4
		9CK4	54.1
		9CK5	22.0
	Late Etowah	9GO8	47.3
		9CK1	47.9
		9CK4	54.1
		9CK5	22.0
		9DO1	48.1
	Middle Lamar	9MU102	53.7
		9GO4	50.8
		9BR6	1.5
		9BR26	8.0
		9BR40	0.4
		9CK1	47.9
		9CK4	54.1
		9CK5	22.0
		9DO1	48.1
9BR6	Middle Lamar	9MU102	54.9
		9GO4	52.6
		9BR26	7.1
		9BR40	1.5
		9CK1	49.3
		9CK4	55.6
		9CK5	23.4
		9DO1	47.8
9BR26	Late Savannah	9MU101	55.9
		9FL3	44.7
		9BR1	8.0
		9BR3	12.1
		9BR6	7.1
		9BR40	8.4
		9CK1	50.1
		9CK4	54.2
		9CK5	29.8

** Distances between pairs of sites with contemporaneous components are presented only one time–for the first site listed.*

Table 11.3. Continued.

Site	Component	Contemporaneous Site	Distance
	Middle Lamar	9MU102	55.4
		9GO4	56.5
		9BR40	8.4
		9CK1	51.1
		9CK4	54.2
		9CK5	29.8
		9DO1	52.4
9BR40	Early Etowah	9MU100	55.2
		9CK4	54.2
		9CK5	22.0
	Late Savannah	9MU101	54.5
		9FL3	53.0
		9BR1	0.4
		9BR3	15.3
		9BR6	1.5
		9CK1	47.9
		9CK4	54.2
		9CK5	22.0
	Middle Lamar	9MU102	53.9
		9GO4	51.2
		9CK1	47.9
		9CK4	54.2
		9CK5	22.0
		9DO1	48.0
9CK1	Late Savannah	9MU101	44.0
		9BR1	47.9
		9BR3	51.4
		9BR6	49.3
		9CK4	7.8
		9CK5	26.5
		9FO4	35.3
	Middle Lamar	9MU102	44.5
		9GO4	49.2
		9CK4	7.8
		9CK5	26.5
9CK4	Early/Late Etowah	9MU100	51.0
		9GO8	58.1
		9CK1	7.8
		9CK5	32.2
		9FO4	31.1
	Middle Lamar	9MU102	52.2
		9GO4	56.3
		9CK5	32.2
9CK5	Early Etowah	9MU100	48.2
		9FL3	45.3
	Late Savannah	9MU101	47.3
		9BR1	22.0
		9BR3	29.6
		9BR6	23.4
		9FO4	45.3
	Middle Lamar	9MU102	47.3
		9GO4	47.8
		9DO1	50.3

Table 11.3. Continued.

Site	Component	Contemporaneous Site	Distance
9TP41	Late Etowah	none	
	Early Lamar	none	
9DO1	Late Etowah/Early Savannah	----**	
	Early/Late Lamar	----	
9FO4	Early Etowah	----	
	Late Etowah	9CK1	35.3
		9CK5	45.3
	Late Savannah	9CK4	31.1
9WH2	Early Etowah	9WH3	2.7
		9RA3	43.3
9WH3	Early/Late Etowah	9RA3	44.7
		9ST1	41.8
		38OC47	50.1
9RA3	Early Etowah	----	
	Late Etowah	9ST1	40.1
		38OC47	44.9
9ST1	Early Savannah	none	
9ST3	Early Lamar	9ST1	6.3
1LE1	Early Savannah	none	
	Middle Lamar	9TP41	51.2
		9TP64	38.4
		1RU61	25.7
		9CE5	27.8
		9ME3	26.0
		9HS2	28.9
1RU61	Early Lamar	none	
	Middle Lamar	9CE5	2.0
		9ME3	2.5
		9HS2	42.5
	Late Lamar	9CE5	2.0
9CE3	Early/Late Etowah	none	
9CE5	Middle/Late Lamar	9ME3	3.0
		9HS2	40.0
9ME3	Middle Lamar	9HS2	39.7
9HS2	Middle Lamar	9TP41	47.4
		9TP64	42.0
9TR1	Late Etowah/Early Savannah	9TR12	4.7
		9BI1	49.8
		9BI12	45.1
	Early Lamar	9TR12	4.7
		9BI2	49.1
		9BI12	45.1
9TR12	Late Etowah/Early Savannah	9BI1	51.2
		9BI12	46.1
9BI2	Early Lamar	9TR12	50.8
		9BI12	9.5
9BI12	Early Savannah	9BL1	55.1
9HK1	Late Savannah	none	
9BL1	Late Lamar	none	
9EB1	Early Etowah	none	
	Early Savannah	9EB85	11.4
		9EB86	20.4
	Early Lamar	none	
9EB86	Early Savannah	9EB85	10.2

*** Dashed line indicates that distances to all sites with contemporaneous components have been listed earlier in the table.*

It is possible that the bimodal pattern of intersite distances observed in northern Georgia has been biased by excluding from analysis sites located immediately beyond the arbitrary boundaries of the study area. A review of the 25 mound sites located closest to the study area (Figure 11.1, numbers 48-72), however, indicates that this is unlikely. Only three sites are separated from sites within the study area by less than 33 km, and in none of these instances can a strong argument be made that mound construction episodes were contemporary. Hiwassee Old Town (40PK3) is located 29.5 km from 40PK16. It has Hiwassee Island (Early and Late Etowah periods) and Mouse Creek (Middle Lamar) phase components (Richard Polhemus, personal communication), but these cannot be stratigraphically associated with mound construction. Nor are they well enough described to determine whether they are contemporary with components at 40PK16. On the Little Tennessee River in North Carolina, Coweta Creek (31MA34) and Naquassee (38MA1) are located 12.8 and 22 km respectively from 9RA3. Both sites have Pisgah (Early Etowah through Late Savannah periods) and Qualla (Early through Late Lamar periods) phase components (Dickens 1976; Egloff 1967). Only the Qualla component at 31MA34 is stratigraphically associated with mound construction, but it evidently dates to the 18th century and postdates 9RA3. The other components at the two North Carolina sites are not well enough described to determine whether or not they are contemporary with 9RA3.

INTERPRETATION OF THE SITE DISTRIBUTION DATA

According to the evidence presented above, there is a strong tendency for contemporary mound sites in northern Georgia to be separated by distances that are either less than 18 km or more than 32 km. How are we to account for this spatial pattern? One contributing factor may be the spacing of rivers in the region. With few exceptions, Mississippian mound sites are located along rivers, usually in or immediately adjacent to the floodplain. Most rivers in northern Georgia are separated from their neighbors by 40 km or more, suggesting that the large number of intersite distances in this range (Figure 11.3) is due at least in part to the spacing of rivers. As Table 11.4 demonstrates, however, straight line distances separating contemporaneous sites located on the same river are, with the exception of 9CK1 and 9CK5, either less than 18 km or more than 32 km. I conclude from this that river spacing is not the only factor responsible for the large distances separating many sites or the bimodal distribution of inter-site distances.

The tendency for Mississippian populations to concentrate where there are large amounts of al-

Table 11.4. Sites with contemporaneous, stratigraphically documented mound construction episodes and located along the same river.

Sites with Contemporaneous Mound Construction Episodes	Distance Separating sites
9MU102 -- 9GO4	8.6 km
9FL3 -- 9BR1	52.8 km
9FL3 -- 9BR3	42.9 km
9FL3 -- 9BR6	51.8 km
9BR1 -- 9BR3	15.4 km
9BR1 -- 9BR6	1.5 km
9BR1 -- 9CK4	54.1 km
9BR3 -- 9BR6	14.9 km
9BR3 -- 9CK4	58.7 km
9BR6 -- 9CK4	55.6 km
9CK1 -- 9CK5	26.5 km
9TP41 -- 9TP64	15.0 km
9WH2 -- 9WH3	2.7 km
9ST1 -- 9ST3	6.3 km
9ST1 -- 38OC47	8.2 km
9ST3 -- 38OC47	14.1 km
9TR1 -- 9TR12	4.7 km
9GE4 -- 9GE5	17.0 km
9GE4 -- 9GHK1	47.6 km
9GE5 -- 9HK1	32.9 km
9HK1 -- 9BL1	46.8 km

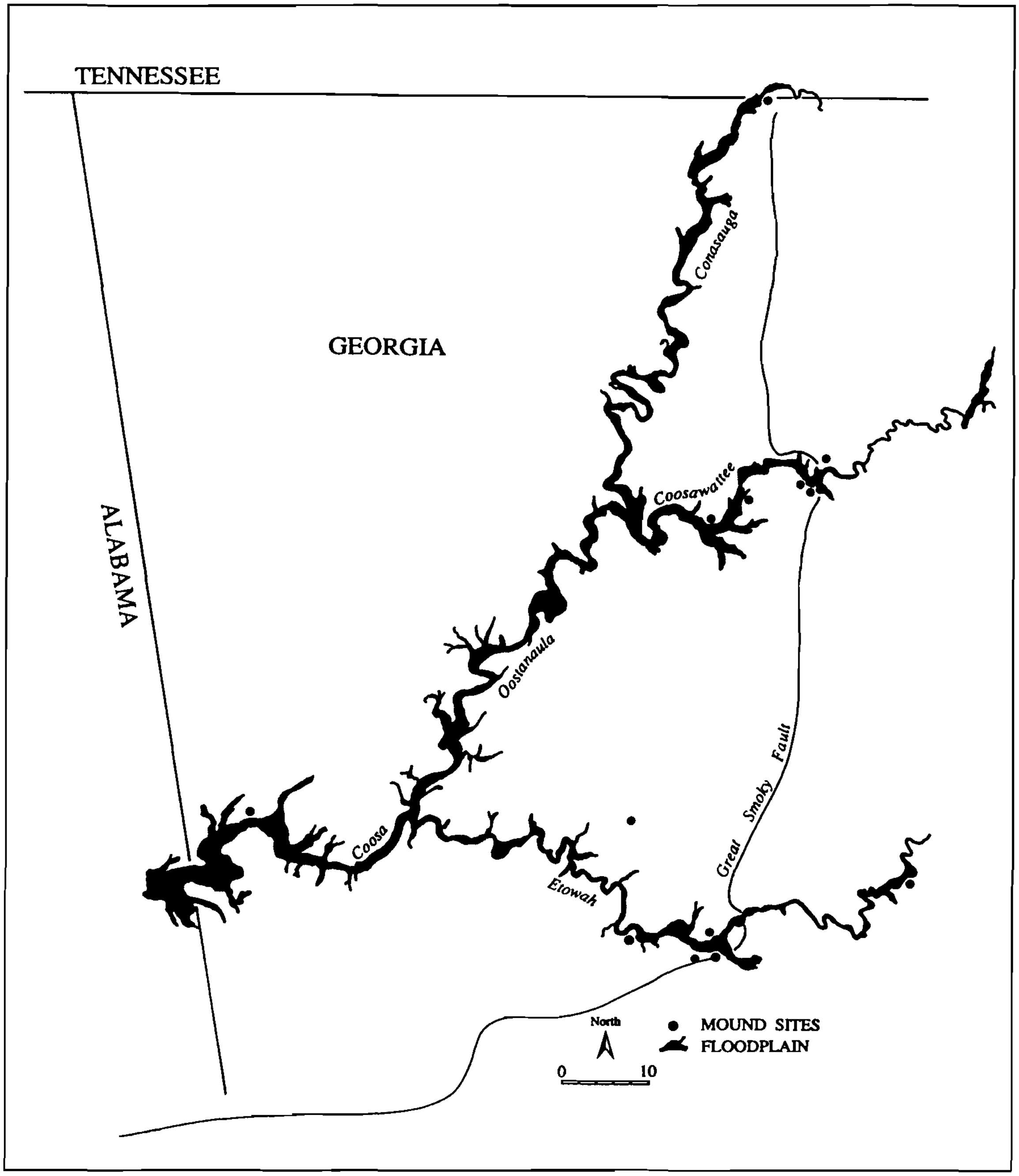


Figure 11.4. Distribution of alluvial bottomland soils and Mississippian mound sites in northwestern Georgia.

luvial floodplain soils suitable for cultivation has been known to archaeologists for some time (Peebles 1978; Smith 1978; Ward 1965). To the extent that there is a causal relationship, we can expect the distribution of alluvial floodplain soils to have influenced the location of Mississippian mound sites as well.

Figure 11.4 displays the distribution of alluvial soils and Mississippian mound sites along the major rivers in northwestern Georgia. The map was constructed by tracing the width of river bottomlands—including active floodplain and alluvial terraces—from USGS 1:100,000 topographic maps and 7.5 min. quadrangle maps. In conformity with expectations, most sites are located adjacent to large expanses of bottomland. Especially striking are 9CK5, located on the Etowah River in the Piedmont Physiographic Province, and the sites located on the Conasauga, Coosawattee, and Etowah Rivers where they cross the Great Smoky Fault and enter the Valley and Ridge Physiographic Province. The latter locations represent the first extensive tracts of floodplain soils encountered after long stretches of narrow river valley in the Piedmont and Blue Ridge Provinces. Since these locations also contained extensive shoals and provided ready access to the natural resources of two physiographic provinces, they clearly had much to offer Mississippian societies.

Within the Valley and Ridge Province, however, the soil-site association is weak. 9BR3 is located on a small stream with virtually no bottomland readily available. 9FL3 is located along a stretch of the Coosa River with relatively narrow bottomland, but with extensive tracts of bottomland located immediately up and down stream. No mound sites are found in the numerous large tracts of bottomland that occur at several points along the Conasauga and Oostanaula Rivers. Nor are there mound sites immediately downriver from 9FL3 where the most extensive bottomlands in the entire region once existed.

A mound of possible Mississippian affiliation existed at one time at the junction of the Oostanaula

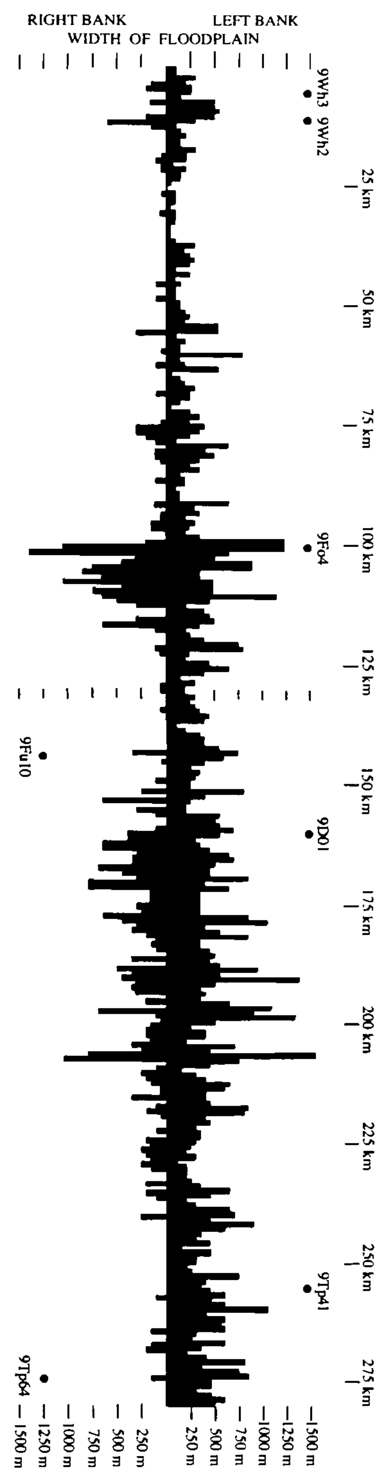


Figure 11.5. Width of alluvial bottom land at 1 km intervals along the Piedmont section of the Chattahoochee River.

and Etowah Rivers. There is a moderate amount of bottomland here, and it is possible that the combination of river junction and extensive bottomland was attractive to Mississippian populations. The junction of the Conasauga and Coosawattee Rivers, however, also has a moderate amount of bottomland, but no recorded mound sites.

Figure 11.5 depicts the width of the bottomland along the Piedmont section of the Chattahoochee River at 1 km intervals. Data for this figure were obtained from U.S.D.A. Soil Conservation Service county soil maps and 1:440 scale aerial photographs. No mound sites occur where there is little bottomland, and three sites, 9WH2, 9WH3, and 9FO4 occur where there is abundant (in absolute terms or relative to surrounding areas) bottomland. On the other hand, there is a 50-mile stretch of river between mile 175 and 225 where bottomland is very extensive, but there are no mound sites.

It is reasonable to conclude from these two examples that availability of alluvial soils is an important determinant of where Mississippian populations and their political centers were located. The fact that Mississippian mound sites are not located near all large tracts of alluvial soil, however, indicates that additional factors are involved in their location and, by implication, their spatial separation.

There is abundant ethnohistorical and archaeological evidence that Mississippian sites with platform mounds served as the administrative centers for Mississippian chiefdoms (Hally 1992; Peebles and Kus 1977). The spatial distribution of mound sites, therefore, should be determined to some degree by the political and economic nature of these polities, especially their internal organizational structure and their external competitive relationships. To the extent that this is true, the spatial distribution of Mississippian mound sites should reflect a number of characteristics of chiefdoms, including their size, spacing, and relative level of political complexity.

Historically known chiefdoms throughout the world exhibit considerable variability in political complexity (Earle 1987; Feinman and Neitzel 1984). Investigators typically use the number of levels in the administrative hierarchies of chiefdoms as a measure of this variability; those polities having only one administrative level above the local community (simple chiefdoms) being less complex than those having two or more levels of administration (complex chiefdoms). This distinction is useful to archaeologists because of the likelihood that it will be manifested in settlement hierarchies: simple chiefdoms have only a single administrative center, while complex chiefdoms have a primary center and one or more secondary centers. In the southeastern United States, number of mounds is usually considered an indicator of a site's administrative importance. Sites that have multiple mounds are identified as primary centers, while those having only a single mound are identified as secondary centers (Anderson 1990; Bozeman 1982; Fowler 1978; Steponaitis 1978).

Based on this line of reasoning, I propose that contemporary mound sites separated by less than 18 km are in most cases the administrative centers for a single polity. North Georgia sites separated by less than 18 km and having contemporaneous mound construction episodes are listed in Table 11.5. In four cases, one site has two or more mounds, while the other member(s) of the group has only a single mound. The sites in each of these groups probably represent the major and minor centers for four different complex chiefdoms.

The group of sites containing Etowah (9BR1) is the most impressive example of a complex chiefdom in the region. During the Late Savannah period, Etowah covered 21 hectares and had six platform mounds, one of which was 18 m high. Two single-mound sites with Wilbanks phase mound construction were located within 15.5 km of Etowah. Two additional single-mound sites with strong Wilbanks components, but lacking stratigraphic evidence for when mound construction occurred, were located 0.2 and 8.0 km from 9BR1 and are probably also minor centers in the polity.

It is not certain whether groups containing only sites with single mounds also represent complex chiefdoms. With a height of 10 m, the mound at 9TR1 is considerably larger than the mound at 9TR12 (4 m). This size difference could reflect the greater importance of 9TR1 in the polity, but it also may reflect the fact that the site served as an administrative center for a longer period of time and as a result had more construction stages.

Sites 9ST1, 9ST3, and 3OC47 each have a single mound. The mound at 9ST1 (4.5 m) is larger than those at 9ST3 (2.25 m) and 38OC47 (1.0 m), but the difference is not very great. All three mounds have suffered considerable size reduction due to plowing and erosion. Considering the number of construction stages that have been destroyed, 38OC47 was probably originally the same height as 9ST1. In the absence of great differences in mound height, it is not possible to make a strong case for any of the three sites' being the primary center in a single complex chiefdom.

In all site groups containing only single-mound sites it is possible that mound construction episodes dating to the same phase are actually not contemporaneous. I have argued elsewhere (Hally 1992) that mound construction and use may have shifted between sites within a single chiefdom as a result of factional competition and breaks in the line of chiefly succession. Such may have been the case in the 9ST1/9ST3/38OC47 and 9WH2/9WH3 site groups, where there is no physical evidence that one site was administratively more important than the other.

There are four groups of close-spaced sites that have contemporaneous occupations but lack contemporaneous stratigraphically-dated mound construction episodes: 9CK1 and 9CK4; 9EB1 and 9EB85; 1RU61, 9CE5, and 9ME3; and 9BI2 and 9BI12. In two of these–9EB1/9EB85 and 9BI2/9BI12–one site has multiple mounds while the other has a single mound.

Contemporary northern Georgia mound sites separated by more than 32 km are best interpreted as being the administrative centers for different polities. This interpretation is supported by the existence of seven spatially definable clusters of mid-16th century sites in the Valley and Ridge section of northwest Georgia, northeast Alabama, and eastern Tennessee (Hally *et al.* 1990). The spatial configuration of these clusters is approximately what would be expected if the administrative centers of neighboring chiefdoms were separated from one another by 32 km or more. All evidence, furthermore, points to these clusters' being the archaeological manifestations of seven distinct chiefdoms. Each consists of between 4 and 7 large habitation sites and one or more sites with definite or possible mound construction episodes. Sites average 2.8 ha in size and are distributed along rivers at approximately 3-5 km intervals. The spatial size of site clusters–the straight-line distance separating towns at each end of the cluster–ranges from 10.8 km to 23.5 km, while the maximum distance separating mound sites and towns within clusters is 18.7 km. Farmsteads and specialized activity sites may have existed beyond these limits, but the zone of intensive occupation in all cases appears to have been considerably less than 30 km in diameter. Finally, the distance separating site clusters averages 33 km.

Evidence from the Moundville culture in Alabama indicates that even the largest, most complex chiefdoms did not substantially exceed the dimensions of the site clusters in the Valley and Ridge Province. With its large (150 ha) multi-mound (20 mounds) primary center and six single-mound secondary centers, Moundville was arguably one of the largest and most powerful chiefdoms in the Southeast. In spite of these impressive features, the maximum distance between recorded settlements at the peak of the polity's development in the 15th century is approximately 29 km, and the greatest distance between primary (Moundville) and secondary mound centers (1TU3) is 20 km (Bozeman 1982).

Etowah (9BR1) and 9EB1 were the primary centers for what were probably two of the largest and most powerful complex chiefdoms in northern Georgia. Both may also have had secondary centers located at distances in excess of 17 km. While the stratigraphic evidence is equivocal, 9CK5 almost certainly had a mound construction episode contemporaneous with the Late Savannah period mounds at Etowah (Hally 1989). 9CK5 is located 22.0 km upriver from the Etowah site. 9EB1 had five platform mounds, which were constructed sometime during the Early Savannah or Early Lamar periods. 9EB85, a single-mound site with an Early Savannah period construction episode, is located 11.4 km away. 9EB86, located 20.4 km from 9EB1, has a single mound that is probably Woodland in age, but which may have had a construction episode during the Early Savannah period.

The available evidence suggests that complex chiefdoms might attain an overall geographic size such that component settlements were distributed over areas measuring as much as 30 km across and outlying secondary centers were situated as much as 22 km from the primary center. The small number of distances in the 18-32 km range separating mound sites in the northern Georgia sample, however, suggests that polities this large were not very common and that they were not able to expand geographically beyond these dimensions.

Table 11.5. Sites located less than 18 km apart with contemporaneous episodes of mound construction.

Site	Number of Mounds	Stratigraphically Documented Construction Episodes
9MU102	3	Barnett
9GO4	1	Barnett
9BR1	6	Wilbanks
9BR3	1	Wilbanks
9BR6	1	Wilbanks
9TP64	2	Bull Creek
9TP41	1	Bull Creek
9WH2	1	Middle Lamar
9WH3	1	Middle Lamar
9ST1	1	Jarrett
38OC47	1	Jarrett
9ST1	1	Tugalo
9ST3	1	Tugalo
38OC47	1	Tugalo
9TR1	1	Lockett
9TR12	1	Lockett
9GE4	2	Dyar
9GE5	1	Dyar

As noted at the beginning of this paper, the only exceptions to the bimodal distribution of distances separating sites with stratigraphcally documented contemporary mound construction (Figure 11.3a) are 9CK1 and 9CK5, separated by 26.5 km, and 9MG46 and 96E5, separated by 28.8 km. According to the argument developed above, these distances are too great for the sites in question to have belonged to the same polity, and they are too small to have separated the administrative centers of distinct polities. The problem posed by these distances disappears when we realize that one member of each pair of sites (9CK5 and 9GE5) was itself a minor center in a complex chiefdom. As the primary center of a complex chiefdom, 9BR1, not 9CK5, would have been in competition with 9CK1, the administrative center for a simple chiefdom. Likewise, 9GE4, not 9GE5, would have been in competition with 9MG46. The politically significant distances, therefore, are between 9BR1 and 9CK1 and between 9GE4 and 9MG46. These distances are 47.9 km and 40.1 km respectively.

DISCUSSION

The evidence from the Valley and Ridge section of northwestern Georgia and eastern Tennessee and Alabama and from Moundville indicates that the territories that were intensively exploited and inhabited by late Mississippi period chiefdoms–their core areas–varied in size from as little as 11 km to as much as 29 km across. Given that the distance between administrative centers of neighboring chiefdoms usually exceeds 35 km and probably averages closer to 45 km (Figure 11.3), the core areas of chiefdoms appear to have been separated from one another by lightly-inhabited and -used zones measuring at least 10 km across and more commonly 20 to 30 km across. These intermediate zones probably functioned as military buffer zones and as reserves for wild food species. By increasing the spatial separation between neighboring polities, they may have served to reduce the frequency and danger of military attack (Anderson 1990; DeBoer 1981). To the extent that the potential for encountering enemy warriors would have reduced the intensity of hunting and gathering within them, the intermediate zones may have supported relatively rich stocks of wild plants and animals that could be exploited when food availability in the core area was temporarily reduced. They may have served also as game reservoirs from which the stock of game animals depleted in the core area could be replenished (Anderson 1990; Hickerson 1965). The distance separating Mississippian chiefdoms in northern Georgia, in short, served as a spacing mechanism that reduced competition between neighboring polities and human pressure on the natural resources of the region.

Why were the core areas of Mississippian chiefdoms in northern Georgia limited to a geographical size of approximately 30 km? The answer according to many researchers is to be found in the nature of the administrative organization of such societies and the increasing costs associated with administering larger territories (Cherry 1978; Earle 1987; Johnson 1982; and Renfrew 1975). Wright (1977) argues that chiefdoms are characterized by a generalized type of political control in which individuals at different levels in the leadership hierarchy have similar duties and responsibilities. As a result, leaders at the community or local level have the capability of acting independently of their superiors at the regional level and may do so if not closely monitored and controlled by the latter.

Chiefs may ensure the loyalty of their subordinates through coercion, gifts of subsistence goods and/or wealth items, and by demonstrating their sanctity and actual political power (Brumfield and Earle 1987; Earle 1987). No matter what approach is used, control undoubtedly entailed considerable travel by leaders and their representatives between local communities and administrative centers. It follows that the greater the distance between center and community, the greater the cost in time and effort of maintaining political control and the less effective that control becomes.

Effective control also depends on the general population's involvement in and willing support of the central political institutions (Johnson 1982, 1987). Non-elite individuals contribute surplus production and labor to these institutions, participate in their ritual activities, and make use of the services they provide. Such participation involves travel and transport of goods to and from the center. The greater the distance between community and center, the greater the cost of such participation and the lower its intensity (Steponaitis 1978).

Researchers agree that the cost of travel and transport places spatial limits on the size of the territory that can be controlled with the political mechanisms available to chiefs (Cherry 1978; Earle 1987; Johnson 1982, 1987; Renfrew 1975; Steponaitis 1978). These costs can be measured in time and effort, but only the former can be readily estimated.

Humans are capable of walking in excess of 30 km in a single day. The DeSoto and Pardo expeditions were able to average 28 km per day through the southeastern United States (Hudson *et al.* 1985). Aztec tlamemes (professional porters) covered five leagues (21-28 km, depending upon whether the *legua legal* or the *lequa comun* is used) per day under load (Hassig 1985:32-33). Indian "runners" in the 18th century southeastern U.S. reportedly covered 20 miles (32 km) in a day (Grant 1980:127).

Distances of 30 km or more would have imposed severe strains on the political control mechanism of chiefdoms. Some portion of the general population, as well as the elite themselves, may have been unable to cover such distances in a day due to age and health. Leaders may have been further encumbered by the necessity to travel in a style befitting their status, that is, bearing ritual paraphernalia and accompanied by an entourage of advisors and attendants. Such distances, furthermore, would require an additional day to conduct business and a third day for the return trip.

In contrast, distances small enough to permit travelers to make a round trip and conduct their business in a single day would have been highly cost effective (Bell *et al.* 1988; Johnson 1987). Given a moderately fast walking speed of 5 km per hour, distances of up to 20 km could have been handled in this manner.

Among the Mississippian chiefdoms in northern Georgia and the Valley and Ridge Province of eastern Alabama and Tennessee, distances between mound center and habitation site and between primary and secondary centers seldom exceed 20 km. This suggests that the ability to travel round-trip between administrative center and subordinate community in a single day was critical to the long-term survival of these polities and may have been the major factor limiting their spatial size.

Only in the cases of the large, complex chiefdoms of Moundville, Etowah, and perhaps 9EB1 do distances equal or exceed 20 km. These chiefdoms were exceptional in regard to amount of public construction, elaborateness of elite burials, and probably population size. For reasons that are not yet clear, their leaders were apparently able to achieve and maintain a somewhat greater degree of political control over subordinate communities and as a result were able to increase the spatial limits of that control.

Early Spanish documents pertaining to the interior portion of the Southeast provide evidence that some chiefdoms were able to extend political control over neighboring chiefdoms. The best documented of these so-called paramount chiefdoms (Hudson *et al.* 1985) is Coosa, which was visited by both the DeSoto and Luna expeditions in 1540 and 1560. According to the narratives of these expeditions, as many as six chiefdoms distributed in linear fashion between present-day Newport, Tennessee and Childersburg, Alabama–a distance of 500 km–acknowledged the political domination of the chiefdom of Coosa (Hudson *et al.* 1985; Hally *et al.* 1990). The latter was located on the Coosawattee River in northwestern Georgia.

The mid-16th century "Oconee Province" discussed by Smith and Kowalewski (1980) was probably also a paramount chiefdom. Distances separating 9BL1, 9HK1, 9MG46, and the 9GE4-9GE5 site group are all in excess of 40 km, suggesting that the sites functioned as administrative centers for distinct chiefdoms. Contemporary Spanish accounts (Hudson *et al.* 1984) indicate, furthermore, that the chiefdoms along the Oconee River were under the control of a paramount chief.

The nature of the relationship between paramount and subordinate chiefdoms in these polities is not clear. The Spanish sources clearly describe the payment of tribute to paramount chiefs and provide evidence for coordination of military activities, formal visits by the paramount chief, and use of a kinship idiom to symbolize the relationship (Hudson *et al.* 1985; Hudson 1988; Smith and Hally 1992).

The distances separating component chiefdoms, and the fact that intervening zones were not inhabited, suggests that paramount chiefdoms were only weakly integrated politically. These distances also suggest that paramounts were only marginally involved, if at all, in the internal administration of subordinate chiefdoms. Presumably this was left in the hands of the local leadership. Paramounts may have seen their relation to subordinate chiefdoms as being primarily a relationship between chiefly elites, and they may have required little more of that relationship than that subordinate chiefs continue to acknowledge their subordination in appropriate ways.

To the extent that this characterization is correct, paramount chiefdoms must have been unstable and short lived. Simple and complex chiefdoms endured for as much as a century or more (Hally 1992). Given their geographical size and attendant cost and difficulty of administering, paramount chiefdoms may have depended heavily upon the leadership skills of individual chiefs for their survival and consequently may have endured for only a decade or two.

SUMMARY AND CONCLUSION

In northern Georgia, Mississippian sites with contemporaneous mound construction and use episodes tend to be separated from their neighbors by either less than 18 km or more than 32 km. In this paper, I have argued that sites in the former case represent primary and secondary centers in the same complex chiefdom or, in a few instances, the shifting through time of administrative centers within a single chiefdom. Contemporaneous mound sites separated by more than 32 km, I have argued, represent the administrative centers for independent chiefdoms. These interpretations are based on the propositions that sites with platform mounds were administrative centers, that all Mississippian chiefdoms had centers with mounds, that number of mounds at a site is indicative of the position of the site in an administrative hierarchy, and that straight line distances are appropriate measures of polity size and interpolity spacing.

Published site distribution data suggest that the spatial patterns characteristic of chiefdoms in northern Georgia were characteristic of complex prestate societies throughout the world. Regional site distribution data are not available for many Mississippian societies in the eastern United States, but where they are, a case can generally be made for polity size not exceeding 40 km. The Moundville case has already been cited. Price (1978) argues that Powers phase sites in southeast Missouri and northeast Arkansas are restricted in distribution to a series of large sand ridges. As such they form a well defined cluster measuring approximately 32 km in maximum dimension (Price 1978: Figure 8.3).

Farther south in Arkansas, Phyllis Morse (1990:78, Figure 5.2) reports that Nodena phase sites are distributed in three clusters, each measuring less than 30 km in maximum diameter, containing at least one mound site, and separated from its neighbors by an uninhabited zone measuring more than 10 km across. Sites of the neighboring and contemporaneous Parkin phase form a single cluster measuring approximately 40 km in maximum diameter (D. Morse 1990: Figure 7.2). A single small Parkin phase site is located approximately 15 km south of the cluster, but given the intervening distance is probably not part of the polity.

Along the Lower Ohio, Tennessee, and Cumberland Rivers, Mississippian mound sites are distributed singly or in groups at intervals of 31-50 km (Muller 1986: Figure 2.1). Not all of these sites are contemporaneous, but the distances separating sites and site groups indicate that each could have functioned as the administrative center(s) for a distinct chiefdom.

Scarry and Payne (1986) assign 24 Early Fort Walton period mound sites in southwestern Georgia and northwestern Florida to six separate polities. The mound sites in four cases are separated from their nearest neighbor by more than 38 km. The Lake Jackson polity, however, contains two clusters of mound sites, each with a single multiple-mound site. The distance separating these two sites (Lake Jackson and Letchworth) is approximately 33 km, suggesting that there were two polities in the Tallahassee area rather than one.

The sixth polity recognized by Scarry and Payne (1986), the Rood's Landing chiefdom, is more difficult to interpret. As defined by Scarry and Payne, the polity contains 9 mound sites distributed in six groups. Distances between mound site groups in three cases exceed 30 km, but in the remaining three cases they range between 21 and 26 km. While it is possible that the latter represent true exceptions to the north Georgia pattern, I think a more likely interpretation is that the sites in question are not contemporaneous. The Early Fort Walton period according to Scarry and Payne (1986) spans 250 years, plenty of time for individual chiefdoms to rise and fall (Hally 1992). The Mississippian chronology in the lower Chattahoochee River Valley, furthermore, is so poorly known that we cannot be sure that all of the 9 sites in question even fall within this interval.

Outside the eastern United States, clusters of sites measuring 25 km to 40 km in maximum diameter are reported for the Middle Formative Rosario phase in the Valley of Oaxaca (Kowalewski *et al.* 1989), the Late Early Horizon and Early Intermediate period in the Santa Valley of Peru (Wilson 1988), and the Early Uruk period in southwest Iran (Johnson 1982, 1987). In a number of regions where settlement pattern data are less complete–Minoan period Crete (Renfrew 1975), Late Classic period southeastern Peten (Hammond 1972), and sixteenth century Panama (Helms 1979)–the average distance between primary administrative centers is reported to be approximately 40 km.

The likelihood that the geographic size of complex pre-state societies throughout the world varies within a relatively narrow range has important implications for the investigation of such societies. To the extent that the spatial pattern observed in northern Georgia is cross-culturally valid, the ability of researchers to identify individual chiefdoms in the archaeological record will increase substantially. Clusters of habitation sites ranging in size between 10 and 30 km can be identified as individual chiefdoms with some confidence, especially if they include one or more sites that functioned as administrative centers. In the absence of evidence for habitation site clusters, sites that have mounds or other characteristics suggestive of administrative importance and that are separated from one another by distances in excess of 30 km can be tentatively identified as administrative centers for individual chiefdoms. In these cases, the existence of chiefdoms can be confirmed and their spatial configuration determined through field survey.

With the ability to identify individual chiefdoms in the archaeological record, it becomes possible to investigate a variety of questions about chiefdoms that were heretofore difficult if not impossible to pursue using archaeological information alone. Enquiry has already been initiated into several such questions, including: the factors affecting the spatial location of administrative centers within chiefdoms (Hally n.d.; Steponaitis 1978); the size of chiefdom populations (Hally *et al.* 1990); the temporal duration of chiefdoms (Hally 1992); and the conditions leading to the growth and decline of chiefdoms (Anderson 1990; Hally 1992). Additional fruitful lines of enquiry are not difficult to imagine.

ACKNOWLEDGEMENTS

Stephen Kowalewski provided useful comments concerning the manuscript and figures. Gisela Weiss-Gresham drafted the figures. I thank them both for their assistance.

REFERENCES

Anderson, David G.
1990 Political Change in Chiefdom Societies: Cycling in the Late Prehistoric Southeastern United States. Ph.D. dissertation, Department of Anthropology, University of Michigan, Ann Arbor.

Bell, Thomas L., Richard L. Church, and Larry Gorenflo
1988 Late Horizon Regional Efficiency in the Northeastern Basin of Mexico: A Location-Allocation Perspective. *Journal of Anthropological Archaeology* 7:163-202.

Bozeman, Tandy K.
1982 Moundville Phase Communities in the Black Warrior River Valley, Alabama. Ph.D. dissertation, Department of Anthropology, University of Michigan, Ann Arbor.

Brumfield, Elizabeth M., and Timothy K. Earle
1987 Specialization, Exchange, and Complex Societies: An Introduction. In *Specialization, Exchange, and Complex Societies*, edited by E. Brumfield and T. Earle, 1-9. Cambridge University Press, New York.

Carleton, Kenneth H.
1989 Eighteenth Century Trails in the Choctaw Territory of Mississippi and Alabama. M.A. thesis, Department of Anthropology, University of Georgia, Athens.

Cherry, John F.
1978 Generalization and the Archaeology of the State. In *Social Organization and Settlement: Contributions from Anthropology, Archaeology, and Geography*, edited by David Green, Colin Haselgrove, and Matthew Spriggs, 411-437. British Archaeological Reports International Series (Supplementary) No. 47.

DeBaillou, Clemens
1965 A Test Excavation of the Hollywood Mound (9RI1). *Southern Indian Studies* 17:3-11.

DeBoer, Warren R.
1981 Buffer Zones in the Cultural Ecology of Aboriginal Amazonia: An Ethnohistorical Approach. *American Antiquity* 46:364-377.

DeJarnette, David L.
1975 *Archaeological Salvage in the Walter F. George Basin of The Chattahoochee River in Alabama.* University of Alabama Press, Tuscaloosa.

Dickens, Roy S.
1976 *Cherokee Prehistory*. University of Tennessee Press, Knoxville.

Earle, Timothy K.
1987 Chiefdoms in Archaeological and Ethnohistorical Perspective. *Annual Review of Anthropology* 16:279-308.

Egloff, Brian J.
1967 An Analysis of Ceramics from Historic Cherokee Towns. M.A. thesis, Department of Anthropology, University of North Carolina, Chapel Hill.

Fairbanks, Charles H.
1946 The Macon Earthlodge. *American Antiquity* 12:94-108.
1950 A Preliminary Segregation of Etowah, Savannah and Lamar. *American Antiquity* 16:142-51.

Feinman, Gary, and Jill Neitzel
1984 Too Many Types: An Overview of Sedentary Prestate Societies in the Americas. In *Advances in Archaeological Method and Theory*, edited by Michael Schiffer, 39-102. Academic Press, Orlando.

Fowler, Melvin L.
1978 Cahokia and the American Bottom: Settlement Archaeology. In *Mississippian Settlement Patterns*, edited by B.D. Smith, 455-478. Academic Press, New York.

Goff, John H.
1953 Some Major Indian Trading Paths Across the Georgia Piedmont. *Georgia Mineral Newsletter* 6:4.

Grant, C.L.
1980 *Letters, Writings, and Journals of Benjamin Hawkins, Vol. 1, 1796-1801*. Beehive Press, Savannah.

Hally, David J.
1979 *Archaeological Investigation of the Little Egypt Site (9MU102), Murray County, GA., 1969 Season*. University of Georgia, Laboratory of Archaeology, Laboratory Series, Report No. 18. Athens.
1980 *Archaeological Investigation of the Little Egypt Site (9Mu102), Murray County, Georgia, 1970-72 Seasons*. Submitted to the Heritage Conservation and Recreation Service, United States Department of the Interior, Contract Nos. 14-10-9-900-390, 1910P21041, 9911T000411, C5546.
1989 The Ecological and Cultural Context of the Etowah Site. Paper presented at the Southeastern Archaeological Conference, Tampa, Florida.
1992 Platform Mound Construction and the Instability of Mississippian Chiefdoms. Ms. on file, Department of Anthropology, University of Georgia, Athens.
n.d. An Overview of Lamar Culture. In *Ocmulgee Archaeology, 1933-1986*, edited by D.J. Hally. University of Georgia Press, Athens.

Hally, David J., and James B. Langford, Jr.
1988 *Mississippi Period Archaeology of the Georgia Valley and Ridge Province*, Laboratory of Archaeology Series, Report No. 25. University of Georgia, Athens.

Hally, David J., and Leila Oertel
1977 *Archaeological Investigations at the Park Mound (9Tp41), Troup County, Georgia, 1972 Season*. Submitted to the Heritage Conservation and Recreation Service, National Park Service, Contract No. 14-10-9-900-395.

Hally, David J., and James L. Rudolph
1986 *Mississippi Period Archaeology of the Georgia Piedmont*. University of Georgia, Laboratory of Archaeology Series, Report No. 24. Athens.

Hally, David J., Marvin T. Smith, and James B. Langford, Jr.
1990 The Archaeological Reality of DeSoto's Coosa. In *Columbian Consequences, Vol. 2. Archaeological and Historical Perspectives on the Spanish Borderlands East*, edited by David Hurst Thomas, 121-138. Smithsonian Institution Press, Washington, D.C.

Hammond, Norman D.C.
1972 Locational Models and the Site of Lubaatun, A Classic Maya Centre. In *Models in Archaeology*, edited by D.L. Clarke. Methuen and Co., London.

Hassig, Ross
1985 *Trade, Tribute, and Transportation: the Sixteenth Century Political Economy of the Valley of Mexico*. University of Oklahoma Press, Norman.

Helms, Mary W.
1979 *Ancient Panama: Chiefs in Search of Power*. University of Texas Press, Austin.

Heye, G.G., F.W. Hodge, and G.H. Pepper
1918 *The Nacoochee Mound in Georgia*. Contributions from the Heye Museum of the American Indian, Heye Foundation 2(1). New York.

Hickerson, Harold
1965 The Virginia Deer and Intertribal Buffer Zones in the Upper Mississippi Valley. In *Man, Culture, and Animals*, edited by A.L. and A.P. Vayda, 43-65. AAAS Monograph.

Hudson, Charles
1988 A Spanish-Coosa Alliance in Sixteenth-Century North Georgia. *Georgia Historical Quarterly* LXXII:599-626.

Hudson, Charles, Marvin Smith, and Chester DePratter
1984 The Hernando de Soto Expedition: From Apalachee to Chiaha. *Southeastern Archaeology* 3:65-77.

Hudson, Charles, Marvin Smith, David Hally, Richard Polhemus, and Chester DePratter
1985 Coosa: A Chiefdom in the Sixteenth-Century Southeastern United States. *American Antiquity* 50:723-737.

Huscher, Harold A.
1972 *Archaeological Investigations in the West Point Dam Area: A Preliminary Report.* Ms. on file, Department of Anthropology, University of Georgia, Athens.

Hutto, Brooks
1970 *Archaeological Survey of the Elbert County, Georgia Portion of the Proposed Trotter's Shoals Reservoir, Savannah River.* University of Georgia, Laboratory of Archaeology Series, Report No. 7. Athens.

Ingmanson, J. Earl
1964a *Archaeology of the South Plateau, Ocmulgee National Monument.* MS on file at the NPS Southeast Archaeological Center, Tallahassee.
1964b *Dunlop and McDougal Mounds, Ocmulgee National Monument.* MS on file at the NPS Southeast Archaeological Center, Tallahassee.

Johnson, Gregory A.
1982 Organizational Structure and Scalar Stess. In *Theory and Explanation in Archaeology*, edited by C. Renfrew, M.J. Rowlands, and B.A. Segraves, 389-421. Academic Press, New York.
1987 The Changing Organization of Uruk Administration on the Susiana Plain. In *The Archaeology of Western Iran*, edited by Frank Hole, 107-155. Smithsonian Institution Press, Washington, D.C.

Kelly, Arthur R.
1972 *The 1970-1971 Field Seasons at Bell Field Mound, Carters Dam.* Submitted to USDI National Park Service, Atlanta.

Kelly, A.R., and Clemens De Baillou
1960 Excavation of the Presumptive Site of Estatoe. *Southern Indian Studies* 12:3-30.

Kelly, Arthur R., and Robert S. Neitzel
1961 *The Chauga Site in Oconee County, South Carolina.* University of Georgia, Laboratory of Archaeology Series, Report 3. Athens.

Kelly, Arthur R., Frank T. Schnell, Donald F. Smith, and Ann L. Schlosser
1965 *Explorations in Sixtoe Field, Carter's Dam, Murray County, Georgia: Seasons of 1962, 1963, 1964.* Submitted to USDI National Park Service, Atlanta.

King, Adam
1991 Excavations at Mound B, Etowah: 1954-1958. M.A. thesis, Department of Anthropology and Linguistics, University of Georgia, Athens.

Kowalewski, Stephen A., Gary M. Feinman, Laura Finsten, Richard E. Blanton, and Linda M. Nicholas
1989 *Monte Alban's Hinterland, Part II: Prehistoric Settlement Patterns in Tlacolula, Etla, and Ocotlan, the Valley of Oaxaca, Mexico.* Memoirs of the Museum of Anthropology, University of Michigan, No. 24. Ann Arbor.

Larson, Lewis H., Jr.
1971 Archaeological Implications of Social Stratification of the Etowah Site, Georgia. In *Approaches to the Social Dimensions of Mortuary Practices*, edited by James A. Brown, 58-67. Society for American Archaeology Memoir 25.

Meyer, William E.
1928 Indian Trails of the Southeast. *Annual Report of the Bureau of American Ethnology* 42:727-857. Washington, D.C.

Morse, Dan F.
1990 The Nodena Phase. In *Towns and Temples Along the Mississippi*, edited by David H. Dye and Cheryl Anne Cox, 69-97. University of Alabama Press, Tuscaloosa.

Morse, Phyllis A.
1990 The Parkin Site and the Parkin Phase. In *Towns and Temples Along the Mississippi*, edited by David H. Dye and Cheryl Anne Cox, 118-134. University of Alabama Press, Tuscaloosa.

Muller, Jon,
1986 *Archaeology of the Lower Ohio River Valley.* Academic Press, Orlando.

Peebles, Christopher S.
1971 Moundville and Surrounding Sites; Some Structural Considerations of Mortuary Practices. In *Approaches to the Social Dimensions of Mortuary Practices*, edited by J.A. Brown, 68-91. Society for American Archaeology Memoir 25.
1978 Determinants of Settlement Size and Location in the Moundville Phase. In *Mississippian Settlement Patterns*, edited by B.D. Smith, 369-416. Academic Press, New York.

Peebles, Christopher S., and Susan M. Kus
1977 Some Archaeological Correlates of Ranked Societies. *American Antiquity* 42:421-448.

Price, James A.
1978 The Settlement Pattern of the Powers Phase. In *Mississippian Settlement Patterns*, edited by B.D. Smith, 201-232. Academic Press, New York.

Renfrew, Colin

1975 Trade as Action at a Distance: Questions of Integration and Communication. In *Ancient Civilization and Trade*, edited by J.A. Sabloff and C.L. Lamberg-Karlovsky, 3-59. University of New Mexico, Albuquerque.

Rudolph, James L., and David J. Hally

1985 *Archaeological Investigations of the Beaverdam Creek Site (9EB85), Elbert County, Georgia*. Russell Papers, Archaeological Services Division, National Park Service, Atlanta.

Scarry, John F., and Claudine Payne

1986 Mississippian Polities in the Fort Walton Area: A Model Generated from the Renfrew-Level EXTENT Algorithm. *Southeastern Archaeology* 5:79-90.

Sears, William H.

1958 The Wilbanks Site (9CK5), Georgia. *Bureau of American Ethnology Bulletin* 169:129-194. Smithsonian Institution, Washington, D.C.

Smith, Bruce D.

1978 Variations in Mississippian Settlement Patterns. In *Mississippian Settlement Patterns*, edited by B.D. Smith, 479-503. Academic Press, New York.

Smith, Hale G. (editor)

1973 *Analysis of the Lamar Site (9Bi7) Materials at the Southeastern Archaeological Center*. Submitted to the Southeastern Archaeological Center, National Park Service, Contract No. CX500031136.

Smith, Marvin T.

1981 *Archaeological Investigations at the Dyar Site, 9GE5*. Wallace Reservoir Project Contribution No. 11. Department of Anthropology, University of Georgia, Athens.

Smith, Marvin T., and David J. Hally

1992 Chiefly Behavior: Evidence from Sixteenth Century Spanish Accounts. In *Lords of the Southeast: Social Inequality and the Native Elites of Southeastern North America*, edited by Alex W. Barker and Timothy R. Pauketat, 99-110. Archaeological Papers of the American Anthropological Association No. 3. Washington, D.C.

Smith, Marvin T., and Stephen A. Kowalewski

1980 Tentative Identification of a Prehistoric "Province" in Piedmont Georgia. *Early Georgia* 8:1-13.

Steponaitis, Vincas P.

1978 Location Theory and Complex Chiefdoms: A Mississippian Example. In *Mississippian Settlement Patterns*, edited by B.D. Smith, 417-454. Academic Press, New York.

Ward, H. Trawick

1965 Correlation of Mississippian Sites and Soil Types. *Southeastern Archaeological Conference Bulletin* 3:42-48.

Wauchope, Robert W.

1948 The Ceramic Sequence in the Etowah Drainage, Northwest Georgia. *American Antiquity* 47:1-34.

1966 *Archaeological Survey of Northern Georgia with a Test of Some Cultural Hypotheses*. Society for American Archaeology Memoir 21. Salt Lake City.

Williams, J. Mark

1975 *The Stubbs Mound in Central Georgia Prehistory*. Submitted to the Southeastern Archaeological Center, National Park Service, Contract No. PX5890-50053.

1988 Scull Shoals Revisited. *U.S. Forest Service Cultural Resources Report* No. 1. Gainesville.

1990a *Archaeological Excavations at Shoulderbone Mounds and Village (9HK1)*. LAMAR Institute, Watkinsville, Georgia.

1990b *Archaeological Excavations at Shinholser (9BL1), 1985 and 1987*. LAMAR Institute, Watkinsville, Georgia.

Williams, Mark, and Gary Shapiro

1987 The Changing Contexts of Political Power in the Oconee Valley. Paper presented at the Annual Meeting of the Southeastern Archaeological Conference. Charleston.

1990 *Archaeological Excavations at Little River (9MG46), 1984 and 1987*. LAMAR Institute, Watkinsville, Georgia.

Williams, Marshall, and Carolyn Branch

1978 The Tugalo Site. *Early Georgia* 6:32-37.

Wilson, David J.

1988 *Prehistoric Settlement Patterns in the Lower Santa Valley, Peru*. Smithsonian Institution Press, Washington, D.C.

Worth, John E.

1988 Mississippian Occupation of the Middle Flint River. M.A. thesis, Department of Anthropology and Linguistics, University of Georgia, Athens.

Wright, Gary T.

1977 Recent Research on the Origin of the State. *Annual Review of Anthropology* 6:379-397.

12 An Examination of the Significance of a Tortoise-Shell Pin from the Etowah Site

Lewis Larson
West Georgia College
Carrollton, Georgia

INTRODUCTION

One of the important problems confronting anyone who attempts an analysis of the mortuary objects accompanying the burials in Mound C at the Etowah site in Bartow County, Georgia is the placement of these materials in the proper chronological frame. While there is no questioning the fact that Mound C was the product of Mississippi Period cultural activity, the relationship of the mound and its mortuary complex to temporal units within the Mississippi Period has not always enjoyed a unanimity of focus. Contention has particularly centered on the dating of the Wilbanks ceramic complex. This ceramic complex is distinguished from the preceding Etowah ceramic complex by an absence of shell tempering. Further, the complicated stamping of the Wilbanks wares is characterized by curvilinear motifs with broad lands and grooves. In this the Wilbanks stamping contrasts with the narrow lands and grooves and nested diamond motifs of the stamping on the Etowah wares. The types belonging to the succeeding Lamar ceramic complex are distinguished from the earlier Wilbanks complex by the use of curvilinear incised motifs on carinated bowls and pinched and noded rims on stamped jars (see Sears 1958:149-178, 189-194 for a discussion and definition of the Etowah, Wilbanks, and Lamar types in the Etowah Valley).

During the 1950s excavations, it was the Wilbanks ceramic assemblage that was found to be associated with the final stage of mortuary ritual at Mound C. At the same time it was determined that the burials of the final stage of Mound C were the source of the bulk of the Etowah site paraphernalia and symbols that have been regularly assigned to the Southeastern Ceremonial Complex. Almost the complete inventory of motifs, ceremonial objects, and costume elements identified by Waring and Holder (1945:18-19) were found with the Wilbanks phase burials in Mound C.

The interpretation of the Mound C mortuary complex is difficult because the excavations of John Rogan in 1885 (Thomas 1887:96-107; 1894:292-310) and Warren King Moorehead in 1925, 1926, and 1927 (Moorehead 1932:68-87) completely truncated the mound, reducing its height by approximately three and one half meters. These excavations destroyed the upper surfaces of each of the five mound construction phases. On the summit of the earliest mound surface there remained but a few centimeters of wall trench length to testify to the fact that the mound had once supported a structure of some sort.

The records of the Rogan and Moorehead excavations do not contain the kind of information that is necessary to determine the chronological relationships of the burials that they excavated. Most of the burials recovered by these earlier investigators appear to have been made through the surfaces of the summits of the several mound constructions. In fact, the burials were probably made through the floors

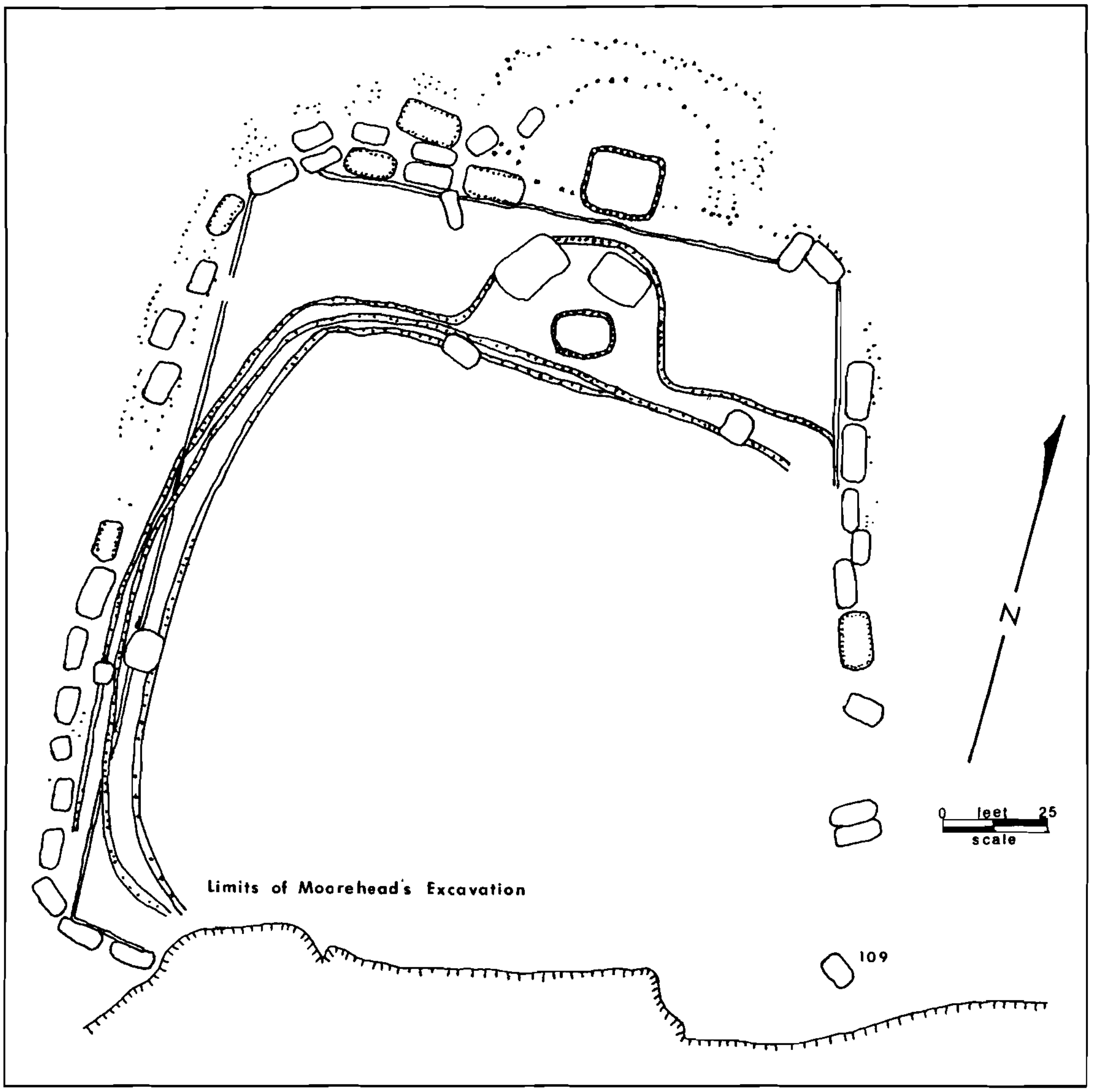


Figure 12.1. Burial plots at base of Mound C, southeast corner, showing Burial #109 in relationship to the other burials in the final sequence.

of the temples that occupied each successive mound summit. The program of excavation of Mound C that I carried out from 1954 through 1961, although it completely removed all of the remaining mound structure, could only systematically investigate the basal portions of the earthwork. Fortunately this effort revealed a pattern of burial placement for many of the burials that had not been disturbed by Rogan or Moorehead. In all some 240 burials were encountered in the mound; however, not all of these burials could be identified as a part of the perceived basal burial patterning. This burial pattern has considerable chronological importance. It would appear that not only were the inhabitants of the Etowah site burying through the floors of structures on the summits of the various Mound C building stages, but they were also burying around basal margins of each of the several mound constructions. Thus the

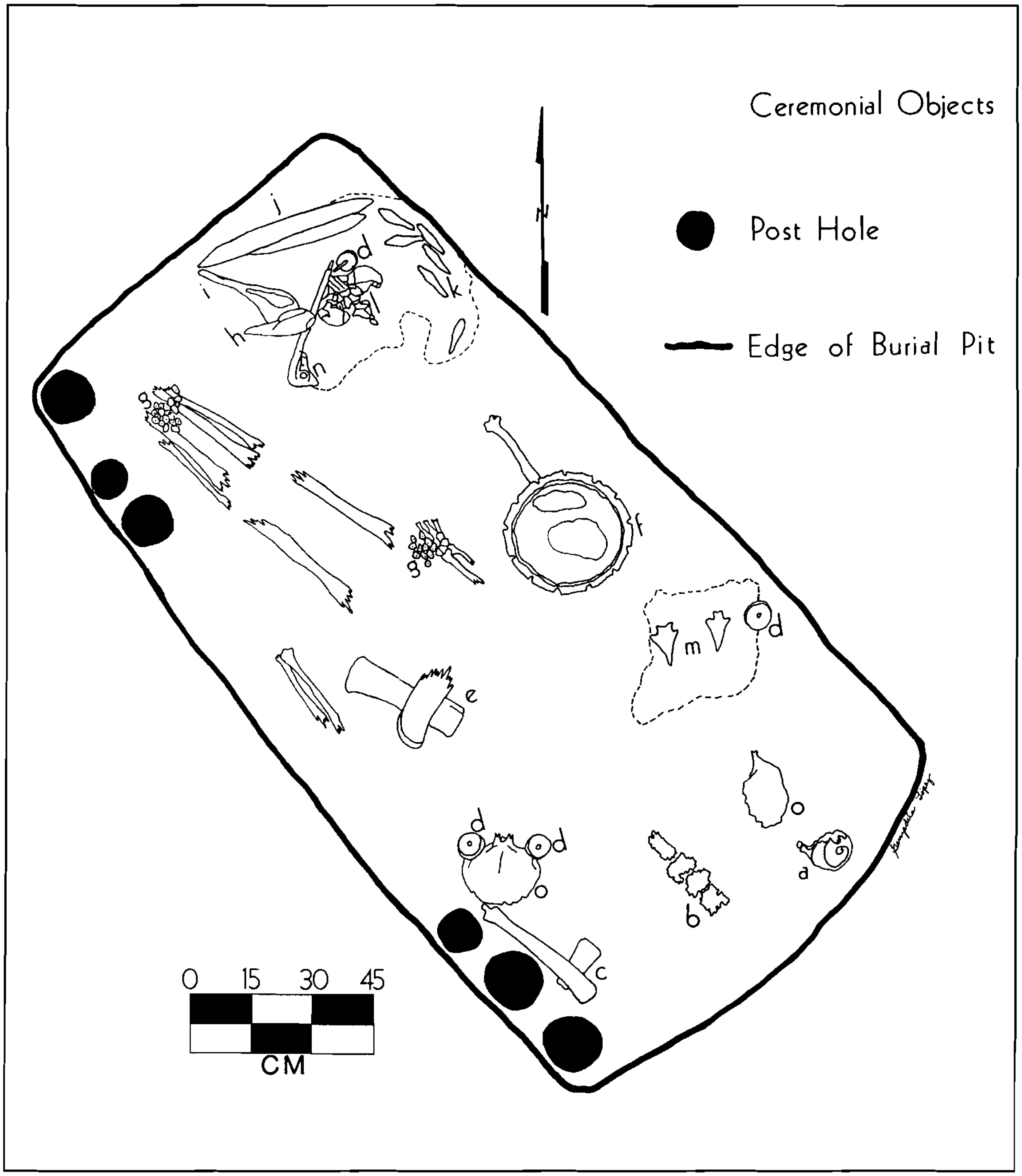


Figure 12.2. Drawing of Burial #109. (a) conch shell bowl; (b) embossed sheet copper baton hair ornament; (c) monolithic axe; (d) wooden copper-covered ear discs; (e) copper celt with portion of preserved wooden handle; (f) stone paint palette on which lie mineral pigments; (g) shell disc beads forming bands at the wrist and ankles; (h) embossed sheet copper flame symbol badge; (i) conch columella pendant; (j) two Dover flint blades; (k) carved wooden and sheet copper covered bear claw rattles; (l) embossed sheet copper bi-lobed arrow hair ornament; (m) embossed sheet copper arrow symbol badges and fragmentary unidentified copper plate; (n) tortoise shell bird effigy hair ornament; (o) skull fragments.

removal of the entire earthwork and the exposure of the mound base revealed a successive series of concentric rings of burials, each encircling what had been the base outline of a mound construction phase (Larson 1971:61).

Included within the last group of burials, i.e., the outer ring correlated with the Wilbanks phase, was one burial with a particularly rich assemblage of ceremonial paraphernalia. This burial, Burial #109, was located on the east side of the mound about halfway between the ramp (also on the east side) and the southeast corner of the mound. Unfortunately, a number of the outer circle of burials on the south side of the mound had been removed by Moorehead as he carried his excavation into and across the mound from that side. This was the only portion of the mound base that was destroyed by his actions (Figure 12.1).

BURIAL #109

Burial #109 lay just beyond the reach of the Moorehead excavation. The burial (Figure 12.2) had been placed in a rectangular pit, 1.22 m wide and 2.44 m long. The pit had been dug approximately 3 m deep into the sterile clay at the mound base. The long axis of the pit was roughly northwest and southeast. Six small postholes were set into the floor of the pit along its western edge. Three each were located at the northwest and southwest corners. Apparently they were vertical posts used to reinforce the western side of the pit. There was no indication of a similar use of posts on the eastern side of the pit or at its ends. Nor was there any evidence of the use of horizontal logs laid over the top of the pit opening in order to seal it. Log tombs were regularly encountered within the final sequence of burials. However, the usual pattern was one with vertical posts supporting all sides of the pit, with additional posts or logs laid across the top of the pit as a cover. Interestingly, log tombs were not used for burials attributed to the sequences of mortuary activity preceding the final, or Wilbanks, sequence.

Traces of badly preserved bone were all that remained of what was apparently a single extended body that lay in the pit created for Burial #109. Initially, I was of the opinion that two bodies lay side by side in the pit, but I am now convinced that there was but a single individual. The displacement of the bone in the pit, e.g., skull parts, was probably a consequence of the shifting of portions of the body attending the burial of a wrapped, partially decayed corpse. The body had been placed so that it was lying parallel to the long axis of the pit. The head was to the south. A preliminary analysis of the skeletal material by Robert Blakely has identified the bone as belonging to a mature adult male (Blakely n.d.).

The body was accompanied by a large number of artifacts. In the southwest corner of the pit, above a fragment of the skull on the western side of the pit was a monolithic ax (Figures 12.2c and 12.3). A copper celt lay across the torso of the individual (Figure 12.2e), while two copper-covered wooden discs lay on either side of the fragmentary skull (Figure 12.2d). These two objects were almost certainly worn fastened to the ears. Each was 45 mm in diameter and each had a rounded boss 27 mm in diameter in the center. This boss projected 5 mm above the surface of the disc. The edge of each disc was 5 mm thick and had a 3mm wide groove incised into it. Similar grooved ear discs were found in other Mound C burials. Several of them had tiny shell beads fitted into the groove around the entire disc circumference. In this instance, however, no shell beads were recovered with the discs. The backs of the discs were gently rounded and each had a perforation, 4 mm in diameter, in the center. Again, based on the recovery of many other copper-covered wooden ear discs, it is certain that straight bone pins, 4 cm to 5 cm in length, were used to fasten the ornaments to the ears, probably by piercing the ear lobes. The pins were inserted into the holes in the backs of the wooden discs (cf. Larson 1959: Figure 2). No pins were found with these two examples from Burial #109.

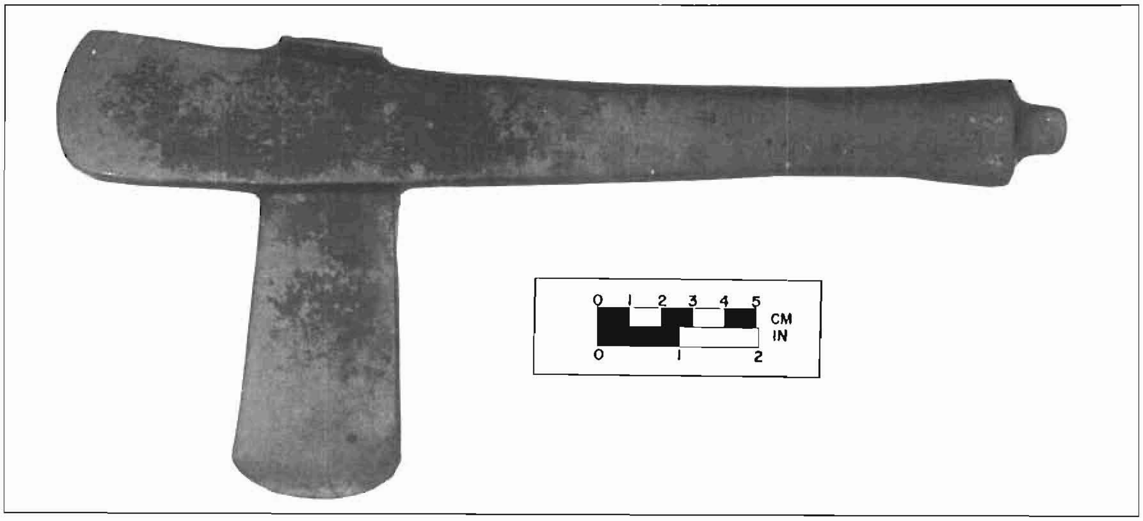


Figure 12.3. Monolithic axe.

Shell beads, the remnants of beaded bands, were scattered in the area of the distal ends of the tibiae and fibulae of the individual (Figure 12.2g). The right and left leg bones lay close together in the northwestern corner of the grave pit. Additional shell beads, again the remnants of beaded bands, lay in the area of the distal ends of the right radius and ulna (Figure 12.2g). An embossed sheet copper baton (Figures 12.2b and 12.4) lay on the floor of the pit in the center of the southeast end of the grave. The baton was in a fragmentary state and badly corroded, so that its length could not be accurately measured. However, it is no less than 230 mm long. Its width, which could be measured, is 63 mm. The base of the ornament had a prepared socket of folded sheet copper, 8 mm wide, into which a flat bone pin must have been inserted in order to fasten it in the hair. Similar sheet copper hair ornaments with similarly mounted bone pins have been recovered from other Mound C burials. In this instance no bone pin was recovered.

The fragments of an almost completely decayed conch shell bowl lay on the floor of the grave immediately above the second skull fragment in the southeast corner of the pit (Figure 12.2a). Poor preservation made a positive identification of the shell impossible, although the indications are that it was probably a *Busycon* species.

Adjacent to the area of the badly preserved bone that constituted the remains of the second (i.e., eastern) skull fragment was a single copper-covered wooden ear disc (Figure 12.2d). It was similar to the two found with the first skull fragment. In this instance the disc was not accompanied by a matching disc to complete a pair. Several centimeters from the ear disc lay the very fragmentary remains of two embossed arrowhead-shaped sheet copper symbol badges (Figure 12.2m).

Elsewhere I have argued that symbol badges constituted parts of headdresses (Larson 1959). In the instance of these particular symbol badges, however, I believe that they were used in an entirely different manner. They lay near an ear disc, and their position suggests that they may have been suspended from it. If this interpretation is correct, these symbol badges would have functioned as parts of ear ornaments rather than parts of a headdress (cf. Phillips and Brown 1978:90, Figure 115).

The ear disc and the two arrow symbol badges lay on fragments of badly corroded sheet copper, apparently the remains of a copper plate. If this was indeed a copper plate, it was so corroded and frag-

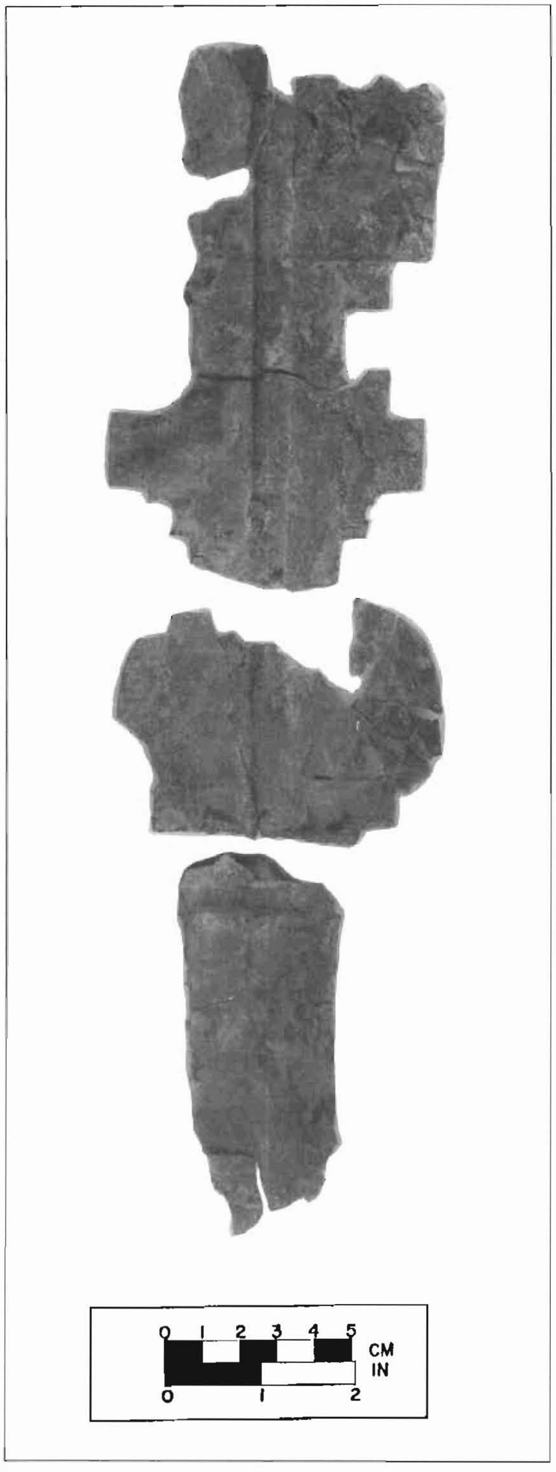


Figure 12.4. Embossed sheet copper baton hair ornament.

mentary that it could not be restored. It was impossible to determine if it had been embossed, nor could its shape be identified.

Lying at the right side of the individual and over his right arm was a scalloped stone disc, 302 mm in diameter (Figure 12.2f). It was almost certainly a paint palette (Figure 12.5). The disc lay with its decorated side up, and on this surface pieces of red ochre and galena had been placed. In at least two other instances where similar discs were found with Mound C burials, the palettes lay face down over lumps of mineral pigment.

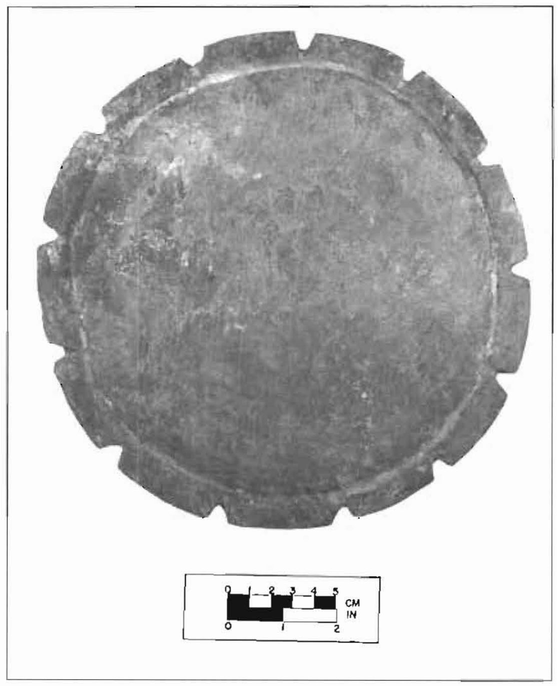


Figure 12.5. Stone palette.

To the right of the feet of the individual lay a large mass composed of many different objects (Figure 12.2d, h, i, j, k, l, n). Most of these objects, by their nature, appeared to be parts of one or more headdresses as well as ornaments designed to be worn singly or freestanding in the hair. As with all other copper and organic materials in the burial, those in this deposit had fallen victim to poor preservation. With the deposit were a number of embossed

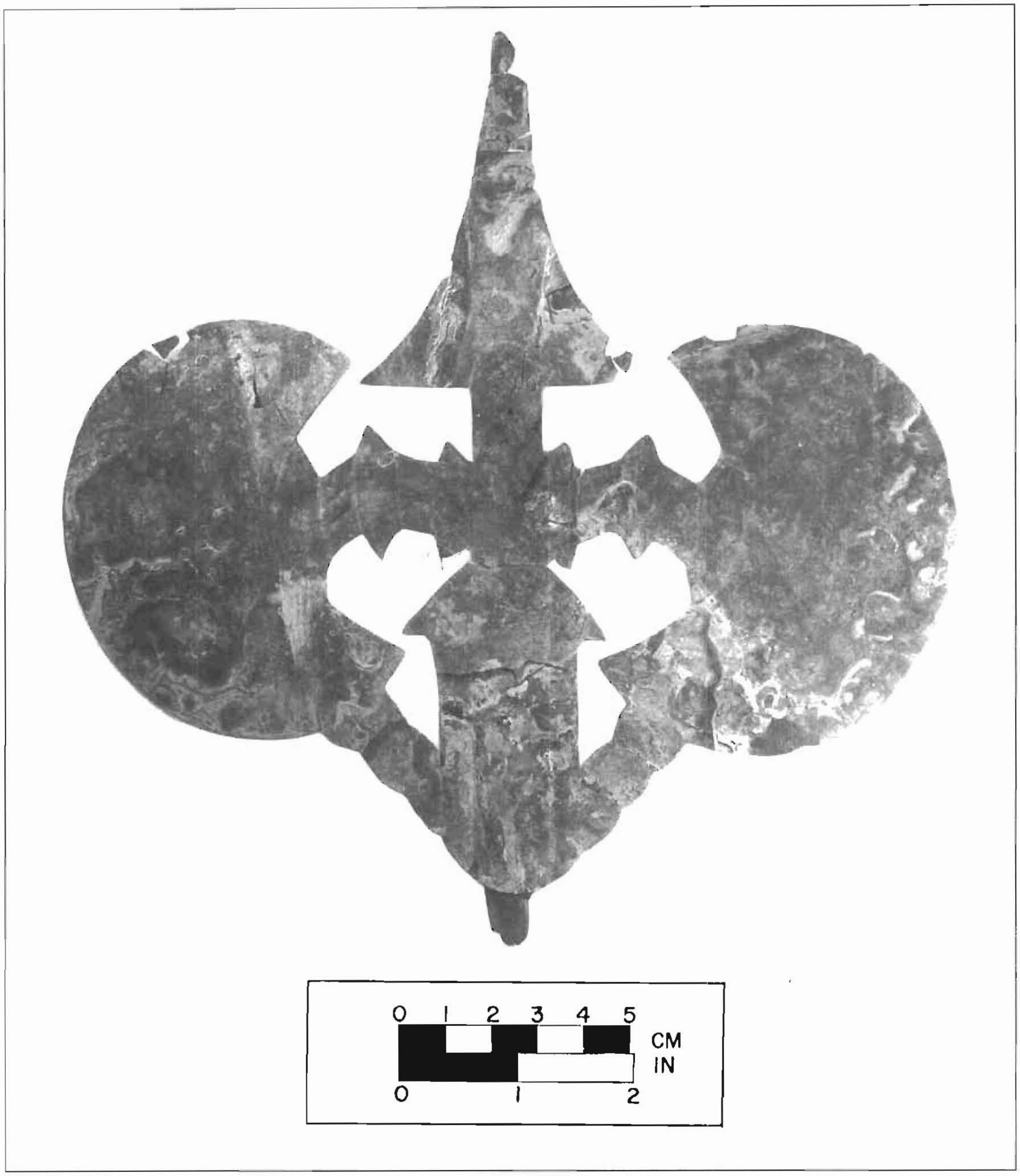


Figure 12.6. Embossed sheet copper bi-lobed arrow hair ornament.

sheet copper objects, including two bi-lobed arrow hair ornaments (Figure 12.21). Both were very fragmentary and almost completely corroded. It was possible to obtain approximate measurements for only one (Figure 12.6) of the ornaments, although the other was about the same size. The one measurable bi-lobed arrow was no less than 250 mm long and 210 mm wide. Both had sockets formed of sheet copper at the base into which were fitted the remains of flat bone pins that had served to hold the ornaments in the hair.

The deposit also included a large piece of sheet copper that appeared to be the cutout of a raptorial bird. To the extent that it was possible to determine, it presented the head of the bird in profile with one of its wings spread. The piece was very badly corroded and broken into many small fragments that made it impossible to tell whether or not the copper had been embossed. There has been no attempt to restore the piece.

Other embossed sheet copper objects in this deposit included five small ornaments that can probably be classed as symbol badges. Two of these appear to be nearly identical, although one is fragmentary and incomplete. The form is that of a bird foot with a portion of the leg attached (Figure 12.7b). Shown in silhouette, the foot is depicted with two toes, each with a distinct talon. Two rectangular extensions protrude from each side of the leg at the top. Three embossed bars cross the leg and appear to form a beaded (?) band encircling it. A cross within a circle, 20 mm in diameter, is embossed on the leg above the band. The unbroken symbol badge has a hole punched through its center 2 mm from the top edge. This hole was presumably for attachment. The unbroken symbol badge was made from two pieces of copper fastened together by a rivet where the foot portion joins the leg. The fragmentary symbol badge can be identified by a one-taloned toe that remains intact.

The use of a bird foot and leg as a symbol badge element is not surprising. Other burials in Mound C produced embossed sheet copper symbol badges that depicted bird heads and bird tails (Larson 1959:109, Figure 1). In addition a large embossed sheet copper representation of a bird wing, not a symbol badge but nevertheless a part of a headdress, lay across the forehead of Burial #20 (Larson 1971: Figure 4).

The remaining three symbol badges are variations on a form that appears to represent a plume in

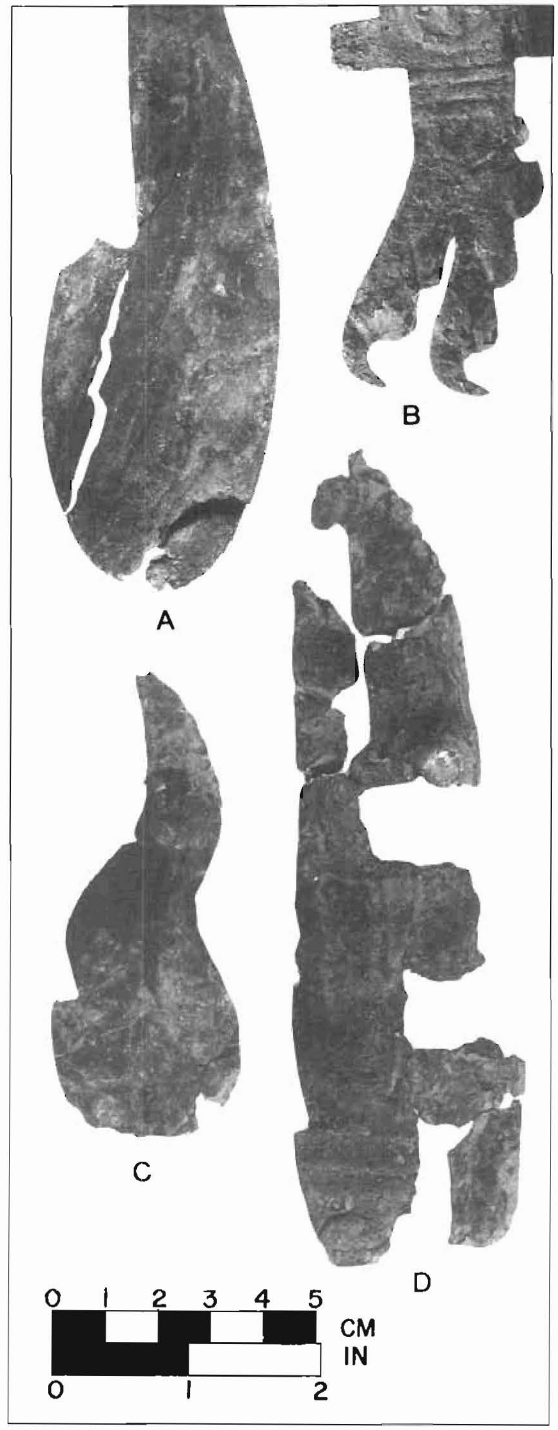


Figure 12.7a. Embossed sheet copper plume symbol badge; 7b. Embossed sheet copper bird talon symbol badge; 7c. Embossed sheet copper plume symbol badge; 7d. Embossed sheet copper plume symbol badge.

outline (Figure 12.7a, c, and d). The two longest of these symbol badges have tiny portions of the bases broken away, and their length cannot be measured accurately. However, they were approximately 142 mm long and 44 mm wide (Figure 12.7a) and 155 mm long and 37 mm wide (Figure 12.7d) respectively. The shortest of the plume symbol badges (Figure 12.7c) is 90 mm long and 48 mm wide. There is a small hole 2 mm in diameter, 7 mm from the bottom edge, in the base of the shortest badge. The other two plume symbol badges probably had similar holes for attachment, but the fragmentary condition of their bases precludes a certain determination. It is of some interest to note that if the holes are indeed for attachment and if the symbol badges of all of the different forms, including not only plumes, but also arrows, batons, bird feet, bird heads, bird tails, and grotesque feline heads (all of these forms were recovered from Mound C burials), were used to ornament headdresses as the Mound C evidence indicates, then my reconstruction of such a headdress is inaccurate in one important detail. I show the reconstructed headdress with the symbol badges fastened to it in an upright position (Larson 1959: Figure 3). The position of the holes in the badges argues to the contary that they would have been in a reversed position when fastened, and they would then appear to have been suspended.

Nine carved wooden, sheet-copper-covered rattles were included in the deposit at the foot of the burial pit (Figure 12.2k). These rattles were more or less identical and were in the form of life-sized bear

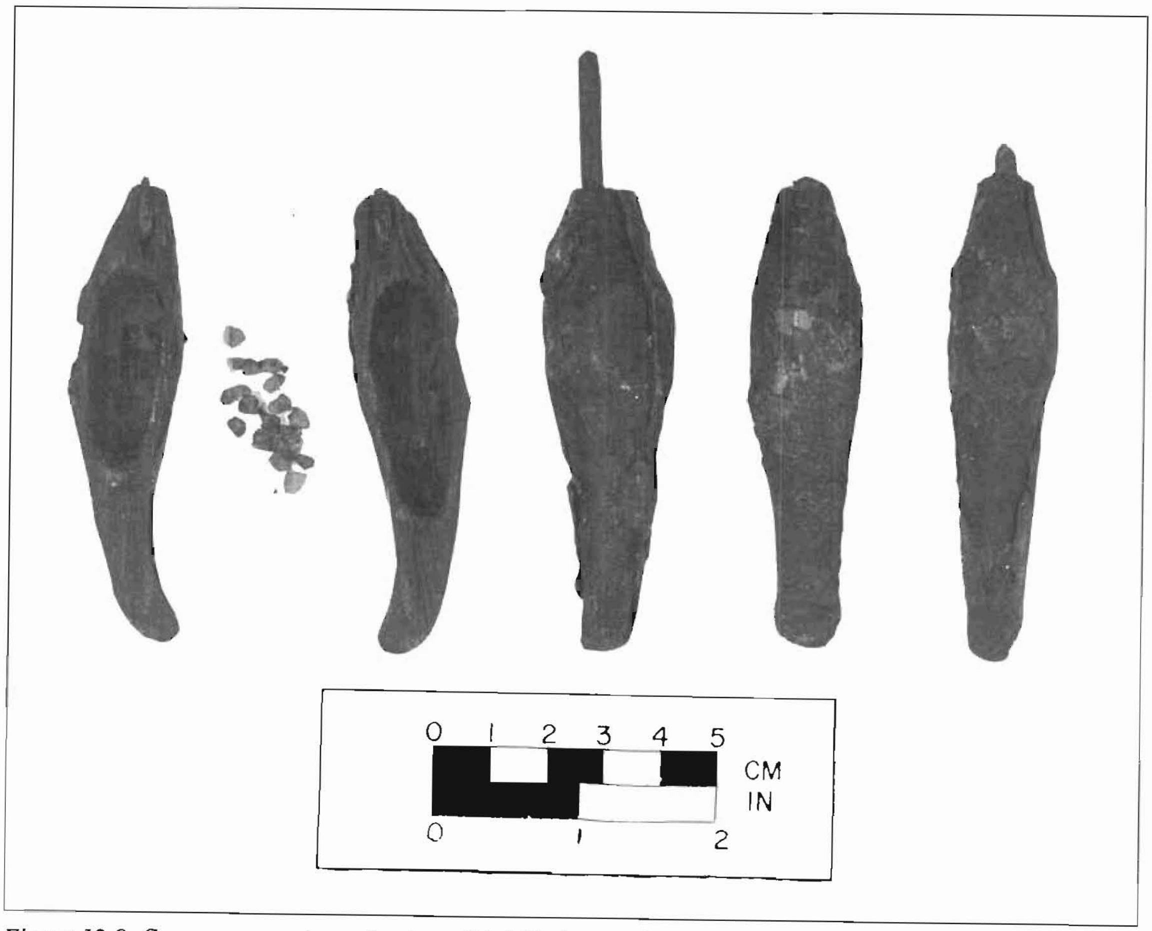


Figure 12.8. Copper covered wooden bear (bird ?) claw rattles.

claws (Figure 12.8). Each rattle was composed of two pieces, halves of the whole claw split longitudinally. A depression was made in the thickest part of each half, so that when the halves were placed together a small cavity was created in the center of each claw. Several dozen tiny pebbles, 2 mm to 3 mm in diameter, were placed in the cavity of each rattle. A bone pin, 4 mm in diameter and 25 mm long, protruded straight out from the base of one of the rattles. Presumably all of the other eight rattles had similar pins for attachment, although evidence of such a pin was found in only one other rattle. Based on their position in the deposit, it was impossible to determine how the rattles were worn or carried.

Clarence Moore recovered almost identical rattles from Burial No. 22, an aboriginal burial of a child, found in a village site on Mason Island, Limestone County, Alabama (Moore 1915:263, Figure 36). Moore solicited the opinion of Charles C. Willoughby regarding the identification of these rattles. In his response to Moore, Willoughby identified the Mason Island ornaments as representations of "the seed pod of some species of the genus *Asclepias* [or milkweed]" (Moore 1915:264). Willoughby also notes that a similar object was recovered by Moore from a mound in Calhoun County, Arkansas (Moore 1909:93, Figure 93) and that there was a pair of such objects from "a stone grave mound, Harpeth River, central Tennessee" and a second pair, "from a burial at Letterman, Arkansas" in the collections of the Peabody Museum, Harvard University (Moore 1915:264-266; Figs. 37-39). Willoughby seemingly based his identification of the rattles as effigies of milkweed pods on the fact that when he split open one of the Harpeth River pair he found that,

> the pebbles, which represent the seeds are carefully placed in position just within the walls of the pod upon a mass of fiber, which fill the remaining space in the cavity. The fiber undoubtedly represents seed down, which is especially conspicuous in the milkweed. This fiber is now of a deep brown color (Moore 1915:264).

An examination of the photograph of the split rattle reveals that the end of the object had been decayed or broken away, exposing the interior cavity. I believe that an argument can be made that what Willoughby supposed to represent milkweed "seed down" was in fact tiny root hairs that had penetrated the cavity of the ornament sometime after it had been buried.

It should be noted that the rattle from Calhoun County, Arkansas and the Harpeth River pair of rattles are not entirely similar to the Mason Island and Letterman, Arkansas pairs or to the nine rattles from Etowah Burial #109. The Calhoun County and the Harpeth River pair had holes for suspension at the tops of the objects, whereas the Mason Island, Letterman, and Etowah specimens had tiny rods issuing from their bases. The latter rattles are also differently shaped. They resemble bear claws, while the first three rattles resemble bear canines (or milkweed pods). Whether or not these differences in shape and what appears to be the manner of fastening indicates a difference in function is difficult to determine.

Other objects in the deposit included two teeth of an unidentified large species of shark, three whelk columella pendants in a condition too fragmentary to allow measurement, fragments of ornaments cut from turtle shell, and a variety of fragments of cordage, fiber, cloth, and cane basketry or matting.

Lying between the northwest end of the grave pit and against the bundle of material comprising the deposit at the feet of the buried individual were two Dover chert blades (Figure 12.2j). They have the typical "flint sword" shape. Although they are not particularly long examples of this class of artifact, the largest of the two blades from Burial #109 is 395 mm in length and 42 mm wide. The other blade is 260 mm long and 42mm wide. The point of each blade is abruptly tapered to a relatively long and narrow point. The basal ends are rounded. The edges of the blades at the basal ends have been ground to facilitate holding in the hand, in the manner that they are held by the costumed figures shown engraved on the eastern Tennessee shell gorgets.

With the exception of one additional artifact to be described shortly, this completes the description of Burial #109 and the inventory of objects found with the individual placed in the grave pit. I have presented this description in some detail first because so few of the burials found in Mound C during its final period of excavation activity have been described in print and, second, because the grave goods represent such a wide range of Southeastern Ceremonial Complex materials occurring in a context that leaves no doubt about their collective contemporaneity. It is, however, the chronological position of these grave goods that has been the stimulus for this paper.

TURTLE SHELL HAIR ORNAMENT

Among the artifacts found with Burial #109 was the additional object referred to above. I have selected it for particular attention and discussion because I believe that it has a singular significance for understanding the temporal position of Burial #109 and, ultimately, all of the burials comprising the final grave sequence encircling the base of Mound C.

The significant artifact (Figures 12.2n and 12.9) is a zoomorphic effigy, cut from marine tortoise shell (technically the *carey*, or thick horny scales that overlie the bones of the carapace). Although the turtle species that was the source of the shell has not been identified, it is in all likelihood the Atlantic hawksbill turtle, *Eretmochelys imbricata imbricata* (Linné). The artifact was recovered from the deposit of materials lying at the feet of the buried individual (Figure 12.2n). It appears to have been used as a hair ornament, although that assertion is based on its form rather than any apparent functional determination that could be made from the context in which it

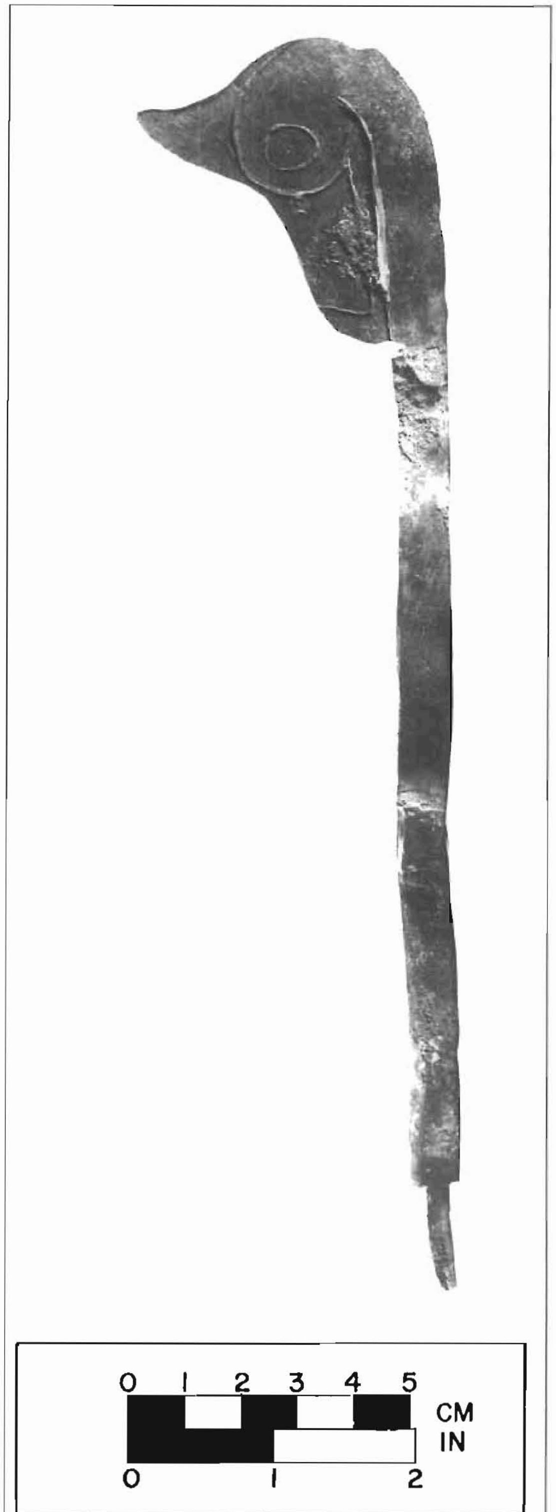


Figure 12.9. Tortoise shell bird hair ornament.

was found. The ornament is 24 cm long over all. It consists of a shaft, 18 cm in length and 1.1 cm wide, at the end of which a crested bird head is portrayed in profile. The head turns down on a long neck that forms the shaft of the pin. Features of the head, the eye and the beak, are delineated by engraved lines. The crest on the head is indicated only in outline, for there are no engraved lines that serve to define it further. The beak is blunt and defined by an engraved line that connects with an engraved circumocular line. The eye itself is indicated by an engraved circle. The head is separated from the neck for a short distance under the middle of the beak by a line that cuts all the way through the tortoise shell. The surfaces that comprise the head of the bird on both sides of the pin are engraved in an identical manner so that the bird can be identified as such when either side of the pin is seen. This, of course, suggests that the pin was employed in a manner that permitted both sides to be seen.

The shaft terminates in a curious stepped and squared end rather than a tapered point. The species identification of this bird and the birds represented on the similar pins discussed below is at best conjectural. Nevertheless, I believe that the most likely species is the belted kingfisher (*Megaceryle alcyon*, Linné), a suggestion first made by Goggin (n.d.:580). A number of other commentators have

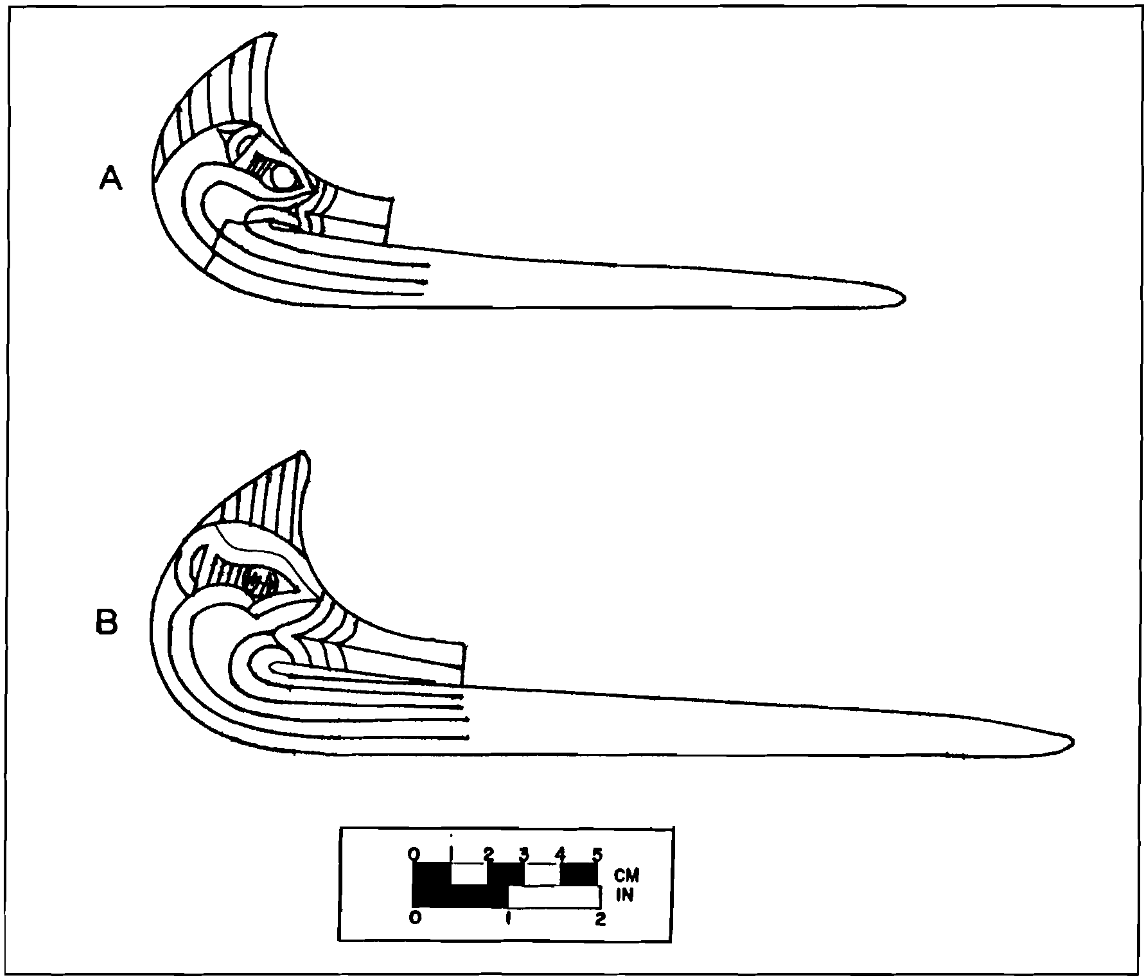


Figure 12.10a. Manatee County gold bird pin redrawn from Holmes (1883:287, Pl. LX2). 10b. Wakulla County gold bird pin redrawn from Goggin (1947:274, Fig. 75f).

suggested that it is one of the large crested woodpeckers, perhaps the Ivory-billed Woodpecker (*Campephilus principalis*, Linné) or the Pileated Woodpecker (*Dryocopus pileatus*, Linné) (cf. Rau 1878:299; Holmes 1883:285). Another possibility is that the representation on the ornament was intended by the artisan to be the head of the Wood Duck (*Aix sponsa*, Linné).

DISCUSSION

Interest centers on the turtle shell bird pin from Etowah because it is similar in artistic concept, and presumably in function, to a series of metal artifacts from Florida. The Florida artifacts, copper and gold pins ornamented with bird heads (Figure 12.10a), were first commented on almost one hundred years ago by Charles Rau in a brief published report on one of the pins (Rau 1878). The provenience of the pin discussed by Rau is identified only as "a mound in Manatee County, Southern Florida" (Rau 1878:298). The description of the pin, however, is given in considerable detail.

> [The Manatee County pin measures] exactly nine inches [22.86 cm] from the point to the middle of the opposite curve. It is cut from a flat piece of gold plate not quite a millimeter in thickness and somewhat thinner toward the edge. The specimen is broken in two pieces, . . . but the two parts fit well together, and thus the original character of the object remains unaltered. On the whole, it is in a good state of preservation though the effects of long exposure are plainly visible. Both faces appear bright and smooth, and the engraved lines which represent exactly the same pattern on both sides seem to be as fresh as on the day when they were traced (Rau 1878:298).

The metal used in the manufacture of the pin is described as consisting, "exclusively of gold and silver, in the proportion of 893 parts of gold to 107 parts of silver" (Rau 1878:299). Rau further characterizes the pin as having been hammered into shape from a large gold coin, a bar of gold, or a piece of sheet gold, because the surfaces of the pin appeared to have "undergone the process of beating" (Rau 1878:300). The incised lines on the surface of the pin were, Rau argues, the product of aboriginal craftsmanship and presumably not European.

> The ornamental lines, though incised with a steady hand, are not uniform in width, and in some places the tracing forms a double line, as though the implement used in lieu of a graver had not been provided with a sharp point. A knife which has lost its extreme point would produce such lines; perhaps also a pointed flint (Rau 1878:300).

There are a number of other examples of the metal pins of the same or similar configuration and style from Florida. Five of the pins for which there are measurements available are within a 6 cm range of length of each other, 22.2 cm to 28 cm (Goggin n.d.:580-581). Three of the specimens are between 22.2 cm and 23.8 cm long, a length that compares favorably with the 24 cm length of the Etowah pin. John Goggin describes and illustrates one of these pins from a site (8Wk11) located on Apalachee Bay within the St. Marks National Wildlife Refuge, Wakulla County, Florida (Goggin 1947:273).

> The crested bird ornament is a figure of a head cut out of sheet copper and incised with deep lines which reproduce similar features on each side [Figure 12.10b]. Its length is 28 cm with a thickness of about 0.5 mm. The exact identification of the bird represented has not been made, but it has been suggested that it is an ivory-billed woodpecker. Associated with this ornament is a small embossed circular disc of gold, 1.3 cm. in diameter, which apparently was

> cemented to the specimen forming an eye of contrasting color. Since the specimen was made, a small hole has eroded beneath the eye (Goggin 1947:273-274).

The ornament, along with a number of other artifacts, reportedly came from a burial or burials at the site excavated by a collector in the late 1930s. The other objects recovered, in addition to the bird pin, included "a circular gold ornament, . . . a decorated copper plate, an embossed copper gorget, a plain copper gorget, a silver and a copper bead, and European objects including a mordaunt, glass beads, and trade bells" (Goggin 1947:273). Goggin did not see the copper plate, but he reports that it was "ornamented by incision . . . [with] human and animal figures depicted on the surface" (Goggin 1947:274). The nature of the design on this plate, found in a fragmentary condition, does not seem to conform to any of those designs on copper plates usually attributed to the Southeastern Ceremonial Complex. Insofar as I am aware, none of the Southern Cult copper plates have designs utilizing human *and* animal figures. Further, the Southern Cult copper plates are usually described as embossed rather than incised, a distinction of which Goggin was certainly aware.

In this regard it might be noted that a copper plate from a site in Gordon County, Georgia has recently come to the attention of archaeologists working in north Georgia. Engraved on its surface are two human figures and one quadruped, elements that apparently constitute a single scene. The manner in which the human figures are represented argues a post-contact date for the plate. Neither the appearance of the figures nor the style employed in their representation seems to have any relationship to the Southeastern Ceremonial Complex. If Goggin was correct in his characterization of the design of the Wakulla County copper plate, and considering the nature of the artifacts with which this plate was supposedly associated, it may well be that the Wakulla County plate has more in common with the Gordon County, Georgia plate than with the copper plates that are usually identified with the Southeastern Ceremonial Complex.

The other objects from the Wakulla County, Florida site are illustrated by Goggin (1947: Figure 75), but none of them can be related specifically to objects on the list of Southeastern Ceremonial Complex paraphernalia compiled by Waring and Holder (1945).

Goggin notes that in addition to the north Florida (Wakulla County) example, seven crested bird pins have been found in the south Florida area. He identifies four sites located between Charlotte Harbor and Lake Okeechobee–Gopher Gully, Curiosity Hammock, Bee Branch I, and Punta Rassa–as having produced that artifact type (Goggin 1947:275 and Figure 74, n.d.:580-581). In a paper focusing on a different subject matter, Allerton, Luer, and Carr identify three sites, 8L17, 8Pb40, and 8Gl9, in the same general area that have produced crested bird pins (Allerton, Luer, and Carr 1984:28 and 30). I assume that one or more of these three sites are included in the four that Goggin names, although I am unable to correlate the two lists.

The catalog of the exhibit *Sacred Circles: Two Thousand Years of North American Indian Art*, organized by the Arts Council of Great Britain, illustrates a crested bird pin from the collections of the University Museum, Philadelphia. It is identified as made of silver and from Punta Rassa, Florida (Coe 1976:64, #19). The caption on the catalog photo, however, is confused and seems to be referring to the Florida artifact type known as the "ceremonial tablet" rather than to the crested bird pin shown in the illustration. This is undoubtedly the pin from the Punta Rassa site to which Goggin refers in his 1947 paper.

The function of the Florida crested bird pins is somewhat less equivocal than the Etowah example. From a description provided by Goggin, it would appear that at least one of the objects was worn on the head, probably as a hair ornament.

> A strip of thin silver about 23 cm. long and less than 2 cm. wide was used as a head band. It was tied on by means of cords which passed through a double perforation in each end of the band. This specimen was collected by Montague Tallant in a burial mound in Glades County. When found it was around a skull, and a metal crested bird effigy lay between the band and the skull (Goggin n.d.:79).

All of the crested bird pins from Florida for which there is any information available are made of precious metals or copper. The use of gold or silver or of copper in combination with one of the precious metals, e.g., the gold eye cemented to the copper body of the Wakulla County specimen, argue that the pins can, almost certainly, be placed in the period following the onset of Spanish exploration in the New World, most probably after A.D. 1521.

The Florida crested bird pins, with the exception of the Wakulla County specimen, seem to be products of Calusa craftsmanship. They occur in the area and at the time of the documented Calusa occupation in Florida. The pins, while supposedly of metals that originated in Mexico or South America, exhibit nothing in concept or style that points to the manufacture of the pins in those areas. In the sixteenth and seventeenth centuries the Calusa regularly salvaged materials, including gold and silver, from the wrecks of Spanish ships driven onto the Florida coast by storms. Thus the metal employed in the manufacture of the crested bird pins was available to the Calusa. Other objects made of gold and silver attributed to the Calusa have been found frequently in south Florida. They readily attest to the probability that the Calusa are also responsible for the crested bird pins. The familiar "ceremonial tablets" are such an example (Allerton, Luer, and Carr 1984).

CONCLUSIONS

The similarities between the Etowah crested bird pin and the Florida crested bird pins are too great to be dismissed as fortuitous and without interest. The coincidence in concept, style, form, and presumably function suggests a relationship that is both temporally and culturally significant.

The use of sea turtle shell at Etowah was not confined to the crested bird pin. Several of the burials comprising the final sequence in the Mound C mortuary activity were accompanied by objects crafted of this material. Some of these are pieces of what appear to be exaggerated effigies of bird wings with lines delineating the feathers engraved in the same manner as the features of the crested bird pin. Although there was no duplication of the crested bird pin in other burials, the other objects of turtle shell were undoubtedly parts of ritual regalia. Thus, the use of the marine turtle shell at Etowah, while not a common occurrence, was not a singular circumstance. It is probable that this nonlocal material circulated within the southeastern exchange system that brought other exotic raw materials and craft goods–the marine shell, the shark teeth, the Dover flint, and the negative painted ceramics–to the Etowah site during the Wilbanks phase.

Although the various large marine turtle species are found in the nearshore and inshore waters along most of the southeastern coast, the Gulf coast of south Florida is the most likely source for the marine turtle shell distributed among the southeastern Indians during the Mississippi Period. Most of the southeastern archaeological sites from which identified skeletal remains of *Cheloniidae*, the sea turtle family, have been recovered are on the Florida peninsula. A disproportionately large number of these sites is in southwest Florida. There are several possible explanations for this perceived distribution, but one that must be considered is that the Calusa were engaged in a sea turtle fishery, of which one product was turtle shell that entered into the southeastern exchange system. It should be noted, however, that if the species responsible for the shell found at Etowah is the Atlantic hawksbill turtle, then the fishery may well have been located in the Florida Keys (Larson 1980:128). Nevertheless, the Florida Keys area was within the range of Calusa cultural and exchange relationships. It is even possible that the Calusa cacique may have exercised economic and political hegemony over the Keys. Laudonnière would seem to suggest such dominion in an account of his questioning two Christians ransomed from the caciques Onathaqua and Mathiaca.

> examining them about the places to which they could have been and how they had come, they answered that fifteen years ago, that three ships, in one of which they were, were lost across from a place named Calos [i.e., Calusa] on shallows that are called the *Martyres* [i.e., the Keys] and that the King of Calos took most of the riches which were on the said ships . . . (translated from Basanier 1586:72 recto).

In summary, the Burial #109 turtle shell pin from Mound C at Etowah is significantly similar to a class of crested bird pins from the Calusa area of south Florida. The similarity is to be seen in form, design, and size of the ornament. The material from which it is made, marine turtle shell, also seemingly links the Etowah specimen to the Calusa area. The use of precious metals in the manufacture of the Florida ornaments establishes a post-contact date for their occurrence, certainly post A.D. 1492, and probably post A.D. 1521. The chronological position of the Florida specimens, in turn, argues for an equally late date for the Etowah pin. How late the Etowah pin dates is difficult to determine, but it cannot be too distant temporally from the Florida artifacts. The unequivocal association of the Etowah ornament with the consequential list of materials ascribed to the Southeastern Ceremonial Complex leads to the inevitable conclusion that these Cult materials are also late in the prehistoric period or very early in the contact period.

I am well aware of the extent to which this conclusion conflicts with the interpretation of the radiocarbon dates for Mound C and for related complexes throughout the Southeast (e.g., Hally and Rudolph 1986:21-26). However, I would argue that it is possible to interpret these same radiocarbon dates in a manner that sees them as compatible with the conclusion that the Wilbanks complex is late 15th century to early 16th century in time. It can be demonstrated that if the existing radiocarbon dates for the Wilbanks complex from Mound C at Etowah are plotted with two standard deviations, the resulting ranges for the dates for the Wilbanks complex are not in conflict with an argument that Wilbanks is late, based on evidence of the turtle shell pin. These dates, all from the University of Michigan laboratory, include M-402: A.D. 1225 ± 200 (Crane and Griffin 1959); M-542: A.D. 1040 ± 200 (Crane and Griffin 1959); M-543: A.D. 1450 ± 250 (Crane and Griffin 1959); and M-1060: 1725 ± 130 (Crane and Griffin 1962). The problems with the interpretation of radiocarbon dating have been discussed by Michael J. Shott (1992), who suggests an approach that will lead to a refinement in the use of radiocarbon dates. The reader is referred to that discussion.

ACKNOWLEDGEMENTS

I wish to express my gratitude to the following individuals who have been of great assistance in the preparation of this paper: Mr. Thomas Beggs of the West Georgia College Learning Resources Center who prepared the photographs and Ms. Genzadela López who executed the drawings. Ms. Sharon McCormick deserves my particular gratitude because she has been involved in every phase of the preparation of this paper from proofreading to assistance in bringing the illustrations to their final form. Richard Dangle, Dean of the School of Arts and Sciences, West Georgia College, kindly provided the funds necessary for the preparation of the drawings.

REFERENCES

Allerton, David, George M. Luer, and Robert S. Carr
1984 Ceremonial Tablets and Related Objects from Florida. *The Florida Anthropologist* 37(1):5-54.

Basanier, M., editor
1586 *L'Histoire Notable de La Floride Situee es Indes Occidentales, contenant les trois voyages faits en icelle par certains Capitaines & Pilotes Francois, descrits par le Capitaine Laudonnière, qui y a commandé l'espace d'unan trois moys: a laquelle a esté adiousté un quatresme voyage fait par le Capitaine Gourges.* G. Auuray, Paris.

Blakely, Robert
n.d. Etowah Skeletal Analysis: Mound C Burial Numbers 1-140. Unpublished skeletal analysis data on file at the West Georgia College Archaeological Laboratory.

Coe, Ralph T.
1976 *Sacred Circles: Two Thousand Years of North American Indian Art.* Arts Council of Great Britain.

Crane, H.R., and James B. Griffin
1959 University of Michigan Radiocarbon Dates, IV. *Radiocarbon* 1:173-198.
1962 University of Michigan Radiocarbon Dates, VIII. *Radiocarbon* 3:105-125.

Goggin, John M.
1947 Manifestations of a South Florida Cult in Northwestern Florida. *American Antiquity* 12(4):273-276.
n.d. The Archaeology of the Glades Area, Southern Florida. Unpublished manuscript typescript, on file at the Florida State Museum.

Holmes, William H.
1883 Art in Shell of the Ancient Americans. *Second Annual Report of the Bureau of Ethnology, Smithsonian Institution,* 179-305. Washington, D.C.

Larson, Lewis H., Jr.
1959 A Mississippian Headdress from Etowah Georgia. *American Antiquity* 25(1):109-112.
1971 Archaeological Implications of Social Stratification at the Etowah Site, Georgia. In *Approaches to the Social Dimensions of Mortuary Practices,* edited by James A. Brown, 58-67. Society for American Archaeology Memoir 25.
1980 *Aboriginal Subsistence Technology on the Southeastern Coastal Plain During the Late Prehistoric Period.* Ripley P. Bullen Monographs in Anthropology and History, No. 2. University Press of Florida, Gainesville.

Moore, Clarence B.
1909 Antiquities of the Ouachita Valley. *Journal of the Academy of Natural Sciences of Philadelphia* 14:1-170. Philadelphia.
1915 Aboriginal Sites on Tennessee River. *Journal of the Academy of Natural Sciences of Philadelphia* 16:170-428. Philadelphia.

Moorehead, Warren K.
1932 Exploration of the Etowah Site. In *Etowah Papers.* Yale University Press, New Haven.

Phillips, Philip, and James A. Brown
1978 *Pre-Columbian Shell Engravings from the Craig Mound at Spiro, Oklahoma.* Peabody Museum Press. Cambridge.

Rau, Charles
1878 Observations on a Gold Ornament from a Mound in Florida. *Annual Report of the Board of Regents of the Smithsonian Institution, for the Year 1877,* 298-302. Washington, D.C.

Sears, William H.
1958 The Wilbanks Site (9CK5), Georgia. *Bureau of American Ethnology Bulletin* 169:129-194. Smithsonian Institution, Washington, D.C.

Shott, Michael J.
1992 Radiocarbon Dating as a Probabilistic Technique: The Childers Site and late Woodland Occupation in the Ohio Valley. *American Antiquity* 57(2):202-230.

Thomas, Cyrus
1887 Burial Mounds of the Northern Sections of the United States. *Fifth Annual Report of the Bureau of Ethnology.* Smithsonian Institution. Washington, D.C.
1894 Report on Mound Explorations of the Bureau of Ethnology. *Twelfth Annual Report of the Bureau of Ethnology.* Smithsonian Institution. Washington D.C.

13 Some New Interpretations of Spiroan Culture History

Frank F. Schambach
Arkansas Archeological Survey
Magnolia, Arkansas

The standard interpretation of the Spiro site–in effect, the paradigm for the archaeology of the Arkansas Valley in Eastern Oklahoma and Western Arkansas–is that Spiro was a Caddoan site, indeed: "The principal and most famous site in the Caddoan area" (Brown 1984a:241), at the apex of "a distinct subregional tradition . . . known as the Arkansas Valley Caddoan to distinguish it from historically related traditions in the Red River Valley" (Brown, Bell, and Wyckoff 1978:170, 194-195). For some years now (Schambach 1988, 1990b, c, d), I have been challenging this interpretation–the Northern Caddoan Area paradigm–on the grounds that there is no documentary evidence and no good archaeological evidence for a Caddoan connection of any sort other than trade. In my view the basic biological and cultural ties of this tradition, which I will call the Arkansas Valley tradition, were, as Bell has speculated (1984a:239), to the east with peoples of the Central Mississippi Valley, not to the south with the Caddoan area or to the west with the Wichita. Contrary to an earlier suggestion of mine (1990b, c, d), the people of this tradition appear to have had a long tenure in the Arkansas Valley, dating back at least to the beginning of the Woodland period (ca. 500 B.C.), when they developed or acquired traits that set them apart from the peoples of the Central Mississippi Valley. I suspect that this tradition was a part, at least, of the long lost ancestral Tunican tradition.

In the only published response to my challenges to date (a stimulating one, if this paper is any indication), Rogers (1991b:65) expressed the view that my "argument that the Ozarks and the Arkansas Basin region were not Caddoan threatens to become a debate over terminology with no foreseeable resolution and very few analytical implications." I, of course, am not so pessimistic. I suspect that Arkansas Valley specialists will soon be forced to abandon their last-ditch argument–pointless in my view–that if the peoples of the Arkansas Valley tradition "were not Caddo, as in the direct ancestors of the historic Caddo" they were "at least linguistically . . . Caddoan" (Rogers 1991b:65). And I think there are many good reasons for eliminating this concept root and branch, beginning with Linnaeus's advice that "The first step of science is to know one thing from another." Long a red herring of major proportions in Southeastern archaeology, it has led, inevitably, to basic errors in our interpretations of Spiro and its place in Southeastern prehistory, in our interpretations of Arkansas Valley culture history, and in our interpretations of the archaeology of what I consider (because it is historically documented) the real Caddoan area of southwest Arkansas, northwest Louisiana, northeast Texas, and southeast Oklahoma.

In this paper I will review and reinterpret various data pertaining to a series of interrelated topics that are pertinent to the basic questions of the identity of

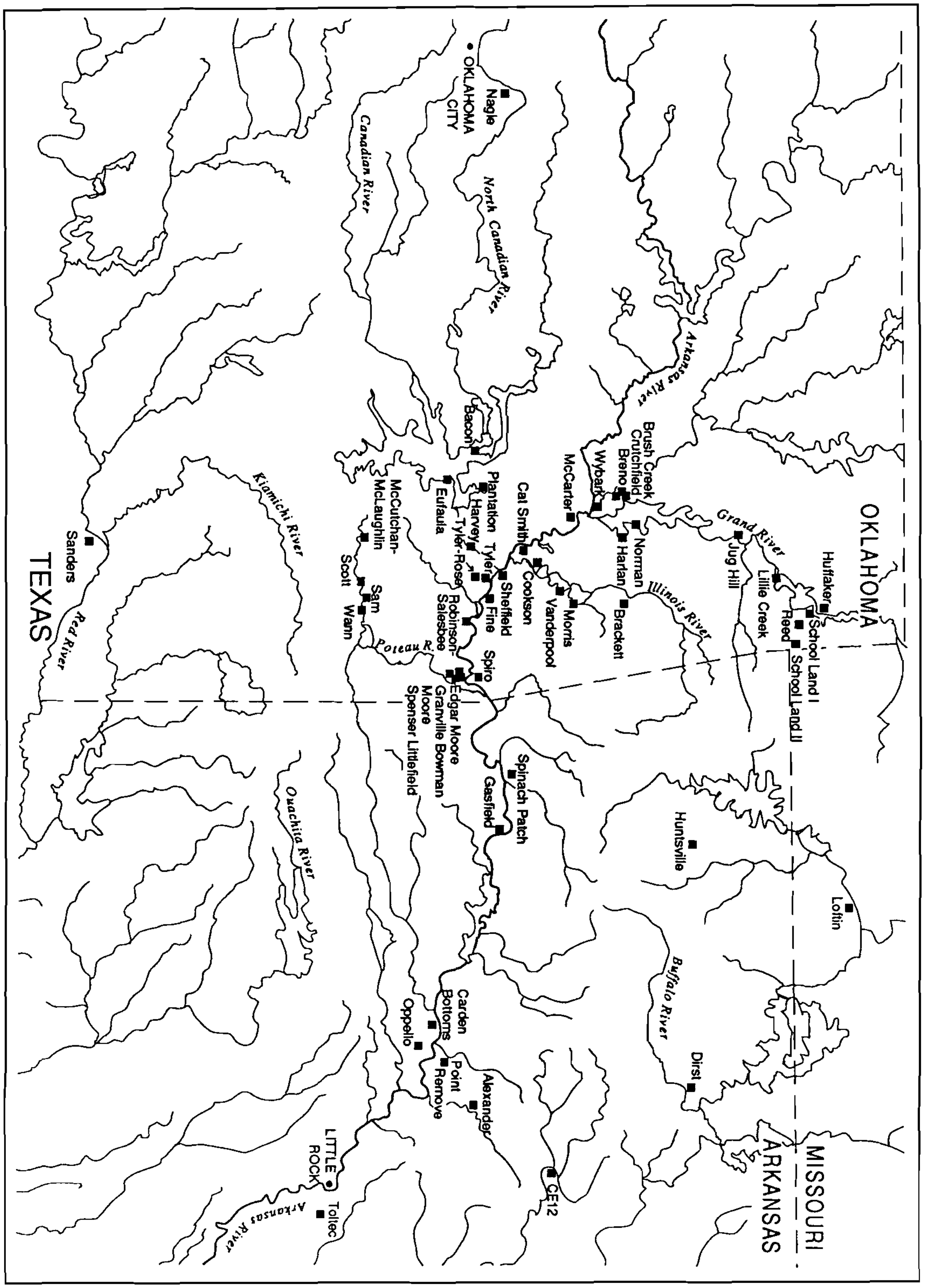


Figure 13.1. Map showing the main sites discussed in the text.

the people of the Arkansas Valley tradition and of their role in the culture history of the Southeast (Figure 13.1). Along the way I will try to correct some misconceptions that I have introduced by, it now appears, shooting from the hip in some of my earlier papers. I hope that the result of this exercise will be something on the order of a new paradigm for Arkansas Valley archaeology, one that intersects with archaeological reality at more points than the old one, which, as I will try to show, no longer intersects at all, although it once seemed to, to all of us. I will begin with a sketch of the Arkansas Valley tradition as it existed in full form during the Mississippi period, from ca. A.D. 1000 to the 16th century. It is based on data that are presented fully in succeeding sections, hence the paucity of references and justifications for some possibly startling statements.

THE ARKANSAS VALLEY TRADITION

The Mississippi period culture of the Arkansas Valley tradition of eastern Oklahoma–which I will call Spiroan culture, following Phillips and Brown (1978:9 and 10) and Rohrbaugh (1984:272)–has some of the basic characteristics of a Middle Mississippian culture, such as platform mounds, burial mounds, rectangular wattle-and-daub houses, charnel houses, a small-village settlement pattern, shell-tempered pottery, red-slipped pottery, storage pits, and hoe horticulture. But combined with these there are certain local variations and certain basic traits derived from the Southwest, the Lower Mississippi Valley, and the Ozarks (not, I think, from the Caddoan area except for traded pots and perhaps a few other traded items) that set it off as the culmination of a distinct regional tradition, not Caddoan, certainly, but not standard eastern Arkansas Middle Mississippian either, that emerged in the Arkansas Valley and the adjacent Ozark Highlands of western Arkansas and eastern Oklahoma no later that 500 B.C.

First, and perhaps foremost, it seems to have had a significantly more diverse subsistence system than traditions to the east or south. This system, which was only marginally efficient according to good bioarchaeological data, featured hoe horticulture of most of the plants of the old Woodland period "Cultivated Starchy Seed Complex" of the Ozark highlands plus some corn. There were three Southwestern cultigens: *Amaranthus hypochondriacus*, *Cucurbita mixta* and a "non eastern complex corn" (Fritz 1989:80-86; 1990:9-11). Bison hunting for food, hides, and bone tools such as scapula hoes was a part of the economy, probably an important one, by no later than A.D. 1100.

The fortified village sites common in the Central Mississippi Valley have not been found. The flat-topped mounds of Spiroan culture were not used as foundations for temples or other special-purpose structures in the Middle Mississippian (and Middle and Late Caddoan) manner. The sophisticated square to rectangular wattle-and-daub houses with two or four center posts and extended, wall-trenched entrances that are characteristic of this tradition have not been found in comparably early contexts farther east, do not occur in the Caddoan area south of the Arkansas, and probably had a Southwestern origin, as Webb (1959:63-64) argued more than thirty years ago (see also Bell in Davis, Wyckoff, and Holmes 1971:82).

Preserved specimens from Spiro (Brown 1976: 10-12) and various Ozark bluff shelters (Scholtz 1975:30-44) attest to a coiled basketry tradition, an oddity for a Southeastern culture, that probably came from the Southwest (Griffin 1952:102). Coiled basketry impressions on countless bases of flat-bottomed, grog-tempered, and shell-tempered jars indicate that the tradition was both lengthy and widespread. Perhaps because coiled baskets that could serve in lieu of pots were available, the ceramic tradition was significantly weaker in terms of the quantities of pottery in use than that of either the Central Mississippi Valley or the Caddoan area. The vessel form that dominated this tradition–the flat-bottomed jar or bowl, as opposed to the round-bottomed bowls and jars of the Central Mississippi

Valley–probably originated in the Lower Mississippi Valley and probably entered the Arkansas Valley tradition during the Middle Woodland period. The decorated Lower Mississippi Valley types French Fork Incised and Coles Creek Incised (Brown 1984b: Figure 4) that occur as minority types, probably trade vessels, in Evans and Harlan phase assemblages in eastern Oklahoma are also Lower Mississippi Valley, as opposed to Central Mississippi Valley, traits. They probably entered the Arkansas Valley tradition via Plum Bayou culture. Evidence that I will review indicates that shell-tempered pottery appeared earlier in the Ozarks, and possibly in the Arkansas Valley tradition, than it did in the Central Mississippi Valley. Furthermore, as I will demonstrate, this was fundamentally a plain pottery tradition. Decorated sherds and pots are relatively and absolutely scarce. Most assemblages have none. On the other hand, assemblages from the ceremonial centers indicate that there was an unusually high level of interareal trade in decorated pots (mostly with the Red River Valley and Ouachita Mountain Caddo, but some with Middle Mississippians in the Central Mississippi Valley) that paralleled the more obvious trade, for which Spiro is famous, in items of shell, copper, and other exotic materials.

Although the Northern Caddoan Area paradigm, which holds that this tradition was a regional variant of Caddoan culture, best known from the Red River Valley of southwest Arkansas and east Texas, requires that it be derived from the ancestral Caddoan culture, namely Fourche Maline (Schambach 1982a), and most Arkansas Valley specialists appear to believe that it did develop out of Fourche Maline, no one has developed a plausible scenario for how this occurred. The problem is that none of the traits truly distinctive of this tradition as of about A.D. 1000–such as hoe horticulture, shell-tempered pottery, four-center-post houses, charnel houses, and flat-topped mounds–was present in Fourche Maline culture.

THE TERRITORY

If the Arkansas Valley tradition was a distinct regional tradition, what were its geographical and cultural parameters? Where were its borders and what were the important intra-cultural and inter-cultural connections between it and the Caddoan area to the south, the Central Mississippi Valley to the east, the Ozarks to the north, and the Plains to the west?

The Southern Border

Our failure to recognize the critical southern border between the Arkansas Valley and the Caddoan area (I have been as much at fault here as anyone else; see Schambach 1982a:186-189) has caused us to conflate the culture histories of two distinct culture areas. This border must have been located somewhere between the southern edge of the floodplain of the Arkansas River and the northern foothills of the Ouachita Mountains, probably never much more than 30 to 50 miles south of the river in most parts of western Arkansas and eastern Oklahoma.

There is substantial archaeological evidence, heretofore masked by the Northern Caddoan Area paradigm, that during the Woodland period such a border, probably as much cultural as geographical, kept people of the Ouachita Mountains out of the Arkansas Valley and vice versa. Archaeologists who know the area well agree that the northern edge of the Ouachita Mountain physiographic region was the northern limit of the distribution of Fourche Maline culture. In eastern Oklahoma no Fourche Maline components–easily recognizable from their thick, rich middens and their abundant Williams Plain pottery–have been identified in the Arkansas Valley or north of it. Galm, the authority on Oklahoma Fourche Maline, recognizes none north of the Poteau Basin and explicitly refuses to extend

Fourche Maline into the Arkansas Valley or north of it (1984:219 and Figure 9.1). Bell (in Davis, Wyckoff, and Holmes 1971:7) states that Fourche Maline sites "are basically restricted to the Ouachita Mountain area . . . the hinterlands" and that he does not "know of any in the Arkansas Valley." Wyckoff's view is that Fourche Maline sites are concentrated "along various streams which drain the northern part of the Ouachita Mountains" (1974:66). Significantly, he also sees the "Caddoan" occupation of the Arkansas Valley as beginning around A.D. 700 when Fourche Maline "farmers" moved into the "uninhabited, fertile bottomlands . . . along the Arkansas, Canadian, Illinois and Grand Rivers" after exhausting their own lands along Fourche Maline Creek and the Poteau River (1980:519-520; Wyckoff and Brooks 1983:91).

The Arkansas Valley in Arkansas is probably not the exception to the foregoing that it might appear to be. Hoffman (1977:33-41) has been more hesitant than I (Schambach 1982a:188-189) about assigning to Fourche Maline his Arkansas Valley "Gober Complex," although he sees the Gary points and plain grog-tempered pottery that are characteristic of Gober Complex sites as evidence of a strong and–note the Northern Caddoan Area paradigm at work–"geographically logical" relationship with Fourche Maline. In fact the Gober Complex differs significantly from Fourche Maline but resembles various "emergent Mississippian" (Smith 1990) sites to the east and northeast in important ways. The layout of the type site, Spinach Patch, with its distinct village plan consisting of a midden area surrounding a rectangular plaza and, apparently, two mounds (Bond 1977:83-84 and Figures 6.2 and 6.3), is like nothing in Fourche Maline with its notoriously amorphous and featureless middens. But it has apparent homologues in the planned settlements at the Zebree site in northeast Arkansas and at other emergent Mississippian sites in the Central Mississippi Valley (Morse and Morse 1983:228-233). So do four other traits, all missing in Fourche Maline as it is known south of the Arkansas Valley (Schambach 1982a): the probable wattle-and-daub houses at the Spinach Patch and Gasfield sites, the probable storage pits at Gasfield, the chipped argillite hoes that are a prime diagnostic of the complex (Hoffman 1977:33-35), and the Steuben projectile points that appear to supplement the assemblage of Gary points and arrowpoints at Spinach Patch (see Bond 1977: Figure 6.6a, b, c, and h.).

Some Arkansas Valley specialists have speculated that relationships between Woodland period occupants of the Ouachitas and those of northeast Oklahoma, either in or north of the Arkansas Valley, were at least occasionally hostile. Their evidence is the skeletons of nine people in a mass grave at the McCutchan-McLaughlin site in the Fourche Maline valley (Powell and Rogers 1980:56-57). Apparently they were killed with 30 projectile points of a distinctive Gary variety that, judging from the material (Boone chert), could have been made in the Arkansas Valley, where it occurs in cobble form, or to the north of the Arkansas Valley, where it outcrops. Powell and Rogers (1980:57) note that Gary points of the same variety and material have been found at the Brush Creek, Crutchfield, Redbird, and Breno sites located in the Verdigris River valley. These sites, all unreported, are apparently located in southwestern Mayes County, close enough to the Arkansas Valley proper to be considered Arkansas Valley sites. Powell and Rogers clearly imply that the raiders could have come from that area, but they do not address the fact that the distribution of points of this unnamed Gary variety is unknown and that, assuming they really are imports in the Fourche Maline Valley, they could have come from some part of the Arkansas Valley much closer to the McCutchan-McLaughlin site than southwestern Mayes County. Galm–anticipating my argument that the Woodland period occupants of the Arkansas Valley were more Mississippian than Fourche Maline–sees the mass grave at McCutchan-McLaughlin as possible evidence of conflict associated with the "introduction of the . . . Mississippian Tradition to this Western periphery area" (1978:251). He also sees the Fourche Maline sequence in the Poteau Basin ending with "an influx of material traits associated with the westward expansion of Mississippian influence (and possibly people) at circa A.D. 700-800" (1984:215).

In a recent paper in which he asserts, contrary to most of his Arkansas Valley coworkers, that the Woodland occupants of the Arkansas Valley in northeast Oklahoma were Fourche Maline people, Rogers (1991a:231) has perforce changed his mind about where the putative killers of the McCutchan-McLaughlin people might have come from, now suggesting that they were a group "with Kansas City Hopewell ties, from northeast Oklahoma." Presumably he is referring to the somewhat Hopewellian looking Cooper phase sites at the juncture of Honey Creek and the Neosho. This is a weak argument, at best, partly because he does not say why he now rules out the Mayes County sites, and partly because the most common and characteristic point type of the Cooper phase is not the Gary point but the Snyders-like, Cooper Corner-notched point (Bell and Baerreis 1951:29-30; Vehik, 1984:182). Thus it is doubtful that any raiders who might have attacked the people of the McCutchan-McLaughlin site did so from a base as far north as Delaware County. Indeed, what would have prompted Cooper phase people to travel about 100 miles south as the crow flies, moving, if Rogers is right, through a Fourche Maline population in the Arkansas Valley most of the time, to attack still other Fourche Maline people in the Fourche Maline Valley? It is more likely that the raid, if such there was, was carried out by early Mississippian-like Spiroans living in the Arkansas Valley against ancestral Caddoans living about 30 miles south of them in the Fourche Maline Valley.

There are several additional lines of evidence for a cultural and biological discontinuity of long standing between the Arkansas Valley and the Caddoan area. One is the recent determination by Barnes and Rose (1990:12), based on comparisons of dental morphology, that contrary to expectations generated by the Northern Caddoan area paradigm, the Mississippi period population of the Arkansas Valley was genetically distinct from the Caddoan populations of the Ouachita Mountains and the Red River Valley. They conclude that "we must now revise our research strategies to . . . delineate the genetic boundaries of the prehistoric peoples of the Trans-Mississippi South." It is significant that the traits compared, agenesis and supernumerary teeth, common among Caddoan populations in the Red and Ouachita River valleys but thought to be rare in Middle Mississippian populations in the Central Mississippi Valley (Barnes and Rose 1990:5), were also rare in the Arkansas Valley. This suggests that the Arkansas Valley population was closer genetically to Middle Mississippians to the east than to Caddoans to the south.

Secondly, recent reviews and compilations of all bioarchaeological data from the Trans-Mississippi South and adjacent parts of the Middle and Lower Mississippi Valley (Burnett 1988; Harmon and Rose 1989; Burnett 1990) have assembled clear osteological and dental evidence for different dietary patterns, different food preparation techniques, and different rates and types of infections in the Arkansas Valley and the Caddoan area. Surprisingly, and in marked contrast to Caddoan populations in the Ouachitas and farther south and to Middle Mississippian populations to the east of them in the Mississippi Valley, the Arkansas Valley population never became "maize dependent," not even–on very strong evidence–the population at Spiro (Burnett 1988:220). The Arkansas Valley population also used stone food-grinding equipment from at least early Woodland times through the end of the Mississippi period, resulting, particularly in the Mississippi period, in a generally high incidence of heavy to severe tooth wear (Burnett 1980). The Caddo, on the other hand, abandoned the stone food grinding implements of their Fourche Maline ancestors early on, as shown by their artifact inventories and their low rate of tooth wear (Schambach 1982a: 178; Burnett 1988). In contrast to the Caddoan area, where rates of serious infections were remarkably low during the Mississippi period (Harmon and Rose 1989:347-349; Burnett 1988:215-216; Brown 1984a:259), the Arkansas Valley in eastern Oklahoma was a hotbed of infection, one of which was probably endemic syphilis or some other form of treponemal disease (Brues 1958, 1959; Brown 1984a:259). The osteitis and osteomyelitis, whose incidences indicate serious infections of severe to epidemic proportions in the Spiro phase Horton and Morris site populations (but, significantly, not in the Spiro site population; Burnett 1988:211-214), are not reported in the Caddoan area, with one exception. That exception, a significant one at the Sanders site in the Red River Valley in east Texas (Burnett 1990:393-397), is discussed later in this paper.

Third, Fritz (1990:12) has argued that the population, or populations, of the Arkansas Valley from Spiro to Spinach Patch to Toltec and of the Ozark Highlands were, like contemporaneous populations in Ohio, Kentucky, Tennessee, and Illinois, heavily involved in the pre-maize horticultural complex beginning early in the first millennium A.D. On the other hand, while recognizing the scarcity of comparable botanical evidence pro or con, she suggests that Fourche Maline and early Caddoan populations might have lacked the pre-maize horticultural complex, remaining in a hunting and gathering mode right up to the introduction of corn horticulture about A.D. 900. I find this plausible, having argued elsewhere that nuts and acorns were the basis of the Fourche Maline adaptation (Schambach and Newell 1990:20). Fritz was contrasting Arkansas Valley populations with Red River Valley populations, but I see no reason not to extend her argument to the Fourche Maline population of the Ouachitas. This would mean that from the Woodland period on the peoples of the Arkansas Valley and the Ouachitas were on opposite sides of a basic cultural discontinuity that seems to have existed throughout much of the Southeast, a discontinuity between the proto Middle Mississippian, Woodland period developers and practitioners of the Eastern Agricultural Complex to the north and the basically non-horticultural, Gulf Tradition peoples to the south.

The Eastern Border

Because the archaeology of the Arkansas Valley in Arkansas, from the Oklahoma line to Little Rock (Hoffman's "Central Arkansas Valley," 1977:3), is still mainly unknown, we do not understand the relationship, if any, between the developmental stages of the Arkansas Valley tradition as it is presently known in eastern Oklahoma and the Plum Bayou culture of central Arkansas. But evidence for some kind of close connection, if not actual Plum Bayou occupation of the entire valley, is accumulating.

House (1985:105-109) has "provisionally" assigned the Coles Creek period component at the Alexander site in Conway County, midway between Toltec and the Oklahoma line, to Plum Bayou culture. Furthermore, Fritz (1990:4-5) has predicted that the essentially pre-maize starchy seed horticultural complex found at Toltec (the type site for Plum Bayou culture) and identified at the Alexander site (House 1985:109) will also be found at Gober Complex sites that Brown (1984b:27) links with the Oklahoma Evans phase between Alexander and the Oklahoma line and at sites in the A.D. 700 to 900 range, i.e., Evans phase sites, in eastern Oklahoma. And both Rolingson (1990:46) and Brown (1984b:12-13) have commented on other apparent links between Plum Bayou culture and the Evans and Harlan phases of the Arkansas Valley tradition.

The two structure types that are among the prime diagnostics of the Arkansas Valley tradition, square or rectangular, wattle-and-daub houses with two or four-center-support-posts, have been discovered as far east as the Greer's Ferry Reservoir area in the White River drainage (CE-12, a site tested by McGimsey [1959:17-19]) and, south of the Arkansas river near Morrilton, at Mound 2 at the Oppelo site (3CN213, tested in 1991 by Arkansas Highway

Department archaeologist John Miller [pers. comm.]). The question is: do the numerous post molds at Toltec (Rolingson pers. comm.), indicating that some type of solid, multiple-wall-post structures were in use, justify the assertion that this house type also occurs in Plum Bayou culture? It would also be interesting to discover what the houses at the Spinach Patch site and the Alexander site looked like.

Flat-bottomed, shell-tempered pots, present but not abundant at the Alexander site (Hemmings 1985: 43), occur as far east as Toltec itself and in the Western Lowlands of the White River drainage in northeast Arkansas. Some or all of the latter examples are apparently part of the recently formulated Owl's Bend Complex (Morse and Morse 1990: Figure 65). Flat-bottomed, grog-tempered pots, generally considered a Fourche Maline trait when found in pre-Harlan-phase contexts in the Arkansas Valley in eastern Oklahoma, were also common, possibly even abundant, to judge from the flat base fragments in the sherd collections in the Plum Bayou assemblage at Toltec (Rolingson pers. comm.).

Thus there are two possibilities with respect to the relationship of the Arkansas Valley tradition with Plum Bayou culture. One is that there was a generic relationship, which would put the eastern border of the Arkansas Valley tradition in central Arkansas. The other is that somewhere in the archaeological *terra incognita* between Little Rock and the Oklahoma line, probably not far west of Little Rock, we will eventually discover a discontinuity between late Woodland-early Mississippi phases of the Arkansas Valley tradition to the west and Plum Bayou culture to the east.

The Northern Border

In 1984 James Brown broke the barrier that had kept us from seeing that the Arkansas Valley tradition extends into the southern Ozarks and, by adding the distinctly non-Caddoan Ozark materials to that tradition, brought to a head the question of its relationship with the Caddoan area (Brown 1984b: 56-58). It now appears that, instead of being a backward area, the Ozarks were important in the initial development of the Eastern Horticultural Complex and that shell-tempered pottery originated there. There seems to be general agreement that the Arkansas Valley tradition, as it is known in eastern Oklahoma, extends into the southern Ozarks in the Elk River, Spavinaw Creek, Illinois River, and Lee Creek drainages. The Loftin phase of the White River drainage in the southern Ozarks area of Missouri is also considered part of this complex on what appears to be good evidence, particularly the two- and four-center-support-post houses that seem to be characteristic of this phase (Chapman 1980:139-148; Brown 1984b:22-25, 27-28; Perttula 1989). Curiously, the Huntsville Mound on War Eagle Creek in the White River drainage in the Arkansas Ozarks does not appear to belong to the complex, although Kay, Sabo, and Merletti (1989:145-151 and Figure 41) and Brown (1984b:27-28) think otherwise. The (largely postulated) oval houses built on successive surfaces of a four-stage platform mound at Huntsville (Kay, Sabo, and Merletti 1989: 145-150) are unlike anything in the Arkansas Valley tradition, where oval houses have not been found and where post mold patterns associated with platform mounds are under the mounds, not on the platforms (Brown, Bell, and Wyckoff 1978:185-186).

The two key questions pertaining to the northern border seem to be: first, is there any relationship between Huntsville and the Prices' Owl's Bend "tradition" of the eastern Ozarks and the Western Lowlands with its flat-bottomed, shell-tempered pots (Morse and Morse 1983:250-253; 1990:160-161)? And, second, does the apparent similarity in ceramics indicate some kind of relationship between the Owl's Bend tradition and the Arkansas Valley tradition? As Perttula (1989:117) has observed, we do not know where the Middle Mississippian occupation of the eastern Ozarks in Arkansas ends and the Arkansas Valley tradition begins. Owl's Bend and Huntsville will figure in the answer to that question.

The Western Border

The western limit of the distribution of farmsteads, village sites and ceremonial centers of the Arkansas Valley tradition coincides with the western limit of the oak-hickory forests of eastern Oklahoma, where the Eastern Woodlands give way to the Southern Plains (Brown, Bell, and Wyckoff 1978: Figure 7.1). Except for short extensions westward up the valleys of eastward-flowing rivers and streams, no Arkansas Valley tradition sites have been recognized west of this boundary, which corresponds approximately with the arc formed by the Arkansas Valley south of the Forks of the Arkansas River (where the Arkansas turns west) and the Grand (Neosho) River valley, which loops back to the northeast from that point (Bell, Brown, and Wyckoff 1978: Figure 7.1; Wyckoff and Brooks 1983:78-79 and Figure 4).

According to the Northern Caddoan Area paradigm, this border was of no particular significance, and there was no substantial traffic across it until the beginning of the so called Fort Coffee "focus" circa A.D. 1450 after the Spiroan phenomenon had run its course and Spiroan culture had faded. Fort Coffee was considered an impure or "diluted" (Bell and Baerreis 1951:97) Plains-influenced Caddoan cultural unit of the period A.D. 1450 to 1600 which developed out of the Spiro "focus" in some unexplained way that supposedly involved cessation of the more ostentatious forms of ceremonial activity, such as mound building, the appearance of shell-tempered pottery, and the sudden movement of many Plains traits into eastern Oklahoma (Orr 1946:240-249, 1952:251; Bell and Baerreis 1951:97; Wyckoff 1967:152, 1970:149-150, 1971:154-164, 1980:8 and Table 1). This period of sweeping change from the west was known as the "Gibson-Fulton transition," a concept invented by Krieger (1946:211-216) and hardened into a shibboleth by Arkansas Valley specialists, who made it their basic operating concept. Step 1 for archaeologists analyzing eastern Oklahoma collections was to decide whether an assemblage was "Gibson" or "Fulton." Often there was no Step 2.

The "Fulton aspect" Plains traits considered new to the area and distinctive of the Fort Coffee focus were: osteological evidence of bison hunting; bison bone tools such as hoes, digging stick tips, rasps, and scrapers; butchering and hide processing tools in the form of diamond-shaped, beveled knives and endscrapers, both strongly associated with bison hunting; sandstone arrowshaft smoothers; storage pits; triangular arrowpoints; T-shaped pipes; and, particularly, shell-tempered pottery (Krieger 1961: 43; Wyckoff 1970:152; 1980: Table 1; Rohrbaugh 1982:28). But no one was sure from where it had come (see Davis, Wyckoff, and Holmes 1971:29-31; Wyckoff 1971:163).

This meant that virtually all the Mississippi period habitation assemblages in eastern Oklahoma wound up being considered Fort Coffee focus, or often just "Fulton aspect," while the ceremonial centers, which were uninhabited and therefore generally lacked these traits (which are all habitation traits) went into the Gibson aspect. This puzzling situation (which moved Bell to muse in 1963 at the 7th Caddo Conference: "is it not a little peculiar that . . . when you find a small village site around Spiro it's very likely to be . . . Fort Coffee?" [in Davis, Wyckoff, and Holmes 1971:55]) prevailed until 1971, when Brown demonstrated that Woodward Plain, the predominant shell-tempered pottery type in eastern Oklahoma, was the main pottery type of the "Gibson aspect" Spiro focus, which he converted to the Spiro phase (Brown 1971; Rohrbaugh 1982:28), thus beginning what I will call throughout this paper the "Fulton-to-Gibson data shift," i.e., the shifting of almost all the erstwhile "Fulton aspect" assemblages into earlier time periods. The most dramatic example so far is Rohrbaugh's demonstration, based on Brown's work plus a large-scale radiocarbon-dating project (Rohrbaugh 1982:229;

1984:271), that 13 of the 15 "Fulton Aspect," Fort Coffee "focus" sites in the Spiro locality are Spiro phase habitation sites. The next hurdle, before which Arkansas Valley specialists are still balking, thus requiring me to boost them here and there in this paper, is to realize that the habitation sites of the earlier Harlan phase are also to be found on the old roster of Fulton aspect sites.

Nonetheless, it is already as clear as it can be that the western border was not, in some mysterious way, closed until the end of the Spiro phase. To the extent that the "Plains traits" of the old Fulton aspect really are Plains traits, as the bison bone tools and the endscrapers and diamond-shaped, beveled knives certainly appear to be, the Spiroans had a significant involvement with bison and probably with Plains people from no later than A.D. 1100 on.

THE WESTERN BORDER AND THE SPIRO PHENOMENON

In fact the involvement of the Spiroans with the people and, particularly, the bison just beyond their western border is the heretofore obscured key to the puzzle of what supported the Spiro phenomenon. The puzzle is this. Had the Craig mound never been opened, sparing us the dazzle effect of the hoards therein, Spiroan culture would be perceived as interesting in its own right, but unimpressive compared to that of the Middle Mississippians to the east or the Caddoans to the south. The Craig mound aside, it amounted to a small number of people (hundreds, I suspect, not thousands) operating at any given time no more than 3 to 5 small ceremonial centers (with small mounds) in an out-of-the-way location at or beyond the western fringe of the Mississippian interaction sphere and the northern border of the Caddoan area. As we have seen, the bioarchaeological data on several populations of Spiroans indicates that they ate very little corn and, as far as the bioanthropologists are concerned, did not have a Middle Mississippian-style agriculturally based ecology (Burnett 1990:219-220). They were still in a Woodland mode of hunting, gathering, and gardening and, judging from their high infection rates, they were not doing very well at it, particularly the populations away from Spiro. How, then, did these people attract to themselves, from a large area to the east of them in the Central Mississippi Valley and beyond, something like 70% of the known prestige goods of the Southeastern Ceremonial Complex, including "thousands of pounds of shell beads," three to four thousand shell cups (Brown 1975:15; 1984a:255), and numerous other items such as decorated pottery and projectile points? And how did they obtain, as well, pottery, pipes, and jasper from the Red River Valley Caddoans and cotton cloth and artifacts of Alibates chert from the Southwest and the Southern Plains?

While there has been no doubt, in recent years at least, that trade was the mechanism that brought most of the goods to Spiro (virtually none was made in the Spiro area) (Phillips and Brown 1978:22; Wyckoff 1980:516; Brown 1983:135), it was impossible, as long as the concept of the Gibson-Fulton transition remained in place, to see that the Southern Plains bison herd, whose range probably began less than 100 miles west of Spiro (Wedel 1961: Figure 4), might have been a factor in that trade. Apart from Phillips and Brown's observation (1978:19-20) that buffalo hair and jackrabbit hair might have been important, and Wyckoff's cryptic remark (1980: 516) that trade between the Mississippi Valley and the Plains made Spiro what it was, no one has been able, or willing, to suggest *what* was being traded for all the goods at Spiro, least of all bison products. No matter that, except for the Southern Plains bison herd, there were no tradable resources available to the Arkansas Valley tradition villagers of eastern Oklahoma that could have had the drawing power to create the hoards at Spiro. In fact the very existence of that herd prior to A.D. 1400 was questioned in a still-influential article by Baerreis and Bryson (1965:74), who argued that bison were not present in significant numbers in the Southern Plains until A.D. 1400, when a period of dry weather in the

Central Plains forced those herds and some human populations southeastward. However, this argument was not based on independent paleontological evidence for the movements and fluctuations of the Plains bison herds at that time (there is none) nor on radiometrically dated archaeological deposits containing bison bone. It is an argument by fiat, the fiat being the concept of the Gibson-Fulton transition. Others interested in the problem of the availability of bison in prehistoric times have not understood this. Thus, Dillahay's conclusion that bison were absent throughout the Southern Plains between A.D. 500 and A.D. 1200-1300 is based heavily on spurious temporal data from eastern Oklahoma and Texas (1974; see particularly 180-182 and Figures 1-6).

According to Creel (1991), recent thinking on the Southern Plains bison herds is that they were in place and teeming at least as early as A.D. 1300. He argues, on archaeological evidence, that the well-documented hide trade between Southern Plains hunters and Pueblo area farmers of the early historic period actually began as early as A.D. 1300, when tools he considers characteristic of the hide trade, the diamond-shaped beveled knife and the endscraper, appeared (or, in the case of the endscraper, reappeared) at sites throughout the Southern Plains and in the Southwest. He suggests that the development of the hide trade was concurrent with, and may have been stimulated by, a dramatic increase in the size of the Southern Plains herd around A.D. 1300. On the other hand Speth and Scott (1989), whom he quotes as providing a reasonable alternative, have suggested that "the apparent increase in bison exploitation ca. A.D. 1300 may not reflect bison population change so much as it reflects the greater need for high quality protein as a result of increasing dependence on maize cultivation . . . on the part of cultivators in Pecos Valley and adjacent upland areas as well as in the Canadian Valley of northern Texas" (Creel 1991:42).

In either case it is probable that by A.D. 1300 Southern Plains hunters were trading bison hides, meat and tallow to Pueblo area farmers, as they were in 1540-1542 when Castaneda, a member of the Coronado expedition, observed: "Over these plains there roam natives following the cattle, hunting and dressing skins to take to the pueblos to sell in winter" (quoted in Creel 1991:41). Besides the perishables, such as Pueblo corn and, perhaps, cotton cloth that they probably received for their bison products, they apparently also got turquoise, obsidian, and Pacific sea shells, all of which began circulating through the southern Plains about A.D. 1300 (Creel 1991:45).

The Fulton-to-Gibson data shift has made it plain that the Spiroans of eastern Oklahoma (and probably people throughout the Southern Plains) were substantial consumers of bison products and were processing bison hides from A.D. 1100 on. The best evidence for bison consumption comes from the W.P.A.-excavated (and unreported, except the bone) School Land I and School Land II sites, small villages on a tributary of the Grand River, with radiocarbon dates in the A.D. 1100s (A.D. 1080 ± 60, 1105 ± 75, 1160 ± 70, 1165 ± 75 and 1255 ± 45; Bell 1984a: Table 10.1). In the collection from the School Land I site–which also contains the earliest dated bison scapula hoes in the Arkansas Valley tradition, according to Wyckoff (1980:469 and Table 85)–33 bison bones comprise 2.73% of the mammal bones and account for an estimated 1,500 pounds of meat, which is 26.17% of the estimated 5,691.25 pounds of meat represented by mammal bones (Duffield 1969: Table I). In the collection from School Land II eight bison bones comprise 34.78% of the food refuse bone and account for 47.62% of the estimated 1,050 pounds of meat represented (Duffield 1969: Table V). Although these are significant totals, the actual consumption could have been higher, considering Wood's argument that peoples of the Steed-Kisker phase (A.D. 850-1300) in a similar ecological situation on the Missouri

River near Kansas City were hunting bison, even though there were no bison bones at village sites in that area. The evidence, he contends, was a hundred miles away in the form of bison bones at a Steed-Kisker hunting camp (1968:171-179).

Other more or less well-dated occurrences of bison bone in assemblages in the A.D. 1100 to 1200 range include: (1) an unspecified number from a "large refuse heap" up to 2 feet deep and about 30 feet across "under the northeast periphery of Mound 1-2" at the unreported, W.P.A.-excavated Norman site (Finkelstein 1940:13); dates on the mound are A.D. 1050 ± 50, 1160 ± 50 and 1470 ± 60; six other dates on the site are 1020 ± 50, 1050 ± 110, 1170 ± 50, 1180 ± 70, 1240 ± 50, and 1250 ± 60, suggesting that the 1470 date is too late (Albert 1992); (2) part of a left femur from Level 16 under the Copple mound at Spiro, which dates (on the basis of two archeomagnetic samples) to A.D. 1180 (Wyckoff 1989:94 and 98; Peterson 1989:38); and (3) three possible specimens from the Plantation site (Wyckoff 1980: Table 84), evidently the two teeth and the scapula fragment identified in the site report as "Bovidae" (Briscoe 1977:238-240). The "acceptable" radiocarbon dates on this site are A.D. 987 ± 125 and A.D. 1252 ± 80 (Briscoe 1977: Table 15). Wyckoff (1980:465) considers it a "Period II" site, A.D. 900-1200.

There are also some significant stratigraphically or ceramically dated occurrences. At the Craig Mound a bison bone was collected from Burial 6 or the pre-mound midden (Brown 1966:39). In either case it would probably be Spiro phase or earlier. The Huffaker site in Delaware county, considered Spiro phase and earlier (Wyckoff 1970: Figure 28), produced 5 pieces of bison bone (Baerreis 1954:42). Finally, a collection from areas A and B at the Wybark site (Lopez 1973), located in the Verdigris River valley about two miles from its confluence with the Arkansas River and the Grand River, contains 168 bison bones (there were 123 deer bones), 45 of them scapula hoes and hoe fragments.

Assuming that endscrapers and diamond-shaped, beveled knives have the association with bison hide processing, if not necessarily with the hide trade, that Creel (1991) suggests, it appears likely (in the wake of the Fulton-to-Gibson data shift) that bison hides were being processed in a big way at habitation sites throughout the Arkansas Valley from the Forks of the Arkansas to Spiro, probably from A.D. 1100 on.

The best example is the just-mentioned Wybark site, probably the remains of a substantial village located in the Forks of the Arkansas, Verdigris, and Brand rivers. Although Lopez (1973:113; 121), who completed his analysis just prior to the Fulton-to-Gibson data shift, perforce put the main occupation in the "Fulton Aspect," there is nothing in the assemblage that is necessarily later than the Spiro phase, which Wyckoff (1980:460 and Table 29) recognized by reassigning it to his Period III (A.D. 1200 to 1400). I suspect it is earlier still and was an important satellite of the nearby Norman mound group (with its recently obtained suite of radiocarbon dates in the A.D. 1050 to 1250 range; Albert 1992), the only Arkansas Valley tradition site other than Spiro to yield significant quantities of Mississippi period prestige goods (Finkelstein 1940; Brown, Bell, and Wyckoff 1978:189).

The small-scale excavations at Wybark produced eight beveled knives and 31 endscrapers. Lopez (1973:110; 38) stated that the "majority" of the latter are of the Plains type and observed–presciently, I would say–that "The quantity and sometimes careful workmanship of the scrapers recovered from Ms-76 suggest an important cultural activity for its inhabitants." The quantity is indeed remarkable–there were almost as many endscrapers (26) as projectile points (36)–particularly considering the small size of the excavation (10 five by five foot squares, four salvaged trash pits and the surface in a small plot called "Area A") (Lopez 1973: Figure 5; Table 1).

Considering that endscapers were probably not butchering tools, but tools "used to remove hair and reduce hide thickness, later steps in hide processing" (Creel 1991:42-43), Wybark looks like a site where more hides were being processed than were needed for home consumption. Most of them were probably taken down the Arkansas River to the Mississippi Valley. They were probably obtained about 100 miles to the west, on the Plains, and brought down the Arkansas to Wybark and other sites by Spiroan hunters (Spiroan men surely weren't devoting much time to corn agriculture) or Spiroan traders.

There are at least three other former Fort Coffee "focus" components with large scraper inventories and diamond-shaped, beveled knives that appear to be in the same time range as Wybark: Sheffield, Tyler-Rose, and Cookson. All three are, like Wybark, the sites of settled hoe horticulturalists with substantial houses, storage pits, and abundant gardening and food processing equipment, and they have comparable ceramic and projectile point assemblages. The Sheffield site, which both Brown (1984a: Table 11.1) and Rohrbaugh (1982:218) now consider Spiro phase, and which has radiocarbon dates of A.D. 1165 ± 185, 1400 ± 30, and 1445 ± 55, produced 24 endscrapers and three diamond-shaped, beveled knives (Lopez 1973: Table IX). Tyler-Rose, which Rohrbaugh (1982:192-199), with some hesitation, considers too early for his Fort Coffee phase, mainly because of the ceramic assemblage, produced 48 endscrapers and four broken diamond-shaped, beveled knives (Cartledge 1970:26-32). Cookson, which Rohrbaugh (1982: 200) and Wyckoff (1980: Table 29) consider Spiro phase, produced 14 end-scrapers and four beveled knives (Lopez 1973: Table IX).

Wybark, Sheffield, and Tyler-Rose are in the Arkansas Valley proper at locations where one might expect to find villagers who made a sideline of processing Plains bison hides for trade downriver. Site location–proximity to hide trade routes–may have been an important factor in the distribution of endscrapers and diamond-shaped, beveled knives in eastern Oklahoma. One serious but not necessarily fatal problem with this interpretation is that Cookson is off the main line in the Illinois Valley. Another is that Rohrbaugh (1982:160) claims endscrapers are "absent from Spiro phase contexts in the Spiro locality," where they should be abundant.

But their absence is probably more apparent than real. Examination of his list of all artifacts obtained in the W.P.A. excavations at the non-mound sites in the Spiro locality (Rohrbaugh 1982:253-394) suggests it is the result of the predilections and recovery techniques of the W.P.A. excavators. Grave offerings dominate overall (including three diamond-shaped, beveled knives from the Spiro phase Edgar Moore, Spencer Littlefield, and Granville Bowman sites; Rohrbaugh 1982:292, 322, 368). Large, easily recognized specimens dominate the inventories of materials from pits and middens. Small specimens such as arrowpoints are underrepresented. The few flakes and flake tools that were recorded in the field have mostly vanished from the collections according to Rohrbaugh's notations. By contrast, the University of Oklahoma's (unreported) 1969 excavations at the Moore site, located among the complex of sites excavated by the W.P.A. crews, produced a normal looking assemblage that includes 20 endscrapers (Rohrbaugh 1982:299-319). Although Rohrbaugh considers the Moore site Fort Coffee phase, he also states (1984:279) that "at least one" of the three houses there belongs to a Spiro phase occupation. I would be surprised if anyone could prove that none of the 20 endscrapers was Spiro phase, and I offer the Moore site as evidence that hide-processing tools were just as abundant in the Spiro locality during the Spiro phase as they were farther up the Arkansas at the Wybark, Tyler-Rose, and Sheffield sites.

How did the Spiroans transform their bison hides, and probably tallow and dried meat also (a full line of bison products, as it were), into the hoards in the Craig mound? The simple part of the answer to that important question is that they were in the right place at the right time. They had the Southern

Plains bison herd (and expert Southern Plains bison hunters) to the west of them, a huge, sophisticated Middle Mississippian population to the east of them, and they controlled a long bottleneck in the Arkansas Valley that contained a good water route linking the two. Something had to happen. What did happen, I suggest, was that the Spiroans became one of the few peoples in aboriginal Eastern North America to conduct long-distance trade for profit. Explaining–and justifying–why I think they were trading for profit and how they might have managed to do that is, of course, the hard part of the question with which I began this paragraph.

The main factor that made that possible (hypothetically speaking, of course) was a burgeoning population in the Central Mississippi Valley that–inevitably, because it lacked the kinds of domestic animals that could produce food and fiber in quantity–was outgrowing the capacity of the local environment to provide clothing and proper nourishment. Gramley (1977) has argued that by the Mississippi period the Eastern tribes were significantly poor in hides and fibers and he makes a convincing case that for the Iroquois and their Owasco ancestors the supply of deer hides posed a limit to population growth.

Although the winter climate is not as severe in the Central Mississippi Valley as it is in old Iroquoia, there are several months, particularly in northeast Arkansas, when warm clothing is necessary for any kind of sustained outdoor activity. During the sixteenth century it was colder; De Soto's army was snowed in for a month in the vicinity of Little Rock, indicating snowfall and temperatures far more extreme than today (Quinn 1979:137). This was about the beginning of the "Little Ice Age," generally dated to between A.D. 1450 and 1850. The occurrence of such comparatively extreme conditions so early indicates that a cooling trend and colder-than-modern temperatures had been in effect for some time, perhaps centuries, creating a demand for hides that could not be met through the exploitation of local deer populations.

The law of supply and demand seems to have operated in the sixteenth century, at least, as it does now: hides had considerable value throughout the Southeast. Everywhere De Soto went, hides and skins rather than, say, clay pots were usually among the presents the Indians brought him of their own accord at first contact. And the Spaniards, all but naked after several misadventures in the first year of the expedition, valued them highly–couldn't get enough of them, actually. In Biedma's account of the Spaniards' stay in Arkansas there are twenty-three references to gifts of "skins" and "cowhides," the latter probably from bison (Quinn 1979:129, 130, 131, 132, 134, 135, 138, 139, 140, and 143).

A shortage of hides or fiber suitable for warm clothing was probably not the only factor stressing Middle Mississippians from A.D. 1200 on. Population was probably outstripping local supplies of meat and fish, forcing people into dangerous overdependence on corn (Rose, Marks and Tieszen 1991:21), which is notoriously deficient in lysine, iron, zinc, and niacin. Bioanthropologists studying Mississippi period populations of the Central Mississippi Valley appear to be moving toward consensus that poor diet was widely responsible for severe iron deficiency anemia and high infection rates involving treponemal organisms, some of which are quite dangerous (Levy 1992:1210). Burnett and Murray (1990) think that these disorders, particularly anemia, because of their evidently extraordinarily severe impact on young women of childbearing age, may have reduced the birthrate drastically, perhaps enough to have been an important factor in the massive population collapse that occurred in eastern Arkansas following the De Soto entrada.

In sum, hides and animal protein and fat were probably in short supply among the Middle Mississippians of eastern Arkansas and beyond. Therefore they would have been valuable, and the Spiroans, who could supply them, were in a position to profit from undertaking to do so. Thus, by A.D. 1200 or earlier the stage was probably set for the developments that created the hoards at Spiro. It is unlikely, however, that these included trade on a scale that could have come anywhere near meeting the needs of the Middle Mississippian population at large. The Spiroans were not capable of moving goods on that scale, and the Middle Mississippian populace, as opposed to the Middle Mississippian elite, was not capable of paying for them in the kinds of goods that made their way to Spiro.

In terms of the recently proposed "Prestige Goods Economy" model for Southeastern economic systems during the Mississippi period (Brown, Kerber, and Winters 1990) the Spiro items were mostly "prestige goods" that were created and circulated for essentially political purposes. They functioned to create and validate social statuses within societies and to regulate relationships between societies. They had nothing to do with the mundane matters of keeping people fed, clothed, and housed; those were the responsibility of kinship groups at the household level. Put precisely, they did not function to "satisfy specific needs, whether they are the accumulation of wealth or the buffering of shortfalls in the local food supply." (Brown, Kerber, and Winters 1990:255).

This poses two problems for my hypothesis that the hoards of prestige goods at Spiro developed out of a situation involving the needs of the Middle Mississippians and the Spiroans' access to bison products. Prestige goods couldn't be used to buffer shortfalls, and they couldn't be accumulated as wealth. The latter is particularly significant because, if it can be shown that they were accumulating as wealth in the hoards at Spiro, then Spiro becomes evidence that this situation had stimulated the development of an economic system of a different kind within Spiroan culture, namely, a rudimentary form of commercial economy that was generating wealth or capital.

According to both the prestige goods model and an earlier model that now seems to be losing support, the redistribution model, the normal way of disposing of prestige goods in Southeastern societies was to bury them with the persons whose statuses they had helped to create and continued to validate, even after death and burial. That is the common explanation for the presence, albeit in abnormally large quantities, of the prestige goods at Spiro. As Brown (1973; 1984a:255) and Phillips and Brown (1978:17) put it: "The symbolic intent" of the "great stacks of wealth that were piled on and next to the three types of elite burials," particularly "the highest ranking, largest, and most spectacular" cedar pole litter burials with their "unusually rich displays of material wealth," was "to display the superior status of the individual on the litter whether alive or dead." Implicit in this explanation is the assumption that the association of the goods with the remains of (therefore) important persons demonstrates that they were prestige goods that represent wealth of a social and political nature, rather than wealth of a commercial nature. In the case of goods not associated with human remains, or of those associated with remains that might not have been those of important persons, that assumption becomes questionable.

And that, I suspect, is the case at Spiro where those whose status was supposedly being memorialized were typically (but unconventionally, compared to other Southeastern sites) represented osteologically by no more than "a few small skeletal mementos" (Phillips and Brown 1978:13). Sometimes there was apparently no bone, as seems to have been the case with litter feature B-108C (Brown 1966:171-172). The human remains associated with Burial 62, the richest litter feature excavated by the University of Oklahoma W.P.A. project, consisted of a skull adorned with a pair of wooden earspools and a few bones of "another adult" . . . "represented in a group of human and animal bones found among the conch shell in the laboratory" (Brown 1966: 114).

Clearly, goods were preponderant on the litters. This is different from high-status burials, including litter burials, throughout the Caddoan area; the remains of the person whose status was being recognized are always prominent, and the grave goods are always clearly his or her accouterments and possessions. If the burials at Etowah are any example

(Larsen 1989:138-139), that is probably true of high-status burials at contemporaneous sites east of Spiro also.

Under these circumstances, it is necessary to ask whether the few and fragmentary human remains accompanying the litter burials were really those of the Spiroan elite? Some appear to have been accidental inclusions; others could represent offerings or retainers. Were the fragmentary skeletal remains of the juvenile and the child found near (not with) the hundreds of shell cups and other items in litter feature B-155 (Brown 1966:214-216) those of people of low rank sacrificed by their parents in a variation of the Natchezan custom of offering children as retainers at the burial of an important person (Hudson 1976:330-332)? Was the skull with wooden earspools with Burial 62 the *raison d'etre* for the feature (the other parts of this personage unaccountably lost or discarded) or was it–hardly unusual in the Southeast–a trophy skull, part of the hoard rather than of the hoarder?

Brown (pers. comm.) has suggested that the scarcity of human bones in the Spiro litter features is due to the loss or discard of the bones of the honored persons as they were transferred from charnel house to litter, or perhaps from graves elsewhere to litters. If so, we must ask why the goods got transferred without similar, nearly complete attrition? The answer would almost have to be that they mattered more than the person. Otherwise, why not move the whole skeleton of the "honored individual" and only a "small memento" of the wealth? Ostensibly, the human bones in these features were secondary. The important elements were the goods and the litters themselves, which were probably fraught with meaning. Brown (1975:9), with an eye to the significance of litters in early historic Southeastern societies, has argued that: "The fact that the very form of the burial facility was a litter implies that the rank of an office of political leadership was being conferred on the deceased," and that "Gradations in the size of the litters likewise imply gradations in rank."

If it is true that the litters at Spiro did not contain the bones of important persons, and if it is true that litters were status symbols denoting high rank, then the "symbolic intent" of litters piled high with prestige goods alone would be twofold. They would show that the crucial link between prestige goods and personages had been severed, that these were goods that had been stripped of their social and political significance. But they would also show that these were goods that were now possessed of high status or value in their own right, that they had been converted into wealth.

In sum, the litters at Spiro are, I suggest, evidence that the Spiroans had found a way to do the impossible so far as prestige goods systems are concerned: to convert prestige goods or "primitive valuables" into commercial valuables or capital. This might have happened in the following way. In a situation such as I have hypothesized, where there was, on the one hand, a society facing shortages of protein, fat, and fiber, and on the other, a small group of people capable of supplying modest quantities of buffalo products, the value of such products could have risen to the point where members of the social elite would have been willing to exchange prestige goods for them. They would not have done so out of any intent to help feed or clothe the population at large, almost certainly an impossibility in any case. Their purpose would have been to convert buffalo products into prestige goods by circulating them (to their own social and political advantage) in the general Southeastern prestige goods economy. This was probably in no way unusual. They would have been using established mechanisms for procuring the goods necessary to the operation of the system. Other people closer to the Gulf Coast would have been procuring marine shell in the same way.

But the Spiroans who were supplying these goods would have been in an unusual situation. Because of their location at the gateway to the Plains, but far from all other Southeastern population centers (see Brown 1975:26), and because theirs was a small society, it would have been possible, perhaps inevitable, for them to generate more prestige goods by feeding the system than they could use as such within their own society or could use by participating in the normal way in the Mississippian system to the east, which would have been the purpose of their activity at the outset. *Ipso facto* these unusable, but not valueless, surplus prestige goods became storable commercial wealth.

At some point in this process, with surplus prestige goods piling up around them, the Spiroans apparently grasped and embraced the concept of trading for profit because there is evidence that by A.D. 1300 they had established an entrepot for long-distance trade in the Red River Valley and probably another in the Canadian River Valley. These were no doubt complemented by a major Arkansas Valley entrepot somewhere between Fort Smith and Little Rock. My guess is that it was at the Point Remove site, near Morrilton, which is either the easternmost Arkansas Valley tradition mound group in the Arkansas Valley or the westernmost Middle Mississippian group.

SPIROAN ENTREPOTS IN TEXAS AND CENTRAL OKLAHOMA

The Red River valley entrepot consisted of the Sanders site about 150 miles southwest of Spiro in Lamar County, Texas and possibly some of the four or five other apparently related components known in Lamar and Fannin counties, Texas and Choctaw County, Oklahoma (Krieger 1946:171-182; see also Wyckoff 1971:85-96 and Phillips and Brown 1978: 166-167). These sites lie in "a narrow north-south belt on both sides of Red River, approximately on the border between eastern forest and open plains" (Krieger 1946:172) that is–not coincidentally, I am sure–also the terminus of the most logical route from Spiro to the Red River Valley: up the Poteau Valley from Spiro, then down the Kiamichi Valley to the Red River Valley.

Because the Sanders site was originally considered the type site of an unusual early Caddoan "Sanders focus" on the western edge of the Caddoan area in the Red River Valley (Krieger 1946:171-218), and because it is still so considered by Caddoan area specialists, my assertion that it was actually a Spiroan trading post doubtlessly requires some support at this point. The key fact here is that the Sanders focus was one of the many fictions born of Krieger's concept of the "Gibson-Fulton transition" and his supporting dictum that shell-tempered pottery in the Caddoan area had to be late prehistoric or historic. As that concept crumbled in the face of radiometric evidence, it became apparent that he had been unjustified in making the mortuary assemblage from Sanders the basis of his Gibson aspect Sanders focus (thus creating a cultural unit with a trait list that, he was forced to admit, "may seem quite ethereal" 1946:203), while relegating most of the midden assemblage to a much later Fulton aspect occupation because of what he considered late "Plains" traits such as plain shell-tempered pottery, bison scapula hoes, endscrapers, and diamond-shaped beveled knives. We now know, however, that all of these are as much at home in Spiro phase assemblages as in later Plains Villager assemblages. Thus, Brown (1984a:262), the architect of the Fulton-to-Gibson data shift, has recognized the Sanders site as a "regional variant" of the Spiro phase. But what Brown and other devotees of the Northern Caddoan Area paradigm still cannot see (because they have not grasped the significance of the differences between the Arkansas Valley tradition and the Caddoan area and do not separate them culturally), is that the Sanders site is not a Red River Valley Caddoan site that (inexplicably) has produced an extraordinary range and number of central Mississippi Valley prestige goods and pottery (four conch shell cups, one of them engraved; one perforated conch shell; 21 shell gorgets, decorated and plain, including "Craig School" specimens that "must have come from the Arkansas Valley" [Brown 1983: 150]; about 5,500 conch shell beads; about 200 olivella shell beads; negative painted pottery; "bean pots"; limestone-tempered Monks Mound Red pottery [Krieger 1946:176-183; see Phillips and Brown 1978:166-169 as to the source of the shell]) and (inexplicably) has a ceramic assemblage (nearly all Woodward Plain, a.k.a. Mississippi Plain, and Sanders Plain, a.k.a. Old Town Red; Brown 1971:145-171) and an array of domestic artifacts (bison bone hoes, stone hoes, stone seed grinding equipment, endscrapers, diamond-shaped beveled knives, bone beamers, bone fish hooks) that cannot be found at any other Caddoan site in the Red River Valley. It is–could it be any plainer?–a site unit intrusion of Arkansas Valley Spiroans.

As it happens, this really could become much plainer soon. Two recent bioanthropological studies in which the Sanders site skeletal population unexpectedly emerged as "markedly different" from Caddoan skeletal populations in the Red River Valley (Burnett 1990:393-399) indicate that the bones will demonstrate what the artifacts can only suggest; that the people themselves were Spiroan immigrants from the Arkansas Valley.

In the course of an M.A. thesis project that involved comparing the ostensibly Caddoan Sanders site skeletons with the Texarkana phase Caddoan skeletons from the Hatchel-Mitchell site 120 miles down the Red River, Dow (1987) discovered that the two populations were genetically different. Having, of course, no inkling that this might be due to the Sanders people being Spiroans from the Arkansas Valley, she attributed this to the possibility that they were interbreeding with Plains people (1987:111).

Another study by Barbara Jackson of the Texas Archaeological Research Laboratory (unpublished; the raw data are summarized by Burnett 1990:393-398) uncovered two additional peculiarities of the Sanders population that Burnett finds impossible to explain within the conceptual framework we archeologists have provided. First, the infection rate of the adult population at Sanders (33.3%) is "dramatically" high compared to other populations in the Red River Valley. In the case of two of the six adults examined, the lesions in evidence are osteitis and osteomyelitis, neither of which has been identified in early Caddoan populations in the Red River Valley or, apparently, elsewhere in the Caddoan area. Thus, the lesions at Sanders seem to point straight to the Spiro phase skeletal populations from the Spiro, Morris, and Horton sites in the Arkansas Valley. There, as we have seen, the incidence of osteitis and osteomyelitis is unusually high, and the osteitis is thought to indicate a high incidence of endemic syphilis or some other treponemal infection (Brown 1984a:259; Burnett 1988:212-214).

Secondly, the infections indicated by these lesions had an abnormal distribution within the population. While the adult infection rate was comparatively high, the nine children studied were infection free; a "confusing picture," Burnett notes (1990:397), "that deserves further testing." The hypothesis to be tested here, I suggest, is that the adults, who were immigrant traders, acquired their lesions as children (endemic syphilis being a contagious disease of childhood; Hackett 1963:10) in their infection-ridden Arkansas Valley homeland. Their children, however, were born at the Red River Valley trading post, far from the Spiroan population center that harbored the pathogens responsible for osteitis and osteomyelitis.

There is, to return now to the discussion of the Sanders site as a Spiroan entrepot in the Red River Valley, good circumstantial and distributional evidence that from this location Spiroan traders were in contact with Southern Plains bison hunters, with the ancestral Kadohadacho and other eastern Caddoan groups in the Red River Valley, with the ancestral Hasinai and other western Caddoans in east Texas, and with Puebloans in eastern New Mexico (probably indirectly through a Pueblo-Southern Plains trade network; Creel 1991).

A trading post at this location–at the terminus of the most logical route from Spiro to the Red River valley–would have given the Spiroans access to whatever bison products and Puebloan goods the Pueblo-Southern Plains trade network might have been moving down the Washita River and the Red

River to the Caddoan area. Their suppliers would have been people of the Washita River phase, probably the ancestral Wichita, who occupied the Washita and South Canadian River drainages of west central Oklahoma from at least A.D. 1150 through A.D. 1400 (Bell 1984b:323). Their artifact inventory includes numerous hide processing tools: bone beamers, bone "hide grainers," diamond-shaped beveled knives, and endscrapers (Bell 1980: 65; 1984b: Figures 14.3-14.5), the latter two, as we have seen, considered diagnostic of participation in the Southern Plains hide trade (Creel 1991). It also includes various items indicative of contact with Southeasterners: a conch shell ornament, a fragment of a decorated stone earspool, and occasional specimens of Southeastern pottery. The most notable of the latter is a human effigy vessel generally considered an import from the Tennessee-Cumberland area of Tennessee (Bell 1984b:322). Furthermore, their artifact inventory is such that evidence that they frequented the Sanders site could easily reside in the unfortunately still-unstudied collections from the middens at Sanders (which Krieger did, after all, attribute to an occupation by Plains people; he may have been partly right about the attribution but wrong about the time). At the moment the best evidence that something of this nature did go on at Sanders is a single smudged black Puebloan sherd that probably came from southeastern New Mexico (Krieger 1946:197, 208).

The evidence for trade downriver to the Caddo country is stronger, although I suspect that the trade upriver was more important. A Haley Engraved bottle (Krieger 1946: Figure 15) shows that the Spiroans were in touch directly or indirectly with Haley phase (ancestral Kadohadacho) people about 150 miles away in the Great Bend region of Southwest Arkansas (Schambach 1982b). Hones of white Catahoula sandstone (Krieger 1946:203) came from farther south in northwest Louisiana. Some 150 sherds of shell-tempered Nash Neck Banded jars (Krieger 1946:197) suggest contacts with Caddoan salt producers in the Little River region of southwest Arkansas and hint that one of the commodities moving upriver was salt. The rare Mississippi Valley prestige goods found at Caddoan sites in the Red River Valley, viz. the Spiro-related conch shell cups and gorgets (Phillips and Brown 1978:165-168) found at the Rhoden site in McCurtain County, Oklahoma, the Bowman site, in Little River County, Arkansas, and the Belcher site in Caddo Parish, Louisiana, also the plain shell cups found at the Foster and Friday sites (Moore 1912: Figures 76, 77, 86), probably passed through the Sanders site entrepot on their way down from Spiro. So too, probably, did the painted bottle from the Haley site that Moore considered "an import from Southeastern Missouri" (1912:550; Plate XXXVIII). The previously inexplicable population of Central Mississippi Valley bird-effigy bowls, or very good local copies thereof, many of them of the "tail rider" variety, that centers in Lafayette and Miller counties in extreme southwest Arkansas (they are not found farther east in the Caddoan area) and in Cherokee, Harrison, Titus, and Red River counties in northeast Texas (Suhm and Jelks 1962:47-49; Plate 24) certainly owes its existence to the Sanders entrepot. These vessels occur in a tight cluster, the northwestern edge of which is located precisely south of the confluence of the Kiamichi River with the Red River. Distributional evidence doesn't come much better than that.

Fifteen sherds "definitely of Titus Focus types" point to contacts with northeast Texas Caddoans in the Sulphur River drainage (Krieger 1946:197). To Krieger's surprise there were also "at least 15 sherds of Frankston Focus types"; these indicate contacts with ancestral Hasinai Caddo people living 100 to 150 miles south of Sanders in the Neches, Angelina, and upper Sabine valleys (Krieger 1946:197).

What kinds of goods were being accumulated at Sanders for portage up the Kiamichi and Poteau valleys to the Arkansas Valley? Since, as we have seen, the Spiroans of eastern Oklahoma had access to bison products and were processing bison hides, there was no need to carry heavy and bulky items of that nature north for the Arkansas Valley trade, although they were probably being moved down the Red River to the Caddo country. Judging from traded specimens found at or near Spiro (Brown 1983; 1984a:245-262; Rohrbaugh 1982:538), the goods that were carried north from the Sanders site entrepot included cotton cloth, perhaps in quantity (also probably some of the types of textiles of unknown source found in the Craig mound, such as the woven bison-hair skirts and bags and some types of baskets), artifacts of Alibates flint (including diamond-shaped beveled knives), Red River jasper, and long-stemmed Caddoan tobacco pipes of the Red River type.

Brown (1983:144, Table 4) recognizes that pots of the Red River Valley types Haley Engraved, Handy Engraved, and Avery Engraved are probably trade items at Spiro, so they should certainly be added to this list. So should every vessel of the early Caddoan types Crockett Curvilinear Incised, Pennington Punctated Incised, Holly Fine Engraved, Hickory Engraved, and (the misnamed) Spiro Engraved whose presence–in extremely small numbers at an equally small number of Arkansas Valley sites–has done so much to cloud our thinking about the nature of the Arkansas Valley tradition. There are, after all, only 18 vessels and 74 sherds of Crockett Curvilinear Incised in the Spiro collections that Brown studied, and only 22 vessels and 108 sherds of Spiro Engraved (Brown 1971:82, 109). Brown's estimates that the 108 Spiro Engraved sherds represent another 92 vessels and the 74 Crockett sherds another 74 are high. The next largest collection of these types is from the Harlan site, where Bell (1972:243-247) found 7 Crockett Curvilinear Incised, 5 Pennington Punctated Incised, 5 Spiro Engraved, 4 Hickory Fine Engraved, and 1 Holly Fine Engraved vessel, but almost no sherds. Outside of these two collections, vessels of these types are scarcer than hen's teeth. On the basis of information I present below on the characteristics of the Arkansas Valley ceramic tradition, I suspect that the total number of vessels of these five types, (including vessels represented by accurately identified sherds) that could be confirmed from all Arkansas Valley tradition collections would be in the neighborhood of 100 to 150. That is not too many for a few decades of overland trade out of the Red River valley via a Sanders site entrepot and the Kiamichi Valley. Not for traders who could move 3,000 to 4,000 conch shell cups (Brown 1975:151) up the Arkansas River to Spiro, presumably from an entrepot about 150 upriver miles away at Point Remove.

There is evidence suggesting that infectious diseases as well as goods moved–with serious if not disastrous results–from the Southwest via the Sanders entrepot or perhaps another on the Canadian River (see below) to the Arkansas Valley and then to the Mississippi Valley. As I understand the bioanthropological literature, which is not as clear as it might be on this point, the childhood osteitis and osteomyelitis that account for the epidemic level infection rates (67% to 85%) in the Spiroan populations from the Morris and Horton sites in eastern Oklahoma (Brues 1958, 1959; Burnett 1988:212-214) are rare to absent in populations of all periods east of Spiro prior to the late Mississippi period, at which time they appeared (as part of a "dramatic rise" in infection rates from 35.3% in the Middle Mississippi period to 90% in the Late Mississippi period) as adult-level infections in northeast Arkansas (Powell, Bridges, and Mires 1991; Burnett 1988:150-151; Rose *et al.* 1984:412). This Late Mississippi period increase in infection rates is presently attributed to population growth and the appearance of large towns and "widespread trade" (Burnett 1988:150-151; Rose *et al.* 1984:418), which is probably true except that the trade was more widespread than we have thought.

The reason for the absence of osteitis in subadult populations in the Southeast is that it is diagnostic of endemic syphilis, a treponemal disease of childhood that is so strongly associated with arid regions that Hackett (1963:8) has remarked that it should be called "treponaridosis." My biomedically untutored evaluation of the situation in eastern Oklahoma (where endemic syphilis has been diagnosed; see Brown 1984a:259) is that that area was much too humid for endemic syphilis to have developed locally, and that the high frequency and severity of the disease as it is manifested in the skeletons from the Morris and Horton sites bespeak a recent introduction from the Southwest. The vector would have been children who were brought from there, possibly for adoption or for use as slaves, neither practice being unheard of in North America in the post-Colombian era. It would appear that in the course of the resulting epidemic among the children in the Spiro area, this disease and whatever disease was responsible for the osteomyelitis spread in the classic manner to the immunologically unprotected adult population, probably producing what Burnett (1988:151) describes as "chronic and extremely debilitating infections."

The broader epidemiological question, should there be any truth in the foregoing, is: were these and perhaps other diseases of Southwestern origin involved in the collapse of Spiro and other major Mississippian centers about A.D. 1450, and in the Mississippian population collapse that most bioanthropologists believe was underway before the De Soto entrada? Did Spiroan traders bring down Mississippian culture by introducing diseases from the Southwest?

If the Spiroans were the traders I make them out to be, there should be other Spiroan entrepots along the Canadian and Arkansas rivers in the plains country of central Oklahoma. However, if they are like the Sanders site, they will be hard to identify from surface debris or midden excavations alone. The evidence that brought the Sanders site to our attention was all in the graves. Had they not been found, the Sanders site would today be passing unnoticed as a Plains Village component. So any Plains Village site in the Arkansas and Canadian drainages could suddenly emerge as another Spiroan entrepot. One good possibility is the Nagle site (Shaeffer 1957) on the North Canadian River near Oklahoma City. There, in an accidentally discovered cemetery, four graves that were professionally excavated after machinery destroyed 12 others all contained–shades of Sanders–exotic artifacts, probably out of the Spiro phase of the Arkansas Valley tradition (Shaeffer 1957:93-97). There were two Woodward Plain jars, one "marine conch shell" bead "identical in shape with necklace beads from Spiro Mound," and five triangular side-notched arrowpoints that Griffin (1961:30) calls "similar to the Cahokia side-notched forms." Two copper-covered sandstone ear-spools were found by a visitor in a trenched area between the four graves that were salvaged. According to Shaeffer and Griffin, both are Baerreis's type A, one of the types he considered diagnostic of the Spiro focus (Baerreis 1957:34), now the Spiro phase.

Much like the Sanders site population, the skeletal population from Nagle is one with bone lesions suggesting "a totally different series of health problems" from those exhibited by populations from nearby Central Plains sites. (Owlsley and Jantz 1989:140). The 20 whole and fragmentary skeletons that were salvaged exhibit "a severe mortality profile, associated with pronounced evidence of bone disease" indicative of dietary deficiencies, possibly scurvy and a syphilis-like bone disease (Owlsley 1989:131; see also Brues 1957). Brues (1959:66) linked this population to the Morris and Horton site populations of eastern Oklahoma on the basis of similar paleopathology, particularly the evidence of a syphilis-like bone disease and on the basis of the frontal-occipital cranial deformation exhibited by skulls from these three sites. The latter is also in evidence at the Sanders site, as she pointed out in her Nagle site report (1957:104), and–unknown to her in 1957-59–at Spiro itself (Brown 1984:159). The cranial deformation identified at Nagle, like the osteological evidence of pathology, is not reported for other central Oklahoma sites. In fact Bell (1984:309) states that "There is no suggestion of any skull deformation" in the skeletons, also studied by Brues, of the Washita River "focus" people who frequented the Oklahoma City area between A.D. 1000 and 1400. Thus, the artifacts and the skeletal evidence from Nagle indicate an occupation by Spiroan intruders who were, it would appear, operating a wide-ranging trading enterprise at significant epidemiological cost to themselves and probably to their clientele.

THE ARKANSAS VALLEY CERAMIC TRADITION

Perhaps the most bizarre aspect of the Northern Caddoan Area paradigm is that it links an area that had what must have been one of the weakest ceramic traditions in the Southeast with an area that had one of the strongest, an area that had a plain pottery tradition with an area that had an unusually rich decorated pottery. This has happened at the expense of misrepresenting the basic pottery types in the Arkansas Valley ceramic tradition as Caddoan types, when they are in fact Mississippi Valley types, and of misinterpreting and overemphasizing the small quantities of Caddoan pottery that are present. Thus, Arkansas Valley specialists seem to believe that they are working with Caddoan pottery, and they appear to have no concept of how different the Arkansas Valley ceramic tradition really is from the Caddoan ceramic tradition and, in some ways, from other Mississippian traditions to the east. Obviously there is a need to elucidate these differences and to set the record straight on the matter of the affiliations of the basic types in the Arkansas Valley ceramic tradition.

In terms of the sheer numbers of sherds and pots on record, the Arkansas Valley ceramic tradition is exceptionally weak compared to either the Caddoan area or the Central Mississippi Valley. Only one collection from one site, the 191 pots and 17,552 sherds (Brown 1971:1) in the W.P.A. collections from Spiro, is respectable by Caddoan area and Mississippi Valley standards. But that collection is no more representative of the Arkansas Valley ceramic tradition in terms of quantity than the engraved shell from the Craig Mound is representative of the quantities of engraved shell to be found at other Arkansas Valley sites. According to data tabulated by Wyckoff (1980: Tables 106, 108, 110, and 112), there were, as of 1980, only 24,758 additional sherds and whole vessels, mostly sherds, on record for all other Arkansas Valley sites in Oklahoma–and that is an inflated figure because it erroneously includes all the Fourche Maline sites south of the Arkansas Valley in the Poteau drainage of eastern Oklahoma. The next largest collection is from the 22-acre Harlan site (comprising "seven mounds and associated village structures and debris") that yielded a meager 63 pots and 1446 sherds (Bell 1972:245-252; Figure 3 and Table 14) during three field seasons of excavations in all the mounds as well as 160 five by five foot squares. The ceramic collection from the largest village on record in the Arkansas Valley in Oklahoma (the extensively excavated but unreported Reed site in the Grand River Valley, the type site for Woodward Plain pottery and a site possessed of 22 excavated house locations, four midden areas, five refuse areas, a burial mound, and a flat-topped mound) consists of 3,489 sherds (Purrington 1971:391; Wyckoff 1980: Table 2). The more recently excavated, and thoroughly reported, Horton site in the Arkansas Valley, the locus of an "important village" according to Wyckoff (1970:11, 24 and 179), yielded 1,210 sherds from 72 five by five foot squares. The excavation of more than 126 five by five foot squares at the Morris site, a large habitation site in the Illinois valley, produced 1,282 sherds (Bell and Dale 1953). Extensive excavations in the ten acres or so of habitation area at the Cookson site, near the confluence of the Illinois and the Arkansas, uncovered 32 pits and the post mold patterns of six houses, but produced only 1,347 sherds (Isreal 1972:10-11, 182-183). Excavations at the Jug Hill, Harvey, and Sheffield sites, respectively, yielded 712, 1,384 and 1,052 sherds (Wyckoff 1980: Table 101). The W.P.A. ceramic collection from all 15 Spiro phase and Fort Coffee phase cemeteries and habitation sites in the Spiro locality comes to 178 whole vessels and 13,918 sherds (Rohrbaugh 1982:400) or about 12 pots and 900 sherds per site.

Excepting Rogers' recent work at Spiro, all other published collections are under 400 sherds each, with some sites, such as the four-house McCarter site (Schaeffer 1957), producing barely more sherds (16) than house patterns; for example: Fine, 23 sherds (Eighmy 1969); Plantation, 353 sherds (Briscoe 1977:129-134); Brackett, 397 sherds (Bareis 1955:8-13); Bacon, 9 sherds (Proctor 1953:51-52); Cat Smith, 321 sherds, (Wyckoff:1967); Lillie Creek, 365 sherds (Wyckoff 1980: Table 101), and Tyler, 306 sherds (Wyckoff 1980: Table 101).

Recent excavations by Rogers and others at Spiro have produced published sherd counts that seem to belie these figures, but they actually reflect superior recovery techniques. Most of the sherds are small–crumbs, really–of a size that earlier excavators did not collect. The excavation of 75 one by one meter squares in Area E, House Mound 5, produced 3,052 sherds, but 1,124 of them were too small to sort except as "indeterminate plain." The rest apparently were not much larger, because the only attribute that could be used to sort Williams Plain and LeFlore Plain (normally sorted according to surface finish and temper) was sherd thickness (Peterson, Moore, Svec, and Rogers 1982:120-130). Rogers' recent work at the Copple mound, part of the Brown Mound group in the Spiro complex, produced 999 sherds from 14 one by one meter squares, but 599 were crumbs too small to permit observations on surface treatment (Swenson 1989: 187-199).

Compared to the Caddoan area and the Mississippi Valley, these figures are clearly and uniformly paltry. The Belcher site, a minor middle to late Caddoan ceremonial center in the Red River Valley in northwest Louisiana (Webb 1959:118), produced a slightly larger collection than Spiro itself: 195 pots and 19,300 sherds. The Davis site in east Texas, a smaller site than Spiro, with only three mounds, but of comparable age and with some pottery types in common, produced over 96,000 sherds representing an estimated minimum of 5,031 vessels (Newell and Krieger 1949:75). The late Caddoan Cedar Grove site, a three-house farmstead in the Red River Valley in southwest Arkansas, yielded, during the excavation of less than 10% of the site, 9,262 sherds (counting only those over 12 mm across), and 67 whole pots from 12 graves, 7 of which had been partially looted (Schambach and Miller 1984:109). At the Standridge site, a tiny Late Caddoan, Ouachita Mountain ceremonial center in the Caddo valley, a site so small it was originally thought to be a farmstead and the barely visible mound a natural rise, partial excavation of the mound produced 26 pots and 3,903 sherds (Early 1988:61).

These figures, not unusual for Caddo habitation sites and ceremonial centers, pale in comparison to those from Caddo salt-making (and, I suspect, pottery-making) sites in southwest Arkansas and northwest Louisiana where sherds can be collected by the hundreds of thousands. For example, the Arkansas Archeological Survey's recent test excavations at the Holman Springs salt site near De Queen produced well over 100,000 sherds from some 20 two by two meter squares, more than twice the number of sherds from all reported eastern Oklahoma collections.

There is a pronounced and undoubtedly significant disparity between the numbers of whole vessels in collections from eastern Oklahoma and the Caddoan area. The three largest recorded eastern Oklahoma collections are the 191 vessels from Spiro (Brown 1971:1), the 63 from Harlan (Bell 1972: 245-252), and the 178 from the 15 Spiro and Fort Coffee phase sites in the Spiro locality (Rohrbaugh 1982:400), a total of 432 vessels. Although I have not made a report by report count, I doubt that there are many more than 50 additional vessels on record from all other sites, and certainly no more than 100. Using the latter figure, to be on the safe side, there would be about 532 vessels in recorded collections from all sites in eastern Oklahoma. This is barely more than the combined (recorded) collections from just two Caddoan sites in southwest Arkansas, the Foster site in the Red River Valley where C.B. Moore (1912) removed from 11 burials in an inconspicuous mound 50 feet in diameter and 4.5 feet high "no fewer than 246 vessels of earthenware and probably many more," and the Washington Mound group in the Ozan Creek drainage where M.R. Harrington (1920:62-63) found 223 vessels associated with 88 skeletons. The Arkansas Archeological Survey's photo files contain pictures of approximately 8,000 whole vessels from Caddoan sites, mostly in southwest Arkansas and, on the basis of 25 years of work in that area, I estimate there are that many more in unrecorded private collections. It would probably be no exaggeration to say that about 20,000 pots have been removed from graves and other contexts in the Caddoan area of southwest Arkansas, northwest Louisiana, northeast Texas, and southeast Oklahoma, or almost 20 times the total from sites of the Arkansas Valley tradition in Oklahoma. And in Oklahoma there has been more professional excavation due to all the government-sponsored work (see Albert 1984).

Fourche Maline sites, while less productive than Caddoan sites, are still more productive than Arkansas Valley sites. The W.P.A.-excavated Cooper site in the Middle Ouachita region of south central Arkansas yielded 7,321 sherds from 125 ten by ten foot squares, and the nearby Means site yielded 10,469 sherds from 125 ten by ten foot squares (Schambach 1970). Fourche Maline sites in the Poteau Basin of eastern Oklahoma are also productive, particularly when compared to Woodland and Mississippi period sites less than 40 miles north in the Arkansas Valley. The Williams site yielded 5,900 sherds (more than any Arkansas Valley site except Spiro) from a W.P.A. excavation of unspecified size (Newkumet 1940:2-6). The Wann site yielded 2,932 sherds from an excavated area 125 feet by 50 feet (Sharrock 1960). A 20 by 30 foot excavation at the Scott site produced 319 sherds (Bell 1953), and the Sam site yielded 1,070 (Proctor 1957:72).

Late Woodland and Mississippi period sites in the Central Mississippi Valley in northeastern Arkansas routinely yield sherd collections of the same magnitude as those in the Caddoan area. For example, the Late Woodland Dunklin phase component at the Zebree site produced a statistically controlled collection of 12,042 sherds, indicating a population of 1.2 million sherds, or 1000 to 1500 pots, for the site, a small village with few houses (Morse and Morse 1987:16-19; 17-33). Additional work at Zebree upped the Dunklin phase collection to 35,072 sherds and produced 65,406 sherds from contexts of the early Mississippi period Big Lake phase (P. Morse and D. Morse 1990:53-55). And at the Bangs Slough site in the Felsenthal region of southeast Arkansas–a Lower Mississippi Valley region–I collected 5,757 classifiable sherds from 25 one by one meter squares (Schambach 1990a:1).

Considering these figures, the average Arkansas Valley tradition site in Oklahoma is practically aceramic. It is possible that more complete reportage of major sites such as Norman, Hughes, School Land I and II, and Lillie Creek, whose ceramic collections are unstudied, would alter the picture of meager ceramic assemblages that the reported sites present. But considering the data from the post-W.P.A excavations at Harlan, Horton, and Spiro, I think not.

The Caddoan area ceramic tradition was fundamentally a tradition of decorated pottery comprising a fine ware and a utility ware, each exhibiting a profusion of decorative techniques, designs, and vessel forms that we have classified into dozens of types and varieties. The Arkansas Valley tradition was fundamentally a plain ware tradition dominated by four types: Williams Plain, Woodward Plain, Sanders Plain, and Poteau Plain. The scarcity of decorated fine wares at all sites is phenomenal compared to the Caddoan area. It suggests that all fine ware specimens (other than the slipped plain types Poteau Plain and Sanders Plain and their apparent companion types, Sanders Engraved and Maxey

Noded Redware) found on Arkansas Valley sites are vessels that were imported from the Caddoan area or the Mississippi Valley (see Brown 1983: Table 4). The same is true of the few decorated utility-ware types (apart from Woodward Applique and Braden Punctated) that occur. Clearly, the Spiroans imported their decorated pottery, to an extent probably unequaled elsewhere in eastern North America, just as they imported their engraved shell and their copper plates. Decorated pottery is one more manifestation of the extraordinary trade network that they developed.

How abundant (or how scarce) is decorated pottery on Arkansas valley sites? In 1980 Wyckoff identified "Pottery, arrowpoints, and other artifacts diagnostic of Caddoan culture . . . at 220 sites in the Arkansas Basin of eastern Oklahoma." One hundred and ninety of these yielded pottery. Although this list includes Fourche Maline sites from the Poteau Basin, Wyckoff's tabulation, by temper and decoration, of the pottery from these 190 sites can be taken as a reasonably accurate representation of the incidence of decorated pottery in the Arkansas Valley tradition (Wyckoff 1980: Tables 106, 108, 110, and 112). There are just under 1,000 sherds and vessels, mostly sherds, decorated with incising or engraving from the 190 sites. This amounts to 2.4% of the 42,501 sherds and vessels on record for the Arkansas Valley and the Poteau Basin.

In the Caddoan area decorated pottery is about 20 times more abundant. On Caddoan sites of all periods decorated sherds account for 40 to 50% of the collections. At the early Caddoan Davis site approximately 38% of the 96,000 sherds were decorated (Newell and Krieger 1949:127-128). At the middle to late Caddoan Belcher site 56% of the 19,300 sherds were decorated (Webb 1959: Table 1). At the late Caddoan Cedar Grove site 47% of the 7,674 sherds were decorated (Schambach and Miller 1984).

Returning to Wyckoff's tabulation, sherds or vessels of all the engraved types found in the Arkansas Valley (Arkadelphia Engraved, Friendship Engraved, Glassel Engraved, Haley Engraved, Handy Engraved, Hickory Engraved, Spiro Engraved, and Sanders Engraved) occurred in only 23 of the 190 ceramic components, leaving 167 Arkansas Valley "Caddoan" components without the *sine qua non* of a Caddoan ceramic assemblage. The total number of sherds and vessels–the table does not distinguish between them–is just 400, or .9%, of the Arkansas Valley and Poteau Basin sample.

Incised fine wares are just as scarce. The common grog-tempered, incised, fine-ware types in the Arkansas Valley (of which the first three are Caddoan-area imports) are: Crockett Curvilinear Incised, Pennington Punctated-Incised, Davis Incised, Canton Incised, Coles Creek Incised, French Fork Incised, Williams Incised, and Beaver Pinched (Wyckoff 1980: Tables 106, 108, 110, and 112). These occurred in 23 of Wyckoff's 190 components (not the same 23 as the engraved wares, although there is some overlap), and the total of sherds and vessels is 367, or .86%, of the Arkansas Valley-Poteau basin collection.

Decorated shell-tempered pottery, which includes the fine-ware type Avery Engraved, the utility-ware types Nash Neck Banded, Braden Punctated, Neosho Punctate-Incised, and Delaware Cordmarked, and the Central Mississippi Valley type Nashville Negative Painted, occurs in 16 of the 190 components, represented by only 223 sherds and vessels, or .52% of the collection. Of these types only Avery Engraved and Nash Neck Banded are Caddoan.

The main indigenous types in the Arkansas Valley tradition are the grog-tempered plain types Williams Plain and LeFlore Plain, the shell-tempered plain type Woodward Plain, and two slipped types, Sanders Plain (red slipped) and Poteau Plain (black or red slipped). At least two of these, Woodward Plain and Sanders Plain, are Middle Mississippian types that Arkansas Valley archaeologists, locked in the Northern Caddoan area paradigm, have Caddoanized by first failing to notice and, later, by ignoring their real relationships.

Woodward Plain, defined by Robert L. Hall in an unpublished Master's thesis in 1951 (Freeman and Buck 1959), is the cornerstone of the Northern Caddoan Area concept, as far as pottery is concerned, in the sense that more than 35 Arkansas Valley "Caddoan" components apparently stand identified on the basis of this type alone; no fewer than 50 "Caddoan" assemblages with more than one pottery type are dominated by it (Wyckoff 1980: Tables 106, 108, 110, and 113.). To Arkansas Valley archaeologists Woodward Plain, which does not occur in the Caddo area except for occasional imports in border regions (e.g., Early 1988:70), is a good diagnostic Caddoan type. Thus, Brown (1984b:23) cites "shell-tempered Caddoan pottery," by which he obviously means Woodward Plain, in partial support of his argument that the Loftin phase of the White River valley is a "north Caddoan" (*sic*) phase. But the "Caddoan" identity of this type is derived entirely from the Northern Caddoan Area paradigm; it is considered a "Caddoan type," not even, mind you, a "Northern Caddoan" type, because it occurs on sites in the Northern Caddoan area.

In the first thorough study of the pottery from the Spiro site Brown, (1971:144) pointed out that Woodward Plain is "a regional version of the shell-tempered jar form so ubiquitous in the mideastern region and the part of the lower Mississippi Valley south to Memphis. It can be reliably distinguished from Neeley's Ferry Plain only by the flat disc base." Neeley's Ferry Plain is now called Mississippi Plain (Phillips 1970:130). According to the basic rule of ceramic typology as it is practiced in the Lower Mississippi valley, the rule of sortability (which states that types must be sortable as sherds; Phillips 1958:119; 1970:26), Woodward Plain should be reduced to variety status within that type. It should be called Mississippi Plain, *var. Woodward*.

Not surprisingly, three "real" Neeley's Ferry Plain/Mississippi Plain pots have been identified at Spiro because of their round bases (Brown 1971:146). This raises a question Arkansas Valley specialists have not dealt with: how much Mississippi Plain of varieties other than the Woodward variety is concealed in the Woodward Plain sherd counts from Spiro and other sites in eastern Oklahoma?

Unlike the engraved and incised types discussed earlier, slipped plain pottery is a significant, locally produced element of the Arkansas Valley tradition. Wyckoff's tabulation (1980: Tables 106, 108, 110, and 112) shows 1,656 sherds and pots of Sanders Plain and Poteau Plain compared to 1,000 specimens of incised and engraved pottery belonging to some 21 different types of diverse ages and provenances. That makes either slipped type about 13 times more common than any incised or engraved type. Slipped plain pottery is rare in the Caddoan area, where there are no red or black slipped types *per se*, although a few incised and engraved types such as Crockett Curvilinear Incised and Hickory Engraved are sometimes slipped, and one, East Incised, is generally slipped. But it is, of course, common in the Central Mississippi Valley as the type Old Town Red.

Sanders Plain, a red-slipped type, at least some of it tempered with finely ground shell, is represented at Spiro by 26 pots and 2,339 sherds, making it second only to Woodward Plain in numerical importance. When Brown (1971:164-169) studied the Spiro pottery, he observed that this type has the same relationship to the Middle Mississippian type Old Town Red (a basic mortuary and ceremonial type of the Parkin and Quapaw phases; Phillips 1970:145) that Woodward Plain has to Mississippi Plain, i.e, these types are "not distinguishable in paste and surface treatment." This need not have surprised him, since Krieger's (1946:186-190) perfunctory and overly loose definition of the type is based on what would now appear to be no more than 21 imported Old Town Red bowls found in the graves of peripatetic Spiroan traders 150 miles from home at their Sanders site entrepot in the Red River Valley. But he accepted Sanders Plain at its face value as a Caddoan type and was then constrained by the Northern Caddoan Area paradigm to find some way to distinguish it from Old Town Red. Therefore, he "decided that the appearance of rounded bases would define Old Town Red bowls and flat bases Sanders Plain vessels" (1971:180). He justified this type distinction with the claim that it was based on "major differences in vessel shapes." Even if that was actually the case, it would be an inadequate justification because vessel shape alone has never been considered a valid reason for type-level distinctions in either Caddoan area or Mississippi Valley ceramic typology, wherein bottles, bowls and jars are routinely lumped in the same types. Nor would it be valid in the specific case of Old Town Red, which includes a plethora of shapes. But Brown's decision wasn't really based on "differences in vessel shapes"; it was manifestly based on one difference between vessels belonging to one shape category, the bowl. Basing a type distinction on a difference of that order is probably unprecedented, and it is certainly impractical because it ignores the eminently sensible rule that types must be sortable as sherds. Since there are, according to Brown's classification, 10 round-bottomed Old Town Red bowls in the Spiro collections and basal fragments of 15 others, which establishes that the type was present in the Arkansas Valley as either a trade ware or an indigenous ware, his decision to make base form the sole diagnostic of Sanders Plain rendered unsortable all the red-slipped body sherds and rim sherds at Spiro (2,692 of them; Brown 1971:207; Table 44) and elsewhere in the Arkansas Valley. Put another way, Sanders Plain isn't very useful as a taxon because, despite the broad use to which it is being put, it can only include whole vessels and base sherds; by definition there is no such thing as a Sanders Plain rim sherd or a Sanders Plain body sherd. Sanders Plain, as redefined by Brown (Krieger's definition being too loose, as Brown points out; 1971:168-179), must be recast into one or more Arkansas Valley tradition varieties of Old Town Red. And I see no reason not to do the same with Poteau Plain.

Proper taxonomy is important. Once Woodward Plain, Sanders Plain, and Poteau Plain have been classified as regional varieties of Mississippi Plain and Old Town Red, Arkansas Valley specialists will find it more difficult to sustain the illusion that the ceramics of that area are in any sense Caddoan.

SHELL TEMPERED POTTERY IN THE ARKANSAS VALLEY TRADITION

Perhaps the most important question pertaining to the culture history of the Arkansas Valley, the question that exposes the most significant and pervasive error in the history of the archaeology of this area, is that of the temporal and geographical ranges of shell-tempered pottery. The source of this error was Krieger's well-known dictum (e.g., 1961:43) that shell temper did not appear in Caddoan pottery until, or slightly before, the historic period, which is a reasonably accurate generalization as far as the Caddoan area is concerned. The error arose when, under the auspices of the Northern Caddoan Area paradigm, that dictum was brought to bear on the pottery of the Arkansas Valley tradition. The result was that significant quantities of shell-tempered pottery in important early assemblages, most notably those from Spiro itself, were either not seen, somehow, or simply could not be credited as being early because everyone knew, with the certainty that only a paradigmatic principle can give, that there was no shell-tempered pottery in the Arkansas Valley until around A.D. 1450. Until the publication of Brown's study in 1971, which informed us, finally, that "Shell temper is the sole paste in jars of the Spiro phase" (Brown 1971:146), the following summary by Bell and Baerreis (1951:41-41) was the conventional wisdom as to the pottery at Spiro:

> The Spiro Focus pottery wares require careful analysis and identification before anything definitive can be accepted. The pottery represented, however, falls into two main groups, the common utility ware and the various wares associated with the burials. The utility pottery is represented by a rather thick, granular clay tempered ware. The distinctive burial ware has a similar paste and tempering material but the sherds are thinner.

Only an outsider to "Caddoan" archaeology, James B. Griffin, demurred. In 1958, in a paper presented at a Society for American Archaeology symposium titled "Relationships Between the Caddoan Area and Neighboring Areas," he stated: "It has now been established that with a significant number of burials within the Craig Mound there are pottery vessels which are clearly of Middle Mississippi origin, either by trade or some other means. It would be interesting to learn some day what proportion of vessels and sherds from the Craig Mound are shell tempered" (Griffin 1961:28-29). Krieger (1961:43) responded with a reassertion of the status quo: "Griffin's observations do not change the generalization that shell temper is completely absent or exceedingly rare in all Caddoan foci except some which date close to the appearance of European trade goods, or afterward, as in Fort Coffee, McCurtain, and Glendora." In a roundtable discussion at the Seventh Caddo Conference in 1963 Griffin went back on the attack with a probing question followed by a series of statements and an exchange with Gregory Perino, another outsider at that time, that amounted to a frontal assault on the Northern Caddoan Area paradigm itself. His final remark suggests he was aware of that:

> Griffin: "Did Sears tell me that there was a fair amount of shell temper in the Spiro sherd collections?" Bell: "Most of it is in sherds and utility ware. There is a lot of Williams Plain in there too. That's the common ware."
>
> Griffin: "There is a definite misconception in the literature that while there might possibly be an aberrant piece or two of shell tempered pottery with burials at Spiro, there was very little shell temper in the Spiro sherd collection. Apparently on close examination, some people at least think there is a reasonable amount, that is, quite a good deal."
>
> Perino (a little later in the ensuing discussion of the age and source of shell tempered pottery in the Arkansas Valley): "But at Spiro, Jimmy, they've got those southern Illinois flint maces, and spuds, and blank faced bottles that are shell tempered."
>
> Griffin: "Yes, I know they have. I was just getting ready to say that any site that was dominated as much by the Central Mississippi Valley as Spiro was, will be bound to have a lot of shell tempered pottery."
>
> Perino: "It's amazing the amount of stuff I've seen that belonged to Cahokia and was made there."
>
> Griffin: "We'd better shut up now–I expect to hear shots ring out."
>
> McGimsey (moderating the session): "Why don't we take a coffee break." (Verbal exchange at the 7th Caddo Conference; Davis, Wyckoff, and Holmes 1971:29 and 32).

Despite Griffin's suspicions and assertions, the paradigm survived. The Fort Coffee focus, considered, as we have seen, the remains of somewhat impure, post-Spiro focus, Plains-influenced Caddoans, remained the dumping ground for most assemblages with shell-tempered pottery until Brown's study appeared. That, as we have also seen, triggered what I call the Fulton-to-Gibson data shift, the undoing of the hallowed concept of the Gibson-Fulton transition and the beginning of the reclassification by Rohrbaugh (1982, 1984) and others of most of the old Fort Coffee focus components into Brown's new Spiro phase (Brown 1984a, 1984b).

The point at issue these days, now that we know that the shell-tempered types Woodward Plain and Poteau Plain dominated the assemblage of the Spiro phase, is how early did shell-tempered pottery of these or other types appear and become important in the Arkansas Valley tradition? Recent excavations by the Arkansas Archeological Survey (Sabo and Guendling, ms. in preparation; Sabo *et al.* 1990) have put a floor under the discussion of this problem by establishing that horticultural, maize-eating, storage-pit-using people of their Middle to Late Woodland Rush Creek phase were heavy users of shell-tempered pottery (three times as much shell temper as grog temper) during their occupation of the Dirst site. That occupation is well dated to A.D. 600-650. The range of dates on the Rush Creek phase is A.D. 500 to 850, and the Dirst site is in the lower part of the Buffalo River valley in north central Arkansas, within the probable geographic range of the Arkansas Valley tradition as elucidated by Brown (1984b: Figure 2).

So the hoary problems of when shell temper entered the Spiroan ceramic tradition and from where it came (the Central Mississippi Valley, the Missouri Valley, the Plains, and the Ozarks have all been suggested at various times) is now narrowed down to the probability that its source was the central Ozark Rush Creek phase sometime after A.D. 600. One question that Arkansas Valley specialists must now consider is whether the Rush Creek phase extended westward into eastern Oklahoma; have Rush Creek phase components, or those of a local equivalent, been systematically misclassified because of their shell-tempered pottery? This is not unlikely given Brown's views (1984b) on the probability of close relations between the central Ozarks and the Arkansas Valley from the Archaic on and given the many eastern Oklahoma assemblages with little or no contextual data that contain obvious Middle Woodland artifacts along with both grog-tempered and shell-tempered pottery.

For example, who can prove that the major component at the Morris site (Bell and Dale 1953), located in the Illinois Valley (and now, unfortunately, inundated by the Tenkiller Reservoir), was really Spiro phase? The 55 infection-ridden skeletons from that site, which, as we have seen, testify that the Arkansas Valley adaptation wasn't as good as might be expected (Brues 1959; Burnett 1988:212), are Spiro phase on the evidence of the pottery from the graves. But the graves were part of a large, shallow cemetery (over half were less than 12 inches deep) that intruded on an earlier midden (Bell and Dale 1953:86). Related to the midden were nine refuse-filled pits and the post mold patterns of two roughly rectangular houses, both much cruder looking than the sophisticated two and four-center-post houses considered diagnostic of the Spiro and Harlan phases. The midden yielded an assemblage that, but for the presence of 734 shell-tempered sherds, could mostly be considered middle to late Woodland period: 569 grog-tempered sherds, 1,020 Gary points (out of 1,571 total points), 43 double-bitted chipped stone axes, 61 chipped stone hoes, 12 celts, 57 manos, 7 milling stones, and 4 boatstones. Bell and Dale could find no contextual basis for separating what they considered the typologically early or "Gibson aspect" traits in the midden from the "Fulton aspect" traits, most notably the shell-tempered pottery. They concluded that a single occupation "transitional in time and characteristics between the Gibson and Fulton aspects" was represented (1953: 131). Might it, instead, be transitional in time and character between the Woodland period and the Mississippi period?

There are, as of the last decade or so, two somewhat contradictory models for the introduction of shell tempering into eastern Oklahoma assemblages. Both are based on the stratigraphy in the burial mound (Mound 1) at the Harlan site, the type site for the Harlan phase, which has given them a great deal of credibility. One, presented by Bell (1984:238), holds that shell-tempered, Woodward Plain pottery is more or less diagnostic of the late Harlan phase, but was rare if not absent earlier, i.e., prior to about A.D. 1050: "Sometime during the occupation at Harlan . . . Woodward Plain becomes available, and it gradually increases in importance." (Bell 1972:262). The other, as described by Brown (1984b:16), holds that "In the early Caddoan Harlan phase (A.D. 950-1250) shell-tempered pottery appears for the first time in the traditional jar form with an everted rim and flat base . . . . During the Harlan phase shell-tempered jars slowly gained in popularity until they replaced completely grog- and grit-tempered jars in the Spiro phase." "Slowly" is the key word in that sentence. Brown (1984b:16) estimates an increase in incidence of from 2% to 31% of the plain pottery at the Harlan site over a time span that he apparently envisions as being in the neighborhood of 200 years.

Both of these models have probably outlived their usefulness because they cannot accommodate evidence, in the form of increasing numbers of radiometric dates, which suggests that shell temper came in rapidly, certainly no later than A.D. 1000, and that all Harlan phase villages will have ceramic assemblages with high proportions of shell temper. As a result, excavated assemblages that probably represent the entire domestic side of the Harlan phase are in limbo because no one can decide if they are Harlan phase or not. The dates say they are, but the models say they can't be. For that reason, the current description of the Harlan phase, which is based almost entirely on the type site, is a paragon of vagueness and equivocation when it comes to the domestic side of Spiroan culture during the Harlan phase (Bell 1984a:228 and 233). On the one hand Bell (1984a:228 and 232) necessarily referred only to the School Land I and School Land II sites–because they are probably the best examples–in his two sketchy paragraphs on Harlan phase settlements and subsistence. On the other hand he did not include them in the list of sites with Harlan phase components presented in his introductory paragraphs (Bell 1984a:221). Instead, they appear in a supplemental list of sites that Wyckoff has assigned, not to the Harlan phase, but to the limbo of the Harlan phase "time period" on the basis of "artifacts and radiocarbon dates." The reason Wyckoff (1980:172-186) put them there (along with the neighboring and similar Reed and Lillie Creek sites), and the reason Bell could not bring himself to remove them to the Harlan phase itself, is that there is a major discrepancy, according to the existing models, between their radiocarbon dates and their ceramic assemblages. Their radiocarbon dates (A.D. 1165 ± 75 for School Land II and A.D. 1080 ± 60, 1105 ± 75, 1160 ± 70 and 1225 ± 45 for School Land I; Bell 1974: Table 10.1) indicate occupation right in the middle of the Harlan phase (A.D. 950 to 1250), as do their square, four-center-post house patterns and their arrowpoints (Wyckoff 1984:172). But their ceramic assemblages, unanalyzed but evidently substantial by Arkansas Valley tradition standards, are more than 90% shell tempered (Wyckoff 1984:172), probably more like 98% in the case of School Land I (Duffield 1980:51).

Obviously, research along these lines has reached a stalemate that can only be broken by confirming the models with new data or getting rid of them. The unanalyzed W.P.A. data that seem to contradict them being what they are, there remains a slight possibility that the models are still usable. But, for two reasons, I think it is time to get rid of them.

The first is that they are both based on a probably unsupportable assumption as to the rate at which shell temper diffused into the Arkansas Valley ceramic tradition; namely that there was gradual increase, at a positively Darwinian rate, following a small-scale introduction that probably amounted to something on the order of 2% of the earliest assemblages, according to Brown (1984b:16). But why would it have taken several hundred years for an innovation that supposedly produces markedly superior pottery–hence its ultimate popularity in the Middle Mississippian realm–to spread through what we know to have been a limited ceramic complex in the small world of Spiroan culture in eastern Oklahoma? Why would it have taken 200 years or more for shell temper to increase from 2% to 31% at the Harlan site? Was it because potters weren't aware of its benefits? Or was it because most of them simply rejected the idea at first? In either case what kept the ball rolling after the 2% reception? What could possibly have caused a few more potters to use it in a few more pots each decade for the next few hundred years? Surely not differential reproductive success among the potters; not in the space of a few hundred years, even granting the possibility that cooking corn (not eaten extensively by the Spiroans) in shell-tempered pots has certain dietary benefits.

Second, both models were, it would appear, developed in the distorting light of a serious misapprehension of the actual statistical nature of the ceramic assemblage upon which they are based, i.e., the whole pots from the burials in Mound 1 at the Harlan site. In the case of a reasonably large ceramic assemblage from a single component at a habitation site, such as a village or farmstead, it is generally safe to assume, as we all do, that the pottery in hand is a valid sample of the population of pottery that existed throughout the cultural unit that is–as we often say–"represented" at the site. But that assumption cannot be made in the case of pots from burials in a mound at a vacant ceremonial center, particularly when small numbers are involved. The people who brought to Harlan the 53 pots found in Mound 1 probably did not select them randomly, and, even if they did, the statistics of small samples warns us that the probability of sampling error is high. Since, as I will show below, these 53 pots fall into three subsets of 6, 41, and 6 vessels that were deposited at three different times during the use-life of the mound, apparently on the order of 100 years or more, the probability that all subsets are equally representative is not high and the probability that the two subsets of six are valid samples is small.

Neither model takes these elementary matters into account, which, incidentally, explains why trade pots from the Caddoan area that appear in modest quantities at ceremonial centers (there were 19 in Mound 1), but almost never at habitation sites, have carried so much more interpretive weight than they should. It also explains why the solid looking contextual data that supposedly support the models, Bell's (1972:146, Table 12) stratigraphically based seriation of the burials and grave goods in Mound 1, does not mean what it is thought to mean.

Bell used his stratigraphic data to group the burials into "early," "middle," and "late" clusters, which had admittedly small, but nonetheless somewhat different, ceramic assemblages. The early cluster, consisting of 44 burials, contained one vessel each of the types Coles Creek Incised, Spiro Engraved, Hickory Engraved, and Crockett Curvilinear Incised, and two of Williams Plain. Significantly, it seemed, there was no Woodward Plain. The middle cluster, with 63 burials, contained 11 vessels of the type Williams Plain, 1 of Spiro Engraved, 2 of Davis Incised, 1 of Holly Fine Engraved, 1 of Sanders Plain, 5 of Pennington Punctated, one "Fingernail Punctated" specimen, 1 Hickory Engraved, 3 Crockett Curvilinear Incised and–again, seemingly significantly–8 Woodward Plain jars. The late cluster, which consisted of 17 burials, contained 1 Hickory Engraved, 1 Crockett Curvilinear Incised, and 3 Woodward Plain.

Taken at face value, this sequence indicates that Woodward Plain appeared midway through whatever part of the Harlan phase happens to be represented in Mound 1. Bell took it at face value, and he also assumed that most of the Harlan phase was represented, hence his model showing the appearance of Woodward Plain midway through the Harlan phase. But that is not the whole story, for Bell, ever careful and ever accurate, also points out that "This chronological sequence is not well supported by the unassociated artifacts found scattered throughout the burial mound when grouped as shown in Table 10" (1972:145). These "unassociated" artifacts were not specimens found out of context. They were specimens found without clearly associated human remains, which means they could be placed in the stratigraphic sequence, albeit with "considerably less confidence." Adding them to the sequence changes things significantly because, among other things, it puts a shell-tempered jar, and two shell-tempered sherds as well, in the early stage. Statistically speaking, even the tentative addition of one shell-tempered pot to the early stage sample of two Williams Plain pots presents the possibility that 33% of the parent population of plain, flat-bottomed jars in the village, hamlet, or farmstead from which these three specimens were drawn when they were brought to the vacant Harlan site ceremonial center as mortuary offerings were shell tempered. Because of the high probability of sampling error, it could easily have been higher or lower too. In any event Bell's model is compromised.

Hindsight is a wonderful thing. The significance of the statistical implications that arise from putting one shell-tempered pot in the early stage assemblage from Mound 1 are obvious from my 1992 perspective, which includes knowledge of shell-tempered assemblages from School Land I, School Land II, Reed, Lillie Creek, and other sites, with radiocarbon dates putting them in the middle of the Harlan phase. But to Bell or anyone else in the 1960s when the concept of the Gibson-Fulton transition was extant and the possibility of shell temper in the Arkansas Valley tradition prior to A.D. 1450 was questionable, it could not have been seen as anything but an aberration.

Be that as it may, the possibility of sampling error does rule in this particular situation, and it compromises both Bell's model and Brown's model in the following way. All of the whole pots, at least, were obviously brought from someplace else. Among them are two time-sensitive, decorated Caddoan types, Hickory Engraved and Crockett Curvilinear Incised, which are shared across the board, indicating near contemporaneity, ceramically speaking, for all three burial stages. Furthermore, there are, as far as we know, no significant differences between the time ranges of these types and those of all but one of the decorated types not shared across the board: Spiro Engraved, Holly Fine Engraved, Sanders Plain, and Pennington Punctate. These are all documented companion types of Hickory Engraved and Crockett Curvilinear Incised. The one apparent exception is the so-called Coles Creek Incised vessel, which gives the early burial stage an aura of significantly greater antiquity than the middle and late stages. But it is a typological monstrosity that carries no interpretive weight, the neck of some kind of wide-mouthed bottle (Bell 1972:245, Plate 26, Figure a), a vessel form that does not exist in the type Coles Creek Incised as it is known in its Lower Mississippi Valley homeland or elsewhere. There is no telling when it was made, and the resemblance to Coles Creek Incised may be accidental. Therefore, the ceramics indicate that sampling error, not time, is probably the factor behind the slight differences between the three assemblages. Furthermore, it is likely in this situation that only the comparatively large sample of 34 "associated" pots from the middle stage burials (compared to the sample of 6 from the early stage and 5 from the late stage) is actually representative of the parent population. That would indicate a parent population in which 42% of the plain ware was the shell-tempered type Woodward Plain. Figuring in the three "unassociated" Williams Plain vessels and the four "unassociated" Woodward Plain vessels that Bell (1972: Table 10) assigned to the Middle stage burials would raise to 46% the estimated frequency of Woodward Plain in the parent population from which all of the Mound 1 vessels were drawn.

What was the time span of that population? During what part of the Arkansas Valley ceramic tradition was the incidence of shell-tempered pottery around 44%? Unfortunately, none of the 40 Harlan site radiocarbon dates, which range from

A.D. 590 to A.D. 1340 (Bell 1972:253), is from Mound 1. Bell thought the activity represented there probably took place between A.D. 900 and A.D. 1200, the time of most of the mound building and other construction at Harlan (1972:258). The dates that are probably closest to the actual time span of Mound 1 are those on the remains of houses found in Mounds 4 and 6 that Bell (1972:142, 256) interpreted as the buried remains of the charnel houses where human remains and grave goods were kept before their disposal in Mound 1. It may not be coincidental that three such houses were built and ultimately burned on the site of Mound 4, which covers their neatly superimposed remains. Assuming there was a correlation between them and the three burial stages in Mound 1, the Mound 3 radiocarbon dates can be extrapolated as follows: the dates on the earliest presumed charnel house, House 3 (A.D. 970 ± 50, A.D. 990 ± 40, and A.D. 1030 ± 70), would apply to the early stage of Mound 1; the House 2 dates (A.D. 990 ± 50, A.D. 1050 ± 70, and A.D. 1110 ± 60) would apply to the middle stage; and the House 1 dates (A.D. 1090 ± 70 and A.D. 1180 ± 70) would apply to the late stage (Bell 1972:254; Tables 10, 12, and 15). A similar correlation with the single house represented by remains under Mound 6 would bring three more dates into the picture (A.D. 860 ± 60, A.D. 960 ± 40 and A.D. 960 ± 609; Bell 1972:257).

Thus extrapolated, the suite of Mound 4 dates suggests a time range for Mound 1 from about A.D. 990 to about A.D. 1100. This is an appropriate range for the Caddoan pottery types represented. If the Mound 6 dates happen to apply, the beginning dates would be about 20 years earlier. These dates indicate that shell temper was probably already well represented in the Arkansas Valley ceramic tradition, with an incidence of about 45%, around A.D. 1050 when, according to the Bell model and the Brown model, it was only weakly represented, with an incidence of about 2%, and was increasing at a Darwinian pace. They suggest that there is nothing anomalous about the apparently early shell-tempered assemblages at sites such as School Land I, School Land II, Reed, and Lillie Creek because the suites of dates from those sites (Bell 1984a: Table 10.1) indicate occupations that were later by a half century or so than the occupation represented by the pottery in Mound 1 at Harlan. The occupations at these sites apparently began about A.D. 1050 and ran through A.D. 1200. Once the intellectual debris left over from the dead concept of the Gibson-Fulton transition is cleared away, there is no reason not to consider them Harlan phase sites.

The last piece of that debris that needs to be dealt with is the belief, common among Arkansas Valley specialists (e.g., Wyckoff 1980:581), that because Reed, Lillie Creek, School Land I, and School Land II are the northernmost major Arkansas Valley tradition sites (all of them are located on the northern Grand River about 50 airline miles north of Harlan), they are not representative of what was going on at the same time farther south. Thus, Brown (pers. comm.) has suggested that the distribution of shell temper in eastern Oklahoma is "time transgressive" in the sense that it appeared earliest at the northern Grand River sites (see also Wyckoff 1980:581) and spread slowly south. The new radiocarbon dates from the Norman site are sufficient to demolish that idea. Norman, a major ceremonial center–with, we have seen, some occupation debris as well–is located about two miles from Harlan. Because its (alas!) unstudied ceramic assemblage is said to be mostly if not entirely shell tempered (Finkelstein 1940:4-5; Orr 1946:249-250), it has been considered the Spiro phase sequel to Harlan with an occupation beginning about A.D. 1200 when activities at Harlan were winding down. But the dates (A.D. 1020 ± 50, 1050 ± 50, 1050 ± 60, 1160 ± 50, 1170 ± 50, 1180 ± 50, 1240 ± 50, and 1250 ± 60; Albert 1992) are congruent with the School Land, Reed, and Lillie Creek dates.

Furthermore, there is some less convincing but noteworthy data that, in the absence of hard evidence to the contrary, suggest that shell tempering was similarly well established in the Arkansas

Valley between the Harlan and Spiro sites around A.D. 1000. A tiny collection of some 25 sherds from Trench C at the Fine site, located near the mouth of the Canadian River (Eighmy 1969:38-43), suggests an incidence of shell tempering in the 30% to 50% range. Carbonized house remains from Trench C returned radiocarbon dates of A.D. 1050 ± 70 and 1140 ± 80 (Bell 1984a: Table 10.1). At Spiro itself, the mound fill of House Mound 1 yielded, besides 455 grog-tempered sherds, 16 sherds of Woodward Plain, 1 of Poteau Plain, 9 of "shell in grog tempered" Williams Plain, 12 of Sanders Plain, 3 of Crockett Curvilinear Incised, and–just to confuse things mightily–one sherd identified as the Historic Choctaw type Chickachae Combed (Brown 1971: Table 41, p. 199). Charcoal from "Fire pit 5" in the house floor beneath the mound returned a radiocarbon date of A.D. 960 ± 70 (Bell 1984a: Table 10.1). Since, apart from the possibility of intrusive material from later occupations (e.g., the sherd of Chickachae Combed), specimens from mound fill either predate or are contemporaneous with what the mound covers, the sherds of Woodward Plain and Poteau Plain could pertain to occupation or activity in the vicinity of House Mound 1 at around A.D. 970. And, because mound fill is a secondary context, there is no necessary correlation between their 3.4% incidence among the ceramics therein and their incidence in whatever assemblage they might have been part of at that time.

There are two conclusions to be drawn from the foregoing discussion of shell tempering and the Harlan phase. One is that the Fulton-to-Gibson data shift probably hasn't gone far enough yet. It was fairly easy for Arkansas Valley specialists to shift a goodly number of old "Fort Coffee focus" components with shell-tempered ceramic assemblages into the Spiro phase (Rohrbaugh 1982, 1984). But they are balking over the prospect that the numerous (albeit poorly) radiocarbon-dated components that are now floating in the limbo of the Harlan "time period" because of their "anomalously" early shell-tempered ceramics (and, probably, other undated ones that are now misplaced in the Spiro phase because of their shell-tempered pottery) must be recognized as the overlooked domestic side of the Arkansas Valley tradition during the interval now known as the Harlan phase. A recent statement by Rogers ("It can be shown over and over again that Harlan phase sites do not have a predominance of shell-tempered pottery" [1991b:66-67]) shows that I am not beating a dead horse here. It also shows that thinking on Arkansas Valley archaeology will have to shift considerably before someone is willing to consider the possibility that some assemblages now above ground, like the Morris site assemblage, might fit somewhere between the beginning of the Harlan phase (i.e., right around A.D. 1000 where the earliest decent looking dates on assemblages with shell temper seem to hover) and the Rush Creek phase of the Central Ozarks–Sabo and Geundling's shell-tempered pottery users of the Middle to Late Woodland period. That is unfortunate because they will not be identified, even if they exist, as long as all assemblages with shell-tempered pottery are automatically considered late Harlan phase or later.

And, secondly, as the Fulton-to-Gibson data shift progresses toward whatever terminus it might reach, it is becoming apparent that the development of the Arkansas Valley tradition from A.D. 600 on was probably all but seamless and that it is time to rethink the four- (or sometimes three-) phase culture chronology now in use. Based as it is on the Caddoan-area, five-phase system (Brown, Bell and Wyckoff 1978:172), it is entirely an artifact of the Northern Caddoan Area paradigm; thus, from the point of view advocated in this paper, there is no reason to expect it to apply to the Arkansas Valley tradition. Nor is there any reason to expect a system that depends operationally on the unusually exuberant decorated ceramic tradition of the Caddoan area to be workable in the Arkansas Valley that, as we have seen, had a predominantly plainware tradition.

So, not surprisingly, this system seems to be unable to handle Arkansas Valley data and is simply collapsing in the face of it. As I have just tried to show, the Harlan phase, as presently defined, is almost useless as a descriptive and organizational concept because, since it relies mainly on diagnostics that are generally found only at ceremonial centers, it excludes domestic components of the cultural unit it is supposed to isolate and describe. If, however, it is redefined as I have suggested, which means bringing in the domestic units with their shell-tempered pottery–supposedly a Spiro phase diagnostic–it collapses into the Spiro phase for lack of other practical diagnostics. For the same reason–lack of practical diagnostics–Wyckoff (1974:57-58) and Rogers (1982:43-44) have collapsed the Evans phase into the "Early" Harlan phase; not the best solution, perhaps, but a good sign that there is a problem. And, at the upper end of the sequence, Rohrbaugh, as we have seen, has collapsed all but four of the old Fort Coffee focus components into the Spiro phase. I suspect that he should have gone further, because distinguishing a late Spiro phase assemblage from a Fort Coffee phase assemblage appears to be largely a matter of nuances that will be difficult to operationalize. Nonetheless, Rohrbaugh has shown the way here, which is, first, to discard the borrowed Caddoan-area chronology and all undocumented notions about the significance of shell temper and other traits and, second, to use only radiometric dates, carefully documented diagnostics, and assemblages from good contexts to rebuild the cultural chronology of the Arkansas Valley tradition from scratch, reexamining every assemblage from the smallest farmstead or campsite to Spiro itself.

WHO WERE THE SPIROANS?

Phillips and Brown (1978:21), after asserting that it is "obviously . . . not possible to pinpoint the descendants of the Spiroan people," go on to repeat, without much enthusiasm, the conventional view that they are the Wichita. The idea of a Wichita connection seems to have originated as a fallacious deduction from the idea behind the concept of the Gibson-Fulton transition, the supposed movement of Plains traits and, possibly, Plains people (the Wichita?) into the Arkansas Valley upon the occasion of the collapse of the Spiroan phenomenon. The fallacy is that, if such a movement did occur, the Wichita were clearly not the Spiroans, but the barbarians from the west who either replaced them or "diluted" their culture.

This idea should have passed into oblivion along with the concept that spawned it, since it couldn't possibly survive on its own. (The archaeological and bioarchaeological evidence indicates that the Wichita are descendants of Plains Villagers of the Washita River phase whose skeletal remains stand in strong contrast to Spiroan skeletal remains, which relate strongly to other Southeastern populations, not Plains populations; see Bell 1984b:323, 1984c:377 and Phillips and Brown 1978:21). But it has been kept alive by the Northern Caddoan area paradigm that, of course, requires that any possible descendants of the Spiroans be "at least linguistically . . . Caddoan" (Rogers 1991:67).

For their sins, the defenders of the Northern Caddoan area paradigm should be required to examine and defend some of the implications of that requirement, which are damaging to the theory that the Spiroans were Caddoan speakers of any stripe. The main one is the question of how Caddoan speaking Spiroans managed to participate in Middle Mississippian society and religion to the extraordinary extent that, in terms of the Northern Caddoan area paradigm, the remains at Spiro indicate. Certainly there was a great deal of bilingualism and intercommunication among members of the major Southeastern linguistic groups. But to assume inter-language group communication and a sharing of basic religious values of a scale and depth that would account for the Spiroan phenomenon is assuming one extraordinary phenomenon to explain another extraordinary phenomenon. Where, in the tribal world, are the ethnographic parallels for interaction of this nature? The choice here is to find them and construct a plausible model, or to drop the theory that the Spiroans spoke a Caddoan language.

Recent developments in Arkansas archaeology indicate that there was probably more of a gulf between Caddoan speakers and the Middle Mississippians than we might have thought a few years ago. Convention has had it that the latter were the ancestors of the Quapaw, whose language belongs to the same phylum, Macro Siouan, as the Caddoan language family (Voegelin and Voegelin 1966). But recent work by Hoffman (1977, 1987), Jeter (1986, 1990), Hudson (1986), and Brain (1988) has all but eliminated that idea. Most eastern Arkansas specialists (except the Morses; see Morse and Morse 1983:320) now think that the Mississippi period peoples of the Arkansas Valley (in Arkansas) and northeast Arkansas spoke one or more languages belonging to the Macro-Algonquin phylum, possibly something in the Muskogean family, possibly Tunican.

Recent work by Hudson (1986) on De Soto's route through sixteenth century Arkansas has produced circumstantial evidence of a linguistic barrier on the order of what might be expected between languages of different phyla. This seems to have been between the Mississippians of central and northeastern Arkansas and a group to the west, called the Tulla, who are generally assumed to have been Caddoan speakers. De Soto encountered it when he reached the settlement of Tanico, in the province of Cayas, located on the Arkansas River in the vicinity of Dardanelle, Arkansas (Hudson 1985: Figure 1). There he began preparing to move upriver toward the Fort Smith area, then the location of a province the people of Tanico called "Tulla." But for the first time during his journey, which might indicate an unusually profound linguistic gulf compared to the rest of the Southeast, he could not find an interpreter. The cacique of Tanico informed him that "the speech of Tulla was different from his: and because he and his forebears had always been at war with the lords of that province they had no converse, nor did they understand each other" (Quinn 1979: 134).

That speech, if it was not pure hyperbole concocted out of tribal animosities, poses another problem for the theory that the Spiroans were Caddoan speakers. If the less-than-ironclad ethnographic identification of the Tulla is correct, the Caddoan speakers–perhaps some Ouachita Mountain Caddo, perhaps some Wichita–who were in the Arkansas Valley around Fort Smith in the sixteenth century couldn't talk to Mississippians living downriver. That does not demolish the theory that the Spiroans were Caddoan speakers. Wichita is different from Caddo, which is the most divergent of the four languages in the Caddoan family (Story 1978:44), so it is entirely possible that the people of Tanico could talk to one group and not the other. But it does demonstrate the weakness of the assumption–implicit in that theory–that linguistic differences were not a significant factor in the interaction between the Spiroans and other peoples to the east.

I imagine that the Wichita theory has survived despite its flaws (which, in the aggregate, appear to be fatal) because it was the only game in town. With or without the constraints of the Northern Caddoan paradigm, the Wichita were the only apparent possibility. Now, however, as a result of recent work by a number of scholars on a variety of problems having nothing directly to do with the question of the missing heir to the Spiroan tradition, a new (and heretofore unlikely) pretender has appeared: the Tunica.

It now appears that they, not the Quapaw, were the Late Mississippi period occupants of the Arkansas Valley and much of the Mississippi Valley in eastern Arkansas. Brain has traced them northward from their present location–and near obscurity–at Marksville, Louisiana to the province of Quizquiz where De Soto encountered them in 1541 (Brain 1988:14; Figure 2). Although Brain would probably still disagree, Hudson and his associates (Hudson, Smith, and De Pratter 1990) have convincingly placed the province of Quizquiz in the vicinity of

Memphis. Since Brain and Hudson seem to agree that people culturally and linguistically identical to the people of Quizquiz were on both sides of the Mississippi, it appears that the provinces De Soto passed through in Northeast Arkansas and in the Arkansas Valley as far west as Tanico were Tunican provinces (Jeter 1986:39-41; Brain 1988b:22). Swanton suggested in 1911, in 1939, and in 1946 that, on linguistic grounds, the province of Tanico on the river of Cayas, then thought to be in the Hot Springs, Arkansas area, was a Tunican province (Brain 1988:22; Dickinson 1980). Now Hudson (1985), Hoffman (1990:210), and others are convinced that the complex of Carden Bottoms phase sites near Dardanelle, about midway between Little Rock and Fort Smith, is the remains of Tanico. Furthermore, Hoffman (1987, 1990), Jeter (1986), Kidder (1987:22-25), and Brain (1988) have linked the so-called Quapaw phase ceramics of Carden Bottoms and elsewhere with the Tunica or other Tunican speakers.

Thus, the question arises: was there any relationship between the people De Soto met at Tanico in 1541 and the Spiroans whose major occupation about 100 river miles up the Arkansas at Spiro had apparently begun to wind down no more than 90 years earlier? Were the people of Tanico Spiroans, whose forebears had been at Spiro, but then moved down the Arkansas Valley (maybe simply regrouping in their own eastern territory) at about the same time, perhaps not coincidentally, that the ceremonial centers at Moundville, Winterville, and Etowah were lapsing into disuse? If so, and if the speech attributed to the cacique of Tanico is taken at face value, the Tulla with whom he and his forebears "had no converse," and who were lance-using buffalo hunters according to the De Soto narratives (Quinn 1979:135), were most likely Wichita who moved in from the Plains as the Spiroans abandoned the western part of their territory. That could explain the historic references (which have been used to support the theory that the Spiroans were Wichita; Phillips and Brown 1978: 21) to a group called the "Mentos," an Osage name for the Wichita in the Fort Smith area around 1700. The conventional view is that they were Caddoans who had taken up buffalo hunting. But there are enough Caddoan pots in the Carden Bottoms collections (Clancy 1985), all obviously traded from the Red River Valley and the Ouachita Valley, to demonstrate that the people of Tanico were in close contact with the Caddo to the south and would have been able to converse with any Caddoans who might have been in the Fort Smith area in 1541.

Archaeologically speaking, all of this is testable and, since the archaeology of the Arkansas Valley between Fort Smith and Little Rock is, to be conservative, 95% unknown, it may well be possible to link the ostensibly protohistoric Tunican assemblages in the Arkansas Valley with the latest Spiroan assemblages from the Spiro locality. There are obvious differences in the ceramics (which is why this connection wasn't made years ago), but it is also true, as we have seen, that the core types of the Spiro phase, Woodward Plain and Sanders Plain, are really misnamed regional varieties of the basic Mississippian types Mississippi Plain and Old Town Red, both abundant in the Carden Bottoms collections (Hoffman 1986: Table 3.1; Sabo and Early 1988: 118-119). As archaeological evidence goes, that is orders of magnitude more than the evidence for a Wichita connection, and it is certainly too much to ignore.

There is also evidence of another kind that I find suggestive because it is the kind of evidence that is inexplicably lacking, it seems to me, in the case of the Wichita. If they were the heirs to the Spiroan tradition, why is it that no one has been able to point to any evidence of that tradition in their culture? Are we to believe–as cultural anthropologists–that the Wichita walked away from Spiro with nothing? What are cultures, after all, except "remembrances of things past"? But there are, I think, such remembrances in Tunican culture. The core traits of Tunican culture, as elucidated by Brain (1988b:329-331), are precisely the ones that I see exemplified in

Spiroan culture. They are: a proclivity for trade; a proclivity for locating themselves at strategic points on major riverine trade routes; a proclivity for travel to distant groups for the purpose of trading; a proclivity for burying extraordinary amounts of goods, especially trade goods, with their dead (Brain 1979) and, above all, a proclivity for entrepreneurship and for the accumulation of wealth as capital, in the European sense.

That concept was almost universally absent among American Indians who, therefore, almost uniformly failed to profit in their trade with the Europeans. But the Tunica, like the peoples of the Northwest Coast, fared somewhat better because they had some understanding of the rules the Europeans were playing by. And that, I think, bespeaks a long culture history in which trade in commodities and the accumulation of capital were important. I am reminded here of the quotation from John R. Swanton with which Jeffrey P. Brain began his book, *Tunica Archaeology* (1988:1):

> The part played by the Tunican peoples in the aboriginal history of the Lower Mississippi Valley would thus appear to have been very great and to render a knowledge of their position and affinities of unusual importance.

ACKNOWLEDGEMENTS

I thank Hester Davis, Charles Ewen, Marvin Jeter, David Kelley, Mary Lucas Powell, and Jerome Rose for their prompt and helpful comments on this paper.

REFERENCES

Albert, Lois E.
1984 Survey of Archaeological Activity in Oklahoma. In *Prehistory of Oklahoma*, edited by Robert E. Bell, 45-63. Academic Press, New York.
1992 *The Norman Site (34W62), Wagoner County, Oklahoma*. Paper presented at the 33rd Annual Caddo Conference, Stephen F. Austin State University, March 22-24.

Baerreis, David A.
1954 The Huffaker Site, Delaware County, Oklahoma. *Bulletin of the Oklahoma Anthropological Society* 2:35-48.
1957 The Southern Cult and the Spiro Ceremonial Complex. *Bulletin of the Oklahoma Anthropological Society* 5:23-28.

Baerreis, David A., and Reid A. Bryson
1965 Historical Climatology and the Southern Plains: A Preliminary Statement. *Bulletin of the Oklahoma Anthropological Society* 13:69-75.

Barèis, Charles John
1955 The Brackett Site, Ck-43 of Cherokee County, Oklahoma. *Bulletin of the Oklahoma Anthropological Society* 3:1-53.

Barnes, James E., and Jerome C. Rose
1990 *Dental Morphological Variants and Kinship Among Prehistoric Caddo*. Paper presented at the 55th Annual Meeting of the Society for American Archaeology, Las Vegas.

Bell, Robert E.
1953 The Scott Site, LeFlore County, Oklahoma. *American Antiquity* 18(4):314-331.
1972 *The Harlan Site, Ck-6. A Prehistoric Mound Center in Cherokee County, Eastern Oklahoma*. Oklahoma Anthropological Society Memoir 2.
1974 Mounds and Fieldwork Near Muskogee, Oklahoma. *Oklahoma Anthropological Society Newsletter* 22(8):6-9.
1980 *Oklahoma Indian Artifacts*. Contributions from the Stovall Museum, No. 4. University of Oklahoma.
1984a Arkansas Valley Caddoan: The Harlan Phase. *In Prehistory of Oklahoma*, edited by Robert E. Bell, 221-240. Academic Press, New York.
1984b The Plains Villagers: The Washita River. In *Prehistory of Oklahoma*, edited by Robert E. Bell, 307-324. Academic Press, New York.
1984c Protohistoric Wichita. In *Prehistory of Oklahoma*, edited by Robert E. Bell, 363-378. Academic Press, New York.

Bell, Robert E., and David A. Baerreis
1951 A Survey of Oklahoma Archaeology. *Bulletin of the Texas Archeological and Paleontological Society* 22:7-100.

Bell, Robert E., and C. Dale
1953 The Morris Site, Ck-49, Cherokee County, Oklahoma. *Bulletin of the Texas Archeological Society* 24:69-140.

Bond, Clell L.
1977 Spinach Patch Site and the River Bank Site. In *Ozark Reservoir Papers*, edited by Michael P. Hoffman, 81-137. Arkansas Archeological Survey Research Series, No. 10.

Brain, Jeffrey P.
1979 *Tunica Treasure*. Papers of the Peabody Museum of Archaeology and Ethnology 71, Harvard University, Cambridge.

1988 *Tunica Archaeology*. Papers of the Peabody Museum of Archaeology and Ethnology 78. Harvard University, Cambridge.

Briscoe, James

1977 *The Plantation Site (Mi63), An Early Caddoan Settlement in Eastern Oklahoma*. Papers in Highway Archeology No. 3. Oklahoma Highway Archeological Survey, Oklahoma City.

Brown, Ian W.

1985 Plaquemine Architectural Patterns in the Natchez Bluff and Surrounding Region of the Lower Mississippi Valley. *Midcontinental Journal of Archaeology* 10(2):231-305.

Brown, James A.

1966 *Spiro Studies, Volume 1. Description of the Mound Group*. Stovall Museum of Science and History, University of Oklahoma Research Institute, Norman.

1966 *Spiro Studies, Volume 2. The Graves and Their Contexts*. Stovall Museum of Science and History, University of Oklahoma Research Institute, Norman.

1971 *Spiro Studies, Volume 3. Pottery Vessels*. Stovall Museum of Science and History, University of Oklahoma Research Institute, Norman.

1975 Spiro Art and Its Mortuary Contexts. In *Death and the Afterlife in Pre-Columbian America*, edited by Elizabeth P. Benson, 1-32. Dumbarton Oaks Research Library and Collections, Washington, D.C.

1983 Spiro Exchange Connection Revealed by Sources of Imported Raw Materials. In *Southeastern Natives and Their Pasts: A Collection of Papers Honoring Dr. Robert E. Bell*, edited by Don G. Wyckoff and Jack L. Hofman, 129-162. Oklahoma Archeological Survey, Studies in Oklahoma's Past No. 11.

1984a Arkansas Valley Caddoan: The Spiro Phase. In *Prehistory of Oklahoma*, edited by Robert E. Bell, 241-263. Academic Press, New York.

1984b *Prehistoric Southern Ozark Marginality: A Myth Exposed*. Missouri Archaeological Society, Special Publication No. 6. Columbia.

Brown, James A., Robert E. Bell, and Don G. Wyckoff

1978 Caddoan Settlement Patterns in the Arkansas River Drainage. In *Mississippian Settlement Patterns*, edited by Bruce D. Smith, 169-200. Academic Press, New York.

Brown, James A., Richard A. Kerber, and Howard D. Winters

1990 Trade and the Evolution of Exchange Relations at the Beginning of the Mississippian Period. In *The Mississippian Emergence*, edited by Bruce D. Smith, 251-280. Smithsonian Institution Press, Washington, D.C.

Brues, Alice M.

1957 Skeletal Material from the Nagle Site. *Bulletin of the Oklahoma Anthropological Society* 5:93-99.

1958 Skeletal Material from the Horton Site. *Bulletin of the Oklahoma Anthropological Society* 6:27-32.

1959 Skeletal Material from the Morris Site, Cd-39. *Bulletin of the Oklahoma Anthropological Society* 7:63-70.

Burnett, Barbara A.

1988 The Biological Synthesis. In *Human Adaptation in the Ozark and Ouachita Mountains*, edited by George Sabo III, Ann M. Early, Jerome C. Rose, Barbara A. Burnett, Louis Vogel, Jr., and James P. Harcourt, 193-220. Arkansas Archeological Survey Research Series No. 31. Fayetteville.

1988 Standridge Bioarcheology and the Middle Ouachita Caddo. In *Standridge: Caddoan Settlement in a Mountain Environment*, edited by Ann M. Early, with Barbara A. Burnett, and Daniel Wolfman. Arkansas Archeological Survey Research Series, No. 29. Fayetteville.

1990 The Bioarcheological Synthesis of the Gulf Coastal Plain Study Area. In *The Archeology and Bioarcheology of the Gulf Coastal Plain*, Vol. 2, 385-508. Arkansas Archeological Survey Research Series No. 38. Fayetteville.

Burnett, Barbara A., and Katherine Murray

1990 The Bioarcheology of Depopulation: The Highways of Devolution. Paper presented at Second Symposium on De Soto in Arkansas, sponsored by The University of Arkansas Museum, Fayetteville.

Cartledge, Thomas R.

1970 *The Tyler-Rose Site and Late Prehistory in East-Central Oklahoma*. Oklahoma River Basin Survey, Archeological Site Report 19. Norman.

Chapman, Carl H.

1980 *The Archaeology of Missouri, II*. University of Missouri Press, Columbia.

Clancy, Phyllis Marie

1985 The Carden's Bottom Puzzle Elucidated. M.A. thesis, Department of Anthropology, University of Arkansas, Fayetteville.

Creel, Darrell

1991 Bison Hides in Late Prehistoric Exchange in the Southern Plains. *American Antiquity* 56(1):40-49.

Davis, Hester A., Don G. Wyckoff, and Mary A. Holmes

1971 *Proceedings of the Eighth Caddo Conference*. Occasional Publication No 2, Oklahoma Archeological Society. University of Oklahoma, Norman.

Dickinson, Samuel D.

1980 Historic Tribes of the Ouachita Drainage System in Arkansas. *The Arkansas Archeologist* 21:1-11.

Dillahay, Tom

1974 Late Quaternary Bison Population Changes on the South Plains. *Plains Anthropologist* 19(65):180-196.

Dow, Laura Ann Brantly
1987 The Genetic Affinitives and Adaptive Success of Three Groups of Late Prehistoric Amerindians from Texas. M.A. thesis, University of Texas, Austin.

Duffield, Lathel F.
1969 The Vertebrate Faunal Remains from the School Land 1 and School Land II Sites, Delaware County, Oklahoma. *Oklahoma Anthropological Society Bulletin* 18:47-65.

Early, Ann M.
1988 *Standridge: Caddoan Settlement in a Mountain Environment*. Arkansas Archeological Survey Research Series No. 29. Fayetteville.

Eisenberg, Leslie
1991 Mississippian Cultural Terminations in Middle Tennessee: What the Bioarchaeological Evidence Can Tell Us. In *What Mean These Bones: Studies in Southeastern Bioarchaeology*, edited by Mary Lucas Powell, Patricia S. Bridges, and Ann Marie Wagner Mires, 70-88. University of Alabama Press, Tuscaloosa.

Finklestein, Joe J.
1940 The Norman Site Excavations Near Wagoner, Oklahoma. *The Oklahoma Prehistorian* 3(3): 12-15.

Freeman, Joan E., and A. Dewey Buck, Jr.
1959 Woodward Plain and Neosho Punctate, Two Shell Tempered Pottery Types of Northeastern Oklahoma. *Bulletin of the Oklahoma Anthropological Society* 8:3-16.

Fritz, Gayle J.
1982 Analysis of Plant Remains from the Spiro Site. In *Spiro Archeology: 1980 Research*, edited by J. Daniel Rogers, 201-214. Oklahoma Archeological Survey, Studies in Oklahoma's Past No. 9. Norman.
1989 Evidence of Plant Use from Copple Mound at the Spiro Site. In *Contributions to Spiro Archaeology: Mound Excavations and Regional Perspectives*, edited by J. Daniel Rogers, Don G. Wyckoff, and Dennis A. Peterson, 65-87. Oklahoma Archeological Survey, Studies in Oklahoma's Past No. 16. Norman.
1990 Agricultural Patterning in the Northern Caddoan Archaeological Region. Paper presented to the 55th Meeting of the Society for American Archaeology, Las Vegas.

Galm, Jerry M.
1984 Caddoan Formative: Wister and Fourche Maline Phases. In *Prehistory of Oklahoma*, edited by Robert E. Bell, 199-219. Academic Press, New York.

Gramly, Richard Michael
1977 Deerskins and Hunting Territories: Competition for a Scarce Resource of the Northeastern Woodlands. *American Antiquity* 42(4):601-605.

Griffin, James B.
1952 An Interpretation of the Place of Spiro in Southeastern Archaeology. *Missouri Archaeologist* 14:89-106.
1961 Relationships Between the Caddoan Area and the Mississippi Valley. *Bulletin of the Texas Archeological Society* 31:27-43.
1967 Eastern North American Archaeology. *Science* 156:175-191.

Hackett, C.J.
1963 On the Origin of the Human Treponematoses. *Bulletin WHO* 29:7-41.

Harmon, Anna M., and Jerome C. Rose
1989 Bioarcheology of the Louisiana and Arkansas Study Area. In *Archeology and Bioarcheology of the Lower Mississippi South in Arkansas and Louisiana*, by Marvin D. Jeter, Jerome C. Rose, Ishmael Williams, Jr., and Anna M. Harmon, 323-354. Arkansas Archeological Survey Research Series No. 37. Fayetteville.

Harrington, M.R.
1920 *Certain Caddo Sites in Arkansas*. Miscellaneous Series No. 10. Museum of the American Indian, Heye Foundation, New York.

Hemmings, E. Thomas, and John H. House
1985 *The Alexander Site, Conway County, Arkansas*. Arkansas Archeological Survey Research Series No. 24. Fayetteville.

Hoffman, Michael P.
1977 The Kinkead-Mainard Site, 3PU2: A Late Prehistoric Quapaw Phase Site Near Little Rock, Arkansas. *Arkansas Archeologist* 16-18:1-41.
1986 The Protohistoric Period in the Lower and Central Arkansas River Valley in Arkansas. In *The Protohistoric Period in the Mid-South: 1500-1700, Proceedings of the 1983 Mid-South Archeological Conference*, edited by David H. Dye and Ronald C. Brister. Mississippi Department of Archives and History, Archaeological Report 18:24-37.
1987 An Examination of the Evidence for the Tunica People on the Arkansas River. Paper presented at the Annual Meeting of the Mid-South Folklore Association, Clarksville, Arkansas.
1990 The Terminal Mississippian Period in the Arkansas River Valley and Quapaw Ethnogenesis. In *Towns and Temples Along the Mississippi*, edited by David H. Dye and Cheryl Anne Cox, 208-226. University of Alabama Press, Tuscaloosa.

House, John H.
1985 The Alexander Site in Arkansas Valley Research. In *The Alexander Site, Conway County, Arkansas*, edited by Thomas E. Hemmings and John H. House, 99-110. Arkansas Archeological Survey Research Series No. 24. Fayetteville.

Hudson, Charles
1976 *The Southeastern Indians*. The University of Tennessee Press, Knoxville.

1985 De Soto in Arkansas: A Brief Synopsis. *Field Notes* 205:3-12. Arkansas Archeological Society, Fayetteville.

Hudson, Charles, Marvin T. Smith, and Chester B. De Pratter

1990 The Hernando De Soto Expedition: From Mobile to the Mississippi River. In *Towns and Temples Along the Mississippi*, edited by David H. Dye and Cheryl Ann Cox, 181-207. University of Alabama Press, Tuscaloosa.

Jeter, Marvin D.

1986 Tunicans West of the Mississippi: A Summary of Early Historic and Archaeological Evidence. In *The Protohistoric Period in the Mid-South: 1500-1700. Proceedings of the 1983 Mid-South Archaeological Conference*, edited by David H. Dye and Ronald C. Brister, 38-65. Mississippi Department of Archives and History Archaeological Report 18. Jackson.

1990 Review of *Tunica Archaeology* by Jeffrey P. Brain. In *Southeastern Archaeology* 9(2):147-149.

Kay, Marvin, George Sabo III, and Ralph Merletti

1989 Late Prehistoric Settlement Patterning: A View from Three Caddoan Civic-Ceremonial Centers in Northwest Arkansas. In *Contributions to Spiro Archeology: Mound Excavations and Regional Perspectives*, edited by Daniel Rogers, Don G. Wyckoff, and Dennis H. Peterson, 129-157. Oklahoma Archeological Survey, Studies in Oklahoma's Past No. 16.

Kidder, Tristram R.

1988 The Koroa Indians of the Lower Mississippi Valley. *Mississippi Archaeology* 23(2):1-42.

Krieger, Alex D.

1946 *Culture Complexes and Chronology in Northern Texas, with Extensions of Puebloan Datings to the Mississippi Valley*. Publication 4640. The University of Texas, Austin.

1961 Reply to Griffin. In *Symposium: Relationships Between the Caddoan Area and Neighboring Areas*, edited by E. Mott Davis. Bulletin of the Texas Archeological Society 31:43-51.

Larson, Lewis H. Jr.

1989 The Etowah Site. In *Southeastern Ceremonial Complex*, edited by Patricia Galloway, 133-141. University of Nebraska Press, Lincoln.

Levy, Dawn

1992 Tales from the Burial Mound. *Science* 257: 1210.

Lopez, David R.

1973 The Wybark Site: A Late Prehistoric Village Complex in the Three Forks Locale, Muskogee, Oklahoma. *Oklahoma Anthropological Society Bulletin* 22:11-126.

McGimsey, Charles R. III

1959 *An Archeological Survey of the Greer's Ferry Reservoir in North Central Arkansas*. Ms. on file, Arkansas Archeological Survey, Fayetteville.

Moore, C.B.

1912 Some Aboriginal Sites on Red River. *Journal of the Academy of Natural Sciences of Philadelphia* 14:482-644. Philadelphia.

Morse, Dan F.

1990 The Nodena Phase. In *Towns and Temples Along the Mississippi*, edited by David H. Dye and Cheryl Anne Cox, 69-97. University of Alabama Press, Tuscaloosa.

Morse, Dan F., and Phyllis A. Morse

1983 *Archaeology of the Central Mississippi Valley*. Academic Press, New York.

1987 *A Preliminary Report of the Zebree Project: New Approaches to Contract Archeology in Arkansas*. Arkansas Archeological Survey Research Report No. 8. Fayetteville.

1990 Emergent Mississippian in the Central Mississippi Valley. In *The Mississippian Emergence*, edited by Bruce D. Smith, 153-173. Smithsonian Institution Press, Washington, D.C.

Morse, Phyllis, and Dan F. Morse

1990 The Zebree Site: An Emerged Early Mississippian Expression in Northeast Arkansas. In *The Mississippian Emergence*, edited by Bruce D. Smith, 51-66. Smithsonian Institution Press, Washington, D.C.

Newell, H. Perry, and Alex D. Krieger

1949 *The George C. Davis Site, Cherokee County, Texas*. Society for American Archaeology Memoir 5.

Newkumet, Phil J.

1940 Preliminary Report on Excavations of the Williams Mound, LeFlore County, Oklahoma. *The Oklahoma Prehistorian* 3(2):2-6.

Orr, Kenneth Gordon

1946 The Archaeological Situation at Spiro, Oklahoma: A Preliminary Report. *American Antiquity* 4:228-256.

1952 Survey of Caddoan Area Archeology. In *Archeology of Eastern United States*, edited by James B. Griffin, 239-255. University of Chicago Press, Chicago.

Owsley, Douglas W.

1989 The History of Bioarcheological Research in the Southern Great Plains. In *From Clovis to Comanchero: Archeological Overview of the Southern Great Plains*, 123-136. Arkansas Archeological Survey Research Series No. 35, Fayetteville.

1989 A Systematic Approach to the Skeletal Biology of the Southern Plains. In *From Clovis to Comanchero: Archeological Overview of the Southern Great Plains*, 137-156. Arkansas Archeological Survey Research Series No. 35, Fayetteville.

Perttula, Timothy
1989 The Late Prehistoric Period in the Western Ozarks of Southwest Missouri. In *Contributions to Spiro Archeology: Mound Excavations and Regional Perspectives*, edited by J. David Rogers, Dan C. Wyckoff, and Dennis A. Peterson, 117-127. Oklahoma Archeological Survey, Studies in Oklahoma's Past No. 16.

Peterson, Dennis A.
1989 Interpreting the Copple Mound Findings. In *Contributions to Spiro Archeology: Mound Excavations and Regional Perspectives*, edited by J. Daniel Rogers, Don G. Wyckoff, and Dennis A. Peterson, 27-41. Oklahoma Archeological Survey, Studies in Oklahoma's Past No. 16.

Peterson, Dennis A., Michael Moore, Suzanne Svec, and J. Daniel Rogers.
1982 APPENDIX A: Artifact Descriptions. In *Spiro Archeology: 1980 Research*, edited by J. Daniel Rogers, 119-160. Oklahoma Archeological Survey, Studies in Oklahoma's Past No. 9.

Phillips, Philip
1958 Application of the Wheat-Gifford-Wasley Taxonomy to Eastern Ceramics. *American Antiquity* 24:117-125.
1970 *Archaeological Survey in the Lower Yazoo Basin, Mississippi, 1949-1955*. Papers of the Peabody Museum of Archaeology and Ethnology 60. Harvard University, Cambridge.

Phillips, Philip, and James A. Brown
1978 *Pre-Colombian Shell Engravings from the Craig Mound at Spiro, Oklahoma*. Paperback Edition, Part I. Peabody Museum of Archaeology and Ethnology, Harvard University, Cambridge.

Powell, Mary Lucas, and J. Daniel Rogers
1980 *Bioarcheology of the McCutchan-McLaughlin Site*. Oklahoma Archeological Survey, Studies In Oklahoma's Past No. 5.

Proctor, Charles
1957 The Sam Site, Lf-28, of LeFlore County, Oklahoma. *Oklahoma Anthropological Society Bulletin* 5:45-91.

Purrington, Burton Lewis
1971 The Prehistory of Delaware County, Oklahoma: Cultural Continuity and Change on the Western Ozark Periphery. Ph.D. dissertation, University of Wisconsin. University Microfilms, Inc. Ann Arbor.

Quinn, David B.
1979 The Expedition of Hernando de Soto and His Successor, Luis de Moscoso, 1539-1543. In *New American World: A Documentary History of North America to 1612*, Vol. 2, 90-188. Arno Press, New York.

Rogers, J. Daniel
1982 (editor) *Spiro Archeology: 1980 Research*. Oklahoma Archeological Survey, Studies in Oklahoma's Past No. 9.
1989 Settlement Contexts for Shifting Authority in the Arkansas Basin. In *Contributions to Spiro Archeology: Mound Excavations and Regional Perspectives*, edited by J. Daniel Rogers, Don G. Wyckoff and Dennis A. Peterson, 159-176. Oklahoma Archeological Survey, Studies in Oklahoma's Past No. 16.
1991a Patterns of Change on the Western Margin of the Southeast, A.D. 600-900. In *Stability, Transformation, and Variation: The Late Woodland Southeast*, edited by Michael S. Nassaney and Charles R. Cobb, 221-248. Plenum Press, New York.
1991b Regional Prehistory and the Spiro Phase. *Southeastern Archaeology* 10(1):63-68.

Rohrbaugh, Charles L.
1982 Spiro and Fort Coffee Phases: Changing Cultural Complexes of the Caddoan Area. Ph.D. dissertation, Department of Anthropology, University of Wisconsin, Madison.
1984 Arkansas Valley Caddoan: Fort Coffee and Neosho Foci. In *Prehistory of Oklahoma*, edited by Robert E. Bell, 265-285. Academic Press, New York.

Rolingson, Martha Ann
1990 The Toltec Mounds Site: A Ceremonial Center in the Arkansas River Lowland. In *The Mississippian Emergence*, edited by Bruce D. Smith, 27-49. Smithsonian Institution Press, Washington, D.C.

Rose, Jerome C., Barbara A. Burnett, Mark W. Blaeuer, and Michael S. Nassaney.
1984 Paleopathology and the Origins of Maize Agriculture in the Lower Mississippi Valley and Caddoan Culture Areas. In *Paleopathology and the Origins of Agriculture*, edited by Mark N. Cohen and George J. Armelagos, 393-424. Academic Press, New York.

Rose, Jerome C., Murray K. Marks, and Larry L. Tieszen
1990 Bioarchaeology and Subsistence in the Central and Lower Portions of the Mississippi Valley. In *What Mean These Bones: Studies in Southeastern Biorchaeology*, edited by Mary Lucas Powell, Patricia S. Bridges, and Ann Marie Wagner Mires, 7-21. University of Alabama Press, Tuscaloosa.

Sabo, George III, and Ann M. Early
1988 Prehistoric Culture History. In *Human Adaptation in the Ozark and Ouachita Mountains*, edited by George Sabo III, Ann M. Early, Jerome C. Rose, Barbara A. Burnett, Louis Vogel, Jr., and James P. Harcourt. Arkansas Archeological Survey Research Series No. 31. Fayetteville.

Sabo, George III, Randall L. Guendling, W. Fredrick Limp, Margaret J. Guccione, Susan L. Scott, Gayle J. Fritz, and Pamela Smith
1990 *Archaeological Investigations at 3MR80 - Area D in the Rush Development Area, Buffalo Na-*

tional River, Arkansas. Vol. 1. Southwest Cultural Resources Center, Professional Papers, No. 38.

Schambach, Frank F.

1970 Pre-Caddoan Cultures in the Trans-Mississippi South: A Beginning Sequence. Ph.D. dissertation, Department of Anthropology, Harvard University, Cambridge.

1982a An Outline of Fourche Maline Culture in Southwest Arkansas. In *Arkansas Archeology in Review*, edited by Neal J. Trubowitz and Marvin D. Jeter, 132-197. Arkansas Archeological Survey Research Series, No. 15. Fayetteville.

1982b The Archeology of the Great Bend Region in Arkansas. In *Contributions to the Archeology of the Great Bend Region*, edited by Frank F. Schambach and Frank Rackerby, 1-11. Arkansas Archeological Survey Research Series, No. 22. Fayetteville.

1988 The Archaeology of Oklahoma. In *Quarterly Review of Archaeology* 9(4):5-9.

1990a The Bangs Slough Project. In *Coles Creek and Mississippi Period Foragers in the Felsenthal Region of the Lower Mississippi Valley*, edited by Frank F. Schambach, 1-18. Arkansas Archeological Survey Research Series No. 39. Fayetteville.

1990b The Place of Spiro in Southeastern Prehistory: Is It Caddoan or Mississippian? *Southeastern Archaeology* 9(1):67-69.

1990c The Place of the Spiro Site in Southeastern Prehistory: A Reinterpretation. Paper presented at the 48th Annual Southeastern Archaeological Conference, Mobile, Alabama.

1990d The "Northern Caddoan Area" Was Not Caddoan. *Caddoan Archeology* 1(4):1-6.

Schambach, Frank F., and John E. Miller III

1984 A Description and Analysis of the Ceramics. In *Cedar Grove: An Interdisciplinary Investigation of a Late Caddo Farmstead in the Red River Valley*, edited by Neal L. Trubowitz, 109-170. Arkansas Archeological Survey Research Series No. 23. Fayettville.

Schambach, Frank F., and Leslie Newell

1990 *Crossroads of the Past: 12,000 Years of Indian Life in Arkansas*. Arkansas Archeological Survey Popular Series No. 2. Fayetteville.

Scholtz, Sandra

1975 *Prehistoric Plies: A Structural and Comparative Analysis of Cordage, Netting, Basketry and Fabric from Ozark Bluff Shelters*. Arkansas Archeological Survey Research Series No. 9. Fayetteville.

Shaeffer, James B.

1950 The Horton Site, A Fultonoid Village Near Vian, Oklahoma. *Bulletin of the Oklahoma Anthropological Society* 6:1-26.

1957 The Nagle Site, Ok-4. *Bulletin of the Oklahoma Anthropological Society* 5:93-99.

Sharrock, Floyd W.

1960 The Wann Site, Lf-27, of the Fourche Maline Focus. *Bulletin of the Oklahoma Anthropological Society* 3:17-47.

Speth, John D., and Susan L. Scott

1989 Horticulture and Large-Mammal Hunting: The Role of Resource Depletion and the Constraints of Time and Labor. In *Farmers as Hunters: The Implications of Sedentism*, edited by Susan Kent, 71-79. Cambridge University Press, Cambridge.

Story, Dee Ann

1978 Some Comments on Anthropological Studies Concerning the Caddo. In *Texas Archeology: Essays Honoring R. King Harris*, edited by Kurt D. House, 46-68. Institute for the Study of Earth and Man, Reports of Investigations 3. Southern Methodist University Press, Dallas.

Suhm, Dee Ann, and Edward B. Jelks

1962 *Handbook of Texas Archeology: Type Descriptions*. Texas Archeological Society, Special Publications No. 1, and Texas Memorial Museum, Bulletin No. 4, Austin.

Swenson, Fern E.

1989 Pottery Recovered During the 1982 Excavations. In *Contributions to Spiro Archeology: Mound Excavations and Regional Perspectives*, edited by J. Daniel Rogers, Don C. Wyckoff, and Dennis A. Peterson, 187-202. Oklahoma Archeological Survey, Studies in Oklahoma's Past No. 16.

Vehik, Susan C.

1984 The Woodland Occupation. In *Prehistory of Oklahoma*, edited by Robert E. Bell, 175-197. Academic Press, New York.

Voegelin C.F., and F.M. Voegelin

1966 *Map of North American Indian Language*. American Ethnological Society and Rand McNally and Company.

Webb, Clarence H.

1959 *The Belcher Mound: A Stratified Caddoan Site in Caddo Parish, Louisiana*. Society for American Archaeology Memoir 16.

Wedel, Waldo R.

1961 *Prehistoric Man on the Great Plains*. University of Oklahoma Press, Norman.

Wood, W. Raymond

1968 Mississippian Hunting and Butchering Patterns: Bone from the Vista Shelter, 23SR-20, Missouri. *American Antiquity* 33(2):170-179.

Wyckoff, Don G.

1967 *Archaeology in the Webber's Falls Lock and Dam Area: A Brief Summary*. Archeological Site Report No. 7. Oklahoma River Basin Survey. Norman.

1970 *The Horton Site Revisited, 1967 Excavations at Sq-11, Sequoyah County, Oklahoma*. Studies in Oklahoma's Past No. 1. Oklahoma Archeological Survey, Norman.

1971 *The Caddoan Cultural Area: An Archeological Perspective*, Oklahoma Archeological Survey, University of Oklahoma, Norman.

1974 *The Caddoan Area: An Archaeological Perspective*. Garland Press, New York.

1980 Caddoan Adoptive Strategies in the Arkansas Basin, Eastern Oklahoma. Ph.D. dissertation, Washington State University, Pullman.

1989 A Synopsis of the 1982 Field Work and Findings. In *Contributions to Spiro Archeology: Mound Excavations and Regional Perspectives*, edited by J. Daniel Rogers, Don G. Wyckoff, and Dennis A. Peterson, 101-104. Oklahoma Archeological Survey, Studies in Oklahoma's Past No. 16.

Wyckoff, Don G. and Robert L. Brooks

1983 *Oklahoma Archeology: A 1981 Perspective of the State's Archeological Resources, Their Significance, Their Problems and Some Proposed Solutions*. Archeological Resources Survey Report No. 16, Oklahoma Archeological Survey, University of Oklahoma, Norman.

14 The Glendora Phase: Protohistoric–Early Historic Culture Dynamics on the Lower Ouachita River

Tristram R. Kidder
Tulane Univeristy
New Orleans, Louisiana

INTRODUCTION

Although the demise of Native American populations is often linked to the appearance of European-introduced epidemic diseases and warfare, such blanket models of Indian-European interaction obscures the complexity of behavior during this particularly dynamic interval. Native Americans were neither passive recipients of European actions, nor were they wholly in control of their own destiny. In the Lower Mississippi Valley and its adjacent tributaries the patterns of interaction among Indians, and between Indians and Europeans, varied greatly. This paper examines one subset of the Native American–European interaction as it occurred in the southern Ouachita River Valley (Figure 14.1). I contend that the fortunes of the Indians in this area were affected not only by the presence of the Europeans, but also by the evidently conscious decisions of the natives themselves. These people chose to follow a specific set of economic and political strategies, which, in the short term, appear to have been highly beneficial. However, due to transformations in the wider sphere of Indian-European interaction in the Lower Mississippi Valley and the Southeast in general, these decisions ultimately proved to be detrimental. The consequence was the extinction of Native American groups in the region by the early-to-middle eighteenth century.

The southern Ouachita River valley was not colonized by Europeans until relatively late in the eighteenth century, and as a result there is almost no extant ethnohistorical detail concerning the native populations of the region. Scarce details can be gleaned from documentary and cartographic sources, which have as a focus the events and peoples of the core area of the Mississippi Valley proper. As a result, the majority of what can be understood about this region must be gathered from archaeology. It is necessary, however, to examine critically the archaeological evidence in order to make sense of the chronology and behavior of Native American populations living in the area just prior to, and during, the contact period. It is only in the context of a well grounded understanding of the archaeology that we will be able to provide an explanation of the behaviors and responses of Native American populations in the face of contact.

HISTORY OF RESEARCH

In 1909 Clarence B. Moore excavated a number of sites on Bayou Bartholomew and the Ouachita River in Louisiana. Moore published his findings in a volume *Antiquities of the Ouachita River* (Moore 1909). Both the illustrated pottery and Moore's own text indicate that these cemeteries yielded a rich array of ceramics that archaeologists today can date to the late prehistoric through early historic periods.

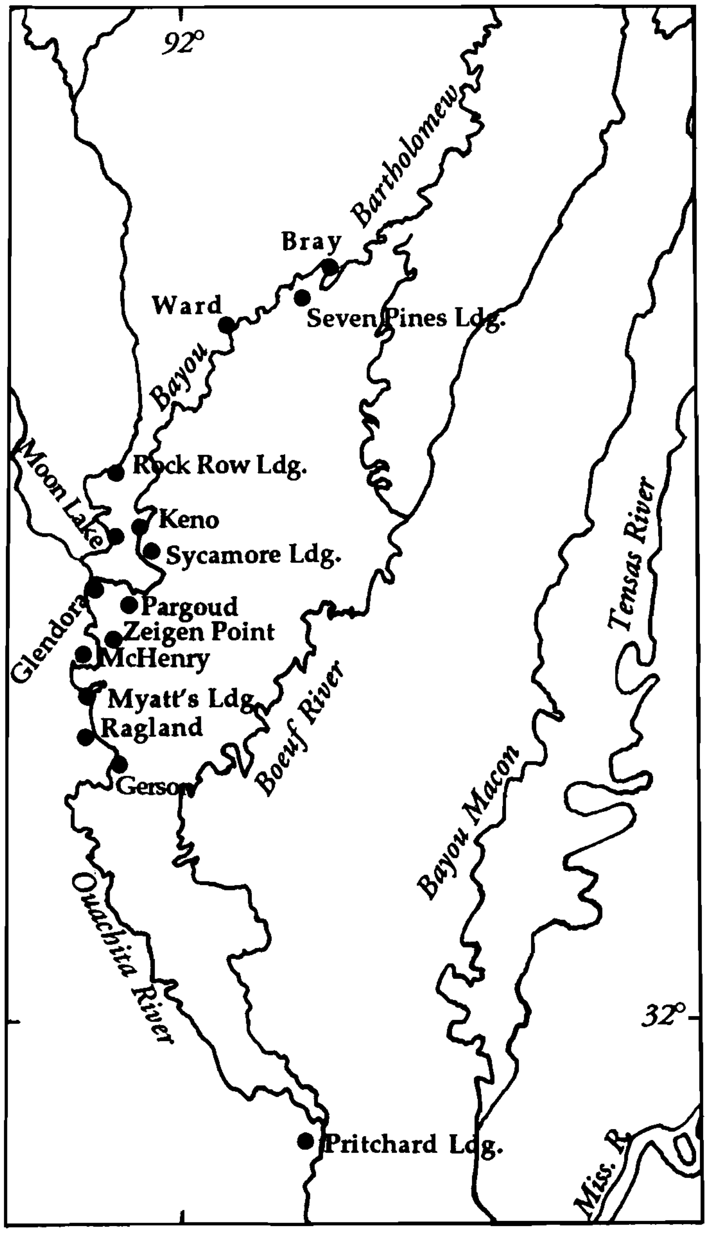


Figure 14.1. Southern Ouachita Valley, Arkansas and Louisiana, showing Glendora phase sites mentioned in text.

Moore's work has since been used for the culture-historical and ethnic identification of the lower Ouachita River as the eastern margin of the Caddoan culture area (Haag 1971; Suhm *et al.* 1954; Webb and Gregory 1978:23, 27; Williams 1964). Several of these cemeteries, notably the Keno and Glendora sites, have been grouped together in the Glendora phase (Williams 1964). Today, however, this view is being revised and modified (Belmont 1985; Gibson 1985a:328-331; Kidder 1990a, 1990b; Webb and Gregory 1978:29). Based on recent research on protohistoric and early historic archaeological assemblages and a detailed reanalysis of many of Moore's vessels that were not published, a considerably more complex picture of the protohistoric and early historic Glendora phase is emerging.

The Glendora phase occupies a significant place in the archaeology of the western portion of the Lower Mississippi Valley. Although not an ethnically "Caddoan" culture, these peoples were in regular contact with groups from the west. Also, the Glendora phase peoples occupied a strategic location on the Ouachita River and were able to influence trade patterns between and among a number of Native American groups throughout the Lower Mississippi Valley and adjacent regions. Given its location and interactions, the Glendora phase must be considered in any discussion of the events of the protohistoric and early historic periods in the region. Understanding the outline of Glendora phase chronology, settlement, and culture history is thus an important element in unraveling the complex events of the protohistoric period in the Southeast.

Using externally defined phases and archaeological complexes, I can identify a rough two-part chronology for the Glendora phase (see also Hally 1972:455-461). The Glendora I subphase is a protohistoric manifestation coeval with the Jordan II subphase (Kidder 1988, n.d.), while Glendora II is an early historic complex. Judging from the limited data provided by Moore and from the equally scanty ethnohistorical record, the Glendora phase cannot have lasted any later than 1720, although it is likely to have collapsed earlier. I will take up the problems of the chronology of the Glendora phase below.

HISTORY OF THE GLENDORA PHASE CONCEPT

Winslow Walker was the first archaeologist to recognize the connections between the Ouachita and the Red River Valley (Walker 1935:12-15), but it was not until 1936, when James Ford formally defined and discussed the Caddoan ceramic complex, that the relationships were made explicit (Ford 1936:72-97). Specifically, Ford noted the presence of complex curvilinear engraving associated with plain, crosshatched, and "ticked"-line motifs (now recognized as Natchitoches Engraved) and observed that the features of his Caddoan ceramic types were derived from mortuary contexts. Ford was the first to identify Keno and Glendora as Caddoan sites (Ford 1936: Figure 2), and he included the Ouachita River in the Caddoan culture area (Ford 1936: Figure 3). In 1941 Kenneth Orr first applied the label Glendora Focus to archaeologically identified historic Caddoan groups (Orr 1941:14). In 1943 Alex Krieger noted that the Glendora focus encompassed "all known historic Caddoan locations" (1943:154). In an article published in 1952, but written four years earlier, Orr noted that the Glendora Focus represented a series of "widely scattered sites" in southeastern Arkansas and north Louisiana (Orr 1952: 251). He suggested that the Glendora focus may have been the remains of the Natchitoches or the Kadohadacho (Orr 1952).

In 1954 Suhm, Krieger, and Jelks published the first formal definition of the Glendora focus. They gave Glendora a broad spatial distribution, extending it north to the Greer and Douglas sites on the Arkansas River and east to the Ouachita River. The central focus of the definition was the presence of Natchitoches Engraved and historic trade goods. Secondary traits were also listed, but were not consistent across all the components (Suhm *et al.* 1954:223-225). The cultural position of the Glendora phase and the eastern margin of the Caddoan culture area was reaffirmed in a series of papers published in 1961 (Davis 1961a, 1961b, 1961c).

In a paper submitted to the Fifth Caddo Conference and published in its proceedings, Stephen Williams suggested some important revisions to the Glendora focus (Williams 1961). The term phase was introduced instead of focus, and the Glendora phase was restricted to Keno, Glendora, Greer, and Douglas, primarily based on the illustrations in Moore [1908, 1909] (Williams 1961:124-125). Williams identified the Glendora phase with the Cahinnio or Ouachita Caddo, and dated it to 1600-1750.

In an article published in 1964 but based largely on data collected a number of years earlier, Williams again called for a redefinition of the historic phases in the Caddoan culture area and withdrew the Greer and Douglas sites from the Caddoan area because they were clearly outside of the distribution of the established Glendora phase (Williams 1964:564, Figure 3). The Glendora phase was restricted to the Keno and Glendora sites and identified as the archaeological remains of the Ouachita Indians (Williams 1964:564). It is apparent that Williams' definition was generally accepted, and the Glendora phase has mostly been used in this restricted sense. In no case did any of the aforementioned authors include the Ward, Seven Pines Landing, Sycamore Landing, or Bray sites in their discussion of the culture history of the Ouachita River, despite a number of illustrations in Moore (1909). Further, no author attempted to explain the obviously non-Caddoan vessels at Glendora and Keno, except to note that Natchez Incised was no doubt intrusive (Suhm *et al.* 1954; Williams 1964).

While the debate over Caddoan culture terminology has abated since the early 1960s, the assumption that the Ouachita was the eastern boundary of the Caddoan area has not been put to rest. Major syntheses of Louisiana prehistory have excluded the Ouachita River from the Mississippi Valley region (Haag 1971:34; Phillips 1970:861). It is evident, however, that this assumption was based on little recent archaeological data, and may well have stemmed from the identification of the Glendora phase as a Caddoan manifestation (Price and Heartfield 1977:98). In 1973 Gregory identified the Glendora phase as probably being the remains of the Ouachita Indians (Gregory 1973: Figure 2, Table 2). However, in 1978 Webb and Gregory revised their position and suggested that it was possible that these were Koroa sites with Caddoan influences (Webb and Gregory 1978:29). However, these authors maintained that the Keno and Glendora sites, and thus the Ouachita River, were in the Caddoan culture area (see their maps in Webb and Gregory 1978:23, 27). They also suggested that regardless of the ethnic affiliation of the natives of the Glendora phase sites, the phase was closely related to the Red River cultures around Natchitoches (Webb and Gregory 1978:29). As one proof that the Glendora phase was closely related to Caddoan groups on the Red River, Webb and Gregory state that horse burials were found at both the Glendora site and at Fish Hatchery (Gregory 1973:191, 223-225, tables 10-11; Webb and Gregory 1978:29). While the Fish Hatchery horse burial is well documented (Walker 1935:3, Figure 1), the evidence for "animal burials" at the Glendora site is not recorded by Moore (although he is cited as the source of the information by Gregory [1973:225]). I have recently argued (Kidder 1990a) that the Ouachita Indians were probably not indigenous to the Ouachita River, but were likely to have been a Caddoan-related group temporarily located east of their homeland at contact.

John Belmont has suggested that the Glendora phase sites were largely the result of occupation by Mississippian-related cultures, with the possible exception of Glendora, which represented "at most a brief Caddoan intrusion" (1985:281). The most recent archaeological research on the Ouachita indicates that the river is not within the Caddoan culture area, but has stronger ties to the east (Gibson 1985b; Kidder 1990b). It is evident, however, that the area must be defined in its own terms, and not in reference to cultures to the east or west (Belmont 1985; Fuller 1985; Gibson 1985a, 1985b, 1985c; Jones 1985; Kidder 1990a, 1990b; Price and Heartfield 1977; Rolingson and Schambach 1981).

Surprisingly, given the long history of the Glendora phase as a culture-historical unit, there is no formal definition of the material traits that make up the complex. The existing definitions are limited to geographic location (such as Orr 1952; Williams

1961, 1964) or temporal position (Krieger 1943). Diagnostic traits considered to define Caddoan cultures on the Ouachita were implicitly or explicitly linked to the presence of specific ceramic types, notably Natchitoches Engraved, Keno Trailed, and complex, curvilinear-engraved, "spool-neck" bottles such as are illustrated by Moore (1909). However, Moore illustrated but a fraction of the total ceramic corpus from these two sites. A close reading of Moore's publication reveals that he illustrated only 20 percent of the ceramics from Glendora, and only six percent of the Keno assemblage (Kidder 1988: Tables 16-24, Figure 30). The presumed archaeological markers of the Caddo Indians are either lacking in the Glendora phase (for instance, animal burials [Webb and Gregory 1978]), or they are so common and widely distributed as to make a precise ethnic identification of their makers impossible (Natchitoches Engraved is a good example).

Ceramically, the Glendora phase is marked by the consistent use of a shell-tempered paste with numerous inclusions and a soft, porous texture. Stylistically, the assemblage is heterogeneous, but it is especially notable for the use of curvilinear designs such as swirls, festoons, arcs, and scrolls. Rectilinear designs are limited to simple patterns, most commonly on small bottle necks and the bodies of carinated bowls. Crosshatching is a common element in both rectilinear and curvilinear designs, often being used as a "filler" within a design. Almost all of the pottery that is stylistically local is incised, although trailing is present in a minority of vessels.

Vessel shapes are relatively diverse and include plates, shallow simple bowls, complex bowls, carinated bowls, restricted short-necked jars, and short- and tall-necked bottles. Vessel modes include punctated and notched lips (the "Jordan" mode), stamped, incised, or punctated designs on the necks of short-necked jars (the "Moore" mode), and rarely red and white pigments rubbed into the lines of some vessels. Unusual vessel forms, such as effigy vessels, tripod bottles, and compound "tubby" pots, are present but rare. Vessel shapes apparently derived from outside the region include the pedestaled jar (a common form in the Natchez region) and the "spool-neck" bottle.

Stylistically non-local ceramics include forms that can be traced to the Caddoan region (based on design, vessel form, and temper), to the Natchez area, and also to the lower Arkansas River region (Kidder 1988). Although these ceramics are the best known feature of the Glendora phase, they make up less than 30 percent of the Glendora site collection and less than 20 percent of the Keno site assemblage (Kidder 1990a: Figure 4). Non-local ceramics are rare outside of these two sites (Jones 1985; Moore 1909).

Other artifacts that make up the Glendora phase are more difficult to discuss with confidence since so little recent excavation has been undertaken. Barbed and lanceolate projectile points, chipped stone celts, "hoes," and pierced celts are mentioned by Moore at most Glendora phase sites. Shell beads, pendants, and cups are also present, but not in great quantities. The protohistoric or early historic occupations of the Glendora phase are marked by the presence of European trade goods, including "hawk bells" (found in child graves), metal bands, bracelets, rings, and chisels. Glass beads were also found, but are poorly described (see below).

The protohistoric Glendora phase settlement pattern appears to indicate a dispersed pattern of occupation of river terraces and levees. By the Glendora II subphase, occupation was limited to only four sites. Almost nothing can be said about subsistence, other than the apparent evidence for maize cultivation (see below). Burial patterns are notable for the utilization of cemeteries, a number of modes of burial (flexed, extended, bundle, urn), and the evidence for constant reuse of limited areas for mortuary purposes. Although not commonly noted, these mortuary practices are distinctly non-Caddoan in form. There is evidence neither for subfloor burials nor for interments placed in deep grave pits, as was common on the Red River and farther west. Communal cemeteries, apparently utilizing charnel house processing facilities (at least in part [Kidder 1988]), were the norm. Interment in earlier prehistoric mounds was also practiced in some cases.

GLENDORA PHASE SITES

Moore excavated 16 sites on the Ouachita River and 7 sites on Bayou Bartholomew in northeast Louisiana. Of these 23 sites, those of interest to this work are Pritchard Landing, Glendora, Keno, Sycamore Landing, Ward, Seven Pines Landing, and Bray. Reca Jones (1985) has recently published data pertaining to the excavations of a local amateur, Manning Durham, who located a number of Glendora phase sites in and around the Monroe, Louisiana, area. The following discussion also draws on my analyses of the Moore collections at the Peabody Museum at Harvard (PMH) and the Buffalo Museum of Science (BMS), and my excavations at protohistoric sites in the adjacent Boeuf Basin (Kidder 1988, n.d.).

Pritchard Landing

The excavations at Pritchard Landing were reported in Moore's 1909 publication and also in his unpublished field notes from 1912-1913 (Belmont 1985:278; Gibson 1985b:224). Moore excavated a cemetery that contained at least 74 crania and recovered 15 vessels from the summit of one of the mounds. John Belmont reports that Moore also found a burial with brass discs at the ears accompanied by a pot (Belmont 1985:278). Belmont (per. comm. to Gibson [1985b:224]) thinks that this burial came from just beneath the surface of Mound A. Moore illustrated one vessel and a ceramic elbow pipe from Pritchard Landing; both artifacts appear to date to the Mississippi period. He also stated that the 15 vessels from the site were not shell tempered (Moore 1909:20). There are seven extant vessels from Pritchard Landing, all of which are shell tempered (Table 14.1). The reason for the discrepancy between Moore's description of the pottery and the collections is unknown. The designs and attributes of these vessels suggest that they date to the protohistoric or early historic periods. The latest occupation at the Pritchard Landing site was limited to intrusive burials in mound tops (Gibson 1985b:230).

Myatt's Landing

Moore excavated 38 burials accompanied by 17 ceramic vessels at Myatt's Landing. He noted that "Shell tempering was present in some instances" (1909:26). The two vessels illustrated (Moore 1909: Figures 6-7) date to the early Mississippi period Pargoud phase. The effigy vessel illustrated by Moore as his Figure 7 is similar to a vessel he found at Boytt's Field in the Felsenthal (Moore 1909: Figure 90). The specimen from Myatt's Landing appears to be grit-grog tempered, while the Boytt's Field vessel is clearly shell tempered. The Peabody Museum has one shell tempered spittoon-shaped bottle with broad, curvilinear interlocking scrolls and a notched, "Jordan" mode lip (PMH, catalogue

Table 14.1. Ceramic vessels from Pritchard Landing (16CT14 [LMS 25-I-2]).

Catalogue No.	Type and Variety
74810A	Hudson Engraved, *var. Hudson*
74810B	Hudson Engraved, *var. Hudson*
74811A	Hudson Engraved, *var. Hudson*
74811B	Leland Incised, *var. Wardville*
74812A	Hudson Engraved, *var. Hudson*
74812B	Mound Tract Incised and Brushed, *var. Mound Tract*
BMS 3396	Barton Incised, *var. Mer Rouge*

no. 74808). This vessel is classified as Leland Incised, *var. Bastrop*, and is evidently part of an early protohistoric Glendora I subphase occupation.

Glendora

Farther upstream, near the confluence of Bayou Bartholomew and the Ouachita River, Moore excavated at Glendora, which has subsequently been destroyed (Jones 1985:109). The site consisted of a cemetery that showed "no superficial indication of what lay beneath, and was hardly perceptibly above the surrounding level." He encountered 121 instances of human remains, which "as a rule, consisted of hardly more than traces of bones–sometimes crowns of teeth only, which were crumbling into dust." The cemetery occupied an area of only one-tenth of an acre, though the bulk of the interments were found in an area measuring "54 feet long by 51 feet wide" Burials were arranged in both extended and bundle form, and were found in relatively shallow pits. Moore's description makes it clear that there were several episodes of burials, as he remarked that "there had been great aboriginal disturbance in the cemetery, caused by interments cutting through others previously made"; these activities had "created sad havoc among bones and pottery" (Moore 1909:28).

Moore observed that "In this cemetery little of interest had been placed with the dead, with the exception of vessels of earthenware." He did, however, mention that "Glass beads were found at six points and ornaments of sheet-brass eighteen times." The metal artifacts were found "in the form of small cones; discs of various diameters; tubular beads; wide, annular ornaments which possibly had been worn on the fingers" (Moore 1909:28). "Two implements of iron or of steel . . . resembling slender lancepoints" (Moore 1909:29) were also recovered. In addition Moore reported finding a conch shell, shell beads, chipped and ground stone tools, pebbles, and an earthenware pipe. Moore located 322 vessels at Glendora, although it is uncertain how many he actually was able to excavate, as "Many . . . were in disintegrating fragments" (Moore 1909:30). Pots were usually found in the vicinity of the head of a burial, and "on no occasion were more than five vessels positively determined to have been placed with one burial" (Moore 1909:31).

Moore's description of the Glendora ceramic assemblage highlights the fact that it is representative of at least three, or probably four components. The Moore collection (Table 14.2) includes a late prehistoric Pargoud phase Pargoud Incised jar. A protohistoric Glendora I subphase component is also evident as is, obviously, an early historic (Glendora II subphase) one. A single Pease Brushed-Incised, *var. Sycamore*, vessel from Glendora suggests a Kinnaird phase component as well (Kidder 1988). The Glendora site produced the largest assemblage of Caddoan ceramics from the Lower Ouachita Basin (Table 14.2). Particularly notable in this regard is the presence of both Natchitoches Engraved, *vars. Natchitoches* and *Gopher*, and Keno Trailed, *var. Glendora* (Moore 1909: Figures 10-54).

The ceramics identified as Caddoan in origin are apparently late, probably dating to the early historic period. It is tempting to associate this fact with the intrusion or migration of peoples from the Red River into the region after 1682. However, this impression is negated by the bulk of the pottery, which is locally made. It would seem to indicate, though, that the contacts between the Caddoan cultures of the Red River and the Ouachita Basin increased in the early historic period.

The meager quantities of early historic European trade goods in the Glendora assemblage argue that the site occupation probably was over by the period of 1700-1710. Later early historic cemeteries include much greater quantities of European artifacts (e.g., Brain 1979), and historic documentation of Indian groups in the region is notable for the absence of natives by the early eighteenth century (see Kidder 1988, 1990a). If the quantity of historic trade goods is a reliable temporal indicator, then the Glen-

Table 14.2. Ceramic vessels from Glendora (16OU32 [LMS 22-H-3]).

Catalogue No.	Type and Variety
74768	Unclassified tripod vessel
74769	Natchitoches Engraved, *var. unspecified*
74770	Natchitoches Engraved, *var. Natchitoches*
74771A	De Siard Incised, *var. De Siard*
74771B	De Siard Incised, *var. Belle Hope*
74771C	Winterville Incised, *var. Sterlington*
74771D	De Siard Incised, *var. De Siard*
74771E	Natchitoches Engraved, *var. Lester Bend*
74771F	Barton Incised, *var. Portland*
74771G	Mississippi Plain, *var. Morehouse*
74771H	De Siard Incised, *var. De Siard*
74771I	De Siard Incised, *var. De Siard*
74771J	Leland Incised, *var. Petticoat*
74771K	Keno Trailed, *var. Styx*
74772	Leland Incised, *var. D'Arbonne*
74773	Leland Incised, *var. De l'Outre*
74774	Keno Trailed, *var. unspecified*
74775	Hodges Engraved, *var. unspecified*
74776	Leland Incised, *var. Pace Lake*
74777	Keno Trailed, *var. Glendora*
74778	Cracker Road Incised, *var. Floodgate*
74779	Leland Incised, *var. Imperial*
74780	Barton Incised, *var. Mer Rouge*
74781	Foster Trailed-Incised, *var. unspecified*
74782	De Siard Incised, *var. De Siard*
74783A	Barton Incised, *var. Filhio*
74783B	Barton Incised, *var. Marble Landing*
74783C	Barton Incised, *var. Rock Row*
74783D	Leland Incised, *var. Wardville*
74783E	Unclassified incised on Mississippi Plain, *var. Morehouse*

dora site is slightly earlier than Keno, but postdates Sycamore Landing, Ward, Seven Pines Landing, and Bray.

Sycamore Landing

Although this site was important during the late prehistoric Pargoud phase, it also supported a protohistoric component as well. The site consisted of a mound with a "circular base" which was "11 feet in height and 130 feet in diameter" (Moore 1909:111). The mound at Sycamore Landing was not excavated by Moore and is now destroyed (Jones 1985:106). Surface collections from the mound area suggest that it predated the protohistoric occupation at the site (Jones 1985). Within sight of the mound was a cemetery on "an imperfectly defined rise above the general level, where the soil was darker than that which surrounded it" (Moore 1909:112). The cemetery area measured approximately 39 by

Table 14.2. Continued.

74783F	Barton Incised, *var. Filhio*
74783G	Barton Incised, *var. unspecified*
74783H	Pargoud Incised, *var. Pargoud*
74783I	Unclassified incised on Mississippi Plain, *var. Morehouse*
74783J	Karnack Brushed-Incised, *var. Karnack*
74783K	Mound Tract Incised and Brushed, *var. Mound Tract*
74783L	Foster Trailed-Incised, *var. unspecified*
74783M	Leland Incised, *var. Wardville*
74784	Leland Incised, *var. Wardville*
74785	Hudson Engraved, *var. unspecified*
74786	De Siard Incised, *var. Belle Hope*
81136	Natchitoches Engraved, *var. Lester Bend*
Glendora A	Cracker Road Incised, *var. Floodgate*
Glendora B	Leland Incised, *var. Russell*
Glendora C	Leland Incised, *var. unspecified*
Glendora D	Leland Incised, *var. Pace Lake*
Glendora E	Leland Incised, *var. Pace Lake*
Glendora F	Leland Incised, *var. Petticoat*
Glendora G	Fatherland Incised, *var. Fatherland*
Glendora H	Pease Brushed-Incised, *var. Sycamore*
Glendora I	Hodges Engraved, *var. unspecified*
BMS 3351	Keno Trailed, *var. Styx*
BMS 3361	Winterville Incised, *var. Sterlington*
BMS 3391	Mound Tract Incised and Brushed, *var. Mound Tract*
BMS 3393	Foster Trailed-Incised, *var. Shaw*
BMS 3397	Barton Incised, *var. Rock Row*
BMS 3398	Keno Trailed, *var. Styx*
BMS 3399	Barton Incised, *var. Filhiol*
BMS 3400	Owens Punctated, *var. Elders*
BMS 3401	Keno Trailed, *var. Glendora*
BMS 3402	De Siard Incised, *var. Belle Hope*

46 feet, and burials were found to extend to a depth between 2 and 4 feet. Human remains were "met with in thirty-eight instances, but it was evident . . . that many burials had entirely disappeared." According to Moore "Rarely was a fragment of bone met with that did not crumble at the touch. Skulls were mere outlines in the soil, and all that remained of some burials were decaying crowns of teeth." The bundle burial was the only form of interment noted (Moore 1909:112).

Moore reported that "Seventy-eight vessels of earthenware, lying singly, in twos, and threes, and in one case four together" were recovered from the site. He further observed that "The pottery from this cemetery, as a rule, is inferior, is without shell-tempering, thick and unevenly fired" (Moore 1909:119). Moore's statement that the vessels were not shell tempered is not wholly supported by the ceramics in the Moore collections from Sycamore Landing (Table 14.3). Nine of the twenty-four ves-

Table 14.3. Ceramic vessels from Sycamore Landing (16MO30 [LMS 22-H-4]).

Catalogue No.	Type and Variety
74788	Mazique Incised/Hollyknowe Pinched combination
74789	Pease Brushed-Incised, *var. Pease*
74790	Pease Brushed-Incised, *var. Sycamore*
74791	Pease Brushed-Incised, *var. Sycamore*
74792	Parkin Punctated, *var. unspecified*
74793	Hollyknowe Pinched, *var. unspecified*
74794	Avoyelles Punctated, *var. unspecified*
74795	Coles Creek Incised, *var. Hardy*
74796	Coleman Incised, *var. unspecified*
74797	Parkin Punctuated, *var. Boeuf Brake*
74798	Mississippi Plain, *var. Morehouse*
74799	Addis Plain, *var. unspecified*
74800A	Hudson Engraved, *var. Hudson*
74800B	Addis Plain, *var. unspecified*
74800C	Unclassified incised on Mississippi Plain, *var. Bonita*
74800D	Addis Plain, *var. unspecified*
Sycamore Landing A	Mississippi Plain, *var. Morehouse*
Sycamore Landing B	Unclassified engraved on Addis Plain
Sycamore Landing C	Leland Incised, *var. Bastrop*
Sycamore Landing D	Addis Plain, *var. Feliciana*
BMS 3386	Afton 2::Curry 1
BMS 5534	Addis Plain, *var. unspecified* (?)
BMS 5651	?:: Buffalo (?)
BMS 5652	Cornell 2:: Antioch(?)

sels are tempered with a finely crushed shell that forms a soft, poorly fired ware. The plain pottery resembles Mississippi Plain, *vars. Bonita* or *Morehouse.*

Leaving aside the Pargoud phase ceramics, which are distinctive in terms of paste and decoration, the nine shell-tempered vessels appear to represent two components. One dates to the Kinnaird phase and the other to the Glendora I subphase. The Kinnaird phase component is marked by the presence of Pease Brushed-Incised, *var. Sycamore*, Parkin Punctated, *var. unspecified*, and unclassified incised with a chevron pattern, on Mississippi Plain, *var. Bonita.* The Glendora I component is recognized by the presence of Hudson Engraved, *var. Hudson*, Leland Incised, *var. Bastrop*, and Mississippi Plain, *var. Morehouse* (one *Morehouse* bowl has the "Jordan" mode and can be classified as a "Walnut Bayou" bowl [Hally 1972:356-357]). The vessel classified as *var. Bastrop* at Sycamore Landing has a wide neck and sharply flaring rim with the "Jordan" mode of lip punctations. An almost exact duplicate of this vessel is found in the Peabody Museum collections from Keno.

The dating of the Glendora I component at Sycamore Landing seems relatively secure. The "Jordan" and "Walnut Bayou" modes, in conjunction with the presence of the *Bastrop* bottle and the absence of any early historic trade goods, indicate a protohistoric date for the assemblage. Presumably the site was abandoned prior to, or at the time of, historic contact. Other artifacts from Sycamore

Landing are less diagnostic of the temporal and cultural affiliations of the site. A number of ceramic elbow pipes were recovered from the site (Moore 1909:116, Figures 110-116), as were four limestone effigy pipes (Moore 1909:112-116, Figures 104-109). Moore also reported that he recovered "fifty-six arrowpoints of chert, all barbed and acutely pointed" (Moore 1909: 112).

Keno

Just upstream and across the bayou from Sycamore Landing is the Keno site. Keno has at least three components: late prehistoric (Pargoud phase), protohistoric (Glendora I subphase), and early historic (Glendora II subphase). The bulk of the collection is representative of the latter two sub-phases. The Keno site consisted of a cemetery that was "an indistinctly defined area hardly appreciably higher than the level of the surrounding field" (Moore 1909:120). An area "82 feet by 86 feet . . . was completely worked through . . . at depths depending on the varying thickness of the layer of dark, loamy clay, which rested on . . . untouched clay . . . and upon the depth of the various grave-pits which extended into undisturbed clay, the deepest of which was 3 feet" (Moore 1909:120-121). Most of the interments were in a small area measuring "28 feet by 28 feet in extent, where graves cut through each other and were present almost throughout" (Moore 1909:121). The human remains were in a horrible state of preservation: "The condition of the human remains in this cemetery was such that not only no bones were saved, but practically all that were found could be contained in a space considerably less than the size of a bucket and consisted almost exclusively of mere outlines of skulls, crumbling crowns of teeth, and occasional spongy fragments of long-bones" (Moore 1909:121).

They found "traces" of human bones in 255 instances, although "This enumeration by us is . . . no exact indication of the number of individuals originally buried in the cemetery . . . many burials . . . had disappeared" (Moore 1909:121). Despite the condition of the bones, Moore observed that ". . . the bunched form of burial had been in excess of the flexed burial or the burial at length" (1909:121). Three, or possibly four, fully extended burials were recorded, and one had been interred on a bark mat with a wooden staff, measuring at least five feet long, at its side. A single example of an urn burial was also noted, and Moore's description (1909:122) suggests that it had been placed in the cemetery at a late date, as its burial pit cut through a burial or burials. Elsewhere in the Mississippi Valley, urn burials are only noted in protohistoric Armorel phase contexts (Williams 1978).

According to Moore, "Four hundred and eighty-five pottery vessels were found by us in the cemetery at the Keno Plantation. By this we do not mean that this number of vessels were saved–far from it–but that at least the number of vessels given by us had been placed in the cemetery by the aborigines." Further, it was reported that "owing to the almost complete absence of bones in the cemetery, we were rarely able to come upon a burial and follow it up with the trowel, as our custom is . . . but often reached vessels first with the spade, and in a manner we do not approve" (Moore 1909:129). In no case were more than four vessels found together.

In addition to pottery, Moore recovered a number of lithic artifacts (points, celts, pierced "hoe-shaped" and "spade-shaped" ceremonial axes, chisels, hones, palettes, plummets, a mass of galena, and chert flakes) (see Kidder 1988: Appendix C for a discussion of these artifacts). He also found ornaments and tools of copper and sheet brass, a brass ring, "possibly a hawk-bell" of sheet brass (with a child burial), a double pointed spike of iron or steel, and numerous glass beads, some of which were noted to be blue. Shell beads, masses of red pigment, and 11 elbow pipes were also found at the site (Moore 1909:122-127).

The ceramic assemblage is similar in some ways to Glendora. However, unlike Glendora, the Keno site is lacking the large numbers of Caddoan-related

Table 14.4. Ceramic vessels from Keno (16MO31 [LMS 22-H-5]).

Catalogue No.	Type and Variety
74714	Winterville Incised, *var. Sterlington*
74715	Winterville Incised, *var. Sterlington*
74716	Winterville Incised, *var. Sterlington*
74717	Winterville Incised, *var. Sterlington*
74718	De Siard Incised, *var. unspecified*
74719	Hudson Engraved, *var. Hudson*
74720	Winterville Incised, *var. Sterlington*
74721	Leland Incised, *var. Petticoat*
74722	De Siard Incised, *var. De Siard*
74723	Mississippi Plain, *var. Morehouse*
74724	Leland Incised, *var. Petticoat*
74725	Barton Incised, *var. Portland*
74726A	Natchitoches Engraved, *var. unspecified*
74726B	Barton Incised, *var. unspecified*
74726C	Leland Incised, *var. unspecified*
74726D	De Siard Incised, *var. De Siard*
74726E	Leland Incised, *var. Wardville*
74726F	Leland Incised, *var. Bovina*
74726G	Mississippi Plain, *var. Morehouse*
74727	Winterville Incised, *var. Sterlington*
74728	Winterville Incised, *var. Red Hill*
74729	Leland Incised, *var. Wardville*
74730	Hudson Engraved, *var. Hudson*
74731	Hodges Engraved, *var. Sandige*
74732	Leland Incised, *var. Petticoat*
74734	Leland Incised, *var. unspecified*
74735	Leland Incised, *var. Imperial*
74736	Hodges Engraved, *var. unspecified*
74737	Leland Incised, *var. Bastrop*
74738A	Leland Incised, *var. Bastrop*
74738B	Hodges Engraved, *var. unspecified*
74738C	Leland Incised, *var. unspecified*
74738D	Mississippi Plain, *var. Morehouse*
74739A	Unclassified Caddoan engraved
74739B	Mound Tract Incised and Brushed, *var. Mound Tract*
74740	Belcher Ridged, *var. Belcher Ridged*
74741	Leland Incised, *var. Imperial*
74742	Foster Trailed-Incised, *var. Finley*
74743A	Cracker Road Incised, *var. Floodgate*
74743B	Cracker Road Incised, *var. Floodgate*
74744	Winterville Incised, *var. Red Hill*
74745	Natchitoches Engraved, *var. Gopher*
74746	Hollyknowe Pinched, *var. unspecified*
74747	Hodges Engraved, *var. Armour*
74748	Cracker Road Incised, *var. Floodgate*
74749	Cracker Road Incised, *var. Floodgate*
74750	Leland Incised, *var. Wardville*
74751	Leland Incised, *var. unspecified*
74752	Cracker Road Incised, *var. Floodgate*
74753A	Leland Incised, *var. Wardville*

Table 14.4. Continued.

74753B	Leland Incised, *var. Spanish City*
74753C	Winterville Incised, *var. Tunica*
74753D	Leland Incised, *var. Petticoat*
74753E	Mississippi Plain, *var. Morehouse*
74753F	Leland Incised, *var. Petticoat*
74753G	Barton Incised, *var. unspecified*
74753H	Leland Incised, *var. Petticoat*
74753I	Barton Incised, *var. unspecified*
74753J	Barton Incised, *var. Portland*
74753K	Barton Incised, *var. Marble Landing*
74753L	Mound Tract Incised and Brushed, *var. Mound Tract*
74753M	Mississippi Plain, *var. Morehouse*
74753N	Barton Incised, *var. Filhiol*
74753O	Barton Incised, *var. Filhiol*
74753P	Leland Incised, *var. Bastrop*
74753Q	Leland Incised, *var. Bastrop*
74753R	Grace Brushed, *var. unspecified*
74753S	Grace Brushed, *var. unspecified*
74753T	Barton Incised, *var. Marble Landing*
74753U	Barton Incised, *var. Mer Rouge*
74753V	Barton Incised, *var. Filhiol*
74753W	Leland Incised, *var. Wardville*
74753X	Leland Incised, *var. Petticoat*
74753Y	Keno Trailed, *var. Glendora*
74754	Hodges Engraved, *var. unspecified*
74756	Carson Red on Buff, *var. Olmond*
74757	Carson Red on Buss, *var. Olmond*
74758A	Addis Plain, *var. unspecified*
74758B	De Siard Incised, *var. Belle Hope*
74758C	Hudson Engraved, *var. Hudson*
74759	Cracker Road Incised, *var. Floodgate*
74760	Winterville Incised, *var. Sterlington*
74761	De Siard Incised, *var. unspecified*
74762	Fatherland Incised, *var. Fatherland*
74763	Unclassified Caddoan engraved
74764	Hodges Engraved, *var. Sentell*
Keno A	Keno Trailed, *var. Glendora*
Keno B	Cracker Road Incised, *var. Floodgate*
Keno C	Hodges Engraved, *var. Sandige*
Keno D	De Siard Incised, *var. Belle Hope*
Keno E	Leland Incised, *var. Bastrop*
Keno F	De Siard Incised, *var. De Siard*
Keno G	Keno Trailed, *var. Glendora*
Keno H	De Siard Incised, *var. De Siard*
Keno I	Mississippi Plain, *var. Morehouse*
Keno J	Winterville Incised, *var. Red Hill*
Keno K	Fatherland Incised, *var. Fatherland*
Keno L	Hudson Engraved, *var. Hudson*
Keno M	Addis Plain, *var. Addis*
Keno N	Hudson Engraved, *var. Hudson*
Keno O	Hudson Engraved, *var. Hudson*

Table 14.4. Continued.

Keno P	Hudson Engraved, *var. unspecified*
Keno Q	Leland Incised, *var. Wardville*
Keno R	Hodges Engraved, *var. Sandige*
Keno S	Leland Incised, *var. Bastrop*
Keno T	Leland Incised, *var. unspecified*
Keno U	Leland Incised, *var. Bastrop*
Keno V	Owens Punctated, *var. unspecified*
BMS 3362	De Siard Incised, *var. unspecified*
BMS 3363	Leland Incised, *var. unspecified*
BMS 3364	Leland Incised, *var. unspecified*
BMS 3365	Hodges Engraved, *var. Sentell*
BMS 3367	Hudson Engraved, *var. Hudson*
BMS 3369	Winterville Incised, *var. Red Hill*
BMS 3370	Hudson Engraved, *var. Hudson*
BMS 3371	Winterville Incised, *var. Red Hill*
BMS 3372	Leland Incised, *var. Wardville*
BMS 3373	Leland Incised, *var. Petticoat*
BMS 3374	Leland Incised, *var. Wardville*
BMS 3375	Leland Incised, *var. Petticoat*
BMS 3377	Winterville Incised, *var. Red Hill*
BMS 3379	Winterville Incised, *var. Spanish City*
BMS 3380	Leland Incised, *var. Bastrop* (?)
BMS 3381	Karnack Brushed-Incised, *var. Karnack*
BMS 3382	Fatherland Incised, *var. Fatherland*
BMS 3383	Hodges Engraved, *var. unspecified (var. Candler?)*
BMS 3384	Winterville Incised, *var. Sterlington*
BMS 3388	Barton Incised, *var. Rock Row*
BMS 3389	Barton Incised, *var. Mer Rouge*
BMS 3390	Leland Incised, *var. Wardville*
BMS 5527	Hudson Engraved, *var. Hudson* (?)
BMS 5531	Natchitoches Engraved, *var. unspecified*
BMS 6082	Hudson Engraved, *var. Hudson*

Natchitoches set ceramics, and there are more (albeit not too many more) Quapaw set diagnostics (Table 14.4). The quantity of early historic trade goods is slightly greater than at Glendora, which might be an indication that the site is more recent. This dating may be more of an illusion than reality, however, and it is certain that the two sites overlap temporally. It is just as certain, though, that the Keno site was not occupied by any major group after the period of 1710-1720. The dearth of early historic trade goods and the absence of evidence in the historical documents suggests that the site was abandoned by this time (Kidder 1990a, 1990b).

Ward

The Ward site lies on the west side of Bayou Bartholomew, south and east of Wardville. The site now lies beneath Bussy Brake and is presumably destroyed. When Moore excavated, the site consisted of "a slight rise in the ground where the soil was darker than the rest of the field" (Moore

1909:151). Thirty-one vessels and one large vessel fragment were found at Ward, associated with thirty-one burials. The skeletons were found to be buried in an extended position with the heads facing "in a southerly direction . . . about parallel to the bayou" (Moore 1909:151). According to Moore (1909:151-152) "It was evident that the skeletons, all of which belonged to adults, with the exception of three adolescents and two children, had been placed in the ground when denuded of flesh, as bones were often out of place and small bones, in some instances, were missing." Bone preservation, both of human skeletal remains and bone artifacts, was good at the Ward site.

As a result of the excellent bone preservation, Ward is the only Glendora phase site from which we have an analyzed skeletal sample (Hrdlička 1909:183). All of the skulls exhibited some form of artificial cranial deformation (Hrdlička 1909:185). Though pathologies were not common at Ward, several, which Hrdlička (1909:184, 232-239) ascribes to syphilis, were recorded for the site. In addition, arthritic changes were represented in several instances.

Of the 31 vessels recovered by Moore, only three are illustrated in his report (1909: Figures 161-162, 164). Seven vessels are now housed at the Buffalo Museum of Science, and four are found in the Peabody Museum collections (Table 14.5). Moore (1909:154) observed "The pottery . . . is, with one or two exceptions, shell-tempered," and that "Curiously enough the bottle was not found in this cemetery." No early historic trade goods were found at the Ward site. The lack of early historic artifacts, in conjunction with the ceramic assemblage, suggests a protohistoric Glendora I subphase date for the site.

Seven Pines Landing

Farther up Bayou Bartholomew, on the east side, Moore excavated at the Seven Pines Landing site. Here, he excavated an area "33 feet by 42 feet in extent . . . [which] was completely dug through by us." The arrangement of the burials was confused, and the condition of the bones poor, "having at times but slightly more consistency than moistened sawdust possesses" (Moore 1909:157). In four instances Moore recognized extended burials, and one "bunched" burial was also discovered. Due to the poor preservation these were the only burials noted, "though altogether bones were encountered in forty-two places" (Moore 1909:157).

Thirty-nine vessels were found at Seven Pines Landing, and "Their position was usually near the skull, sometimes singly, never more than two together" (Moore 1909:159). The pottery was "shell tempered in most cases, had been insufficiently

Table 14.5. Ceramic vessels from Ward (16MO12 [LMS 21-I-5]).

Catalogue No.	Type and Variety
74814A	Leland Incised, *var. Wardville*
74814B	Leland Incised, *var. Wardville*
74815A	Hudson Engraved, *var. Hudson*
74815B	Barton Incised, *var. Mer Rouge*
BMS3359	Mississippi Plain, *var. Morehouse*
BMS3360	Barton Incised, *var. Rock Row*
BMS3368	Leland Incised, *var. unspecified*
BMS3376	Mississippi Plain, *var. Morehouse*
BMS3385	Leland Incised, *var. Wardville*
BMS3387	Leland Incised, *var. Bastrop*

Table 14.6. Ceramic vessels from Seven Pines Landing (16MO10 [LMS 21-I-4]).

Catalogue No.	Type and Variety
74801	Leland Incised, *var. Bastrop*
74802	Leland Incised, *var. Bastrop*
74803	Pease Brushed-Incised, *var. Sycamore*
74804A	Winterville Incised, *var. Spanish City*
74804B	Pouncey Pinched, *var. unspecified*

kneaded, so that the distribution of tempering material was uneven, and later, presumably, the paste had been insufficiently fired" (1909:159). Three vessels from Seven Pines Landing were illustrated by Moore (1909: Figures 171-173), and five pots were found in the Peabody Museum collections (Table 14.6). These ceramics indicate that Seven Pines Landing supported two components, one dating to the Kinnaird phase and another to the Glendora I subphase.

The ceramic assemblage demonstrates continuity of occupation from one component to another. As an example, the single *var. Sycamore* jar has a neck with the "Herringbone" mode, as do two of the protohistoric period *Spanish City* vessels (see, for example, Moore 1909: Figure 172). The description of the burials at Seven Pines Landing indicates that interments were placed through previous burials, suggesting some time depth to the site formation. The absence of historic trade goods is a reasonable confirmation that the site was abandoned prior to ca. 1682.

Bray

The last of Moore's sites on Bayou Bartholomew is the Bray site. Moore and his workers excavated an area that was "46 feet in diameter." As was so often the case, the area was "completely dug through" (Moore 1909:162). Moore describes the site as being located on a "hardly perceptible elevation" (Moore 1909:161), but later, in a discussion of the skeletal material, he states that "The majority of bones in the mound were . . . scattered throughout" (Moore 1909:162). It is therefore unclear if a true mound was represented, or if this was a term used to describe the elevation that marked the site.

Bone preservation was generally poor, and the bones were often isolated or unconnected with an identified burial. Despite this disarray "seventeen burials were exactly noted, all of adults, with two exceptions–but these were a small proportion of the interments present in the mound" (Moore 1909: 162). Moore (1909:162) observed that "Of the fifteen adult burials, thirteen lay at full length on the back and two were extended, face down . . . The heads of all the adult burials were in a southerly direction with the exception of three." One extended skeleton was buried in a pit with another skeleton located "in the same pit . . . but somewhat above it" (Moore 1909:163).

Excavations uncovered 26 vessels, "of which only two were intact." Vessels were placed near the head, and "never exceeded two with a single burial." The pottery "some of which had shell-tempering while some was without it, was as a rule inferior . . . Many were disintegrated beyond repair" (Moore 1909:163). Moore describes two gourd-shaped vessels of "porous" or "coarse yellow ware" (reasonably inferred to be shell tempering), one with "rude" decoration (Moore 1909:166). Two vessels from Bray were found in the Peabody Museum collection. One is a Leland Incised, *var. Bastrop* jar, while the other is classified as Parkin Punctated, *var. Boeuf Brake*. This latter jar has a set of concentric circles centered on prominent nodes, not unlike those found on Foster Trailed-Incised (e.g., Schambach and Miller 1984: Figures 11-16b, 11-21a, 11-25a, 11-

28a, c). This jar also has a single row of punctations at the junction of the body and rim. Elsewhere this pattern has been referred to as the proto-Tunican mode by Brain (1988).

At least 11 elbow pipes were found at Bray (Moore 1909:163, Figures 175-182). Their form is fairly diverse, but most appear to be shell tempered. Moore (1909:163) reports that these pipes were usually found near the skull, and in one instance a pipe was found against the left side of the jaw of an extended skeleton. The Bray site seems to be similar to Seven Pines Landing in that it supports two components, one dating to the Kinnaird phase, and one to the Glendora I subphase. The evidence suggests that there was no strong division between the two components. Bray is situated not far from the Matheny site, which has a Jordan phase component (Kidder 1986, 1988). The boundaries between the Glendora I and Jordan phase occupations have not been ascertained, but the present evidence suggests that Bray shares more with Glendora I than Jordan. At an earlier date Bray appears to have been an outlying village site of the Kinnaird phase occupation at Matheny. The Bray site assemblage indicates a late prehistoric and early protohistoric date for the site.

Moon Lake

Just north of Monroe, Louisiana, Manning Durham excavated an important Glendora phase component at the Moon Lake site (Jones 1985:111-112). Here 24 skeletons were encountered, aligned in four rows of six individuals each. All of the skeletons were "extended and supine on a north-south axis" (Jones 1985:112). The cemetery was "capped with reddish clay soil, strikingly different from the surrounding brown sandy loam" (Jones 1985:111). Jones notes that Moon Lake was excavated in 1947, and consisted of a two hectare area, with the cemetery capped with clay (1985:111). Gregory, however, records that the site consisted of an earth and shell midden, and had been excavated in 1941 (1973:192). The two authors also differ in the number of shell beads accounted for by Durham, with Jones listing four adult burials as having been interred with 125 shell beads (presumably she means 125 beads per individual), for a total of 500 beads (Jones 1985:111). Gregory (1973:192) lists a total of 525 shell beads, and also "12 collumnella [*sic*] pendants, beads." He also lists Keno Trailed and Foster Trailed-Incised as being present in the Moon Lake assemblage. No mention of these vessels is found in Jones' account of the Durham collection.

Four of the "adult" skeletons were interred with shell necklaces "of approximately 125 beads measuring 2.0 to 7.0 mm in length" (Jones 1985: 112). In addition, two shell buttons with a hole in the center were recovered (Jones 1985:112, Figure 7, no. 13), along with two "paper thin" round copper discs, 5.2 cm in diameter, and at least 81 historic glass beads (Gregory 1973:192). Each burial was accompanied by "a vessel placed in the vicinity of the head" (Jones 1985:112).

Presumably there were originally 24 vessels in the Moon Lake assemblage, but Jones notes that Durham only had 15 when she conducted her analysis. Jones (1985:152-153, Figures 6-7) illustrates ten vessels and describes an eleventh in the text. All of the vessels, with one possible exception, are shell tempered (Jones 1985:152-153). There are three Barton Incised jars in the Durham collection, and one can be identified as *var. Portland* (Jones 1985: Figure 7, no. 11). One of the vessels (Jones 1985: Figure 6, no. 5) is listed as a Barton Incised jar (assigned tentatively to *var. Estill* [Jones 1985:152]), but would now be identified as Owens Punctated, *var. unspecified.* A bottle (Jones 1985: Figure 6, no. 4) is listed as Fatherland Incised, *var. Stanton* (shell tempered) (Jones 1985:152), but would be more appropriately identified as Cracker Road Incised, *var. Petticoat.* A Natchitoches Engraved, *var. Lester Bend* bowl was also recovered (Jones 1985:152, Figure 6, no. 5), as was a De Siard Incised, *var. unspecified* bowl with an additional scroll element below the paneled rectilinear design (Jones 1985:153, Figure 7, no. 8). The latter vessel has the "Jordan" mode of lip punctation. A unique vessel is illustrated by Jones (1985:153, Figure 7, no. 7). This bowl has a restricted neck, globular shoulders, and a pedestaled base. The shoulder is decorated with rectilinear "U"- and "hook"-shaped elements, and the pedestaled base has sets of five vertical lines extending from the base to the junction of the shoulder. Jones suggests that this shell-tempered vessel might be identified as Fatherland Incised (Jones 1985:153), but this is an inappropriate identification. The vessel shape has certain affinities to Womack Engraved as reported at the Gilbert site in Texas (Story *et al.* 1967: Figures 50, 52), but it cannot be identified as this type either.

Another unusual vessel published by Jones (1985:152, Figure 6, no.2) can be related to similar vessels at the Glendora site. This is a compound Hudson Engraved "tubby" pot, with both rectilinear and curvilinear crosshatch-filled designs. Jones notes that red pigment was found in the lines and that the shape of the vessel is similar to ones reported at the Glendora site (Moore 1909: Figures 56, 61). The vessel was no longer in the Durham collection when it was studied by the author, but it is similar to vessels in the Morehouse 7 subset. A plain shell-tempered bowl was also found at Moon Lake. It is shown in Jones' illustration as having a teapot-like spout (1985: Figure 6, no. 1). However, the text notes that the spout was reconstructed by Durham (Jones 1985: 152).

The historic trade beads are briefly discussed by Gregory (1973:192). He records that 81 beads were in the Durham collection, and lists the following types: "blue, white, red on blue, blue on white striped, cornaline d'aleppo [*sic*], clear glass." The only diagnostic type listed is the Cornaline D' Aleppo, which has a mean date of 1727 (Brain 1979:106), but includes a much wider temporal distribution. The blue beads, though not diagnostic as recorded, may be similar to the blue beads recorded by Moore at Keno (Moore 1909:122). Jeffrey Brain has noted that this bead assemblage would appear, based on the meager description provided by Gregory, to date to the late seventeenth or early eighteenth centuries (pers. comm. 1987). The Moon Lake site is one of the few Glendora II subphase sites in the region. It is likely that the site represented a single component, as the burials were aligned in rows, were closely spaced, but were not touching. Gregory has suggested that Moon Lake was one of the last sites of the Ouachita Indians on the Ouachita River prior to their movement west to the Natchitoches region (Gregory 1973:237).

Rock Row Landing

Not far north of Moon Lake Durham excavated the Rock Row Landing site, located on the Ouachita near the junction of Lonewa Bayou (Jones 1985:110-111, Figures 4-5). Here, 10 extended burials were excavated from a "midden mound." Jones (1985:133-134, 152, Figures 4-5) notes that Durham recovered 18 vessels, but only 14 are described and 12 are illustrated. Thirteen of the 14 vessels described by Jones are shell tempered, and the fourteenth is not identified as to temper. Four vessels from Rock Row are plain, although one of the undecorated bowls has a scalloped rim (Jones 1985: Figure 4, no. 1) and one has four "peaks" and the "Jordan" mode (Jones 1985: Figure 4, no. 8). A third bowl is not illustrated (Jones 1985:133), and a plain jar was also found (Jones 1985: Figure 4, no. 4). A single jar with simple rectilinear incising is identified as Barton Incised, *var. Rock Row* (Jones 1985: Figure 5, no. 11). Three De Siard Incised bowls were found at Rock Row; two are identified as *var. Belle Hope* (Jones 1985: Figure 4, no. 2, Figure 5, no. 13) and one as *var. De Siard* (Jones 1985: Figure 4, no. 9). One of the *Belle Hope* bowls and the *De Siard* bowl have the "Jordan" mode (Jones 1985: Figure 4, nos. 2, 9). Two Cracker Road Incised vessels were also found at Rock Row. One can be identified as *var. Floodgate*. This vessel is a pedestaled jar with an everted and rolled rim (Jones 1985: Figure 4, no. 7). The second Cracker Road vessel is a bottle with a flaring rim and flat base. This can only be identified as *var. unspecified* (Jones 1985: Figure 5, no. 10).

One jar from Rock Row has a compound design with a Barton Incised neck and a simple incised volute on the body (Jones 1985: Figure 4, no. 7). Another jar has a rectilinear Barton Incised pattern on the neck with a single row of deep punctations on the shoulder (Jones 1985: Figure 5, no. 12). The punctations are similar to the "Tunican" mode. A shoulderless jar has two rows of deep punctations (possibly stamped?) zoned by incised lines (Jones 1985: Figure 4, no. 6). A jar with a running scroll and zoned punctated band around the neck is described, but not illustrated (Jones 1985:152).

No historic trade goods were recovered at Rock Row. Jones records that Durham found a shell hoe and a bone needle at the site. In addition a large vessel was found to cover six smaller pots, and each of the six vessels contained maize (Jones 1985:110). Other vessels apparently contained decayed food remains. The ceramic assemblage at Rock Row would appear to date to the Glendora I subphase.

Zeigen Point

The Zeigen Point site was on the Ouachita River in what is now downtown Monroe (Jones 1985: 116); it has subsequently been destroyed. The site has yielded a small Glendora phase occupation. Jones illustrates six vessels from the Durham collection (Jones 1985: Figure 13) and describes a seventh vessel in the text (Jones 1985:157-158). Durham apparently excavated 16 burials (Jones 1985: 116) that were "caving into the Ouachita River" (Jones 1985).

The vessels from Zeigen Point are all small, and include several miniatures. A plain shell tempered vessel with an unidentified animal effigy rim adorno was found, as was a plain shell tempered jar. A small bowl from the site can be identified as Leland Incised, *var. Bovina* (Jones 1985:157, Figure 13, no. 1). The exterior has three sets of "S"-shaped scrolls, and the interior "closely resembles Leland Incised" (Jones 1985:157). One illustrated bowl can be classified as De Siard Incised, *var. Belle Hope* (Jones 1985: Figure 13, no. 3). This vessel has the "Jordan" mode. Jones (1985:158) also describes a miniature vessel with a neck decorated with "two parallel incisions" which form a "step" pattern. The body has a scroll design with oblique hatching. Tentatively this might be assigned to De Siard Incised, *var. unspecified.* Two jars cannot be classified. One has simple vertical trailed lines running from the neck to the base (Jones 1985: Figure 13, no. 5), and the other has a compound design with a punctated or stamped neck and deeply incised festoons pending from a line at the neck (Jones 1985: Figure 13, no. 7). The ceramics from Zeigen Point indicate a Glendora I subphase occupation.

Pargoud

The Pargoud site, also in Monroe, appears to have had a Glendora II subphase component. Although the excavations here have never been published, it has been reported that "Within the first 4 cm of stratum 1 [in Mound B], blue French tade [*sic*] beads dating from about 1700-1730 were recovered" (Price 1979:5.6). Gregory (1973:193) has reported that shell-tempered plain sherds and 211 beads (listed as "whites, blues, and blacks") were also found at the site.

Gerson

South of Monroe Durham excavated at the Gerson, or Filhiol Mound, site (Jones 1985:127-128). Here in the midst of a Pargoud phase midden were several protohistoric burials that yielded three shell-tempered vessels (Jones 1985:161-162). Two vessels are bowls, and both can be identified as De Siard Incised. One is assigned to *var. Belle Hope*, while the other is classified as *var. unspecified* (Jones 1985: Figure 17, nos. 3-4). The *Belle Hope* bowl has the "step" motif and also the "Jordan" mode (Jones 1985: Figure 17, no. 4). The third vessel is a tall-neck jar with the "Jordan" mode (Jones 1985: Figure 17, no. 9). The design consists of opposed festoons on the neck and a continuous concentric meander on the body. The designs are executed in a fine-line technique, and the vessel can be tentatively assigned to Cracker Road Incised, *var. unspecified.* Gerson would appear to be a small Glendora I subphase occupation.

Ragland

Also south of Monroe, but located east of the Ouachita, is the Ragland site. The site is located on the southern end of a ridge near the junction of Prairie Bayou and Petticoat Bayou, roughly halfway between the Ouachita and Bayou Lafourche. The site consisted of midden areas and a flat-topped mound, one meter high, with a north-south axis of roughly 50 meters, and an east-west dimension of 60 meters. A midden 20 to 40 cm deep covered the mound. Durham excavated between 12 and 14 burials from the "northeast apron" of the mound. It is not possible to ascertain whether these burials were intrusive into the mound. All the burials were extended and articulated and were oriented north-south. Nine vessels were recovered, all apparently having been found near the skulls of the burials (Jones 1985:127).

Eight of the nine vessels are shell tempered. The one exception is a Pargoud phase bottle (Jones 1985:161, Figure 16, no. 9). Three of the vessels bear brushed designs. One can be assigned to Grace Brushed, *var. unspecified* (Jones 1985:160, Figure 16, no. 4), while the other two can be classified as Mound Tract Incised and Brushed, *var. Mound Tract* (Jones 1985:160, Figure 16, nos. 3, 5). Two of the vessels illustrated by Jones are classified as Cowhide Stamped (Jones 1985: Figure 16, nos. 1-2) but really do not conform to the type as currently defined (see Kidder 1988: Appendix B). These vessels have the "Jordan" mode and a compound design on the neck and body. One has a punctated or stamped neck, with a simple meandering design on the body (Jones 1985: Figure 16, no. 1), while the second has a Barton Incised, *var. Portland* design on the neck and a simple scroll pattern on the body (Jones 1985: Figure 16, no. 2). Two vessels can be classified as Hudson Engraved (Jones 1985: Figure 16, nos. 6, 9) and a third conforms to the type Karnack Brushed-Incised, *var. unspecified* (Jones 1985: Figure 16, no. 8). Ragland is particularly interesting as it is evidently contemporary with the protohistoric Jordan II subphase (Kidder n.d.). The presence of several brushed and brushed-and-incised vessels is particularly noteworthy, as is the use of the "Jordan" mode of lip modification.

McHenry

The only other site excavated by Durham that had some evidence of Glendora phase occupations is McHenry. Four Glendora phase vessels were recovered from a burial or burials in a Pargoud phase midden and mortuary (Jones 1985: Figure 15, nos. 1-2, 5, 9). Three of the vessels are plain, although one has a "proto-Tunican" mode and a crudely pinched lip (Jones 1985: Figure 15, no. 1). The fourth vessel is a tall-neck jar with a chevron pattern of neck decoration and a plain body (Jones 1985: Figure 15, no. 9). McHenry apparently dates to the Glendora I subphase.

REGIONAL COMPARISONS

The Glendora phase was obviously not an isolated manifestation; rather, this population interacted with a number of cultures in surrounding regions. The heterogeneous nature of the Glendora phase ceramic assemblage points to contacts with every major culture in the Lower Mississippi Valley and adjacent regions. This section will place the Glendora phase in its regional context and point out salient features of the interaction between the peoples of the lower Ouachita and surrounding ethnic groups.

The Boeuf Basin

The Jordan phase in the Boeuf Basin has some close similarities to the Glendora phase (Kidder n.d.). The connections are only noted between the Jordan II and Glendora I subphases, though, as the two phases do not overlap completely in time. Specific ties between the two subphases are noted in the Morehouse subsets and also in direct modal associations. Notable in this regard is the presence of the "Jordan" mode in Glendora I and the "Ouachita" and "Moore" modes in Jordan II. The two subphases share the same basic ceramic ware, and many vessel shapes are duplicated. There are some important differences, however. The most glaring fact about Glendora I in relation to the Jordan phase is the nearly complete absence of brushed pottery. The only connection here is in Mound Tract Incised and Brushed, *var. Mound Tract*, and the brushed pottery at Gerson. Other than this one type, brushing is exceedingly scarce in the Glendora phase. Also, the Glendora phase has few representatives of the Morehouse 3 subset, which at Jordan is a probable indication of contacts with the Tunica. As will be discussed below, the differences in settlement patterns between the Jordan and Glendora phases are also striking (Kidder 1990b).

Southern Lower Ouachita/Tensas Basin

The Tensas Basin appears to have supported a low population during the span of the Glendora phase. The two known protohistoric and early historic phases, Canebrake and Taensa, show only indirect ties to Glendora I or II. The Canebrake phase (Kidder 1988), which would seem to be contemporary with Glendora I, has yielded a modest amount of Morehouse 2-4 subset ceramics and also has evidence of contacts with the Emerald phase to the east. The Canebrake assemblage also includes Mississippi Plain, *var. unspecified* bowls and jars with the "Jordan" mode, and several instances of the "Moore" mode are also documented.

The Pritchard Landing site assemblage is an important part of the interpretation of the protohistoric period in the southern lower Ouachita. Here we find a good example of the Morehouse 5 subset, which includes the "Jordan" and "Moore" modes, as well as the "Ouachita" jar and the bottle with appliqued collar. The presence of Hudson Engraved and Mound Tract Incised and Brushed ties the site's protohistoric complex to the Glendora phase. But its physical distance from other Glendora phase sites makes it difficult to include Pritchard Landing in the phase without stretching it beyond its limits.

Pritchard Landing has close stylistic and modal ties to the Canebrake site protohistoric assemblage, and it is physically much closer to Canebrake than to any other Glendora phase site. Further research is clearly necessary, but it is evident that the Canebrake and Pritchard Landing site components are too closely related to ignore their similarity. It is possible to speculate that Pritchard Landing is a slightly later occupation by the same ethnic group that made up the Canebrake phase in the Tensas Basin. The Taensa phase seems to have had no interaction with the peoples of the Glendora phase, at least as well as can be told from our meager collections. Rather, the Taensa phase manifests close ties to the Natchez region immediately to the east (Williams 1967).

Lower Yazoo Basin

Contacts between the Ouachita and lower Yazoo Basin were muted during the Glendora phase. One reason for the weak interaction was physical distance, and another was the presence of the Jordan phase that was positioned between the Glendora I and Wasp Lake II subphases. The manifestations of contact with the Ouachita are found in the Morehouse 2 and 4 subsets in the Glendora I subphase, and the Holly Bluff 2-3 and Yazoo 7 and 8 subsets in the Yazoo (Brain 1988). But these connections were weakly developed in both the Ouachita and the Yazoo. Even in the early historic period there is little evidence of increased contact between the

Glendora II and Russell phases. Evidently there were Mississippian influences entering into the Lower Ouachita, but they were being selectively adopted and were never wholeheartedly embraced. Just as notable, though, is the lack of Glendora phase markers in the Wasp Lake II or Russell phases (Brain 1988).

Natchez Bluffs Region

The connections between the Natchez Bluffs region and the Lower Ouachita were stronger during the Glendora I subphase but indirect at best in the early historic period. The ties in Glendora I are to the Emerald phase, while there is only minimal evidence of contacts between the Glendora II and Natchez phases. Specific ties to the Natchez region are noted by the presence of the St. Catherine 1 subset in Glendora I. This subset duplicates the temper, vessel forms, and several specific types and varieties that are recorded for the Emerald phase (Brain *et al.* n.d.; Brown 1985: Table 2). It is of great interest to note that Leland Incised, *var. Pace Lake* is found both in the Glendora phase as well as in Emerald phase contexts at the Anna site in Mississippi (Cotter 1951: Figure 20, nos. 1-2). The same connection is evident with De Siard Incised, *var. Belle Hope* (Brown 1985: Figure 37h-l; Cotter 1951: Figure 16, nos. 11-12). Surprisingly, Fatherland Incised, *var. Fatherland* is somewhat rare in the Glendora phase. Also missing from the Glendora I subphase and St. Catherine 1 subset are the Maddox Engraved varieties. Possibly these were replaced by Hudson Engraved on the Ouachita. Furthermore, it is worth recording that Glendora I markers are not specifically mentioned as present in the Emerald phase, unless *Pace Lake* is being imported from the Ouachita.

By the Glendora II subphase there is almost no evidence of sustained contact with the Natchez Bluffs region. There are some indirect ties, particularly in the Morehouse 9 subset, but the relationship between the Natchez phase and Glendora II is essentially generic. Vessel shapes and certain design motifs on Cracker Road Incised, *var. Floodgate* are derived from the Natchez region, but the connections could have been filtered through intermediate regions, or more likely were local interpretations of earlier St. Catherine 1 subset ideas. There is also little evidence of contacts going the other way–that is, from the Ouachita to the Natchez region. A few Natchitoches 1 subset diagnostics appear at the Fatherland site (Nietzel 1965: pl. 11a, gg), but this subset is not necessarily diagnostic of specific connections to the Ouachita.

Bartholomew-Macon Region

There are some particularly strong ties between the Glendora I subphase and the Tillar phase of southeast Arkansas. However, by Glendora II the Bartholomew-Macon region appears to have been depopulated, and there is no evidence of contacts in that direction. The ceramics that make up the Morehouse 6 subset are well represented in the Tillar phase. This is especially true of Leland Incised, *var. Bastrop*, and Winterville Incised, *var. Spanish City*. A single Winterville Incised, *var. Red Hill* bottle with a modified hourglass neck from Keno is almost duplicated by a vessel from the Austin site (Jeter 1986: pl. 4.5c). Important modal ties include the "Jordan," "Tunican" (or more appropriately the "Tillar"), and "Moore" modes, long- and short-neck bottles, and the "Ouachita" jar. The Tillar and Hog Lake phases are closely related to both Jordan II and Glendora I, but in terms of specific connections there is a stronger parallel with Glendora I.

Perhaps because the Glendora and Tillar phases are known solely from mortuary contexts, the ceramic connections are limited to one subset. The lack of well documented mortuary analysis in both phases hampers direct comparison, but it is evident that Glendora and Tillar were following a similar pattern as regards their mortuary programs. Both phases include burial in cemeteries, although the Tillar and Hog Lake phases include interments in mounds (possibly constructed during the protohistoric period) (Jeter *et al.* 1979:47). The dead were buried in a variety of positions, but in both phases charnel house mortuary activities are indicated by the presence of bundle and extended defleshed burials.

Arkansas River Region

There is little evidence of connections between the Glendora I subphase and any manifestations on the Arkansas River. Ties to the Arkansas at this time are general, and may have been mediated through the Jordan II subphase populations to the east. However, in Glendora II there is a sudden appearance of Menard Complex ("Quapaw phase") diagnostics reflected by the Quapaw set. Further ties are noted by the presence of willow leaf arrow points in Glendora phase components (Kidder 1988: Appendix C).

In Glendora II the Quapaw markers are Carson Red on Buff, *var. Olmond* (including the "Helmet" bowl), Nodena Red on White, and Old Town Red (bottles and teapot-shaped vessels). The important diagnostic Wallace Incised is missing from the Quapaw set in Glendora II, but this absence could be due to a sampling problem. The specific vessel modes in the Glendora II Quapaw set are further proof of strong connections to the Arkansas.

In the Quapaw and Carden's Bottom phases there is evidence of contacts with the Ouachita as well. This interaction is most notable in certain modes, such as the "Jordan" mode, and the spool-neck bottle (Hoffman 1975-1977: Figures 4-6). An important aspect of the presence of Glendora II markers in the Quapaw phase is the presence of Barton Incised, *var. Rock Row* (Hoffman 1975-1977: Figure 6, no. 46b, Figure 10, no. 55a; House 1986:28-29), a Morehouse 8 subset diagnostic. There is an undefined variety of Mound Tract Incised and Brushed in both the Quapaw and Carden's Bottom phases (Clancy 1985: Figures 28, 34 [identified as Barton Incised, *var. Carden's Bottom*]; Hoffman 1975-1977: Figure 9, no. 31a; House 1986), which is a possible sign of contacts between the Ouachita and Arkansas River.

Moore's excavations at sites on the Lower Arkansas River produced a number of vessels that can be dated to the protohistoric and early historic periods. Specific connections to the Glendora phase are hard to sort out because these ceramics are also similar to those from the Jordan phase. Evidence for Morehouse 5 subset ceramics is found at Menard (Ford 1961: Figures 11-13; Moore 1908: Figure 61) and Old River Landing (Moore 1908: Figure 37). Not surprisingly there was considerable interaction between the cultures along Bayou Bartholomew and the Arkansas River, as Menard Complex markers are also known in the Tillar phase. The evidence for the connections between Glendora II and the Carden's Bottom phase is less obvious, possibly due to the way the phase has been constructed (Clancy 1985), and partly because the interaction would probably have been conducted via the Tillar phase peoples.

Felsenthal Region

There is good evidence that the people of the Caney Bayou phase in the Felsenthal region were interacting with the Glendora I subphase, but there does not seem to be any reason to believe that Caney Bayou was contemporary with Glendora II. The Caney Bayou phase as defined by Rolingson and Schambach encompasses a broad time range, but most of the ceramics can be identified as protohistoric (Rolingson and Schambach 1981:193). Important markers in this regard are assigned to the Morehouse 5 and 6 subsets, and include Mound Tract Incised and Brushed and Winterville Incised, *var. Red Hill* (Rolingson and Schambach 1981: Figures 44c, 45a, b, d). Modal connections are represented by the "Jordan" and "Moore" modes, the tall-neck jar, and the use of punctations to zone incised lines (Rolingson and Schambach 1981: Figures 36, 44, 45; White 1987: Figures 17, 27d, 28a, 29a, 30c).

In addition there are representative markers of the Natchitoches 2 subset in the C.B. Moore collection from the Boytt's Field site in south-central Arkansas. The markers at Boytt's Field are Keno Trailed, *vars. Styx* and *unspecified.* This site also yielded evidence of the Morehouse 5 subset, most notably a Mound Tract Incised and Brushed, *var. Mound Tract* jar and a bottle of the same variety (Moore 1909: Figures 83, 88). A Hudson Engraved, *var. unspecified* bottle (Moore 1909:84) is further proof of the connections to Glendora I. Limited evidence of the Morehouse 5 and 6 subsets was also found at the Gee's Landing and Gordon sites (White 1970: Figure 13b-c, h, 1987: Figures 17, 28-30).

Burials in the Caney Bayou phase were found in cemeteries and also as mound-top interments (Kelley 1984:45; Rolingson and Schambach 1981:193-198; Weinstein and Kelley 1984:433). The burial program apparently included charnel house structures, and burials were found most commonly in the bundle form, though extended burials were common at Boytt's Field (Moore 1909:83), Gee's Landing, and Gordon (White 1971, 1987). Moore recovered a number of intact burials at Boytt's Field, which, when examined by Hrdlička, revealed the same patterns of cranial deformation as had the Glendora I skeletons at the Ward site in Louisiana (Hrdlička 1909:176-177, 185).

The Caney Bayou phase also shares a dispersed settlement pattern similar to that noted for Glendora I. Occupations are generally quite small and were often located on earlier middens (Rolingson and Schambach 1981:193-195). Cemeteries contained few burials and possibly represented family mortuaries. No major centers are known in the Caney Bayou phase, although earlier mound sites were utilized for habitation and burial (Weinstein and Kelley 1984:433). It is really no surprise that the Caney Bayou phase had so much in common with Glendora I, given their geographic proximity.

Red River Region

There are two phases in the Red River region of southwest Arkansas and northwest Louisiana, Belcher III-IV and Chakanina, that can be compared to both Glendora subphases. The former is protohistoric and the latter protohistoric and early historic in age. The Belcher III-IV phase was defined at the Belcher site by Webb (1959) and is related to the Glendora I subphase via the Red River I and Morehouse 5 and 8 subsets. The Chakanina phase (Trubowitz 1984) is temporally equivalent to the Glendora II subphase and is identified in Glendora II by the Red River 2 and Natchitoches 1 and 2 subsets.

Specific Red River 1 markers in the Glendora I subphase include Belcher Ridged, *var. Belcher Ridged,* Glassell Engraved, and Hodges Engraved without shell tempering. Elements of the Morehouse 5 subset include Hudson Engraved and Mound Tract Incised and Brushed (Moore 1912: Figure 81; Webb 1959: Figure 109a, e-j, n). The Morehouse 8 subset is found intact in the Belcher phase, with both Barton Incised, *vars. Filhiol* and *Rock Row* having been identified in appropriate contexts (Webb 1959: Figures 53d, 121a-c). The Morehouse 8 subet also has parallels to the Red River 2 subset, which suggests a certain degree of continuity across both space and time.

It would appear that the Belcher phase society was one of the major groups interacting with the Glendora I peoples from the west. Overland routes from Red River to the Pine Bluff region are known to have crossed the Ouachita at the confluence of Bayou Bartholomew, so it is no surprise to find evidence of strong contacts (see below). The Chakanina phase was an important element in the trade between the Caddo and the Ouachita in the early historic period. The markers of contact between Glendora II and Chakanina are found in the

Red River 2 set, most notably varieties of Foster Trailed-Incised, Hodges Engraved, and Karnack Brushed-Incised.

The Natchitoches 1 and 2 subsets are manifest by the presence of Keno Trailed and Natchitoches Engraved. The ties with Natchitoches 1 are particularly evident at the Cedar Grove site, where Schambach and Miller have identified all of the members of the subset, including Natchitoches Engraved, *var. Gopher* (1984: 124, Figure 11-27a). Natchitoches 2 is rare in the Great Bend region, being represented only at the Cedar Grove site by a single specimen of Keno Trailed, *var. Glendora* (Schambach and Miller 1984: Figure 11-18a-b). This vessel is, however, one of the few examples of the *Glendora* variety identified outside of the Glendora phase (see also Webb 1945: pl. 11, no. 4, pl. 14, no. 1). Although no early historic trade goods were found at the Cedar Grove site where the Chakanina phase was defined (Trubowitz 1984), there is little doubt that the phase belongs in the Caddo V period and is contemporary with Glendora II.

DISCUSSION

The Glendora phase has been defined in the past as a Caddoan culture based on the work of C.B. Moore. Analysis of ceramics, settlement patterns, and burial practices from Glendora phase sites has proved that this is an incorrect concept that must be discarded if the culture history of the lower Ouachita Basin is to be understood properly. Although Caddoan ceramics are present at these sites, they do not form a majority of the assemblage, and more importantly, other traits, such as burials and mortuary practices, do not appear to be Caddoan in form or nature. This paper has focused on redefining the Glendora phase and establishing its temporal and spatial boundaries and external cultural contacts. It is only in this context that we can understand the culture dynamics of the region during the late prehistoric through early historic periods.

I have proposed that the Glendora phase be subdivided into two subphases that have a strong degree of continuity. The Glendora I subphase is argued to be protohistoric in age, while Glendora II is an early historic phase. The dating of these subphases cannot be based on internal stratigraphy, as none currently exists. Rather, correlations to known ceramic complexes with reasonably well ordered stratigraphic sequences have been employed. The temporal and spatial information provided in the preceding section can be used to create an outline of the Glendora phase settlement system in northeast Louisiana. There are, however, some limitations because of inadequate data. Because of the way the data have been recorded, most of what can be said concerning settlement patterns must really be understood as representing the distributions of mortuaries, which are presumed to reflect nearby habitation areas. Nevertheless, it is possible to begin to understand the Glendora phase settlement system as it changed through time.

The Glendora phase settlement pattern appears to be different from that of the Jordan phase. Whereas the Jordan site was the dominant center of the Jordan phase (Kidder 1988), there does not seem to be any one major site occupation dating to the Glendora phase. Furthermore, the Glendora phase peoples were not building mounds, although on occasion they utilized extant mound structures for burial purposes. Instead, the Glendora phase pattern appears to be one of dispersed occupations along Bayou Bartholomew and Ouachita River. The one exception is the Ragland site, which is found at the end of a small ridge overlooking the Lafourche swamps. The spatial extent of the Glendora phase is considerable, but if the Pritchard Landing site is removed from the phase, it would be significantly reduced.

Sites seem to have been fairly small during the Glendora I subphase, and during this period they were widely scattered. It is impossible to judge how large the Keno or Glendora sites might have been during Glendora I, but they do not appear to have been significantly bigger than contemporary sites. During the Glendora I subphase, site populations were probably small, if the burials are any reflection of the population size. Since we have no evidence concerning the subsistence system, it is only possible to speculate that the majority of Glendora I subphase sites were agricultural hamlets. Burials were interred at the small sites, and the size of the cemeteries is probably an indication that they were family or lineage mortuaries. The burial practices noted by Moore and others suggest that some kind of charnel house structure was probably associated with Glendora phase sites, although no such structure has yet been identified.

By the early historic Glendora II subphase a dramatic shift in the settlement pattern is evident. In contrast to Glendora I, the occupations are spatially concentrated, and, importantly, the number of sites is reduced to only three or four. It seems possible that the Glendora II settlement pattern is a reflection of village fusion, presumably in the face of disease and population loss. The Glendora II subphase sites were also more strategically located than they were during Glendora I. The four sites (Glendora, Keno, Moon Lake, and Pargoud) are all located at or around the junction of the Ouachita, Bayou Bartholomew, and Bayou De Siard. This area was also the location where the overland trail from Natchitoches crossed the Ouachita (Lafon 1806; see also Webb and Gregory 1978). From this strategic river junction, the Glendora II phase peoples could command the overland trade with the Caddo and, in part, the trade up Bayou Bartholomew to the Arkansas River. In addition the Natchez region was accessible via the Ouachita.

As it is now understood, the Glendora phase must be viewed as a cultural manifestation indigenous to the Ouachita River. It has its origins in the late prehistoric Kinnaird phase, but was also heavily influenced by the nearby Jordan phase. The Glendora I and II subphases have some important continuities, but it is also evident that the onset of early historic European contact brought about significant changes in the nature of the phase. The Glendora II subphase seems to have drawn on a diverse group of peoples from surrounding regions that were fused together to create a multiethnic society.

CONCLUSIONS

By roughly 1550 the Ouachita, Boeuf, and Tensas basins, like much of the Southeast, were feeling the impact of European-introduced diseases and sociopolitical change. Populations in the Tensas Basin declined dramatically, and the entire settlement system seems to have collapsed. Between roughly 1550 and 1680, there was a brief population concentration at the Jordan site, which I believe was a result of the migration of peoples westward from the Tensas (Kidder 1988, n.d.). The reason for this migration may have been the introduction of diseases and socio-political change caused by the physical presence of Europeans. Although the Glendora I phase on the Ouachita was contemporary with the Jordan phase, there were few similarities between the complexly organized Jordan phase peoples and the apparently simple societies on the Ouachita. Apparently the Jordan phase was dominating the political and economic resources of the entire region.

But there was to be another dramatic change in the culture dynamics sometime between 1682 and roughly 1700. During this interval, the Jordan phase society seems to have collapsed, and there were renewed developments in the lower Ouachita Basin, notably around the junction of Bayou Bartholomew. As Europeans changed the economic and political structure of the cultures of the area, it was the Indians who could most rapidly and flexibly adapt who would survive. The movement of peoples to the confluence of the Ouachita and Bayou Bartholomew was an apparent attempt by the natives to respond to the altered economic conditions of the early historic period, but it was both geographically inappropriate and probably too late as well. Apparently the impetus for these developments was the presence of Europeans and/or their trade goods. Located on or near Bayou Bartholomew, early historic Glendora II phase Indians could pursue both the overland trade to the Caddo and the routes northeast to the Arkansas River and southeast to the Mississippi. The meager ethnohistorical evidence suggests that these peoples were probably the ancestors of the Koroa and possibly other undocumented Indian groups from the Felsenthal who had formerly been identified with the Caney Bayou phase (Kidder 1988).

By the early 1720s, or probably earlier, the lower Ouachita was abandoned by native groups. The exact reasons are unclear, but appear to be due to shifting native trade patterns, the European presence on both the Red and Mississippi rivers, and the lack of French goods reaching the Ouachita. By 1706-1707 the Tunica Indians, now located near the mouth of the Red River (Brain 1988), were commanding the Caddoan trade and were presumably occupying the "middleman" position once dominated by the Glendora phase peoples.

The French directly altered the Caddo trade as they sought to establish a border defense against the Spanish. The need to dominate the Red River led to a policy whereby resources were directed to the Indians of that region. As this pattern evolved, the Ouachita River was ignored because it was peripheral to French interests. The French were better served by building native barriers against the English on the east and Spanish on the west. Thus the natives were encouraged to settle on the Yazoo River and around Natchitoches, in part to provide a defense against hostile European powers and their Indian allies.

As the Ouachita River was ignored, or virtually so, the natives were unable to procure European goods that were becoming increasingly important to maintaining economic and social status and prestige. The meager quantities of European goods in the Glendora II subphase sites is mute testimony to the inability of this region to command European tribute and/or trade (cf. Brain 1979, 1988). Furthermore, isolation on the Ouachita would have meant that the Indians of the region would not have had access to French guns and ammunition, both for hunting and to wage war and defend themselves. Technological inferiority placed the native populations of the lower Ouachita Basin in an untenable position by the early eighteenth century. Incursions at this time by well-armed Indian groups from the east, particularly the Chickasaw and Choctaw, were blamed for the decimation of native communities in northeast Louisiana (Le Page Du Pratz 1756 [II]:243). Inevitably this "marginalization" of the Ouachita region resulted in its nearly complete depopulation by the early-to-middle eighteenth century (Anonymous n.d. a, n.d. b, n.d. c). Given the economic and political dynamics of Indian-European interaction in the Lower Mississippi Valley, the Ouachita region simply ceased to have any value for either society.

When the lower Ouachita was once again resettled by Indian groups, it was a response to the presence of Europeans at the Poste du Ouachita. However, these natives were not indigenous to the region, nor were they permanent residents. The Poste du Ouachita provided the economic incentive for the reoccupation of the Lower Ouachita, but even then it was a brief phenomenon, lasting only to the 1830s. But by then the former inhabitants of the region existed in name only, for they had been killed off or had fused with other groups. The epitaph for these natives was provided by Filhiol, commander of the Poste du Ouachita, who remarked in 1786 that

> All the remains that are exposed everywhere . . . proclaim that the nation which inhabited it [the Ouachita River] in former times must have been numerous. What became of it is not known; the oldest people of the place do not recall having seen a single one of them, and if some travelers . . . did not vouch for having seen 5 or 6 . . . bearing the name of Ouachitas,

one would doubt that a nation that was so called had existed . . . (Dickinson 1990: 12).

REFERENCES

Anonymous
n.d.a Map of the Lower Ouachita River [ca. 1720-1725]. Bibliothèque Nationale, Paris.
n.d.b Idée du Ouachita [ca. 1786]. Sandel Library, Northeast Louisiana University, Monroe.
n.d.c Map of the Lower Ouachita River [ca. 1720s]. Library of Congress, Washington.

Belmont, John S.
1985 A Reconnaissance of the Boeuf Basin, Louisiana. *Louisiana Archaeology* 10:271-284.

Brain, Jeffrey P.
1979 *Tunica Treasure*. Papers of the Peabody Museum of Archaeology and Ethnology 71. Harvard University, Cambridge.
1988 *Tunica Archaeology*. Papers of the Peabody Museum 78. Harvard University, Cambridge.

Brain, Jeffrey P., Ian W. Brown, and Vincas P. Steponaitis
n.d. Archaeology of the Natchez Bluffs. Ms., Lower Mississippi Survey, Peabody Museum, Harvard University, Cambridge.

Brown, Ian W.
1985 *Natchez Indian Archaeology: Culture Change and Stability in the Lower Mississippi Valley*. Mississippi Department of Archives and History, Archaeological Report 15. Jackson.

Clancy, Phyllis M.
1985 The Carden's Bottom Puzzle Elucidated. M.A. thesis, Department of Anthropology, University of Arkansas, Fayetteville.

Cotter, John L.
1951 Stratigraphic and Area Tests at the Emerald and Anna Mound Sites. *American Antiquity* 17(1):18-32.

Davis, E. Mott
1961a The Caddoan Area: An Introduction to the Symposium. *Bulletin of the Texas Archeological Society* 31:3-10.
1961b Proceedings of the Fourth Conference on Caddoan Archeology. *Bulletin of the Texas Archeological Society* 30:2-33.
1961c Proceedings of the Fifth Conference on Caddoan Archeology. *Bulletin of the Texas Archeological Society* 31:77-143.

Dickinson, Samuel D.
1990 An Early View of the Ouachita Region. *The Old Time Chronicle* 3(7):12-18.

Ford, James A.
1936 *Analysis of Indian Village Site Collections From Louisiana and Mississippi*. Louisiana Department of Conservation, Anthropological Study 2. New Orleans.
1961 *Menard Site: the Quapaw Village of Osotouy on the Arkansas River*. Anthropological Papers of the American Museum of Natural History 48(2):133-191.

Fuller, Richard S., Jr.
1985 Archaeological Survey of the Southern Boeuf Basin, Louisiana: 1984. *Boeuf Basin Research Notes* 2, Lower Mississippi Survey, Peabody Museum, Harvard University, Cambridge.

Gibson, Jon L.
1985a Ouachita Prehistory. *Louisiana Archaeology* 10:319-335.
1985b Mounds on the Ouachita. *Louisiana Archaeology* 10:171-270.
1985c An Evaluatory History of Archaeology in the Ouachita Valley of Louisiana. *Louisiana Archaeology* 10:25-101.

Gregory, Hiram F. Jr.,
1973 Eighteenth Century Caddoan Archaeology: A Study in Models and Interpretations. Ph.D. dissertation, Department of Anthropology, Southern Methodist University, Dallas. University Microfilms, Ann Arbor.

Haag, William G.
1971 *Louisiana in North American Prehistory*. Melanges 1. Museum of Geoscience, Louisiana State University, Baton Rouge.

Hally, David J.
1972 The Plaquemine and Mississippian Occupations of the Upper Tensas Basin, Louisiana. Ph.D. dissertation, Department of Anthropology, Harvard University, Cambridge.

Hoffman, Michael P.
1975-1977 The Kinkead-Mainard Site, 3PU2: A Late Prehistoric Quapaw Phase Site Near Little Rock, Arkansas. *Arkansas Archeologist* 16-18:1-41.

House, John H.
1986 The Mississippian Sequence in the Menard Locality, Eastern Arkansas. Paper presented at the Southeastern Archaeological Conference, Birmingham.

Hrdlička, Aleš
1909 Report on an Additional Collection of Skeletal Remains from Arkansas and Louisiana. *Journal of the Academy of Natural Sciences of Philadelphia* 14:171-249.

Jeter, Marvin D.
1986 Tunicans West of the Mississippi: A Summary of Early Historic and Archaeological Evidence. In *The Protohistoric Period in the Mid-South: 1500-1700*, edited by David H. Dye and Ronald C. Brister, 38-65. Mississippi Department of Archives and History, Archaeological Report 18. Jackson.

Jeter, Marvin D., David B. Kelley, and George P. Kelley
1979 The Kelley-Grimes Site: a Mississippi Period Burial Mound, Southeast Arkansas, Excavated in 1936. *Arkansas Archeologist* 20:1-52.

Jones, Reca B.
1985 Archaeological Investigations in the Ouachita River Valley, Bayou Bartholomew to Riverton, Louisiana. *Louisiana Archaeology* 10:103-169.

Kelley, David B.

1984 Aboriginal Occupation of the Upper Felsenthal Region. In *Archaeology and Paleogeography of the Upper Felsenthal Region: Cultural Resources Investigations in the Calion Navigation Pool, South-central Arkansas*, by R.A. Weinstein and D.B. Kelley, 25-49. Coastal Environments, Inc., Baton Rouge.

Kidder, Tristram R.

1986 *Final Report on Archaeological Test Excavations in the Central Boeuf Basin, Louisiana, 1985*. Lower Mississippi Survey Bulletin 10. Lower Mississippi Survey, Peabody Museum, Harvard University, Cambridge.

1988 Protohistoric and Early Historic Culture Dynamics in Southeast Arkansas and Northeast Louisiana, A.D. 1542-1730. Ph.D. dissertation, Department of Anthropology, Harvard University. University Microfilms, Ann Arbor.

1990a The Ouachita Indians of Louisiana: An Ethnohistorical and Archaeological Investigation. *Louisiana Archaeology* 12:179-202.

1990b Ceramic Chronology and Culture History of the Southern Ouachita River Basin: Coles Creek to the Early Historic Period. *Midcontinental Journal of Archaeology* 15:51-99.

n.d. Excavations at the Jordan Site (16MO1), Morehouse Parish, Louisiana. *Southeastern Archaeology*.

Krieger, Alex D.

1943 Archaeological Horizons in the Caddo Area. *3rd Mesa Redonda*: 154-156. Sociedad Mexicana de Antropología, México D.F.

Lafon, B.

1806 *Carte Generale du Territoire D'Orleans Comprenant Aussi la Floride Occidentale et une Portion du Territoire Du Mississipi*. Lower Mississippi Survey Map Library, Peabody Museum, Harvard University, Cambridge.

Le Page du Pratz, Antoine

1758 *Histoire de la Louisiane* (3 volumes). De Bure, La Veuve Delaguette, Lambert, Paris.

Moore, Clarence B.

1908 Certain Mounds of Arkansas and of Mississippi. *Journal of the Academy of Natural Sciences of Philadelphia* 13:480-605.

1909 The Antiquities of the Ouachita Valley. *Journal of the Academy of Natural Sciences of Philadelphia* 14:1-170.

1912 Some Aboriginal Sites on Red River. *Journal of the Academy of Natural Sciences of Philadelphia* 14:483-644.

Neitzel, Robert S.

1965 Archaeology of the Fatherland Site: The Grand Village of the Natchez. *Anthropological Papers of the American Museum of Natural History* 51(1):3-108.

Orr, Kenneth G.

1941 The Eufala Mound: Contributions to the Spiro Focus. *Oklahoma Prehistorian* 4(1):2-15.

1952 Survey of Caddoan Area Archeology. In *Archeology of Eastern United States*, edited by James B. Griffin, 239-255. University of Chicago Press, Chicago.

Phillips, Philip

1970 *Archaeological Survey in the Lower Yazoo Basin, Mississippi, 1949-1955*. Papers of the Peabody Museum of Archaeology and Ethnology 60. Harvard University, Cambridge.

Price, G.R. Dennis

1979 Cultural Resources Survey Report: Appendix to Design Memorandum No. 58. Report submitted to the U.S. Army Corps of Engineers, Vicksburg District, Vicksburg.

Price, G.R. Dennis, and Lorraine Heartfield

1977 Archeological Test Excavation of Site 16OU15 Located on the Monroe to Sandy Bayou Levee Ouachita River Levees, Ouachita Parish, Louisiana. Report submitted to the U.S. Army Corps of Engineers, Vicksburg District, Vicksburg.

Rolingson, Martha A., and Frank F. Schambach

1981 *The Shallow Lake site (3UN 9/52) and its Place in Regional Prehistory*. Arkansas Archeological Survey, Research Series 12, Fayetteville.

Schambach, Frank F., and John E. Miller

1984 A Description and Analysis of the Ceramics. In *Cedar Grove: An Interdisciplinary Investigation of a Late Caddo Farmstead in the Red River Valley*, edited by N. Trubowitz, 109-170. Arkansas Archeological Survey, Research Series 23. Fayetteville.

Story, Dee Ann, Byron Barber, Estalee Barber, Evelyn Cobb, Herschel Cobb, Robert Coleman, Kathleen Gilmore, R. King Harris, and Nora Hoffrichter

1967 Pottery Vessels. In *The Gibert Site: A Norteño Focus Site in Northeastern Texas*, edited by E.B. Jelks, 112-187. Bulletin of the Texas Archeological Society 37.

Suhm, Dee Ann, Alex D. Krieger, and Edward B. Jelks

1954 *An Introductory Handbook of Texas Archeology*. Bulletin of the Texas Archeological Society 25.

Trubowitz, Neal L., (editor

1984 *Cedar Grove: An Interdisciplinary Investigation of a Late Caddo Farmstead in the Red River Valley*. Arkansas Archeological Survey Research Series 23. Fayetteville.

Walker, Winslow M.

1935 *A Caddo Burial Site in Natchitoches, Louisiana*. Smithsonian Miscellaneous Collections 94(14). Washington, D.C.

Webb, Clarence H.

1945 A Second Historic Caddo Site at Natchitoches, Louisiana. *Bulletin of the Texas Archeological and Paleontological Society* 16:52-83.

1959 *The Belcher Mound: A Stratified Caddoan Site in Caddo Parish, Louisiana*. Society for American Archaeology Memoir 16. Salt Lake City.

Webb, Clarence H., and Hiram F. Gregory
1978 *The Caddo Indians of Louisiana.* Louisiana Archaeological Survey and Antiquities Commission, Anthropological Study 2. Baton Rouge.

Weinstein, Richard A., and David B. Kelley
1984 *Archaeology and Paleogeography of the Upper Felsenthal Region: Cultural Resources Investigations in the Calion Navigation Pool, South-central Arkansas.* Coastal Environments, Inc., Baton Rouge.

White, Patsy K.
1970 Investigation of the Cemetery at the Gee's Landing Site, 3DR17. *Arkansas Archeologist* 11(1):1-20.
1987 The Gordon Site: A Middle Coles Creek to Late Mississippi Period Occupation in Ashley County, Southeast Arkansas. *Arkansas Archeologist* 25-26:1-51.

Williams, Stephen
1961 A Proposal for the Revision of Terminology for the Historic Phases (Foci) in the Caddo Area. In *Proceedings of the Fifth Conference on Caddoan Archeology,* edited by E.M. Davis, 122-125. Bulletin of the Texas Archeological Society 31.
1964 The Aboriginal Location of the Kadohadacho and Related Tribes. In *Explorations in Cultural Anthropology,* edited by W.H. Goodenough, 545-570. McGraw Hill, New York.
1967 On the Location of the Historic Taensa Villages. *Conference on Historic Site Archaeology Papers 1965-1966* 1:3-13.
1978 Armorel: A Very Late Phase in the Lower Mississippi Valley. *Southeastern Archaeological Conference Bulletin* 22:105-110.

15 The Depopulation and Abandonment of Northeastern Arkansas in the Protohistoric Period

Michael P. Hoffman
University of Arkansas
Fayetteville, Arkansas

INTRODUCTION

One of the most dramatic changes in Southeastern Indian history took place in northeastern Arkansas between 1543 and 1673. A large area of the Mississippi Alluvial Valley north of the mouth of the White River, which had been densely inhabited by people living in complex chiefdoms in A.D. 1542-43, had no permanent inhabitants in 1673. Speculation about causes for this great change and what happened to the Native Americans of northeastern Arkansas has intrigued scholars for many years, but increasingly in the last twenty years, data have become available from several fields of research that allow speculations to be more informed, if no less contradictory. Now bioarchaeologists, linguists, and paleoclimatologists, along with traditional archaeologists, historians, and ethnologists, have become involved in the controversy. To honor Professor Stephen Williams, I here review current explanations of the great northeastern Arkansas depopulation mystery.

THE VACANT QUARTER AND ARMOREL CONCEPTS

Stephen Williams has contributed two concepts important for understanding changes in the northern portion of the Lower Mississippi Alluvial Valley in the A.D. 1300-1700 period, those of the Vacant Quarter and the Armorel phase. (Williams 1980, 1990).

The Vacant Quarter refers to a large area of land "centering on the mouth of the Ohio River, on the Mississippi from Cahokia, Illinois, on the north to New Madrid, Missouri on the south, up the Ohio River to Evansville, Indiana, and up the Cumberland River to the Nashville Basin" (Williams 1990:173) which was virtually devoid of permanent inhabitants by A.D. 1550 even though it had been the location of many town centers of Mississippian culture previously (Figure 15.1). Williams envisions the progressive abandonment of this territory beginning in the north around Cahokia and shifting to the south (1990:177). With the exception of R. Barry Lewis (1990), Williams' Vacant Quarter Hypothesis has become widely accepted by researchers who study prehistory in the region (Smith 1986; Morse and Morse 1983). Instead of a catastrophe or catastrophes creating population decimation, Williams (1990:177) suggests that Indian peoples relocated to new areas. These areas have become archaeological "hot spots" for sixteenth-century occupational evidence. These "hot spots," which include northeastern Arkansas, show evidence of population increase and cultural florescence in the latter fifteenth and early sixteenth centuries.

The Vacant Quarter Hypothesis provides useful background information for explanations of the depopulation of northeastern Arkansas in the six-

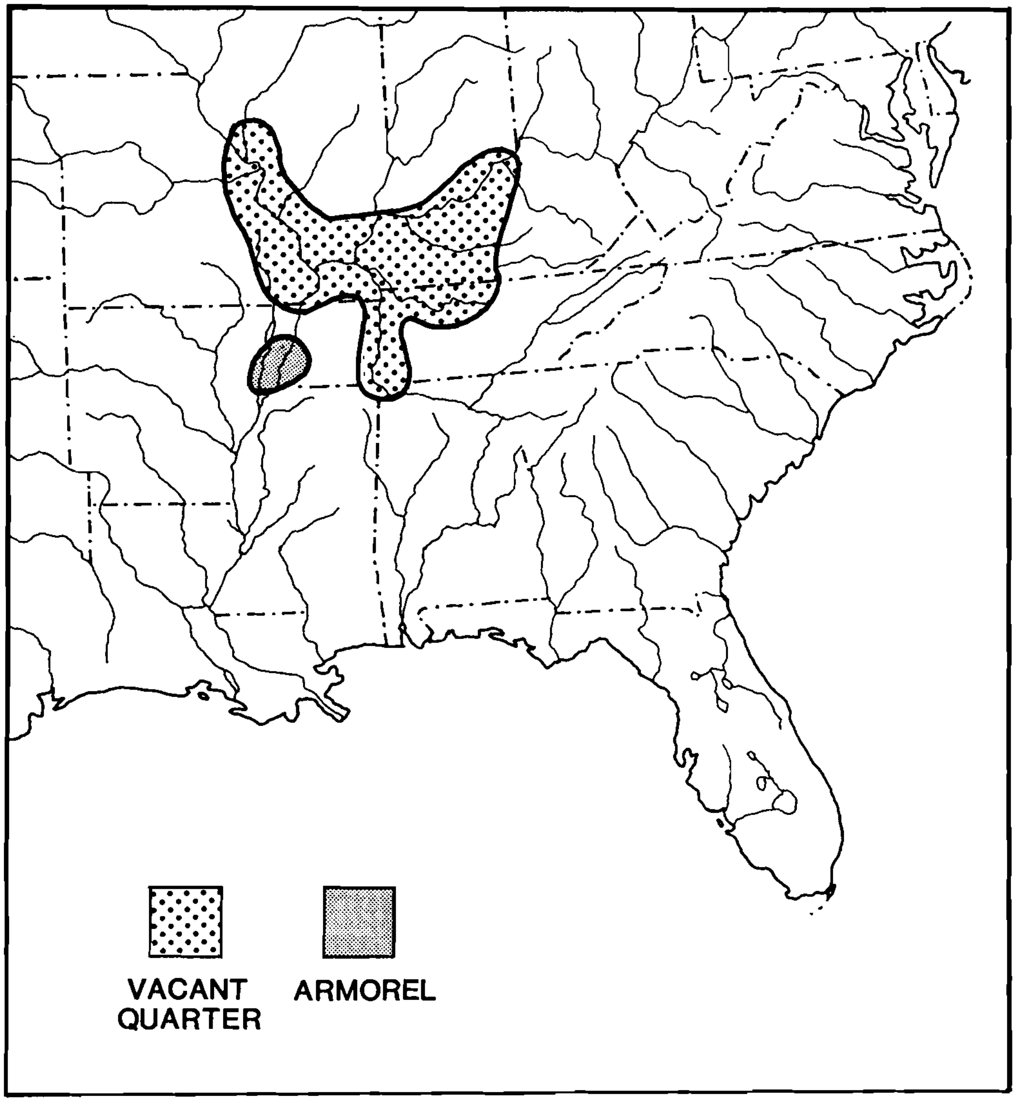


Figure 15.1. The Vacant Quarter and Armorel Phase ca. A.D. 1500. From Williams 1990:174.

teenth century. Depopulation is shown not to be unique to the protohistoric or historic periods. The changes in northeastern Arkansas can also be compared to settlement changes that began several hundred years earlier in the northern portion of the Mississippi and Lower Ohio alluvial valleys. Explanations for these earlier changes clearly cannot be related to European contact. After briefly considering and rejecting catastrophic single causes such as disease, drought, and earthquake, Williams (1990:176) suggests that political instability associated with large populations was a reason for Vacant Quarter abandonment.

There are a number of late Mississippian phases in northeastern Arkansas that persisted at least until the middle of the sixteenth century (Figure 15.2).

These include the Nodena phase along the Mississippi River above Memphis to just above the Missouri line (D. Morse 1990), the Parkin phase along the Little and St. Francis Rivers (P. Morse 1990), the Walls-Belle Meade phase in the Memphis locality (G. Smith 1990), the Kent phase of the lower St. Francis River drainage (House 1987), and the poorly known Old Town phase on the Mississippi River below the mouth of the St. Francis River (Figure 15.2; Morse and Morse 1983:297-298). All of these phases were characterized by maize dependency, nucleated fortified town-mound centers, and a complex chiefdom level of political organization (Morse and Morse 1983:280). Both the Nodena and Parkin phases crystallized in the latter part of the fourteenth century about the time of the beginning of abandonment of the Cairo Lowlands region. Morse and Morse (1983:282-83) consider it likely that people from the north were absorbed into these Arkansas phases.

Professional archaeologists in Arkansas currently believe that the northern De Soto expedition route postulated by Charles Hudson and Dan Morse (Hudson 1985) fits the archaeological, linguistic, geographical, and ethnological information better than the more southern Arkansas routes favored by John Swanton (1939) and Jeffrey Brain (1985). If that is the case, the Nodena phase can be equated with the chiefdom of Pacaha and that of Parkin with Casqui, and the De Soto narrative descriptions of populous, flourishing, warring chiefdoms apply to them. Whether the northern or southern De Soto routes are more accurate is not really that important; certainly the northeastern Arkansas town sites were flourishing in the first half of the sixteenth century. One hundred thirty years later, with the next European accounts, these chiefdoms were gone, and the area of northeastern Arkansas was devoid of permanent human habitation. Thus the area was abandoned sometime between A.D. 1543 and 1673.

The archaeological recognition of the post-A.D. 1543 period in northeastern Arkansas has proved to be very difficult. A few European trade goods with poor contexts have been reported at sites categorized as Nodena, Parkin, Kent, and Walls phases by Morse and Morse (1983:284-98). These include some objects pretty clearly of Spanish manufacture, such as Clarksdale bells and chevron beads from the Parkin and Campbell sites (Price and Price 1990:68; D. Morse 1990:91). Other metal artifacts and blue glass beads found at several sites may belong to the sixteenth century and be of Spanish origin or maybe French trade goods dating as late as the mid-seventeenth century. Unfortunately, these European trade items indicate only that use of several town centers continued for some time after the initial European contact in the area.

Stephen Williams (1980) focused on the terminal Mississippian occupation of northeastern Arkansas with the definition of the Armorel phase, which he thinks dates to A.D. 1500-1700. Central to the Armorel concept are distinctive ceramics and other artifacts shared by components at various northeastern Arkansas sites (and the Missouri Campbell site) that have been classified as belonging to late Nodena, Parkin, Walls, and Kent phases by the Morses and other researchers. These distinctive ceramics and other artifacts represent a "washing over" of earlier phase differences and in Williams' mind a distinctive new phase called Armorel (Williams 1980:105). The excavations done by amateur archaeologist Leo Anderson at the Campbell site (Chapman and Anderson 1955) were regarded by Williams as having produced the best single published description of the unit. The ceramic features selected by Williams (1980:107) as distinctive of the terminal Mississippian Armorel phase, including applique decoration, arcaded handles, and distinctive shapes such as teapots and stirrup-mouth vessels, have been sustained in further research. The lithic complex postulated to be characteristic of the Armorel phase includes willow-leaf shaped Nodena points, small chipped "pipe drills," small snub nose end scrapers, and catlinite disc pipes (1980:107). With the exception of the catlinite pipes, these lithic items are also present in seventeenth-century Arkan-

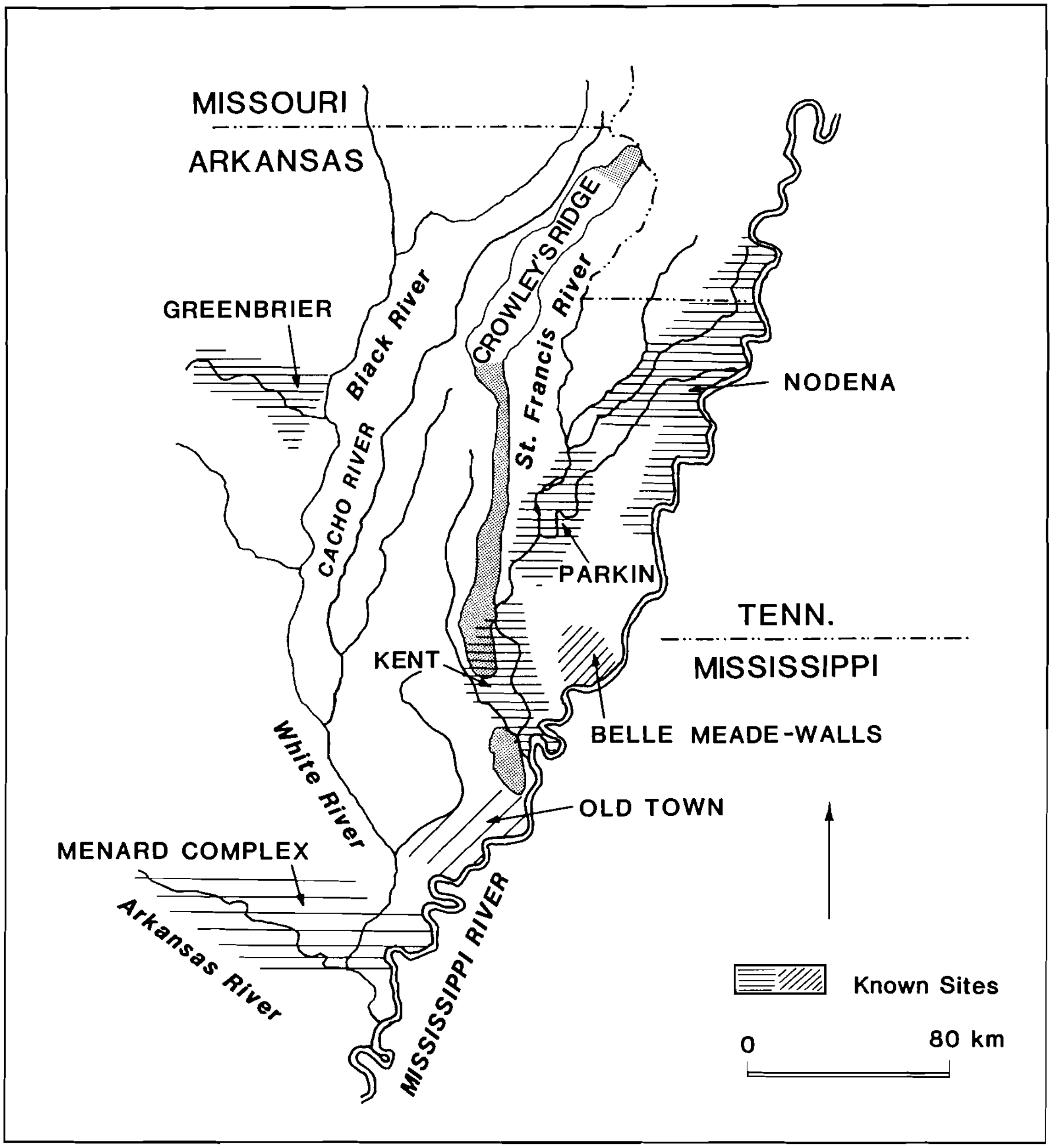


Figure 15.2. Sixteenth-century archaeological phases in northeast Arkansas. From D. Morse 1991:44.

sas River sites (Hoffman 1977). The chipped stone items form the nucleus of the Oliver lithic complex believed by Brain (1988:262) to be indicative of Siouan Quapaw hunters moving south, first into northeastern Arkansas, then farther south to northwestern Mississippi.

Arkansas archaeologists have largely ignored the Armorel phase postulate without stating their reasons. Perspectives in Cambridge and Arkansas are different. Williams looks at the terminal Mississippian artifacts and sees both distinctive stylistic differences from earlier phases and synchronic similarities from locality to locality, while Morse and Morse's (1983) perspectives relate to the undeniably separate chiefdoms described by the De Soto expedition and their archaeological phase correlates. The Armorel phase is used by several archaeologists who do research in the Lower Mississippi Valley (Price and Price 1990; Brain 1988). I find it useful to distinguish a terminal Mississippian artifact complex that ended after the De Soto incursion, but am wary of using the term if it implies fusion or amalgamation of the people of previously separate chiefdoms. We don't yet know that to be the case.

DISEASE AND DEPOPULATION

Most archaeologists interested in the region have attributed the depopulation of northeastern Arkansas to epidemics of diseases introduced initially by Europeans and Africans (P. Morse 1990:133; G. Smith 1990:168; Williams 1990:180; Griffin 1990:14; Morse and Morse 1983:320; Brain 1980:270). Until recently, however, no researchers seriously attempted to test the hypothesis that European-introduced epidemic disease was a primary cause of the protohistoric depopulation of northeastern Arkansas. In the early 1980s George Milner (1980) and Henry Dobyns (1983) provided behavioral and biological models addressing the impact of disease on native peoples that are archaeologically relevant. Two recent studies address the timing of northeastern Arkansas and general Lower Mississippi Alluvial Valley disease epidemics (Ramenofsky 1987; Burnett and Murray 1991a) and arrive at contradictory conclusions about whether disease depopulation was significant in the sixteenth-century northeastern Arkansas area. Ramenofsky, an archaeologist, did not use bioarchaeological data, but instead relied on settlement count information in order to contrast settlement numbers between A.D. 1400 and 1540 (period I), between 1541 and 1699 (period II) and between A.D. 1700 and 1764 (period III; 1987:54). She found a dramatic decrease between period I and II that she interpreted as primarily caused by epidemic disease:

> The independent evaluation of both archaeological and historical records supported the second hypothesis: Catastrophic population loss began in the sixteenth century. Based on this evidence, several conclusions are warranted: The De Soto entrada was not a "false dawn" in the Lower Mississippi Valley, but marked the beginning of both European contact and the onset of aboriginal population loss; the beginning of history and aboriginal population change were simultaneous events (Ramenofsky 1987:71).

Another finding that complicates the significance of her study was that it showed a progressive southward trend in area depopulation in the Lower Mississippi Valley, a process that Williams (1990) has demonstrated began in late Mississippi (prehistoric) times. On the other hand Ramenofsky's work indicates that population decrease as measured by settlement counts in the A.D. 1541-1699 period occurred all along the Lower Mississippi Alluvial Valley and thus was not limited to northeastern Arkansas. In other words, the causes of the decline were widespread.

Burnett and Murray (1991a), both bioarchaeologists, emphasize skeletal data along with historical information to establish one important point: the De Soto expedition did not bring smallpox or other epidemic disease to northeastern Arkansas. It was not an active disease among the members of the expedition (Burnett and Murray 1991a:3), and, if it had been carried in *a dried form* to northeastern Arkansas, the Spanish would have chronicled an epidemic in the two years they were in the area. The expedition was in the northeastern Arkansas town of Pacaha for 40 days in close contact with its inhabitants: smallpox symptoms normally appear within 24 hours of exposure. There were no epidemic disease outbreaks described by the Spanish while in Arkansas (Burnett and Murray 1991a:20).

Burnett and Murray compare skeletal populations from the (predominantly) seventeenth-century Menard Complex (formerly Quapaw phase) of the lower Arkansas River with those of the Nodena and Parkin phases to assess evidence for epidemic disease. The Nodena and Parkin phase samples were assumed to be representative of sixteenth-century post-De Soto populations in the area. Both the northeast Arkansas and Menard Complex samples were compared to a major, clearly late prehistoric population from Moundville. The demographic profile and stress indicators of the Menard Complex population were consistent with epidemic and associated stress mortality (1991a:17). The Nodena and Parkin phase samples contrasted sharply with those of the Menard complex and were similar to those of the relatively healthy Moundville prehistoric Mississippian population. The mortality of young women particularly contrasted between the two Arkansas areas (Burnett and Murray 1991a:9-10). Burnett and Murray interpret these data to mean that depopulation through epidemic disease was prevalent along the lower Arkansas River in the seventeenth century but was not present during the sixteenth century in northeastern Arkansas. Thus, in their view epidemic disease was not a causal factor of the protohistoric abandonment of northeastern Arkansas.

A major problem with Burnett and Murray's stimulating paper is they did not isolate a post-1540 northeastern Arkansas skeletal sample. As defined by Morse and Morse, both the Nodena and Parkin phases began around A.D. 1400 (P. Morse 1990:125; D. Morse 1990:76-77). No skeleton analyzed from northeastern Arkansas had a European trade item with it, thus no part of the sample can be confidently placed after the De Soto entrada. This imprecision in dating is not the fault of Burnett and Murray; they are using the chronological categories of the local archaeologists. However, if William's Armorel phase terminal Mississippian distinction were used by local archaeologists, perhaps a post-De Soto sample could be isolated.

Another criticism of the kind of paleo-demographic study that Burnett and Murray attempt is voiced by Ramenofsky (1987:23). In a number of historical instances both inside and outside North America, social disarray during epidemics was so great that the dead weren't buried. Boldly, Ramenofsky states (1987:24) "If the North American experience is at all comparable to the European experience, then information necessary for the question of population collapse is not contained in cemeteries."

Thus the issue of whether epidemic disease contributed to the depopulation of northeastern Arkansas is still unsettled. I think it is pretty clear that the De Soto expedition did not introduce such disease to the area. It is also pretty clear that the settlement count decreases dramatically in the post-De Soto period in the whole Lower Mississippi Valley, including northeastern Arkansas. It is also pretty clear that Menard complex populations along the lower Arkansas River were experiencing epidemic disease mortality and depopulation in the seventeenth century. People were disappearing from northeastern Arkansas in the sixteenth century, but, until post-De Soto skeletal samples are isolated from the area, the role of epidemic disease there will remain unknown.

DROUGHT AND DEPOPULATION

Recent successful dendrochrological studies on bald cypress trees in eastern Arkansas (Stahle *et al.* 1985) appear to document a drought period between A.D. 1549 and 1577. Burnett and Murray (1991a: 21-22) use this information to make a good case that deteriorating moisture conditions for farming were an important factor in the late sixteenth-century depopulation of northeastern Arkansas. They also use De Soto narratives to strengthen their argument (1991a:21-22):

> Three of the De Soto chronicles have left us with eye-witness accounts of a drought that the people of the province of Casqui were experiencing when the De Soto entrada arrived. It will be remembered that De Soto and his men made a large wooden cross and placed it on a mound at Casqui (Bourne 1904; Varner and Varner 1988). In Biedma . . . . . it is reported concerning the raising of the cross at Casqui . . . "his people call upon it for rain, of which their fields had great need, as their children are dying of hunger" (Bourne 1904:27). The Gentleman of Elvas quotes Casqui speaking to De Soto, "you know of great droughts the maize in our fields was perishing, and no sooner had I and mine thrown ourselves on our knees before it, asking for water, than the want was supplied" (Bourne 1904:128).

Thus, both paleoclimatological and historical information from the mid-sixteenth century in northeastern Arkansas coincide to document that it was a dry time and that the chiefdom of Casqui was experiencing stress because of drought. The chiefdoms of northeastern Arkansas in the early sixteenth century were densely populated, with large nucleated town centers. Bioarchaeological evidence indicates that they were dependent on maize. It is conceivable that 26 years of dry conditions could stimulate the abandonment of northeastern Arkansas.

There are several questions to be answered, however, before the sixteenth-century drought can be accepted as a major causal factor for the depopulation. One is its extent. One of Stahle's cypress sample localities is on the Arkansas river drainage, yet both the central and lower Arkansas River valley regions were densely populated in the sixteenth and the first three quarters of the seventeenth centuries and in that time probably received some people from northeastern Arkansas. The sixteenth-century drought did not depopulate the Arkansas River Valley; its depopulation, except for four villages of Quapaws near the mouth of the River, occurred in the latter part of the seventeenth century after bioarchaeology documents epidemic disease. Drought in northeastern Arkansas late in the sixteenth century also does not explain widespread settlement-count evidence of depopulation in the lower Mississippi Valley in the sixteenth century or the north-to-south progression of depopulation in the Lower Mississippi Valley beginning much earlier. Nevertheless, the dendrochronological findings of drought are exciting and have to be figured into depopulation explanations. A major task will be developing archaeological and bioarchaeological models of the effects of drought on a primarily agricultural people and then looking for them in the ground.

ENTER THE QUAPAWS

There is no consensus on the ethnic identities of the inhabitants of northeastern Arkansas in the mid-sixteenth century (D. Morse 1990, 1991; Rankin 1988; Hoffman 1992), but a reasonably good case can be built that the people of Pacaha, at least, spoke a Tunican language (Rankin 1988:8). The major ethnic alternative, championed by Dan Morse (1991), is the Quapaw, who spoke a Dhegihan Siouan language. Rankin, a Dhegihan Siouan language specialist, finds no Siouan words in any of the De Soto narratives, while the three words recorded at Pacaha–mochila, macanoche and kaloosa–reflect Tunican phonology, phonetics, and semantics (1988:9-10). While the matter is by no means settled, my current view is that the Quapaw were not yet in Arkansas, or at least not archaeologically visible there, in the 1540s.

Both the historic Tunica tribe and the Quapaw have migration myths that document their conflicts. The Tunica myth is as follows:

> There stood a mountain, and in the mountain one day a crevice opened up. The Tunica emerged from this. When they had

> all come forth, they settled nearby. The Tunica lived on the land, and there they hunted.
> One day outlanders came to the place from which the Tunica emerged. The Tunica fought but then stopped, for they did not wish to fight.
> And so they went down the Mississippi, until they came to a place where it met another river. There they stopped and settled. A French priest came and dwelt among them. He remained with them for many years . . . (Brain 1990:13).

The river flowing into the Mississippi was the Yazoo River in Mississippi, where the Tunica were contacted by the French in 1699. The rest of the tradition, not given here, accurately documents the several subsequent movements of the tribe to the south and west (Brain 1990:13).

The Quapaw myth given to George Izard in 1827 is:

> The first red skins whom we met with were settled some way below the Ny-Whoutteh-Junka (the Little Muddy River, now the St. Francis); they were called Tonnika. We attacked them and put them to flight. Some time afterwards we entered this river, which we call Ny-Jitteh (Red River, now the Arkansas). We soon discovered that there were other red skins (Indians) in the country. Parties were sent out to look for them. They were found encamped in the Great Prairie (between the Post of Arkansas and the town of Little Rock). We attacked them; they made a valiant resistance, but we beat them and drove them away. This nation called itself Intouka; the whites at that period gave them the name of Illinois. Then we were left entire masters of this country (Bissell 1982:72).

Thus both the Tunica and Quapaw myths agree that the Tunica were forced out of their original homelands; the Quapaw claim the credit but the Tunica myth designates no culprit. If the Quapaw myth is reasonably accurate geographically, their initial recorded confrontation with the Tunica was between the mouths of the St. Francis and Arkansas Rivers, where the protohistoric Kent and Old Town phases existed in the sixteenth century (Morse and Morse 1983:270), and adjacent to the archaeological Menard Complex at the mouth of the Arkansas River. Both the Kent and Old Town phases have ceramics that are similar to those of the northwestern Mississippi Parchman-Huspuckena phase that Brain (1988) identifies with the Tunica and the De Soto chiefdom of Quiz Quiz (based on a southern crossing of the Mississippi River). Thus, it is not unreasonable to accept a Tunican identification for those phases.

The Quapaw migration myth is a fragment, and the earlier part of the saga was never recorded *verbatim*. However, several observers relayed that there was a tribal tradition of movement southward from the lower Ohio River area (Hoffman 1990:214-15). The fragment recorded by Izard implies that people were not encountered in the Arkansas area until the lower St. Francis River locality was reached; if that were true, most of northeastern Arkansas was uninhabited already. The Quapaw myth gives no time clue; the Tunica myth indicates that they were driven from their homeland before they moved to the mouth of the Yazoo River (some time prior to 1699).

The archaeological record of the Quapaw is a matter of great debate (Hoffman 1990, 1991; Morse 1991). It is clear that Ford's (1961) identification of the late Mississippian components at the Menard site as Quapaw was too simplistic and that the subsequent "Quapaw Phase" terminology was unfortunate and misleading. Postulates about Quapaw archaeological visibility range from Dan Morse's (1990, 1991) view that the Nodena Phase (Pacaha) was at least partly ancestral to the tribe, to Belmont's

(Brain 1988:281) idea that the Oliver lithic complex represented Quapaw males who came from afar and married local women, to Hoffman's (1990, 1992) assertion that their archaeological remains haven't yet been identified. Each of these scenarios may be equally likely or unlikely, and each is unverified. Thus, it is only speculation to say that Quapaw harassment of the people of the late sixteenth-century northeastern Arkansas chiefdoms helped to stimulate the abandonment of the area. This assertion is based only on Quapaw and Tunica myths, their later enmity, and the general southward movement of ethnic groups in the Lower Mississippi Valley in Mississippi and Protohistoric times.

WHERE DID THEY GO?

Whether socio-political instability, disease, drought, or Quapaw incursion singly or in combination depopulated northeastern Arkansas, no researcher envisions extinction of all the peoples there. Probably all archaeologists working in the area would agree with Ramenofsky that:

> It seems that residential instability and/or village reduction coupled with amalgamation processes were adaptive responses to new selective pressures. . . . When village population fell below a threshold necessary for defense and maintenance, mobility or amalgamation developed as attempts to correct the situation (1987:67-68).

There is much logic and some evidence that the lower and central Arkansas River valley absorbed some northeastern Arkansas peoples. The valley had a large population throughout the sixteenth century and for the first three quarters of the seventeenth century (Hoffman 1986, 1987; Jeter, Cande and Mintz 1989). Both archaeological and bioarchaeological analyses have revealed significant heterogeneity among Arkansas River sites of the period (Hoffman 1977; Murray 1989), which might indicate movements to that area. Generally, there is little significant Mississippian development along the Arkansas River valley until late Mississippian and protohistoric times. Then suddenly there are numerous sites. Certainly it became an archaeological "hot spot" then, which it hadn't been before in the Mississippi Period. The Marquette map of 1673 indicates that there was linguistic heterogeneity on the Arkansas River in the late seventeenth century (Rankin 1988). The Quapaw, labeled Akansea, were on the Mississippi River near the mouth of the White River, while some distance up the Arkansas River were towns ethnically linked to the Tunicans (Tonika, Papikaha, Mem8eta) and the linguistically related Koroans (Akoroa, Matora; Rankin 1988:11-12). Rankin (1988:11) notes the similarity between the northeastern Arkansas De Soto era town name of Pacaha and the Papikaha of the Marquette map and speculates that the same town and people may be represented. A movement south by Tunican speakers seems even more logical when it is noted that under the northern De Soto route alternative generally favored by Arkansas researchers, there was a mid-sixteenth-century town called "Tanico" on the Arkansas River (Hoffman 1992). Tunicans in northeastern Arkansas could join their linguistic kin in the late sixteenth and seventeenth centuries. The Coligua of the De Soto narratives that the Morses (1983:312) identify as the Greenbriar Phase on the White River near Batesville was linked by Swanton (1939:52) to the Karoa, who were also on the Arkansas River in 1673 according to the Marquette map. If Swanton's postulated linkage of the chiefdom of Casqui with the Kaskinampo (Swanton 1939:54), a Muskogean speaking people, is correct, they moved east to the Tennessee River and were eventually absorbed by the Koasati (Swanton 1952:224-25).

Dan Morse (1991) has had the most temerity in postulating a specific movement from northeastern Arkansas to the Arkansas River. His thesis is:

> People, primarily the Nodena phase, occupied north east Arkansas in the fifteenth, sixteenth, and seventeenth centuries. At the end of the seventeenth century only the Quapaw and Mitchigameas were present. The Mitchigameas are documented to be an Illinois tribe who traded and hunted in the summer in Arkansas. The Quapaw were the only permanent residents and were located about 100 miles south of the Nodena phase Bradley site (Province of Pacaha). As Pacaha numbers decreased and as the Algonquins pushed into formerly unoccupied areas of Illinois, the St. Francois Mountains as a source of stone for tool manufacture may have been made increasingly difficult to exploit. Similar stone was available near Searcy, accessible via the White River (based on Survey and private collections and files). A shift to the mouth of the Arkansas and White Rivers would have been advantageous from both this standpoint and for the ready access of the Ouachita Mountains and neighboring tribes such as the Tunica and Caddo. The French considered the mouth of the Arkansas sufficiently advantageous to establish Arkansas Post (D. Morse 1991:54).

Even though I was (and am) unconvinced that the Nodena Phase (the chiefdom of Pacaha in the De Soto northern route reconstruction) and the Quapaw tribe are linked, stimulated by the hypothesis that the people of Nodena moved to the locality around the mouth of the Arkansas river, I gave a paper on the topic at the 1988 Meeting of the Mid-South Archaeological Conference (Hoffman 1988) and draw on it here. To test Morse's hypothesis I used ceramics and postulated that if there was a movement of Nodena phase peoples to the area of the mouth of the Arkansas River, then strong ceramic similarities should exist between Nodena phase pottery and at least some of the protohistoric pottery of the lower Arkansas River Region. I compared Nodena phase pottery from published descriptions and from University (of Arkansas) Museum collections from the Nodena, Bradley, and other Nodena phase sites with ceramics from the Menard complex, then called the Quapaw phase (Hoffman 1988:2-6). I found little similarity between Nodena phase and Menard complex ceramics:

> Nodena and Menard complex ceramics are broadly similar only because they both are of Mississippian culture–and because archaeologists have used the same general ceramic classificatory system for the central and lower Mississippi River Valley. Ceramic pastes, forms and decorations differ markedly between the two phases even though there has to be some overlap in time. Bell Plain is popular in the Nodena phase; it is not in the Menard complex. Mississippian Plain varieties also contrast (Nodena–*Neeley's Ferry* vs. Menard Complex–*Nady*) and even though this contrast may be primarily a function of environment, distinctive Nodena phase vessel shapes or decorations do not occur on local Arkansas River pastes. Nodena phase mortuary associated jars overwhelmingly have strap or arcaded handles or the symbolic handles of the Campbell Applique mode; Menard complex jars almost without exception lack these features. Nodena phase bottles are overwhelmingly the standard Mississippian form with a globular body and wide, short or medium length neck; Menard complex bottles are overwhelmingly those which have a spheroidal body and hour glass neck (usually red filmed). Both red filming and painting occur in low percentages in Nodena mortuary pottery while red filming, most often in a distinctive band form, is the most common decorative technique on bottles, tea pots, and helmet bowls

of the Menard Complex and painting (Carson Red on Buff, Nodena Red on White, Avenue Polychrome) is relatively common on bottles, tea pots and bowls. Helmet bowls and red filmed tea pots are virtually absent in the Nodena phase ceramic complex. Head pot forms contrast. The notched horizontal applique strip so common to the vessels of the Nodena phase is virtually absent on Menard complex ceramics. Bowl rim effigies are relatively common in Nodena ceramics but are less common in the Menard complex although there is similarity in some forms represented. Nodena rim effigy bowls are unslipped; those of the Menard complex are overwhelmingly red filmed.

There are a few specific similarities between the Nodena phase and Menard complex. At the Nodena site a red filmed gourd effigy (Morse 1973:17) is very similar to several such specimens from the Menard complex Kinkead-Mainard site near Little Rock (Hoffman 1977: Fig. 10:56B; Fig. 8:25A; Fig. 12:52A) Horizontal arrangements of punctations on large bottles with short necks called Campbell Punctated by Chapman and Anderson (1955) occur on some specimens at Kinkead-Mainard (Hoffman 1977: Fig. 6:15D; Fig. 9:35A) but the neck styles are different and the Kinkead-Mainard specimens have Arkansas River valley style body incising. At the Kinkead-Mainard site there are three pottery vessels which are not of the local Mississippi Plain paste and appear to be direct imports from northeastern Arkansas. A large shallow bowl (32-101-59) has a notched horizontal applique strip on its rim and could easily have come from the Nodena phase as well as from other protohistoric phases in northeast Arkansas (Hoffman 1977:25). Another specimen is a Parkin Punctated jar with "Campbell Applique" vertical rim fillets. It is similar to the *Harris variety* of the type which is present in the Walls, Parkin and Nodena phases. A jar from the Nodena phase Bradley site is very similar to the Kinkead-Mainard pot (Phillips, Ford and Griffin 1951: Fig. 94M). A Rhodes Incised double jar (or bowl) was also found at the Kinkead-Mainard site (Hoffman 1977:27). It could have come from almost any of the protohistoric northeastern phases (Hoffman 1988:7-8).

Thus, specific similarities of the ceramics of the Nodena Phase and the Menard Complex are relatively few, particularly when the pottery of the Menard complex is compared with other nearby phases (as we shall see). A problem in appraising the Morse hypothesis is that we don't know how much ceramic chronological change to expect between the fifteenth- and sixteenth-century Nodena Phase and the sixteenth- and seventeenth-century Menard complex or, putting it another way, how much ceramic continuity should be expected. Nevertheless, based on present samples, the ceramic evidence does not support a significant movement of Nodena phase people to the Arkansas River in the seventeenth century.

On the other hand, there are a lot of ceramic similarities between the Menard complex of the lower Arkansas River Valley and adjacent northeastern Arkansas phases. Both professional archaeologists (House 1986) and collectors (Hathcock 1982; Westbrook 1982) have recognized that the mortuary pottery of the Menard complex and that of the Old Town, Kent, and Walls phases of the Mississippi and lower St. Francis Rivers just north of the Menard complex have many similarities. Collectors, in fact, lump these phases together into a "Quapaw Culture." House, after a study of new mortuary data from the Menard Complex sites Massey, Poor, and Wallace, concludes: (1986:9) "The predominance of shared attributes favors the alignment of these components with late phases along the Mississippi River to the east and north. These would be Old Town, Kent and Walls (Phillips 1970), in approximately that order." The Kent phase, particularly in its many distinctive vessel shapes, including bottles with hourglass necks, teapots, and helmet bowls, and the high proportion of red filming and painting, is similar to Menard Complex mortuary ceramics. However, there are also significant differences in frequencies of Bell Plain and Wallace Incised and other characteristics.

Thus a strong archaeological case can be made that surviving peoples of the Old Town, Kent, and Walls phases did move to the Arkansas River. The evidence that archaeological phases from farther north contributed significantly to the seventeenth-century population of the Arkansas River is less strong.

A possible destination for some of the Nodena people, based only on anecdotal information, is up the White River to the eastern portion of the Ozarks. There collectors have excavated large late Mississippian cemeteries near Mountain View, Arkansas (with no records of European trade goods) that have pottery vessels similar to Nodena types. Also, local newspaper accounts from the turn of the century record the discovery of large cemeteries along that portion of the White River. This information is only suggestive, however.

CONCLUSIONS

Many uncertainties preclude firm conclusions about reasons for the depopulation and abandonment of northeastern Arkansas in the late sixteenth and early seventeenth centuries, but we know more about why and where than thirty years ago. The protohistoric depopulation of northeastern Arkansas continued successive abandonments of areas of the Mississippi Alluvial Valley that began in the late fourteenth century and continued until the late seventeenth century. Epidemiological information makes it clear that the De Soto expedition did not bring smallpox to Arkansas. There is no bioarchaeological evidence of epidemic disease in northeastern Arkansas, perhaps because no unambiguous late sixteenth-century skeletal samples have been isolated and studied. In other areas of the lower Mississippi Alluvial Valley a dramatic decrease in settlement count in these "Protohistoric Dark Ages" has been widely interpreted to signify depopulation by epidemic disease. There was a significant period of drought in late sixteenth-century northeastern Arkansas in the era the area was abandoned. The drought, which presumably also affected at least a portion of the Arkansas River Valley, did not stimulate depopulation and abandonment there. Enmity between the Quapaw and Tunican peoples in northeastern Arkansas and the Arkansas River Valley spurred the Tunican move from Arkansas by the end of the seventeenth century and could have been responsible for earlier settlement relocations. It is probable that peoples from northeastern Arkansas moved to the Arkansas River Valley in the late sixteenth and early seventeenth centuries, particularly from nearby chiefdoms.

With the above "facts" at hand, it is possible to construct a scenario for the depopulation and abandonment of northeastern Arkansas. By A.D. 1400 the area experienced an influx of people from the adjacent "Vacant Quarter." Population density increased significantly, as did population nucleation in large fortified towns. Maize dependency became more profound, as evidenced by bioarchaeological analysis. Conflict over resources among chiefdoms was significant, as described in the De Soto narratives. In late prehistoric times in other parts of the Southeast, including the Vacant Quarter, economic and political stresses probably stimulated regional breakdowns and population dislocations. The late sixteenth century might have been northeast Arkansas' turn regardless of the De Soto entrada, disease, or drought. However, the highly maize dependent, nucleated, warring towns of northeastern Arkansas must also have been highly vulnerable to disruption by the special circumstances of the sixteenth century. Although the initial timing of epidemic disease in eastern Arkansas is unknown, it certainly was a factor in stimulating population mobility and cultural simplification among northeast Arkansas peoples or their descendants. A long drought in the later sixteenth century could have tipped the balance toward economic and political system breakdown. Finally, Quapaw and Tunican conflict, while perhaps postdating the heyday of chiefdoms in northeastern Arkansas, both played out the long-term pattern of southward movement of ethnic groups in the Lower Mississippi Alluvial Valley and terminated the Mississippian culture in eastern Arkansas.

All of the above ideas need to be operationalized by the construction of complex, multiple-variable models to understand how they articulate. Also, better dated archaeological and bioarchaeological samples are sorely needed. Unfortunately, the political state of archaeology is such that new samples are highly unlikely to become available in northeastern Arkansas. We may have to continue speculating indefinitely.

REFERENCES

Bizzell, David W.

1981 A Report on the Quapaw: The Letters of Governor George Izard to the American Philosophical Society, 1825-1827. *Pulaski County Historical Review* 29:66-79.

Bourne, Edward G., editor

1904 *Narratives of the Career of Hernando de Soto in the Conquest of Florida*, 2 Vols. A.S. Barnes, New York.

Brain, Jeffrey P.

1979 *Tunica Treasure*. Papers of the Peabody Museum of Archaeology and Ethnology 71, Harvard University, Cambridge.

1985 Introduction: Update of De Soto Studies Since the United States De Soto Commission Report. In Classics of Smithsonian Anthropology, Reprint of *Final Report of the U.S. De Soto Expedition Commission*. Smithsonian Institution Press, Washington, D.C.

1988 *Tunica Archaeology*. Papers of the Peabody Museum of Archaeology and Ethnology 78. Harvard University, Cambridge.

1990 *The Tunica-Biloxi*. Chelsea House Publishers, New York.

Burnett, Barbara A.

1989 Bioarcheology of the Goldsmith Oliver 2 Site and the Protohistoric Period in Arkansas. In *Goldsmith Oliver 2(3PU306): A Protohistoric Archeological Site Near Little Rock, Arkansas*, edited by M.D. Jeter, K.H. Cande and J.J. Mintz, 146-213. Draft Report submitted by the Arkansas Archeological Survey to the Federal Aviation Administration.

Burnett, Barbara A., and Katherine A. Murray

1991a Death, Drought and de Soto: The Bioarcheology of Depopulation. Revised paper delivered at 1989 "De Soto in Arkansas" Symposium, Fayetteville.

1991b Don't Blame de Soto. Paper presented at November 20, 1991, Department of Anthropology Colloquium, University of Arkansas. Fayetteville.

Chapman, Carl H., and Leo O. Anderson

1955 The Campbell Site, a Late Mississippian Town Site and Cemetery in Southeast Missouri. *The Missouri Archaeologist* 17:10-119.

Dobyns, Henry F.

1983 *Their Number Become Thinned. Native American Population Dynamics in Eastern North America*. University of Tennessee Press, Knoxville.

Ford, James A.

1961 *Menard Site: The Quapaw Village of Osotouy on the Arkansas River*. Anthropological Papers of the American Museum of Natural History 49(2):133-191.

Griffin, James B.

1990 Comments on the Late Prehistoric Societies in the Southeast. In *Towns and Temples Along the Mississippi*, edited by David H. Dye and Cheryl Anne Cox, 5-15. University of Alabama Press, Tuscaloosa.

Hathcock, Roy

1983 *The Quapaw and Their Pottery*. Hurley, Camden.

Hoffman, Michael P.

1977 The Kinkead-Mainard Site, 3PU2: A Late Prehistoric Quapaw Phase Site near Little Rock, Arkansas. *Arkansas Archeologist* 16-18:1-41.

1986 The Protohistoric Period in the Lower and Central River Valley in Arkansas. In *The Protohistoric Period in the Mid-South: 1500-1700*, edited by David H. Dye and Ronald C. Brister, 24-37. Mississippi Department of Archives and History Archaeological Report 18.

1987 An Examination of the Evidence for the Tunica People on the Arkansas River. Paper presented at the Annual Meeting of the Mid-South Folklore Association, Clarksville, Arkansas.

1988 A Comparison of Terminal Mississippian Period Ceramics of the Lower Arkansas River Valley with Those of Northeastern Arkansas. Paper presented at the Mid-South Archaeological Conference, Paducah, Kentucky.

1990 The Terminal Mississippian Period in the Arkansas River Valley and Quapaw Ethnogenesis. In *Towns and Temples Along the Mississippi*, edited by David H. Dye and Cheryl Anne Cox, 208-226. University of Alabama Press, Tuscaloosa.

1991 Quapaw Structures, 1673-1834, and Their Comparative Significance. In *Arkansas Before the Americans*, edited by Hester A. Davis, 55-68. Arkansas Archeological Survey Research Series No. 40.

1992 Protohistoric Period Tunican Indians in Arkansas. *Arkansas Historical Quarterly* LI, 1, 3053.

House, John H.

1986 The Mississippian Sequence in the Menard Locality, Arkansas County, Arkansas. Paper given at the Arkansas Post Tricentennial Symposium.

1987 Kent Phase Investigations in Eastern Arkansas 1978-1984, *Mississippi Archeology* 22:46-60.

1991 The Mississippian Sequence in the Menard Locality, Eastern Arkansas. In *Arkansas Before the Americans*, edited by Hester A. Davis, 6-39. Arkansas Archeological Survey Research Series No. 40.

Hudson, Charles

1985 De Soto in Arkansas: A Brief Synopsis. *Field Notes* 205:3-12, Arkansas Archeological Society. Fayetteville.

Jeter, Marvin

1986 Tunicans West of the Mississippi: A Summary of Early Historic and Archeological Evidence. In *The Protohistoric period in the Mid-South, 1500-1700*, edited by David H. Dye and Ronald C. Brister, 324-337. Mississippi Department of Archives and History Archaeological Report 18.

1990 Review of Tunica Archaeology by Jeffrey Brain. *Southeastern Archaeology* 9(2):147-148.

Jeter, Marvin, K.H. Cande, and J.J. Mintz, editors

1989 *Goldsmith Oliver 2 (3PU306): A Protohistoric Archeological Site Near Little Rock, Arkansas.* Draft report submitted by the Arkansas Archeological Survey to the Federal Aviation Administration, Southwest Region.

Lewis, R. Barry

1990 The Late Prehistory of the Ohio-Mississippi Rivers Confluence Region, Kentucky and Missouri. In *Towns and Temples Along the Mississippi*, edited by David H. Dye and Cheryl Anne Cox, 35-58. University of Alabama Press, Tuscaloosa.

Milner, George

1980 Epidemic Disease in the Postcontact Southeast: A Reappraisal. *Mid-Continental Journal of Archaeology* 5:39-56.

Morse, Dan F.

1990 The Nodena Phase. In *Towns and Temples Along the Mississippi*, edited by David H. Dye and Cheryl Anne Cox, 69-97. University of Alabama Press, Tuscaloosa.

1991 On the Possible Origin of the Quapaws in Northeast Arkansas. In *Arkansas Before the Americans*, edited by Hester A. Davis, 40-54. Arkansas Archeological Survey Research Series 40. Fayetteville.

Morse, Dan F., and Phyllis A.

1983 *Archaeology of the Central Mississippi Valley.* Academic Press, New York.

Morse, Phyllis A.

1981 *Parkin: The 1978-79 Archeological Investigations of a Cross County, Arkansas Site.* Arkansas Archeologial Survey Research Series 13. Fayetteville.

1991 The Parkin Site and the Parkin Phase. In *Towns and Temples Along the Mississippi*, edited by David H. Dye and Cheryl Anne Cox, 118-134. University of Alabama Press, Tuscaloosa.

Murray, Katherine A.

1989 Bioarchaeology of the Post Contact Mississippi and Arkansas River Valleys 1500-1700 A.D. M.A. thesis, Department of Anthropology, University of Arkansas, Fayetteville.

Phillips, Philip

1970 *Archaeological Survey on the Lower Yazoo Basin, Mississippi, 1949-1955.* Papers of the Peabody Museum of Archaeology and Ethnology 60. Harvard University, Cambridge.

Phillips, Philip, James A. Ford, and James B. Griffin

1951 *Archaeological Survey in the Lower Mississippi Alluvial Valley, 1940-1947.* Papers of the Peabody Museum of Archaeology and Ethnology 15. Harvard University, Cambridge.

Price, James E., and Cynthia R. Price

1990 Protohistoric/Early Historic Manifestations in Southeastern Missouri. In *Towns and Temples Along the Mississippi*, edited by David H. Dye and Cheryl Anne Cox, 59-68. University of Alabama Press, Tuscaloosa.

Ramenofsky, Ann F.

1987 *Vectors of Death: The Archaeology of European Contact.* University of New Mexico Press, Albuquerque.

Rankin, Robert L.

1988 Language Affiliations of Some De Soto Place Names in Arkansas. Paper presented at the "De Soto in Arkansas" Symposium, University of Arkansas Museum, Fayetteville.

Smith, Bruce D.
1986 The Archaeology of the Southeastern United States: From Dalton to de Soto, 10,500-500 B.P. *Advances in World Archaeology* 5:1-92.

Smith, Gerald P.
1990 The Walls Phase and Its Neighbors. In *Towns and Temples Along the Mississippi,* edited by David H. Dye and Cheryl Anne Cox, 135-169. University of Alabama Press, Tuscaloosa.

Stahle, D.W., M.K. Cleaveland, and J.G. Hehr
1985 A 450-year Drought Reconstruction for Arkansas, United States. *Nature* 316 (6028):530-32.

Swanton, John R.
1911 *Indian Tribes of the Lower Mississippi Valley and Adjacent Coast of the Gulf of Mexico.* Bureau of American Ethnology Bulletin 43. Smithsonian Institution, Washington, D.C.
1939 *Final Report of the United States De Soto Expedition Commission.* U.S. Congress, 76th Congress First Session, House Document 71. U.S. Government Printing Office, Washington, D.C.
1952 *The Indian Tribes of North America.* Bureau of American Ethnology Bulletin 145. Smithsonian Institution, Washington, D.C.

Varner, John, and Jeanette J. Varner, translators
1988 *The Florida of the Inca.* University of Texas Press, Austin.

Westbrook, Kent
1982 *Legacy in Clay: Prehistoric Ceramic Art of Arkansas.* Rose, Little Rock.

Williams, Stephen
1980 Armorel: A Very Late Phase in the Lower Mississippi Valley. *Southeastern Archaeological Conference Bulletin* 22:105-10.
1990 The Vacant Quarter and Other Late Events in the Lower Valley. In *Towns and Temples Along the Mississippi,* edited by Davd H. Dye and Cheryl Anne Cox, 170-80. University of Alabama Press, Tuscaloosa.

16 William Bartram and the Direct Historic Approach

Ian W. Brown
University of Alabama Museum of Natural History
Tuscaloosa, Alabama

The people who began studying prehistoric Indians in the late nineteenth century were generally the same individuals who were also studying them ethnologically, and it was these same people who drew comparisons and connections between the Indians of the present and the Indians of the past. Eventually the technique of using archaeological complexes from historic Indian sites to establish links with prehistoric assemblages would be called the "Direct Historical Approach." William Duncan Strong (1935:296) described this approach as proceeding "from the known historic [documentary-ethnological] into the unknown prehistoric [archaeological]." The basic procedure is to select historic sites of known tribes, analyze and describe the cultural complexes for these sites, and, finally, extend the links backward in time to protohistoric and prehistoric archaeological cultures (Fenton 1952: 333-335; Steward 1942; Wedel 1977:7; Wedel and Demallie 1980:111-112; White 1977:104; Willey and Sabloff 1980:108-109).

The Direct Historical Approach has had a long and productive history in twentieth-century archaeology (Collins 1927; 1940; Fenton 1940; Goggin 1947; Griffin 1943; 1945; Grinnell 1918; Harrington 1922:142-146, 172-173; Heizer 1941; Kroeber 1936; Nelson 1914:9; Parker 1916; Schenck and Dawson 1929:407; Smith 1948; Stirling 1940; Strong 1935; 1940; 1953; Swanton 1932:72; Wedel 1938; 1940; 1942). Waldo Wedel (1938) was the first person to use the actual term, but a quarter of a century earlier Roland B. Dixon had already outlined its utility in his presidential address to the American Anthropological Association (Dixon 1913:558-559, 565).

Duncan Strong and Waldo Wedel deserve the most credit for the application of this method in their work in the Central Plains. The Direct Historical Approach did not have its birth in this area, but it certainly achieved its greatest development there. By 1930 the history and ethnology of Plains tribes were well understood, yet few other areas in North America were so poorly known prehistorically. Clark Wissler's (1907:44-46) and Alfred Kroeber's (1928:394-396; 1939:78-79) position that the Plains were generally uninhabited prior to Western contact (the introduction of the horse) stunted archaeological work in the area (Frison 1973:151, 153). Not until the 1920s, when artifacts started to be found in association with extinct Pleistocene megafauna (as in Roberts 1935) did it become apparent to most people that Indians had a long history in the Plains and that archaeological work was needed to fill in the large temporal gap. The Plains became a perfect area for working from the known to the unknown.

Although there has been some well-warranted critique of the Direct Historical Approach in recent years (Brose 1971; Forbis 1963; Lightfoot 1980: 197-199; Ramenofsky 1981; 1987:105-106; White 1971:19-23), it continues to be a useful tool in Indian studies (Bishop and Smith 1975; Brain 1978; Brown 1982; Carlson 1970; Deagan 1978; Dozier 1970;

Frison 1979; Heidenreich 1979; Larson 1978; Milanich 1978; Noble 1975; Wedel and Wedel 1976; Wright 1968). The Direct Historical Approach has largely been a twentieth-century phenomenon. Most anthropologists of the late nineteenth century were more concerned with classifying the evolutionary position of the Indians than with resolving historical questions as to how their societies changed through time. John Wesley Powell, for example, was a firm believer in social evolution. Consequently, as the Director of the Smithsonian Institution's Bureau of (American) Ethnology for over two decades, he had a major impact on the theoretical positions and research directions of his staff (Darrah 1969; Fowler and Fowler 1969; Hinsley 1981:125-143; Mark 1980:165-166, Endnote 33).

One of the most important projects of the Bureau in the early years of its existence was the Mound Survey, run by Cyrus Thomas (1894). The principal goal of the Mound Survey was to determine once and for all who built the hundreds of earthen mounds that were widely distributed over the eastern half of the United States. For years it had been debated whether or not a lost race of mound builders was responsible for these tumuli (Brown 1981; 1987; Hallowell 1960:84-85; Judd 1967:18-20; Shetrone 1930; Silverberg 1968; Smith 1981; 1985; Stoltman 1973:121-126; Willey and Sabloff 1980:41-43; Williams 1979). A full-scale project, lasting eight years and covering twenty-four states, was expected to resolve the issue. Obviously, such a study had to be deeply concerned with the remains of historic Indians to see if there was a link between the past and the present. If it could be demonstrated that historic items had been included in the construction of mounds, this evidence would constitute irrefutable proof that known tribes, rather than "lost civilizations," were responsible for the mounds. Cyrus Thomas's report, published in 1894, effectively resolved the problem as to who were the mound builders, and historic archaeological materials provided the key (Thomas 1894:713-718).

William Bartram, a famous naturalist-explorer of the Southeast, both knew and proved a century earlier that the Indians were responsible for these mounds. In 1789 he wrote an essay entitled, "Observations on the Creek and Cherokee Indians." This document was written two years prior to the publication of his *Travels* (Bartram 1791), but it had a strange and unfortunate history. Bartram never lived to see it in print. His manuscript ended up in the hands of a Mobile, Alabama, resident who found it amidst a box of waste paper. Thinking that it might be important, he sent it to Samuel G. Morton of Philadelphia. Eventually, through the efforts of Ephraim G. Squier, Bartram's work came out as an article in the *American Ethnological Society Transactions* in 1853 (Bartram 1853; Swanton 1928).

Of significance to the history of archaeology, Bartram used a form of the Direct Historical Approach in his essay, but instead of employing archaeological materials, he used settlement patterns

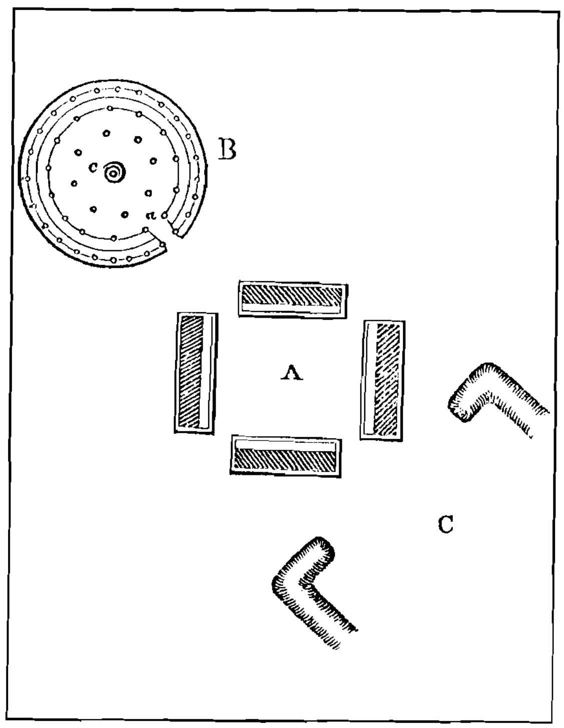


Figure 16.1. Historic Creek Indian ceremonial center (Bartram 1853: Fig. 3).

(Bartram 1853:51-54). He described, in detail, the eighteenth-century plan of the Creek Indian ceremonial area (Figure 16.1). It consisted of a public square (1A–four rectangular buildings arranged in a square), a rotunda (1B), and a chunky yard for playing ritualized games (1C). He then compared the historic arrangement of the ceremonial area to the ancient form of two-mound sites evident in the region (Figure 16.2). These mounds were still being used to some extent at the time of his visit. As the tripartite division was the same in the two patterns, Bartram reasoned that the prehistoric rotunda had been erected on one mound, the public square on the other, while the chunky yard existed in the open space between the mounds. In effect, Bartram demonstrated that the historic Creek ceremonial settlement pattern descended from the ancient one and, therefore it was possible to reconstruct the activities that occurred on and around the prehistoric mounds. Bartram not only told the world that the Indians were responsible for building the mounds, but in doing so he used the Direct Historical Approach.

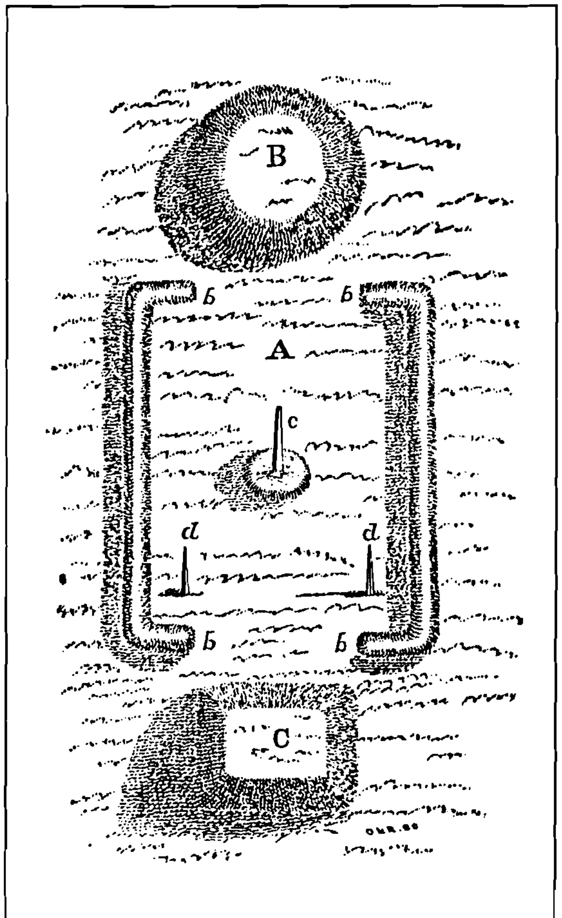


Figure 16.2. Prehistoric Creek Indian mound complex (Bartram 1853: Fig. 2).

By an unusual set of circumstances Bartram's words were heard by few. We have seen that over half a century passed between when Bartram wrote this article and when it was published. When the 1853 edition of the *American Ethnological Society Transactions* finally did come out with Bartram's paper, only twenty-five copies were distributed before a fire destroyed the rest (Bieder and Tax 1976:14). Once again, the profession and general public remained unaware of Bartram's logical, well-reasoned interpretation. In 1909, 120 years after his original manuscript was written, the 1853 edition of the *Transactions* was reprinted. Bartram's essay was no longer particularly relevant because, after all, fifteen years previously Cyrus Thomas had convincingly demonstrated that Indians were responsible for building mounds. Bartram's published work still remains the earliest known application of the Direct Historical Approach, however, and for that as well as his contribution to the destruction of the mound-builder myth, he deserves acclaim in the history of North American archaeology.

ACKNOWLEDGEMENTS

I am appreciative to the following individuals for their advice and critique of the original version of this manuscript: Jeffrey P. Brain, Richard S. Fuller, James B. Griffin, Judith Habicht Mauche, Vincas P. Steponaitis, William C. Sturtevant, and Bruce Trigger. The greatest thanks goes to Stephen Williams who, over the years, generously shared his knowledge about the prehistoric-historic transition and the history of North American archaeology. In this article I tried to combine a little bit of both for him.

REFERENCES

Bartram, William
1791 *Travels through North and South Carolina, Georgia, East and West Florida.* James and Johnson, Philadelphia.
1853 Observations on the Creek and Cherokee Indians, by William Bartram 1789, with Prefatory and Supplementary Notes by E.G. Squier. *Transactions of the American Ethnological Society*, Vol. 3, Pt. 1, Art. 1.

Bieder, Robert E., and Thomas G. Tax
1976 From Ethnologists to Anthropologists: A Brief History of the American Ethnological Society. In *American Anthropology: The Early Years, 1974 Proceedings of the American Ethnological Society*, edited by John V. Murra, 11-22. West Publishing Co., St. Paul.

Bishop, Charles A., and M. Estellie Smith
1975 Early Historic Populations in Northwestern Ontario: Archaeological and Ethnohistorical Interpretations. *American Antiquity* 40(1):54-63.

Brain, Jeffrey P.
1978 The Archaeological Phase: Ethnographic Fact or Fancy? In *Archaeological Essays in Honor of Irving B. Rouse*, edited by Robert C. Dunnell and Edwin S. Hall, Jr., 311-318. Mouton Publishers, The Hague.

Brose, David S.
1971 The Direct Historic Approach to Michigan Archaeology. *Ethnohistory* 18(1):51-61.

Brown, Ian W.
1981 Cyrus Thomas and the Mound Explorations of the Bureau of (American) Ethnology. *Southeastern Archaeological Conference Bulletin* 24:54-57.
1982 An Archaeological Study of Culture Contact and Change in the Natchez Bluffs Region. In *La Salle and His Legacy: Frenchmen and Indians in the Lower Mississippi Valley*, edited by Patricia Galloway, 176-193. University Press of Mississippi, Jackson.
1987 Review of Report on the Mound Explorations of the Bureau of Ethnology, by Cyrus Thomas, with an Introduction to the 1985 Edition by Bruce D. Smith. *American Antiquity* 52(2):414.

Carlson, Roy L.
1970 Archaeology in British Columbia. *British Columbia Studies* 6-7:7-17.

Collins, Henry B., Jr.
1927 Potsherds from Choctaw Village Sites in Mississippi. *Washington Academy of Sciences Journal* 17(10):259-263.
1940 Outline of Eskimo Prehistory. In *Essays in Historical Anthropology of North America*, 533-592. Smithsonian Miscellaneous Collections 100. Washington, D.C.

Darrah, William C.
1969 John Wesley Powell and an Understanding of the West. *Utah Historical Quarterly* 37(2):146-151.

Deagan, Kathleen A.
1978 Cultures in Transition: Fusion and Assimilation among the Eastern Timucua. In *Tacachale: Essays on the Indians of Florida and Southeastern Georgia during the Historic Period*, edited by Jerald T. Milanich and Samuel Proctor, 89-119. Ripley P. Bullen Monographs in Anthropology and History 1. University Presses of Florida, Gainesville.

Dixon, Roland B.
1913 Some Aspects of North American Archeology. *American Anthropologist* 15(4):549-566.

Dozier, Edward P.
1970 Making Inferences from the Present to the Past. In *Reconstructing Prehistoric Pueblo Societies*, edited by William A. Longacre, 202-213. University of New Mexico Press, Albuquerque.

Fenton, William N.
1940 Problems Arising from the Historic Northeastern Position of the Iroquois. In *Essays in Historical Anthropology of North America*, 159-251. Smithsonian Miscellaneous Collections 100. Washington, D.C.
1952 The Training of Historical Ethnologists in America. *American Anthropologist* 54(3):328-339.

Forbis, Richard G.
1963 The Direct Historical Approach in the Prairie Provinces of Canada. *Great Plains Journal* 3(1):9-16.

Fowler, Don D., and Catherine S. Fowler
1969 John Wesley Powell, Anthropologist. *Utah Historical Quarterly* 37(2):152-172.

Frison, George C.
1973 The Plains. In *The Development of North American Archaeology*, edited by James E. Fitting, 151-184. Anchor Press/Doubleday, Garden City.
1979 The Crow Indian Occupation of the High Plains: The Archaeological Evidence. *Archaeology in Montana* 20(3):3-16.

Goggin, John M.
1947 A Preliminary Definition of Archaeological Areas and Periods in Florida. *American Antiquity* 13(2):114-127.

Griffin, James B.
1943 *The Fort Ancient Aspect: Its Cultural and Chronological Position in Mississippi Valley Archaeology.* University of Michigan Press, Ann Arbor.
1945 An Interpretation of Siouan Archaeology in the Piedmont of North Carolina and Virginia. *American Antiquity* 10(4):321-330.

Grinnell, George
1918 Early Cheyenne Villages. *American Anthropologist* 20(4):359-380.

Hallowell, A. Irving
1960 The Beginnings of Anthropology in America. In *Selected Papers from the American Anthropologist 1888-1920*, edited by Frederica De Laguna, 1-90. American Anthropological Association, Washington, D.C.

Harrington, Mark R.
1922 *Cherokee and Earlier Remains on Upper Tennessee River.* Indian Notes and Monographs 24. Museum of the American Indian, Heye Foundation, New York.

Heidenreich, C. Adrian
1979 The Bearing of Ethnohistoric Data on the Crow-Hidatsa Separation(s). *Archaeology in Montana* 20(3):87-111.

Heizer, Robert F.
1941 The Direct-Historical Approach in California Archaeology. *American Antiquity* 7(2), Pt. 1:98-123.

Hinsley, Curtis M., Jr.
1981 *Savages and Scientists: The Smithsonian Institution and the Development of American Anthropology, 1846-1910.* Smithsonian Institution Press, Washington, D.C.

Judd, Neil M.
1967 *The Bureau of American Ethnology: A Partial History.* University of Oklahoma Press, Norman.

Kroeber, Alfred L.
1928 Native Culture of the Southwest. *University of California Publications in American Archaeology and Ethnology* 23(9):375-398.
1936 Prospects in California Prehistory. *American Antiquity* 2(2):108-116.
1939 *Cultural and Natural Areas of Native North America.* University of California Publications in American Archaeology and Ethnology 38.

Larson, Lewis H., Jr.
1978 Historic Guale Indians of the Georgia Coast and the Impact of the Spanish Mission Effort. In *Tacachale: Essays on the Indians of Florida and Southeastern Georgia during the Historic Period*, edited by Jerald T. Milanich and Samuel Proctor, 120-140. Ripley P. Bullen Monographs in Anthropology and History 1. University Presses of Florida, Gainesville.

Lightfoot, Kent G.
1980 Mormon Sociopolitical Development in Northern Arizona, 1876-1906: Implications for a Model of Prehistoric Change. *Ethnohistory* 27(3):197-223.

Mark, Joan
1980 *Four Anthropologists: An American Science in its Early Years.* Science History Publications, New York.

Milanich, Jerald T.
1978 The Western Timucua: Patterns of Acculturation and Change. In *Tacachale: Essays on the Indians of Florida and Southeastern Georgia during the Historic Period*, edited by Jerald T. Milanich and Samuel Proctor, 59-88. Ripley P. Bullen Monographs in Anthropology and History 1. University Presses of Florida, Gainesville.

Nelson, Nels C.
1914 *Pueblo Ruins of the Galisteo Basin, New Mexico.* Anthropological Papers of the American Museum of Natural History 15, Pt. 1. New York.

Noble, William G.
1975 Applications of the Direct Historic Approach in Central District of Mackenzie, N.W.T. In *Proceedings: Northern Athapaskan Conference, 1971* Vol. 2, edited by A. McFadyen Clark, 759-785. National Museum of Man, Mercury Series, Canadian Ethnology Service Paper 27. Ottawa.

Parker, Arthur C.
1916 The Origin of the Iroquois as Suggested by Their Archeology. *American Anthropologist* 18(4):479-507.

Ramenofsky, Ann
1981 Historic Aboriginal Depopulation and Settlement Changes: Responses to European Diseases. Paper presented at the 46th Annual Meeting of the Society for American Archaeology, San Diego.
1987 *Vectors of Death: The Archaeology of European Contact.* University of New Mexico Press, Albuquerque.

Roberts, Frank H.H., Jr.
1935 *A Folsom Complex: Preliminary Report on Investigations at the Lindenmeier Site in Northern Colorado.* Smithsonian Miscellaneous Collections 94(4). Washington, D.C.

Schenck, W. Egbert, and Elmer J. Dawson
1929 *Archaeology of the Northern San Joaquin Valley.* University of California Publications in American Archaeology and Ethnology 25(4).

Shetrone, Henry C.
1930 *The Mound-Builders.* D. Appleton and Company, New York and London.

Silverberg, Robert
1968 *Mound Builders of Ancient America: The Archaeology of a Myth.* New York Graphic Society Ltd., Greenwich, Connecticut.

Smith, Bruce D.
1981 The Division of Mound Exploration of the Bureau of (American) Ethnology and the Birth of American Archaeology. *Southeastern Archaeological Conference Bulletin* 24:51-54.
1985 Introduction to *Report on the Mound Explorations of the Bureau of Ethnology* by Cyrus Thomas. Smithsonian Institution Press, Washington, D.C.

Smith, Hale G.
1948 Two Historical Archaeological Periods in Florida. *American Antiquity* 13(4):313-319.

Steward, Julian H.
1942 The Direct Historical Approach to Archaeology. *American Antiquity* 7(4):337-343.

Stirling, Matthew W.
1940 The Historic Method as Applied to Southeastern Archeology. In *Essays in Historical Anthropology of North America* 117-123. Smithsonian Miscellaneous Collections 100. Washington, D.C.

Stoltman, James B.
1973 The Southeastern United States. In *The Development of North American Archaeology*, edited by James E. Fitting, 116-150. Anchor Press/Doubleday, Garden City.

Strong, William D.
1935 *An Introduction to Nebraska Archeology*. Smithsonian Miscellaneous Collections 93(10). Washington, D.C.
1940 From History to Prehistory in the Northern Great Plains. In *Essays in Historical Anthropology of North America* 353-394. Smithsonian Miscellaneous Collections 100. Washington, D.C.
1953 Historical Approach in Anthropology. In *Anthropology Today: An Encyclopedic Inventory*, edited by A.L. Kroeber, 386-397. The University of Chicago Press, Chicago.

Swanton, John R.
1928 The Interpretation of Aboriginal Mounds by Means of Creek Indian Customs. *Smithsonian Institution Annual Report for 1927*, 495-506. Washington, D.C.
1932 The Relation of the Southeast to General Culture Problems of American Pre-history. In *The Conference on Southern Prehistory*, Dec. 18-20, 1932, edited by Carl Guthe, 60-74. National Research Council, Washington, D.C.

Thomas, Cyrus
1894 Report on the Mound Explorations of the Bureau of Ethnology. *Twelfth Annual Report Bureau of Ethnology*, 27-742. Smithsonian Institution, Washington, D.C.

Waring, Antonio J., Jr.
1968 The Southern Cult and Muskhogean Ceremonial. In *The Waring Papers*, edited by Stephen Williams, 30-69. Papers of the Peabody Museum of Archaeology and Ethnology 58. Cambridge.

Wedel, Mildred Mott, and Raymond J. Demallie
1980 The Ethnohistorical Approach in Plains Area Studies. In *Anthropology on the Great Plains*, edited by W. Raymond Wood and Margot Liberty, 110-128. University of Nebraska Press, Lincoln and London.

Wedel, Waldo R.
1938 *The Direct-Historical Approach in Pawnee Archeology*. Smithsonian Miscellaneous Collections 97(7). Washington, D.C.
1940 Culture Sequence in the Central Great Plains. In *Essays in Historical Anthropology of North America*, 291-352. Smithsonian Miscellaneous Collections 100. Washington, D.C.
1942 Archeological Remains in Central Kansas and Their Possible Bearing on the Location of Quivira. *Smithsonian Miscellaneous Collections* 101(7). Washington, D.C.
1977 The Education of a Plains Archeologist. *Plains Anthropologist* 22(75):1-11.

Wedel, Waldo R., and Mildred Mott Wedel
1976 Wichita Archeology and Ethnohistory. In *Kansas and the West: Bicentennial Essays in Honor of Nyle H. Miller*, edited by Forrest R. Blackburn *et al.*, 8-20. Kansas State Historical Society, Topeka.

White, John R.
1977 Ethnoarchaeology, Ethnohistory, Ethnographic Analogy, and the Direct-Historical Approach: Four Methodological Entities Commonly Misconstrued. *The Conference on Historic Sites Archaeology Papers* 11:98-110.

White, Marian E.
1971 Ethnic Identification and Iroquois Groups in Western New York and Ontario. *Ethnohistory* 18(1):19-38.

Willey, Gordon R., and Jeremy A. Sabloff
1980 *A History of American Archaeology*, 2nd edition. W.H. Freeman and Company, San Francisco.

Williams, Stephen
1979 The Division of Mound Exploration of the Bureau of Ethnology: "How it All Began . . ." Document of the Smithsonian Institution/ Lower Mississippi Survey Conference on the Mound Exploration Division of the Bureau of American Ethnology. Unpublished ms., Lower Mississippi Survey, Peabody Museum, Cambridge.

Wissler, Clark
1907 Diffusion of Culture in the Plains of North America. *Proceedings of the Fifteenth International Congress of Americanists* 2:39-52.

Wright, James V.
1968 The Application of the Direct Historical Approach to the Iroquois and the Ojibwa. *Ethnohistory* 15(1):96-111.

PART IV

HISTORIC ARCHAEOLOGY

17 Community, Commodities, and the Concept of Property in Seventeenth-Century Narragansett Society

William A. Turnbaugh
University of Rhode Island
Kingston, Rhode Island

INTRODUCTION

Exchange relationships are among the most universal and ancient of human interactions. The variable distribution of natural resources within bounded territories, the need to maintain regular contact with other groups, and the desire to acquire exotic materials to serve as markers of social rank are all stimulants to trade. In these transactions each society imposes its own culturally-defined values on its natural resources and crafted goods, ranking their desirability on one scale or another–rarity, form, utility. Certain products may be esteemed primarily for their social significance, while the value of others is deemed to be more strictly economic. These assessments may not always be shared by exchange partners, for each may perceive a given item in ways not recognized by the other.

Western Europeans and North American Indians began trading as soon as they came into contact along the Atlantic coast and generally maintained these relationships for decades. The flow of goods between dissimilar societies set in motion a host of irreversible changes. Most studies, including this one, acknowledge that European wares played a major role in stimulating some very fundamental transformations in native culture. At the same time, there can be no question that the introduction of tobacco, potatoes, furs, precious metals, and many other products from America likewise had a profound impact on Old World peoples and economies.

Familiar explanations for the popularity of European goods among native recipients on several continents generally cite either their assumed technological superiority or the supposedly high status and/or novelty value associated with the new products. The limited purpose of the present essay requires us to ignore these issues and, instead, to focus on a more fundamental topic. The dramatic shift in the *context* of exchange in native society has received little attention up to now. The example of the seventeenth-century Narragansett Indians of southern New England may be used to demonstrate how the transfer and adoption of European notions of exchange displaced traditional concepts and, at the same time, opened the way for the expansion and acceptance of the European market system and its goods. We will also trace the evolving perception of personal property among the Narragansett as they came to embrace European notions of material wealth.

CONTEXTS OF EXCHANGE IN NARRAGANSETT SOCIETY

Most exchange in the pre- and protohistoric periods in southern New England had been primarily what we may characterize as *community-oriented.* This concept was shared widely throughout native America. Local resources as varied as herbs or hides, as well as skilfully made tools and craft items, readily changed hands and were highly valued–though not in a commercial or strictly economic sense. In traditional Narragansett society, interpersonal transactions generally served social and community-enhancing purposes rather than individual advantage. Exchange frequently functioned as a highly visible and symbolically charged public display intended to promote or affirm civic coordination and unity and, likewise, to assure the beneficence of the gods. For example, socially significant events commemorated by prescribed wampum exchanges included rites of passage such as marriage (Williams 1936 [1643]:148) and momentous public occasions such as the coronation of a new sachem (Chapin 1931:94).

Traditional community-oriented exchange transactions were rooted in the basic *sharing* of food and other provisions that characterized relations among family members and intimates (Williams 1936 [1643]:73). Freely extended to guests and strangers, sharing became *hospitality* (Williams 1936 [1643]:16; Mourt 1966 [1622]:42; Verrazzano, in Quinn 1979, I:285). Formal *gift exchanges* established relations between equals or between persons of unknown status (Bartlett 1963 [1874]:382; Verrazzano, in Quinn 1979, I:285; Mourt 1966 [1622]:34), as when deer were presented to principal Pilgrims (Mourt 1966 [1622]:61) or tobacco was passed around upon meeting others along a trail in the woods (Williams 1936 [1643]:73). In specified contexts, the presentation of *tribute* acknowledged and strengthened the binding ties between a people and their leaders. Narragansett sachems received tribute from their subjects within the tribe upon their coronation and on other extraordinary and significant occasions, as when a deer was killed in water (Chapin 1931:94; Williams 1936 [1643]:176, 141). Wampum strings and other forms of tribute were claimed also from dominated neighbors–the Nipmuck, Coweset, and Niantic, among others (Chapin 1931:42; Gookin 1970 [1792]:8). The sachems, in turn, were instruments of *redistribution*, allocating some of the resources of the society to their followers on public occasions such as the annual harvest festival (Williams 1936 [1643]:129, 180; Chapin 1931:89).

In each of these transactions–through sharing, hospitality, gift exchange, tribute, redistribution–traditional exchange served primarily to foster the native community. There was little or no truly economic motivation for these interactions. In a real sense nothing was "for sale." Surrounded by the natural resources needed to sustain themselves, and relying on a basic level of technology that every member of society could master, the Narragansett were nearly self-sufficient. In their culture exchange was essentially a social activity.

The English and Dutch newcomers brought with them their own very different notions of exchange. Their intentions were decidedly commercial rather than social. Europeans had, over many centuries, perfected an exchange system based on commodities, currency, and the concept of profit. For the people from the Old World, it seemed only natural that the primary purpose of exchange should be to acquire or accumulate as much as possible of something useful or valuable, or to seek a profit in disposing of it to others. Europeans viewed their world–and rather quickly came to view America–as a warehouse of commodities, useful resources that might have commercial value in an international economic system. They were inclined to assign a pecuniary value to nearly everything.

The context of exchange in the Northeast evolved as market-motivated Europeans arrived on the scene in ever greater numbers. In the initial stages of contact, a few baubles, "trinkets" (Verrazzano, in Wroth 1970:138) and "trifles" (Brereton 1966 [1602]:8), had been bestowed upon the curious peoples encountered by exploring parties along the New England coast. It appears the Indians interpreted these profferings as applications by the newcomers to be included in the Indians' own established exchange network. They readily acceded to the European "requests," acknowledging the overtures with customary responses and small offerings of their own, including the sharing of food and hospitality (Verrazzano, in Quinn 1979, I:285; Mourt 1966 [1622]:42) and the symbolic presentation of skins (Mourt 1966 [1622]:34). For their part, Europeans only occasionally reciprocated in a manner that would have been recognized by their Algonquian hosts as appropriate to the situation, as when they presented gifts to the Pokanoket sachem (Mourt 1966 [1622]:33-40), acknowledged services rendered by guides and messengers (Mourt 1966 [1622]:47; Bartlett 1963 [1874]:165), rewarded an Indian hero (Mourt 1966 [1622]:51), and appeased a native mother whose sons had been carried off to England years earlier (Mourt 1966 [1622]:50).

Europeans were not fully attuned to the Indian etiquette of gift exchange and so were not long content with small tokens of affiliation. They quickly became insistent on receiving only certain kinds of commodities in return for their offered goods, and they expected to obtain them in quantities that far exceeded the normal standards of social exchange. In meeting these demands southern New England Indians were drawn increasingly into a commercial relationship with the Europeans. They were becoming participants in a global economy, one that was predominantly *commodity-oriented*, which, with its emphasis on utility, accumulation, and profit, contrasted dramatically with community-oriented exchange.

To underscore one of the essential differences between Native American and European societies: the Indians had nothing for sale; the newcomers would give nothing away. Yet, each earnestly desired what the other had. The resolution of this dilemma led to great changes, especially in native society.

WAMPUM

The transformation of wampum shell beads was central to the process of cultural change in the Northeast, especially along the southern New England coast. Wampum traditionally had been greatly valued as a symbol of social standing and respect in Narragansett culture. Wampum production was a laborious procedure requiring the precise cutting, drilling, and polishing of hard blanks broken from clam and conch shells. Even with concentrated effort, it was possible to make only a few beads each day. The difficulty in producing the special cylindrical shell beads with stone-age technology made wampum relatively scarce, even though the raw material was abundant; therefore, wampum was an appropriate and highly visible marker of one's standing in the community (Williams 1936 [1643]:157).

Individuals normally acquired small amounts of wampum through participation in formal public events such as victory celebrations, harvest festivals, and marriage ceremonies (Williams 1936 [1643]:129, 148, 180; Chapin 1931:89). Or larger quantities might be conveyed to a prominent person as a token of honor, as when a headman took office. Wampum could not be bought, it was bestowed. It was not money, but a symbol of social position and community esteem. Nevertheless, Europeans generally confused wampum's role as a status marker in a social context with the familiar function of monetary wealth in Old World society (see Williams 1936 [1643]:157-158). Finding no other convenient substitute for coin, they fostered the use of wampum as a medium of circulating currency in a commercial context, even introducing it to tribes who had had no previous experience with it (Ceci 1977:196-197). The English and Dutch traders brought metal drills and files to encourage Indians to produce more shell money more rapidly (Williams to John Winthrop, Dec. 15, 1648, in LaFantasie

1988:264). Whites also insisted on exchanging wampum for most of the products they wanted, and soon all those who dealt with Europeans had access to it. Substantial fines levied by English courts against Indian transgressors pumped additional quantities of wampum into circulation (Robinson 1990:160-161). In time, the shell beads were transformed into just another commodity. Wampum's social value became debased as it was monetized and invested with economic value at an established rate of six white beads or three purple beads to the English penny (Williams 1936 [1643]:152)–before inflation, devaluation, and later demonetization destroyed its economic role altogether (Potter 1835:46; Weeden 1978 [1890]:42-44).

ACCESS AND ACCUMULATION

That the Indians of southern New England at least initially became willing partners in the new era of exchange is not to be doubted. In 1643, Roger Williams related that the Narragansett had already come to regard themselves as deprived by comparison with the materially-rich English:

> they are easily perswaded that the *God* that made *English* men is a greater *God*, because Hee hath so richly endowed the *English* above *themselves*: But when they heare that about sixteen hundred yeeres agoe, *England* and the *Inhabitants* thereof were like unto *themselves*, and since have received from *God, Clothes, Bookes,* &c. they are greatly affected with a secret hope concerning *themselves* (Williams 1936 [1643]:A4).

Perhaps not content to wait quite so long for prosperity, the Narragansett avidly engaged in trade with their new neighbors. Tribal leaders invited Williams to set up a trading post at Cocumscussoc, in the heart of Narragansett territory (Woodward 1971; Williams, Nov. 17, 1677[?], in LaFantasie 1988:752). Through his establishment and several others like it in southern New England, much of the maize, furs, venison and wampum that once circulated through traditional native exchanges were now redirected to English and Dutch merchants and colonists, or were being sold rather than passed along to neighboring tribes. The Narragansett were particularly well-situated to enjoy the new economic climate. As William Wood had observed in 1634, "they rest secure under the conceit of their popularity and seek rather to grow rich by industry than famous by deeds of chivalry" (Wood 1977 [1634]:81).

The Narragansett played a pivotal role in the classic "triangle" trade that developed by the second quarter of the seventeenth century (Ceci 1977:278-279). First, inexpensive goods from Europe were exchanged for wampum produced primarily by the Narragansett or obtained by them from their allies or neighbors; then, traders transported this wampum inland and exchanged it for furs; which, finally, were returned to Europe to be sold at great profit. As the "minters" (Wood 1977 [1634]:81) of the wampum and as primary recipients of European goods, the Narragansett controlled two of the three classes of commodities. They took advantage of their position by trading both European goods and wampum, as well as some more traditional products of their own making, to remote tribes "who are ignorant at what cheap rates they obtain [English commodities] in comparison of what they make them pay, so making their neighbors' ignorance their enrichment" (Wood 1977 [1634]:81).

The ethnohistorical accounts suggest the Narragansett took to their new commercial role with some alacrity, even though doing so required an abandonment of many older notions regarding the purposes and procedures of exchange. Williams dryly recorded the invention of a significant new term in Narragansett vocabulary: "*Cuppaimish* I will pay you, which is a word newly made from the *English* word pay" (Williams 1936 [1643]:161). He gives us a measure of the Narragansett's growing sophistication and business sense in other vocabulary phrases, including:

Tatuppauntuhommin	*To weigh with scales.*
Cowenaweke	*You are a rich man.*
Nummouanaquish	*I come to buy.*
Keeskwhim teaug mesin	*Pay me my money.*
Machetu	*A poore man.*

(Williams 1936 [1643]:159-170)

The Indians quickly attained a reputation as shrewd bargainers, as Williams the trader could attest from experience:

> They are marvailous subtle in their Bargaines to save a penny: And very suspicious that *English* men labour to deceive them: Therefore they will beate all markets and try all places, and runne twenty thirty, yea, forty mile, and more, and lodge in the Woods, to save six pence (Williams 1936 [1643]:163).

They were very selective buyers, too, expressing their preference for specific kinds of goods and avoiding whatever they considered to be inferior merchandise (Williams 1936 [1643]:156, 160; Potter 1835:17; Williams 1646, in Chapin 1916:241).

Indian choosiness notwithstanding, European goods rapidly attained importance in Narragansett society. They replaced most native objects in filling technical needs, stimulated new economic relationships, and were even assimilated into Narragansett ritual and belief systems. Still, these new products were not equally accessible to every member of Narragansett society, either living or dead. Primary or direct access to the goods seems to have been limited or even restricted to a relative few. Williams suggests that among the Narragansett, all who wished to do so could make wampum (Williams 1936 [1643]:152); but in fact the process demanded sufficient time and skill that the number of moneymakers could not have been large. Other trade commodities, such as prepared pelts and elaborately carved stone smoking pipes, likewise represented considerable investments of effort.

Negotiating with Europeans at their trading posts must have been an intimidating prospect for most Indians, given linguistic and logistical considerations. For a few individuals, trading seems to have become a specialized occupation. Narragansett trading expeditions were generally made up of 10 to 20 members, and, according to Williams, all were sharp and experienced bargainers (Williams 1936 [1643]:159-163).

With the exchange system fostering differential access to valued European products, few Narragansett would have been able to obtain all they desired. Many must have perceived themselves as socially and materially deprived. On the other hand, those able to acquire the exotic goods might use them in several ways. They could employ them in a manner that was consistent with traditional usage–enhancing their own social status and prestige through possessing, displaying, or disposing of valued items. Others, electing to participate more fully in the new commercial climate, might advance their own economic position by profitably reselling the trade goods within their community or beyond it.

Aside from the "professional" traders, one other segment of Narragansett society that stood in a favored position to gain most directly from these transactions was the sachems. In southern New England, at least in the first half of the seventeenth century, leadership among the Narragansett and neighboring groups, such as the Pokanoket (or Wampanoag), was invested in a hierarchy of sachems and under-sachems (Simmons 1978:193). The sachems assigned lands, settled disputes, imposed judgments, presided at ceremonies, protected their followers. For these services they were entitled to tribute of corn, hides, and wampum.

Among the Narragansett, the position of sachem was hereditary through the male line. Sachems were assisted and advised by councils of prominent men. Although acknowledged as leaders, and enjoying the right to substantial tribute at specified times and on frequent occasions, the sachems' real power ultimately resided in their ability to persuade others:

> The *Sachims*, although they have an absolute Monarchie over the people, yet they will not conclude of ought that concernes all, either Lawes, or Subsides, or warres, unto which the people are averse, and by gentle perswasion cannot be brought (Williams 1936 [1643]:142).

A dual sachemship characterized the Narragansett in Roger Williams' time (Williams 1936 [1643]:140-141). Canonicus, an older man, took the leading role in domestic affairs, while Miantonomi, his brother's son, had responsiblity for relations with other tribes (both friendly and hostile) and with the English. Oussemaquin or Massasoit was the contemporary head sachem of the neighboring and rival Pokanoket people. Each of these sachems during this critical period was endowed with personal qualities that inspired the confidence of his followers and commanded the respect of English authorities as well.

Given their role among their people and their control over the lands of their followers, the sachems were regarded quickly and conveniently by the newcomers as the functional equivalents of European kings. As such, they were expected to speak for their people and to conduct all important transactions on their behalf. Likewise, it was generally the sachems who personally accepted payments from the English for the use or purchase of Indian lands. Buyers seldom concerned themselves with the details of how such proceeds were or were not allocated among the sachem's people, except when forced occasionally to pay out a second or third time for the same piece (see Williams 1646, in Chapin 1916:241). (The matter of what, exactly, was being purchased was disputable as well, but that is another matter; see Cronon 1983:60-61.)

Roger Williams directly contributed to the material wellbeing of the Narragansett sachems on many occasions, including those of land dealings. However, his transactions differed from those of most of his countrymen in that Williams recognized that his "purchases" were of more a diplomatic than a strictly commercial nature. The distinction was too fine for most of his contemporaries to appreciate, but Williams repeatedly explained that he had not bought the Rhode Island lands from the Narragansett sachems in the ordinary sense, but, instead, had acquired them as a "gift" of "love" from the natives:

> Were it not for the favor God gave me with Canonicus, none of these parts, no, not Rhode Island, had been purchased or obtained, for I never got any thing out of Canonicus but by gift (Bartlett 1963 [1874]:406).

The confusion over this matter stemmed from the fact that some merchandise (lands on one side, wampum and coats on the other) had indeed changed hands in these instances:

> It was not price nor Money that could have purchased Rode Iland; Rode Iland was obtained by Love .... *It is true, I advised a Gratuitie to be presented* to the Sachim [Miantonomi] and the Natives ... (Williams' statement, Aug. 25, 1688, in LaFantasie 1988:485; emphasis added).

And again:

> concerning the islands Prudence and ... Aquedenick, be pleased to understand your great mistake: neither of them were sold properly, for a thousand fathom [of wampum] would not have bought either, by strangers. The truth is, not a penny was demanded for either, and what was paid

> was only gratuity, though I chose, for better assurance and form, to call it sale (Williams to John Winthrop, ca. June 14, 1638, in LaFantasie 1988:165).

This transaction (as well as others like it) was, in reality, a gift-exchange of the traditional Narragansett form, the sachems granting Williams their lands and he reciprocating with a modest return token that acknowledged his indebtedness to them. Still, the increasing frequency of these interactions, plus the formalization of the sachems' role in representing their people, afforded opportunities for these individuals to amass personal possessions at a much faster rate than any of their tribesmen, probably including the native traders. While the sachems of earlier periods undoubtedly enjoyed the same relatively advantageous position (in interacting on behalf of their people with exchange partners in other native communities, for example) the material proceeds of the earlier negotiations appear to have been redistributed more widely through the community (Williams 1936 [1643]:128-129).

A consummate diplomat, required frequently to travel, trade, and live among what he once described as "the thickest [concentration] of the barbarians" (Bartlett 1963 [1874]:269), Roger Williams maintained good relations with the Narragansett sachems by anticipating and administering to their increasing material appetites:

> They had my person my shallop and Pinnace and hired servant etc. at Command on all occasions, transporting 50 at a time, and lodging 50 at a time at my howse. I never denied them ought they desired of me. Caunounicus laid me out Ground for a trading howse at Nahigonset with his owne hand but he never traded with me, but had freely what he desird Goods Mony etc. so that tis simple to imagine that many hundrets excused me to the last of that mans breath whom (dying) sent for me and desired to be buried in my cloth of Free gift and so he was (Williams' statement, Nov. 17, 1677[?], in LaFantasie 1988:752).

> I never denied [Canonicus] or Miantonomo whatever they desired of me as to goods or gifts or use of my boats or pinnace, and the travels of my own person, day and night .... (Williams, June 18, 1682; in Bartlett 1963 [1874]:406).

Miantonomi and Canonicus were alert to any opportunity to acquire gifts from Williams or his high-placed friends. Williams obligingly put in a good word for the sachems whenever he could. The older sachem apparently had a sweet tooth, while his nephew expressed a particular interest in English armaments:

> Canonicus would gladly accept of a box of eight or ten pounds of sugar, and indeed he told me he would thank Mr. Governour for a box full (Williams to Gov. John Winthrop, May 1, 1637; in LaFantasie 1988:72; see also Williams to Winthrop, Feb. 28, 1637/38, and May 9, 1639, in LaFantasie 1988:145, 198).

> For any gratuities or tokens: Caunonicus desires Sugar and Miantunnomu powder (Williams to Gov. Henry Vane or John Winthrop, May 13 1637; in LaFantasie 1988:79).

> I pray sir, forget not to reward this messenger with a Coate, as allso some powder for Miantunnomu (Williams to Gov. John Winthrop, July 10 1637; in LaFantasie 1988:98).

Miantonomi was especially eager to obtain powder and shot after claiming one of three guns captured during the English expedition against the Pequots in the spring of 1637 (Williams to Gov. John

Winthrop, July 10 1637; in LaFantasie 1988:96). The sachems seem also to have maintained a hoard of European cooking kettles; according to Williams: "[Miantonomi] sayth he hath many [kettles] of his owne, and indeed when I came first hiether I saw neere 10 or 12 wch himselfe and Canounicus had" (letter from Williams to Gov. John Winthrop, ca. Sept. 9, 1637; in LaFantasie 1988:118). Miantonomi was even accused on one occasion of trying to add to his collection by theft (*Ibid.*).

In time, the Narragansett became increasingly self-conscious of their new kind of wealth. Precautions against theft apparently became necessary and commonplace in a society where stealing had once been unthinkable. Williams documents this loss of innocence by the 1640s:

> Most commonly their houses are open, their doore is a hanging *Mat*, which being lift up, falls downe of it selfe; yet many of them get *English* boards and nailes, and make artificiall doores and bolts themselves, and others make slighter doores of *Burch* or *Chestnut* barke, which they make fast with a cord in the night time, or when they go out of town, and then the last (that makes fast) goes out at the Chimney which is a large opening in the middle of their house .... (Williams 1936 [1643]:38-39).

Moreover:

> Many of them begin to be furnished with *English* Chests; others, when they goe forth of towne, bring their goods (if they live neere) to the *English* to keepe for them, and their money they hang it about their necks, or lay it under their head when they sleepe (Williams 1936 [1643]:40).

ARCHAEOLOGICAL CORRELATES

Archaeological evidence directly corroborates some of Williams' observations and confirms the notion that wealth had become a personal, rather than communal, asset during the seventeenth century.

Few late prehistoric or protohistoric burials in Rhode Island or elsewhere in southern New England contained durable offerings (Brenner 1984:185 ff.; Cook 1985:50-51). During the contact period, mortuary offerings expanded at two levels. First, many more graves in each cemetery were provisioned. This trend accelerated through the seventeenth century until nearly three-quarters of the Narragansett in a cemetery dating to ca. 1650-1670 were favored with offerings. Second, although there was a general increase in the number of individuals buried with offerings, some of them received substantially more goods than did others (Turnbaugh 1984a:15, 1984b; Robinson 1990:219-237).

During excavations at the 1620-1660 Narragansett Indian cemetery site at West Ferry, in Jamestown, Rhode Island, William Simmons encountered the double interment of an old man and a young child in Burial 5 (Simmons 1970:82-89). This grave was by far the most elaborate and well-provisioned of the four dozen excavated in 1966-1967, only half of which held any offerings. Simmons speculated that the elder individual in Burial 5 may have been the sachem Mixanno, son of Canonicus, who had died prior to 1658. The ornamented frame of a small wooden trunk rested over the torso of the adult. Measuring 57 cm long, 34 cm wide, and 24 cm deep, the box had been constructed from carved sections pinned together with short wooden pegs and decorated with sheet brass cut-outs and tacks. Its basketry or leather sidewalls and hinges had decayed. A brass buckle once clinched a strap to secure the lid. What the trunk had contained was no longer evident. The same grave also yielded an iron padlock and an accompanying iron key. The lock had an oval hasp and a triangular body; a swinging escutcheon plate concealed the keyhole.

Mixanno had acquired considerable wealth through his position and his dealings with the English. Williams characterized both the sachem and his eldest son as "being rich in peag," or white wampum (in LaFantasie 1988:489). Their greed annoyed Williams, who complained, for example, that Mixannno and his sons unfairly required payment–wampum was the standard currency–from the English for fodder cut on their lands:

> these two present dissenting Sachims [Scuttop and Quequaquenuit] . . . & their Father [Mixanno] deceased, have long and most barbarously abused the [English] Inhabiants of Rode Iland, about the cutting of Grasse on Qunnunnagut, driving them (for their peace Sake) to hire and pay for, at extreame rates, their owne Grasse wch the former Great Sachim [Canonicus, father of Mixanno] most freely granted to us (Williams' 1658 statement, in LaFantasie 1988:486).

Among the grave goods that distinguished Burial 5, contended to be that of Mixanno, were wampum beads and undrilled shell blanks. Only one additional grave, that of a young child, held finished wampum beads at the West Ferry site (Simmons 1970:138). Wampum's rarity (just 52 finished beads) and restricted distribution at this cemetery is consistent with its traditional value as a social marker, but Williams' just-quoted remarks reveal that wampum's role had been undergoing a transformation within native society.

By only a short time later, as another cemetery began to receive the Narragansett dead, wampum's significance had shifted. The RI-1000 site, in North Kingstown, Rhode Island, held the remains of 56 individuals, 47 of whom were found in undisturbed graves during excavations by the Rhode Island Historical Preservation Commission in 1982 and 1983 (Robinson and Gustafson 1983; Turnbaugh 1984a; Robinson, Kelley and Rubertone 1985). Of the latter, more than one-quarter (12 of 47) were furnished with wampum. However, the distribution of the more than 2700 beads was highly selective. Ninety percent of the wampum accompanied only four individuals, a three- or four-year-old child (Burial 10) and three adolescents (Burials 3, 40, 49) (Turnbaugh 1984a:30). Each of these young people had died at a ritually-critical age (Rubertone 1989:41), but this observation alone cannot fully account for the quantities of wampum buried with them, since others of the same age were not treated to equal amounts. The most likely explanation is that these four, at least, were members of prominent Narragansett lineages (Robinson 1990:236-237).

The presence of wampum in these particular graves denotes its continued role as a social marker. However, examination of wampum's overall distribution pattern in the RI-1000 graves leads into another dimension–the relationship between wealth and wellbeing. Those individuals associated with wampum appear to have been healthier. A microscopic analysis of the longbones of persons buried with wampum, compared to those without, revealed fewer Harris (interrupted-growth) lines, indicating that those with the wampum suffered less exposure to childhood malnutrition and disease (Robinson 1990:176-178). As Robinson observes:

> It does seem clear . . . from the burials at RI 1000 that some individuals were better cared for than others and those that were better cared for were the ones with the wampum, wampum that was used to buy corn in [the starving year] 1648, wampum that had increasingly become the means, not simply for extra-regional diplomacy or as gifts to the gods, but for buying things for everyday use and survival . . . . The fact that, in general, those with wampum were better off, suggests further that the ability to "pay"

> had become an important and necessary part of the Narragansett way of life (Robinson 1990:178-179).

Aside from the wampum, other native offerings declined both on a per-grave basis and overall in these later cemeteries, while European objects and materials became more prominent as grave goods (Turnbaugh 1984b). The nature and patterned distribution of these items reveals much about the changing nature of Narragansett society.

For unknown reasons, few adult males were among those buried at RI-1000. Males of all ages accounted for only 15 of the 43 interments that could be sexed. Ten of the males were adults or late adolescents; of these, nine were accompanied by grave goods. While all nine individuals with grave goods had received European items, only four of them were accompanied also by native products. As was the case with the wampum, just a few burials accounted for most of the offerings. Just one-third of the males (n=5), one aged between 15-17 (Burial 3) and the others aged 25-35 years (Burials 2, 15, 25, 38), retained fully 90 percent of the European goods associated with all male burials. These items included brass finger rings, seal-top spoons, knives, axes, hoes, and white clay pipes. Two of these richly-endowed burials (3, 15) also held traditional native-made offerings, including a bone or antler comb, graphite nodules, and a whetstone.

Significantly, wampum or wampum-making supplies were associated with four of these same five individuals. Shell blanks, a set of wampum drills, and an abrading stone accompanied Burials 3, 25, and 15, respectively. Burial 38 held a small quantity of white and purple wampum (38 beads), while nearly one-third of all wampum beads (n=848) recovered from the RI-1000 cemetery were associated with Burial 3, one of those discussed above. Given his young age (15-17 years) and the lavish amount of completed wampum bestowed upon him, Burial 3 may have been a member of a sachem's lineage.

The strong correlation between wampum *production* activities and European goods in the remaining four graves is suggestive of a different source of wealth. These features held considerable quantities of European goods, plus (in three cases) the equipment for producing the wampum with which to buy such goods. Each was the grave of a young man who had died before reaching 35 years, probably sometime in the 1660s. They probably typified the members of Narragansett trading expeditions as first described by Williams two decades earlier (1936 [1643]:159, 163). It seems very likely that Burials 2, 15, 25, and 38 represent the graves of those who had been adept and active traders with the English.

SUMMARY

Narragansett leaders adapted the inherent advantages of their traditional social station to fit the new socio-political environment in which they and the Europeans interacted during the mid-seventeenth century. They successfully modified community-oriented transactions so as to assert more control over wampum and divert other economically valuable goods to themselves and their immediate followers. For others, an alternative route to personal wealth and status was through a new commodity-oriented commercial relationship with European traders.

REFERENCES CITED

Bartlett, John R., ed.
1963 Letters of Roger Williams, 1632-1682. *The Complete Writings of Roger Williams* 6. Reprint of 1874 edition. Russell and Russell, Inc., New York.

Brenner, Elise M.
1984 Strategies for Autonomy: An Analysis of Ethnic Mobilization in 17th Century Southern New England. Ph.D. dissertation, Department of An-

thropology, University of Massachusetts, Amherst. University Microfilms, Ann Arbor.

Brereton, John
1966 *A Briefe and True Relation of the Discovery of the North Part of Virginia.* Reprint of 1602 edition. Readex Microprint Corporation, Ann Arbor.

Ceci, Lynn
1977 The Effect of European Contact and Trade on the Settlement Pattern of Indians in Coastal New York, 1524-1665: The Archaeological and Documentary Evidence. Ph.D. dissertation, Department of Anthropology, City University of New York. University Microfilms, Ann Arbor.

Chapin, Howard M.
1916 *Documentary History of Rhode Island.* Preston and Rounds Co., Providence.
1931 *Sachems of the Narragansetts.* Rhode Island Historical Society, Providence.

Cook, Lauren J.
1985 *The Rhode Island Burial Survey: Part One, A Study in Documentary Site Location.* Boston University Center for Archaeological Studies, Office of Public Archaeology Report of Investigations 28. Boston.

Cronon, William
1983 *Changes in the Land: Indians, Colonists and the Ecology of New England.* Hill and Wang, New York.

Gookin, Daniel
1970 *Historical Collections of the Indians of New England.* Reprint of 1792 edition. Towtaid, Boston.

LaFantasie, Glenn W., ed.
1988 *The Correspondence of Roger Williams.* Two volumes. Published for the Rhode Island Historical Society by Brown University Press/ University Press of New England, Hanover.

Mourt, G.
1966 *Journal of the English Plantation at Plimoth.* Reprint of 1622 edition. Readex Microprint Corporation, Ann Arbor.

Potter, Elisha R., Jr.
1835 *The Early History of Narragansett.* Rhode Island Historical Society Collection 3. Providence.

Quinn, David B., ed.
1979 *New American World: A Documentary History of North America to 1612.* Five volumes. Arno Press and Hector Bye, Inc., New York.

Robinson, Paul A.
1990 The Struggle Within: The Indian Debate in Seventeenth-Century Narragansett Country. Ph.D. dissertation, Department of Anthropology, State University of New York, Binghamton.

Robinson, Paul A., and Gail Gustafson
1983 A Partially Disturbed 17th Century Indian Burial Ground in Rhode Island: Recovery, Preliminary Analysis, and Protection. *Connecticut Archaeological Society Bulletin* 45:41-50.

Robinson, Paul A., Marc A. Kelley, and Patricia E. Rubertone
1985 Preliminary Biocultural Interpretations from a Seventeenth-Century Narragansett Indian Cemetery in Rhode Island. In *Cultures in Contact: The Impact of European Contacts on Native American Cultural Institutions, A.D. 1000-1800*, edited by William W. Fitzhugh, 107-130. Smithsonian Institution Press, Washington, D.C.

Rubertone, Patricia E.
1989 Archaeology, Colonialism and 17th-Century Native America: Towards an Alternative Interpretation. In *Conflict in the Archaeology of Living Traditions*, edited by R. Layton, 32-44. Unwin and Hyman, London.

Simmons, William S.
1970 *Cautantowwit's House: An Indian Burial Ground on the Island of Conanicut in Narragansett Bay.* Brown University Press, Providence.
1978 Narragansett. In *Handbook of North American Indians, Vol. 15, Northeast*, edited by Bruce G. Trigger, 190-197. Government Printing Office, Washington, D.C.

Turnbaugh, William A.
1984a *The Material Culture of RI-1000, A Mid-17th Century Narragansett Indian Burial Site in North Kingstown, Rhode Island.* University of Rhode Island, Kingston.
1984b Sociocultural Significance of Grave Goods from a 17th Century Narragansett Cemetery. Paper presented at Annual Meeting of Society for American Archaeology, Portland, Oregon.

Weeden, William B.
1978 *Economic and Social History of New England, 1620-1789.* Reprint of 1890 edition. Corner House Publications, Williamstown.

Williams, Roger
1936 *A Key into the Language of America.* Reprint of 1643 edition. Rhode Island and Providence Plantations Tercentenary Commission, Providence.

Wood, William
1977 *New England's Prospect.* Reprint of 1634 edition. University of Massachusetts Press, Amherst.

Woodward, Carl
1971 *Plantation in Yankeeland.* Cocumscussoc Association, Wickford, Rhode Island.

Wroth, Lawrence C., ed.
1970 *The Voyages of Giovanni da Verrazzano: 1524-1528.* Yale University Press, New Haven.

18 The Newport Tower: Revisiting New England's Fantastic Archaeology

Steven R. Pendery
Boston Landmarks Commission and
Peabody Museum, Harvard University

In his recent study of American "fantastic archaeology," Stephen Williams calls attention to a relationship between the increase in fringe archaeological literature and the general absence of popular studies of North American prehistory (Williams 1991:5-6). The success of fantastic explanations for archaeological phenomena and of authors Barry Fell and Eric Von Daniken in particular is representative of this situation. The response of professional archaeologists and scientists, including White (1974), Cazeau and Scott (1979), Radner and Radner (1982), Feder (1990), as well as Stephen Williams (1991), has been to confront and to debunk a seemingly endless stream of irrational explanations of archaeological phenomena. Admirable as these studies are, fertile environments for pseudoscience persist because of the general absence of popular archaeological literature addressing topics of public interest. Nowhere is the need for this type of literature so great as in New England.

New England has a long tradition of fantastic archaeology because of its geographic relationship to Europe and presence of sites with enigmatic architectural and epigraphic features, usually more apparent than real. Fantastic archaeological site architecture tends to be of two general categories: (1) unique constructions such as Mystery Hill and the Newport Tower, and (2) more generic forms of megalithic architecture, including the stone chambers of interior New England. These sites and their inscriptions provide most of the "evidence" for Ibero-celtic, Phoenician, and Viking contact with New England (Fell 1976, 1980). Alternate explanations have been offered that point to various origins of such "megalithic" structures as root cellars and building foundations of the late 18th and early 19th century (Vescelius 1956; Neudorffer 1979). The Newport Tower, the subject of this paper, was tested and shown beyond dispute to have a colonial origin by William Godfrey (1951a, 1951b). There are in fact no authenticated artifacts that constitute proof of pre-Colonial, Old World contact with New England, other than a Norse coin found at the Goddard site in northern Maine (Skaare 1979).

New England is unable to shake its association with the fantastic in part because these "controversial" sites fall within the purview of the relatively new archaeological sub-field of historical archaeology. While archaeological study of historic sites in New England dates back at least as far as the 1850s (Deetz 1977:29), it was only in the 1960s that the first academic courses in it were taught and the Society for Historical Archaeology formed. Historical archaeologists have simply failed to recognize and to popularize the broader levels of significance of controversial sites in the context of the American frontier process, although Neudorfer (1979, 1980) is one exception. Blame is equally shared with the field of early American vernacular architecture, which has virtually ignored these non-domestic structures (Glassie 1970; Hubka 1984). The lack of systematically collected data and academic or popular studies involving these sites has directly contributed to the proliferation of New England fantastic archaeology.

The purpose of this paper is to examine the historic and cultural context of one of New England's best known "fantastic" archaeological sites, the Newport Tower, and to argue for its significance in the context of 17th-century garden and landscape history. Two general research questions are addressed: (1) is this site representative of idiosyncratic behavior as described by Deetz (1977:30-31) or a rare survival of more prevalent, patterned behavior, and (2) does it tell us anything new about the early American frontier process? A possible role for archaeological research to elucidate remaining questions is presented.

HISTORY AND ARCHAEOLOGY OF THE NEWPORT TOWER

Scholarly review of the Newport Tower is not readily found in textbooks of American architecture or archaeology except in the context of its disputed Norse origin. This situation is unfortunate, given that the best evidence to date indicates that the tower is one of New England's earliest masonry structures and perhaps America's only example of a 17th-century prospect tower.

The Newport Tower is located close to the center of Touro Park in downtown Newport, Rhode Island (Figure 18.1). The Tower is twenty-four and a half feet in height (7.46 meters) and of approximately the same outer diameter. A cylindrical top is supported by eight columns or legs, each of which is about seven and a half feet high (2.3 meters). Studies by Godfrey and others, undertaken before the Newport City Council banned excavations beneath the tower in 1955, revealed that today's soil grade is at least 18 inches (46.0 cm.) above the original (Godfrey 1951:129). The tower is constructed of field stone with clamshell mortar and there is evidence of at least two subsequent episodes of spackling and consolidation. Both interior and exterior surfaces show evidence of pargeting, or covering with mortar, that may have been finished with a finer plaster. Each column rests on a substantial stone drum. The tower interior contains apertures including three windows and four smaller openings. Also present are six niches, beam holes, stair tread holes, and a fireplace with two 6 x 8 inch (15.2 x 20.3 cm) flues that open on the tower's exterior surface.

The earliest documentary reference to the Newport Tower is contained in a February, 1677 deed for land for a Jewish cemetery that refers to "ye Stone Mill." The next reference is contained in the 1677 will of the first governor of Rhode Island, Benedict Arnold. In this he refers to "my Stone Built Wind-miln" and "my Stone-built Wind-Milln." Another 1677 reference may be found in the Governor's record of the death of his granddaughter, Damaris Golding, who died "and lyeth interred under a tombe in my land between my dwelling house and stone wind mill." Arnold owned a substantial area of land surrounding the tower and extending to the harbor. In 1740 a son-in-law of Arnold's, Edward Pelham, mentioned the tower in his will, and in 1767 his son-in-law devised it to his sons. Gilbert Stuart, a Rhode Islander, painted a view showing the tower apparently being used as a hayloft between 1770 and 1775. Between 1776 and 1779 the British occupied Newport and apparently used the mill as a powder house. As they departed in 1779, a charge was detonated inside the tower (which may explain why so little of the original pargeting is left intact).

The Newport Tower attracted little attention during the early nineteenth century, but in 1823 it was the subject of several articles in the Providence *Gazette* that concluded with a "debate" whether it was a windmill or an ancient tower. In his thorough review of the history of this debate, Philip Ainsworth Means (1942) detects a "rising tide of romantic speculation," especially from New Yorkers. Doctor Thomas Hopkins Webb (1801-1861), secretary of the Rhode Island Historical Society, was mainly responsible for developing the theory of Viking origins. After moving to Boston in 1838, Webb sent a series of letters describing the tower to Charles C.

Figure 18.1. View of Newport Tower from the West, November 1991 (photograph by Pendery).

Rafn, who in 1837 had published his *Antiquitates Americanae sive scriptores Septentrionales rerum ante-Columbianarum in America.* The descriptions so impressed Rafn that he uncritically cited in the second edition of *Antiquitates Americanae* (1839) the tower, along with the nearby Dighton Rock, as evidence for twelfth-century Viking presence in the Narragansett Bay. Rafn inaugurated the prototype method of study, whereby European antecedents for the tower were documented. The origin of the Newport Tower as Governor Arnold's windmill was suddenly thrown into doubt by Rafn, although local scholars remained unconvinced, as both documentation and oral history supported the windmill hypothesis (Melville 1847). The tower was of considerable public interest during the 1840s due in part to Longfellow's reference to it in his "Skeleton in Armor" poem based on the 1832 discovery of a skeleton accompanied by metal artifacts in nearby Fall River. Documentation of the tower improved considerably, including sketches by artist Frederick Catherwood and many others. An 1854 bequest allowed the City of Newport to preserve the tower in place within Touro Park where it remains today.

Doubts surfaced about the Governor Arnold windmill theory because the tower was unlike any tower windmill in either the Old or New World, with one exception. David Melville, of the Newport Historical Society, first called attention to the only similar structure in the Western World, Chesterton Mill in Warwickshire, England, in a letter to the *Newport Herald of the Times and Rhode Islander* (Melville 1847). His observation about the similarity of the Newport and Chesterton towers was probably based on a crude illustration of the Chesterton example. This cylindrical tower, supported by six legs, was constructed by Inigo Jones or one of his students as an observatory for Sir Edward Peyto (Figure 18.2). The tower probably complemented several other improvements to his manor house and garden. Sir Edward died in 1643, and inspection of the structure by windmill expert Rex Wailes in 1937 suggested that it had easily been converted to a windmill by the 1730s because of its original revolving roof (Means 1942:183-187). It was widely and incorrectly believed that Benedict Arnold was originally from a town near Chesterton and that he used the mill as a model for his stone windmill that replaced Peter Easton's mill when it blew down in 1675. In 1858 John Gorham Palfrey (1796-1881) published the Arnoldian windmill hypothesis in his three-volume *History of New England* (Palfrey 1858-1864). The Arnoldian and Norse theories were fully defined at that point, with the former enjoying popular support.

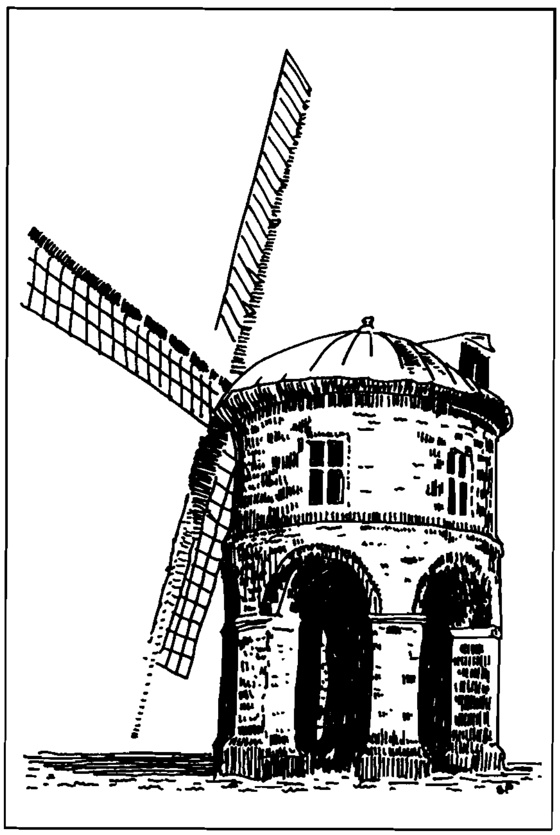


Figure 18.2. Chesterton Mill, Warwickshire, England.

A second flurry of interest in the Newport Tower occurred immediately prior to World War II, marked by the 1942 publication of a book on the topic by Philip Ainsworth Means, a specialist in South American archaeology. Means presents a pro-Norse origin for the tower, which he buttresses with a critical evaluation of the Arnoldian theories and by presenting comparative data on Norse churches. Means presents a thorough review of Newport Tower literature, dwells on the deficiencies of the Arnoldian theories, and uses this to develop his case for a Norse origin. His ten "nails in the coffin" of the Arnoldian theory include the fact that Benedict Arnold was probably not familiar with the Chesterton Mill, and if he had been, it had not yet been converted to a windmill before 1635. Also, the manpower shortage in Newport during King Philip's War (1675-1676) would have made it impossible for Arnold to build a mill from the ground up to replace Newport's only windmill owned by Peter Easton. Means expresses the belief that Governor Arnold and a few elderly workmen could easily have converted an existing stone tower into a windmill (Means 1942:192).

Philip Means and Hjalmar R. Holand (of Kensington rune stone fame) supported each other's theories about the Norse presence in the United States. This is especially apparent in Holand's 1946 publication, *America 1355-1364*, which cites the Newport Tower as evidence. As Means pointed out in 1942, archaeological fieldwork was one way of determining the origins of the Newport Tower once and for all. Accordingly, a committee of the Society for American Archaeology was formed, consisting of J.O. Brew, Hugh Hencken, Philip Phillips, Junius Bird, Singleton P. Moorehead, and Frederick Johnson. The project was sponsored through the Preservation Society of Newport County over a period of two years, in 1948 and 1949. A Harvard graduate student in archaeology, William S. Godfrey, Jr., was selected to direct the excavations under the supervision of Hugh Hencken (Godfrey 1951:123).

During the 1948 field season, Godfrey and his enthusiastic but inexperienced crew excavated a trench 3 feet (1 meter) wide directly beneath the tower and extending 75 feet (23.0 meters,) beyond it in a north-northwest and south-southeast direction (Godfrey 1951a:69). The intention was to intercept an outer, concentric foundation should the structure be a Norse church. No such foundation was uncovered, and no artifacts earlier than the colonial period were found. There was significant disturbance in the tower center from earlier excavations of pits. Godfrey was able to identify the method of foundation construction, and found only colonial period artifacts in direct association with the foundation stones (Godfrey 1951a: Figure 21). The 1949 field season focused strictly on the area beneath the tower itself. An original and unduly complicated excavation plan was implemented involving trenches and trench fragments radiating from the tower center (Figure 18.3). Trenches were, for the most part, excavated in 1/4-foot arbitrary levels, and "repeated attempts to excavate on so-called "natural levels" were unfortunately failures." The site stratigraphy depicted in Godfrey's report was apparently based on "study of the trench faces and repeated cleanings" (Godfrey 1951a:70, 1951b:127). Because of the presence of numerous pit features, Godfrey was aware of the problem of contamination but could do little about it unless features were obvious. Except for the area beneath the gateway entry in the iron fence surrounding the tower, nearly the entire area beneath the tower was excavated, leaving no undisturbed portion of the site for later investigators.

Godfrey's research design was directed toward establishing whether or not the Newport Tower had a colonial origin. During the first season, the results were affirmative, and subsequent excavation even

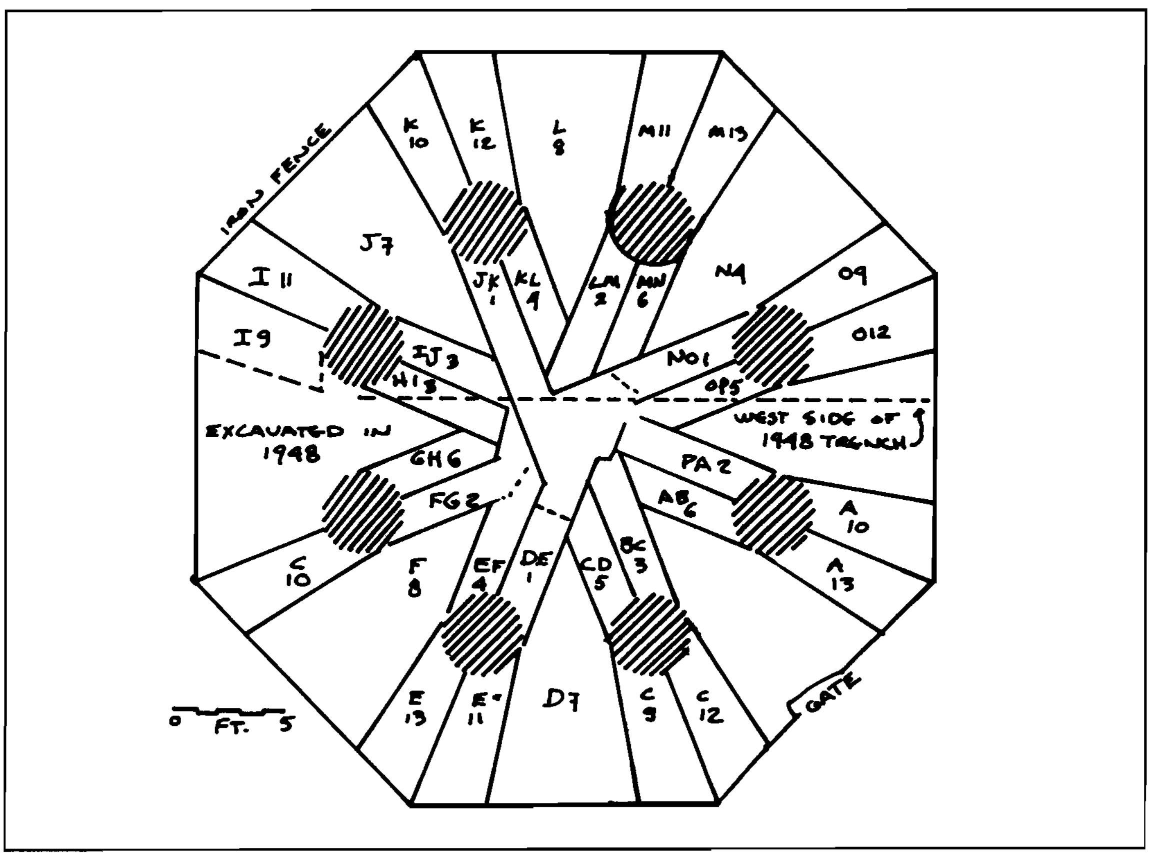


Figure 18.3. Layout of completed trenches (after Godfrey 1951).

yielded fragments of millstones (Godfrey 1951b: 128). Establishing a more exact construction date proved to be more difficult. First, relatively few artifacts were uncovered in direct association with lower elevations of the foundations. Second, Godfrey's own training did not prepare him for dating historic artifacts in other than a very general way. J.C. Harrington examined the artifact collection and helped in identification. No inventory of artifacts has been located for Godfrey's excavation, but the artifact collection located today at the Newport Historical Society displays numerous vague or misleading identifications. Pearlware is not correctly identified, nor is common New England lead-glazed red earthenware.

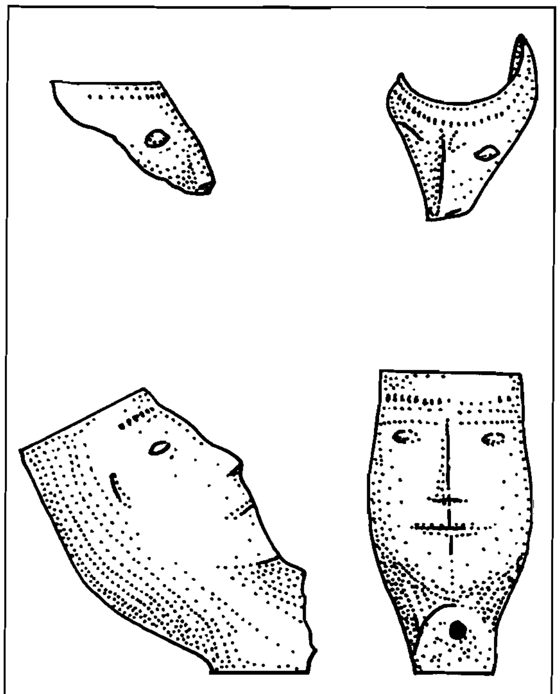


Figure 18.4. Sir Walter Raleigh or Jonas clay tobacco pipes: Newport Tower (top) and James Garrett site, Charlestown, ca 1640-1660 (bottom). Height of bottom pipe is 1 9/16 inches (4.0 cm.).

Three problems must be addressed in order to assign a specific date range for the construction of the Newport Tower from Godfrey's data: (1) the lack of direct correspondence between artifacts excavated by Godfrey in arbitrary 3-inch levels and the natural levels and pit features shown in the site profiles, (2) the paucity of diagnostic artifacts in key locations, and (3) the questionable artifact attributions by both Harrington and Godfrey and the lack of a complete artifact collection today. Study of the existing collection is informative, however, as the labeled artifacts may be dated and identified as to their horizontal and vertical provenience. The earliest artifacts represented are tobacco pipe bowl fragments. Six of these are of seventeenth-century forms, and one of these represents a "Jonas" or "Sir Walter Raleigh" pipe variant that has been found in New England archaeological deposits dating to the period 1640 to 1660 (Figure 18.4; Pendery 1987: 156; Brown 1987:169). These and other pipe bowl fragments from the third quarter of the seventeenth century are distributed within seven feet (slightly over two meters) from the tower in trench H, sections S, T, U and W. As Trench H was cut across the park, this indicates a small concentration of early pipes near the tower. No other artifacts from this period were identified at the site. A wide variety of eighteenth-century artifact types were represented, however, including numerous fragments of French Rouen ware, English white salt-glazed stoneware, local lead-glazed red earthenware, and the granite millstone fragments.

This updated assessment of Godfrey's archaeological study of the tower suggests a construction date in the 1660s at the latest, and a relatively low intensity of use involving the preparation and consumption of food until the early eighteenth century. This is consistent with the specialized uses of the structure as described in the mill, hayloft, and garden folly theories. The high frequency of post-1780 artifacts appears to correspond with the aftermath of the attempted destruction of the tower by the British. Subsequently, refuse accumulated at the site even after public access was restricted by fencing after 1855.

ARNOLD'S PROSPECT TOWER

Godfrey's research supported a colonial origin for the tower and laid to rest any serious speculation about Norse origins, although this "debate" continues down to the present day (Pohl 1966; Penhallow 1991). In his 1951 Harvard Ph.D. dissertation Godfrey expresses the opinion that the structure was originally built as a garden folly by Governor Arnold following the Chesterton model and that its medievalisms were to be explained in this context. This echoes a similar conclusion reached by the architectural historian Henry Russell Hitchcock in 1939. By careful evaluation of the same evidence available to Means and without the benefit of archaeological evidence, Hitchcock concluded: "it seems more probable now that it [the Newport Tower] is the earliest New England structure to reflect in its general design the new academic architecture of seventeenth century England. For it certainly ressembles the observatory that Inigo Jones erected in 1632 for Sir Edward Peyto, at Chesterton in Warwickshire" (Hitchcock 1939:15).

In his concluding chapter to *Fantastic Archaeology*, Stephen Williams describes North American prehistory as the "real" fantastic archaeology. In like fashion, the truth behind New England's "fantastic" sites can be at least as interesting as the thread-worn theories of Barry Fell or even Philip Ainsworth Means. As of 1991, the best evidence suggests that the Newport Tower represents one of two extant examples in the Western World of a specialized type of garden building, possibly New England's only surviving direct link with northern European Renaissance architecture, and one of the earliest masonry buildings in the region. At this point the interesting and significant research should begin, and not end (as it apparently has).

In his study of the English Renaissance garden, Roy Strong examines the meanings of the formal garden:

> It was a symbol of pride and an expression of royal and aristocratic magnificance: man conquered the earth, tilled and planted it, subjecting it to his will. By means of the garden we can follow the change in attitudes to the natural world as the viewer studied its contents . . . the magical world of the late Renaissance, with its preoccupation with occult forces and influences, gives way to the age of experiment and of the Royal Society . . . . The garden evolves from a series of separate, enclosed, emblematic tableaux to a sequence of interconnecting spaces whose vital link is the vista and *point de vue* (Strong 1979:11).

Two points made by Strong may be directly relevant to the interpretation of the Newport Tower. The first involves the relationship between political power and gardens. Since the time of Henry VIII in England and Francis I in France, the palace garden was a symbol of royal power and prestige (Strong 1979:101). This was initially expressed by a display of heraldry on garden walls and elsewhere. Until the late 17th century, formal gardens of aristocrats usually consisted of a series of conjoined, walled enclosures that was particularly conducive to this heraldic display (Taigel and Williamson 1991:6). Garden buildings, including towers, follies, grottos, and summer houses, were featured in addition to garden plots separated by walkways.

A second point made by Strong is the ephemeral and transitory nature of gardens. Planting materials are, of course, highly ephemeral, but so are garden buildings, terraces, and walkways susceptible to rot and erosion following even brief periods of neglect. Even more destructive was the frequent remodeling of gardens. Renaissance gardens were subject to change by the addition of elements such as grottos, fountains, and automata. But the craze for the English garden of the type designed by Capability

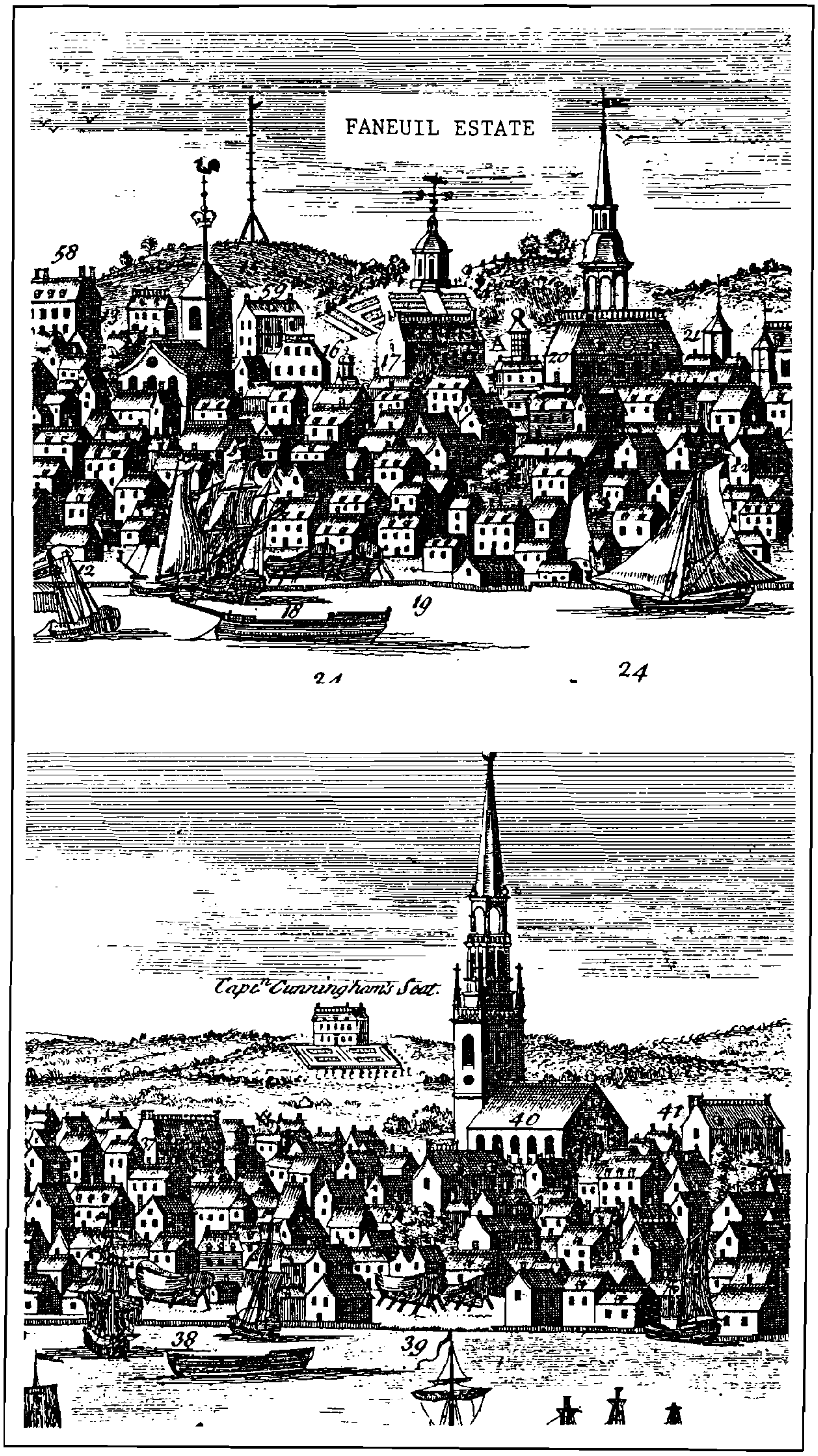


Figure 18.5. 1743 view of Boston by William Price showing Faneuil estate on Pemberton Hill (top) and Cunningham estate in Brighton (bottom).

Brown from the 1720s onward swept away virtually all earlier formal English gardens. There are even few representations of Renaissance gardens left (Strong 1979:11; Taigel and Williamson 1991:6). Our lack of familiarity with these early gardens and their architectural features is not surprising for these reasons.

In colonial North America formal gardens have been linked with the aristocratic pretensions of planter and merchant classes, and archaeology has played an important role in the study of this phenomenon (Yentsch *et al.* 1987; Leone 1984). Some of the earliest archaeological studies of early formal gardens were undertaken by the Colonial Williamsburg Foundation (Noel-Hume 1974) and more recently at Mount Vernon and at Bacon's Castle in Virginia (Kelso 1989; Luccketti 1989). In the 18th-century Paca garden in Annapolis, Maryland, an allegorical relationship between William Paca's ascent to the governorship and the development of his formal garden has been argued by Leone (1984). Formal gardens of New England's merchant class tend to be poorly preserved because of continuous urbanization in and around Newport, Providence, Boston, and Portsmouth. Formal garden plans of first generation colonists are virtually unknown despite the survival of some seed lists (Leighton 1986:190). However, archaeological study of a late 17th-century Charlestown, Massachusetts site of the houselot and garden of Province Treasurer Jonathan Phillips revealed a spacious estate consisting of house, garden, and orchard situated on the edge of town (Pendery 1987:266-269). The 1737 Thomas Hancock House on the south side of Boston's Beacon Hill contained terraces or glacis climbing to the summit (Watkins 1926:7). Eighteenth-century views of Boston show country seats with formal parterres, such as the Cunningham estate in Brighton and the Faneuil estate on Pemberton Hill (Figure 18.5).

Benedict Arnold's ascent to power in the New World and the political importance of formal gardens may help to explain why the Newport Tower was built. His family was from Ilchester in Somerset, England, where Benedict was born in 1615. His grandfather, Nicholas, was a tailor by trade, but we know little about his father, William. The Arnold family arrived in Hingham, Massachusetts in 1635 when Benedict was nineteen years old. After a period of adjustment involving a move to Pawtucket and then to Providence, Benedict rose to a position of affluence. He was a skilled speaker of Algonquian and probably was involved in the Indian trade. He was taxed in Providence as the richest man in the entire colony. He moved to Newport in 1651 and became politically active as an Assistant between 1655-1661, and as President of the Colony from 1657 to 1663. He was named Governor in 1663 with the arrival of the New Charter from King Charles II. He died in Newport on June 19, 1678 (Arnold 1935:53).

Shortly before he moved to Newport in 1651, Benedict Arnold bought land there from Jeremy Clarke fronting Thames Street facing the harbor. This 16-acre property is today defined by Thames Street, Bellevue Street, Mill Street, and Prospect Hill Street (Figure 18.6). Arnold's property was possibly divided into two components. The lower parcel fronted the harbor, extended east to the location of today's Spring Street, and contained his stone fort with two cannon, warehouses, wharf, house, and garden. The upper parcel extended between Spring Street and Bellevue Avenue and contained a three square rod family burial plot and Arnold's "stone built windmill." A line or path connected the house and windmill according to Arnold's 1677 will (Arnold 1935:54-55).

Arnold built his house about 1651 and set it back from the street, possibly to accomodate a garden or entry gate with driveway. The house, which may have been a Rhode Island "stone-ender," had at least one substantial stone side that resisted demolition in 1780. The extent of Palladian influence on the Arnold mansion is unknown; however, Boston's first Palladian house dates to 1688. Local legend was that the windows had bars similar to those of a prison and

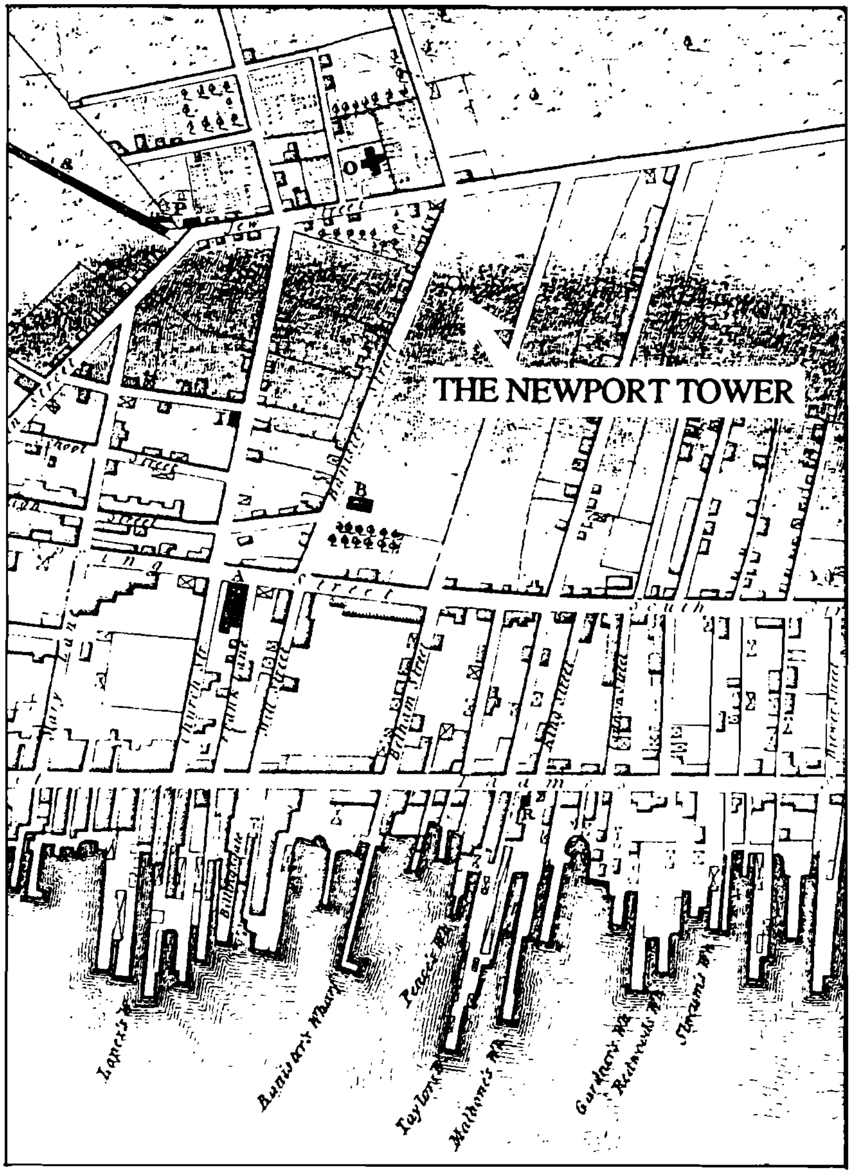


Figure 18.6. Location of the Newport Tower on Fadden map of 1777.

that there were tall figures of some sort on the property's fence posts (Brooks 1851:67-68).

The linear sequence of mansion house, formal garden, and parkland was not uncommon in the planning of early eighteenth-century British country seats on both sides of the Atlantic. Johannes Kip captured the more notable English examples in a series of engravings dating to 1707 (Harris and Gervase-Stops 1984). In several of these the house, garden, and surrounding parkland are integrated by rigid bilateral symmetry and the use of a great avenue set axially to the main house, much the same as at Versailles. Garden walls are used for espalier and for enclosing garden beds and ponds. Beyond the gardens, sylvan parks and avenues stretch out to the horizon (Figures 18.7 and 18.8).

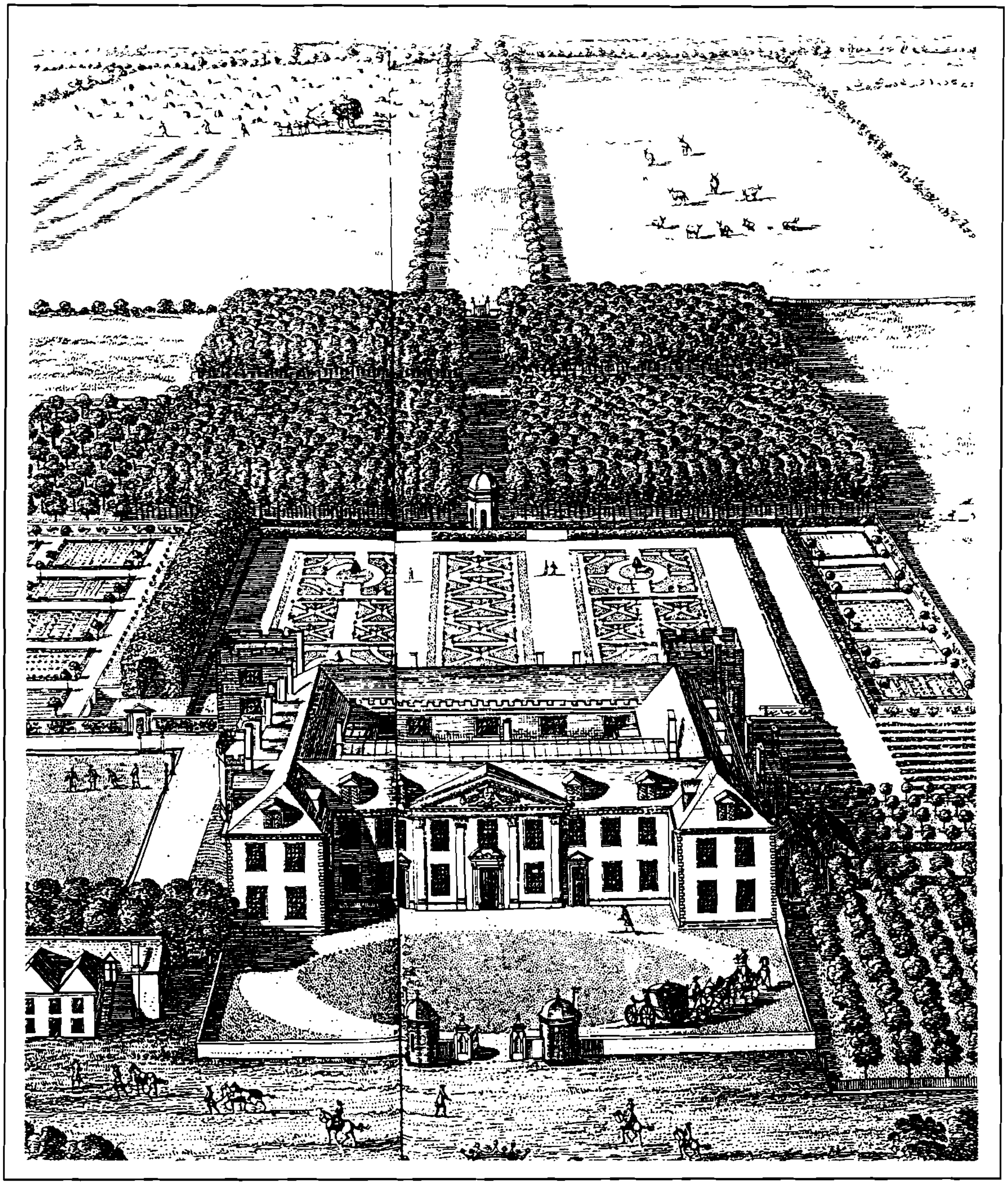


Figure 18.7. Estate of Robert Earl of Lindsey by Kip (1707).

Kip's engravings portray a variety of garden towers used in formal landscapes about thirty years after the Newport Tower was built. In Renaissance gardens towers were usually found in the corners of walled enclosures. Garden towers apparently helped to elevate a visitor to a vantage point over the garden. By 1700 garden towers were set on flat ground, on raised terraces, and on the tops of

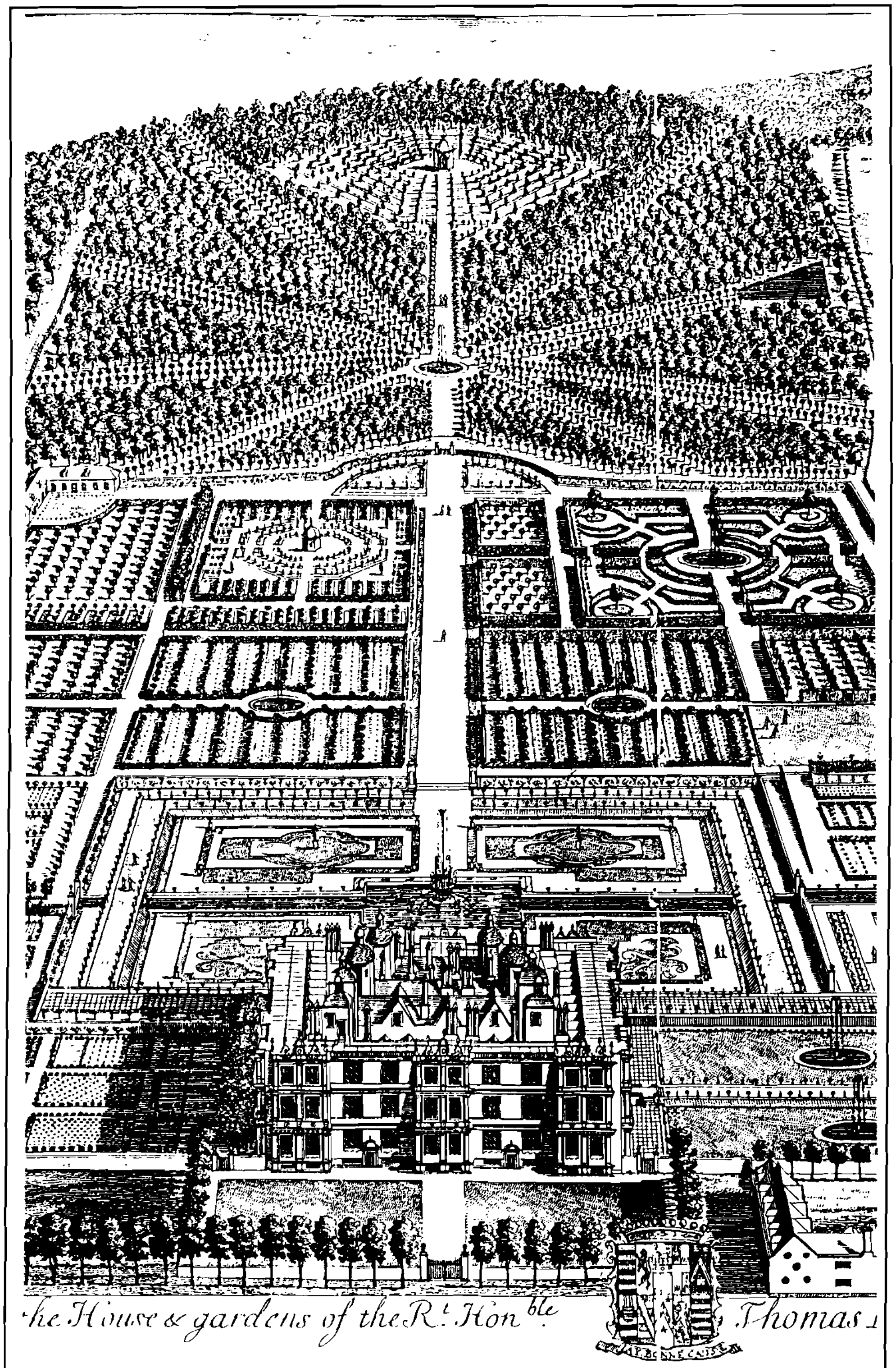


Figure 18.8. Estate of Thomas Lord by Kip (1707).

mounds. They were often round or polygonal, two- or three-storey masonry structures supported by columns or arcades. Fenestration, naturally, was oriented toward points of interest. By 1700 these towers were calling more attention to themselves as interesting focal points in garden planning.

Prospect towers are evident in Kip's engravings, but the majority of surviving examples date after 1740, at least in Scotland where such buildings have been subject to scholarly review (Buxbaum 1989: 155-161). These towers were located on hilltops at a distance from the main house and would provide a visual focus to the skyline in addition to a view over the estate and shelter to those in need. Short towers containing statuary were apparently inspired by Roman and Greek temples (Figure 18.9). The interior and exterior pargeting of the Newport Tower may have been an attempt to give it the appearance of marble or limestone as an allusion to classical antiquity.

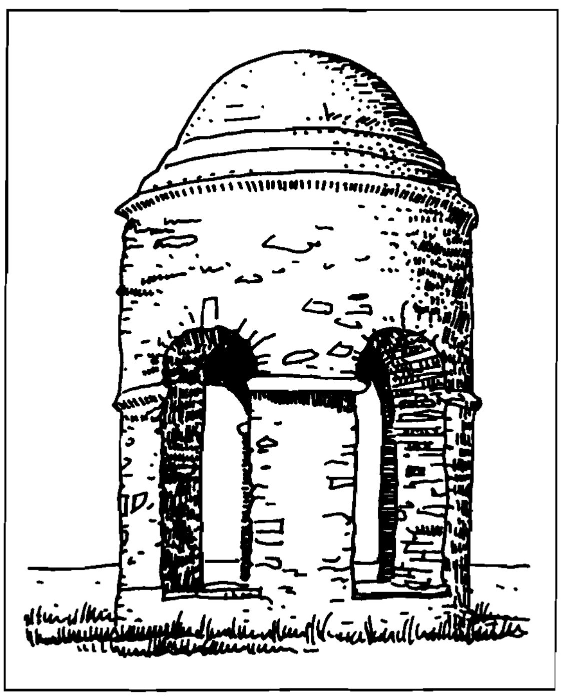


Figure 18.9. Temple of Venus crowning Doune Hill above Duff House, Scotland (after Buxbaum 1989:158).

Governor Arnold's prospect tower appears to be an unusually precocious architectural statement of his ascent to the pinnacle of power in an American colony. It is the sole surviving example of formal estate architecture of seventeenth-century New England and one of possibly two seventeenth-century towers of its type in the English-speaking world. The tower is all that is left of Governor Arnold's improvements to his sixteen-acre property unless future archaeological work identifies other areas of Touro Park where they might be preserved in the form of walkways, garden walls, or planting beds. During a time of tremendous economic and social stress due to King Philip's War, the tower was converted into a windmill as a magnanimous act of an astute politician. Ironically, a structure that received virtually no commentary in its day will be associated with Arnold's name long after his other contributions are forgotten.

CONCLUSIONS

This brief essay has examined New England fantastic archaeology with respect to three issues. The first is that historical archaeologists have a responsibility to address controversies that surround New England fantastic archaeology sites. Second, many New England fantastic sites, either unique constructions as at Mystery Hill or generic building types such as Vermont stone chambers, may provide important information on the New England frontier process. These sites may challenge assumptions that survival alone was of paramount importance to first generation settlers, that architecture was strictly utilitarian, that all gardens were for food production, and that "megalithic" or medieval-style buildings must have an Old World origin. Third, as Stephen Williams and others have indicated, one of the real dangers of fantastic archaeology is that it denies groups their actual heritage by offering absurd explanations for archaeological phenomena. In the case of the Newport Tower, a century of speculation about Norse origins detracted from the painstaking comparative research that would have identified it as an architectural component of Governor Arnold's Newport estate, one of the earliest masonry structures in New England, and one of the few surviving examples of a seventeenth-century prospect tower in the Western Hemisphere.

ACKNOWLEDGMENTS

Stephen Williams, Bruce Bourque, and the Committee for Research on Norse Activities in North America provided the inspiration for this study. The author thanks Mayor Robert J. McKenna and Susan Cooper of the City of Newport for access to the Newport Tower and its artifacts, the Newport Historical Society, the Office for Public Archaeology at Boston University, Dr. James Stoltman, and, most of all, Elisa and Lily.

REFERENCES

Arnold, Elisha Stephen
1935 *The Arnold Memorial: William Arnold of Providence and Pawtucket, 1587-1675, and a Genealogy of his Descendents.* The Tuttle Publishing Co., Rutland, Vermont.

Brooks, Charles T.
1851 *The Controversy Touching the Old Stone Mill.* Newport, Rhode Island.

Buxbaum, Tim
1989 *Scottish Garden Buildings: From Food to Folly.* Mainstream Press, Edinburgh.

Cazeau, Charles, and Stuart Scott
1979 *Exploring the Unknown: Great Mysteries Reexamined.* Plenum Press, New York.

Deetz, James F.
1977 *In Small Things Forgotten.* Anchor Books, New York

Feder, Kenneth
1990 *Frauds, Myths, and Mysteries: Science and Pseudoscience in Archaeology.* Mayfield Publishing, Mountain View, California.

Fell, Barry
1976 *America B.C.: Ancient Settlers in the New World.* Quadrangle/New York Times, New York.
1980 *Saga America.* Time Books, New York.

Glassie, Henry
1968 *Pattern in the Material Folk Culture of the Eastern United States.* University of Pennsylvania Press.

Godfrey, William S.
1951a Digging a Tower and Laying a Ghost: The Archaeology and Controversial History of the Newport Tower. Ph.D. dissertation, Harvard University, Cambridge.
1951b The Archaeology of the Old Stone Mill in Newport, Rhode Island. *American Antiquity* 12:120-129.

Harris, John, and Gervase Jackson-Stops
1984 *Britannia Illustrata: Knyff and Kip.* Paradigm Press, Bungay, England.

Hitchcock, Henry-Russell
1939 *Rhode Island Architecture.* Rhode Island Museum Press, Providence.

Holand, Hjalmar
1932 *The Kensington Stone.* Ephraim, Wisconsin.
1946 *America, 1355-1364.* Duell, Sloan and Pearce, New York.

Hubka, Thomas
1984 *Big House, Little House, Back House, Barn.* University Press of New England, Dartmouth, New Hampshire.

Leighton, Ann
1986 *Early American Gardens.* University of Massachusetts Press, Amherst.

Leone, Mark, and Paul Shackel
1984 Interpreting Ideology in Historical Archaeology: The William Paca Garden in Annapolis, Maryland. In *Ideology, Power and Prehistory,* edited by D. Miller and C. Tilley, 25-36. Cambridge University Press, Cambridge.

Luccketti, Nicholas
1989 Archaeological Excavations at Bacon's Castle, Surrey County, Virginia. In *Earth Patterns: Archaeology of Early American and Ancient Gardens and Landscapes,* edited by William Kelso, 23-42. University Press of Virginia, Charlottesville.

Means, Philip Ainsworth
1942 *Newport Tower.* Henry Holt, New York.

Melville, David
1847 Letter in *Newport Heralder of the Times and Rhode Islander,* June 21.

Neudorffer, Giovanna
1979 Vermont's Stone Chambers: Their Myth and Their History. *Vermont History* 47(2):79-147.

Noel-Hume, Audrey
1974 *Archaeology and the Colonial Gardener.* Colonial Williamsburg Archaeological Series 7, Colonial Williamsburg, Virginia.

Palfrey, John Gorham
1858-1864 *History of New England.* Little, Brown, and Company, Boston.

Pendery, Steven R.
1987 Symbols of Community: Status Differences and the Archaeological Record in Charlestown, Massachusetts, 1630-1760. Ph.D. dissertation, Department of Anthropology, Harvard University. University Microfilms, Ann Arbor.

Penhallow, William, and Michael Brennan
1991 Archaeoastronomy of the Old Stone Tower, Newport, R.I., USA. Ms. on file, Department of Physics, University of Rhode Island, Kingston.

Pohl, Frederick J.
1966 *The Viking Explorers*. Thomas Y. Cromwell, New York.

Radner, Daisie, and Michael Radner
1982 *Science and Unreason*. Wadsworth, Belmont, California.

Rafn, Carl C.
1837 *Antiquitates Americanae*. J.H. Schulz, Copenhagen.

Skaare, Kolbjorn
1979 An Eleventh Century Norwegian Penny Found on the Coast of Maine. *NNF-NYTT. Meddelelser fra Norsk Numismatisk Forening* 2:12-17.

Strong, Roy
1979 *The Renaissance Garden in England*. Thames and Hudson, New York.

Taigel, Anthea, and Tom Williamson
1991 Some Early Geometric Gardens in Norfolk. *Journal of Garden History* 11(1,2):4-77.

Vescelius, Gary S.
1956 Excavations at Pattee's Caves. *Bulletin of the Eastern States Archaeological Federation* 15:13-14.

Wailes, Rex
1948 *Windmills in England*. The Architectural Press, London.

Watkins, Walter Kendall
1926 The Hancock House and its Builder. *Old Time New England* 17(1):3-19.

White, Peter
1974 *The Past is Human*. Taplinger, New York.

Williams, Stephen
1991 *Fantastic Archaeology: The Wild Side of North American Prehistory*. University of Pennsylvania Press, Philadelphia.

Yentsch, Anne E., Naomi F. Miller, Barbara Paca, and Dolores Piperno
1987 Archaeologically Defining the Earlier Garden Landscapes at Morven: Preliminary Results. *Northeast Historical Archaeology* 16:1-29.

19 The Archaeology of Piersey's Hundred, Virginia, within the Context of the Muster of 1624/5

Norman F. Barka
College of William and Mary
Williamsburg, Virginia

INTRODUCTION

Historical Archaeology has long flourished along the James River in eastern Virginia at such seventeenth-century sites as Jamestown (Harrington 1952; Cotter 1958), Piersey's Hundred (Barka 1976), Kingsmill (Kelso 1984), The Maine (Outlaw 1990), Martins Hundred (Noel Hume 1991), Jordans Journey (Mouer pers. comm., 1992), Pasbehaigh (Lucketti pers. comm., 1992), and others. However, an important early document, the Muster of 1624/5, has been relatively neglected by archaeologists as a source of information about these and other early English settlements, especially when viewed in a comparative, holistic framework.

The present paper has several aims: (1) to examine and analyze the material culture listed in the first detailed census taken in North America, the Muster of 1624/5; (2) to describe the structural and artifact content of one archaeological site dating to this period, namely, Piersey's Hundred; and (3) to assess similarities and differences of settlements by a comparison of traits.

THE MUSTER OF 1624/5

Background

The Virginia Muster was a house-to-house inventory of people and provisions ordered by the English Crown after it took over management of the colony from the Virginia Company. Between 1607, the date of initial English settlement at Jamestown, and 1624/1625, the date of the Muster, English settlements had spread along most of the James River. The Muster, or census, was made by one or more individuals who visited each settlement and listed certain kinds of information, including names of inhabitants, ages, sex, race, date of arrival in Virginia, presence of children, place of birth, place of residence in Virginia, social position, household and family status, provisions available, etc. The census takers began with the uppermost reaches of the James River and proceeded downriver toward the Chesapeake Bay (Figure 19.1).

Irene Hecht (1973) was the first historian to analyze closely the information presented in the Muster, especially that related to the demography of the inhabitants; she did not deal with provisions and property holdings, which will be the main subject of this paper.

As Hecht and others have pointed out, the Muster is often difficult to deal with because similar information is often listed differently, and some data are contradictory. As with any census, some of the information is undoubtedly biased and incomplete. Whether or not all settlements were described accurately as to the presence or absence of certain features will never be known, except possibly through detailed archaeological research. The Muster is an incomplete document in another sense as well, due to the recent (March, 1622) Indian massacre in which approximately 25% of the colony's population had been killed and numerous settlements had been harmed or abandoned. These factors have to be kept in mind when working with the Muster.

Hecht's analysis of the Muster can be summarized briefly as follows:

> Altogether 1,216 people are listed as living in settlements along the James River, including 932 males (76.7%), 270 females (22.1%), and 14 unidentifiable as to sex. This was a young, largely male population, as 76.1% of those whose age can be determined were under 30; the largest group of females is under 14 and the next largest group is in the 20-24 age bracket. No females are over 39 years of age, whereas 63 males are in the 40 to over 50 age category. The population was overwhelmingly white, with only 23 Negroes and 2 Indians listed. The majority (1,085 or 89.1%) was born in England or Europe, while only 78 (6.4%) were children born in Virginia. Social titles are given for 62 people and 429 people (35% of population) are identified as servants.
>
> Three levels of social organization are evident: the muster, the household, and the family. The muster was a grouping that possibly had some military or defensive significance. The 187 musters (as listed by Hecht [1973]) varied in size from 34 one-man musters to one that had 70 individuals. Most musters were headed

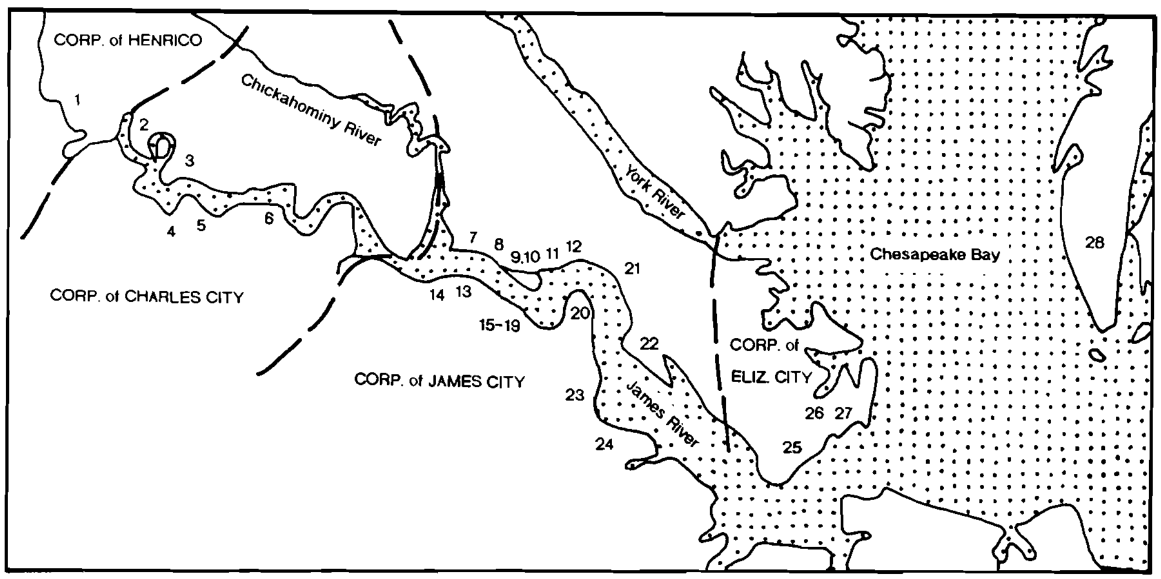


Figure 19.1. Map showing approximate locations of settlements reported in the Muster of 1624/25.

1. College Land
2. Neck of Land
3. West & Shirley Hundred
4. Jordans Journey
5. Chaplains Choice
6. Pierseys Hundred
7. Pasbehaighs
8. The Maine
9. James City
10. James Island
11. Neck of Land
12. Archers Hope
13. Burrows Hill
14. Paces Paines
15. Smiths Plantation
16. Blaneys Plantation
17. Mathews Plantation
18. Crowders Plantation
19. Treasurors Plantation
20. Hog Island
21. Martins Hundred
22. Mulbury Island
23. Wariscoyack
24. Basses Choice
25. Newport News
26. Elizabeth City
27. Company Land
28. Eastern Shore

by one person; 46 of 62 socially titled individuals were muster heads. The household is defined by Hecht as a group of people sharing in some way a common store of provisions and property, and such household groups can be identified by an associated listing of goods. A muster and household were often the same; sometimes households were subdivisions of a muster. The 308 households listed averaged 3.9 persons each, although sizes ranged from 1 to 36, and about half of the population was located in households of five persons or less. Half of the households that had servants had only one or two, but more than half of the servants were concentrated in 14 households. The third level of social organization is the family. Although persons listed in the census belonged to a muster or household, less than half had family with them in the colony. The nuclear family was the basic unit of family life.

In 1625 Virginia was divided into four "corporations": Henrico, James City, Charles City, and Elizabeth City (Figure 19.1). With the exception of Henrico, each corporation contained a number of separate settlements, for a total of 28 (Hecht lists 30 settlements, as she separates the Governor's Plantation at Paspahaigh from the general Paspahaigh settlement and counts Dr. Pott's as a separate settlement; these divisions are not indicated as separate in the margins of the document). The majority of the population lived in the James City corporation, followed by Elizabeth City, Charles City, and Henrico. The largest individual settlements by far were Elizabeth City (254 people) and James City (125). In terms of numbers of separate named or identified settlements, James City had 18, Charles City had 5, Elizabeth City 4, and Henrico 1.

Provisions and Property

In addition to data on people, the Muster is a source of information on the material culture and provisions of the early seventeenth-century settlements. Certain provisions and properties are listed, and quantities are given. For example, the Muster of Amias Bolte of West and Shirley Hundred plantation is listed as follows:

> THE MUSTER OF AMIAS BOLTE
>
> Amias Bolte aged 23 years [arrived] in the Neptune in August 1618. PROVISIONS: Corne, 8 bushells. ARMES AND MUNITION: Powder, 1 lb; Peece fixt, 1; Coats of Male, 2 and a headpeece. SWINE AND POULTRIE: Swine, 2; Poultrie, 12.

It is probable that these are selective data, i.e., information deemed important to the compiler. For example, pottery, glassware, and many other artifacts found archaeologically on these early seventeenth-century sites are not mentioned in the muster.

Although all settlements are separated from one another by the terms "The Muster of the Inhabitants of [specific settlement]," the census divides a majority of settlements into individual musters. In total, the Muster is divided into 179 separate musters, of which 168 are named for individuals, 10 are musters of servants or "men" of individuals ("The Muster of Mr Abraham Piersey's Servants"), and one is designated as "The Muster of the Thomas Keie Kompany"). Most musters occur in Elizabeth City Corporation (89/50% of total), followed by Charles City (64/36%), James City (26/14%), and College Land in Henrico (0). Although James City Corporation had the largest number of named settlements (18), muster organization was not as important in this corporation as in Elizabeth City and Charles City (Table 19.7).

There are 107 terms used in the Muster to denote provisions and property. They can be grouped into five general categories: (1) armor; (2) weapons; (3) food and provisions; (4) buildings and fortifications; and (5) transport related. Each category of terms will be listed, identified (where necessary) and analyzed, with frequency of occurrence and distribution being noted.

(1) There are 18 terms for *Armor*: armors and coates; armors steele coats and coats of male; armours; armour complett; buffe coat; coat of male; coat of plate; coat of steele; coats of male, headpeece; coats quilted; corslett; corslett complete; head peece; jack-coat; jacketts; jacks; quilted coats; steele coat.

All terms refer to some form of armor, which was body or head gear, made of iron and/or cloth or leather, designed to protect the wearer against weapons or missiles. Some terms refer to one type of armor, while other terms lump two or more varieties, such as "armors steele coats and coats of male." Different terms on occasion probably refer to the same or similar forms of armor, such as "armours" and "coat of steele" or "steele coat." Some definitions of lesser-known items are necessary: "coat of plate" refers to armor made of steel plates, as distinguished from mail, scale or brigandine; "coats of male" or chain mail is basically armor made of interlaced links; "buffe coat" refers to a heavy leather coat worn as armor; "corslett" or corselet/corslet refers to the ordinary armor of a pikeman in the sixteenth century; "corslett complete" refers to the entire suit of such armor; "quilted coats" or "coats quilted" may refer to armor made of several thicknesses of linen or other cloth quilted together; "jackcoat," "jacketts," and "jacks" refer to body armor worn by the rank and file in the fifteenth and sixteenth centuries, either padded coats or one interlined with mailplates or horn (Stone 1961).

A total of 695 pieces of armor, as broadly defined above, is listed in the Muster (Table 19.1). The majority of terms refer to protective gear made of iron or steel. Head pieces or helmets were apparently much less frequent than body armor.

Table 19.1. Armor.

Type of Armor	Quantity
armors	310
coate of male	206
armors, steele coats and coats of male	54
coats of male, headpeece	24
quilted coats	16
coat of steele	15
armor complett	13
corslett	13
head peece	12
armors and coates	8
jacks	6
corslett complete	5
steele coat	5
buffe coat	2
coats quilted	2
jacketts	2
jack coat	1
coat of plate	1
Total	695

Some form of armor was present in all settlements (Table 19.2), although in small quantities in some. Eight of 28 plantations, all in James City Corporation, had fewer than 10 pieces of armor. However, James City (122) had the most armor of any settlement, followed by Elizabeth City (71), and three sites along the upper reaches of the James River: Jordans Journey (44), West and Shirley Hundred (42), and Neck of Land (37). The Corporation of James City, having the most settlements, also had the most armor (379 pieces or 54.5% of the total), followed by Charles City (160/23%), Elizabeth City (139/20%), and Henrico (17/2.5%).

(2) There are 29 terms for *Weapons*: chamber; fauconett; hanger; lead; lead and bulletts; lead and shott; match; matchcockes; murderers; murderers for the forte; muskets matchcockes; ordnance mounted; peece; peeces fixt; peeces not fixt; peeces of ordnance; peeces of ordnannce mounted; peeces serviceable; petronell; pistol; powder; rapier; roules of mach; shott; snaphaunce; snaphannce pieces; sword and dager; swords; targett.

The weapons and weapons-related materials can be divided into four basic categories: small weapons; ordnance; powder/shot; and other items.

i. Small weapons (includes guns and swords)
   a. guns: chamber (may refer to the breech end of a gun barrel which contains the charge); matchcockes (matchlock–the earliest type of mechanism used on firearms, whereby a slow-burning match, held on a pivoted serpentine, hinged down to ignite the flash powder in the pan); muskets; peece (probably another term for a snaphaunce-like weapon); peeces fixt (an unknown term, but it may refer to a matchlock converted to an ignition system based on flint, or it may simply refer to a fully prepared or loaded weapon; peeces not fixt; peeces serviceable; petronell (a short firearm, either a light form of carbine or a long pistol); pistol; snaphaunce (an early form of lock in which a cock holding a piece of flint strikes a steel which hinges vertically over the pan; the steel is not combined with the pan cover as in the later flintlock) (Fryer 1971:16; Peterson 1972:304).
   b. swords: hanger (a short curved sword); rapier; sword and dager; swords.

Small weapons listed in the Muster total 1046 guns and 412 swords (includes all varieties). As is documented in Table 19.3, all settlements had weapons listed, ranging in number from nine at Smiths Plantation to 260 at Elizabeth City. The overwhelming majority of weapons present on the settlements were peeces (668/63.9% of guns), peeces fixt (175/16.7%), and swords (412). Related or different guns formed only 19.4% of the sample: pistols (61/5.7% of guns); matchcockes (57/5.4%); snaphaunce (1) and snaphaunce peeces (total of 48/4.8%); peeces serviceable (23/2.2%); peeces not fixt (8/0.8%); petronell (6/0.6%).

Peeces were present on all plantations, and ranged in frequency from 1 at Piersey's Hundred to 200 at Elizabeth City. Combining the total of all small gun weapons and swords, the following breakdown can be seen among corporations:

Henrico:	17 guns and 6 swords.
Charles City:	148 guns and 47 swords.
James City:	468 guns and 220 swords.
Elizabeth City:	413 guns and 139 swords.

The majority of matchlocks, a weapon with an earlier and more primitive ignition system, occurs at 6 sites, mainly in James City Corporation. Martins Hundred had 26 (80%) of all matchlocks in the colony.

Swords occur in the inventory of 23 of 28 settlements, with the majority occurring at Elizabeth City (97), Mulbury Island (42), James City (40), and Piersey's Hundred (34). Swords are not listed for the following sites: West and Sherley Hundred; Jordans Journey; Chaplains Choise; Burrows Hill; and Mathews Plantation.

ii. Ordnance (with frequency noted): fauconett (falconet, a small cannon of about 2 inch bore) (1); murderers (a small piece of ordnance) (13); murderers for the forte (3); ordnance mounted (5); peeces of ordnance (8); peeces of ordnance mounted (5).

In all 35 pieces of ordnance are listed in the Muster (Table 19.4). Only two types of ordnance are actually named–the murderer (16) and one falconet;

Table 19.2. Frequency distribution of armor by settlement.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	TOTAL
HENRICO																	
College Land	7	10															17
																	(17)
CHARLES CITY																	
Neck of Land	12			14	6	3		2									37
West & Shirley 100	7	2		16	9	3		4				1					42
Jordans Journey	11			26	6			1									44
Chaplains Choice	7			5											2		14
Pierseys Hundred	20			3													23
																	(160)
JAMES CITY																	
Pasbehaighs	14			4		2		1	2					1			24
The Maine	11																11
James City	27			79									16				122
James Island	8			1													9
Neck of Land	1			2													3
Archers Hope	9																9
Burrows Hill	2			2				1				1					6
Paces Paines	9																9
Smiths Plant.	9																9
Blaneys Plant.	7			2									2			6	17
Mathews Plant.							24										24
Crowders Plant.	6																6
Treasurors Plant.				2	3		30	1									36
Hog Island										5							5
Martins Hundred	14			13		4		4			1						36
Mulbury Island	21	1															22
Wariscoyack	9			2					4								15
Basses Choice				9					7								16
																	(379)
ELIZABETH CITY																	
Newport News	20																20
Elizabeth City	44		8	19													71
Company Land	17			3													20
Eastern Shore	18			4		8		6									28
																	(139)
TOTALS	310	13	8	206	24	12	54	20	13	5	1	2	18	1	2	6	695

Column headings:
1. armors
2. armor complett
3. armors and coats
4. coat of male
5. coats of male, headpeece
6. headpeece
7. armors, steele coats and coats of male
8. coat of steele/steele coat
9. corslett
10. corslett compleat
11. coat of plate
12. buffe coat
13. quilted coats/coats quilted
14. jack coat
15. jacketts
16. jacks

Table 19.3. Frequency distribution of guns by settlement.

	Match cocke	Snap. peece	Peece	Peece fixt	Peeces nt.fix	Peeces serv.	Petron.	Pistol	Total
HENRICO									
College Land			13	4					17
									(17)
CHARLES CITY									
Neck of Land			6	23				2	31
West & Shirley 100				40		4		1	46*
Jordans Journey		3	15	22			1		41
Chaplains Choice			3	11				2	16
Pierseys 100			1	13+					14
									(148)
JAMES CITY									
Pasbehaighs	2	8	18	4					32
The Maine			14						14
James City		6	76	5				5	92
James Island			27	1				2	30
Neck of Land			11	2				2	15
Archers Hope			15					1	16
Burrows Hill	2	1	5						8
Paces Paines			12				2	2	16
Smiths Plant.			9						9
Blaneys Plant.	10				8	1		3	32
Mathews Plant.						18			18
Crowders Plant.			12						12
Treasurors Plant.			30	10				3	43
Hog Island	6		17					1	24
Martins 100	26			26					52
Mulbury Island			26	1					27
Wariscoyack			5	13				1	19
Basses Choice			15				1	3	19
									(468)
ELIZABETH CITY									
Newport News			16						16
Elizabeth City		29	200			2	29		260
Company Land			88					3	102
Eastern Shore			34					1	35
									(413)
TOTALS	57	47	668	175	8	23	6	61	1046

**total includes 1 snaphaunce [not listed]*
+ink blot covers number of peeces fixt belonging to Abraham Piersey.

the remainder are referred to in general terms by the fact that they are mounted. Ordnance is nearly equally divided between three corporations: Charles City (14); James City (11); and Elizabeth City (10). However, ordnance occurs at only 11 of 28 settlements.

Table 19.4. Frequency distribution of ordnance by settlement.

Settlement	Quantity of Ordnance	Corporation
Piersey's Hundred	8	Charles City
Chaplains Choice	6	Charles City
Elizabeth City	5	Elizabeth City
James City	4	James City
Newport News	3	Elizabeth City
Company Land	2	Elizabeth City
Neck of Land	2	James City
Treasurors Plantation	2	James City
Blaney's Plantation	1	James City
Martins Hundred	1	James City
Basses Choice	1	James City
Total	35	

Ordnance is indicated as being mounted only at James City, Treasurors Plantation, Newport News, and Elizabeth City Company Land. At Chaplains Choice, three murderers are indicated as being "for the forte."

iii. Powder and shot: lead; lead and bulletts; lead and shott; powder; shott.

For purposes of calculation, it is assumed that all lead listed in the Muster was utilized for shot/bullet manufacture. A total of 9,553 lbs. of lead was recorded (Table 19.5).

The amount of lead present on individual settlements varied substantially. Four plantations–The Maine, Smith's Plantation, Mulbury Island, and Wariscoyack, all in James City corporation–had no lead. In other settlements, lead varied from a quantity of 12 lbs. (at Blaney's Plantation) to 2878 lbs. (Elizabeth City); the majority of plantations had several hundred pounds of lead on hand.

All plantations, with the exception of Newport News, had some powder; supplies varied from 3 lbs. to over 155 lbs. at individual plantations, with the majority having under 30 lbs. The following settlements had the most powder on hand: Elizabeth City (155.25 lbs.); Eastern Shore (154.25 lbs.); Martins Hundred and James City (each 81 lbs.).

iv. Other: targett (a small shield or buckler, especially one of circular form [Jester and Hiden 1964:62]). This item is listed for the company land in Elizabeth City.

(3) *Foodstuffs* are subdivided into animal and vegetable. Twenty-three terms pertain to animal foods: bacon flitches; breeding sowes; bull; calfes; cattell neete; cattell young and old; cowes; goats; horse; kidds; milch cowes; neat cattell; neat cattell young and old; piggs; poultrie; sow piggs; sowes; swine; swine young and old; yearelings and calves; young swine. Also to this list can be added the following: butter cheese and other necessaries, butter cheese oyle etc., fish, drie fish, and wett fish.

Nine terms pertain to vegetable foods: corne; corne and pease; English meale; English wheat; meale; oate-meale; oyle; pease; pease and beanes.

Because many terms refer in different ways to the same animal or foodstuff, the terminology for animals can be summarized as follows:

Table 19.5. Frequency distribution of lead and powder by corporation.

Corporation	Quantity of Lead	Quantity of Powder
Henrico	52	11.5
Charles City	2064	182
James City	2912	482.5
Elizabeth City	4525	381.5
Total	9553 lbs.	1057.5 lbs.

i. cattle (n=361 or 32.8% of domestic mammals, excluding horse)
   a. cattell (3), cattell neete (15), cattell young and old (19), neat cattell (146), neat cattell young and old (159).
   b. bull (2), calfes (7), cowes (6), milch cows (1), yearelings and calves (3).

ii. pigs (n=518 or 47.1%)
   a. breeding sowes (6), piggs (100), sow piggs (6), sowes (3), swine (305), swine young and old (84), young swine (14).
   b. bacon flitches (2) (sides of ham).

iii. goats (n=220 or 20.1%)
   a. goats (170), kidds (50).

iv. poultry (n=777)

v. horse (n=1). It is doubtful whether this animal was intended as a source of food.

vi. fish (n=59,108)
   a. fish (58,608), drie fish (400), and wett fish (100).

Totals: larger animals (1100); poultry (777), and fish (59,108).

Table 19.6 shows the distribution of domestic animals by settlement. Charles City had the largest number of animals (1034 or 55.1% of the total) and possessed all listed poultry, followed by James City (721 or 38.4%) and Elizabeth City (121 or 6.5%). If one eliminates the poultry, which may not have been counted in other corporations, James City would have had the larger percentage of domestic animals (65.5%). In any case, James City had the largest number of meat animals by far: 181 cattle, 209 pigs, and 121 goats, for a total of 511 animals. If one includes James Island, the number of these animals increases to 577, or 52.5% of the cattle-pig-goat population in the colony. The next largest assemblage of these animals was at Elizabeth City, with a total of 66 cattle, pigs and goats.

Five of 28 plantations, all in James City corporation, have no animals listed: Smiths, Blaney's, Mathews, Mulbury Island and Wariscoyack. College Land, Burrows Hill and Basses Choyse only have one animal each. The only horse listed for the entire muster is from James City, owned by Sir Francis Wyatt.

A total of 59,108 fish is listed; the majority occur once again at James City (9,580) and James Island (15,128) for a combined percentage of 41.8% of the total. Elizabeth City lists 11,450 (19%), the next largest amount. Nine plantations have no fish listed: Chaplains Choise, The Maine, Smiths Plantation, Blaneys Plantation, Mathews Plantation, Hog Island, Mulbury Island, Wariscoyack, and Eastern Shore.

Quantities of vegetable foodstuffs are measured three ways in the Muster, by numbers of barrels, hogsheads, or by bushels.

i. corne (n=1980.5 barrels and 2821.5 bushels, by far the largest food crop listed in the muster).

ii. pease (n=10 barrels/hogsheads and 53.5 bushels).

iii. corne and pease (n=300 bushels).

iv. pease and beans (n=37 bushels).

v. meale (n=14 barrels/hogsheads and 23 bushels).

vi. oatemeale (n=1 barrel and 23 bushels).
   Remaining foodstuffs are listed in smaller quantities.

Total vegetable foodstuffs for the colony: 2005.5 barrels/hogsheads and 3259 bushels.

Table 19.6. Frequency distribution of domestic animals by settlement.

	Cattle	Pigs	Goats	Poultry	Horse	Total
HENRICO						
College Land		1				1
						(1)
CHARLES CITY						
Neck of Land	34	19		247		300
West & Shirley 100	21	61		263		345
Jordans Journey	20	24		219		263
Chaplains Choice		3		48		51
Piersey's Hundred	44	31				75
						(1034)
JAMES CITY						
Pasbehaighs		6				6
The Maine		12				12
James City	181	209	121		1	512
James Island	3	58	5			66
Neck of Land	11	30				41
Archers Hope		8				8
Burrows Hill		1				1
Paces Paines	14	13				27
Smiths Plantation						0
Blaneys Plantation						0
Mathews Plantation	7					7
Crowders Plantation						0
Treasurors Plantation		2	15			17
Hog Island		9				9
Martins Hundred	10	4				14
Mulbury Island						0
Wariscoyack						0
Basses Choice		1				1
						(721)
ELIZABETH CITY						
Newport News	15					15
Elizabeth City		19	47			66
Company Land	1	5	32			38
Eastern Shore		2				2
						(121)
TOTAL	361	518	220	777	1	1877

Corn was the major food crop, forming 98.7% of the total barrel/hogshead capacity of crops noted at plantations and 86.6% of the bushel capacity. The quantity of corne ranges from 10 to 529 barrels/hogsheads and 10 to 596 bushels. All plantations, with the exception of Blaney's, have some quantity of corne listed, whereas the occurrence of other crops is spotty.

When the percentages of all crops are combined (barrels/hogsheads with bushels), the corporation of James City had the highest total of foodstuffs, followed by Charles City and Elizabeth City, with Henrico far behind. Named plantations can be ranked as follows (top five), beginning with the one with the greatest quantity of crop foodstuffs: Elizabeth City, West and Sherley Hundred, Jordans Journey, Pasbehaighs, Eastern Shore.

The combined totals of crop foods, animals, and fish reveal that the top five plantations in terms of food quantity are James City, Elizabeth City, West and Sherley Hundred, Jordans Journey, and James Island. Some plantations at the opposite end of the scale are Blaneys, Crowders, Wariscoyack, Burrows Hill, and Neck of Land (James City).

(4) There are 20 terms for *Buildings/Fortifications*: church; dwelling house; dwelling houses in several pallisadoes; forte; forte palled in; garden; house; house framed for silk worms; large court of guard; large forte; large forte palled in; pallizado; store; store houses; store with other cabbens; store within pallisadoe; stores; tobacco houses; vine yard; wind mill.

These terms can be classified into the following groups, with frequencies noted:

i. Buildings: (n=338)
   a. house, dwelling house or dwelling houses in several pallisadoes (279/82.5% of total buildings); storehouse, store, store with other cabbens, or store within pallisadoes (48/14.2%); tobacco house (8/2.4%); windmill (1); church (1).
   b. house framed for silk worms (1).

Buildings form the largest category of the structures listed in the Muster; as with other items, they are merely listed and not described in any way. Houses/dwelling houses are distinguished from stores and tobacco houses. Only one windmill (at Piersey's Hundred) and one church (James City) are mentioned. In terms of distribution by corporation, Elizabeth City has 146 (43.2%) of the total buildings, James City has 107 (31.6%), Charles City has 77 (22.8%), and Henrico a total of 8 (2.4%).

As Table 19.7 shows, the number of houses/dwellings at any one named plantation ranges from zero (Smith's Plantation and Mulbury Island) to 68 (Elizabeth City). The next largest distribution of houses (22 each) are at James City and Jordans Journey. The greatest variety of buildings occurs at Piersey's Hundred: house, dwelling house, store houses, tobacco houses, and windmill; 4 of 5 of these occur in the muster of Abraham Piersey.

Given the fact that the Muster gives population figures for each settlement and for units within each settlement, i.e., the household, one can obtain information on not only building quantity and distribution, but also on the number of buildings per household and the number of people living in buildings. The assumption is made that if only one house is listed per household, then everyone in the household lived in one house. If two or more houses are listed per household, which is often the case, it is impossible to determine how many people of the household lived in each house.

As can be seen from Table 19.8, most households had only one house associated with them. Fifty-four households had 2 to 5 houses listed, and one household (at Piersey's Hundred) had 10 houses associated with it. Curiously, 120 households in the Muster have no houses; it is doubtful whether this is a mistake of the census taker, as houses are mentioned for 61% of the 308 households within the settlements. Perhaps many people lived in ruder huts/dwellings that were not recorded.

Viewed from the vantage point of single-house households, the number of people living in one house ranged from 1 to 8, with the exception of two households that list 17 and 25 people, respectively.

Table 19.7. Frequency distribution of population, musters, structures.

Settlements	1	2	3	4	5
HENRICO					
College Land	22	0	16	8	0
CHARLES CITY					
Neck of Land	44	15	16	16	0
West & Shirley 100	61	17	17	17	0
Jordans Journey	56	15	15	22	0
Chaplains Choice	17	7	7	2	0
Piersey's Hundred	57	10	11	12	8
	(235)	(64)	(66)	(69)	(8)
JAMES CITY					
Pasbehaighs	43	1	18	1	0
The Maine	35	1	13	3	0
James City	125	8	23	22	4
James Island	51	0	17	11	0
Neck of Land	16	0	5	6	0
Archers Hope	14	0	4	4	0
Burrows Hill	7	0	3	3	1
Paces Paines	13	0	4	5	0
Smiths Plantation	10	1	9	0	0
Blaneys Plantation	15	1	1	3	3
Mathews Plantation	25	0	1	1	3
Crowders Plant.	6	0	1	2	0
Treasurors Plant.	40	2	2	8	4
Hog Island	53	3	6	6	0
Martins Hundred	26	0	6	7	0
Mulbury Island	30	1	13	0	0
Wariscoyack	19	4	4	6	1
Basses Choice	12	4	4	3	0
	(540)	(26)	(134)	(91)	(16)
ELIZABETH CITY					
Newport News	20	1	1	4	0
Elizabeth City	254	54	53	68	0
Company Land	94	16	18	19	18
Eastern Shore	51	18	20	20	17
	(419)	(89)	(92)	(111)	(35)
TOTALS	1216	179	308	279	59

Column headings:
1. total population
2. frequency of named musters
3. frequency of households
4. frequency of houses, dwelling houses
5. frequency of other structures (stores, tobacco house, windmill, church house framed for silk worms)

Out of a sample of 133 households that included a total of 464 people (38% of the entire population of colonists), the following observations can be made:

28 people lived 1 to a house (28 houses)
56 people lived 2 to a house (28 houses)
84 people lived 3 to a house (28 houses)
72 people lived 4 to a house (18 houses)
50 people lived 5 to a house (10 houses)
36 people lived 6 to a house (6 houses)
56 people lived 7 to a house (8 houses)
40 people lived 8 to a house (5 houses)

A "Density Index," which measures the relative degree of crowding at a site, can be obtained by dividing the total number of people at a site by the total number of known houses present at the site. These Density Indices are recorded for each site in Table 19.9. If one discounts the two highest values, which are unusual, the average values for corporations are as follows: Henrico (2.7); Charles City (4.4); James City (4.1); and Elizabeth City (2.7), signifying more crowded living conditions in Charles City and James City. A high value may indicate the preponderance of cruder shelters.

Table 19.8. Number of houses per household.

0 houses per household	= 120 households (39%)
1 house per household	= 133 households (43%)
2 houses per household	= 40 households (13%)
3 houses per household	= 9 households (3%)
4 houses per household	= 3 households (1%)
5 houses per household	= 2 households (0.6%)
10 houses per household	= 1 household (0.4%)
TOTAL	308 households

ii. Special Features (2)

In addition to the building category discussed above the only non-buildings mentioned in the Muster are a garden and a vineyard, both of which occur at Treasurors Plantation. (Table 19.10).

Table 19.9. Density index, arranged from most to least crowded.

Site	Corporation	Index Value
Pasbehaighs	James City	43.0
Mathews Plantation	James City	25.0
The Maine	James City	11.6
Hog Island	James City	8.8
Chaplains Choice	Charles City	8.5
James City	James City	5.7
Blaneys Plantation	James City	5.0
Newport News	Elizabeth City	5.0
Treasurors Plantation	James City	5.0
Company Land	Elizabeth City	4.9
Piersey's Hundred	Charles City	4.7
James Island	James City	4.6
Martins Hundred	James City	3.7
West & Shirley Hundred	Charles City	3.6
Elizabeth City	Elizabeth City	3.7
Archers Hope	James City	3.5
Wariscoyack	James City	3.1
Crowders Plantation	James City	3.0
Basses Choice	James City	3.0
College Land	Henrico	2.7
Neck of Land	Charles City	2.7
Neck of Land	James City	2.6
Paces Paines	James City	2.6
Jordans Journey	Charles City	2.5
Eastern Shore	Elizabeth City	2.5
Burrows Hill	James City	2.3

iii. Fortification-Related Features (n=34)

a. forte (3); forte palled in (1); large forte (1); large forte palled in (1); pallizado (27); large court of guarde (1).

Fortification-related terms are present for only 8 of 28 settlements. Table 19.11 lists these occurrences.

It is interesting to note that ordnance is only listed for 5 settlements with fortifications: Elizabeth City (5 murderers); Elizabeth City Company Land (2 ordnance mounted); James City (4 peeces of ordnance mounted, 1 large court of guarde); Chaplains Choice (3 murderers for the forte, 2 murderers, and 1 falconett); and Treasurors Plantation (1 peece of ordnance and 1 peece of ordnance mounted). The settlements listed as having ordnance, but no fortifications, are Piersey's Hundred, Neck of Land in James City, Blaneys Plantation, Martins Hundred, Basses Choice, and Newport News.

(5) There are five terms pertaining to *Transport*: barque; boat; canow; shallop; skiffe.

The term "boat" is used to describe the most prevalent type of watercraft. Out of a total of 44 watercraft mentioned in the muster, 39 (88.6%) are boats, canoes and shallops account for 2 each, and 1 craft is a barque. The watercraft are distributed as follows: James City Corporation–17 (38.6%);

Table 19.10. Buildings and features other than houses.

Settlement	Building/Feature	Quantity
Eliz.Ci.Comp.Land	Stores	18
Eastern Shore	Stores	17
Piersey's Hundred	Tobacco houses	4
	Store houses	3
	Wind Mill	1
Blaneys Plant.	Tobacco houses	3
Mathews Plant.	Stores	3
James City	Store, store houses	3
	Church	1
Treasurors Plant.	Stores with other cabbens	2
	House framed for silkworms	1
	Vine-yard of 2 acres	1
	Store house	1
	Garden of an acres & a half	1
Burrows Hill	Tobacco house	1
Wariscoyack	Store within pallisadoe	1
TOTAL		61

Elizabeth City–13 (29.5%); Charles City–11 (25%); and College Land in Henrico–3 vessels (6.9%).

In terms of individually named settlements, James City has the most vessels (8 or 18.2%), followed by Elizabeth City (6/13.6%), and Elizabeth City Company Land (5/11.4%). Other plantations have between 1 and 3 vessels each. Most plantations have only one type of vessel, usually a "boat," whereas two plantations have 2 vessel types, and one has three vessel types (James City, which has 1 barque, 6 boats, and 1 shallop).

Eleven plantations, all in James City corporation, have no vessels listed.

Table 19.11. Fortifications listed in muster.

Settlement	Fortification	Quantity
Elizabeth City	pallizado	19
Eliz.Ci.Comp.Land	pallizado	5
Wariscoyack	dwelling houses in several pallisadoes	2
	store within pallisadoe	1
Treasurors Plant.	forte palled in	1
	large forte	1
	large forte palled in	1
Eastern Shore	forte	1
Crowders Plant.	forte	1
James City	large court of guard	1
Chaplains Choice	forte	1
TOTAL		34

THE ENCLOSED SETTLEMENT

In an attempt to relate the information in the Muster to archaeological data we will now turn to an excavated site and briefly examine its history, features, and artifact distribution.

Extensive surface collection and/or archaeological excavations conducted at Piersey's Hundred (also called Flowerdew Hundred) in Prince George County, Virginia, have uncovered numerous seventeenth-century sites, including a large site that has come to be called the Enclosed Settlement. This site, presently situated on the immediate shore of the James River, is dated by associated artifacts to the 1620s and probably was established by Abraham Piersey, Cape Merchant of the Colony.

Site Description

The Enclosed Settlement had been built almost entirely of wood; archaeological evidence, therefore, consists mostly of earth stains that mark the former locations of wooden posts, post holes, pits, and/or trenches. Although such features can be delineated, certain factors have made identification and interpretation very difficult and often impossible: (1) due to deep modern plowing and the inherent shallowness of the site, only the lowest several inches of the features remained, especially trench and certain post mold features; (2) in addition, since the settlement was built over an Indian village, it is impossible at times to distinguish between English and Indian postmolds, especially those that occur in shallow trenches; (3) repeated flooding of the site by the James River made excavation difficult and rendered the northern portion of the site undefinitive.

The Enclosed Settlement was a seemingly cohesive living/working unit that basically included two to four buildings and a well, all enclosed by a fence or wall, with a gate or entrance on the south side. This unit has maximum measurements of 240 feet (east-west) by 120 feet (north-south) (there is no recognizable/definitive north wall or side, due to river erosion). The unit can be divided into two parts, each of which will be described in a summary manner in the following pages: the surrounding fence or wall and interior space and structural features (Figure 19.2).

Surrounding Fence/Wall

One of the most difficult features of the site to interpret accurately is its boundary, marked by linear soil stains and, in some areas, postholes/postmolds. Collectively, these features are interpreted as the outer walls/fences of the settlement. The west wall and two-thirds of the south wall of the Enclosed Settlement were constructed in a similar manner, but the east wall and one-third of the south wall were constructed differently.

(1) WEST WALL

The West Wall trench consists of a trench stain and 8 posthole/postmold features, both oriented in a north-south direction. The northern end of the trench terminates near the present James River and turns eastward to form a possible portion of the North Wall. At its south end the trench turns eastward to join the South Wall.

(2) SOUTH WALL

The trench stain is 78 feet long, 1.5-2.5 feet wide, and presently less than 0.3 foot in depth. If plow zone depth is taken into account, the original trench possibly measured 1.0-1.5 feet in depth. Numerous postmold-like features were discerned in the southern 30 feet or so of the trench, mostly beneath the trench fill. Since similar postmolds, many possibly Indian in origin, occur immediately outside of the trench as well, it is impossible to say what postmolds, if any, are contemporary with the wall trench. Those postmolds found beneath the trench fill but within the trench boundaries are so numerous and closely-spaced as to defy interpretation, unless one postulates a closely-spaced sapling or wattle type fence.

Parallel to the trench but two feet to the east are the remains of 8 squarish soil discolorations or postholes, the majority containing good evidence of postmolds. The postholes vary in size from 1.1 by 1.6 feet to 2.5 by 2.6 feet, in present depth from 0.31 feet to 1.1 feet. Postmolds are circular (ca. 0.7 foot in diameter) or rectangular (ca. 0.7x0.7 foot). Two postholes are paired, whereas some postholes contain multiple postmolds. The features are spaced 8-10 feet apart.

(3) SOUTHWEST WALL

As a continuation of the west wall, the western two-thirds of the south wall is 134 feet in length and has a configuration similar to the west wall, albeit with more (12) equally-spaced (10 feet intervals) postholes/postmolds on the interior side of the trench. The trench has several small breaks in it, which probably represent points where the soil stain had been plow destroyed. The eastern end of the southwest wall is recognized by a right-angled turn, which presumably marks the western edge of a gate or entrance area.

(4) ENTRANCE AREA

A possible entrance area is present in the south wall trench, where the trench turns right angles from an east-west orientation to north-south direction, creating a gap 24 feet wide. The interpretation of this area as an entranceway or gate area is further strengthened by the fact that the inner wall trench and posthole/postmold configuration does not continue within this gap area, and that this gap more or less lines up with the gap separating Structures 1 and 2 [to be discussed below].

Although no postholes/postmolds were readily apparent in the wall trenches in this area, at least 9 of these features are present in the gap area. One possible alignment exists, a V-shaped arrangement pointed toward the south.

(5) SOUTHEAST AND EAST WALLS

East of the "entranceway" gap is a slightly different and confusing pattern of earth stains. Two

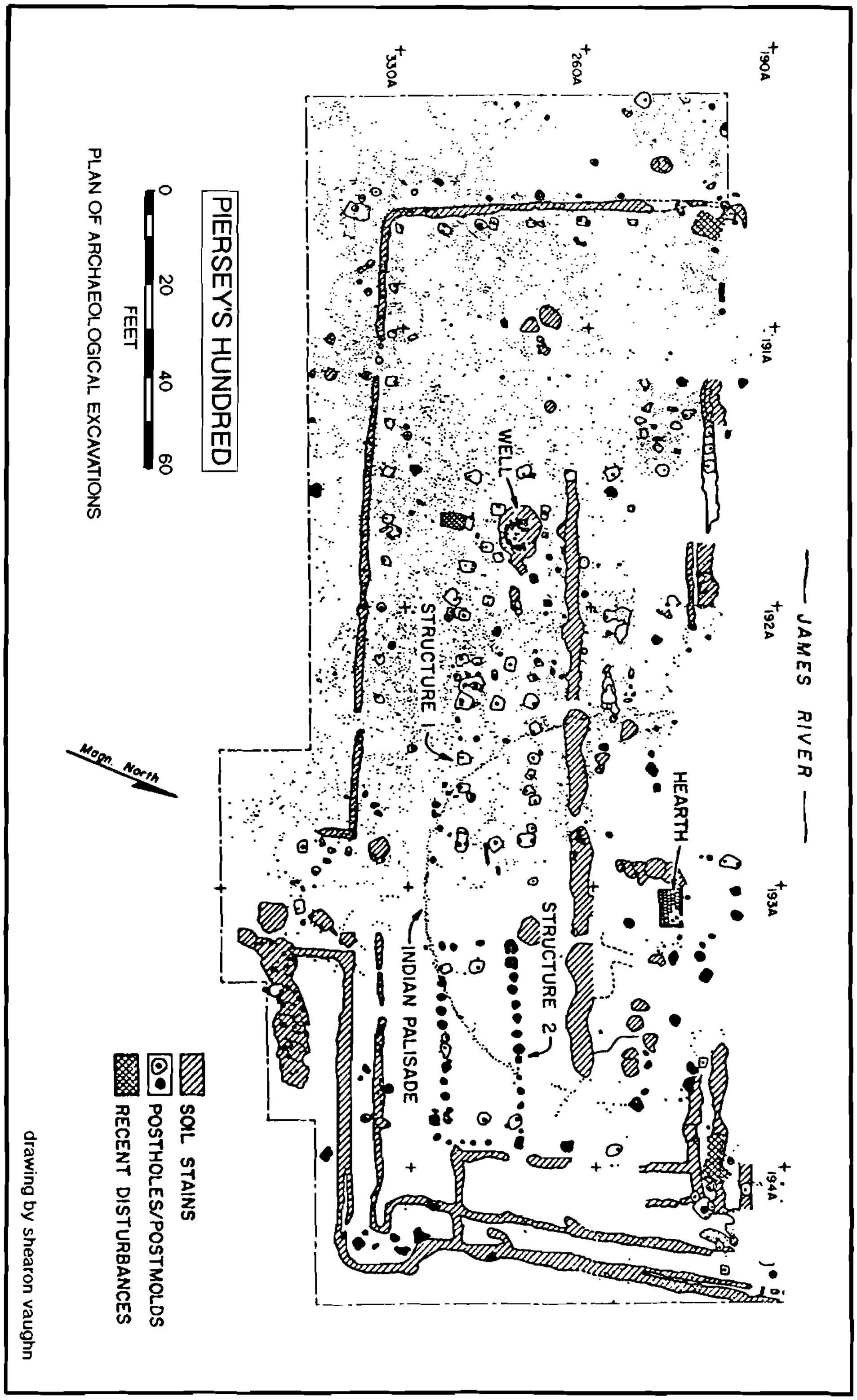


Figure 19.2. Plan map of excavations of Piersey's Hundred.

parallel trenches, spaced ca. 5 feet apart, run some 68 feet east-west, make a bastion-like configuration in the southeast corner, and then run some 97 feet northward to the river's edge. The trenches are very shallow but up to three feet in width; there is no evidence of postmolds in the trenches themselves. There is also incomplete evidence for a second set of parallel and connecting trenches to the immediate west of the east wall(s). In sum, there are possibly four parallel, north-south trenches that define the east wall.

There is evidence for postholes/postmolds between the trenches, especially in the east wall; at least two of these features intrude into the trenches, signifying a later construction date for the postholes/molds.

(6) NORTH WALL

The definition of a north wall for the Enclosed Settlement is unclear, due to river erosion. In any case, archaeological evidence in this area is sporadic and difficult to interpret.

Several factors indicate the possible presence of a north wall near the present river's edge, including the probable curvature of the northern end of the west wall toward the east and the presence of possible trench stains oriented east-west in several areas near the present shore of the James River (Figure 19.2).

Interior Space and Structural Features

Interior space can be divided into three basic areas: (1) a southern area adjacent to the entrance, bounded on the north by a central trench feature, containing two buildings (Structures 1 and 2) and an adjacent well; (2) the area to the north, situated between the central trench feature and the present James River, containing evidence of a hearth and other possible structural evidence; and (3) the western quarter of the site, which is largely devoid of structural features.

(1) AREA 1

Structures 1 and 2, as all other possible buildings at the site, are earth-fast (timbers embedded into the ground in any way) buildings (see Carson *et al.* 1981). Both structures are of similar size, but, interestingly, wooden posts were put into the ground in slightly different ways.

Structure 1, defined by the remains of a regular series of postholes/postmolds, measures 30 by 16 feet, with an additional 9 foot wide lean-to or shed on the west side. Structural evidence consists of 34 postholes and/or postmolds, including evidence of a wooden chimney on the southwest exterior of the main structure. Chimney evidence includes postmolds, an area of burned soil, and the occurrence of probable burned daub fragments.

Structure 2, situated 22 feet to the immediate west of Structure 1, measures 42 by 16 feet; its east end abuts the east wall complex. The north and south walls of the building are delineated by rows of closely set, irregularly shaped postholes about 18 to 24 inches in diameter and circa 3 feet on centers. Two rows of postholes, 5 feet apart, mark each end of the building; these postholes are smaller and shallower, about one foot in diameter and about 2.5 to 4 feet from center to center. The inner rows are each formed by three large postholes; all six are about 3 feet square, as are two more located halfway along each sidewall. Four of these larger holes contain roughly squared postmolds, about one foot square and one foot deep.

The three large postholes probably mark the ends of the main building, and therefore the length of the building was 32 feet, with 5 feet left over at each end for lean-to sheds lightly constructed of hole-set puncheons. No hearths or burned areas were found, meaning that chimneys were not present in Structure 2.

A circular, dark soil stain, associated with cobblestones and brick, was found 28 feet to the immediate west of Structure 1. This feature, which measured about 5 feet in diameter, was the remains of a filled-in well situated within the Enclosed Settlement. Several posthole/molds are present around the well, signifying the former presence of a platform and/or covering structure.

The well casing was made of large quartzite river cobbles packed in orange or grey clay daub. The foundation of the casing consisted of four large pieces of oak, the totality of which formed a complete circle. The depth of the well was a little over 7 feet; the interior diameter measured about 5 feet at the top and 3.5 feet at the bottom. Various portions of the casing had been repaired with brick.

A long, irregular, and shallow trench, oriented in an east-west direction, was found in the center of the Enclosed Settlement. The trench parallels Structures 1 and 2 some 5-12 feet to the north. The trench is 129 feet in length and 1.8 to 4.7 feet in width. The trench was discontinuous as found, probably due to its extreme shallowness (0.1-0.8 foot in depth).

In areas of the trench where lighter-colored fill dirt was present, postmolds were recognized within the trench fill; the molds were generally small (0.2 foot in diameter) and formed no apparent pattern. Some of these postmolds extended up to 0.7 foot beneath the bottom of the trench. The placement of postmolds suggests that they are Indian in origin and predate the Enclosed Settlement.

In sum, the function of this central trench remains unknown. One explanation for the trench may be that it represents the archaeological remains of a wide hedgerow used to shelter Structures 1 and 2.

(2) AREA TO NORTH OF CENTRAL TRENCH

The main definable feature in the area between the central trench and the present shore of the James River is a unique cobble and brick construction, possibly a hearth. Built upon a daub base, the 8 by 5 feet hearth is surrounded by various post holes, ash and burnt clay deposits, and other more amorphous features. In the hearth itself at least two 4 by 4 foot areas are defined by a single course of brick.

West of the hearth is a rubble area of 11 by 5 feet, consisting of randomly strewn cobbles, clay roofing tile fragments, and brick fragments. There is no clear evidence of an associated structure.

(3) WEST INTERIOR

The western quarter of the Enclosed Settlement is different because of the general lack of the kind of features found in other parts of the site and the low artifact content. The area is bounded by the west, south, and north walls; the eastern boundary is defined by a north-south line of equally spaced postholes/molds situated 10 feet west of the well.

This probable fence line may have served as a partition between the work/living area to the east and a 60-70 by 55 foot open space to the west. Virtually no features nor artifacts were found in this ca. 3500 square feet area.

Artifacts and Distribution

At final count 27,368 artifacts were found within the Enclosed Settlement site; another 2,624 artifacts were found immediately outside of the enclosure. Of the artifacts found within the site area, approximately 65% are European-derived and date to the period of the Enclosed Settlement, and 35% are pre-site or Indian in origin. The discussion presented below will deal only with the contemporary historic artifacts.

The major artifact types found can be grouped into the following categories:

Hardware and Building Materials: nails, roofing tiles, keys, gudgeon, pintle, staples, hasp, hinges, lock pieces, bricks.

Tools: saw, bit, adze, hoe, axe, sickle blade, plane blade, lancet, pestle, gouge, chisel, punch.

Weapons: gun parts (include snaphaunce safety, frizzen spring, battery, sideplates, as well as barrels, a breech plug, a wheellock sear), pistol belt hanger, shot (iron and lead), sprue, gun flints, halberd, and swords (blade, scabbard tip, pommel, hilts, a basket sword guard), cartridge caps, caltrop, and a bullet mold, cross-bow quarrel, and cannon worm.

Armor: armor includes a nearly complete breastplate, chain mail, body fasteners, a burgonet cheekpiece, brigandine plates.

Food and Drink Related: stemware, leather tankard, ceramics, case bottle, knife, iron pot.

Personal Items: needle, tobacco pipes, numerous types of glass beads, buttons, aiglets, straight pins, coins, spurs, clothing hook, jews harp, comb, thimble, pipe tamp.

Miscellaneous: tacks, rivets, crucible, baling seal, casting counters, buckles, gnomon, scrap, coin weight.

Certain artifact types are much more numerous than others. A total of 7,640 wrought nails (28% of total artifacts), both whole and fragmentary, represents the most frequent type of artifact, followed by flint debitage (2,546/9.3%), tobacco pipe stems (1,727/6.3%), and lead shot (1,500/5.5%). Together these artifacts make up 49.1% of the total artifact sample at the Enclosed Settlement.

The vast majority of artifacts was found in the northern half of the site, i.e., in the area to the north of Structures 1 and 2 and the central east-west oriented earth stain. A total of 22,440 artifacts was found in this area, which constitutes 82% of the total artifacts found. The remaining 18% (4,928 artifacts) were found to the south of the central earth stain.

Within the northern area, most artifacts were concentrated in the northeastern area of the site. Four 10x10 foot excavation units, situated some 70 feet east of the hearth near the east wall stains, contained nearly 5,000 artifacts.

In order to illustrate better the placement of artifacts within the Enclosed Settlement, the number and percentage of artifacts found spatially associated with major features are noted below:

Structure 1 area–247 artifacts (00.9%)
Structure 2 area–399 artifacts (01.5%)
Well area–649 artifacts (02.4%)
Hearth area–4150 artifacts (15.2%)
West of well*–100 artifacts (00.4%)

*1500 sq. foot area west of well and south of central earth stain.

This distribution also shows the relative paucity of artifacts found in the southern half of the site.

DISCUSSION

As examined in previous pages, the Muster of 1624/5 provides important information about early English settlements along the James River in Virginia. The 1,216 inhabitants of these 28 settlements were primarily males under 30 years of age. Nearly 42% of the population were servants; among non-servants, only 62 people (5%) had social titles.

Although all of the settlers mentioned in the census can be thought of as relative newcomers to Virginia, some had been in the new environs slightly longer than others. The Muster provides information on year of arrival to Virginia for about one-fifth of the settlers, as follows (year of arrival followed by number of arrivals in that year): 1607 (6); 1608 (7); 1609 (8); 1610 (26); 1611 (5); 1612 (1); 1613 (7); 1614 (0); 1615 (1); 1616 (13); 1617 (10); 1618 (35); 1619 (35); 1620 (59); 1621 (39); 1622 (28); 1623 (28); 1624 (18).

Remembering that a devastating massacre of settlers by Indians had taken place only a few years before the census was taken in 1622, at least 52 people who had arrived during the first 5 years of the colony survived in 1625. A majority of settlers still alive in 1625 had arrived in the last 5-7 years prior to 1625. One can postulate that people had a variety of experience or inexperience in dealing with the Virginia environment and cultural milieu.

The Virginia settlements, as evidenced from both the Muster and archaeological research, at this still-early stage of analysis exhibit both uniformity in overall settlement pattern and type of provisions present and, at the same time, differences in specific settlement configuration, number of houses, "fortification"/boundary systems, and the like. Archaeological excavations at Jordans Journey, Piersey's Hundred, the Maine, Kingsmill, and other sites illustrate these contradictions. Uniformity, to some extent, is due to an English mind-set of what a settlement should look like, influenced heavily by climate, natural resources, presence of hostile Indians, etc. Differences are due, in part, to variations in human experience, the number of skilled people present at a particular settlement, the location of the settlement, population frequency, availability of resources/provisions, ownership, etc.

In an attempt to gauge relative uniformity at the settlement level a collective grouping of attributes mentioned in the Muster can be considered for each settlement and the results applied on a comparative basis in order to achieve a relative ranking of settlements based upon these attributes (Tables 19.12 and 19.13). The 14 attributes which possess numerical components are as follows: population of each settlement; number of musters; number of households; number of people with titles; number of servants; amount of armor, weapons, ordnance, domestic animals, vegetable food resources, items of transport, houses, special structures, and fortifications at each settlement (Table 19.13). In other words, one can hypothesize that differences in settlement configuration are reflected in the frequency of attributes.

The rank of each settlement was calculated by comparing the frequency of each attribute for each settlement and then ranking the settlements for each attribute. For example, population of each settlement was ranked from highest population (1) to the settlement with the lowest population (28).

After all 28 settlements had been ranked for each of 14 attributes, the average overall rank for each settlement was attained by adding all rank numbers for each settlement and dividing by 14. From these averages, an overall settlement ranking was achieved (#1 the highest).

James City, the headquarters of the colony, emerges as the No. 1 settlement in the colony based upon a combined rating of population, provisions present, inhabitants with social titles, etc. (Table 19.12). The majority of wealthy and/or influential people of the colony lived at James City: Francis

Table 19.12. Overall ranking of settlements (see Table 19.13 for data).

Rank	Settlement
01	James City
02	Elizabeth City
03	West & Shirley Hundred
04	Jordans Journey
04	Elizabeth City Company Land
05	Neck of Land (CC)
05	Piersey's Hundred
06	Eastern Shore
07	James Island
07	Treasurors Plantation
08	Martins Hundred
08	Pasbehaighs
09	Chaplains Choice
09	Hog Island
09	Mathews Plantation
09	Mulberry Island
10	the Maine
10	Newport News
10	College Land
11	Basses Choice
11	Neck of Land (JC)
11	Paces Paines
11	Wariscoyack
12	Archers Hope
12	Blaneys Plantation
13	Burrows Hill
13	Crowders Plantation
13	Smiths Planation

Note: settlements with the same average rank were given the same rank.

Table 19.13. Relative ranking of settlement attributes.

Settlements	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
HENRICO																
College Land	16	6	5	16	10	9	7	12	17	8	20	17	5	4	11	10
CHARLES CITY																
Neck of Land	9	6	6	14	2	6	7	5	10	8	3	6	5	5	6	5
West & Shirley	4	5	3	8	1	5	7	4	5	8	2	3	5	5	4	3
Jordans Journey	6	7	4	6	3	2	7	3	7	8	4	4	5	4	5	4
Chaplains Choice	19	11	5	13	6	15	7	15	18	2	7	12	4	5	10	9
Piersey's Hundred	5	9	5	3	4	7	3	9	20	1	5	7	5	5	6	5
JAMES CITY																
Pasbehaighs	10	4	6	16	9	16	7	8	9	8	18	9	5	7	9	8
The Maine	12	8	6	12	9	14	7	16	20	8	14	20	5	7	11	10
James City	2	2	1	2	5	2	4	1	3	4	1	1	4	1	2	1
James Island	8	5	6	15	11	8	7	17	11	8	6	5	5	6	8	7
Neck of Land	20	13	6	14	10	11	7	20	19	6	8	23	5	6	12	11
Archers Hope	22	14	6	14	11	13	7	17	18	8	16	22	5	7	13	12
Burrows Hill	26	15	6	17	11	14	6	18	23	8	20	24	5	7	14	13
Paces Paines	23	14	6	14	11	12	7	17	18	8	10	14	5		12	11
Smiths Plantation	25	10	5	18	10	17	7	17	22	8	21	21	5	7	14	13
Blaneys Plantation	21	17	5	18	10	14	5	12	14	7	21	27	5	7	13	12
Mathews Plantation	15	17	4	5	11	16	5	8	16	8	17	15	5	5	10	9
Crowders Plantation	27	17	5	14	11	15	7	18	21	8	21	26	4	7	14	13
Treasurors Plantation	11	16	3	9	9	9	4	6	6	6	11	16	3	7	8	7
Hog Island	7	12	4	9	8	11	7	19	13	8	15	23	5	5	10	9
Martins Hundred	14	12	5	11	10	10	7	6	4	7	13	13	5	5	9	8
Mulbury Island	13	8	6	10	9	17	7	10	12	8	21	11	5	7	10	9
Wariscoyack	18	14	6	10	7	11	6	14	15	8	21	25	3	7	12	11
Basses Choice	24	14	5	18	7	14	7	13	15	7	20	19	5	7	12	11
ELIZABETH CITY																
Newport News	17	17	5	7	11	13	7	11	18	5	12	18	5	6	11	10
Elizabeth City	1	1	1	1	11	1	7	2	1	3	6	2	1	2	3	2
Company Land	3	4	2	4	11	4	1	11	2	6	9	8	2	6	5	4
Eastern Shore	8	3	3	9	11	3	2	7	8	8	11	10	4	3	7	6

Note: if two settlements have the same frequency of any attribute, they were given the same ranking number within each attribute category.

Column headings:

1. population
2. number of musters
3. number of households
4. number of people with titles
5. number of servants
6. frequency of armor
7. frequency of weapons
8. frequency of ordnance
9. frequency of domestic animals
10. frequency of vegetable food resources
11. frequency of transport items
12. frequency of houses
13. frequency of special structures
14. frequency of fortifications
15. average rank
16. rank

Wyatt, Governor; Abraham Piersey, Cape Merchant of the Virginia Company; George Yeardley, former Governor; John Pott, Company Physician; and others.

Elizabeth City is ranked No. 2, followed by West and Shirley Hundred, Jordans Journey–Elizabeth City Company Land, and Neck of Land (Charles City) and Piersey's Hundred (both with the same ranking of 5). It is interesting to note that four of the seven wealthiest settlements occur in Charles City Corporation. The other settlement in this corporation, namely Chaplains Choice, ranks No. 9.

The five lowest ranking settlements–Archers Hope, Blaneys Plantation, Burrows Hill, Crowders, and Smiths Plantation–ranked nearest the bottom for each attribute class. For example, Smiths Plantation had fewer people and households, as well as houses and provisions, than the other settlements. The settlements between the highest five and lowest five vary in attribute frequencies and relative rank.

Piersey's Hundred ranked 5th in overall rank; it was an above average settlement, due to its association with former Governor George Yeardley and Abraham Piersey. Piersey's main property at this site may have been the Enclosed Settlement, and it may have been the largest site at Piersey's Hundred at the time of the Muster.

This ranking of settlements should be thought of as an hypothesis to be tested when more archaeological data become available.

CONCLUSIONS

The Jamestown community, established in 1607, marked the beginning of permanent English settlement in Virginia. Within the first few decades of the seventeenth century, nearly 50 additional settlements had been established east and west of Jamestown along the James River (Hatch 1957). However, hundreds of English settlers were slain by Indians in 1622, necessitating the abandonment of many of these settlements. In consequence, by 1624/1625 the census takers were able to describe only 28 remaining places of habitation.

These settlements were dispersed along the entire length of the James River, but a majority was situated on the lower or eastern portion of the river in James City and Elizabeth City corporations. The settlements were established close to the river and along adjacent creeks, perhaps in many cases in areas formerly cleared of trees by the Indian population.

A surprising revelation to students of Chesapeake history and archaeology has been the nearly universal occurrence of earthfast buildings on seventeenth-century archaeological sites, that is, wooden buildings whose framing members were standing or lying directly on the ground or set in postholes (Carson *et al.* 1981:136). It is very probable that the buildings mentioned in the Muster were of this type of impermanent architecture. A contemporary description of a typical building of this type is given in the Records of the Virginia Company: "Their houses standes scattered one from another, and are onlie made of wood, few or none of them beeing framed houses but punches [posts] sett into the Ground And covered with Boardes so as a firebrand is sufficient to consume them all" (Morgan 1975:112).

Wealth or social position as gained through public office or sale of tobacco, which brought very high prices in the 1620s, was not displayed in houses, but instead in the quantity of labor and supplies available, which is reflected in the Muster. A dozen or more individuals had amassed large numbers of servants/tenants and material goods, including the following: Abraham Piersey, Cape Mercant (Piersey's Hundred and James City); George Yeardley, former Governor (Flowerdew Hundred, James City, and Hog Island); George Sandys, Treasuror (Treasurors Plantation); Samuel Mathews, Council member (Mathews Plantation); Francis Wyatt, Governor (James City and Pasbehaighs); Edward Blaney, Company agent

(Blaneys Plantation and James City); Edward Bennett, London merchant (Wariscoyack).

Typical provisions listed for most households in the Muster include various quantities of food, both vegetable and animal, armor and weapons, and houses. Those settlements associated with titled individuals had much larger quantities of goods, in addition to numerous servants or tenants. Abraham Piersey had 36 servants at Piersey's Hundred as well as 300 bushels of corn and peas, 200 lbs. of lead, 6 "peeces" of ordnance, 18 structures, including a windmill, and many other essential goods. In contrast, poorer people had next to nothing. James Tooke of the Maine, for example, had only 6 bushels of corn, one weapon (peece), and one piece of armor; no house is listed for his household.

Categories of goods listed in the Muster are minimal compared to the actual types of artifacts found on archaeological sites. A typical artifact assemblage for the period consists of a variety of imported European pottery, possibly locally made vessels as well, clay tobacco pipes, table glass, and a variety of metal weapons, armor, hardware, etc. Economic differences can often be determined through the presence of such expensive items as Chinese porcelain (present at the Piersey's Hundred site), Venetian table glass, etc.

In future studies, the information contained in the Muster of 1624/1625 should be more fully utilized by comparing it closely to archaeological data available from a variety of early seventeenth-century sites in Virginia and the Chesapeake. Up to the present, there has been little comparison between sites. It is now time to analyze closely settlement traits and artifact content from excavated sites in order to establish early English behavioral patterns.

REFERENCES

Barka, Norman F.
1976 Archaeology of Flowerdew Hundred: Stone House Foundation Site–An Interim Report. Southside Historical Sites, Inc., Department of Anthropology, College of William and Mary, Williamsburg, Virginia. (unpublished).

Carson, Cary, Norman F. Barka, William M. Kelso, Garry Wheeler Stone, Dell Upton
1981 *Impermanent Architecture in the Southern American Colonies*. Winterthur Portfolio 16(2/3):135-196.

Cotter, John L.
1958 *Archeological Excavations at Jamestown, Virginia*. Archeological Research Series 4, National Park Service, Department of the Interior, Washington, D.C.

Fryer, Douglas J.
1971 *Antique Weapons A-Z*. Thomas Nelson, Inc., New York.

Harrington, J.C.
1952 Historic Site Archeology in the United States. In *Archeology of Eastern United States*, edited by James B. Griffin, 335-344. University of Chicago Press, Chicago.

Hatch, Charles E., Jr.
1957 *The First Seventeen Years, Virginia, 1607-1624*. The University Press of Virginia, Charlottesville.

Hecht, Irene W.D.
1973 The Virginia Muster of 1624/5 As a Source for Demographic History. *William and Mary Quarterly*, 3rd. Series, 30(1):65-92.

Jester, Annie L., and Martha W. Hiden, editors
1964 *Adventurers of Purse and Person Virginia 1607-1625*. Sponsored by the Order of First Families of Virginia, 1607-1624, 2nd edition.

Kelso, William M.
1984 *Kingsmill Plantations, 1619-1800, Archaeology of Country Life in Colonial Virginia*. Academic Press, San Diego.

Morgan, Edmund S.
1975 *American Slavery, American Freedom: The Ordel of Colonial Virginia*. W.W. Norton & Co., Inc., New York.

Noel Hume, Ivor
1991 *Martin's Hundred*. University Press of Virginia, Charlottesville.

Outlaw, Alain Charles
1990 *Governor's Land, Archaeology of Early Seventeenth-Century Virginia Settlements*. University Press of Virginia, Charlottesville.

Peterson, Harold L., ed.
1972 *Encyclopedia of Firearms*. The Connoisseur, London.

Stone, George Cameron
1961 *A Glossary of the Construction, Decoration, and Use of Arms and Armor In All Countries and In All Times*. Jack Brussel, New York.

20 An Instant in Time: An Analysis of Marked Ceramic Vessels

Daniel W. Ingersoll, Jr.
St. Mary's College of Maryland
St. Mary's City, Maryland

In 1962, James A. Ford suggested that the ideal sample for use in seriation would be "a sampling of the ceramic population representing an instant in time." This essay considers the shape of such an instant in time using tightly dated marked ceramics from two historic sites with brief, known deposition spans.

THE EXPERIMENT

As Deetz and Dethlefsen (1965:196-197, 1966: 502, 1967:30) have demonstrated by using changing gravestone styles to evaluate the assumptions underpinning seriation, historic sites and artifacts provide ideal opportunities for archaeological experiments–tests of method and theory. In the present case study involving two historic sites, a wealth of documentary and archaeological evidence facilitates the establishment of accurate controls, not only for such variables as date and location of manufacture of artifacts but also for site function and chronology of site formation. The goal of this experiment calls for the exploration of the relationships among the distribution of artifact date ranges, site function, and the duration of site deposition.

THE ARCHAEOLOGICAL SITES

Two archaeological sites supplied the ceramic samples: the Custer Road Dump site (20MK17) and Puddle Dock. The former, located on Mackinac Island, Michigan, on lands of the Mackinac Island State Park Commission and near Ft. Mackinac, was excavated and described by David Brose (1967). While primarily a military dump, civilians associated with the fort also contributed to the site's formation (Brose 1967:69). Custer Road Dump, a stratified mound rising about 8 feet above the ground surface, consisted of 8 clearly visible artifact bearing strata, which generally exhibited the pattern of a coal ash layer rich in artifacts and faunal remains sealed off from the next level by a zone of relatively sterile soil (Brose 1967:42-43). Levels, numbered from the bottom up, were designated I (1876-1879) through VIII (1893-1895). From the stratification, historic records, and numerous tightly datable artifacts, Brose was able to infer that "[t]he mound thus represents a series of distinct depositions over a rather short period of time" (1967:43). Among the closely datable artifact categories were cartridges, pressed glass, bottles, ceramics, and clay pipes. Especially helpful in achieving precise chronology for each level were marked ceramic vessels, bottle patent dates, coins, campaign pins, and military equipment adoption dates (Brose 1967:69).

The latter site, Puddle Dock, a site within Strawbery Banke, Inc., Portsmouth, New Hampshire, was excavated and described by the author (Ingersoll 1971a, 1971b). The excavations investigated an evolving site complex of wharf, quay, filled-in waterway, factory/warehouse, and junkyard as part of a waterway community from 1630 to the present.

For the purposes of this paper, only two of the upper strata of the filled-in waterway receive attention: Layers 5 and 11 (layers were numbered from the top down). Puddle Dock, a waterway that fed into the Piscataqua River, gradually became silted in; in 1899 in response to complaints of "summer nuisance" (odors from decaying offal!) the city sealed off the waterway's mud flats with clean sand (Layer 12), then over the next several months spread tons of solid waste (Layer 11) composed of furnace ashes from coal mixed with household wastes, and finally topped that with more clean sand (Layer 8). Layer 11, the city dump for 1899, could be dated to less than a one year's deposition period by archaeological evidence: newspaper fragments, playbills, marked ceramics, and pollen (only summer and fall pollen were present). Layer 5 was given a one-year, 1908, deposition period on the basis of bottle patents, a date also consistent with the marked ceramics as well as artifacts from the underlying strata. Possible remains of a shed, storage platform, or other simple structure in Layer 5 suggest that this layer represented the first use of the new "made" ground as a junkyard, a land use pattern that persisted until urban renewal in the 1960s.

Both sites contribute single component stratigraphic units of high integrity due to very short-term deposition periods and to effective isolation by relatively clean fills. Both sites served similar functions (dumps) and dated to about the same time, the late nineteenth to early twentieth century. These site characteristics established the controls needed to facilitate the study of artifact distributions in short-term deposition units.

MARKED CERAMICS

The artifactual data consist of marked ceramic specimens (see Figures 20.1 and 20.2). Marked sherds offer the historic sites archaeologist several advantages, including the close dating of sherds/vessels and design styles, the identification of vessel manufacturing sources, and the reconstruction of trade networks. Intriguing vignettes of social history survive in the marks: an 1877 Laughlin Brothers Pottery Company (Ohio) mark "depicted the American eagle triumphant over a prostrate British lion"; in 1878, Knowles, Taylor and Knowles (Ohio) "patented its first pottery mark, an American bison which clearly signified American chauvinism" (Gates and Ormerod 1982:10). English potters marked wares (usually transfer printed or stamp impressed on the undersides of vessels) as early as the mid-eighteenth century; the nineteenth century saw the beginning of the regular use of marks on many refined paste earthenwares. Fortunately for historic sites archaeologists, English and American ceramic trademarks have been well studied by

Figure 20.1. Mark printed by underglaze black transfer on base of undecorated hard paste earthenware saucer (3 sherds) from layer 11, Puddle Dock. The manufacturer was Thomas Hughes, Waterloo Road, Burslem, England. A date range of 1860-1894 was assigned on the basis of information from Godden (1964:339). Illustrated in Ingersoll 1971a: plate 172.

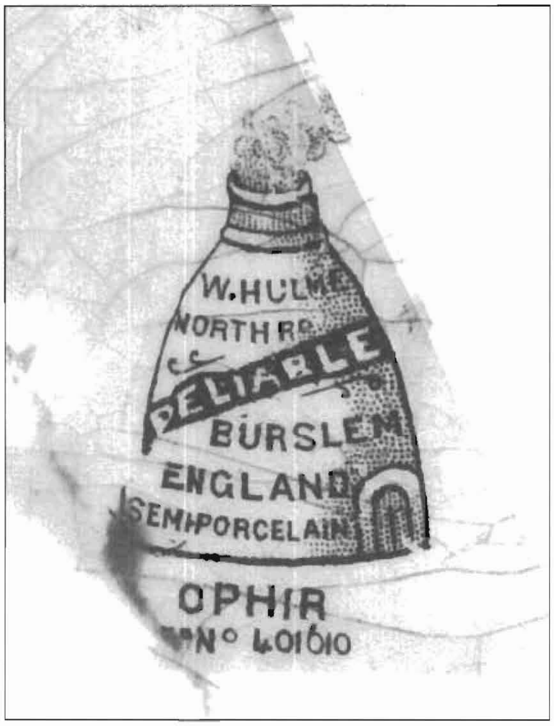


Figure 20.2. Mark printed by green underglaze transfer on base of white hard paste earthenware (1 sherd), vessel function undetermined, from layer 5. The manufacturer was William Hulme, Wedgwood Works, Burslem, England. Ophir was possibly a pattern name, but the sherd showed no visible traces of a design; possibly other parts of the vessel were decorated by transfer print or impression. The registration number 401610 would have been used in 1902 (Godden 1964:341, 528). Illustrated in Ingersoll 1971a: plate 204A.

ceramic historians (some examples: Barber 1904; Gates and Ormerod 1982; Godden 1964; Kovel and Kovel 1968; Mankowitz 1968; Thorn 1947). Marks are datable both by their style and by the manufacturing firms' initials, names, logos, and even printed dates or date coding systems. Ceramic historians have compiled data from the business periods of most firms, and have noted general trends in stylistic attributes of marks. For example, with respect to stylistic attributes, English marks with pattern names (such as "Roselle") are usually post 1810; marks with an abbreviation of the word "Limited" are usually post 1860; and marks with garter-shaped designs were used from the 1840s, etc. (Godden 1964:11).

The archaeologist who wants to identify and date marks should consult several sources on each mark, since ceramic historians often differ in their date assignments. Dating by means of marks is fairly dependable but should not be regarded as absolute. In some cases the sources are inaccurate or vague; in other cases problems are inherent in the marks themselves. For example, some Americans apparently imitated English marks (Gates and Ormerod 1982:9), and some English marks dated by the trade mark registration system occasionally grace vessels sold or produced several years after the year of the registration number. Similarly, the word "England," which was required on ceramics exported to the United States after 1891, at times appears on earlier vessels. For example, a printed (as opposed to molded) blue-edge plate in a Peabody Museum, Harvard University, type collection (962-2-40/8989) is imprinted with ENGLAND, which would suggest a post-1891 date, (in response to the McKinley Tariff Act of 1890) but it also sports a registration mark coded for January 21, 1881. As typical archaeological specimens, marks do not always enter collections intact–trying to identify a snippet of a mark on a sherd may entail hours of thumbing through the "mug shots" to get a match. Finally, not all marks find their way into the literature. In spite of such drawbacks, however, ceramic marks furnish the historic sites archaeologist with an excellent dating and identification device.

CONSTRUCTION OF THE GRAPHS

In this experiment the frequency distributions of dated marks from two stratigraphic layers at Puddle Dock and a single level and all eight levels combined at Custer Road Dump were graphed. The information used to date the Puddle Dock specimens is reported in Ingersoll (1971: Plates 169-205), where all the specimens are described and il-

Table 20.1. Chronological data on marked sherds from layer 11, Puddle Dock. N = 29; 1890.0 = Mean; Asterisks indicate that two marks were illustrated in the same plate. Source Totals: United States = 16; England = 11; France = 1; Germany = 1.

Mark Identification	Source	Central	Range	Plate
Minton, Stoke	England	1850.0	1850	179
THOMAS HUGHES	England	1877.0	1860-1894	172
GCM & Co.	Ohio	1880.0	1870-1890	182
GM & Co.	Ohio	1880.0	1870-1890	175
M. & CO.	Ohio	1880.5	1879-1885	188
T. & R. BOOTE	England	1885.0	1885	184
NEPCo.	MA	1887.0	1886-1888	173
D & D	NJ	1887.0	1880-1894	190A
HOTEL	US (?)	1888.5	1875-1900	190F
W.M. Co.	NJ	1889.5	1879-1900	187
J & E Mayer	PA	1890.5	1881-1900	190H
GREENWOOD CHINA	NJ	1893.0	1886-1900	178
THE WHEELING . . .	WV	1893.0	1886-1900	174
ELSMERE	Ohio	1893.5	1887-1900	190D
CARTWRIGHT BROS.	Ohio	1893.5	1887-1900	169
C.C.T.P. CO.	Ohio	1894.5	1889-1900	170
COOK & HANCOCK	NJ	1895.0	1890-1900	186
K et G	France	1895.5	1891-1900	185
GERMANY	Germany	1895.5	1891-1900	189
WOOD & SON	England	1895.5	1891-1900	181
HENRY ALCOCK . . .	England	1895.5	1891-1900	180
WOOD & SON	England	1895.5	1891-1900	190C
WOOD & SON	England	1895.5	1891-1900	171
WILKINSON'S	England	1895.5	1891-1900	190B
ENGLAND	England	1895.5	1891-1900	190E
W.A. & Co.	England	1896.5	1893-1900	176*
W.A. & Co.	England	1896.5	1893-1900	176*
CPCCo.	NJ	1897.0	1894-1900	177
PARIS WHITE	MA	1898.5	1897-1900	183

lustrated. Several sources were cited for each date; source conflicts on dating were resolved on a basis of majority source consensus or on the author's judgments of source quality. The same procedure was applied to redate the Custer Road Dump ceramic marks to achieve methodological consistency; the reworked Custer Road data are not provided here, but the original assignments are found in Brose (1967:55-59).

After each mark was assigned a date range, a central date (median), the mid-year of the date range, was calculated (see Tables 20.1 and 20.2). Some mark date ranges as determined from the literature extended beyond the stratum's deposition period. For this reason, the end date of the range of each mark was cut off at the terminal deposition date of the stratum, except, of course, when the mark's end date predated the stratum's terminus. The

Table 20.2. Chronological data on marked sherds from layer 5, Puddle Dock. N = 13; 1894.4 = Mean. Source totals: United States = 4; England = 6; Holland = 1; France = 2.

Mark Identification	Source	Central	Range	Plate
ELSMORE & . . .	England	1865.0	1859-1871	200
New England Pottery . . .	MA	1887.0	1886-1888	194
NEWWHARF	England	1892.0	1890-1894	198
GREENWOOD CHINA	NJ	1893.0	1886-1900	197
CARTWRIGHT BROS.	Ohio	1893.5	1887-1900	199
SEVERN	NJ	1898.0	1898	196
WOOD & SON	England	1899.0	1891-1907	195
Wood & Son	England	1899.0	1891-1907	202
G & P, CARNATION	England	1899.5	1891-1908	201
UTRICHT, HOLLAND	Holland	1899.5	1891-1908	203
K et G	France	1899.5	1891-1908	204B
Haviland France	France	1900.5	1893-1908	204C
W. HULME, OPHIR	England	1902.0	1902	204A

central dates were then displayed as ten year averages logged at mid-decade points (see Figures 20.3 to 20.6). In the original report (Ingersoll 1971a) other methods were assayed, including a five year averaging of central dates, and a very tedious method that distributed the relative proportion or percentage represented by each sherd in the sample over each year of the sherds' individual time spans. Since the 10 year averaging method seemed to produce the most consistent and comparable series of graphs, it was selected. Note that the graphs display half of the sample percentages, as would be the case with the traditional lenticular seriation diagrams (battleship shaped curves) if bisected vertically. Although these graphs somewhat resemble seriation diagrams, they do not compare relative frequencies of components/types across time, and they cut the curves off at the stratum's terminus, rather than the artifacts'.

The graphs were constructed by summing the marks' central dates falling into each decade, counting each decade as 0-9, and rounding off borderline cases (1889.5 would be counted as 1890, etc.). The decade sums were converted to percentages of the total sample, and divided by 2. Thus, half of the sherd sample was graphed and displayed to the right of the vertical axis as bars. A trend line was drawn to the right of the bars to help visualize the distribution. If both left and right halves were shown, and the area between the trend lines filled, a truncated battleship shaped curve resembling a "champagne glass" would result. All the distributions approximated the champagne glass form.

Two graphs from Puddle Dock are shown. One depicts (Figure 20.3) all the marked specimens from layer 11, squares 1 and 2, x-1, (29 marks) and the other (Figure 20.4) shows marked specimens from layer 5, x-3 (13 marks). The total *vessel* count for layer 11, square 1, x-1, was 106, and the marked sherds from that square (16-20 depending on how "fits" to other sherds in squares in Layer 11 are counted) represent about 15-19% of the total. Marked sherds might be considered a "type," but the category would cross-cut paste and design categories. Marked vessels may have another significance as a category, however, in that marked wares are usually somewhat fancier and more expensive than unmarked wares–the significance is largely economic. The Puddle Dock sample sizes are small, but I think that this problem is offset, to some

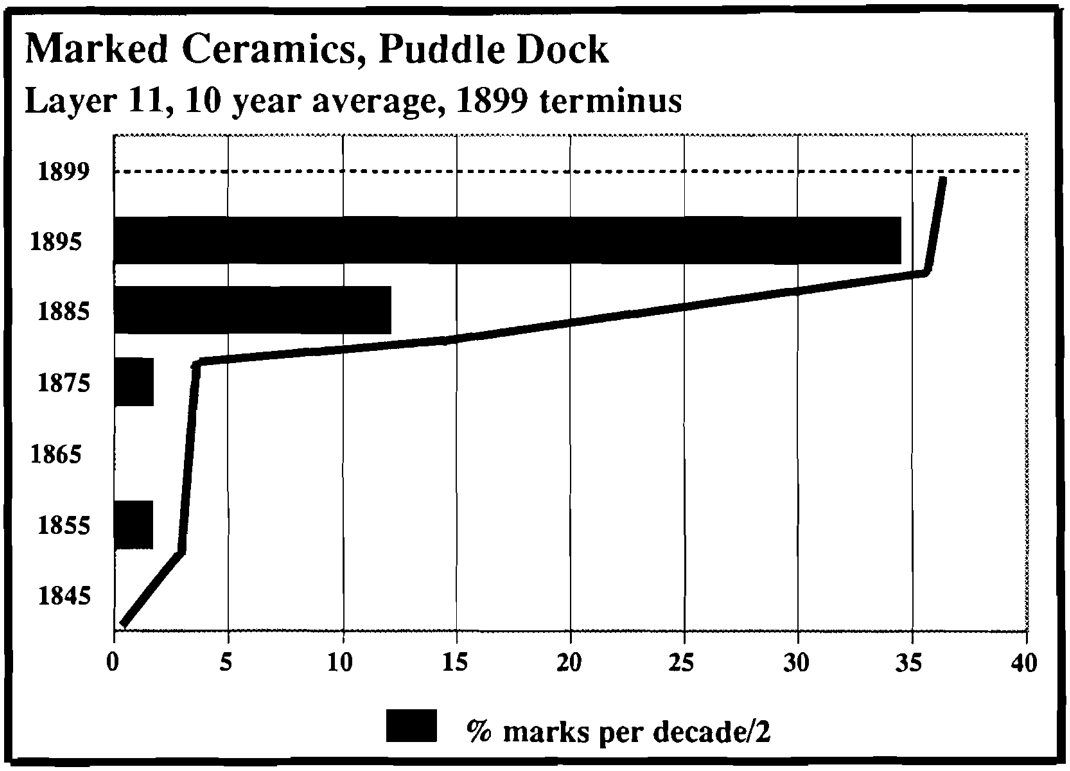


Figure 20.3. Marked sherds (29) from layer 11, squares 1 and 2, x-1, Puddle Dock, deposition period 1899.

extent, by the non-random nature of the collections. All sherds were kept from the strata, and the sample was virtually complete, unlike a surface collection.

The marked sherds at Custer Road Dump in the original site report are published with stratigraphic provenience (Brose 1967). An unknown number of marked sherds made by the Trenton China Co., stamped U.S.Q.M.D. (Quarter Master Department), have been left out of the Custer Road Dump graphs presented here, because counts were not available in the site report. This company was in operation from 1859 to 1891 (Barber 1904:68), and, because of this long period of production, represented by about 38% of the associated military ware present in each stratum (Brose 1967:55), it is thought that the contribution of these marks would have been constant through time. Two graphs are shown for the Custer Road Dump Site. Level I, with a deposition period of 1876-1879, is comprised of 8 sherds (Figure 20.5). The other levels, with counts of about 4 to 5 marked sherds each, were considered too small to graph. However, a graph was prepared showing all eight levels (41 marks) with a total of 19 years of deposition, from 1876 to 1895 (Figure 20.6). The strata were dated as follows: level I, 1876-1879; level II, 1879-1880; level III, 1880-1883; level IV, 1883-1886; level V, 1886-1888; level VI, 1888-1891; level VII, 1891-1893; and level VIII, 1893-1895 (Brose 1967:69).

INTERPRETING THE GRAPHS

The shapes of graphed distributions provide a tool for recognizing regularities as well as anomalies in archaeological data. In a seriation of eastern Massachusetts gravestone designs Deetz (1968) ob-

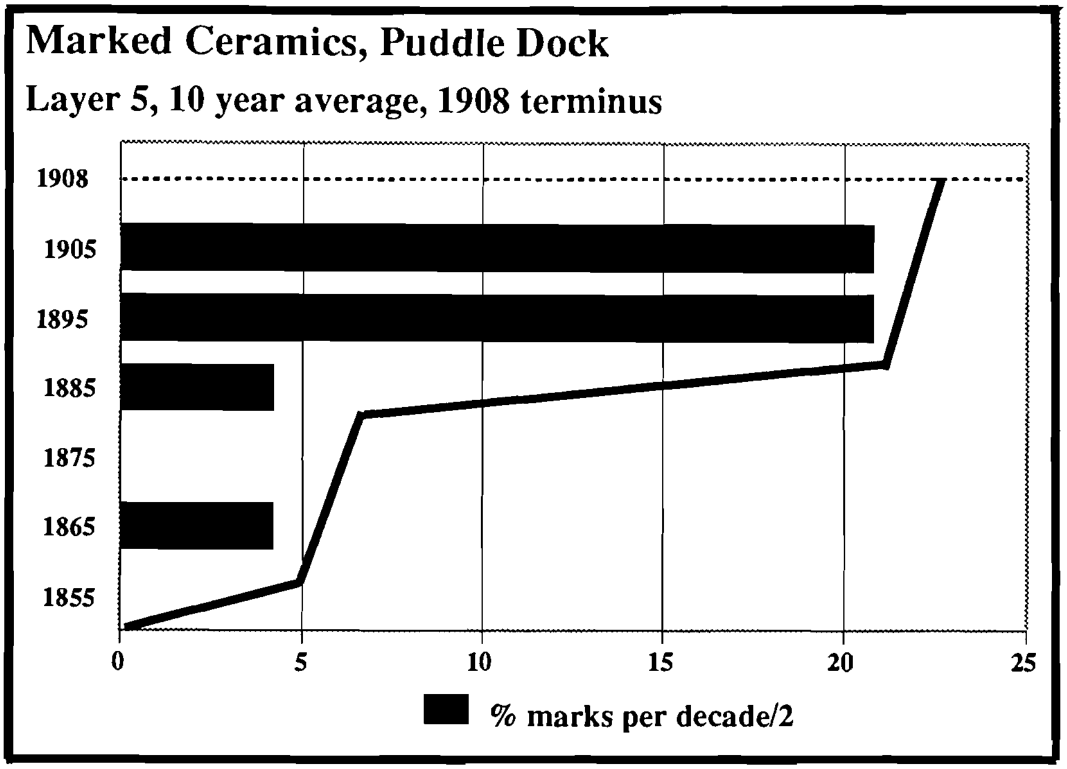


Figure 20.4. Marked sherds (13) from layer 5, x-3, Puddle Dock, deposition period 1908.

tained unusual "wasp-waisted" curves for the death's head style in cemeteries on Cape Cod. He accounted for this departure from the normal battleship-shaped curves by linking the shift in the Cape's market center from Plymouth to Boston to the Cape's mid-eighteenth-century conversion from a farming to a fishing economy (due to exhaustion of sandy soils). The battleship-shaped curve for the Cape's death's heads initially traced the classical decline in frequency for the type–nearly ending around 1750–but then charted a rebound in popularity, leading to a final phasing out sometime after 1800. The result: two popularity peaks punctuated by decline, hence the wasp waist in the curve. Deetz's inference was that the resurgence of death's heads on the Cape resulted from purchasing stones in Boston, a more conservative environment than Plymouth, where death's heads endured longer as a popular style.

In this study the shapes of graph curves have been used to assess the configuration of known, short-term deposits. The forms the graphs assume for the three short-term deposition units appear remarkably similar. At Puddle Dock layers 5 and 11 both produced "top heavy," champagne-glass-shaped curves (Figures 20.4 and 20.3), as did level 1 at Custer Road Dump (Figure 20.5). The graph of all the levels at Custer Road Dump produced a curve that had a more "pot bellied," champagne-glass-shape, due to the longer deposition period (Figure 20.6).

The short-term deposition-unit graphs are characterized by the pronounced increase of frequencies approaching the end of the depositional period. Fre-

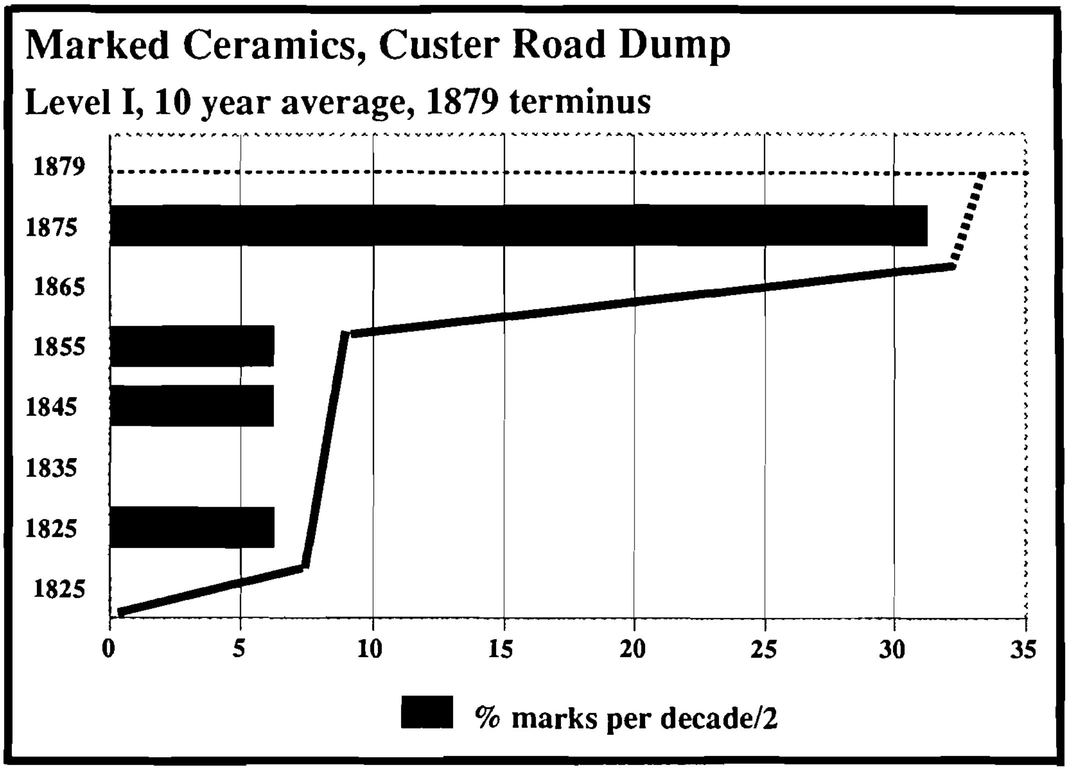


Figure 20.5. Marked sherds (8) from level I, Custer Road Dump, deposition period of 1876-1879.

quencies of marked pieces twenty years earlier than the deposition period are low. In part, the shape of the curves may be explained as a function of short vessel life spans. At Puddle Dock 69% of the marked vessels discarded in 1899-1900 (layer 11) were ten years old or fewer. An average of the central dates for layer 11 is 1890.0, giving an average life expectancy for all vessel types of just under ten years; the average for layer 5 is 1894.4.

In an ethnographic study at Tzintzuntzan, a contemporary Mexican village, Foster (1960) measured differential breakage rates and pottery life expectancy and found that, in general, pottery vessels in daily use had a life expectancy of about one year. Vessels that occupied protected positions in households or that were used only occasionally registered longer life expectancies. Foster suggested that factors such as soft paste, low prices, unprotected positions, and frequent usage would tend to shorten vessel life expectancy, while their converses would increase it. Incidentally, Foster made vessel counts in several households with the idea that, in the future, archaeologists working with prehistoric sites might be able to make estimates of population size or household size and duration of occupancy.

With late nineteenth-century, industrial-age ceramics, vessel life span appears to be substantially longer than that of folk ceramics. While no breakdown as to function or daily versus occasional use has been provided here as Foster did, the majority of the marked vessels at Puddle Dock were of the sort to see daily use. In relation to Tzintzuntzan pottery one of the major differences is likely paste strength: nineteenth-century mass-produced wares like ironstone were made to last. Apparently, sometimes they lasted long enough for people to get tired

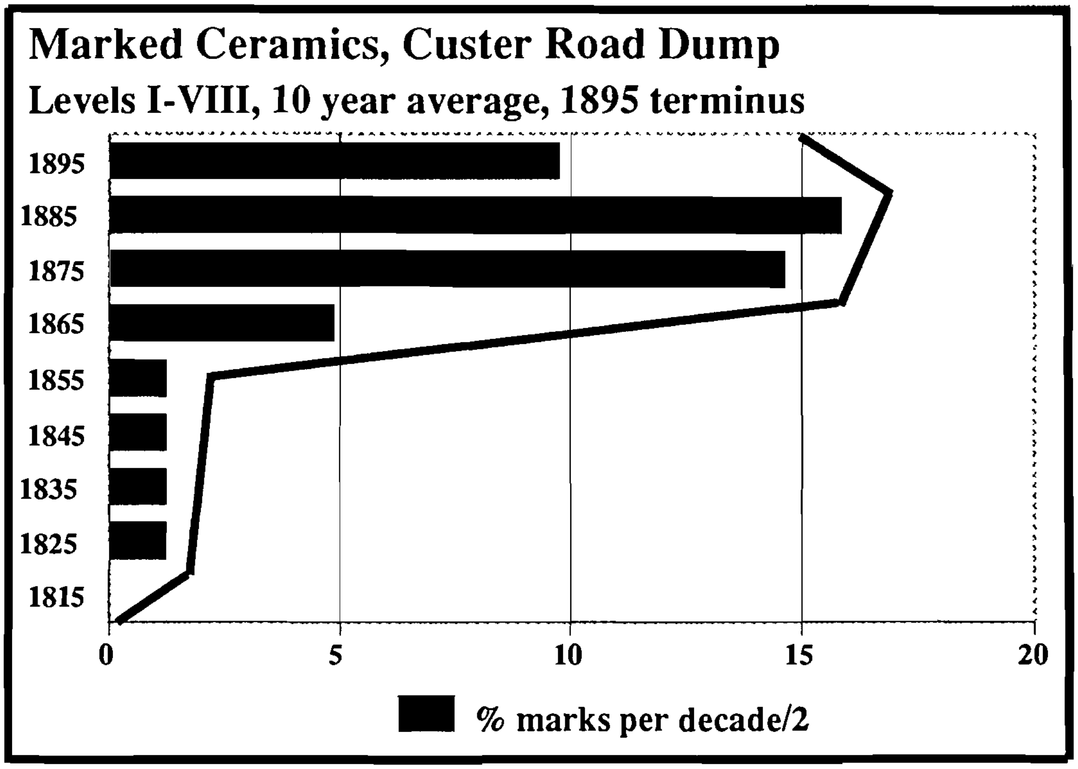


Figure 20.6. Marked sherds (41) from all 8 levels, Custer Road Dump, deposition period of 1876 to 1895.

of them and throw them away, intact! In addition storage of ceramics was probably more secure in nineteenth-century Portsmouth, where shelves and cabinets sheltered everyday ceramics as a matter of course–one of the commandments of the Georgian order was and is to enclose and hide functional or utilitarian material culture. As at Tzintzuntzan, pottery in Portsmouth and Custer Road was relatively inexpensive and could easily be replaced when broken, although the source of replacement was not local as at Tzintzuntzan.

Other factors thought responsible for the champagne-glass shape of the curves are site function and duration of site formation. In all cases here the strata involved are dumps with relatively short periods of deposition. These dumps received deliberately collected materials that were consciously categorized as waste and disposed of on a regular basis. The lag between discard due to breakage, exhaustion, or rejection of artifacts/ecofacts and their disposal should be a short one for nineteenth- and twentieth-century urban or institutional dumps. Given a known depositional cutoff, and a short-term formative period, it is not surprising that the archaeological units at Puddle Dock and Custer Road would generate curves with such heavy representation of dates near the depositional cutoff. Long-term occupation or dwelling components, for example a house occupied for 150 years, would logically produce curves of more classical lenticular form, because of the more gradual accumulation of lost or broken artifacts. Given datable artifacts, this hypothesis could be tested. To some extent, the graph of levels I-VIII at Custer Road Dump, which is really a record of a series of dumps, may illustrate the effects of longer-term site formation in that the trend line appears more lenticular or battleship-shaped than the single, short-duration units. If site function and occupational duration are significant factors in the formation of the trend lines, it should be possible in the future to assess occupation spans on historic sites more accurately using this approach. The present study provides a basis for comparison.

CONCLUSIONS

Ford (1962:41) has suggested that the ideal sample for use in seriation would be "a sampling of the ceramic population representing an instant in time." Instants in time, archaeological or documentary, are perhaps confined to sites destroyed by unique catastrophes, such as Herculaneum, in which a room's current material inventory would be preserved, or to estates recorded by probate inventory (see Teller 1968; Cummings 1964), but a component of less than a year's duration should be almost as good. Layer 11 at Puddle Dock has been dated independently of the artifacts to a period between summer 1899 and early spring 1900. In terms of standard prehistoric methods of seriation, how might the ceramic sample from layer 11 (treating the sample apart from documentary information) have been evaluated? A strict adherence to occurrence (presence/absence) seriation would have suggested an "occupation" period of 50 years or more, because of the presence of some mid-nineteenth-century design styles and trademarks. The very short deposition period would probably not have been inferred. A seriation based on relative frequencies would order the layer with respect to other sites but would still give few clues as to the relative duration of the deposition of archaeological materials. When type frequencies as well as presence/absence are taken into account, as in the calculation of a mean ceramic date, a more accurate estimate of the age and duration of the archaeological unit would be obtained (South 1977:228; see South 1977:201-230 for discussion of his mean ceramic date formula), but the layer's duration of formation would still be overestimated. The mean ceramic date method would furnish an accurate average artifact date but not an estimate of duration.

Based on data from three short-term deposition units from two sites, it would appear that standard methods of estimating site dates and duration of deposition or occupation make sites appear older and of longer duration than they actually are. This kind of experiment, repeated with historic sites of various functional designations, would help to gauge further the relationships between the distribution of artifact date ranges, site function, and the duration of site deposition. The method used here requires the tight dating of artifacts and archaeological units. While this might not be possible with prehistoric sites, generalizations about chronological data distributions derived from the experiments might prove applicable to prehistoric data. In addition experimental data from historic sites could be used to develop computer models to simulate and predict site duration from type frequency distributions.

ACKNOWLEDGEMENTS

The author would like to thank the National Science Foundation, Strawbery Banke, Inc., the Peabody Museum of Cambridge, Massachusetts, and Stephen Williams for generous support of the Puddle Dock research project from 1968-1970. This paper is a revised version of a part of chapter 6, which appeared in Ingersoll 1971a. Lucy Meyers of St. Mary's College transcribed the text to computer disk.

REFERENCES

Barber, Edwin Atlee
1904 *Marks of American Potters*. Patterson and White Co., Philadelphia.

Brose, David
1967 The Custer Road Dump Site: An Exercise in Victorian Archaeology. *The Michigan Archaeologist* 13(2):37-128.

Cummings, Abbott Lowell, ed.
1964 *Rural Household Inventories (1675-1775)*. The Society for the Preservation of New England Antiquities, Boston, Massachusetts.

Deetz, James
1968 Late Man in North America: Archeology of European Americans. *Anthropological Archeology in the Americas*, 121-130. The Anthropological Society of Washington, D.C., Washington, D.C.

Deetz, James, and Edwin S. Dethlefsen
1965 The Doppler Effect and Archaeology: A Consideration of the Spatial Effects of Seriation. *Southwestern Journal of Anthropology* 21(3):196-206.
1966 Death's Heads, Cherubs, and Willow Trees: Experimental Archaeology in Colonial Cemeteries. *American Antiquity* 31(4):502-510.
1967 Death's Head, Cherub, Urn and Willow. *Natural History* 76 (3):29-37.

Foster, G.M.
1960 Life-expectancy of Utilitarian Pottery in Tzintzuntzan, Michoacán, Mexico. *American Antiquity* 25(4):606-609.

Gates, William C., and Dana E. Ormerod
1982 The East Liverpool Pottery District: Identification of Manufacturers and Marks. *Historical Archaeology* 16(1-2):1-358.

Godden, Geoffrey A.
1964 *Encyclopaedia of British Pottery and Porcelain Marks*. Crown Publishers, New York.

Ingersoll, Daniel W.
1971a Settlement Archaeology at Puddle Dock. Ph.D. dissertation, Department of Anthropology, Harvard University, Cambridge, Massachusetts.
1971b Problems of Urban Historical Archaeology. *Man in the Northeast* 2:66-74.
1981 Cumulative Graphs and Seriation. In *Modern Material Culture: The Archaeology of Us*, edited by Richard A. Gould and Michael B. Schiffer, 255-266. Academic Press, New York.

Kovel, Ralph M., and Kovel, Terry H.
1968 *Dictionary of Marks: Pottery and Porcelain*. Crown Publishers, Inc., New York.

Mankowitz, Wolf, and Reginald Haggar
1968 *The Concise Encyclopedia of English Pottery and Porcelain*. Frederick A. Praeger, Publishers, New York.

South, Stanley
1977 *Method and Theory in Historical Archeology*. Academic Press, New York.

Teller, Barbara Gorley
1968 Ceramics in Providence, 1750-1800: An Inventory Survey. *Antiques* (October):570-577.

Thorn, C.J.
1947 *Handbook of Old Pottery and Porcelain Marks*. Tudor Publishing Co., New York.

21 Estimating Site Occupation Spans from Dated Artifact Types: Some New Approaches

Vincas P. Steponaitis
University of North Carolina
Chapel Hill, North Carolina

Keith W. Kintigh
Arizona State University
Tempe, Arizona

Over the years, archaeologists have developed many quantitative methods for dating proveniences based on artifact types. Most such methods involve some form of seriation, which is based on a model that treats individual proveniences as points to be placed in sequence along a temporal scale. Because of this underlying model, seriation methods can be productively applied only to proveniences that represent relatively brief intervals (i.e., points) in time, such as gravelots, pits, and short-term midden deposits. Complete site assemblages or surface collections, on the other hand, are often unsuitable for seriation because there is no guarantee that such collections represent short, much less equivalent, spans of time. Quite the contrary, sites, even if found within the same region, can have greatly differing spans of occupation, which may overlap to varying degrees. If one attempts to seriate such sites, the solution will be at worst meaningless, and at best a distortion of the true chronological relationships, since seriation methods are inherently incapable of recognizing different spans of occupation or expressing the different degrees of temporal overlap that may exist (Rouse 1967; Dunnell 1970; Cowgill 1972; Marquardt 1978).

Among those most actively concerned with this problem have been historical archaeologists, who, over the past 20 years, have proposed a number of quantitative algorithms for estimating the temporal span during which a site was occupied, given an assemblage of artifact types whose periods of manufacture (or use) are known (South 1972, 1977; Salwen and Bridges 1977; Bartovics 1980, 1981; Carlson 1983). What these algorithms seek has been termed *arrangement* (Schiffer 1975), as distinct from *seriation*, for the result is not simply a relative ordering, but rather an estimate of the actual beginning and ending dates of each site's occupation.

By far the best known of these arrangement algorithms is South's (1972, 1977) "visual bracketing method." This method begins with a "ceramic bar graph" like Figure 21.1, in which the horizontal axis represents time and the span of each ceramic type is depicted as a bar parallel to this axis. South described the essentials of his procedure as follows:

> a method I have used for a number of years involves placing a vertical bracket to the left and right on the ceramic bar graph, with the resulting time span between being the *interpreted* period, inside of which the occupation of the site took place. The placing of the left [starting] bracket is determined by choosing the point at which at least half of the ceramic type bars are touching or intersecting the bracket. The right [ending]

bracket is placed generally using the same rule; however, it must be placed far enough to the right to at least touch the beginning of the latest type present. An exception to this is surface collections from sites revealing multiple occupation periods as revealed in a gap or discontinuity between the ceramic bars of the first occupation period and those of the later period. In such cases brackets for both occupations must be placed . . . [South 1977:214].

South demonstrated the effectiveness of this pioneering method by applying it to a series of historic sites with known dates (1977:214-230). Yet despite its heuristic value, the method does have certain practical and theoretical limitations. One minor problem stems from an ambiguity in the rule for estimating dates: in practice, there is often more than a single point at which a bracket may touch or intersect half the bars. Thus, different researchers employing South's algorithm might well arrive at different estimates of a site's occupation span (Jelks 1972:177-178). Another, more worrisome problem

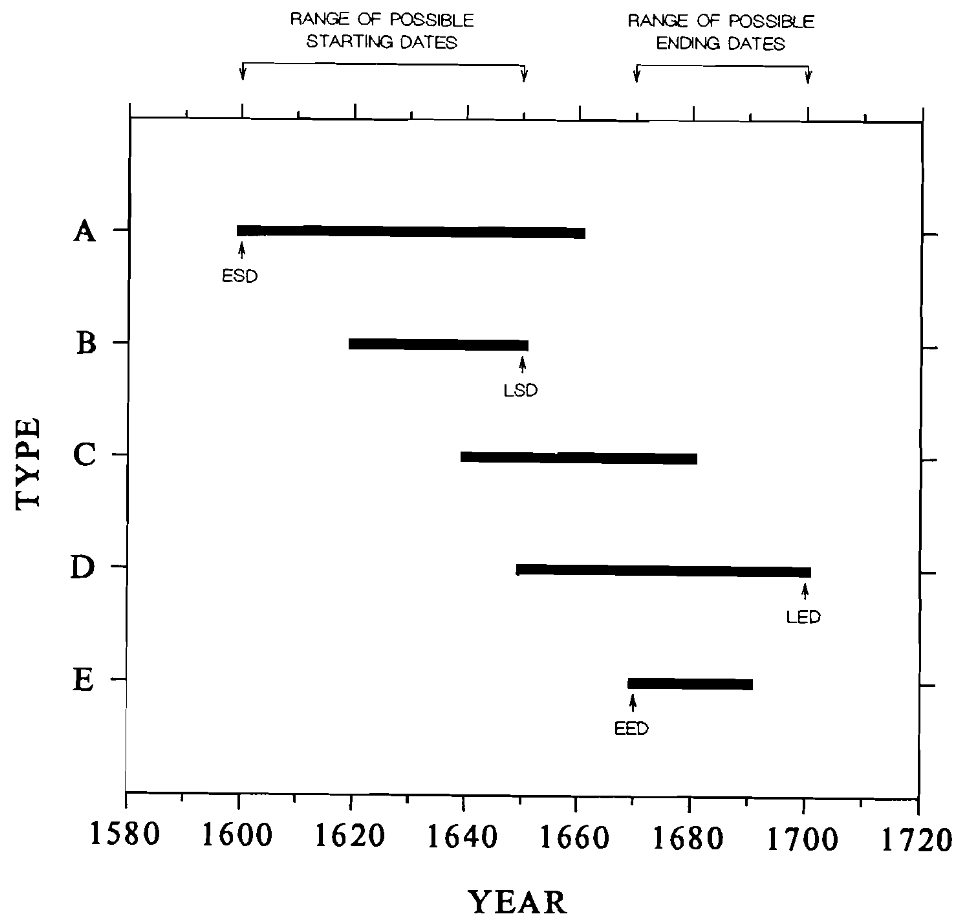


Figure 21.1. Ceramic bar graph for a hypothetical assemblage of five types. Logically, the boundaries of the site's occupation span may be defined by four key dates: the earliest starting date (ESD), the latest starting date (LSD), the earliest ending date (EED), and the latest ending date (LED). See text for further explanation.

is the lack of a theoretical justification for placing the brackets where he does. While the method seems to work, it is not at all clear *why* it should work. Other things being equal, it is usually better to use methods whose underlying logic is more explicit, so their theoretical and practical limitations can be better understood.

Salwen and Bridges's (1977) method uses a modified version of South's ceramic bar chart, in which the height of each bar is varied in proportion to the type's abundance. Abundant types can thus be given greater weight than rare ones in interpreting the graph qualitatively. They further suggest that a site's occupation span can be estimated quantitatively by calculating weighted means of the initial and final manufacturing dates, respectively, of the types comprising the assemblage. These means, they say, "should represent the most probable initial and final dates of occupation of the sites or features from which the collections were obtained, just as the mean ceramic date should mark the central tendency" (1977:167-169). Although this quantitative procedure is unambiguous, it too suffers from a lack of theoretical justification in that Salwen and Bridges fail to offer any mathematical or logical argument in support of the assertion just quoted.

Bartovics (1980, 1981) and Carlson (1983) take a different approach to estimating occupation spans. Their methods, although independently invented, are essentially the same: both rely on a graph of what Carlson calls a "composite ceramic distribution." For each type, the number of sherds in the assemblage is mathematically distributed over the known range of that type's manufacture or use. The distribution can be assumed to be uniform, Gaussian (i.e., "normal"), or of any other shape. The distributions of the individual types are then added together to produce the composite distribution for a given assemblage. This curve is analogous to a probability distribution, with the area under the curve suggesting the likelihood that the site was occupied over any given interval of time. Beginning and ending dates are then estimated by visual inspection; the site's span of occupation is assumed to correspond to the "fattest" part of the curve. This approach has the advantage of being based on an explicit mathematical model, but, in the absence of any rules for bracketing occupation spans, it shares the drawback of South's technique of being highly subjective in its application.

Building on these previous attempts, our goal here is to present some additional methods that entail both an explicit theoretical framework *and* explicit rules for estimation. We begin by presenting a simple mathematical model that shows how site occupation spans are logically related to the known use-dates of the artifact types that are found in the archaeological record. This model is then used as a basis for constructing two algorithms–one using type *presence*, the other using type *frequencies*–for estimating the actual span of occupation. The utility of these algorithms is examined by applying them to data from historic sites in the southeastern United States.

THE MODEL

Let us begin by considering an ideal archaeological site for which the following conditions hold: (1) artifacts are deposited at the site continuously throughout its occupation and (2) the artifacts deposited at any point in time are a representative sample of those generally in use. Let us further assume that the overall period of each artifact type's use is known. For any type (i), this period of use can be bracketed in terms of two dates: the initial date (a_i) when the type begins to be used, and the terminal date (z_i) when the type disappears.

Given these conditions, one can easily deduce the range of dates within which the occupation and abandonment of the site must have occurred. A site's occupation could not have started any earlier than the initial production date of the earliest type at the site; otherwise an earlier type should be present. Thus, the earliest possible date the occupation could have begun (henceforth referred to as the earliest starting date, or ESD) is the earliest initial date of any artifact type present, or

$$\text{ESD} = \min(a_i).$$

Similarly, the latest possible starting date (LSD) of the site's occupation is the earliest terminal date of any artifact type present, or

$$\text{LSD} = \min(z_i).$$

If the site had started any later, the type with the earliest termination date would not occur. Exactly the same logic can be used to find the range of possible ending dates for the site's occupation. The earliest ending date (EED) is equal to the latest initial date of any type present, whereas the latest ending date (LED) is the latest terminal date of the types present. These relationships can be rewritten as follows:

$$\text{EED} = \max(a_i),$$
$$\text{LED} = \max(z_i).$$

To the extent that the assumptions of the model hold true, the site's occupation must have begun during the interval between the ESD and the LSD and must have ended during the interval between the EED and LED. No other interpretation is logically valid.

These concepts are simply illustrated in Figure 21.1, where types A through E are shown as being present at the site in question. The earliest starting date is 1600, the initial date of type A. The latest starting date is 1650, the terminal date of type C. Similarly, the earliest ending date is 1670, marked by the introduction of type D, and the latest ending date is 1700, marked by the termination of type B. Hence, the site was first occupied between 1600 and 1650, and abandoned between 1670 and 1700.

The model just presented does nothing more than specify the range of possible starting and ending dates for a site's occupation. Such a result is useful, so far as it goes, but in many situations a more precise estimate of the occupation span may be required. For this reason we have developed two algorithms for deriving unique, "best" estimates of the starting and ending dates, estimates that are constrained to fall within the theoretically plausible ranges.

MIDPOINT METHOD

The first method simply takes the midpoint of the plausible range to be the best estimate of the date in question. In other words, the estimated starting date (EstSD) can be computed as

$$\text{EstSD} = (\text{ESD} + \text{LSD})/2,$$

and the estimated ending date (EstED) can be expressed as

$$\text{EstED} = (\text{EED} + \text{LED})/2.$$

These estimates are not only simple to calculate, but also have the statistical advantage of minimizing the largest error that could conceivably occur. That is, the error (e) in the length of the estimated occupation span can never exceed half the sum of the starting and ending ranges, or

$$e_{max} = [(\text{LSD} - \text{ESD}) + (\text{LED} - \text{EED})]/2.$$

Where the maximum error is small relative to the estimated span of occupation, this method is virtually guaranteed to produce a satisfactory result.

The effectiveness of this algorithm can be illustrated by applying it to ceramic assemblages from two sets of historic sites in the southeastern U.S.

The first set consists of 12 eighteenth-century sites from the Carolinas with documented dates of occupation–the same sites against which South originally tested his visual bracketing method (South 1977:214-230). The necessary ceramic data were supplied by South (1977:254-259, Table 31). Following South's suggestion, types with date ranges greater than 140 years were excluded from the analysis because such types are of little help in constructing fine chronologies and tend to inflate artificially the range of possible starting and ending dates. South himself dealt with this problem by excluding types 26, 39, and 65 entirely and by subdividing the span of type 49 into two segments–one used for sites that are "obviously" of the seventeenth century and the other used for sites believed to be of the eighteenth century (South 1977:213). This way of handling type 49 seems problematic for several reasons, not the least of which are (1) the inconsistency with the handling of other long-lasting types, (2) the circularity inherent in using different spans for the same type based on preconceptions about the site's date, and (3) the fact that the subdivisions are purely arbitrary, and that changing their boundaries could affect the outcome of the analysis. For present purposes, we simply eliminated type 49 from all our calculations, thereby treating it just like the other types with overly long spans.

Table 21.1 summarizes for each site the historical dates, the estimated spans based on South's method, and the estimated span based on our midpoint method. The average error of the estimated

Table 21.1. Historical and estimated dates for eighteenth-century sites.

Site Name	Historical Dates[a]	Estimated Dates[b]			
		Bracketing Method[c]	Midpoint Method[d]	Percentile Method (12.5–87.5)[e]	Percentile Method (35–90)[f]
First Fort Moore	1716–1747	1690–1775	1660–1791	1645–1762	1680–1762
Fort Moore	1716–1766	1690–1775	1722–1767	1710–1760	1720–1760
Brunswick Ruin S7	1734–1776	1740–1775	1720–1791	1710–1785	1745–1790
Brunswick Ruin S15	1726–1776	1740–1795	1722–1817	1705–1795	1740–1795
Brunswick Ruin N1	1731–1776	1720–1775	1722–1777	1725–1765	1745–1765
Brunswick Ruin S2	1731–1776	1740–1795	1720–1817	1710–1795	1735–1795
Brunswick Ruin S18	1763–1776	1720–1795	1720–1817	1755–1795	1775–1800
Fort Prince George	1753–1768	1740–1780	1720–1797	1735–1790	1755–1790
Brunswick Dump S10	1776–1830	1760–1820	1722–1830	1750–1830	1800–1835
Goudy's Post Plow Zone	1751–?	1740–1800	1737–1787	1750–1795	1770–1795
Goudy's Post Cellar	1751–1760	1740–1775	1737–1760	1735–1765	1750–1765
Paca House	1763–?	1740–1780	1730–1800	1735–1790	1750–1795

[a] After South (1977: Table 33).

[b] All estimates are based on the ceramic type dates given by South (1977: Table 31). The sherd counts for each site are also taken from South (1977: 253-260). All ceramic types with spans of 140 years or greater were excluded from the analysis (i.e., types 26, 39, and 49).

[c] As described by South (1977: 214-216). All the dates herein were derived by applying the method consistently as follows: the brackets were moved inward toward the mean ceramic date until each bracket just intersected the spans of at least half the types present in the sample. The right bracket, however, was never moved to the left of the latest beginning date of the types in the assemblage. It should be noted that the estimates so derived do not always match the estimates published by South (1977: Figure 33, Table 33). The source of these discrepancies is not entirely clear.

[d] The proportional maximum error–i.e., the maximum possible error (e_{max}) divided by the estimated span of occupation–is calculated for each site as follows: First Ft. Moore, .53; Ft. Moore, 1.78; Brunswick S7, 1.11; Brunswick S15, .79; Brunswick N1, 1.28; Brunswick S2, .75; Brunswick S18, .75; Ft. Prince George, .94; Brunswick S10, .55; Goudy's Post Plow Zone, 1.20; Goudy's Post Cellar, 2.50; Paca House, .86. Values of this index less than one indicate that the greatest possible error is smaller than the estimated span of occupation; the lower the value, the less serious the potential for error.

[e] The beginning date is estimated from the 12.5th percentile; the ending date is estimated from the 87.5th percentile.

[f] The beginning date is estimated from the 35th percentile; the ending date is estimated from the 90th percentile.

Table 21.2. Average error of estimated dates.[a]

Estimation method	Average error relative to all historical dates (years)	Average error relative to plausible historical dates (years)
South's bracketing method	15.5	13.7
Midpoint method	22.9	15.8
Percentile method (12.5-87.5)	16.9	12.9
Percentile method (35-90)	13.4	9.9

[a] The average error is the arithmetic mean of the absolute value of the difference between the estimated starting or ending date and the corresponding historical date.

starting and ending dates for each site, when compared to the historical dates, is 22.9 years for our method and 15.5 years for South's (Table 21.2).

At first glance it seems that our method has a somewhat greater tendency to overestimate a site's actual occupation span than does South's method. Yet when we examine the data more closely, it becomes apparent that the problem may stem not so much from the estimating procedures, but from the historical dates to which the estimates are being compared. Note that for five of the 12 sites, the alleged historical dates fall *outside* the range of plausible dates as determined by our theoretical model (Table 21.3). In each of these cases, the historical ending date is substantially earlier than the earliest possible ending date derived from the ceramics in the deposit. This can only mean two things: either the historical dates are wrong, or the archaeological deposits are contaminated with later

Table 21.3. South's historical dates compared with theoretically possible dates.

Site Name	Range of Possible Starting Dates[a]	Historical Starting Date[b]	Range of Possible Ending Dates[a]	Historical Ending Date[b]
First Fort Moore	1650–1700	(1716)[c]	1762–1820	(1747)
Fort Moore	1660–1775	1716	1740–1802	1766
Brunswick S7	1670–1770	1734	1762–1820	1776
Brunswick S15	1660–1775	1726	1795–1840	(1776)
Brunswick N1	1660–1775	1731	1759–1802	1776
Brunswick S2	1660–1770	1731	1795–1840	(1776)
Brunswick S18	1660–1770	1763	1795–1840	(1776)
Fort Prince George	1660–1770	1753	1775–1820	(1768)[d]
Brunswick Dump S10	1660–1775	1776	1820–1840	1830
Goudy's Post Plow Zone	1700–1775	1751	1775–1820	?
Goudy's Post Cellar	1700–1775	1751	1740–1780	1760
Paca House	1690–1770	1763	1780–1820	?

[a] Based on the model described herein.

[b] Dates that fall substantially outside the theoretically plausible range are given in parentheses.

[c] This historical date is rendered "implausible" by the presence of 39 Bellarmine sherds (type 66), whose period of manufacture is said to have terminated at 1700 (South 1977: Table 31). This discrepancy, however, may well be due to an "heirloom effect," in that the durable Bellarmine bottles could well have remained in use long after they ceased being made. Hence, it is entirely possible that the historical date is correct in this case.

[d] Although South places the beginning of Debased Rouen Faience (type number 21) at 1775 for formula dating purposes, he notes that it may occur as early as 1755 on French sites. If the type's span is pushed back to the latter date, then the earliest ending date (EED) for this assemblage becomes 1760, which then renders the historical ending date plausible.

material (a problem that South himself discusses in the case of First Fort Moore [1977:222]). Whichever explanation is correct, one can say *for certain* that the ceramic assemblages at these five sites could not possibly have been deposited entirely within the historical spans reported. This example clearly demonstrates the advantages of using a dating algorithm that is based on an explicit theoretical model, for it is only when the data were examined in light of this model that the anomalies in the historical record became apparent.

As a further test, we also applied the algorithm to historic sites from the region of Natchez, Mississippi, which was first colonized by Anglo-American settlers in the late eighteenth century. Surveys of this region carried out by the Lower Mississippi Survey from 1971 to 1973 produced numerous surface collections from sites of this period (Williams 1979; Brain, Brown, and Steponaitis, n.d.); these collections are now housed at the Peabody Museum, Harvard University. Included in the present analysis are all sites with significant representations of late eighteenth-century English ceramics that were found in the Natchez Bluffs south of Vicksburg and north of the Louisiana state line–the area of the old Natchez District (Swearingen 1934:33).

Relying on the ceramic type dates provided by South (1972, 1977) and Noel Hume (1970), these collections were used to estimate site occupation spans according to the method just described. Unlike the previous case, we lack documentary dates with which to check the estimates for individual sites. However, when the number of sites that date to every tenth year from 1770 to 1800 is plotted (Figure 21.2b), we find a pattern of monotonic increase through time that parallels the trend seen in historic census figures for the same region (Figure 21.2a). Although we would not claim that each site has been dated with perfect accuracy, it does seem that our estimated dates are at least close enough to the true dates to reflect the overall pattern of population growth described in the documentary record.

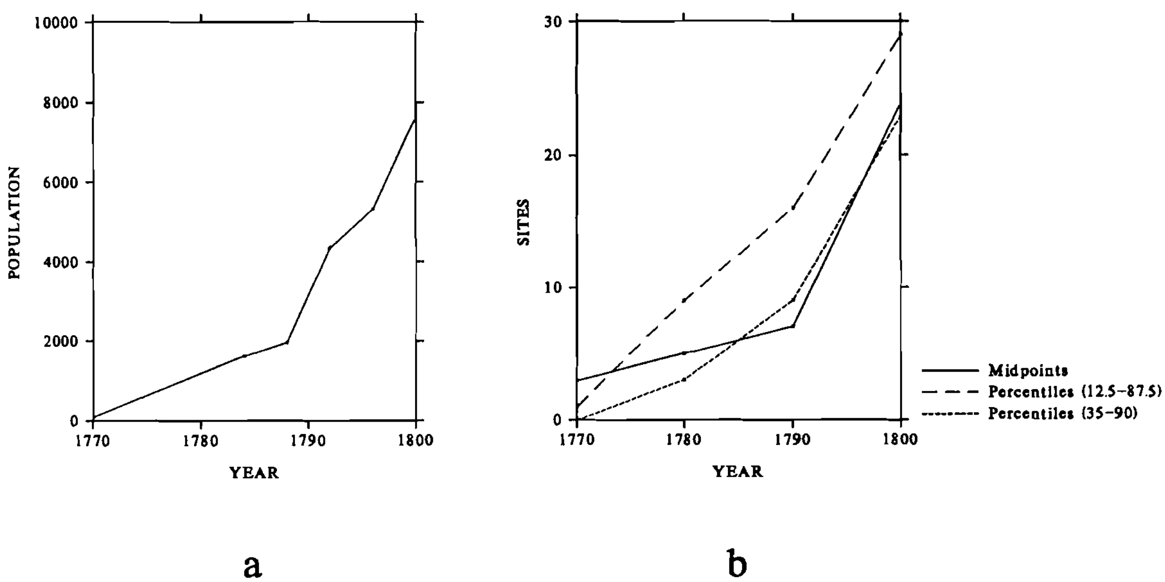


Figure 21.2. A comparison of historical census data and archaeological settlement data from the region of Natchez, Mississippi: (a) historical population trend for the old Natchez District (from Swearingen 1934:34, 36; James 1968:16, 42; Sydnor 1938:17); (b) the number of sites occupied at each decade boundary from 1770 to 1800, estimated using the methods described in this paper.

PERCENTILE METHOD

We have also developed a second method that takes into account the relative frequencies of artifact types rather than just their presence. In concept, our method follows the work of Salwen and Bridges (1977) and, more closely, that of Bartovics (1980, 1981) and Carlson (1983). However, unlike the latter two methods, in which the researcher is expected to draw an intuitive conclusion from a graphical presentation, ours provides an explicit algorithm for estimating starting and ending dates within the constraints of the mathematical model outlined earlier.

Generally speaking, the evidentiary value of a type for dating a site's occupation depends on at least two factors: (1) the type's abundance in an assemblage (the more abundant a type, the more important it is for dating a site), and (2) the length of the type's period of use (a type that was distributed for 300 years is generally of less value for questions of dating than one that was produced for only 30 years). These points have been made previously by various authors (e.g., South 1977:213, 217; Jelks 1972:176; Salwen and Bridges 1977).

Since all types are affected by both of these factors, often in opposite directions, we need a way to balance their effects. For example, the importance for dating of an abundant, but long-lived type is increased by its frequency, but decreased because of its long period of manufacture. In addition we need a way to combine the information contributed by several types to derive estimated starting and ending dates for an occupation.

Let us begin by observing that each sherd contributes chronological information of a probabilistic sort. Lacking any better information, the probability that a sherd was deposited in a given year depends on the frequency distribution of its type through time. Prior to the type's starting date, the probability of a sherd being deposited is zero, but during and after the period of manufacture, we assume that the frequency distribution looks like a unimodal "battleship curve." This curve can be assumed to be either symmetrical (i.e, a gradual increase in popularity followed by an equally gradual decline) or asymmetrical and skewed to the right (i.e., a rapid increase in popularity followed by an extended and more gradual decline). A number of authors have suggested that the latter is a more realistic model for present purposes, and we agree (Walker 1972:130-131; Fitting 1972:161; Cleland 1972:185-186; Liggett 1972:199). If we view this curve as a probability density function, then it will have a total area of 1.0, and by determining the area between any two points on the time axis, we in effect calculate the probability that a given sherd was deposited during the interval defined by those points.

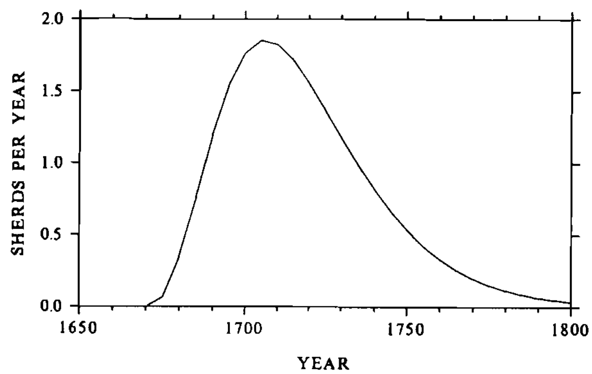


Figure 21.3. The assumed distribution of a single type through time (lead-glazed slipware, 100 sherds, patterned after a gamma distribution with alpha equal to 3).

Next, let us transform that curve into one with an area that is equal to the number of sherds of this type at a particular site. Now, the vertical axis represents a deposition rate (sherds per year), and the area between any two points along the horizontal axis is a probabilistic estimate of the number of sherds of this type that were deposited during that time interval (Figure 21.3). If we add together the temporal distributions of all types present at the site, we end up with what Carlson (1983) calls a composite ceramic distribution (Figure 21.4). The area under this curve is simply the total assemblage size,

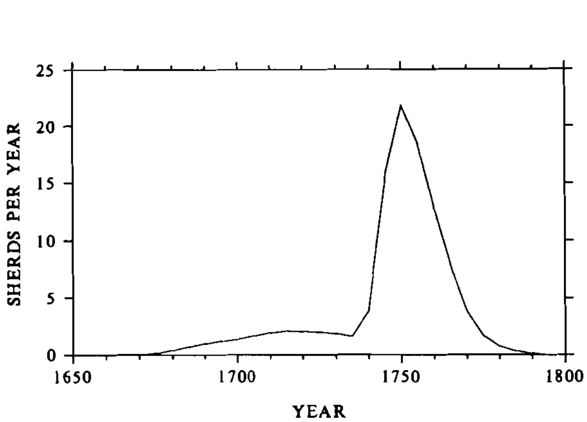


Figure 21.4. The composite ceramic distribution for Brunswick Ruin N1, North Carolina.

and, loosely speaking, the area within any temporal interval estimates the number of sherds that may have been deposited during that period.

Given this model, it seems reasonable to interpret the higher parts of this curve as times of denser occupation. One might, more tentatively, identify the major positive and negative inflection points with the beginning or end of occupation. Note that in spite of certain simplifying (and perhaps even simplistic) assumptions, this procedure has the desired effects. First, types with greater frequencies do have greater influence on the results because they contribute more total area to the composite distribution. Second, the importance of types with long production periods is reduced because their area is spread more widely along the time axis.

While these graphs clearly have interpretive value, we have not yet specified how we might use them to derive estimated starting and ending dates for a site's occupation. In the absence of additional information, we suggest placing the estimated starting date (EstSD) and estimated ending date (EstED) in such a way that 75% of the area of the curve is between these two points, and the remaining area is split equally on the two sides. This procedure is analogous in statistical terms to constructing a 75% confidence interval around the distribution's mean. Thus, the EstSD is placed at the 12.5th percentile, and the EstED at the 87.5th percentile. Hence, the occupation period of the site is identified with the "deposition" of 75% of the probabilistic sherds (Figure 21.5a). One further qualification is necessary: the EstSD and EstED must fall within the plausible ranges defined by our model. If either of the "boundary percentiles" falls outside of its plausible range, then the estimated starting or ending point becomes the date *within* the plausible range that is closest to the percentile originally chosen.

This algorithm was implemented with a program written in Turbo Pascal on an IBM-compatible microcomputer. The changing popularity of each type was modeled by a gamma distribution–a

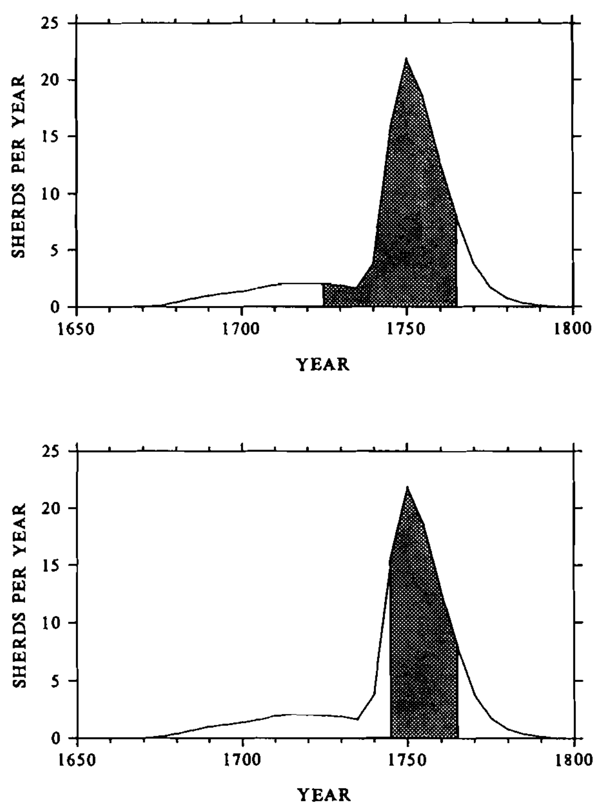


Figure 21.5. The composite ceramic distribution for Brunswick Ruin N1, North Carolina, showing the estimated occupation spans derived using the percentile method. Shaded area represents the portion of the curve defined by (top) the 12.5th and 87.5th percentiles; and (bottom) the 35th and 90th percentiles.

skewed, unimodal curve with a long tail that slopes gradually down to the right (Mood and Graybill 1963:126-129). The shape of this function is governed by a parameter called alpha; the greater the value of alpha, the less skewed the distribution. For present purposes, alpha was set at 3, a value that produced an intuitively pleasing curve in which the popularity of a type rises about twice as fast as it declines (see Figure 21.3). Changing this value to 2 or 4 made little difference in the results, never altering the estimated dates by more than five years. The distribution was scaled to fit each type's history of use: the origin was placed at the starting date, and the tail was placed so that a type produced for *n* years had 1/*n*th the area of the curve (i.e., an average year's production) to the right of the ending date.

When this algorithm was applied to South's data discussed previously (Table 21.1), it produced an average error of 16.9 years with respect to all the historical dates and an average error of 12.9 years with respect to the dates that are plausible in light of our model (Table 21.2). The latter figure is nearly 20% lower than the corresponding error produced by the midpoint method and about 6% lower than the error produced by South's visual bracketing method. Such minor differences could easily be sample-dependent, and it would be unwise to make too much of them. Nevertheless, the initial results suggest that our percentile method works at least as well as, and perhaps marginally better than, the midpoint and visual bracketing methods.

In fact it may be possible to improve the performance of the percentile method even further. Since the choice of boundary percentiles is arbitrary (in the sense that it is not specified by theory), it makes sense to choose the percentiles that are most likely to produce empirically satisfying results (i.e., percentiles that tend to fall within the plausible date-ranges and to approximate historical dates as closely as possible). While the 12.5-87.5 percentile convention just discussed seems to work reasonably well with the data at hand, it is appropriate to ask whether another set of boundary percentiles might have worked better. One way of addressing this question is to determine where the historical starting and ending dates actually fall within the composite ceramic distribution for each of our sites. As shown in Table 21.4, the plausible historical starting dates

Table 21.4. The percentiles of historical starting and ending dates in the composite ceramic distribution of each site.

Site Name	Historical Starting Date[a]	Starting Date Percentile[b]	Historical Ending Date[a]	Ending Date Percentile[c]
First Fort Moore	(1716)	(61.9)	(1747)	(79.5)
Fort Moore	1716	41.2	1766	96.2
Brunswick S7	1734	25.9	1776	75.6
Brunswick S15	1726	40.8	(1776)	(94.8)
Brunswick N1	1731	25.1	1776	98.6
Brunswick S2	1731	41.6	(1776)	(93.5)
Brunswick S18	1763	25.5	(1776)	(48.5)
Fort Prince George	1753	46.7	(1768)	(64.9)
Brunswick Dump S10	1776	28.8	1830	89.7
Goudy's Post Plow Zone	1751	17.5	?	?
Goudy's Post Cellar	1751	54.6	1760	85.9
Paca House	1763	36.8	?	?

[a] Dates that fall substantially outside the theoretically plausible range are given in parentheses (see Table 21.3).

[b] The average percentile of the plausible starting dates (i.e., those not in parentheses) is 35.0.

[c] The average percentile of the plausible ending dates is 89.2.

fall between the 17th and 55th percentiles and have an average very close to the 35th percentile. Similarly, the plausible historical ending dates consistently fall between the 55th and 99th percentiles, with a mean at about the 90th percentile. This suggests that the 35th and 90th percentiles are better estimators of the starting and ending dates, respectively, than the percentiles used in our initial test (Figure 21.5b). A glance at Table 21.2 confirms this suspicion: the average error with respect to the plausible historical dates drops to 9.9 years in comparison to 12.9 years for the other percentile boundaries.

Of course, the latter exercise is not really a test of our method since the procedure used in estimating dates was circular; that is, the estimated dates were based on percentiles that had been calibrated to fit the known historical dates of the very same sites. One can easily imagine other situations, however, in which such calibration procedures could be used without circularity. Say, for example, one is working in a region that contains numerous historical sites, *some* of which have documented historical dates. These known dates could be used to determine the "best-fit" boundary percentiles, which in turn could be applied in estimating the starting and ending dates of the undocumented sites.

With these considerations in mind, the percentile method was also applied to our data from the Natchez region (Figure 21.2b). The 12.5-87.5 percentile boundaries yield the expected pattern of increase in the number of sites through time, albeit with little change in slope after 1790. Interestingly, the 35-90 percentile boundaries produce even better results, duplicating almost exactly the historical pattern of population growth (Figure 21.2a). Indeed, this curve mirrors the historical data even better than the one based on the midpoint method, especially in the interval between 1770 and 1790. While it would be premature to generalize from this single case, our results hint that the 35-90 percentile boundaries may work well in dating eighteenth-century sites throughout the southeastern U.S., not just in the Carolinas. Clearly, further experimentation is warranted before this matter is resolved.

CONCLUSION

We have described two methods for estimating the starting and ending dates of a site's occupation based on known temporal spans of the pottery types found in the assemblage. These methods appear to be as good or better predictors than South's visual bracketing method, and have some distinct advantages over his and other methods that have been proposed. Both of our methods are grounded in a theoretical model that specifies the logical limits within which these dates must fall; unlike some previous attempts, both of our methods also entail unambiguous procedures for generating the estimated dates. It now remains for us to consider further some of the limitations and potentials of these techniques as a basis for future application and refinement.

It is important to stress that our methods of estimation depend on a prior knowledge of when ceramic types were *used*. Most published dates on eighteenth-century European pottery, however, are based on documentary evidence of when these types were *made* (e.g., Noel Hume 1974; South 1972, 1977). Needless to say, the two kinds of dates are not always congruent. Although the earliest date of manufacture should provide a reliable *terminus post quem*, most types were probably used for at least some years after manufacturing ceased. Moreover, the vagaries of international commerce sometimes caused interruptions of supply, making certain types unavailable even while they were still being made (Walker 1972:128-130; Jelks 1971:178). Such factors, if ignored, could well cause errors of estimation, but they are not problems inherent in the logic of our methods *per se*. Rather, the problem is an empirical one: if the use-dates of individual types in a particular region are not determined with reasonable accuracy beforehand, any method of estimation will fail. For eighteenth-century English pottery in the southeastern U.S., manufacture dates seem to provide a reasonable outline of when types were used, at least as a first approximation (South 1972, 1977). As researchers continue to refine the dating of these types (e.g., Miller 1991:5-11), our ability to estimate site occupation spans will improve accordingly.

It is also important to reiterate that our dating methods should be applied only to assemblages that can reasonably be assumed to represent single, continuous occupations. Multicomponent sites or assemblages contaminated by postdepositional mixture will generally yield erroneous estimates. Obviously, this places a burden on the analyst to screen out such cases, a process made more difficult by the fact that one would only consider using these methods on assemblages whose dates are not precisely known from documentary sources. In addition to the usual kinds of contextual evidence an archaeologist might employ, the graphs on which our dating methods are based can provide some useful clues in this regard. For example, a gap in the ceramic bar chart–an interval somewhere between the earliest starting date and the latest ending date in which no type occurs–almost certainly indicates a gap in occupation. Similarly, multiple modes (or "peaks") in the composite ceramic distribution curve might well suggest the presence of multiple occupations. Another circumstance that should evoke caution occurs when one of the boundary percentiles falls outside the plausible range of starting or ending dates. While this circumstance is not necessarily problematic, it might sometimes be caused by a single "stray" sherd–either an heirloom or a postdepositional contaminant–whose presence distorts one or more of the key dates that define the plausible ranges. In such cases it usually makes sense to eliminate the offending sherd and estimate the dates again.

Finally, there is no theoretical reason why the methods described here should be limited in their application to historic sites alone. As chronometric techniques improve and chronological evidence accumulates, prehistoric phases and ceramic types are being dated with ever more precision. Indeed, chronological discrimination of types and varieties on the order of 50 years or less is becoming increasingly common (e.g., Kintigh 1986). In some parts of North America our chronological resolution within late prehistoric periods is probably sufficient to use these methods productively, especially on sites of longer duration. This potential still remains to be explored.

ACKNOWLEDGMENTS

If it weren't for Stephen Williams, this paper would not have been written, for it was he who helped stimulate the senior author's interest in both historic ceramics and chronology. It was also under Williams's overall direction that the test data from the Natchez region were collected. Earlier incarnations of this paper were read at the International Symposium on Data Management and Mathematical Methods in Archaeology (sponsored by Commission 4 of the International Union of Pre- and Protohistoric Sciences, Denver, 1985) and at the Southeastern Archaeological Conference (Birmingham, 1985). The Natchez research was supported in part by the National Endowment for the Humanities, the National Geographic Society, and the Wenner-Gren Foundation for Anthropological Research. We are grateful to Stanley South, David Anderson, Linda Carnes-McNaughton, and Laurie Cameron Steponaitis for their helpful comments at various points along the way. Copies of the Turbo Pascal program described on p. 358 are available from the authors on request.

REFERENCES

Bartovics, Albert F.
1980 Beyond Mean Ceramic Dating Formulae in Historical Archaeology. Paper presented at the Annual Meeting of the Council for Northeast Historical Archaeology, Albany, New York.

1981 The Archaeology of Daniels Village: An Experiment in Settlement Archaeology. Ph.D. dissertation, Department of Anthropology, Brown University, Providence, Rhode Island.

Brain, Jeffrey P., Ian W. Brown, and Vincas P. Steponaitis
n.d. Archaeology of the Natchez Bluffs. Ms. in possession of the authors.

Carlson, David L.
1983 Computer Analysis of Dated Ceramics: Estimating Dates and Occupational Ranges. *Southeastern Archaeology* 2(1):8-20.

Cleland, Charles C.
1972 Some Notes on South's Ceramic Dating Technique. *The Conference on Historic Site Archaeology Papers* 6:185-186.

Cowgill, George L.
1972 Models, Methods and Techniques for Seriation. In *Models in Archaeology*, edited by David L. Clarke, 381-424. Methuen, London.

Dunnell, Robert C.
1970 Seriation Method and its Evaluation. *American Antiquity* 35:305-319.

Fitting, James E.
1972 Evolution, Statistics and Historic Ceramics. *The Conference on Historic Site Archaeology Papers* 6:158-162.

James, D. Clayton
1968 *Antebellum Natchez*. Louisiana State University Press, Baton Rouge.

Jelks, Edward B.
1972 Comments on Stanley South's Paper: "Evolution and Horizon as Revealed in Ceramic Analysis in Historical Archaeology." *The Conference on Historic Site Archaeology Papers* 6:175-178.

Kintigh, Keith W.
1985 *Settlement, Subsistence, and Society in Late Zuni Prehistory*. University of Arizona Anthropological Papers 44. University of Arizona Press, Tucson.

Liggett, Barbara
1972 Answer to South, *et al. The Conference on Historic Site Archaeology Papers* 6:195-200.

Marquardt, William H.
1978 Advances in Archaeological Seriation. In *Advances in Archaeological Method and Theory*, Vol. 1, edited by Michael B. Schiffer, 257-314. Academic Press, New York.

Miller, George L.
1991 A Revised Set of CC Index Values for Classification and Economic Scaling of English Ceramics from 1787 to 1880. *Historical Archaeology* 25(1):1-25.

Mood, Alexander M., and Franklin A. Graybill
1963 *Introduction to the Theory of Statistics*. 2nd ed. McGraw-Hill, New York.

Noel Hume, Ivor
1970 *A Guide to Artifacts of Colonial America*. Knopf, New York.

Rouse, Irving
1967 Seriation in Archaeology. In *American Historical Anthropology: Essays in Honor of Leslie Spier*, edited by C.L. Riley and W.W. Taylor, 153-195. Southern Illinois University Press, Carbondale.

Salwen, Bert, and Sarah T. Bridges
1977 Cultural Differences and the Interpretation of Archaeological Evidence: Problems with Dates. *Researches and Transactions of the New York State Archaeological Association* 17(1):165-173.

Schiffer, Michael B.
1975 Arrangement vs. Seriation of Sites: A New Approach to Relative Temporal Relationships. In *The Cache River Archeological Project: An Experiment in Contract Archeology*, assembled by Michael B. Schiffer and John H. House. Arkansas Archeological Survey Research Series 8. Fayetteville.

South, Stanley A.
1972 Evolution and Horizon as Revealed in Ceramic Analysis in Historical Archaeology. *The Conference on Historic Site Archaeology Papers* 6:71-116.
1977 *Method and Theory in Historical Archaeology*. Academic Press, New York.

Swearingen, Mack B.
1934 *The Early Life of George Poindexter: A Story of the First Southwest*. Privately printed at Tulane University Press, New Orleans; distributed by the University of Chicago Libraries, Chicago.

Sydnor, Charles S.
1938 *A Gentleman of the Old Natchez Region: Benjamin L.C. Wailes*. Duke University Press, Durham.

Walker, Iain C.
1972 Comments on Stanley South's "Evolution and Horizon as Revealed in Ceramic Analysis in Historical Archaeology." *The Conference on Historic Site Archaeology Papers* 6:127-157.

Williams, Stephen
1979 Archaeological Surveys in Southwest Mississippi. In *National Geographic Society Research Reports, 1970 Projects*, 581-590. National Geographic Society, Washington, D.C.

22 Intra-Group Diversity in Midwest American Jewish Cemeteries: An Ethnoarchaeological Perspective

David Mayer Gradwohl
Iowa State University
Ames, Iowa

INTRODUCTION AND STATEMENT OF PURPOSE

Members of the Jewish faith were among the early European settlers in Brazil, the Caribbean Islands, and the eastern seaboard of North America during the seventeenth and early eighteenth centuries. These Jews were primarily Sephardim, that is, descendants of people who resided in Iberia prior to the Inquisition and the expulsion of Jews from Spain by King Ferdinand and Queen Isabella in 1492 (Blau 1976; Fishman 1973; Grinstein 1980; Handlin 1954; Learsi 1954; Levinger 1944; Sarna 1986). During the late eighteenth century and on into the nineteenth and twentieth centuries, Jewish immigrants to the United States consisted largely of Ashkenazim, whose countries of origin were in western, central, and eastern Europe. The influx of Ashkenazim to the United States in the early nineteenth century was so large, indeed, that in 1823 the noted German poet and social philosopher, Heinrich Heine, sarcastically commented that "a time would come when Jews would be munching unleavened bread on the banks of the Mississippi" (Marcus 1974, vol. 1:11). Although the first instance of Mississippian matzah munching cannot be documented at the present time, Heine was probably mistaken by at least several decades. Archival sources point to the fact that some Jews had already settled in New Orleans and the St. Louis vicinity by the beginning of the 1800s (Fleishaker 1957:3, 6). Within the next half century, a number of communities throughout the American midlands included Jewish citizenries of sufficient size for the establishment of synagogues, cemeteries, and other institutions that have been, and continue to be, ongoing expressions of the Judaic presence beyond the larger and more familiar metropolitan centers in the eastern United States.

Cemeteries, in particular, offer a tangible source for studying these historical events and for analyzing the dynamics wherein groups and individuals maintain their particular identities within the larger American population (Dethlefsen 1981, 1977; Meyer 1989; Nutty 1984). These data are especially amenable to analysis by the techniques of ethnoarchaeology wherein material cultural patterns can be linked with specific ideas from written texts of the historic past and the cognitive domains expressed by the words and observed actions of living people. The purpose of this paper is to discuss Jewish identities as manifested in cemeteries and associated mortuary behavior patterns in the midwestern United States. Most studies of cultural diversity in America have dealt with *inter*-group differences and identities. Particular attention has been devoted to defining boundary-maintaining mechanisms *between* groups. Oftentimes, however, the diversity of subgroups *within* the larger groups is as extreme as the differences between those groups. This study focuses on matters of *intra*-group diversity that can be observed in Orthodox, Conservative, and Reform

Jewish cemeteries. The differences are considerable; and they offer some interesting challenges to both the prehistorian and the historic archaeologist in exploring the reasons for variations within material cultural assemblages.

RESEARCH METHODS AND CONCEPTS

As stated above, the present study employs the ethnoarchaeological approach to analyzing and interpreting human behavior. This methodological framework was formalized during the late 1960s and 1970s, although examples of the approach can be found in the literature prior to the time when the term "ethnoarchaeology" was coined. The reports of Richard Gould on his fieldwork among the Tolowa of California and the aboriginal people of Australia are usually regarded as marking the beginning of ethnoarchaeology in its contemporary usage (Gould 1966; 1968). William Rathje's (1974) Tucson garbage project, John Yellen's (1976) work among the !Kung hunters of the Kalahari desert, William H. Adams' (1977) research on the Silcott community in Washington, and Lewis Binford's (1978) study of the Arctic Nunamiut were other significant contributions to the growth of the ethnoarchaeological approach. Gould's 1978 edited volume entitled *Explorations in Ethnoarchaeology* and his *Living Archaeology* published in 1980 firmly established the importance of ethnoarchaeology in the literature and in the repertoire of methods employed by archaeologists around the world. A minimalist definition of ethnoarchaeology is offered by Rathje and Schiffer (1982:391): "The study of living societies by archaeologists." These authors further elaborate the approach by stating that "Ethnoarchaeologists document events from two perspectives: the artifacts involved, and associated behaviors and beliefs" (Rathje and Schiffer 1982:196). More recently, David Hurst Thomas (1989:654) has defined ethnoarchaeology as "The study of contemporary peoples to determine processual relationships that will aid in unraveling the archaeological record."

The present research project fits the above definitions in that historic and contemporary mortuary patterns among midwestern American Jews are being observed by archaeologists. The principal data dealt with here have been collected in Lincoln, Nebraska and Des Moines, Iowa by my wife, Hanna Rosenberg Gradwohl, and me since 1982. Preliminary results of our research in Lincoln have been published elsewhere (Gradwohl and Gradwohl 1988); our work in Des Moines is still in progress. In our project both artifacts and associated behaviors and beliefs are considered. Artifacts consist primarily of mortuary monuments (vertical gravestones that are perpendicular to the ground) and markers (horizontal gravestones that are parallel to the ground surface); other structural considerations involve the location and internal organization of the cemeteries. The sample includes eight cemeteries (two Reform, two Conservative, and four Orthodox) containing nearly 4,000 gravestones. We are attempting to record all monuments and markers in this phase of our study rather than dealing with a stratified or random sample. To facilitate this operation and collect data suitable for computer analysis, we have developed a field record form with variables including gravestone form and size, social unit represented, epitaphs, language of inscriptions, and symbols utilized.

Beyond the standard discussions of Jewish death and mourning customs (Lamm 1981; Klein 1979; Trepp 1971), the behavior and belief systems over the past century and a quarter are being studied in three primary ways. First, we are examining archival documents such as deeds, articles of incorporation, and other written statements associated with the establishment of the cemeteries. Second, we are collecting oral historical and written information pertaining to contemporary mourning customs. In Judaism, for example, records are kept and rituals are performed vis-a-vis the *jahrzeit* or death-anniversary of a deceased relative. At the synagogue and/or home, a light or candle is customarily lit and the *Kaddish* prayer is recited. Oftentimes family members additionally visit the grave of the deceased kin to recite the *Kaddish* on these occasions. Included in our database relative to the observance of *jahrzeits* are personal letters, death anniversary lists in family prayerbooks, and separate books for the recording of death anniversaries.

Third, we are conducting interviews and collecting information on extant Jewish burial practices. Data have been obtained from funeral directors, members of the *Chevra Kadisha*, or Holy Burial Society, and personnel from cemetery monument companies. In Lincoln the *Chevra Kadisha* keeps its equipment at the Butherus-Maser & Love Mortuary, which conducts most of the funerals for the city's Conservative and Orthodox Jews. On the other hand, most of Lincoln's Reform Jews use the Roper and Sons Mortuary and do not avail themselves of the services of the *Chevra Kadisha*. In Des Moines, the *Chevra Kadisha* is operated from the Beth El Jacob Synagogue. Interview data coupled with a perusal of published obituary notices verify that almost all Jewish funerals in Des Moines are conducted by Dunn's Funeral Home. All of these mortuaries stock wooden coffins without metal fittings as required by Orthodox and many Conservative Jews (Lamm 1981:16-17). In Lincoln, Speidell

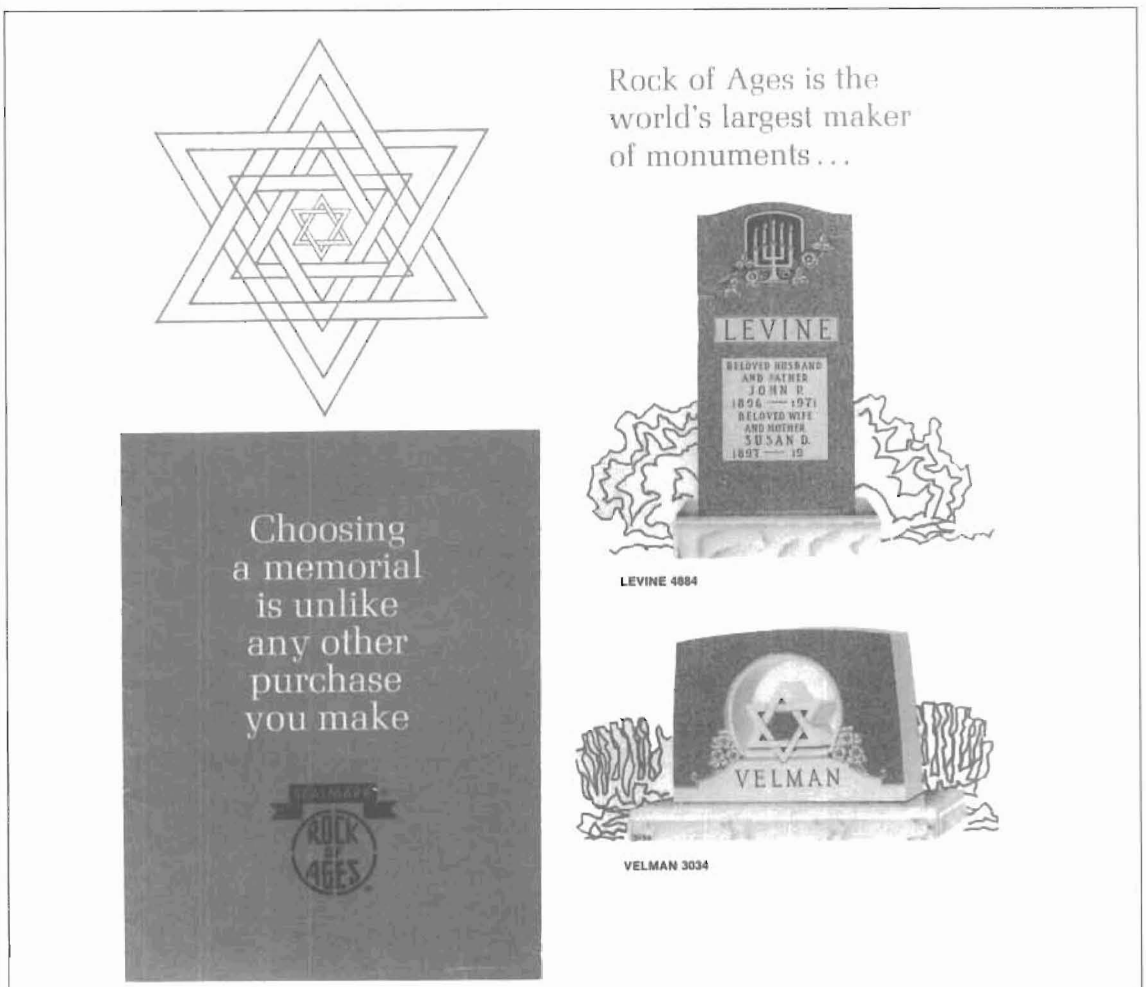


Figure 22.1. Example of sales literature used by monument companies to sell gravestones to Jewish customers (Adapted from a brochure printed by the Rock of Ages Monument Company).

Monuments Inc. handles most of Lincoln's "Jewish trade" for cemetery monuments and markers. In Des Moines, most Jews purchase their monuments and markers from the Des Moines-Winterset Monument Company, which advertises itself as "Iowa's largest memorial dealer." Most of the stock at these dealers is for Christian or non-denominational clients, while others are specifically for Jewish customers. Several of their brochures are aimed specifically at customers of Jewish faith (see Figure 22.1). Jewish motifs, for example, are used in illustrations of the monuments. Additional texts explain some of the traditional Judaic symbols: the *Torah* scrolls, the *menorah* or candelabrum, the Star or Shield of David known as the *Mogen David*, and the *jahrzeit* light of remembrance (see Figure 22.2). Customers are also informed that epitaphs may be sculptured in Hebrew letters as well as English. To facilitate their job of carving inscriptions, the monument dealers have a template in which the letters of the Hebrew alphabet have been assigned numbers. Jewish customers–or more frequently, their rabbis–write out a number sequence for the desired Hebrew epitaph. One template at the Des Moines-Winterset Monument Company bears the hand-written annotation 6-3-22-17-27 which is the pentagram of five Hebrew letters standing for an often-used phrase translated as "May his [her] soul be bound up in the bond of eternal life" (Tehee nafsho[h] tzerurah bitzror ha-hayim)–see Figure 22.2L (lower). Our

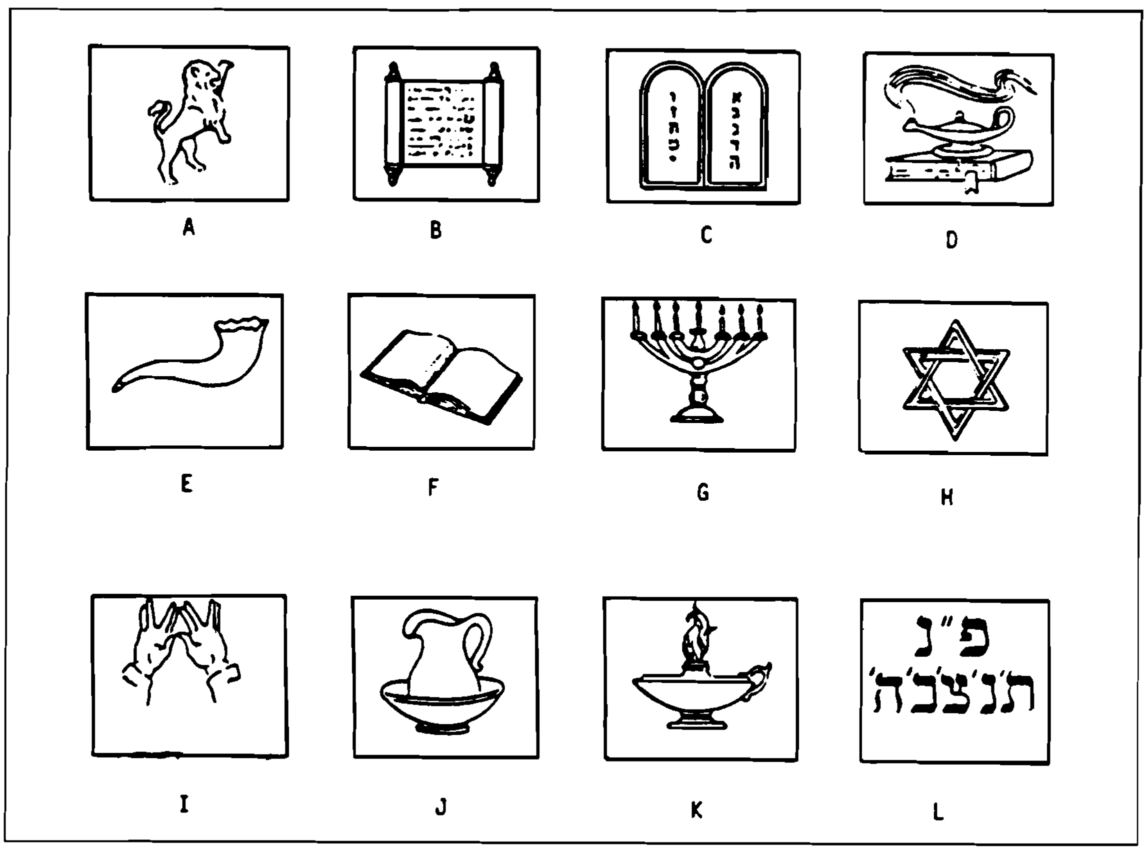


Figure 22.2. Jewish symbols from pamphlet "How to Choose Your Monument" printed by Rock of Ages Monument Company. (A) Lion of Judah; (B) Torah or Scroll of the Pentateuch; (C) Ten Commandments or Mosaic Decalogue; (D) Bible and Lamp–Light of the Soul; (E) Shofar; (F) Bible, book, knowledge; (G) Menorah; (H) Star of David, shield, divine protection; (I) Kohanim hands raised in priestly benediction; (J) Pitcher and bowl, symbol of the Levites; (K) Jahrzeit lamp, remembrance; (L) Upper: "Here Lies," Lower: "May his/her soul be bound up in the bond of eternal life."

information on contemporary burial practices also includes observations on the setting of new stones and their ritual unveiling or dedication by the family of the deceased. This ceremony usually, but not always, occurs one year after the death of the person being memorialized.

As indicated above, most of the data for our research can be collected in an objective manner. We can also approach the scene as participant observers. In Lincoln, especially, we are able to exploit the subjective experiences we had in Jewish congregations while growing up in that city. Since our families still reside there, we are able to utilize networks of kinship and friendship in gathering information and cross-checking conflicting sources. Some of this information (for example, questions pertaining to intermarriage and religious conversion) is potentially sensitive and must be kept confidential, but it is often crucial to understanding different kinds of Judaic identity as expressed in the cemeteries. Since Ames (our residence for 30 years) is only 40 miles from Des Moines, participant observer roles have also facilitated our work there though to a lesser degree than in Lincoln. Beyond the systematically-gathered databases from Lincoln and Des Moines, we have some comparative information from Jewish cemeteries in Nevada, Missouri, Illinois, Indiana, Kentucky, Pennsylvania, New York, New Jersey, Rhode Island, Virginia, North Carolina, and Georgia. The patterns discerned for the Ashkenazim in the Midwest are paralleled elsewhere in the United States although, as might be expected, the situation in certain metropolitan areas, such as Chicago and New York City, is more complex due to the presence of sizeable groups of Sephardim and various sects of Chasidim, the ultra-Orthodox Jews from Eastern Europe. In line with Thomas's above-quoted definition of ethnoarchaeology, however, our database on contemporary Jewish mortuary patterns is sufficient to shed light on processual relationships latent in the archaeological record. In this case, the processes include cultural continuity and conservatism, culture change, acculturation, and the dynamics of ethnic survivals.

The ultimate definition of who is a Jew involves an arena where angels fear to tread–perhaps because the rabbis, Talmudic scholars, gastronomic wizards, and political activists are too busy doing battle. Folk humor states that where there are three Jews there will be four different opinions. For the academic purposes of this paper, however, Jews are considered an ethnic group linked through time and space by some adherence to Judaism. Their observed mortuary behavior is regarded as one expression of their ethnicity. The term "ethnic group" is used here in the strict sense defined by George DeVos (1975:9):

> An ethnic group is a self-perceived group of people who hold in common a set of traditions not shared by others with whom they are in contact. Such traditions typically include 'folk' religious beliefs and practices, language, a sense of historical continuity, and common ancestry or place of origin. The group's actual history often trails off into legend or mythology, which includes some concept of an unbroken biological-genetic generational continuity, sometimes regarded as giving special characteristics to the group.

DeVos goes on to point out some of the dimensions along which ethnicity may be reflected in material culture, a matter of importance to the ethnoarchaeologist:

> the ethnic identity of a group of people consists of their subjective symbolic or emblematic use of any aspect of culture in order to differentiate themselves from other groups. These emblems can be imposed from the outside or embraced from within.

> Ethnic features such as language or clothing or food can be considered emblems, for they show others who one is and to what group one belongs. A Christian, for example, wears a cross; a Jew, the Star of David.

DeVos could have gone one step further, for the ethnic identities of Jews are manifested not only in life, but in death. Death and life reflect each other as expressed in some of the Hebrew euphemisms for cemetery: *Beth A Haim* means "House of Life" and *Beth Olam* means "House of Eternity" (Ydit 1971:272).

HISTORICAL BACKGROUND

Detailed considerations of the settlement of Iowa and Nebraska by citizens of Jewish faith have been presented elsewhere. The history of the Jewish settlement of Iowa has been discussed by Fleishaker (1957), Glazer (1904), Rosenthal (1957), and Wolfe (1941), while the experience of Jews in Nebraska has been chronicled by Auerbach (1927), Gendler (1968), Levitov (1976), Newmark (1981), and Rosenbaum and O'Conner-Seger (1981). Therefore, only a few salient historical events are summarized here. The written history of Iowa began in June of 1673 when the French explorer, Louis Jolliet, in the company of a Jesuit priest, Jacques Marquette, and five voyageurs descended the Wisconsin River to its confluence with the Mississippi River. The land across the river to the west, intermittently claimed by French and Spanish authorities, was acquired by the United States from France in 1803 as part of the Louisiana Purchase. In the meantime, Julien Dubuque had prospected for galena, the mineral from which lead is smelted, and established his "Mines of Spain" in a locality subsequently known as Dubuque, Iowa. Between 1830 and 1851, the United States Government entered into treaties with the resident Native Americans and secured the land for white settlement. The Iowa Territory was established in 1838, having been included for the previous four years within the Michigan and Wisconsin territories.

In 1833 a French Jew named Alexander Levi settled in the Mississippi River community of Dubuque. He opened a grocery store there and distinguished himself in 1837 by travelling to St. Louis and becoming the first foreigner to be naturalized as a citizen of Iowa. By 1846, the year in which Iowa achieved statehood, a community–subsequently named Des Moines–was established at the confluence of the Des Moines and Raccoon rivers. One of the earliest settlers in Des Moines was William Krouse, a merchant of Jewish faith who was born in Germany. In 1857 the state capital was moved from Iowa City to Des Moines, which by that time boasted a population of over 3,500 people. As the city's population continued to grow, Jewish merchants, peddlers, businessmen, and their families joined the commercial enterprise. A number of small shops ultimately evolved into large stores; some of these are still in business today. During the 1860s Jewish families apparently met in private homes or business establishments to celebrate religious holidays.

By 1870, the need for more permanent religious institutions was perceived. In that year the first corporate expression of Judaism in Des Moines occurred with the incorporation of the Emanuel Cemetery Association. In the words of Frank Rosenthal (1957:50):

> To perform the last rites for a fellow Jew and to bury him in consecrated ground was the sacred duty of every Jewish adult. Thus it became of utmost importance to purchase a cemetery without further delay. By 1870 there were enough Jews living in Des Moines to make such a step possible; and in July of that year, the men of the Westside bought a piece of land adjoining Woodland Cemetery and the Emanuel [Cemetery] Association was duly incorporated.

Today, some 12 decades later, there are between 3,000 and 3,500 Jews living in Des Moines.

Meanwhile, separate Jewish congregations formed in Des Moines along lines of theological traditions, ritual distinctions, and differences in national origin. Also operating, as it still does today, was the geographic factor of the Des Moines River, which divides the city's "Eastside" from the "Westside." The state capitol building was constructed on the Eastside between 1870 and 1886. The first settlement, however, occurred on the Westside, and that area continued to be the favored location of residences and businesses for the more established and prosperous citizens. The majority of early pioneer Jews in Des Moines settled on the Westside. In 1873 they incorporated a congregation called Temple B'nai Jeshurun, and they soon embraced the principles and practices of Reform Judaism. Shortly later, in 1876, a group of Orthodox Jews on the less-prosperous Eastside founded the Congregation B'nai Israel, later known as the Children of Israel Synagogue. One faction within the Children of Israel Synagogue felt that the congregation was too liberal. Hence, they resigned and formed the Beth El Jacob Synagogue that, to this day, is the most Orthodox of the synagogues in Des Moines. In 1901 a second faction within the Children of Israel Synagogue perceived the mandatory practices of that congregation as too Orthodox. Therefore, they split off and founded the Tifereth Israel Synagogue that followed the rituals of Conservative Judaism, a movement that emerged as an essentially middle-of-the road third "branch" of Judaism in the United States during the nineteenth century. Just as the temple and three synagogues have had separate sanctuaries in which the living pray, so they have had separate cemeteries in which to bury their dead. Since 1879, B'nai Jeshurun has owned Emanuel Cemetery for the burial of Reform Jews. In 1884, the Children of Israel established an Orthodox burial ground known as the Eastside Cemetery. In 1904, the newly-founded Tifereth Israel Synagogue established a cemetery for Conservative Jews contiguous with Des Moines' municipal Glendale Cemetery. Meanwhile, the burial space at the Eastside Cemetery was approaching its limit. Consequently, Des Moines' Orthodox Jews acquired three separate spaces adjacent to the Tifereth Israel Cemetery: one for the Children of Israel Synagogue, one for Beth El Jacob Synagogue, and one for a fraternal organization known as the Order of Brith Abraham. These four separate but adjacent burial areas are referred to collectively as "Jewish Glendale."

The history of Jews in Nebraska parallels that in Iowa, though it starts somewhat later. During Nebraska's Territorial days, in the late 1850s and early 1860s, some Jewish traders were among the inhabitants of Omaha. One of the more colorful businessmen was Julius Meyer, a native of Germany, who plied his trade with the regional Omaha, Ponca, Pawnee, and Lakota Indians. Settlement of the area by Jewish families increased following the admission of Nebraska as a state in 1867. During the 1870s and 1880s members of the Jewish faith were residents of several Nebraska cities and towns, where they were primarily engaged in mercantile enterprises. During this time–and possibly as early as the 1860s–some Jews had settled in Lincoln. Two separate congregations were formed during the 1880s. Temple B'nai Jeshurun was incorporated in 1884 and explicitly committed to the principles of Reform Judaism. In 1885 Tifereth Israel Synagogue was founded and ultimately evolved from Orthodox to Conservative Judaism. Even today, saying one belongs to "the Temple" or "the Synagogue" is, in a sense, a cognitive code expressing some degree of social distance, differing philosophical orientations, and varying observances of specific religious rituals. Under these circumstances, it is not surprising that two separate Jewish cemeteries were established. In 1886, the Chebra B'nai Jehuda Cemetery Association purchased land for a Conservative/Orthodox cemetery now known as the Mount Carmel Cemetery at the northern end of the city limits. Legally, the Mount Carmel Cemetery Association is an entity separate from Tifereth Israel Synagogue, although many Jews in Lincoln are not aware of that fact. On the other hand Lincoln's Reform Jews were buried at the Mount Lebanon Cemetery, originally located within the Yankee Hill Cemetery south of town. In 1899 the Mount Lebanon Cemetery was moved closer to Lincoln's metropolitan center–that is, a section within the Wyuka Cemetery that had been founded as a state and municipal cemetery in 1869. In 1904 the Mount Lebanon Cemetery Association deeded the cemetery section over to B'nai Jeshurun Congregation. The Temple has been in charge of the Mount Lebanon Cemetery since that time. Today, some 1,000 to 1,200 Jews reside in Lincoln.

INTRA-GROUP THEOLOGICAL AND RITUAL DIVERSITY

Several generalizations can be made about the diversity of Jews in Iowa and Nebraska in terms of their national origin, theological orientation, and ritual practice. The majority of early Orthodox Jews immigrating to this region were *Eastern* Ashkenazim; they came largely from Russia, Poland, and the Baltic countries. In addition to the languages of the countries in which they lived, most of the Eastern Ashkenazim spoke Yiddish, a dialect of Middle High German interlaced with Hebrew words and local idioms. Their newspapers and books were written in Yiddish with Hebrew letters. These Jews brought with them, and continued, the theological orientation and liturgical practices of European Orthodoxy (cf. Bamberger 1971:312-315, 347-350). Members of these synagogues rather strictly observed Mosaic and rabbinical laws that regulate many areas of behavior. For example, in terms of ritual dress, men in prayer are required to wear a *yarmulke*, or skull cap, and a fringed *tallis*, or prayer shawl; during morning prayers, they also wrap leather *tefillin* or phylacteries around their foreheads and left arms. In terms of diet kosher laws prohibit the eating of pork, shellfish, and other foods; milk and meat foods may not be mixed or eaten at the same meal; separate dishes and food preparation utensils must be used for milk and meat products. Priestly statuses–those of the *Kohanim* and *Levites*–are recognized, and many rituals insure their ritual purity. In terms of the specific forms of individual and group prayer, there are many laws and traditions: ten adult men (a *minyan*) are required for communal prayers, men and women are seated separately within the sanctuary, the chanting of certain prayers is done by a cantor, boys go through the ritual of *Bar Mitzvah* at age 13, girls normally do not receive a formal religious education, women are required to cleanse themselves in a *mikveh* or ritual bath after their menstrual periods. It should be noted that some of these Orthodox practices are directly manifested in terms of material culture; others manifested indirectly; and still others are probably not reflected in any material culture context.

On the other hand the early Reform Jews were primarily *Western* Ashkenazim; they came mostly from Germany, Austria, Alsace-Lorraine, and France. Typically, their first language was German or French. The temples they founded in Des Moines and Lincoln adopted the principles of Reform Judaism, brought from Europe by leaders such as Rabbi David Einhorn and Rabbi Isaac Mayer Wise and adapted to the American scene (Schwartzman 1971; Silverman 1970). The Reform Jewish movement started in Europe in the late eighteenth and early nineteenth century following the emancipation of Jews in Germany and France. Although some Reform congregations were established in the United States between the 1820s and 1840s, the main American Reform theological platforms were established at the Philadelphia Conference of 1869 and the Pittsburgh Conference of 1885. Reform leaders declared their intention to "accept as binding only the moral laws and maintain only such ceremonies as elevate and sanctify our lives" in their practice of Judaism (Schwartzman 1971:215). Reform practices–which overtly rejected a number of specific Mosaic and traditional rabbinical laws–

included the equal participation of women in religious worship and education, the use of a choir and musical instruments (especially the organ) in addition to or instead of the ritual chanting of a cantor, rejection of the absolute obligation to follow kosher dietary laws, and the optional rather than required wearing of religious paraphernalia. Of particular interest to our study of the material aspects of Jewish mortuary behavior are several specific theological planks in the Pittsburgh Platform of 1885. First was a call for the use of vernacular languages (i.e., English and German) as well as Hebrew in religious services. Trilingual prayerbooks from this period are explicit artifacts of the changes in ritual practice and theological orientation in Reform Judaism. Second was the disavowal of the hope of a return to Zion, that is, a homeland in Palestine. Rabbi Wise, in fact, exclaimed "We are unalterably opposed to political Zionism. The Jews are not a nation but a religious community . . . . America is our Zion . . . . The mission of Judaism is spiritual not political" (Philipson 1936:15). A third plank rejected the social and ritual prerogatives of the priestly castes, the *Kohanim* and *Levites*.

As mentioned above, Conservative Judaism evolved, in large part, as a third branch of Judaism within the United States (Davis 1963; Sklare 1972). Among its leaders were Rabbi Solomon Schechter and Rabbi Isaac Leeser. Early on, most of the Conservative Jews were *Eastern* Ashkenazim. In their synagogues one can observe different combinations of Orthodox and Reform ritual practices. In many Conservative synagogues, for example, men are expected to wear the *yarmulkeh* and *tallis*, but are seated together with women in the sanctuary. It is not uncommon to hear choirs in addition to cantors in Conservative synagogues; organs or other musical instruments, however, would be rare in those sanctuaries. At most Conservative synagogues girls receive religious education and go through a ceremony called *Bas Mitzvah* which was developed as a ceremony parallel to that undergone by boys at age thirteen. Similarly, individual followers of Conservative Judaism select differently from the Orthodox and Reform ritual repertoires. Some follow kosher dietary rules, others do not. Some observe the rituals of the priestly castes, others do not.

DIFFERENCES IN CEMETERY LOCATIONS AND THE EMBELLISHMENT OF GRAVESTONES

Although each community has its own history and each temple or synagogue has its own *minhagim*, or particular religious customs, some generalizations can be offered concerning the two Reform Jewish cemeteries (Lincoln's Mount Lebanon Cemetery and Des Moines' Emanuel Cemetery), two Conservative Jewish cemeteries (Lincoln's Mount Carmel Cemetery and Des Moines' Tifereth Israel section of "Jewish Glendale"), and four Orthodox Jewish cemeteries (Des Moines' Children of Israel Eastside Cemetery and the "Jewish Glendale" sections for the Children of Israel Synagogue, Beth El Jacob Synagogue, and former plot for the Order of Brith Abraham, which is now managed by the Beth El Jacob Synagogue). The two Reform cemeteries consist of sections *within* larger municipal cemeteries that serve various Christian and non-denominational groups, military veterans, and secular sodalities. Both are

Figure 22.3. General view of Mt. Lebanon Cemetery, Lincoln.

Figure 22.4. General view of Emanuel Cemetery, Des Moines. Note two large mausolea in background.

relatively open spaces with little separate demarcation. From a distance, Mount Lebanon Cemetery (see Figure 22.3) is hardly recognizable as a separate section of the Wyuka Cemetery other than being bordered by curving roads on three sides and by a row of widely-spaced deciduous trees (perhaps a former fence line) on the fourth side. Emanuel Cemetery is only slightly set apart within the larger Woodland Cemetery (see Figure 22.4). A chain link fence on the west and north separates Woodland Cemetery as such from adjoining house lots. A portion of the southern boundary of the Emanuel section is marked by low bushes while the eastern boundary is not marked at all. On the other hand the

Figure 22.5. Gate of Mt. Carmel Cemetery, Lincoln.

Orthodox and Conservative cemeteries are more often separate, enclosed spaces and/or explicitly demarcated as Jewish mortuary areas. Mount Carmel Cemetery, for example, is bounded by a wrought-iron fence, coniferous trees, thick shrubs, and locked gates (see Figure 22.5). Four Stars of David embellish the gates as insignia of Judaic identity. Perhaps, given the previously-mentioned figurative reference to cemeteries as "houses," the Stars of David are also a reminder of the passage in Deuteronomy (11:20) to abide by the words of the Lord and follow the commandment that "Thou shalt write them upon the doorposts of thy house and upon thy gates . . ." In this sense, the Stars of David might

Figure 22.6. Gate and surrounding fence of Children of Israel Eastside Cemetery, Des Moines.

stand for the *mezuzah*, a small container holding parchment lettered with Biblical quotations, which is attached on the right side of doorways in traditional Jewish homes. In Des Moines, the Children of Israel Eastside Cemetery is also surrounded by a wrought-iron fence, locked gates, and, for the most part, heavy shrubbery (see Figure 22.6). There are, however, no signs or Judaic emblems on the Eastside Cemetery gate. The steep tree-covered slope down to University Avenue along the southern side of "Jewish Glendale" provides an effective boundary. The northern boundary is demarcated by a roadway, but otherwise the demarcation is not as evident as

Figure 22.7. Individual vertical monuments, Beth El Jacob Synagogue Cemetery at "Jewish Glendale," Des Moines.

with the Eastside Cemetery. At the main vehicular gate to "Jewish Glendale," however, there is a very large Holocaust memorial monument that designates the area as a Jewish space. Formerly, the Rosenbaum Memorial Chapel stood in the Tifereth Israel Cemetery section near the pedestrian path to University Avenue. Now the area is marked only by a large bronze plaque commemorating Jewish war veterans.

Analysis of our data collected stone by stone indicates even more obvious differences in the monuments and markers within these cemeteries.

Figure 22.8. Rissman monument at Children of Israel Synagogue Cemetery at "Jewish Glendale," Des Moines. Note use of English, Hebrew, and transliterated Yiddish; "Bubby" and "Zayde" mean grandmother and grandfather in Yiddish.

Eight general patterns characterize Orthodox cemeteries. First, there are typically many individual, single, vertical monuments, giving the cemetery a somewhat crowded appearance (see Figure 22.7). Many of the stones are of uniform size, perhaps reflecting the equality in which all Orthodox Jews are supposed to be buried–in simple wooden coffins and plain shrouds (*tachrichim*).

Figure 22.9. Jacob Panor monument in Children of Israel Eastside Cemetery, Des Moines. Note Stars of David plus formulaic epitaph in Hebrew as emblems of Judaic identity.

Relatively few markers or large family plots are observed. No mausolea are present–evidence of the strong rabbinical proscription against above-ground burial (Lamm 1981:57).

Second, relatively large numbers of red, pink, or black gravestones are noted. This may be a carryover from the Baltic countries or Eastern Europe, where darker colored gravestones are more available and preferred. We have, incidentally, noted this preference among the gravestones of Latvian-Americans buried in Wyuka Cemetery. The selection of darker colored stones may alternatively have something to do with conspicuous consumption, since these stones are usually more expensive than gray granites; it is possible that individuals may compete with stone quality rather than stone size per se.

Third, most of the stones exhibit Hebrew epitaphs. Occasional inscriptions in Yiddish are also noted (see Figure 22.8). These epitaphs typically include the Hebrew name of the deceased, the father's Hebrew name, and the date of death in the Jewish ritual calendar in addition to abbreviations for the phrases "Here lies" and "May his soul be bound up in the bond of eternal life" (Lamm 1981: 191-192). Thus the Hebrew inscription on Jacob Panor's monument (see Figure 22.9) is translated "Here lies Jacob, son of Zvi Panor. He died on 22 Tamuz in the year 5684. May his soul be bound up

Hebrew Months	Transliteration	Usually Coincides With (Variations of up to 3/4 month)
תשרי	Tishre	September
חשון	Heshvan	October
כסלו	Kislev	November
טבת	Tevet	December
שבט	Shevat	January
אדר	Adar	February
אדר ב·	(Adar II in leap year)	
ניסן	Nisan	March
אייר	Iyar	April
סיון	Sivan	May
תמוז	Tamuz	June
אב	Av	July
אלול	Elul	August

Hebrew Alphabet and the Numerical Values:

א - 1	ז - 7	מ - 40	ק - 100
ב - 2	ח - 8	נ - 50	ר - 200
ג - 3	ט - 9	ס - 60	ש - 300
ד - 4	י - 10	ע - 70	ת - 400
ה - 5	כ - 20	פ - 80	
ו - 6	ל - 30	צ - 90	

Figure 22.10. Chart showing correlation of Hebrew months with months in the civil calendar (Upper); Chart showing numerical values of letters in the Hebrew alphabet used for rendering dates on tombstones (Lower). Adapted from Kranzler 1979:20-21.

in the bond of eternal life." The month of Tamuz in the Jewish ritual calendar corresponds to June or July in the Gregorian calendar (see Figure 22.10). The year 5684 refers to the time that has elapsed since the presumed creation of the earth. Hebrew dates are rendered by a number system that is assigned to certain letters in the alphabet (Kranzler 1979:20-21). To get a date in the Gregorian calendar one tallies up the values of the Hebrew letters and then adds 1240. Thus the Hebrew year 5684 corresponds to the civil year 1924. As discussed previously, the death anniversary date, or *jahrzeit*, is especially important in Judaism because at that time the living traditionally recite the Kaddish prayer and perform other rituals in memory of their deceased kin. Longer epitaphs may include Biblical passages, laudatory adjectives for the deceased, or the fact that the person died on the Sabbath or a religious holiday

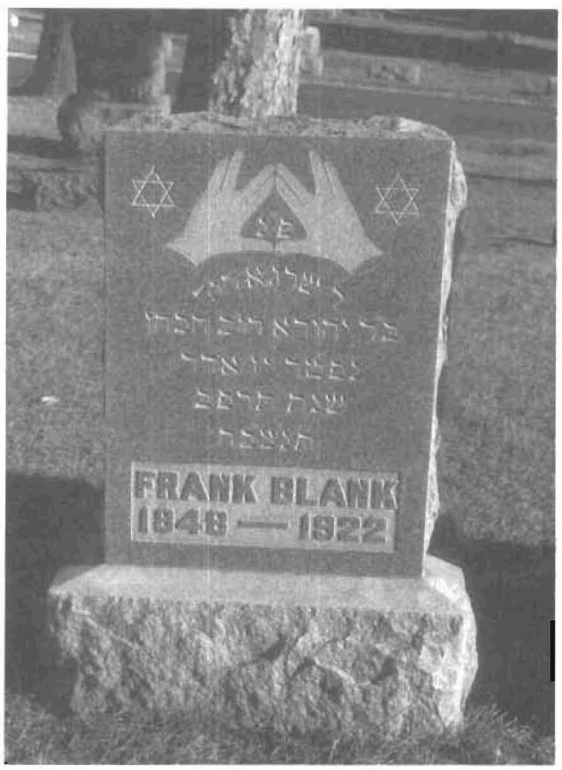


Figure 22.11. Monument of Frank Blank in Order of Brith Abraham Cemetery at "Jewish Glendale," Des Moines. Note motif of hands raised in benediction which is the emblem of the Kohanim priestly caste.

such as *Rosh Hashonah* (Jewish New Year), *Yom Kippur* (Day of Atonement), *Succos* (Festival of Booths), *Pesach* (Passover), *Shavuos* (Pentecost Festival of Weeks), and *Rosh Hodesh* (the New Moon).

Figure 22.12. Monument of Izchak Tager in Mt. Carmel Cemetery, Lincoln. Note motif of wash basin and ewer which is the emblem of the Levite priestly caste.

A fourth Orthodox characteristic consists of the use of Judaic symbols. As mentioned above, monument companies normally have special brochures that they use in selling gravestones to Jewish customers. The most frequent general Judaic symbols are the Star of David, menorah or candelabrum, lamp (representing the *jahrzeit* or death-anniversary remembrance light), scroll (the Torah or Pentateuch), book (the Bible), and tablets (the Ten Commandments). The Star (or Shield) of David is typically associated with males, while the menorah is

Figure 22.13. Detail of monument in Beth El Jacob Cemetery at "Jewish Glendale," Des Moines. Note photograph of deceased printed on porcelain.

correlated with females. It should be recalled that women have the duty of lighting the Sabbath and holiday lights.

Fifth, we note emblematic or epigraphic references to the priestly castes. The *Kohanim* or high priests are indicated by a symbol representing hands with spread fingers raised in benediction (Figure 22.11). In other instances the priestly status is indicated by the name of the deceased's father rather than an artistic symbol. For example, the epitaph of

Figure 22.14. Krantz monument in Tifereth Israel Synagogue Cemetery at "Jewish Glendale," Des Moines. Note pebbles at base of monument left as "calling cards" by those who have visited the deceased.

"Reb Abraham Bar Moshe Ha-Kohen" (Mr. Abraham, son of Moses, the Kohen, or high priest) would signify that Abraham was also a Kohen on the basis of patrilineal descent. A female, of course, cannot inherit priestly status, but the status of her father would be indicated by the rendering of her Hebrew name on her gravestone–for example, "Rachel, daughter of Abraham Ha-Kohen." Similarly, the status of the *Levites*, the caste of temple attendant priests who are traditionally responsible for the cleanliness of the sanctuary, might be indicated by the symbol of a ewer and basin (see Figure 22.12). Or the status might be signified in the epitaph as, for example, that of "Reb Abraham Zvi Bar Shmuel David Ha-Levy" (Mr. Abraham Zvi, son of

Figure 22.15. Central family monument and associated markers of the Mayer and Schlesinger extended families at Mt. Lebanon Cemetery, Lincoln.

Samuel David, the Levy, or temple attendant priest).

A sixth Orthodox-associated trait is also exhibited by such epitaphs, namely, the use of the title or term of address, "Reb." While this term can be an abbreviation for the title of rabbi, it more often is used as a term of respect roughly rendered as "mister."

A seventh trait, probably associated with Eastern Europe, consists of the use of photographs on tombstones (see Figure 22.13). Officially this practice is strongly discouraged by most Orthodox rab-

Figure 22.16. View of Emanuel Cemetery, Des Moines. Note large central family monuments and associated markers in foreground; two large mausolea show in the background.

bis in terms of the avoidance of the use of graven human images (Lamm 1981:191). In this instance, however, folk tradition often wins out over rabbinic proscription.

An eighth Orthodox-associated pattern consists of the presence of pebbles deposited on a number of gravestones (see Figure 22.14). This tradition in Orthodoxy may represent memorial "calling cards" left by mourners or possibly a vestige of the very ancient practice in which funeral attendants actually filled the pit with dirt after the coffin was lowered into the grave (Bocher 1976).

Figure 22.17. Meyer Family monument and associated markers in Mt. Lebanon Cemetery, Lincoln. Note exclusive use of English on inscriptions.

The Reform Jewish cemeteries present five general characteristics that differ strikingly from the Orthodox cemeteries. First, there are large family plots with relatively imposing central monuments and associated markers (see Figure 22.15). In Des Moines the limits of these family plots may be further demarcated by stone curbs. Also found in Des Moines' Emanuel Cemetery are two mausolea that symbolize the family in material structural form (see Figure 22.16). Generally throughout the United States we note that above-ground disposal of the dead is permitted in Reform Jewish cemeteries but strictly prohibited in the Orthodox tradition. Mount Lebanon Cemetery in Lincoln has no mausolea, perhaps because they may be restricted to certain sections of Wyuka.

Figure 22.18. Marker of Edward Cerf at Mt. Lebanon Cemetery, Lincoln. Note the indication of place of birth (Phalsbourg, France) and death (Lincoln, Nebraska); note also the Hebrew pentagram standing for the phrase "May his soul be bound up in the bond of eternal life."

Second, most of the Reform monuments and markers are hewn out of gray granite. Third, the majority of monuments and markers have no Hebrew epitaphs. Inscriptions are normally in English only (see Figure 22.17). At Mount Lebanon, for example, 97.5% of the monuments exhibited

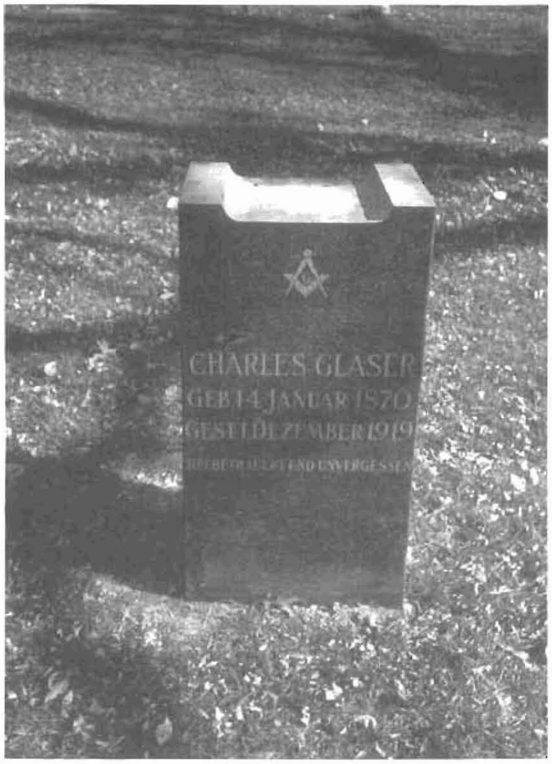


Figure 22.19. Monument of Charles Glaser at Mt. Lebanon Cemetery, Lincoln. Note in particular the inscription in German.

inscriptions in English only. Hebrew, if used at all, is limited to short epitaphs that are normally abbreviated. Most frequently used, for example, are the two letters signifying "Here lies" and the pentagram standing for "may his soul be bound up in the bond of eternal life." Exceptions occur in Des Moines' Emanuel Cemetery, which was the city's only Jewish burial ground until 1884.

Fourth, relatively few Judaic religious symbols are found on gravestones in the Reform cemeteries. Even the monument and marker of Rabbi Eugene Mannheimer, spiritual leader of Des Moines' Reform Jews for almost half a century, have no Judaic indicator other than the title "rabbi." At Mount Lebanon Cemetery, 93% of the gravestones exhibited no Judaic symbols at all. Emblems of the *Kohanim* and *Levites* are not found on gravestones in the Reform cemeteries since Reform Judaism, in a stride toward egalitarianism, repudiated those priestly castes during the nineteenth century.

Fifth, a significant number of gravestones in Reform cemeteries indicate the deceased's place of birth–almost invariably in Western Europe or the United States–reflecting the fact that most of these people are *Western* Ashkenazim (see Figure 22.18). The only instances of German epitaphs are recorded in Reform cemeteries (see Figure 22.19). Place of birth is only rarely indicated on gravestones in Orthodox or Conservative cemeteries.

The intermediate position of Conservative Jews is, as predicted, found not only in their synagogues but in their cemeteries. This is particularly clear in the Tifereth Israel Cemetery in Des Moines, where that synagogue was formed as an intentionally progressive split from Orthodoxy. Lincoln's Tifereth Israel Synagogue, on the other hand has

Figure 22.20. Monument of Bertha Goldberg at Mt. Carmel Cemetery, Lincoln. Note the inscription in Yiddish written in Hebrew letters.

always accommodated whatever Orthodox Jews resided in that city. In Conservative cemeteries one can expect to find some single monuments designating individuals in the Orthodox fashion and also some large family monuments with many markers following a Reform practice. In Des Moines' Conservative Jewish cemetery there is one mausoleum; curiously enough, the burial structure was erected by a family of well-known philanthropists who, historically, have been pillars of the Reform Temple, B'nai Jeshurun. While in Des Moines the mausoleum may seem out of place, they are not infrequent in Conservative cemeteries we have seen in New York and other large cities. Some monuments at Mount Carmel and at Des Moines' Tifereth Israel Cemetery have an impressive array of Judaic religious symbols (including the insignia of the

Figure 22.21. Monument of M. Hoffman in Tifereth Israel Synagogue Cemetery at "Jewish Glendale," Des Moines. Note the Yiddish inscription and also the symbol of the Arbeiter Ring or Workmen's Circle, a Yiddish cultural and social welfare sodality.

Kohanim and *Levites*) or long epitaphs in Hebrew following the Orthodox tradition. That *minhag* is also manifested in the frequent use of photographs on tombstones and the occasional presence of pebble "calling cards" deposited by the living at the graves of their deceased family members. Occasional epitaphs in Yiddish point to the *Eastern* Ashkenazi origin of the majority of early Conservative Jewish families. This Old World connection is manifested in Yiddish epitaphs rendered in Hebrew characters (see Figure 22.20) or occasionally in English letters. In one case a monument exhibits the symbol of the *Arbeiter Ring* or Workman's Circle, a fraternal organization oriented toward Yiddish culture and language (see Figure 22.21). The use of non-religious symbols could be interpreted as a concession to secularism, but occupational symbols are known to occur frequently in Old World Orthodox cemeteries. On the other hand some gravestones fit the Reform paradigm in their total absence of Jewish religious symbols and Hebrew epitaphs.

SUMMARY AND CONCLUSIONS

As discussed above, there are clear distinctions between Reform, Conservative, and Orthodox Jewish cemeteries in Iowa and Nebraska. Differences in material forms are associated with differences in behavioral and ideational systems. At the same time there are some striking exceptions to these overall patterns. These cases may frustrate the process of tidy categorization, but they are extremely instructive as to the dynamics of individual and group Judaic identities. A thorough analysis of these exceptions is beyond the scope of this particular discussion. Suffice it to say, however, that members of Orthodox or Conservative congregations may go to their eternal rest in Reform cemeteries if they choose to have their bodies cremated or if they wish to be buried with a spouse who has not converted to Judaism. Either of these factors would undoubtedly bar their burial in any of Des Moines' Orthodox cemeteries and probably the Conservative cemeteries as well. In order to be buried at Mount Carmel Cemetery, for example, there are two requirements: you must be a member of the Mount Carmel Burial Association and you must be Jewish. Another factor that blurs the distinction at Emanuel Cemetery is the fact that all of Des Moines' Jews, regardless of intra-denominational preference, were buried there prior to the establishment of the Children of Israel Synagogue's Eastside Cemetery. On the other hand, especially in Des Moines, a variety of socio-geographic reasons may find Reform Jews buried at "Jewish Glendale" rather than in Temple B'nai Jeshurun's Emanuel Cemetery. Increasingly, the printed obituaries of Des Moines' Jews indicate that the congregants of Temple B'nai Jeshurun eschew burial at Emanuel Cemetery–which is now, in effect, an inner city cemetery–in favor of "Jewish Glendale," which is farther west. It should be noted that this apparent preference for burial location parallels the development of new and fashionable residential suburbs in west Des Moines.

In conclusion, we have summarized evidence showing that the separate location of cemeteries, differing inscriptions and symbols on gravestones, and varying burial practices reflect historical, social, linguistic, and theological distinctions among adherents of Reform, Conservative, and Orthodox Judaism. In many ways the open or relatively unbound spaces of the Reform Jewish cemeteries *within* municipal cemeteries are a paradigm for the kind of integration the Western Ashkenazi Jews quickly assumed in American society. The lack of Judaic emblems on their gravestones, however, should not be mistaken for assimilation if, by that term, a loss of cultural heritage is meant. The essence of classical Reform Judaism, indeed, emphasized the choice of cognitive principles and behaviors rather than adherence to traditional material boundary-maintaining practices. The sheer fact that Reform Jews in Lincoln and Des Moines choose to be buried in cemeteries consecrated as "hallowed ground" is a powerful cognitive and behavioral matter in itself. On the other hand Orthodox Jews continue to express their cultural and ideational distinctiveness by maintaining separate cemeteries that are normally closed or bound spaces, marked by Judaic symbols, and organized in a manner expressing their long-standing religious traditions. The Conservative Jewish cemeteries, in effect, provide a middle ground for the continuum of varying material forms and somewhat polarized ideas in Judaism. This correlation of different material configurations with specific behavioral patterns and cognitive domains is possible through the ethnoarchaeological approach. The dimensions of the *intra*-group variations are instructive taken along with the *inter*-group distinctions that have been typically emphasized in the literature. The demonstration of this sort of within-group variation should offer some additional understanding of the kinds of differences archaeologists observe in material assemblages from contexts that cannot as easily be linked to known behavior patterns.

Furthermore, the ethnoarchaeological data from Jewish cemeteries in this study offer intriguing insights into the processes of ethnicity and individual ethnic identities. Once more I return to the words of George DeVos (1975:17) who argued that:

> Ethnicity . . . . is in its narrowest sense a feeling of continuity with the past, a feeling that is maintained as an essential part of one's self-definition. Ethnicity is also intimately related to the individual need for collective continuity. The individual senses to some degree a threat to his own survival if his group or lineage is threatened with extinction. Ethnicity, therefore, includes a sense of personal survival in the historical continuity of the group . . . . If one's group survives, one is assured of survival.

Applying DeVos' statement to the cemetery data, we maintain that the choices that families make in regard to place of burial and the embellishment of gravestones are material expressions of ethnicity.

Furthermore, along the dimension of *intra*-group variations, these choices are a material index of some differing strategies of survival among Jewish groups and individuals within the larger American society. The symbols, literally chiseled into stone, are tangible emblems of ongoing group traditions and individual identities. The dead enter the everlasting House of Life and are associated with the emblems that identified them during their ephemeral life on earth. The living kindred and friends visit the cemetery to recite *Kaddish* prayers, observe *jahrzeit* anniversaries, and express the bonds of relationship that even death cannot shatter. In so doing, the living reinforce their own sense of personal identity and that of the group to which they belong.

REFERENCES

Adams, William H.
1977 *Silcott, Washington: Ethnoarchaeology of a Rural American Community.* Reports of Investigations 54. Laboratory of Anthropology, Washington State University, Pullman.

Auerbach, Ella Fishman
1927 Jewish Settlement in Nebraska: General Survey. Unpublished typescript. Nebraska State Historical Society, Lincoln.

Bamberger, Bernard J.
1971 *The Story of Judaism.* Schocken Books, New York.

Binford, Lewis
1978 *Nunamiut Ethnoarchaeology.* Academic Press, New York.

Blau, Joseph L.
1976 *Judaism in America: From Curiosity to Third Faith.* University of Chicago Press, Chicago.

Bocher, Otto
1976 *Der Alten Juden Friedhof in Worms.* Gesellschaft Buchdruckerei, Neuss.

Davis, Moshe
1963 *The Emergence of Conservative Judaism: The Historical School in 19th Century America.* Jewish Publication Society of America, Philadelphia.

Dethlefsen, Edwin S.
1977 Social Commentary from the Cemetery. *Natural History* 86(6):32-39.
1981 The Cemetery and Culture Change: Archaeological Focus and Ethnographic Perspective. In *Modern Material Culture: The Archaeology of Us*, edited by R.A. Gould and M.B. Schiffer, 137-159. Academic Press, New York.

DeVos, George
1975 Ethnic Pluralism: Conflict and Accommodation. In *Ethnic Identity: Cultural Continuities and Change*, edited by George DeVos and Lola Romanucci-Ross, 5-41. Mayfield Publishing Co., Palo Alto.

Fishman, Priscilla
1973 *The Jews of the United States.* Quadrangle, New York.

Fleishaker, Oscar
1957 The Illinois-Iowa Jewish Community on the Banks of the Mississippi River. Ph.D. dissertation, Yeshiva University, New York.

Gendler, Carol
1968 The Jews of Omaha: The First Sixty Years. M.A. thesis, University of Omaha, Omaha.

Glazer, Simon
1904 *The Jews of Iowa: A Complete History and Accurate Account of Their Religious, Social, Economical and Educational Progress in This State.* Koch Brothers Printing Co., Des Moines.

Gould, Richard A.
1966 *Archaeology of the Point St. George Site, and Tolowa Prehistory.* University of California Publications in Anthropology 4. Berkeley.
1968 Living Archaeology: the Ngatatjara of Western Australia. *Southwestern Journal of Anthropology* 24(2):101-122.
1978 (editor) *Explorations in Ethnoarchaeology.* University of New Mexico Press, Albuquerque.
1980 *Living Archaeology.* Cambridge University Press, Cambridge.

Gradwohl, David Mayer, and Hanna Rosenberg Gradwohl
1988 That is the Pillar of Rachel's Grave Unto This Day: An Ethno-archaeological Comparison of Two Jewish Cemeteries in Lincoln, Nebraska. In *Persistence and Flexibility: Anthropological Perspectives on the American Jewish Experience*, edited by Walter P. Zenner, 223-259. State University of New York Press, Albany.

Grinstein, Hyman B.
1980 *A Short History of Jews in the United States.* Soncino Press, London.

Handlin, Oscar
1954 *Adventure in Freedom: Three Hundred Years of Jewish Life in America.* McGraw-Hill Book Co., New York.

Klein, Isaac
1979 *A Guide to Jewish Religious Practice.* Jewish Theological Seminary of America, New York.

Kranzler, David
1979 *My Jewish Roots: A Practical Guide to Tracing and Recording Your Genealogy and Family History.* Sepher-Hermon Press, New York.

Lamm, Maurice
1981 *The Jewish Way in Death and Mourning.* Jonathan David Publishers, New York.

Learsi, Rufus
1954 *The Jews in America: A History.* World Publishing Co., Cleveland.

Levinger, Lee J.
1944 *A History of the Jews in the United States.* Union of American Hebrew Congregations, Cincinnati.

Levitov, Betty
1976 Jews, The Exodus People. In *Broken Hoops and Plains People: A Catalogue of Ethnic Resources in the Humanities–Nebraska and Thereabouts,* 291-336. Nebraska Curriculum Project, Lincoln.

Marcus, Jacob R.
1974 *Memoirs of American Jews, 1775-1865.* KTAV Publishing House, Inc., New York.

Meyer, Richard E., ed.
1989 *Cemeteries and Gravemarkers: Voices of American Culture.* UMI Research Press, Ann Arbor.

Newmark, Maurice
1981 Beginnings of the Pioneer Congregation of Lincoln, Nebraska. *Western States Jewish Historical Quarterly* 14(1):45-49. [Original 1918].

Nutty, Coleen Lou
1984 Cemetery Symbolism of Prairie Pioneers: Gravestone Art and Social Change in Story County, Iowa. *Journal of the Iowa Archeological Society* 31:1-135.

Parzen, Herbert
1964 *Architects of Conservative Judaism.* Jonathan David, New York.

Philipson, David
1936 Personal Contacts with the Founder of the Hebrew Union College. *Hebrew Union College Annual* 11:1-18.

Rathje, William L.
1974 The Garbage Project: A New Way of Looking at the Problems of Archaeology. *Archaeology* 27(4):236-241.

Rathje, William L., and Michael B. Schiffer
1982 *Archaeology.* Harcourt Brace Jovanovich, Inc., New York.

Rosenbaum, Jonathan, and Patricia O'Conner-Seger
1981 *Our Story: Recollections of Omaha's Early Jewish Community 1885-1925.* Omaha Section of the National Council of Jewish Women, Omaha.

Rosenthal, Frank
1957 *The Jews of Des Moines: The First Century.* Jewish Welfare Federation, Des Moines.

Sarna, Jonathan D., ed.
1986 *The American Jewish Experience.* Holmes and Meier, New York.

Schwartzman, Sylvan D.
1971 *Reform Judaism Then and Now.* Union of American Hebrew Congregations, New York.

Silverman, William B.
1970 *Basic Reform Judaism.* Philosophical Library, New York.

Sklare, Marshall
1972 *Conservative Judaism: An American Religious Movement.* Schocken Books, New York.

Thomas, David Hurst
1989 *Archaeology.* Holt, Rinehart and Winston, Inc., Fort Worth.

Trepp, Leo
1980 *The Complete Book of Jewish Observance.* Behrman House, Inc., New York.

Wolfe, Jack
1941 *A Century with Iowa Jewry.* Iowa Printing & Supply Co., Des Moines.

Ydit, Meir
1971 Cemetery. *Encyclopedia Judaica* 5:271-276. Macmillan, New York.

Yellen, John E.
1976 Settlement Patterns of the !Kung: An Archaeological Perspective. In *Kalahari Hunter-Gatherers,* edited by R.B. Lee and I. DeVore, 47-72. Harvard University Press, Cambridge.