CADDOAN ARCHEOLOGY

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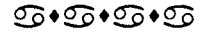
EDITOR'S PAGE

Help wanted! In preparation for the 1997 Caddo Conference (and into the future as well), we are updating the conference mailing list. All of you who subscribe to this journal/newsletter (except libraries) should receive the flier. If you do not, and if you are interested in receiving a copy, please let me know. Also, if your address has changed, please send us a change of address. If you know of someone who would be interested in attending the conference, but who did not get a copy of the flier, send us their name and address.

I'm sorry that this issue of Caddoan Archeology is "a bit" late. I've been having some computer glitches, and things have also been really busy around here. With the new computers, we're also using new versions of software. Hopefully, the next issue won't be quite so far off schedule (at least if the computers don't throw me off again).



At long last, I have joined the information age of the late twentieth century! I'm hooked up to the internet and have e-mail. The task of checking references, and of working up bibliographies for seminars and for publications aimed at a general audience has become much easier. I'm learning how to send and receive files through e-mail, too. Those of you who send information to me, and who have e-mail, will now be able to send it in electronically. Jim Corbin has already taken advantage of this to send me the previous Caddo Conference mailing list.

In addition, there are all kinds of interesting sites worldwide dealing with archeology (and a lot of other things in which I have an interest). I can see how people become "addicted" to surfing the net. There's no way you could ever check out everything (nor would you really want to; there's some really weird stuff out there).

Anyhow! My e-mail address is lealbert@ou.edu. After I get used to all of this "new-fangled" stuff, we should be able to speed up transfer of information for Caddoan Archeology. . . . . or just say hi occasionally!

REGIONAL NEWS

OKLAHOMA

Bob Brooks, Oklahoma's State Archeologist, sent in this news from Oklahoma:

A number of newsworthy events took place in the past few months in eastern or southeastern Oklahoma. The Corps of Engineers, Tulsa District, sponsored data recovery excavations at the Hickory Ridge site (34HS75) in Haskell County during the early summer. The purpose of this work was to recover evidence of a possible Coles Creek component at the site. The Corps will also be sponsoring salvage work at a late prehistoric house exposed by erosion at Haley's Point (34MA15) in Marshall County on Lake Texoma. Haley's Point is a Bryan focus site and additional work here, combined with the analysis of previously excavated materials, could lead to a better understanding of this cultural complex. Lastly, Larry Neal of the Oklahoma Archeological Survey will be returning to the Fish Weir site (34PU331) in Pushmataha County. This is Oklahoma's only documented prehistoric fish trap, dating to some 3000 years ago.

In the management area, 51 small archeological surveys were conducted during the past four months in eastern and southeastern Oklahoma. Sites recorded during this work represent small lithic scatters as well as historic farmsteads. Also, the Ouachita National Forest's land exchange with Weyerhauser passed through Congress and was signed into law. This exchange will involve the transfer of some 28,000 acres of US Forest Lands in southeastern Oklahoma to Weyerhauser for some 105,000 acres of Weyerhauser property. (These figures have changed some with additional acres being exchanged to the Forest Service). Meeks Etchieson of the Ouachita National Forest has been working on survey and assessment of sites on forest lands which will be exchanged.

Meeks Etchieson of the Heritage Resources Program, Ouachita National Forest Supervisor's Office, US Forest Service, sends this information about a site being tested on these exchange lands beginning the week of November 4, 1996. He will let us know about the results of this testing, possibly in the next issue:

A prehistoric site (34MC737) is located in the Tiak Ranger District of the Ouachita National Forest in southeastern

McCurtain County, Oklahoma, on lands that are included in a proposed land exchange with Weyerhauser Company. An initial visit to the site occurred in February 1996. The 2700 square meter site lies on a series of low knolls on the floodplain (about 350 ft AMSL) north of the Surratt Branch. The vegetation along the creek is a mixed hardwood bottomland forest.

The site was discovered during shovel testing in a series of three low, prairie bump-like knolls. The tops of these knolls are approximately 1.5 meters above the surrounding flat floodplain. The site is about 45 meters north-south by 60 meters east-west. The creek channel is about 15 meters south of the southern edge of the site. About 12 meters east of the easternmost knoll, a small side drainage enters Surratt Branch from the north.

During the site documentation process, 12 shovel tests revealed that cultural deposits occur between 22 and 55 cm in depth. The soil is a brown sandy loam. One shovel test excavated on the floodplain between the knolls produced no cultural materials. Four shovel tests were excavated on the southwesternmost knoll, with three of these containing cultural materials. Six fragments of firecracked rock were recovered from two of the shovel tests; one also included small fragments of charcoal. This suggests that burned rock features may be present on this knoll. One small, heavily reworked Gary dart point was also recovered from this knoll.

Five shovel tests, all containing cultural materials, were dug in the larger, elongated easternmost knoll. Two shovel tests near the center contained charcoal fragments. One of these also produced a fragment of deer tibia exhibiting butchering marks, as well as an Ensor-like dart point. In addition, the soil in this particular shovel test consisted of a dark brown sandy loam; a midden may be present in this area.

One shovel test was placed in the northwesternmost knoll. This produced only a single charcoal fragment.

The lithic debris recovered from these tests were relatively few in number (n=11). They consisted of thinning flakes, secondary flakes, and tertiary flakes. These few items suggest that tool maintenance was a minimal activity at the site. Lithic materials include local cherts (n=9) and novaculite (n=2).

The presence of firecracked rock in four of the shovel tests, together with charcoal fragments in three of them, suggests that the potential for intact buried features is high. Bone preservation within the site is believed to be good, based on the presence of bone in one of the shovel tests. This site is believed to have the potential to provide important information regarding the prehistoric past of southeastern Oklahoma. Therefore, it is believed to be potentially eligible for inclusion on the National Register of Historic Places.

SOUTHWESTERN MISSOURI

Jack H. Ray of the Center for Archaeological Research, Southwest Missouri State University at Springfield, Missouri, sent this information:

I am in the process of compiling data on the availability, procurement, and use of chipped stone resources (i.e., chert, quartzite, rhyolite, argillite) throughout the Ozark Highlands, which includes portions of Missouri, Arkansas, Oklahoma, Kansas, and Illinois. Any information provided will be considered, but I am especially interested in early (pre-1980) descriptions of lithic resources and analyses concerning the prehistoric use of chert resources. Data on chert use obtained from test excavations (Phase II) and full-scale excavations (Phase III) are preferred, especially if stages (Archaic *vs.* Woodland), periods (Early Archaic *vs.* Late Archaic), or phases (Titterington, Neosho, etc.) were confidently delineated. As an example from southwestern Missouri, recent work has revealed that Burlington chert was much preferred by Calf Creek knappers, whereas other Early-Middle Archaic (Graham Cave/Big Sandy) knappers living in the same area relied more on Jefferson City chert for the production of their chipped stone tools.

I know of a number of reports which contain references to the description and prehistoric use of chert resources, but I suspect that there are a number of others that I am not aware of (especially in Arkansas, Oklahoma, and Illinois). If you know of such chert studies in the Ozarks, I would appreciate a full reference/ citation of the work, or better yet, a photocopy of those pages/chapters dealing with chert utilization. Also, please provide your phone number and address in case I need to contact you for further details. Any information received and used in my study as a result of this request will be duly acknowledged.

Please send all information to: Jack H. Ray, Center for Archaeological Research, Southwest Missouri State University, Springfield MO 65804; phone (*work*) 417-836-4888, (*home*) 417-889-2554; e-mail: JHR929t@vma.smsu.edu.

Jack also sent the following abstract:

Conner, Michael D., (editor), with contributions by Michael D. Conner, Jessica Fadler, Neal H. Lopinot, Jack H. Ray, and Jeffrey K. Yelton

1996 A Late-prehistoric Neosho Tradition Occupation in the Spring River Valley of Lawrence County, Missouri. Submitted to Historic Preservation Program, Missouri Department of Natural Resources by Center for Archaeological Research, Southwest Missouri State University, Springfield.

In 1995, the Southwest Missouri State

University Archaeological Field School undertook excavations at two sites near Mt. Vernon, Missouri, at the junction of the Spring River and Honey Creek. The Dahlman site, 23LA259, is a valley-bottom habitation site, and the Spring River Earthwork, 23LA45, formerly known as the Old Spanish Fort, is located on the high ridge between the two valleys.

Preliminary information indicated that late prehistoric material was present at Dahlman. While no positive evidence existed as to the cultural association of the earthwork, which consists of a ditch and low earthen berm about 100 m in diameter, it was believed to be late prehistoric in age. The purpose of the project was to determine if late prehistoric deposits were present at Dahlman and to recover evidence that might lead to determining the temporal and cultural association of the Spring River Earthwork.

Excavations at Dahlman produced two pit features, seven post molds, and one lithic cache. Ceramic and radiocarbon evidence indicates that the remains are related to the late prehistoric Neosho phase defined in northeastern Oklahoma, southwestern Missouri, and northwestern Arkansas. The project also produced the first botanical data from a Neosho site obtained with modern flotation techniques. Maize was ubiquitous at the site, but no other cultigens were recovered.

One unit in the ditch at 23LA45 produced material similar to that at Dahlman, and charcoal from the deposit dated to the same time as two samples from Dahlman (*ca.* cal A.D. 1450). Although not conclusively demonstrated, it appears that the earthwork was built during late prehistoric times by the people living at the Dahlman site.

Data from the two sites as well as a lithic quarry area on the slope between them suggest that chert procurement was an important activity of the occupation. Local Burlington chert was used to manufacture numerous scrapers and knives utilized in processing buffalo hides and may have been traded to chert-poor areas to the west and northwest.

LOUISIANA

RED RIVER LEVEES SURVEY

This item was submitted by David B. Kelley of Coastal Environments, Inc.:

Coastal Environments, Inc. (Baton Rouge, Louisiana) is conducting an intensive survey for the US Corps of Engineers, Vicksburg District, prior to proposed levee improvement work in the

Red River valley of southwestern Arkansas. The survey will ultimately extend from I-30 to the Louisiana state line and include approximately 125 miles of levee and 1000 ft riverside or to top bank. Although the construction will occur in a much smaller area along the levee, the wide right-of-way is being surveyed in order to encompass any borrow areas. The survey is being conducted by three- or four-person crews with crew members walking transects 100 ft apart and excavating shovel tests every 100 ft. In addition, a bucket auger is being used to sample to a depth of six to eight ft in areas where deeply buried material may be present. When completed, this project will represent one of the largest intensive surveys undertaken in the Red River valley.

To date approximately 56 miles of levee right-of-way have been surveyed. Fifty-two new sites have been recorded, and 19 previously recorded sites have been re-examined. Native American occupations were present at 38 sites, and 87 percent of these contained Caddoan components. Late Caddo occupations (n=20) were much more common than Early (n=11) or Middle Caddo (n=8) ones, but this is due in part to the difficulty of identifying Middle Caddo assemblages in this region. Fourche Maline components were present at 40 percent of the Native American sites, and Archaic components at about 16 percent. Most of the Historic components date to the late nineteenth or early twentieth centuries, but at least one early nineteenth century occupation has been located.

The most interesting findings of the survey thus far are the new data obtained on C.B. Moore's Foster and Friday Places. Prior to the present project, it was thought that little remained at either of these important sites due to river action, years of cultivation, and extensive disturbance by pothunters. Although the mounds recorded by Moore at these sites do not appear to have survived, extensive midden areas have been identified at both. In addition to the major Late Caddo occupations documented by Moore, both sites have Early Caddo and Fourche Maline components. Among the "new" sites are two that appear to be long lost friends: the Joe Russell and C.M. Shaw Places from which Judge Harry Lemley collected specimens early in this century. These sites had not been relocated since that time, and their recording by the present survey is another important contribution.

In conjunction with the archeological survey, geologists with the Corps of Engineers Waterways Experiment Station and Louisiana State University are carrying out detailed geomorphic mapping of much of the valley. Their data, along with the results of the archeological research, will be used to reassess the current model of the geomorphic history of this portion of the Red River valley. A draft report has been submitted on the portion of the archeological survey completed to date, and a report on the geological study should be available soon.

UPCOMING MEETINGS AND EVENTS

1997

February 3 - March 5 (tentative) *America's Smithsonian.* This is a traveling exhibit celebrating the Smithsonian's 150th anniversary. It will feature everything from dinosaurs to space travel. No other information available at present.

March

14-15 *39th Caddo Conference.* University of Oklahoma (Norman), Warrior Auditorium (Anadarko), and the Caddo Tribal Complex (Binger). A preliminary announcement and call for papers will be sent out soon. For information, contact Robert L. Brooks or Lois E. Albert, Oklahoma Archeological Survey, 111 E. Chesapeake, The University of Oklahoma, Norman OK 73019-0575; telephone (405) 325-7211; fax (405) 325-7604; e-mail (Lois Albert) lealbert@ou.edu.

April

2-6 *Society for American Archaeology Annual Meeting.* Opryland, Nashville TN. Contact: SAA, 900 Second St NW, Suite 12, Washington DC. Telephone: 202-789-8200.

10-13 *Computer Applications and Quantitative Methods in Archaeology, 25th Anniversay Conference.* The University of Birmingham (UK). Information can be found on the Internet at http://www.bufau.bham.ac.uk/caa97/caa97.htmor http://www.wisc.edu/anthropology/sas/sas.htm (this last is the web page for the Society for Archaeological Sciences).

13-17 *Symposium on Geochemistry and Archaeology.* American Chemical Society Spring Symposium. San Francisco CA. For details, contact Richard Evershed at r.p.evershed@bristol.ac.uk.

June

4-7 *Symposium on Bison Ecology and Management in North America.* Holiday Inn, Bozeman, Montana. This will be a forum for information and discussion on utilizing various disciplines to understand and manage bison in North America. Sessions will provide insight into how disease, genetics, ecology, management, prehistory, and tribal concerns can affect bison. For information, contact Bison Symposium, Montana State University, 235 Linfield Hall, Bozeman MT 59717; telephone (406) 994-3414.

September

2-4 *Symposium on the application of all aspects of science within archaeology.* Department of Archaeology, University of Durham. This is a biennial conference hosted by this department for the last ten years. Papers and posters on all areas of research within the conference theme are invited. It is expected that the conference will include papers on: human evolution, chronometry, ecosystems, technology and trade, prospection methods, data processing, geoarchaeology, and biochemical analysis. Prospective participants should send their name, address, and a 100-200 word abstract to the address below. The conference sessions will be held in the Department of Archaeology, with accommodation and meals in nearby Grey College. For further details, to submit an abstract, or to apply for a space for demonstration or commercial display, please contact: Archaeological Sciences '97, Department of Archaeology, University of Durham, South Road, Durham, DH1 3LE, England. Telephone: 0191 374 3625; FAX: 0191 374 3619; e-mail: A.R.Millard@Durham.ac.uk.

THE WOMACK, GILBERT, AND PEARSON SITES: EARLY EIGHTEENTH CENTURY TUNICAN ENTREPOTS IN NORTHEAST TEXAS?[1]

Frank Schambach,
Arkansas Archeological Survey

For the past few months, I have been working on a detailed response to a paper by James Bruseth, Diane Wilson, and Timothy Perttula (1995) published in the fall issue of *Plains Anthropologist*. There, these authors challenge my Sanders entrepot hypothesis (Schambach 1995) and my new paradigm for the Mississippi period archeology of the Arkansas Valley (Schambach 1993), claiming that the Sanders focus, as propounded by Alex D. Krieger (1946), is alive and well, so much so that they have renamed it the Sanders phase to ready it for service in the 1990s and beyond.

As I was finishing my response to that paper, with the intention of summarizing it at this conference, some exciting new evidence emerged which caused me to change my plans. This evidence, I think, settles the argument about the Sanders site because it proves that the people buried in the 21 graves at Sanders were, as I have been arguing on both archeological and bioanthropological grounds, an intrusive population from the Arkansas Valley. It also supports my hypothesis that the Mississippi period population of the Arkansas Valley was significantly different, culturally and biologically, from the Caddo populations south of the Ouachita Mountains.

Perhaps the best way to present this new information is to cite the short section titled "**The Bioanthropology of the Skeletons from the Sanders Site**" that appears in my response (soon to be published) to Bruseth, Wilson, and Perttula's challenge:

> I must begin by clarifying an important point that Bruseth et al. (1995:225) have obfuscated. I am not the one who "argues that the skeletal sample from [the Sanders] site is markedly different from Caddoan populations down the Red River". I'm not qualified to made that kind of argument. I merely pointed out (Schambach 1993: 204-205; Schambach 1995:10-11) that the bioanthropologists have recently begun to notice and puzzle over peculiarities in this group of skeletons compared to those from historically and archeologically documented Caddo sites farther east in the Red River Valley that are inexplicable in terms of Krieger's Sanders focus hypothesis. The first was Dow (1987) who

observed that the Sanders skeletons differ significantly from the Hatchel-Mitchell skeletons and offered the *ad hoc* explanation that this was because people at Sanders were intermarrying with Plains people. Then Burnett (1990:393-399), analyzing unpublished data assembled by Jackson, observed that the infection rate in the Sanders skeletons was "dramatically high" compared to other populations in the Red River Valley, an observation that Wilson (personal communication, February 1996) now considers valid. And it was Burnett, not I, who concluded that the Sanders skeletons are "markedly different" overall from Caddo skeletal populations from sites east of Sanders in the Red River Valley. Then Wilson (1993:11) added to the growing list of differences the observation that the Sanders skeletons evince an unusually high degree of degenerative joint disease of a type indicating to her that the Sanders people may have regularly carried heavy loads on their backs or heads and might have done "a great deal of travel[ing]" on foot.

My contribution to this process has been to assemble these observations and note that they raise the same question about the Sanders skeletons that I raise about the artifacts found with them. If they are Caddo, as the conventional wisdom would have it, why are they different in these ways?

And I have pointed out how these differences, inexplicable in terms of Krieger's hypothesis, make sense in terms of mine. A group of traders from the Arkansas Valley would have been genetically different in ways detectable osteologically from people in the Red River Valley (Barnes and Rose 1990:12; Schambach 1993:190-193). Their skeletons could be expected to show, as some of the Sanders skeletons do, evidence of infections with the diseases of childhood that happen to be grimly characteristic of the Mississippi period population of the Arkansas Valley (Brues 1958, 1959; Brown 1984:259; Burnett 1988:212-214), but not of the Red River Valley. And the skeletons of long-distance traders regularly plying the 150 mile riverine and overland route between Spiro and Sanders could be expected to show the kinds of stress induced degeneration that Wilson has identified in the Sanders site skeletons.

I also predicted (Schambach 1993:203) that as these bioanthropological studies progressed, conclusive osteological evidence that the Sanders site skeletons

represent an Arkansas Valley population might emerge. As luck would have it, such evidence has recently been presented to me by none other than Diane Wilson, one of the coauthors of the "Bruseth et al." attack on my Sanders entrepot hypothesis. I will conclude this section with a brief review of Wilson's new data and a short discussion of their implications for my hypothesis.

Wilson's data (discussed here with her permission) are in a paper prepared for presentation at the 61st Annual Meeting of the Society for American Archaeology (Derrick and Wilson 1995; Wilson and Derrick 1996) on styles of "cranial modeling", i.e., head deformation, exhibited by all skulls from presumed "Caddo" contexts in east Texas. Derrick and Wilson's crucial discovery was that two distinct styles of cranial modeling, produced by different techniques, were in use in the Red River Valley. There is a "tabular" style (Figure 1) which was obviously the norm for the Caddo throughout east Texas and (as Wilson has informed me) southwest Arkansas, since (except one specimen from a site in the Neches drainage) it is the only one represented at all but two sites, Sanders and the nearby Womack site. At these sites, a readily distinguishable "annular" style prevails. My response to that information was a December 7, 1995 letter to Wilson containing the following paragraphs:

> "Do you agree that the 'circular' deformation apparently 'produced by a circular binding from the frontal region to the occiput' that Brues describes as 'almost universal' in the Horton population, and which -- she notes -- was 'similar to that observed at the Nagle site, which was equated with the type described by Stewart from the Sanders site in Texas' is what you and Sharon Derrick are describing?"
>
> "If so, it seems to me that the limited Red River Valley distribution of this type of cranial modeling supports my hypothesis that the Sanders people were Spiroans from the Arkansas Valley."

Wilson's reply, upon reviewing the papers by Brues (1957, 1958, 1959) and Stewart (1941), was in the affirmative. Therefore I consider the "Sanders site problem" solved. Wilson and Derrick have, I think, supplied conclusive evidence that most of the people represented by the

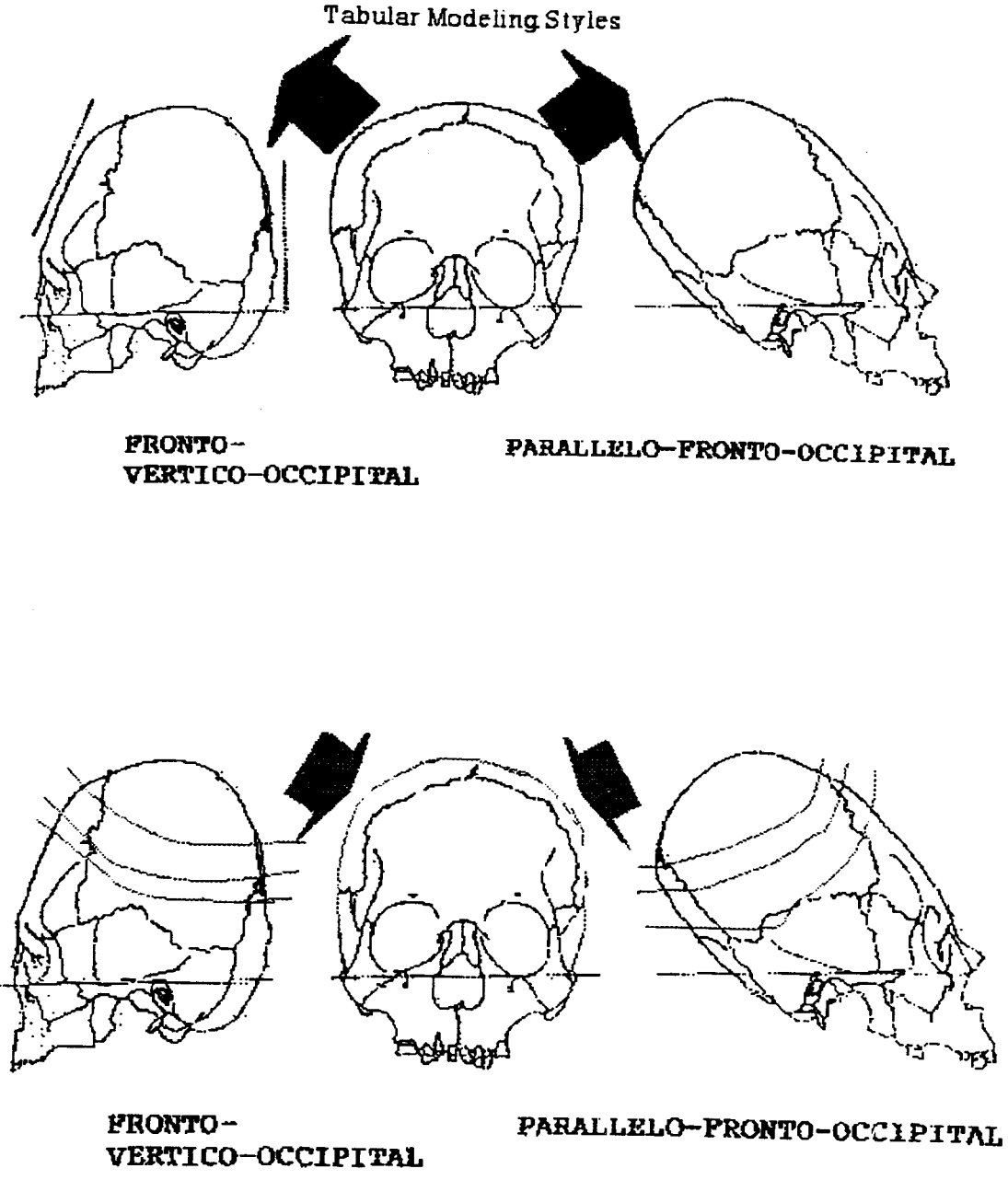


Figure 1. Tabular and Annular Styles of Cranial Modeling (after Wilson and Derrick 1996).

> skeletons from the graves at Sanders were from the Arkansas Valley, just as most of the goods buried with them are from the Arkansas Valley. The Sanders site is a textbook example of a site unit intrusion. My Sanders entrepot hypothesis explains this intrusion.

But who were these Arkansas Valley people known archeologically as the Spiroans? Was anyone in the Southeast practicing annular cranial modeling in historic times? So far, I have been able to find only one reference to this practice, the following passage from Garcilaso's account of the de Soto entrada (Varner and Varner 1951:457-458):

> The people in this province of Tula differ from all those our Spaniards encountered previously; for, as we have said, the others are fine and handsome, whereas these, both male and female, have loathsome countenances. Even though naturally well featured, they render themselves hideous with devices wrought upon their persons. Their heads are incredibly long and taper off towards the top having been made this way by artifice; for from the moment they are born their heads are bound and are left thus until they are from nine to ten years of age.

This is a perfect description of the annular style of cranial modeling, which makes the head look very long, seen from the front or the side, and makes it "taper off towards the top" when seen from the front (Figure 1). Parallelo-fronto-occipital flattening also causes the head to look long from the front or side, but it does not cause it to taper above the ears. It causes it to bulge above the ears, to look hyperbrachycephalic, because of the pressure applied from the front and the back.

Garcilaso also gives a plausible description of how annular deformation was accomplished. Instead of binding the infant to a cradle board every night for the first two years or so of life, as seems to have been the custom almost universally in the Southeast (Swanton 1946:539 *ff.*), the people of Tula simply wrapped their children's heads with broad bands of cloth or leather which they wore more or less constantly until they were eight or nine. The Spaniards must have seen this for themselves because, as is evident from Garcilaso's account, they couldn't communicate with the people of Tula very well, even through interpreters.

Since there is now little doubt that Tula was in the Arkansas Valley, somewhere in the Fort Smith/Spiro area (Early 1993:74-75; Hudson 1993:146-147), Garcilaso's observation fits the bioarcheological data perfectly. As far as I have been able to discover, the Arkansas Valley in eastern Oklahoma is the one place on their route throughout the Southeast where the Spaniards could have seen the annular style of cranial

modification. It seems to have been characteristic of and unique to this particular Arkansas Valley population from at least A.D. 1100 on (Brues 1957, 1958, 1959). So I think Garcilaso was accurate on this point, despite his generally poor reputation as a historian.

If Garsilaso was describing annular cranial modeling, the implications are very interesting. Most importantly, it would mean that the people of Tula, whom Early (1993:73) equates with the Fort Coffee phase, were probably one and the same with the people of the Spiro phase, which ended in that area about 100 years earlier. I have already pointed out that this was probably the case, because the Spiro phase in the Arkansas Valley differs from the Fort Coffee phase only in nuances in the ceramic assemblage (Schambach 1993:231; Rohrbaugh 1984:279-281). Thus, Garcilaso's description reinforces archeological data which indicate that the Spiroans were still in the Spiro locality 100 years or so after the apparent collapse of Spiro. Furthermore, it indicates that neither the Spiroans nor their Fort Coffee phase descendants were the Wichita who, on solid archeological and historical data (Bell 1984a:309, 323; Brooks 1989:78; Stewart 1941:349; Owsley 1989:133), did not practice "cranial modeling" of any style. Nor, I would say, on massive and -- as we see here -- steadily accumulating archeological and bioanthropological evidence (Schambach 1993:190-193), were they related culturally or biologically to the Caddo south of the Ouachitas.

It is also clear from the accounts of Garcilaso and the other chroniclers of the de Soto entrada that these Tula, heirs to the Spiroan tradition, a tradition of long-distance trading in my view (Schambach 1993:196-208; 1995), were still importing buffalo products from central Oklahoma in 1542, and, probably, still trading them to people in the Mississippi Valley. Garcilaso reports that "In the town .... our men found serving as bed covers a great number of cowhides which had been softened and dressed without removing the hair; and there were in addition many others waiting to be dressed. Moreover there was beef; but no cows were to be seen in the fields, and it could never be learned from whence the hides had been brought" (Varner and Varner 1951:457). They had been brought, I would say, from the same place the Spiroans obtained buffalo products several centuries earlier, the old Spiroan entrepot near Oklahoma City, 170 miles west of Spiro (Schambach 1993:207-208; 1995:19-20, n.31). And I would bet that some of the hides and "beef" the Spaniards saw at Tula were on the way down the Arkansas to northeastern Arkansas where the Spaniards saw various buffalo products, all probably imported from the Plains by the people of Tula (Quinn 1979:130, 133, 180, 184).

This "new" bioanthropological and documentary evidence that the Spiroan trade network survived the apparent collapse of Spiro as a "ceremonial

center" ca. 1450 is supported by independent archeological evidence that organized trade between the Mississippi Valley and the Red River Valley via the Arkansas Valley continued after 1450. The most striking evidence of this post-Spiro phase trade is the large population of Mound Place Incised bird-effigy bowls that centers in Miller and Lafayette counties in extreme southwestern Arkansas and in Cherokee, Harrison, Titus, and Red River counties in northeastern Texas. These bowls are not found farther east in Arkansas south of the Arkansas Valley (Suhm and Jelks 1962:47-49). Phillips, Ford, and Griffin (1951:147-148) noted 45 years ago that "some of these are very close to St. Francis forms [i.e., forms common in the St. Francis Basin between Memphis and Forrest City, Arkansas], indicating a northeast to southwest movement". The occurrence of Mound Place Incised bowls in Ft. Coffee phase contexts (Rohrbaugh 1982:476-478) and at Sanders[2] indicates the route over which they were transported from northeastern Arkansas to northeastern Texas and southwestern Arkansas. The turquoise that occurs in Fort Coffee phase contexts in the Arkansas Valley (Rohrbaugh 1982:547) and is abundant in what appear to be post-Sanders phase contexts at the Sanders site and other nearby sites (Jurney and Young 1995:21-23) is additional evidence of this trade and is indicative of the geographical and cultural areas that were probably involved in it.

Who were these descendants (I would say) of the Spiroans that the Spaniards encountered in 1542 as the Tula and, perhaps, as long distance traders, if they were not -- as they could not have been -- the Wichita or the Caddo? Barring the possibility that the people of Tula vanished following their encounter with de Soto, as did so many people in northeastern Arkansas, that leaves only my hypothesis (Schambach 1993:221-224) that they were the Tunica.

Until I read Derrick and Wilson's paper, I considered the proposition that these people were Tunica more of a cultural-historical interpretation than an hypothesis because, short of the possibility of using DNA data, I did not see how it could be tested. But there, I found an observation that raises the possibility that it can be tested against archeological and historical data in a way that could confirm it conclusively. This was the observation that the annular style of cranial modeling appears -- let me remind you -- at two sites in the Red River Valley, Sanders and the nearby Womack site. When I first read that, I tried to discount it, hoping the annularly modeled crania from Womack would prove to be from graves that lacked the early eighteenth century French trade goods characteristic of that site, that they were from earlier, Spiroan, graves like those at the nearby Sanders site. But that hope was dashed when I called Diane Wilson, who assured me that at least three of the annularly modeled crania were from graves with historic burial offerings and Womack Engraved pottery. Therefore I was forced to deal with the

puzzling fact that some four to five hundred years after the Spiroan entrepot at Sanders was abandoned, the same area was occupied by another group of intruders in the Red River Valley who used essentially the same locally distinct style of annular, as opposed to tabular, cranial modeling as had the Spiroans before them. And as I began to think about this new information and about the dilemma it seemed to pose, I realized that it has fascinating implications.

The current interpretation of the main component at the Womack site is that it represents an A.D. 1700-1730 occupation by an "intrusive" Indian group "tentatively" identified as the Wichita or the Kichai which possessed a wide variety of French and Indian trade goods (Harris et al. 1965:360; Story et al. 1990:346; Perttula 1992:171). But if the annular modeling the Womack skulls exhibit means anything, it means these people were not Wichita of any stripe, neither Arkansas Valley Wichita nor western Red River Valley Kichai, because neither the Wichita nor the Kichai practiced cranial modeling. Nor, as we have seen, can they be Caddo. Nor, and this is the dilemma, can they be traced to the Arkansas Valley, as I have done with the earlier Sanders site population, because the consensus among archeologists working with the Arkansas Valley data is that the Spiro locality was abandoned by about 1620 (Perttula 1992:142, 161; Brown 1996:27; Rogers 1996:68). The Spiroans (or "Tulans") were gone, and no one (except, perhaps, me) knows where. So who were the people with annular rather than frontally-occipitally "modeled" heads who were living and burying their dead at the Womack site between 1700 and 1730?

The archeological evidence indicates that -- dare I even say it? -- they were the Tunica. While that may sound outlandish, I am not the first to notice a connection between Womack and Tunican sites in the Red River mouth area. As Harris et al. (1965:360) pointed out in the original Womack site report, the European trade bead types and some of the gun parts found at Womack indicate "a definite connection between Angola Farm, Fish Hatchery site, the Nassonite Post, and (*sic*) Womack site". As Harris et al. also noted, Angola Farm is a Tunica site excavated by Ford (1936:129-140) in 1934 and occupied, according to Swanton, from about 1709 until 1729 (Harris et al. 1965:358).

Harris et al. (1965:360) argued that "some of the items common to these sites represent goods distributed by the La Harpe party" but, for three reasons, I think otherwise. For one thing, the dates they assign to the European artifacts indicate that the Womack site could have been in use for about twenty years before La Harpe arrived. Secondly, there is no convincing documentary or archeological evidence that any member of the La Harpe party visited this site. And, third and most importantly, a significant number of the aboriginal goods from Womack also point down the Red River to Angola Farm and other Tunica sites. Indeed, I think there is enough similarity

between the assemblage from Womack and the assemblage from Angola Farm to support the interpretation that Womack represents a site unit intrusion from Angola Farm and that the graves at Womack are classic Tunica graves.

According to Harris et al. (1965:315), 47 of the 56 bead types found at Womack are also found at the Angola Farm site and their 1700-1729 date for Womack is based primarily on numerous close similarities between gun parts found at Womack and gun parts found at the historically dated Angola Farm site (Harris et al. 1965:327,331,332,335,340, 341,343). The correspondences in aboriginal artifacts are also close: the Natchitoches Engraved *var. Natchitoches* bowl (Harris et al. 1965:Figure 4B; Schambach and Miller 1984:124, Figure 11-11) from Womack is an import from down the Red River, probably from south of the Arkansas line, since pottery of that type is rare in the Great Bend region. The Tunica were using Natchitoches Engraved *var. Natchitoches* because it appears in sherd form at Angola Farm (Brain 1988:Figure 137e), but they were probably obtaining it by trade from the Natchitoches area. The aboriginal conch shell beads and pendants from Burial 6 at the Bloodhound Hill site, a Tunica cemetery located a short distance north of the Angola Farm site, with which it appears to be approximately contemporaneous (Brain 1988:Figure 126, 173:Figure 146), resemble the conch shell beads and gorgets from the graves at Womack (Harris et al. 1965:305-306, Figure 7). The snub-nosed end scrapers that are so abundant at Womack have their counterparts, as Perttula (1992:172-173) notes, at the Tunican Haynes Bluff, Russell, and Bloodhound sites. At Bloodhound, one was found next to the left hand of an adult male in Burial 7 (Brain 1988:398). These scrapers remind me of Father Gravier's observation (Brain 1988:296) that although Tunica men did not hunt "they dress [deer and buffalo skins] the best of all Indians that I have seen". I'll bet they did. As I have said elsewhere (Schambach 1993:198-200), I think they were doing it 500 years earlier, using the same snub-nosed scrapers, at sites like Wybark, Sheffield, Tyler-Rose, and Cookson located on the Arkansas River between the Forks of the Arkansas and Spiro. I'll say more about these scrapers in a few minutes.

Finally, there are the graves at Womack. These, you will recall, cannot be Caddo graves because of the annular cranial modeling. The cranial modeling indicates that they can't be Wichita or Kichai either, as does the general absence of 1) diagnostic Wichita or Kichai traits from the graves and other contexts at Womack, and 2) the absence of independent documentary evidence that either group was living this far south and east prior to 1730. But these graves are, I suggest, *absolutely typical Tunica graves* in the sense that they contain precisely the same congeries of European trade goods and non-Tunican aboriginal pottery and other goods from various local and non-local sources that appears in every Tunica grave on record. The

one distinctive characteristic that all Tunica graves identified so far have in common is that the offerings found in them consist mainly of traded goods. They contain little, if anything, that is obviously Tunica-made, so little that the only way they can be identified as Tunican is that they are found at historically documented Tunica sites, and they contain European and aboriginal goods of the right period. This is not a new observation. James A. Ford (1936: 140) noted 60 years ago with respect to Tunica pottery that the Tunica at Angola Farm had "taken over the pottery of the Caddo and Natchez rather thoroughly".

Therefore I consider the historic component at the Womack site a site-unit intrusion from the historic Tunican Angola Farm site. Furthermore, I suggest that the historic component at the Gilbert site (Jelks 1967), located about sixty miles south of the Womack site on the upper Sabine, also represents a Tunican intrusion, only there the intrusion was from the Trudeau site where the Tunica were living during the time the Gilbert site was occupied. We do not have the benefit of Harris and Blaine's scholarship when it comes to comparing the assemblage from the Gilbert site (dated to approximately 1750, mainly on the basis of European bead types and various gun parts; Blaine and Harris 1967:41,47,61,67,71,79,80,81; Harper et al. 1967:104), with that from the Trudeau site, which is dated historically to 1731-1764 (Brain 1988:66). However, it is obvious that there are many correspondences. At least 17 of the 58 bead types recognized at Trudeau also appear at Gilbert (Brain 1979:116-131) and, as is apparent from Brain's work (1979:214-216), there are some very specific similarities between the gun parts from Gilbert and the guns from Trudeau. For what it is worth to those who (foolishly, I would say) accept Brain's argument that the pottery he calls Winterville Incised *var. Tunica* is an infallible marker for a Tunican occupation wherever it is found, there is also the fact that, as I pointed out in 1984 (Schambach and Miller 1984:121-122) the "Emory Punctated" pottery reported from Gilbert (Story et al. 1967:135-139, Figure 57g,h,i) appears to be practically identical to the Winterville Incised *var. Tunica* from Trudeau (Brain 1979:234-237).

There is, of course, just one last piece of evidence that is needed to confirm the rather complex hypothesis I am developing here. The hypothesis is that the Spiroans/Tulans that Garcilaso described on the Arkansas in 1541 were the Tunica, as I have been suggesting for years (Schambach 1993). In 1541 they were, hypothetically, still living near Spiro in the Arkansas Valley and their trade network, by then in place for over 500 years, was still up and running. Hypothetically, they were still moving salt, bows, pottery, bison products, and other kinds of commodities and prestige goods over long distances. This probably continued until about 1650, when the sudden introduction of Spanish horses from the Southwest, and French guns and other trade goods from the

Mississippi Valley disrupted their ancient system. Horse transport superseded human transport and guns superseded bows for warfare, putting the Tunica out of business as far as the vital western half of their trade network was concerned. So, knowing what was going on everywhere in North America between the Pueblo area and the Mississippi Valley, as I imagine they would have if they were the long-distance traders I think they were, most of them moved from the Spiro locality directly to the Yazoo (as they would move from there to the Red River mouth area about 50 years later) to try to insinuate themselves into the hide trade that English traders based on the southern Atlantic coast were operating in that area. Thus, I hypothesize, when they entered history there on the Yazoo in 1699 (Brain 1988:294) they had not come, anciently, from farther up the Mississippi Valley in the Upper Sunflower region as Brain (1988:266-277) conjectures on very poor archeological evidence. They had come, recently and probably directly, from the Fort Smith locality in the Arkansas Valley. Probably because they were trying to cut themselves in on the profits rather than merely supply hides at low cost to the English as Indians were supposed to do, they soon got in trouble on the Yazoo, and moved again to the Red River mouth area and, initially, the Angola Farm site.

I will tell you in a moment why I think they went there and how I think they subsequently amassed the wealth that has come to be called the "Tunica Treasure". But first, there is still the matter of the type of evidence needed to confirm my hypothesis about the central involvement of the Tunica in the prehistoric Spiroan trade network and in the operation of entrepots in east Texas during the early historic period. That evidence is, of course, crania from historically documented Tunica graves exhibiting the annular style of modeling that was characteristic of the Spiroan population of the Arkansas Valley in eastern Oklahoma from A.D. 1100 to 1541. Unfortunately, that evidence is not available, not necessarily because the Tunica did not practice annular cranial modeling, but because (as far as I know) there are no crania from documented Tunica graves that are sufficiently intact for observations on the presence and style of cranial modeling to be made. All we have at the moment is Father Gravier's observation that the Tunica deformed their children's heads (Brain 1988:295), but no historical or bioanthropological evidence of the style of modeling they used.

So confirmation, or rejection, of my hypothesis that the Spiroans were the Tunica and that the so-called Norteno focus sites in northeastern Texas were actually Tunican entrepots must wait the discovery either of intact skulls from historically documented Tunica graves or of explicit historical documentation of the type of cranial modeling they used. But in the meantime, since there are, as far as I can see, no other equally plausible competing hypotheses, I am

encouraged to continue to build my case for Tunican entrepots in northeastern Texas on other kinds of evidence.

The final, and crucial, piece of evidence I want to consider here bears on two related questions. How did the Tunica amass the wealth in goods (the "treasure") that Leonard Charrier looted from about a hundred of their graves at the Trudeau site (Brain 1979)? And how might the operation of entrepots in northeastern Texas have been involved in that process?

The consensus on the source of the Tunica treasure seems to be that the Tunica acquired it by functioning as "middlemen" in trade between the French and other tribes. The Tunica are supposed to have profited from this trade by virtue of their strategic location in the Red River mouth area between the French in New Orleans and the Caddo, Wichita, Osage, Quapaw and other tribes living upriver in Louisiana, Mississippi, Arkansas, Texas, and Oklahoma (Brain 1979:280-282; Perttula 1992:201; Kidder 1993:237). The problem with this view is that no one has tried to explain exactly what the Tunica did as "middlemen", other than that they had positioned themselves athwart a bottleneck in the main river route between the French and all of these tribes. But how would this have produced profits for them? Were they collecting tolls or tribute from French or Indian traders moving through their territory?

I doubt that the Tunica's role as "middlemen" was either that limited or that passive. I would suggest that, rather than having, accidentally or otherwise, found a way to profit from an endeavor controlled by the French, as everyone seems to assume, the Tunica were profiting from this "French and Indian trade" because, as had (in my view) been their practice for 500 years with salt, bow wood, and other commodities, they were running it lock, stock, and barrel. I would suggest that they controlled the supply of Indian goods to the French, and of French goods to the Indians by establishing their own entrepots at the sources of valuable Indian commodities, specifically the Womack, Gilbert, and (possibly) Pearson (Duffield and Jelks 1961) sites, and moving goods to and from them themselves.

The goods moving through these entrepots probably included all the important commodities of the eighteenth century French-Indian frontier (Gregory 1973:289): salt, hides, Osage orange bows, European guns and ammunition, other European goods ranging from axes to ornaments, and particularly (I think) horses. Horses, generally considered one of the main sources of Tunica wealth (Gregory 1973:11; Swanton 1911:312; Brain 1979:282), were probably also one of the main reasons, if not the main reason, for Tunican interest in the Womack, Gilbert, and Pearson sites. Indeed, as I will try to demonstrate, they probably account for the locations of these sites as well.

There seems to be no doubt that the

Tunica were horse traders. The Tunica chief, Cahura-Joligo, was "renowned for his involvement in the horse trade" and a wealthy man by European standards for that time and place because of it (Gregory 1973:11; Swanton 1911:312; Brain 1979:282). Because he was a chief, he probably was not involved in this enterprise alone; the whole tribe would have been involved and profiting from it. The key question about this horse trade, the mechanics of which are unknown, is: where did the Tunica get horses in the period between 1700 and 1760, the time the "Tunica treasure" was accumulated? Conventional thinking on the horse trade in Louisiana between 1700 and 1760 is that, despite Spanish opposition to trade between the Caddo and the French in Louisiana (particularly if it involved guns and ammunition), horses were filtering into the French territory in the Mississippi Valley from the Spanish settlements in the Southwest via the Hasinai and the Natchitoches (Gregory 1973:281). Some evidently were, but that does not explain the Tunican involvement in the horse trade. Had they managed, somehow, to insinuate themselves between the French and the Hasinai? Nor, there is reason to believe, does it account for all the horses that were coming into Louisiana. By 1720, according to Wedel (1981:36-37), the Spaniards were making it "more difficult" for the French to get horses "through Hasinai Caddo middlemen", and the French were exploring the possibility of getting them from the Wichita or the Osage. One reason for Bienville's interest in La Harpe's second trip to the Wichita, according to Wedel (1981:37-38) was "the fact that the Tawakoni [Wichita] were reported to have large numbers of horses", although the French did not know where they were getting them.

I think there is good historical and circumstantial evidence that the Tunica, by virtue of their long and continuing familiarity with the Red River Valley in the vicinity of their old entrepot at the Sanders site, and with their old homeland, the Arkansas Valley, knew about the Wichita's horses long before the French heard about them, that they knew the Wichita were getting them from a significant and growing feral herd in northeastern Texas that was unknown to Europeans of that era (and has remained unknown to archeologists and most historians of this one), and that they were involved with the Wichita in the procurement of horses from this herd, some of which they traded to the French in Louisiana.

This hypothesis came to mind when I began thinking about why the Gilbert site, located near the headwaters of the Sabine, should, like Womack sixty miles to the north on the Red River, be loaded with French trade goods. It seems likely that whatever the so-called "Norteno focus" people who frequented the Womack site did to get French trade goods in quantity, those frequenting the Gilbert site a few decades later probably did too. But unlike Womack, which was well situated to command what trade and travel there was up and down the Red River Valley, Gilbert seems to have been

off the beaten path, an unlikely spot for trade goods to accumulate in quantity. There is, however, one important common element in the locations of these sites: both are precisely on the edge of the Blackland Prairie (Fenneman 1938:102-108, Figure 27, Plate VII), as are the two other northeastern Texas sites with so-called "Norteno focus" components, Sanders and Pearson. Thus the question is: what was attracting people to the edge of the Blackland Prairie between 1700 and 1760? Certainly it was not buffalo hunting, considering the dearth of buffalo bones in the faunal remains from the sites themselves and Lynott's (1980) argument that the grasses of the Blackland Prairie were not attractive to buffalo. But there is historical evidence that the attraction was feral horses.

According to the environmental historian Dan Flores (1985:102 n.8), when Domingo Teran de Los Rios traveled to the Upper Nasoni village on the Red River in northeastern Texas in 1691 with the intention of establishing a mission, he brought "more than 1000 horses and mules ... at least 200 of which were lost". Apparently, some of these animals colonized the Blackland Prairie so successfully that they increased into a large feral herd whose existence remained unknown to Europeans until American "mustangers" discovered it around 1800. It then became a major source for the feral horses they brought into the southeast. In 1802 alone "an estimated 7300 Texas horses", most of them apparently from the Blackland Prairie herd, "passed through Louisiana to eastern markets". Despite what must have been heavy pressure from the mustangers, the herd seems to have survived until about 1820, at least. When the trader Anthony Glass ascended the Sulphur River from Natchitoches in 1808, he began to see feral horses immediately upon entering the Blackland Prairie. Seven days later on Bois d'Arc Creek about "75 miles from its mouth", and from the Sanders and Womack sites, he "saw great numbers of wild horses" (Flores 1985:43-44). When Thomas Nuttall visited the Red River Valley in 1819, he found the first "native stands" of bois d'arc growing on "the Horse-prairie [basically the northern extension of the Blackland Prairie across the Red River], 15 miles above the mouth of the Kiamesha" and directly across the Red River from the Womack and Sanders sites. This prairie, Nuttall (1980:173) explained, "derives its name from the herds of wild horses, which till lately frequented it, and of which we saw a small gang".

Granted that the Blackland Prairie horse herd was of a size to be economically important around 1800, could it have been an important source of horses for the Wichita and the Tunica about a hundred years earlier? Probably. Between 1691, the time of the Teran expedition, and 1701, the time horses started appearing in Tunica villages in the Red River mouth area, according to Gregory (1971:281), and the time (hypothetically speaking) the Womack entrepot was probably opened by the

Tunica, the horses lost by the Spaniards could have increased, assuming 50 to 100 animals to start and the exponential growth characteristic of such situations at that time (Crosby 1972:82-84), to 5000 animals or more. By 1719, the year La Harpe established his post on the Red River and the approximate time that the occupation of the Womack site ended, there could have been as many as 50,000 horses on the Blackland Prairie. By 1750, the generally agreed upon central date for the occupation of the Gilbert site, this herd could have numbered in the millions, mathematically speaking. Ecologically speaking, it had probably stabilized at the maximum carrying capacity for the Blackland Prairie. Considering the number of square miles involved, and the apparent scarcity of buffalo (Lynott 1980) which would otherwise have competed with the horses for food, their number could have been in the hundreds of thousands.

The Tunica, who -- according to my hypothesis that the Sanders site was a Spiroan entrepot (Schambach 1995) -- had been exploiting the trade potential of the Blackland Prairie bois d'arc for 500 years, would have known about this growing horse herd early on. So, I suspect, would the Caddo of the western reaches of the Red River beyond the Great Bend, since they had to go to the Blackland Prairie to get their bow wood. So, by 1719, might the Osage, since in that year La Harpe (Smith 1958/9:383) met a party of twenty of them coming down the Kiamichi (headed, perhaps, for the Blackland Prairie to get horses and bois d'arc) when he traveled from his newly established "Nassonite Post" on the Red River to the Arkansas Valley. But most importantly, I think, as far as the question of the function of the Womack site is concerned, the Wichita must have known about the Blackland Prairie horse herd. I suspect that it was an important source, if not the main source, for the "large number of horses" (Wedel 1981:37) that the Wichita had, the horses that attracted the attention of the French in New Orleans, *ca.* 1720. Although the crania with annular modeling from the graves at Womack indicate that someone other than the Wichita (the Tunica, in my opinion) "owned and operated" the Womack site, there is archeological evidence that Harris et al. (1965:360) were right in associating the Wichita with the Womack site in some way. End scrapers of Kay County, Oklahoma chert, a catlinite pipe fragment (Harris et al. 1965:291-292,294,298) and, less specifically, triangular arrowpoints (Harris et al. 1965:Figure 1b-e) and clay elbow pipes found at Womack (Harris et al. 1965:Figure 6i-j) indicate contact with people in north central Oklahoma. On the other hand, artifacts from the historic Wichita Bryson-Paddock (Hartley and Miller 1977) and Deer Creek (Wedel 1981) sites on the Arkansas River in north central Oklahoma, particularly sherds from Bryson-Paddock of Womack Engraved pottery (Bell 1984b:Figure 17,3h) that must have come from the Red River Valley and European trade beads of the same types found at Angola Farm and Womack (Brain 1979:116-

131), indicate who the early historic period central Oklahomans visiting the Womack site were.

Thus it would appear that soon after 1700 the Wichita began bringing buffalo hides and other buffalo products, catlinite pipes, and (I imagine) locally procured and halter trained feral horses to the Tunica entrepot at Womack to exchange for the French guns and ammunition they needed to fight their battles with the Apaches and to keep their neighbors to the northeast, the Osage, off their backs. Therefore, I suggest that the Tunica obtained their treasure mostly by exploiting between 1700 and 1760, in cooperation with the Wichita of north central Oklahoma, a supply of feral Spanish horses on the Blackland Prairie that the Europeans did not know about. Their first entrepot was at Womack, with some occupation of the nearby Sanders site, the site of their original Red River entrepot 500 years earlier. Considering that some of the European trade goods indicate the site could have been occupied as early as 1675 (Harris et al. 1965:360) and considering Gregory's (1973:281) observation that the Tunica were obtaining horses from somewhere up the Red River as early as 1701, I estimate that the entrepot at Womack had been in operation for about 20 years when La Harpe established the Nassonite Post 110 miles down the Red River from it in 1719. The trade goods at Womack indicate that occupation ended prior to about 1729 (Harris et al. 1965:360), while those from the Gilbert site (Jelks 1967:243), and possibly the nearby Pearson site (Duffield and Jelks 1961:79), indicate that those occupations began about the time Womack was abandoned. This suggests that the Tunica moved their entrepot south to the headwaters of the Saline soon after La Harpe opened his post in 1719. Their main reason may have been to avoid bringing horses down the Red River past the newly established French post, thus blowing the cover on the Blackland Prairie horse herd. Increasing pressure from the Osage may have been another factor. Or, maybe they just found the Gilbert site more convenient to their home base at the mouth of the Red River.

Considering that snub-nosed endscrapers were probably not butchering tools but tools "used to remove hair and reduce hide thickness, later steps in hide processing" (Creel 1991:42-43), the extraordinarily large numbers of these tools at Womack (872 specimens; Harris et al. 1965:294-295) and Gilbert (418 specimens; Jelks 1967:197-198) leave no doubt in my mind that these were hide trading as well as horse trading entrepots, places where the Tunica received raw hides from the Wichita and prepared them for transport and trade to the east and southeast in Arkansas and Louisiana.

In conclusion, I would say that I agree completely with Gregory (1973:v, 275; Jeter et al. 1989:238-239) that when Europeans began moving into the Lower Mississippi Valley, the Trans-Mississippi South, and the Southern Plains, they didn't have to go to the trouble of finding out for themselves what the re-

sources of this vast area were and of establishing the complex system of trade relationships with dozens of different tribes that was necessary to obtain them. They simply plugged themselves into "an established Indian trade network", something that was easy for them to do since the Indians welcomed the goods they had to offer. I am convinced that this Indian trade network was, as Gregory says, "a very complex system of barter extending from the Mississippi River to eastern New Mexico ... and from the Arkansas River to the Gulf" and that, as he also says, "Items exchanged included ceramics, salt .... hides, Osage orange (bois d'arc) wood for bows, and horses and Native American slaves from the Plains and Southwest". All I am doing in reinterpretation of the Spiroan phenomenon, of the Sanders and Nagle sites, and now of Womack, Gilbert, and possibly, Pearson, is marshaling evidence that this was indeed a "system" and that it was the creation of the Tunica. I think that there is good evidence that they (as the Spiroans) established it around A.D. 1000, that they (as the Tula) were running it when the Spaniards invaded the Mississippi Valley in 1542, and that they were still running it one hundred and sixty years later when the French arrived in Louisiana.

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END NOTES

1) This is a lightly revised version of the paper I read at the 38th Caddo Conference, Natchitoches, Louisiana, March 29, 1996.

2) In December 1995, I observed one excellent specimen of this type, consisting of most of a single pot in fragments, in the Texas Archeological Research laboratory collections from the midden area at the Sanders site.