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## **Contents**

**Reminiscences of Archeology in Texas: 1947-1968**  
Edward B. Jelks

**Accidental Plains Archeologist: Neither Courage Nor Naiveté?**  
David M. Gradwohl

**The Paleoindian Laird Bison Bone Bed in Northwestern Kansas**  
Jack L. Hofman and Jeannette M. Blackmar

**James H. Howard, Ethnographer (1925-1982)**  
**Observations and Recollections of a Friend**  
Alan R. Woolworth

**In Response to Hanks' "The Status of Siksika Blackfoot Women"**  
**Comments from Blackfeet Community College Students**  
Alice B. Kehoe and Darrell R. Kipp, Editors

**Response to the Blackfeet Community College Students**  
Jane Richardson Hanks

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# KANSAS ANTHROPOLOGICAL ASSOCIATION

The Kansas Anthropological Association is the oldest amateur archeological organization in the state. Its membership is made up of individuals and institutions interested in the prehistoric and historic peoples of the area. The objectives and goals of the Association are the preservation and interpretation of archeological and ethnographic remains within the state; the scientific study, investigation, and interpretation of archeological remains and ethnographical materials; the publication and distribution of information concerning Kansas archeology and ethnology; and the development and promotion of a greater public interest and appreciation for the heritage of the state.

Types of memberships and dues:

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# CONTENTS

Reminiscences of Archeology in Texas: 1947–1968.....	Edward B. Jelks	1
Accidental Plains Archeologist: Neither Courage Nor Naiveté?.....	David M. Gradwohl	19
The Paleoindian Laird Bison Bone Bed in Northwestern Kansas.....	Jack L. Hofman and Jeannette M. Blackmar	45
James H. Howard, Ethnographer (1925–1982) Observations and Recollections of a Friend.....	Alan R. Woolworth	59
In Response to Hanks' "The Status of Siksika Blackfoot Women" Comments from Blackfeet Community College Students.....	Alice B. Kehoe and Darrell R. Kipp, Editors	71
Response to the Blackfeet Community College Students.....	Jane Richardson Hanks	73
Book Reviews.....		75
About the Authors.....		85
1997 Kansas Anthropological Association Members.....		87

# Contents

Introduction	1
Chapter I	10
Chapter II	25
Chapter III	40
Chapter IV	55
Chapter V	70
Chapter VI	85
Chapter VII	100
Chapter VIII	115
Chapter IX	130
Chapter X	145
Chapter XI	160
Chapter XII	175
Chapter XIII	190
Chapter XIV	205
Chapter XV	220
Chapter XVI	235
Chapter XVII	250
Chapter XVIII	265
Chapter XIX	280
Chapter XX	295
Chapter XXI	310
Chapter XXII	325
Chapter XXIII	340
Chapter XXIV	355
Chapter XXV	370
Chapter XXVI	385
Chapter XXVII	400
Chapter XXVIII	415
Chapter XXIX	430
Chapter XXX	445

TABLE OF CONTENTS OF THE SECOND VOLUME OF THE WORKS OF THE AUTHOR

# REMINISCENCES OF ARCHEOLOGY IN TEXAS: 1947-1968

Edward B. Jelks  
Professor Emeritus, Illinois State University

*The Kansas Anthropologist 18(2):1-17*

*A retrospection of archeological activities in Texas during the period 1947 to 1965 is presented as viewed through the experiences of the author, with particular focus on the Texas River Basin Surveys.*

## BACKGROUND

In January 1942, shortly after the Japanese attack on Pearl Harbor, I dropped out of The University of Texas at Austin (UT), where I was a pre-med student, and signed on as a medical corpsman in the Navy. After boot camp and brief training at Mare Island Hospital in California, that fall I found myself working as an operating room technician in a Navy field hospital on Guadalcanal, an experience that soured me on a career in medicine.

At the end of the war, I mustered out of the Navy and reentered UT for the spring semester of 1946. By then I was married with a baby a few months old. I wanted to take advantage of the G.I. Bill to get a college degree in the shortest possible time and then get a job.

I chose a B.A. in English literature as the quickest way to a degree, English having been my minor as a pre-med. Needing a minor for the English major, I decided on anthropology after taking an introductory course from George Engerrand. By the time I had completed the B.A., the archeology courses I had taken from Tom Campbell and J. Charles Kelley for the anthropology minor had sealed my fate.

Hooked on archeology, I postponed full-time gainful employment and, with the support of my wife, Judy, started working on an M.A. in anthropology. The decision to give up a medical career in favor of a lifetime of digging holes in the ground did not find favor with my parents and other family members, but Judy and I took the plunge anyhow. Now I can look back on the past half century with no regrets about our decision.

By the fall of 1949, I had completed the requirements for the M.A. — except for a thesis. At that time Bob Stephenson, director of the Smithsonian's River Basin Surveys (RBS) program in Texas, hired me as his assistant. I became acquainted with Bob while doing research for my thesis at UT's old "nut lab," where both the university's archeological

collections and the RBS office and laboratory were housed.

Stephenson had been scrimping along with virtually no funds for fieldwork since taking over the RBS operation from Joe Ben Wheat in 1947. Despite that handicap he managed to conduct preliminary surveys at several of the larger reservoir projects that were just getting under way, economizing by using his family car for transportation, camping at car side wherever he happened to be at the end of a day, cooking over a campfire instead of eating at restaurants, and the like.

Early in 1950 Stephenson joyfully received the first allocation of RBS funds substantial enough to finance serious excavations, so in March we set out with great expectations and enthusiasm for Whitney Reservoir on the Brazos, where I got my first professional fieldwork at three rockshelters (Figure 1), an open prehistoric site, and a large late-eighteenth- and early-nineteenth-century Indian village, the Stansbury site (Figure 2), containing a lot of European trade materials (Stephenson 1970; Jelks 1970). In July we moved to Lavon Reservoir on the East Fork of the Trinity River where — by October when the money ran out — we had excavated a large part of the Hogge Bridge site, type site of the Wylie focus.

In June 1951 Stephenson left for graduate studies at the University of Michigan, and I took over as director of the RBS program in Texas. For the next 17 years I continued directing salvage archeology at reservoir projects in Texas and Louisiana with several changes in institutional affiliation.

In 1953 the National Park Service (NPS) took over administration of the Texas RBS program from the Smithsonian, giving me a new boss, Erik Reed in Santa Fe, in place of Frank Roberts in Washington. From October 1954 to July 1956, the Texas RBS office was closed temporarily when funds were cut off, and I was transferred to Colonial National Historical Park in Virginia, where I directed



**Figure 1.** Pictograph Cave, Whitney Reservoir, 1950. Ed Jelks taking notes. Photograph courtesy of the Smithsonian Institution.



**Figure 2.** Stansbury site, Whitney Reservoir, 1950. Standing, left to right: Frank Roberts, Ed Jelks, and John Corbett; George Benson crouching in foreground. Photograph courtesy of the Smithsonian Institution.

exploratory excavations at Yorktown Battlefield and served as John Cotter's assistant for extensive exploration of the site of colonial Jamestown.

In July 1956 I returned to Austin and reactivated the Texas RBS office there, still under NPS administration, with laboratory and office space provided by UT in Pearce Hall on the main campus. The program continued in much the same way until the summer of 1958, when the NPS decided to handle RBS fieldwork by contract instead of in-house. I was to be transferred to the NPS regional office in Santa Fe, but Judy and I decided to stay in Austin for several reasons: first, we wanted our son Chris to complete high school in the Austin schools, and second, for the first time UT was about to offer a Ph.D. in anthropology. So I worked out arrangements to have UT continue the RBS program in Texas under contract with the NPS.

I was hired by UT to organize the Texas Archeological Salvage Project (TASP) as a university agency to administer the RBS program in the state. Thus, I was able to maintain regular employment, the RBS program continued without a break, Chris got to stay with his schoolmates, and I had a chance to work on a Ph.D. in my spare time.

The following years saw a marked expansion of the RBS program. At the north edge of Austin, a deserted magnesium plant, built during World War II, was given to UT by the federal government to be developed into a research facility and to be named Balcones Research Center. There were a lot of empty buildings there, and the RBS had outgrown available space on UT's main campus. In 1958 I moved TASP to Balcones, where its successor survives today as a function of the Texas Archeological Research Laboratory (Figure 3).

In May 1965, after seven years of plugging away, I received my doctorate — the first one in archeology awarded by UT — which opened up new opportunities for me in academe. Offered a faculty position at Southern Methodist University (SMU) in Dallas, where Fred Wendorf had established an anthropology department a year or two before, I found myself embarking on a new career as a teacher in the fall of 1965. However, I found time while at SMU to organize a salvage contract program and continued working part time on RBS projects.

At the end of a six-month research fellowship at the Smithsonian in Washington, D.C., in August 1968, my first-hand involvement in Texas archeology



Figure 3. Ed Jelks at TASP laboratory, Balcones Research Center, 1959.

ceased for all practical purposes. I resigned from SMU and joined the faculty of Illinois State University in Normal, where I remained until retirement in 1983.

Between 1950 and 1968 I worked at hundreds of sites at about 40 reservoir projects, scattered over Texas and western Louisiana: in the piney woods of the east; in central, north-central, and western Texas; in the lower Rio Grande valley of southern Texas; and in the Texas Panhandle. The following pages contain reminiscences about my archeological experiences in those days, about research practices of the time, and about some of the many professional and avocational associates with whom I worked.

## TEXAS ARCHEOLOGY IN THE 1950S

When I began graduate studies in anthropology at UT in 1948, there were four archeologists in Texas who could be termed professionals by virtue of job title and advanced degrees in archeology/anthropology. In addition to Bob Stephenson, they were Tom Campbell and Charles Kelley, both on the teaching faculty of the Department of Anthropology at UT, and Alex Krieger, a nonteaching UT research scientist. Several geologists had been doing scientific field archeology for years, especially at Paleoindian sites, notably E.H. Sellards and Glen Evans at the Texas Memorial Museum and Grayson Mead at Texas Tech. Historians Curry Holden at Texas Tech and Victor Smith at Sul Ross also were competent archeologists.

To the best of my knowledge, the only university in Texas offering a degree in archeology at the time was UT, where one could earn a B.A. and M.A. in anthropology with an archeology specialty. Today the Council of Texas Archeologists has a membership of approximately 200, all with professional credentials, and all the major universities in Texas have anthropology degree programs and archeologists on their faculties.

We few archeology students at UT in the late 1940s became aware that, in the minds of at least some of the professionals, Texas had been divided, like ancient Gaul, into three parts, with one archeologist having proprietary rights to each part. Krieger's primary domain was East Texas, but he also held rights in the Panhandle because of his publications on the Antelope Creek phase; Campbell focused on the coastal region; Kelley claimed a broad area encompassing central Texas and the Trans-Pecos region. I recall an occasion when one proprietor was outraged because another professional was alleged to have trespassed on his territory.

Paleoindian sites, few and far between, were viewed as a special case and were pretty much up for grabs for whoever could find one. However, it was generally recognized that the team of Sellards and Evans had some priority on Paleoindian sites because of their previous work at Plainview, Lubbock Lake, Miami, Blackwater Draw, and other early sites. Krieger also had a long-standing involvement in Paleoindian research, some of it in collaboration with Sellards and Evans.

By the mid 1950s the tripartite archeological segmentation of Texas had disintegrated, largely because RBS archeologists were working pretty much all over the state. Also, two of the proprietors pulled up stakes. Kelley joined the faculty at Southern Illinois University in 1950, and Krieger moved to California to become director of the Riverside Museum in 1956. Jack Hughes led a migration of archeologists to Texas colleges and universities when he came to West Texas State College at Canyon in 1951. He was followed in the next 10 years or so by Jane Kelley to Texas Tech, Frank Hole to Rice, and George Agogino to Baylor.

In the late 1950s and early 1960s, several new archeologists appeared on the scene and began to make their presence known. First E. Mott Davis and then Jeremiah Epstein were hired by UT, Davis to fill the research position vacated by Krieger and Epstein to assist with the RBS program. Having Ph.D.s, both subsequently became full-time teaching faculty in the Department of Anthropology. Mott's major RBS fieldwork was in the Caddoan area; Jerry's, in the Lower Pecos River area. Dee Ann Suhm (Story), having completed her Ph.D. at UCLA, was hired as anthropology curator at the Texas Memorial Museum. She later became my assistant at TASP and, still later, TARL director and a UT professor.

I hired Dee Ann to fill a part-time secretary position at the Austin RBS office in the early 1950s while she was an undergraduate student at UT. I tried to give promising students an opportunity for some hands-on archeological experience in the RBS office and lab, even if they might be somewhat deficient as typists, as Dee Ann was in the beginning. Dee Ann, my wife, Judy, and I developed strong bonds of friendship. We still keep in touch, though she resides in Texas, while Judy and I have lived in Illinois for almost 30 years.

By the late 1950s RBS job opportunities, with the promise of professional employment after graduation, were beginning to attract more and more students to archeology in Texas. Some stayed on to become leaders in Texas archeology: Dee Ann, Curtis Tunnell, Harry Shafer, LeRoy Johnson, Jr., Elton

Prewitt, and Jim Corbin were among those who got early field and laboratory training as RBS employees in the late 1950s and early 1960s.

In addition to the professionals, there were a number of avocational archeologists with various backgrounds who, by the 1950s, had made important contributions to knowledge of Texas archeology by publishing descriptions of their fieldwork and, sometimes, synthetic interpretations. Abilene osteopath Cyrus Ray was the best known of these. A founder of the Texas Archeological Society (TAS), he devoted many years to studying and publishing extensively about the prehistory of the Abilene region. Other prominent avocational archeologists included printer Frank Watt of Waco, leading light of the Central Texas Archaeological Society; railroad engineer R.K. Harris of the Dallas Archaeological Society; businessman Richard Worthington of the Houston Archaeological Society; and attorney O.L. Sims of Paint Rock, who was instrumental in arranging permanent protection for the nearby spectacular pictographs for which the town was named. They, along with many others all over the state, maintained a close working relationship with professionals. Pediatrician Clarence Webb of Shreveport made major contributions to knowledge of Caddoan area prehistory, including not only northwestern Louisiana but also adjacent sections of Texas, Arkansas, and Oklahoma.

### **THE TEXAS ARCHEOLOGICAL SOCIETY**

The TAS has been a significant influence on archeology in Texas since its founding in 1928. Initially named the Texas Archeological and Paleontological Society, the TAS was nurtured and led for its first 20 years by Cyrus Ray, who served as president and editor and was a major article contributor to the society's annual bulletin from 1928 to 1948.

From its inception the TAS has continued to provide a forum where professional and avocational archeologists become acquainted with one another, exchange information, and mingle socially at the annual meetings. A long-standing tradition of close cooperation between professionals, collectors, and avocational archeologists was begun by UT professor James E. Pearce in the first three decades of the twentieth century. This tradition became firmly entrenched in the last half of the century, in large measure due to programs of the TAS, such as the annual summer field school, which fosters scholarly collaboration between professionals and amateurs.

### **THE RIVER BASIN SURVEYS**

Shortly after the end of World War II, the River Basin Surveys program began. Administered by the Smithsonian Institution and the National Park Service, the RBS was a nationwide effort to salvage archeological resources endangered by dam and reservoir construction. The major RBS offices were in Lincoln, Nebraska, Austin, Texas, and Eugene, Oregon. The program continued in full force from the late 1940s until most of the major post-war dam and reservoir projects were completed in the 1970s.

The RBS office in Austin was opened in 1945 under the direction of Joe Ben Wheat. After completing important fieldwork at Addicks Reservoir near Houston, Wheat left for Colorado, and the office was closed temporarily until Robert L. Stephenson took over in 1947. Once funding reached an adequate level to support intensive site excavation in 1950, RBS fieldwork began to produce large amounts of archeological data that led to more and more interpretations and reinterpretations of prehistoric and protohistoric Texas cultures.

### **CHRONOLOGIES, PHASE DEFINITIONS, AND ARTIFACT TYPOLOGIES**

As of the early 1950s, artifact typologies, regional chronologies, and definitions of cultural phases had been advanced by several researchers. Krieger had organized post-Archaic East Texas prehistory into a number of foci, which were grouped into two aspects, following Will McKern's Midwestern Taxonomic System (MTS) for cultural classification. But Krieger arranged his aspects in chronological sequence, in contrast to the assumption underlying the MTS that cultural units in the system were atemporal. Campbell also had used the MTS in defining Archaic and post-Archaic foci and aspects for the Texas coastal area, again ascribing them temporal values within a regional chronology. Kelley had followed a similar plan in identifying foci, aspects, and phases for the central Texas region, arranging them in chronological sequence. Kelley, Krieger, and Campbell also had defined numerous types of artifacts, most notably chipped-stone projectile points and, especially in the Caddoan area of eastern Texas, pottery types. These were based generally on concepts of typological classification adumbrated by Krieger (1944) in his paper, "The Typological Concept."

## TEXAS ARCHEOLOGY — 1950S STYLE

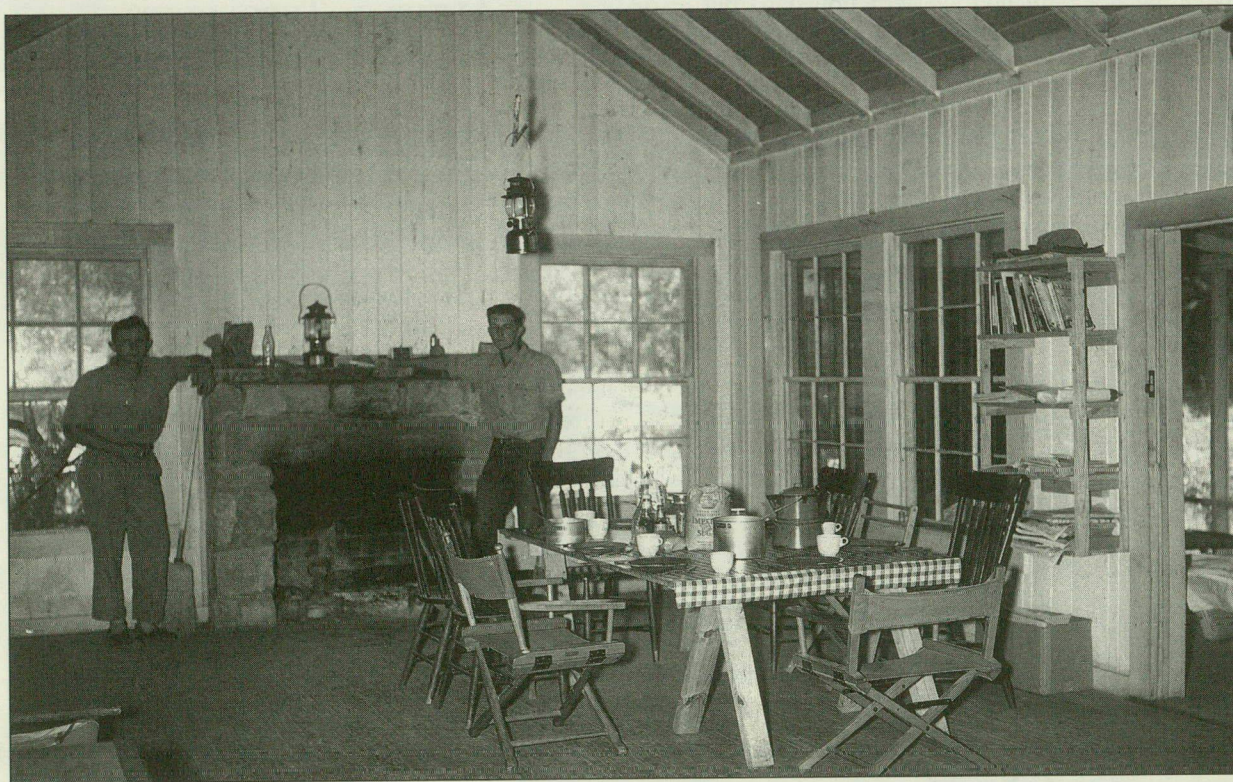
### FIELD LIVING CONDITIONS

There have been many refinements in dating and definition of types and phases since those days, and of course new types have been recognized, and new phases have been defined since the 1950s. But it is noteworthy that many elements of the archeological classifications developed and promulgated by Campbell, Kelley, and Krieger still are current, including names for important types and culture units (generally phases now instead of foci). These three pioneers deserve credit for limning the broad outlines of several major prehistoric cultural patterns in Texas that have served archeologists well over the years.

The flood of data coming from the RBS program had an earlier counterpart in WPA archeology of the 1930s and early 1940s, administered by UT. WPA crews of 100 to 200 shovel hands, usually supervised by one archeologist and perhaps an assistant, carried out salvage excavations at several dam and reservoir projects on the Colorado, Brazos, and Red rivers. Immense quantities of artifacts and field notes generated by the WPA projects survive as primary data of unique importance to current interpretative studies, despite the gross excavation methods necessitated by excessively large field crews overseen by too few trained supervisors.

Before 1950 excavation crews almost always lived in tent encampments and cooked on portable gasoline stoves. In 1950 at UT's archeology lab, where the RBS office and lab were housed, there were not only all the university's artifact collections but also a large storeroom filled with field tools and camping gear, mostly left over from WPA days: eight-man tents, kerosene lanterns, Coleman stoves, cots, and the like. This equipment was used the previous year at a UT field-school camp that I attended, directed by Charles Kelley, on the Mexican border in western Texas near Redford.

When getting ready to begin excavations at Whitney Reservoir in March 1950, Stephenson and I were prepared to set up a tent camp if necessary, but we were lucky to find a hunting cabin on Steiner Valley ranch that the owners let us use (Figure 4). It had electricity, a propane stove, space heaters, and even hot and cold running water. This spoiled us, so



**Figure 4.** Field headquarters at Steiner Valley Ranch, Whitney Reservoir, 1950. Ed Jelks (left) and Ed Moorman. Photograph courtesy of the Smithsonian Institution.



**Figure 5.** Cataloging artifacts at field headquarters, Lavon Reservoir, 1950. Clockwise: Bob Stephenson (with pipe), Cecil Calhoun, E.O. Miller, and Ed Moorman. Photograph courtesy of the Smithsonian Institution.

when we moved to Lavon Reservoir in June, Stephenson talked to Rex Housewright, a member of the Dallas Archaeological Society, and Rex arranged for us to occupy the home economics house at the high school in Wylie, again equipped with utilities and a modern kitchen (Figure 5).

Eventually it dawned on me that living in a house was vastly superior to living in tents, hauling drinking water, bathing in a creek, reading at night by a lantern, and cooking over a Coleman stove. After becoming director of the Texas RBS office in 1951, I always looked for a vacant house on a farm or in a town for living quarters and a lab. And I was usually able to find one. Before long I figured out that it was legal to hire a cook, after which we did so whenever possible, and thereafter we really lived in civilized style in the field (Figure 6).

## FIELD CREWS

At Whitney and Lavon in 1950, our dozen or so shovel hands were recruited mainly from local laborers whose usual employment was as seasonal farm hands (Figure 7). Their pay was \$35 per week, a cut above the minimum wage of the time. In addition to Stephenson and me, our supervisory staff consisted of E.O. Miller, a retired schoolteacher from Moody with some knowledge of central Texas archeology, and Adolph Witte, a former schoolteacher from Henrietta

with a background in geology, who previously had worked for E.H. Sellards and Glen Evans at several archeological sites, including Plainview and Blackwater Draw.

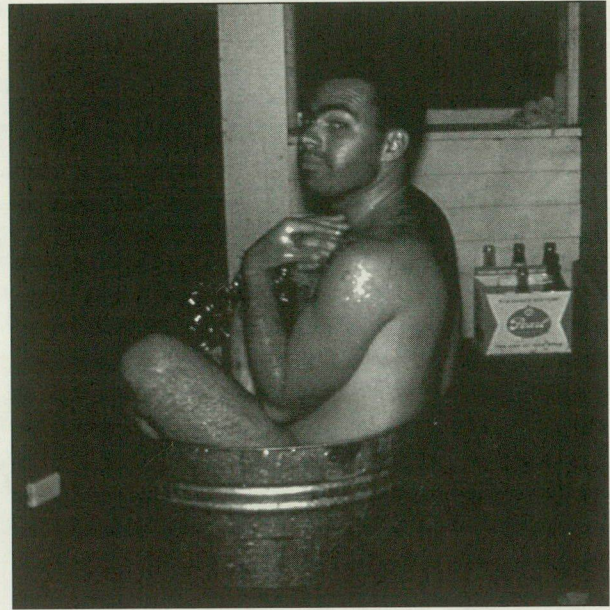
Until the 1950s, when UT and other universities in the state began developing a stable of students seeking field experience and paying jobs, we continued to hire profession laborers as shovel hands for RBS projects, usually at the employment offices maintained by the state in each county. As a commentary on the economy of the time, it was not uncommon to have 50 or 60 applicants for 10 or 12 shovel-hand openings at minimum wage.

As the RBS program expanded, we began to have several crews in the field, simultaneously working different reservoir projects. To meet this requirement, we began to hire full-time crew chiefs with civil service ratings. Typically these were young professionals right out of graduate school with master's degrees and little field experience. A crew chief usually was provided with a field foreman, typically someone who, starting out as a shovel hand, showed keen interest in the fieldwork, picked up quickly on survey and excavation methods, and exhibited leadership qualities. After adequate on-the-job training and seasoning, these were signed on with full-time civil service appointments, working in the Austin laboratory during the winters when fieldwork was closed down or curtailed. Some foremen stayed on for

several years and advanced to the stage where they helped write reports as co-authors with the crew chiefs. E.O. Miller, Ed Moorman, and Bill Davis were memorable examples of this brand of archeological technician.

In the beginning our field crews were comprised solely of men. Female students attended university field schools, and some archeologists took wives or daughters in the field with them on occasion. But there were those who were outspokenly opposed to having women on field crews because, they argued, not only was the work too arduous, but women should not be exposed to the coarse language and sometimes crude behavior ascribed to professional male laborers working on construction crews, archeological crews, and the like. Yet as universities began turning out female archeologists, it was obvious that they were entitled to opportunities to practice their profession and that this bias would soon be eliminated.

The first woman I sent in the field to supervise a crew was Dee Ann (Suhm) Story, in about 1963 as I remember. Being well acquainted with Dee Ann's personality and professional qualifications, I sent her with no qualms to dig some sites (at Waco Reservoir, as I recall) with an all-male crew. She turned in



**Figure 6.** LeRoy Johnson in Texas Jacuzzi, McGee Bend Reservoir, 1956. Photograph courtesy of the Smithsonian Institution.



**Figure 7.** Sheep Cave, Whitney Reservoir, 1950. Ed Moorman at screen on right. Photograph courtesy of the Smithsonian Institution.

her usual sterling performance, this time as a field archeologist, charming her crew and earning their respect as a supervisor, despite initial reservations that some of them had about working for a woman. By the time I moved to Illinois in 1968, female archeologists, students, and fieldworkers were commonplace in Texas as in other parts of the country.

## FIELD METHODS AND STRATEGIES

### Surveys

Stephenson conducted his first surveys of reservoir projects alone. After Stephenson left for Michigan in 1951, I did several small surveys by myself, but before long it became standard practice to send out two-person survey teams. We used a three-stage approach to conducting a typical survey of a reservoir area. The first step was to look through the site files at UT to see if they held any records of sites in the area. Through the years hundreds of sites had been reported to UT by staff archeologists, amateurs, collectors, farmers, school children, and others. These were filed by county, so it was easy to compile a list of recorded sites in a particular project area, together with site location information and names and addresses of informants. Through the grapevine operating between professionals, avocational archeologists, and collectors, we often were able to locate at least one person who was familiar at first hand with some sites in a survey area.

The second step was to contact potential informants — sometimes by mail or telephone before setting out and sometimes by looking them up after arriving in the project area. Informants frequently would guide us personally to sites and/or share what information they had about particular sites, including letting us examine their artifact collections. Some of these people, many of them TAS members, kept accurate records of the sites they collected from, catalogued their collections, and were eager to help out in any way possible. The following are notable examples.

Frank Watt of Waco spent about half a century scouring the countryside for archeological sites over several central Texas counties. His help was invaluable in locating important sites at Whitney and Waco reservoirs on the Brazos River.

Bogey Price of Atlanta and Pete Miroir of Texarkana were familiar with scores of important sites in the northeastern corner of Texas. They guided Stephenson to major sites at Texarkana Reservoir during his survey there in 1948. Several members of the Dallas Archaeological Society — especially King

Harris, Bill Crook, Lester Wilson, J.B. Sollberger, and Rex Housewright — guided us to numerous sites in north-central Texas, including important sites at Lavon, Lake Dallas, and Lewisville reservoirs.

Especially noteworthy was George Benson, a rancher in Hill County who had hunted, fished, and trapped over much of the territory that became submerged under Lake Whitney. A man with only a grade school education, his classroom was the countryside, where the commonplace knowledge he amassed about the plants, animals, and prehistory of the central Brazos valley truly was remarkable. George had observed the locations of numerous artifact scatters and rockshelter sites, but he not only did not disturb them in any way, he misdirected arrowhead hunters about their locations in order to protect them.

When Stephenson appeared at Benson's ranch house in 1947 seeking permission to look over his property for sites that would be impacted by Whitney Reservoir, Benson told him that he recognized the importance of keeping looters away from archeological sites and had done his best to protect them, expecting that sooner or later someone like Stephenson, who knew how to study them properly, would come along. Benson personally took him to a number of important sites, including the major ones excavated at Whitney in 1951: Pictograph Cave, Sheep Cave, Buzzard Cave, and the Steele site (Stephenson 1970). Furthermore, he volunteered to work as a shovel hand during the 1951 and later seasons at Whitney. A few years later Benson found and took me to the Blum and Kyle rockshelters, key stratified sites that first demonstrated a temporal separation of the Austin and Toyah phases (Jelks 1953, 1962).

The third step in conducting a survey was to contact owners of lands to be impacted by the reservoir project, ask their permission to look around, inquire if they knew of any archeological sites on their property, and then search the property for surface indications of archeological sites. Because the time available for carrying out a survey was severely restricted by deplorably limited funds, only high probability places were visited — often only places that were easily accessible. For that reason maps of those early surveys tend to show sites clustered along access roads. The days of 100 percent walkover, sampling by walking transects, and controlled surface collection were years in the future.

When a survey was completed, a report was prepared that included a brief description of each site, its apparent cultural affiliations (based on artifacts collected from the surface), and recommendations for

further work, if any. Two or three dozen copies of these reports were made: file copies for government offices and copies for distribution to vocational archeologists and other interested people. Several of the early survey reports were published in the TAS bulletin. In the beginning, survey reports included maps showing where all the sites were located, but we stopped distributing reports with site locational information in them after the map of Amistad Reservoir with all the recorded sites on it was published in a Del Rio newspaper about 1958, leading to a lot of looting by relic hunters.

## Excavations

The meager funds allocated each year to the Texas office of the RBS were spent on the most urgent projects, that is, those where dam construction was under way and inundation of important known sites was imminent. The idea was to collect a sample of data from a "representative" group of known sites in a project area. In the earlier years there might be enough time and money to sample two to five sites before water was impounded behind a dam. For example, five sites were sampled at Whitney, two at Lavon, and three at Texarkana — nowhere near a truly representative sample of the hundreds of sites inundated by those reservoirs. However, even this small sample produced data that significantly altered, refined, and added new dimensions to the interpretive constructs of Texas prehistorical and historical archeology current at the time.

A popular way of excavating open sites in central Texas with WPA crews in the 1930s and 1940s was called the "broadcast" method. A typical crew consisted of 100 to 200 laborers, supervised by one archeologist and a field foreman. A trench 5 feet wide and long enough to hold up to 100 men standing side by side, 5 feet apart, was dug across one edge of the site to a depth below the occurrence of cultural materials. Then the long wall of the trench facing the site was peeled back by the workmen, each in his 5-foot section keeping pace with his fellow workers, until the excavation had moved completely across the site. Once the open area was wide enough, the back dirt was thrown back into the area already excavated. Thus an entire site — often hundreds of feet across — was excavated and backfilled by a line of men moving like a flock of locusts across the site. When artifacts were found, they were placed in kraft paper bags, labeled with square numbers and arbitrary levels.

Transverse profiles of the site were recorded periodically as the long vertical face progressed. This

method kept a large number of men busy without close supervision. The opposite problem faced RBS archeologists after World War II. There was not enough time, money, or labor to excavate any site completely, so a sampling approach was adopted. Sampling designs were strictly judgmental; that is, digging was generally by squares selectively placed where surface indications suggested that middens or other features might be present (Figure 8). Randomized sampling and other approaches based on inductive statistics were still a few years ahead.

Also in the future was the insistence of the New Archeology movement that one cannot conduct scientific archeological field research without developing formal, explicitly stated hypotheses before beginning data collection. Since we knew nothing about this kind of stuff at the time, we could happily proceed to conduct our fieldwork along judgmental lines, often digging a site just to see what was there and usually generating hypotheses and theories, one after another, as we proceeded. And, in spite of the New Archeology's dictum to the contrary, the data that we collected did "speak to us" and told us many things about cultural, as well as descriptive, artifact types; about the inventories of tools, architecture, burial customs, environmental exploitation, and other culture traits affiliated with particular human societies of the past; about chronological sequences of phases and artifact types in different regions of Texas.

We did not collect or save all the kinds of materials in those days that archeologists commonly collect now. Radiocarbon dating was a couple of years old when I began doing fieldwork in 1950, so we did make a practice of collecting charcoal samples for  $^{14}\text{C}$  dating, being careful to not touch them with the hands or leave them exposed to the air and possible contamination any longer than necessary. Usually we rolled a charcoal sample up in aluminum foil and placed it in a glass jar for transport to the lab. Studies of stone-knapping waste in the United States were just getting under way, and RBS archeologists, unaware of their potential, did not collect waste stone flakes or discarded cores unless there was something unusual about them, such as being of exotic material (e.g., obsidian or Alibates agatized dolomite). We saved only "artifacts," distinguishing them from unimportant manufacturing by-products. Workers unskilled in recognizing artifacts were instructed to bag every piece of stone, bone, shell, or other substance they found, so back at the lab the dross could be separated from the "goodies" and discarded. Flakes and blades that showed edge wear



**Figure 8.** Lewisville site, 1950. Paleontologist Ted White (left center) at Pleistocene fossil location; Clovis hearth at feet of Ed Jelks. Photograph courtesy of the Smithsonian Institution.

or other signs of use generally were saved and tabulated in reports as “utilized flakes.”

I recall a time about 35 years ago when Jerry Epstein announced that he had found burins in



**Figure 9.** Jerry Epstein (right) showing Mott Davis burins that he had found at Amistad Reservoir, 1959.

Centipede and Damp caves at Amistad Reservoir. No one had ever recognized burins in Texas before, and they looked like randomly fractured waste flakes to us. Jerry had to do some tall talking, but eventually he convinced us, and soon every field archeologist began looking for burins along with other chipped-stone artifacts (Figure 9).

Other than artifacts, RBS archeologists routinely collected animal and plant specimens from excavated sites. Workers were instructed to save all bones, no matter how small, as well as other biota, such as fish scales, mollusk shells, seeds, etc. Later in the laboratory, these were sorted through, and some specimens (e.g., bone splinters with no identifiable diagnostic features) were discarded.

We did routinely save soil samples from the different strata, feature matrices, and other structural components of a site with the expectation that maybe they contained pollen or other substances that might yield significant data. Actually, most of the soil samples sat for years on shelves; they are still sitting there for all I know. In the early 1960s we began to collect pollen-profile samples properly from several stratified sites that were analyzed as part of the site reports (cf. Johnson 1964). Eventually the collection of pollen samples became standard procedure.

## Research Objectives

Educated as anthropologists, we were interested ultimately in cultural process, but our immediate concerns were with identifying congeries of artifact types and other culture traits that, by inference, could be ascribed to particular groups of people living in certain regions at specific time periods. As these "foci" or "phases" were identified, we attempted to arrange them in chronological sequences by region. The recognition and definition of what were viewed as cultural/historical artifact types played a major role in these constructs. That is, the elements of material culture ascribed to a phase consisted largely of recognized types, some of which were specific to that phase, others of which were shared with other phases, synchronically or diachronically.

In short, our immediate research objectives for the most part were to organize empirical observations made in the field and the laboratory into a descriptive cultural/historical overview of the Indian cultures of Texas. Secondary objectives, once artifact types, phases, chronologies, etc. had been defined, were to infer lifeways of the respective phases: resource utilization, foodways, trading patterns, and the like.

## NOSTALGIC MISCELLANY

### PERSONALITIES

In the early, formative years of my career, several colleagues were especially influential in shaping my thoughts and attitudes about archeology. The following brief reminiscences are intended to put several of these personalities into focus.

My two archeology professors at UT, Charles Kelley and Tom Campbell, my first two mentors, strongly influenced my orientation toward archeological research. Kelley's enthusiasm for worrying out knowledge about past Texas cultures from the archeological and ethnohistorical records was contagious, and I still share that enthusiasm with him. Campbell emphasized a rigorous adherence to the practice of supplying all relevant empirical data when advancing any kind of archeological interpretation, a practice that is considered absolutely necessary in the "hard" sciences, but which, sadly, is not followed by too many archeologists.

Bob Stephenson was my boss and close associate between my first employment at the RBS office in the fall of 1949 and his departure for Michigan in July 1951. We worked together excavating important sites at Whitney and Lavon reservoirs. Bob had a special



**Figure 10.** Alex Krieger and Ed Jelks, Scharbauer site (where the Midland Woman skull was found), ca. 1953. Photograph courtesy of Texas Archeological Research Laboratory, The University of Texas at Austin, Negative No. 41MD1(50).

talent for organizing and administering archeological programs, and it was a great learning experience for me to help him plan and carry out RBS projects. He was a good teacher, so I had picked up with some measure of confidence when Frank Roberts appointed me to direct the program when Bob left.

The archeologist with whom I was most closely associated over the longest time in those days was Alex Krieger. A full-time research scientist at UT, Krieger was nationally recognized as an authority on the Caddoan area and on Paleoindian cultures, having won the Viking Medal for his publication on the early Caddoan George C. Davis site, co-authored with Perry Newell (Newell and Krieger 1949). A man of wide-ranging interests, Alex was essentially a theoretician and above all an accomplished synthesizer of empirical data with little experience in conducting field excavations. Between 1951 and 1954 I spent a lot of time with Alex, traveling about to look at exciting new finds (for example, Midland Woman in western Texas [Figure 10] and Candelaria Cave in Coahuila), looking over Falcon Reservoir on the lower Rio Grande, attending conferences, or just going to visit a collector who had reported finding something of particular interest. He fell into the habit of stopping by my office frequently, when I was not in the field, just to use me as a sounding board for his thoughts about artifact typology, the diffusion of traits from Mesoamerica to the southeastern United

States, how to get the university to give him money to curate the collections properly, what route Cabeza de Vaca took across Texas in the 1530s, or whatever happened to be on his mind.

In 1953 for one of Tom Campbell's classes, Dee Ann Suhm (Story), combed the literature and put together basic trait lists for the Texas archeological phases recognized at the time. Campbell suggested that she expand it and submit it for publication in the TAS bulletin. When it turned into more work than expected, Dee Ann invited me to be co-author. When Krieger found out what we were working on, he offered to join us. After the manuscript was well along, Alex, with his interest in typology, proposed that we add a section with definitions of projectile point and pottery types, which we did. The thing kept growing. We decided to define new types as well as the ones already recognized. Finally the effort turned into a 598-page monster that took up a whole volume of the annual bulletin (Suhm, Krieger, and Jelks 1954).

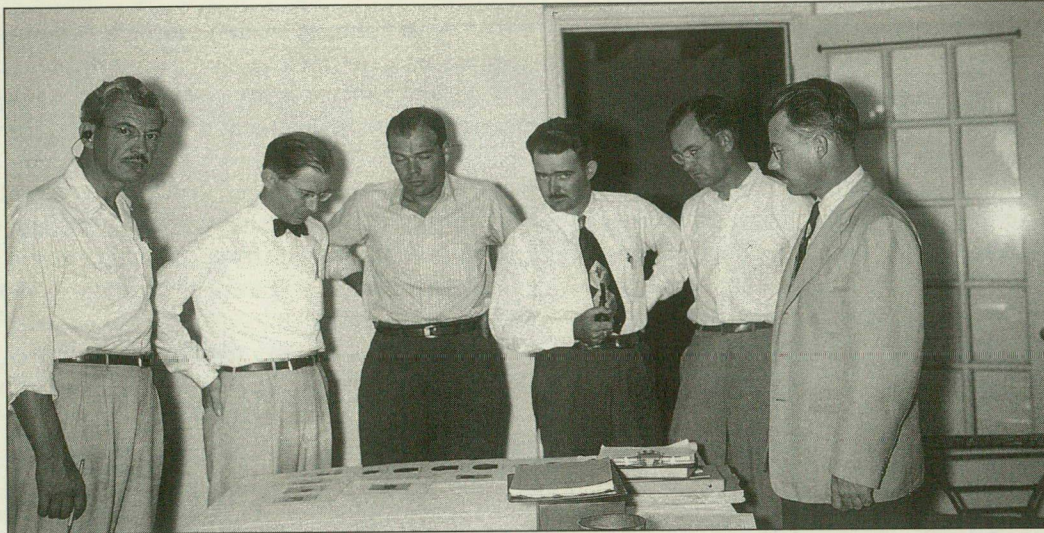
Glen Evans, a geologist at the Texas Memorial Museum in Austin, worked on a number of important Paleoinian sites with E.H. Sellards, director of the museum. They did the initial excavations at the Plainview and Lubbock Lake sites. They also conducted exploratory excavations at the Blackwater Draw (Figure 11) and Midland sites. After Sellard's death in 1961, I became more closely acquainted with

Evans, who routinely visited my field excavations to help me interpret their geological implications. Glen was an unparalleled wizard at this, and through his patient, hands-on instruction I slowly learned the basics of interpreting geologic contexts containing archeological remains, a subject that few geologists were interested in at the time. Without the geological knowledge I learned from Glen, I would have overlooked many things about geologic contexts at archeological sites.

Among my fondest memories are times when some of these mentors got together with colleagues and debated the relative merits of one interpretation versus another. These spirited discussions took place in many different settings: at field camps, at sites under excavation, in automobiles driving across country, in offices and laboratories. An example was the ongoing debate about connections between late prehistoric cultures of the Caddoan area and the lower Mississippi River valley. Krieger, chief architect of the Caddoan chronology, Bob Bell, who came to the University of Oklahoma in about 1950, and Clarence Webb were most knowledgeable about the Caddoan area, while a sizeable group, most notably Jim Ford, Jimmy Griffin, and Phil Phillips, had put together a corresponding set of phases for the lower Mississippi valley. But there was an archeologically unknown swath about 150 miles wide separating the two. Furthermore, neither Krieger, Bell, nor Webb



**Figure 11.** RBS crew visiting dig of E.H. Sellards and Glen Evans at Blackwater Draw, New Mexico, 1950. The truck served as a personnel carrier for the crew from Lavon Reservoir, Texas. Back row, left to right: E.O. Miller, Cecil Calhoun, Adolph Witte, Calvin Rueter, and Bob Stephenson. Front row, left to right: Rex Housewright, Ed Moorman, Ed Jelks. Photograph courtesy of the Smithsonian Institution.



**Figure 12.** Early Caddoan Conference, Shreveport, Louisiana, 1950. Left to right: Alex Krieger, Clarence Webb, Lynn Howard, Bob Stephenson, Bill Haag, and John Cotter. Photograph courtesy of Smithsonian Institution.

had ever worked in the lower Mississippi valley, and the others had never worked in Caddoan area.

In the early 1950s the practice began of convening an annual Caddoan Conference, where Krieger, Bell, Ford, Webb and sometimes Griffin, Bill Haag, Stu Neitzel, and others sat around a table and went after it day and night for two or three days (Figure 12). I felt very privileged to sit and listen to these stalwarts gnaw on every conceivable interpretation of how the cultures of the two areas correlated or didn't correlate, how to explain the striking similarities between the two areas and Mesoamerica (e.g., flat-topped mounds, ceremonial art motifs, etc.), and other meaty topics. Krieger and Ford in particular held strong but differing opinions about how to interpret certain things, and neither was about to yield an inch to the other.

By the early 1960s attendance at the Caddoan Conference began to increase dramatically as newcomers became involved: first Mott Davis, Bob McGimsey, Dee Ann Story, Charlie Steen, Dave Barreis, Lathel Duffield, Curtis Tunnell, and later many others. When meeting in Austin or Norman, we began to invite students to sit around the sides of the room and listen to the pearls of wisdom dropping from the lips of the experts. Before long students were permitted to ask questions, then make comments, and eventually to join the circle. Now the annual Caddoan Conference has become a major regional event with close to a hundred people in attendance.

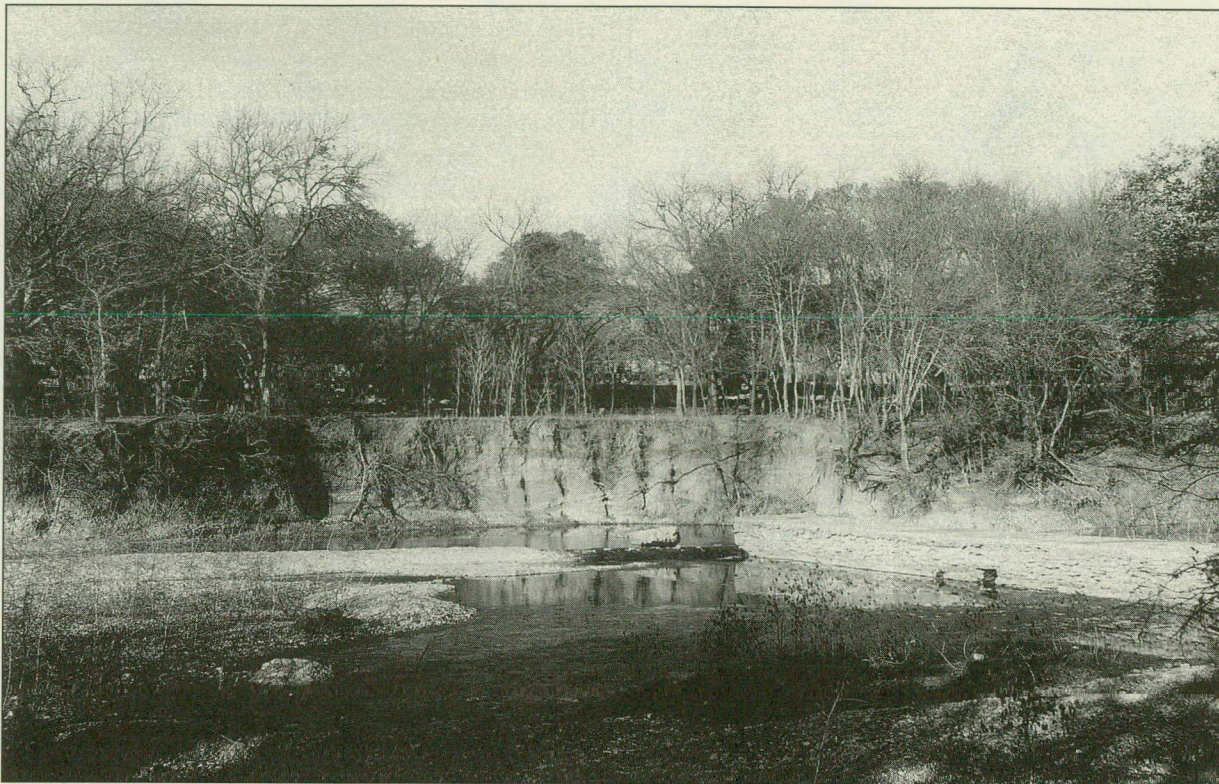
One of the last major research projects I was involved with between 1965 and 1967 before leaving

Texas was a pilot study of Wichita Indian archeology and ethnohistory (Bell, Jelks, and Newcomb 1967). Funded by the National Science Foundation, there were four collaborators: Bob Bell, who headed the study of Wichita archeology north of Texas; Tyler Bastian of the Museum of the Great Plains, who assisted Bell; Bill Newcomb, director of the Texas Memorial Museum, who did the ethnohistory; and I, who directed the archeology in Texas. We were successful in identifying in the field a number of documented historic village sites of the six tribes of the Wichita Confederacy. Test excavations at several sites, both in Texas and in Oklahoma, produced good results. Four graduate students at SMU who participated in the project became career archeologists: Kathleen Gilmore, Norma Hoffrichter, Dessamae Lorrain, and Ned Woodall.

The potential for a continuation of this preliminary study was evident, and we laid out grand plans for further long-range studies. However, Bell and Newcomb became involved in other research, Bastian left the Museum of the Great Plains, and I moved to Illinois, so our plans were first postponed and eventually abandoned. This would be a great project for someone to pick up now.

## BREAKTHROUGHS

Despite operating naively without a hypothetico-deductive approach, we did make serendipitous discoveries from time to time that increased knowledge of Texas prehistory significantly. In winding up



**Figure 13.** The Blum site, Whitney Reservoir. View looking across the Nolands River at cutbank; rockshelter in background. Photograph courtesy of the Smithsonian Institution.

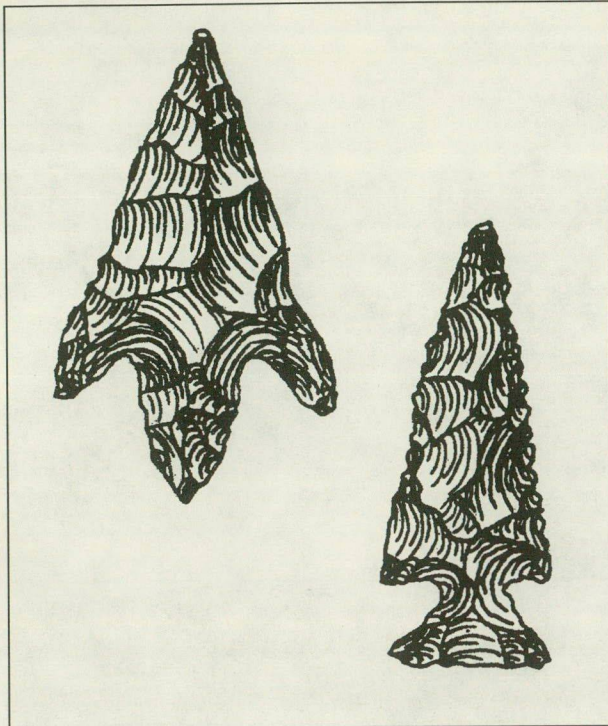
my reminiscences I would like to recall one example of such breakthroughs.

In 1952 George Benson, the rancher who had been so much help to us at Whitney two years earlier, chanced upon a small rockshelter in the Nolands River valley near Blum at the edge of Lake Whitney. A cutbank of the Nolands channel 18 feet high in front of the shelter revealed stratified alluvial deposits containing evidence of human occupation down to their lowest levels. The alluvium extended into the rockshelter, and its thickness suggested the potential for a lengthy chronological sequence. When Benson called the site to my attention, I decided to explore it with the hope finding early Archaic, or even possibly Paleoindian, remains in the lower levels (Figure 13).

I spent about three weeks testing the site in 1952, excavating a sizeable section of deposits within the shelter and a trench in the alluvium between the shelter and the river. As we started taking down the initial squares, all we found at first was modern trash: bottles, tin cans, an automobile battery, and other junk. After digging down a foot, then another foot, and still another foot, all we had found was modern debris, and I began to wonder if the lower levels were as old

as I had thought. Then, a little more than 3 feet below the surface, prehistoric trash began appearing, and we continued with somewhat dampened optimism. The deposits were fairly rich, and artifacts began showing up in quantity, along with the usual chert flakes, garbage bones, and the like. Among the finds were a number of arrow points, all of the Perdiz type, considered a marker of the Toyah phase ca. A.D. 1200–1500 (Kelley 1947).

A brief review of the generally accepted late prehistoric chronology for central Texas is necessary at this point. In scores of central Texas sites dug before 1952, it was repeatedly observed that on or near the surface of massive Archaic deposits — especially burned-rock middens — there usually was a relatively light scattering of arrow points, as distinguished from the Archaic dart points in the underlying deposits. The arrow points were of two major types: Scallorn, ascribed to the Austin phase, and Perdiz, ascribed to the Toyah phase (Figure 14). As no stratigraphic separation had been observed between Scallorn and Toyah in the largely Archaic sites, it had been concluded that the two point types were contemporaries, dating to the period ca. A.D. 1200–1500 (Krieger 1946:165–168; Kelley 1947).



**Figure 14.** Perdiz (left) and Scallorn (right) arrow points. Perdiz is a marker of the Toyah phase; Scallorn is a marker of the Austin phase. Both are pictured here at 150 percent of actual size.

Back at the Blum site, as we began to get into the cultural deposits and continued finding Perdiz points, I started to think, "Maybe we have stumbled on a pure Toyah Phase component with no Scallorn points mixed in." However, after digging through a couple of feet of Toyah materials, we hit a thin layer of yellowish soil, 3 or 4 inches thick, containing scant cultural debris. We did find several arrow points in it, including both Perdiz and Scallorn. And continuing to dig deeper, we came upon a discrete stratum, containing a lot of cultural materials, including lots of Scallorn but no Perdiz.

In the end we recovered excellent samples of cultural remains from both the upper and lower strata. And there was an absolutely clean stratigraphic separation of Perdiz in the upper stratum from Scallorn in the lower stratum, with a thin mixed stratum in between. Also between the two strata there were distinct differences in some of the other artifacts — especially knives and scrapers — as well as in the garbage, which contained bison bones in the Toyah zone but none in the Austin zone.

What did this mean? Was Scallorn (and consequently the Austin phase) earlier than Perdiz (and the Toyah phase) over all of central Texas, or were they

generally coeval, the Blum shelter not being truly representative? Results of radiocarbon analyses, received several months later, indicated that the lower stratum dated ca. A.D. 800–1100; the upper stratum, ca. A.D. 1200–1500.

Several years later at the Smith Rockshelter more than 100 miles south of Blum, Dee Ann Suhm (1957) found the same sequence with similar radiocarbon dates. Eventually other sites dug by several archeologists confirmed the temporal distinction between the Austin and Toyah phases and their respective marker arrow point types, Scallorn and Perdiz. Other distinctions held too, such as bison with Toyah but none with Austin.

I started out at Blum hoping to find a lengthy sequence, spanning perhaps 8,000 or 10,000 years in an 18-foot-thick alluvial deposit, but I learned in the end that it had taken less than 1,000 years for the deposit to accumulate and only a century for the upper 3 feet. However, the site did produce data that corrected a widespread misconception about the temporal relationship between the two last prehistoric phases in central Texas. Through the years there were other breakthroughs with results as revealing as the Austin-Toyah example.

The problems we were working on were primarily uncomplicated ones that could be addressed by drawing inferences, in a single step, directly from empirical observations made in the field. There were no complex, a priori processual models, no general systems models, no interaction spheres, no density equilibrium models, or the like to be tested. We followed no rigidly randomized sampling strategies. We simply selected for study those sites that seemed most likely to produce abundant data from different time periods, that were the largest, or best stratified, or oldest, or most unusual, or which had the best potential for yielding wooden artifacts or other perishables. Sometimes a site such as Blum surprised us with completely unexpected information, sometimes we found pretty much what we expected, sometimes the results were disappointing. But it was all very exciting and fulfilling. And above all it was fun.

**Acknowledgments.** Most of the photographs illustrating this paper are from the archives of the Smithsonian Institution and the Texas Archeological Research Laboratory at The University of Texas at Austin. It is a pleasure to identify and extend sincere thanks to the two people who tracked down those photos: Deborah Hull-Walski, Collections Manager of the Smithsonian's Department of Anthropology, and Carolyn Spock, Head of Records at TARL.

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## NOTES

## ACCIDENTAL PLAINS ARCHEOLOGIST: NEITHER COURAGE NOR NAIVETÉ?

David M. Gradwohl  
Iowa State University

*The Kansas Anthropologist 18(2):19-43*

*A serendipitous opportunity to participate in "salvage" excavations at a protohistoric Arikara Indian village, threatened by the construction of Fort Randall Reservoir in South Dakota, propelled the author into a career in Plains archeology. This reminiscence looks back on some of the more memorable experiences in the author's professional pursuits as well as his good fortune in the mentors, colleagues, and students with whom he has worked.*

The letter was, frankly speaking, a bit daunting. It was an invitation to join a group of "senior" Plains anthropologists in reminiscing about "their experiences in pre- and post-World War II Plains archeology." My professional anthropological experiences are definitely post-World War II. I must confess, however, that I do remember Pearl Harbor, although I was only seven years old at the time. Nonetheless, I still flinch a bit at being addressed as a "senior," even though, in truth, I not only qualify for the AARP cheap motel rates but also Iowa State University's early retirement program and even initial social security benefits. When I read and hear about Plains archeology in the 1930s and 1940s, I sometimes think that I missed the "good stuff." On the other hand — especially from the vantage point of the 1990s with increasingly bureaucratic universities and inane-Procrustean cultural resource management (CRM) projects — I realize that the 1950s and 1960s were not all that bad. They were, in fact, exhilarating years for me and provided a focus for my almost impossible number of interests. Moreover, the sense of excitement has still not worn off as I pursue my current anthropological projects. En route, I had the opportunity to apprentice with some marvelous mentors — the real "golden oldies" — and the pleasure of working with many noteworthy colleagues and students. Hence I will engage in adding to the folklore of Plains anthropology. My retrospective anecdotes will range back through the four and a half decades that I have been associated with this particular discipline. If there is a serious point to this discussion, especially for younger colleagues entering the field, it would be that Plains anthropology is still fun and provides a wide choice of fascinating data bases and questions well worth investigating.

The first part of my essay's title, "Accidental Plains Archeologist," is obviously swiped from Jesse D. Jennings' interesting memoirs (Jennings 1994). I feel free to do so, inserting the qualifier "Plains," since my being an archeologist is really much more accidental than in Jennings' case. The story begins in Lincoln, Nebraska, where I grew up generally unaware of my hometown's unique status in Plains anthropology. When I graduated from high school in 1951 and applied for admission to the University of Nebraska, I was, in today's parlance, "totally clueless" as far as an academic major was concerned. If for no other reason than being a maverick, I only knew that I did not want to follow my father and older brother into the profession of law. Over the years I had taken the required career preference and interest tests. Each time the counselors told me what I already knew. They said I had an interest in music. That was certainly not news to me, since I took (and even enjoyed) piano lessons. But even as a young child, I knew that I did not have the necessary gifts to be a professional musician. The counselors also told me that I had very low interests and apparently mediocre aptitudes in mechanical things. This also was not news to me, since I recognized that I had been the only boy at Irving Junior High School afraid of flunking the required shop courses that alternated with science courses. The science classes were intriguing and generally easy, but the wood, metal, and auto mechanic shop courses were beyond my comprehension. I distinctly remember one specific matter that the career counselors told me, however, on the basis of my test scores. They each said, "Your interests are not channeled." In retrospect I would like to think that they meant my interests were eclectic, but, alas, I suspect that is the way one said "clueless" in the 1940s and early 1950s. To his

credit, one prescient counselor suggested that I read Clyde Kluckhohn's recently published *Mirror for Man*. I read the book and found it quite enjoyable, but, professionally speaking, I did not see myself in that mirror at that time.

It was thus that I entered my freshman year at the University of Nebraska without a goal other than my hope of avoiding being drafted into the armed forces. Under those circumstances I was enrolled in what was then called "Junior Division," which was, I think, code-talk for "Arts and Sciences, Undeclared, or Without an Inkling." I decided to take courses I would need to fill my general education requirements: English, history, math, Spanish, speech, and, of course, the required Reserve Officer's Training Corps or ROTC (a subject, incidentally, in which I received my lowest college grade!). Looking forward to the summer, I was resigned to returning to seasonal construction work in which I had previously been engaged as a card-carrying member of the International Hod Carriers, Building, and Common Laborers Union of America Local #540. The work consisted of digging ditches and was absolutely boring, but I was rewarded with the then-respectable Union wage of \$1.37½ per hour. Late in the spring my good friend James Bailey, an engineering major from Lincoln, informed me that he had gotten a job on a "salvage" archeological crew with the Nebraska State Historical Society (NSHS). Since he was the first person to apply for the crew, Bailey had been made the field assistant. The NSHS, however, still needed more workers, then affectionately referred to as "shovel bums." Curious about the possibility of getting out of my hometown for the summer and hanging out with several of my buddies, I sought out Marvin F. Kivett at the Historical Society's office, then in the Nebraska state capitol building (Figure 1; see Gradwohl 1994). Kivett (known to his friends as "Gus") explained to me that the Society would be working along the Missouri River near Chamberlain, South Dakota, that the crew would live in a camp with tents and no running water, would spend all day digging, and be paid a grand wage of \$.75 per hour. I also was led to believe that the archeological excavations would be much bigger than in construction work and that we would take copious notes on all our discoveries. This all sounded good to me except the bad news about non-Union wages. The good news was two-fold: a) I would not have to carry a hod, and b) while poverty was apparently a vow archeologists had to take, silence and chastity were not also required. So I joined the crew.

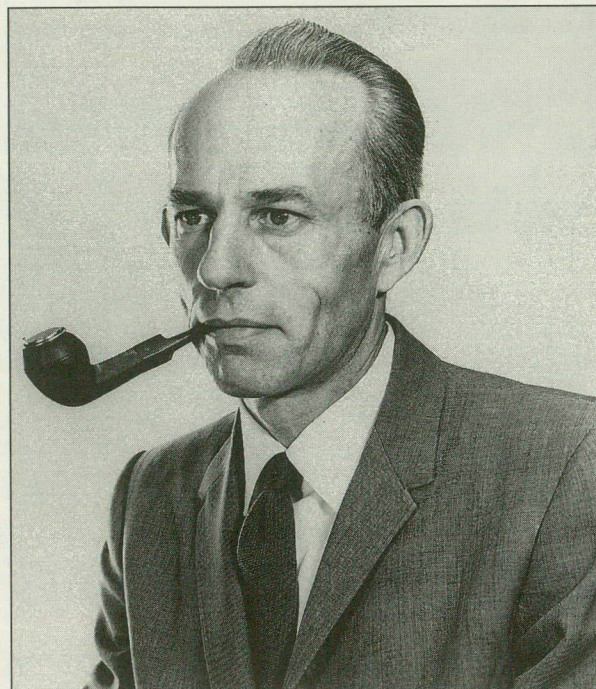


Figure 1. Marvin F. Kivett. (NSHS photograph)

The Historical Society's field camp at the Oacoma site (39LM26 and 39LM27) was everything Kivett had promised. We had old army tents for sleeping quarters, a large and very unwieldy surplus bi-pyramidal tent for a kitchen, dining hall, and lab, and A. T. Hill's old trailer for Gus, Caroline, and Ronnie Kivett along with their dog Rocky. Kivett had hired a woman named Bea Rea as a cook, so we did not even have to do our own cooking. We hauled water from Chamberlain in 5-gallon "jerry" cans. The cans were left in the hot sun all day and thus provided hot water for field showers and clothes washing. Since Al Rea, on whose land we were camping, was a junk dealer of sorts, Bailey and I were able cover the floor of our tent with marble slabs. All the comforts of home! And then there was the camp kaibo. My first task assigned by Kivett was to dig the latrine pit, a sort of doo-doo-detail, saved for the greenest of greenhorns. I had no idea what size pit to dig, so I dug a nice big one that would last all season. In fact it was too wide for the wooden two-seater that the museum carpenters had put together for the field camp. So we jury-rigged a partial pit cover with army surplus canvas. This worked fine and dandy for our field party, but one uninformed visitor from the University of Nebraska crew slipped down into the upper stratigraphic levels of the pit before he could extricate himself from the situation. After we had been at the

site for a few days, a very mild breeze came up and the bi-pyramidal mess tent blew down and ripped. We quickly regrouped and built a sort of ramada-like structure to carry us through the season. We had the luxury of an ice box and were able to obtain ice from a gas station in Chamberlain. The ice was shipped in from Mitchell. One morning when we went to pick up ice, the gas station attendant said that the ice from Mitchell had not been delivered, but if we came back during the afternoon, they would have "Huron ice." He meant, of course, ice from Huron, South Dakota, but we had understood "urine" ice and were not too excited about the possibilities.

At this juncture I must point out that my debut into Plains archeology was not universally well received. Gus Kivett seemed satisfied enough with my work, but Caroline Kivett had reservations. It seems she took an almost immediate dislike to me. I was from the "right" side of the tracks, and, she claimed, I had been rather officious in the matter of reorganizing the mess tent after it blew down. Her account of the event and mine differ. Following the demise of our original mess tent, I had indeed gone into Chamberlain's grocery stores and scrounged orange crates to serve as shelves for the crew's pots, pans, and dishes. Caroline asserted that I had refused to share

the orange crates with her for the Kivett's side of the tent. Frankly, I do not remember her asking for orange crates and cannot imagine that I would have had the temerity to deny a request from the field director's wife. At any rate I was the only member of the 1952 crew to continue on in archeology as a career. To her credit Caroline later acknowledged the irony of this fact to me. She had predicted to Gus that I would not last out the season at Oacoma. In essence I never went away, so we became close friends. For all I know, Caroline may claim that she, and not Gus, was my mentor. The orange crate controversy was finally settled in 1996, when I found some of these curious antiques while cleaning out the basement of my mother-in-law's house. I presented them to Caroline, and she signed a document acknowledging receipt of the crates and absolving me of all culpability, real or imagined, for the affair in 1952.

Oh, yes — the archeology at the Oacoma site was pretty spectacular (Kivett 1962a). We excavated circular earthlodge floors, cache pits, and other features representing the early historic Arikara Indians (Figure 2). I had not heard previously of the Arikara Indians but quickly came to appreciate their culture and history through information provided by Kivett. Gus was a rather quiet person and, after all, not



**Figure 2.** Circular earthlodge floor at the Oacoma site (39LM26), excavated in 1952. NHSH crew from left to right: Tom Stock, Jim Bailey, Peter Schmitt, Bernie Leonard, Tom Payne, Dennis Johnson, David Gradwohl, Ronnie Kivett, and Caroline Kivett. (Photograph by Marvin Kivett)

running a field school, so we did not have formal lectures. But we soon learned that asking a few questions resulted in detailed and instructive discourses that afforded the pleasures of auditing a course on archeology and Plains prehistory without having to take the final exam. By the end of the summer, I had developed a consuming interest in archeology. In fact I took salvage archeology at the Oacoma site so personally that I refused to quit digging on the last afternoon before power equipment had been engaged to backfill the site. I was right in the middle of excavating a rich cache pit, filled with all sorts of goodies, including pottery, projectile points, scrapers, awls, and catlinite pendants. Kivett allowed me to excavate after nightfall with the assistance of a Coleman lantern. As we left Oacoma the next morning, dirt was being pushed back into our excavations. I had a lump in my throat and have hated backfilling operations ever since. The Oacoma site is now covered by the waters of Fort Randall Reservoir (now known as Lake Francis Case).

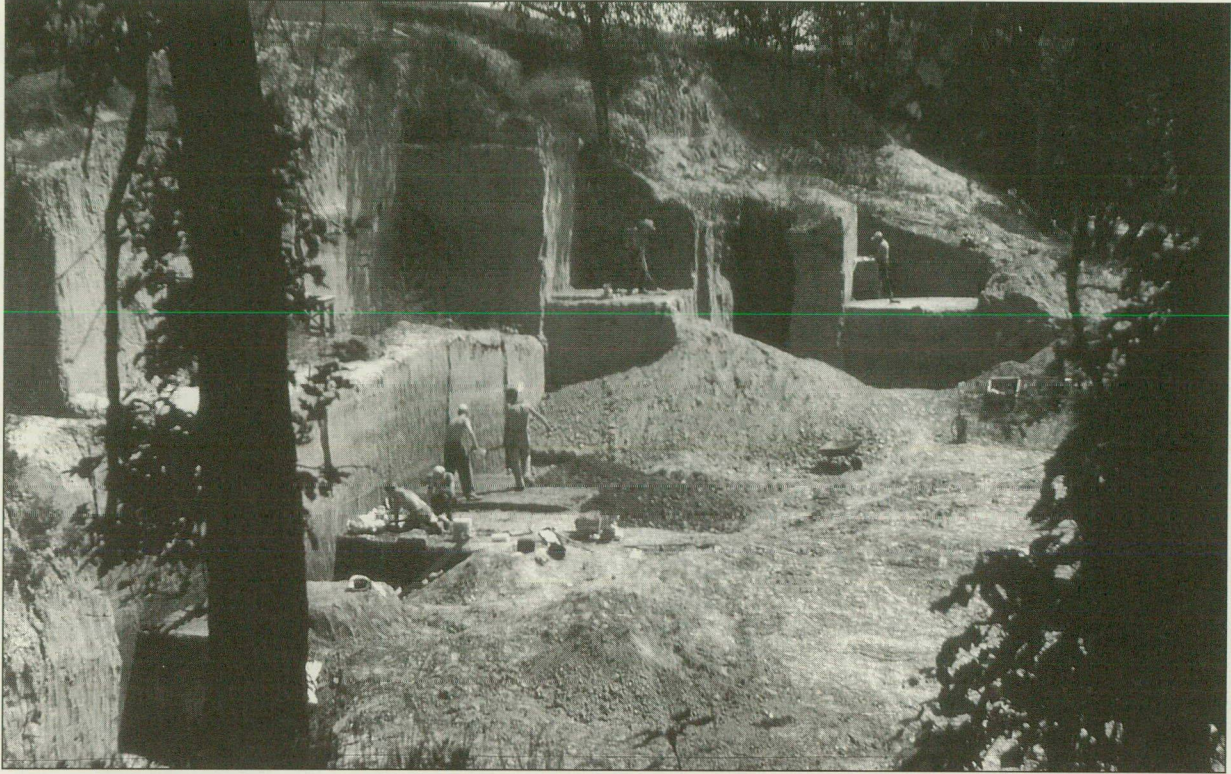
Once back in Lincoln, before university classes started, I immediately changed my registration for my sophomore year. I dropped a political science class, which did not sound too exciting anyway, and substituted in its place Introduction to Anthropology, taught by E. Mott Davis. In retrospect I know that it would take an extremely inept instructor to make the subject matter of anthropology boring. But Davis, though modest, was a professor of extraordinary skill in terms of organization, clarity of presentation, relevant details, and dry humor. I still have my class notes that attest to his pedagogical expertise. During my sophomore year I also took an Introduction to Geology course under Benjamin H. Burma, another charismatic (though not so modest) professor. Although I did not have the formal prerequisites, Burma invited me to go on the geology department's spring field trip to the Four Corners area. I was highly flattered — not realizing that the main reason for the invitation was the department's desire to increase its revenues by more field trip fees. Nonetheless, learning geology in the field was an exciting experience and deepened my interest in earth sciences as well as anthropology.

Meanwhile Davis was urging me to register for the 1953 University of Nebraska field school. I had some misgivings about the proposal, not in terms of the archeology or credit hours, but in the matter of finances. Shelling out tuition money for field experience struck me as a questionable course after my whole previous summer's experience at the Oacoma site. Economically things are not supposed to go that way. My digging activities had gone from union

wages of \$1.37  $\frac{1}{2}$  per hour to the archeological pittance of \$.75 per hour, and now I was faced with a negative cash flow for my efforts. I grumbled to Davis that I could not afford to go to summer school. Davis shot back with his characteristic lack of subtlety, "If you are going into anthropology as a career, you can't afford NOT to go on the field school." Given the poor job opportunities in our discipline in the early 1950s, I was still not at all sure that anthropology would be my career choice. Several of my friends, however, had signed up for Davis' crew, so I registered for nine hours of field and lab credit and found myself at a field camp near Cambridge, Nebraska. Our crew was involved with salvage archeology in the Medicine Creek Reservoir (now called Harry Strunk Lake). Most of our work was at Red Smoke (25FT42), a stratified, multi-component Paleoindian site (Figure 3). Red Smoke combined interesting problems in geomorphology, stratigraphy, paleontology, and lithic technology (Figure 4). The dating of "Early Man" occupations to the range of 8,000 to 12,000 years ago was still a hot issue in the early 1950s (Davis 1953). A recent conference at Cambridge, entitled "Medicine Creek Valley, Nebraska, 1947-1997: 50 Years of Federal Archaeology" was an instructive reminder that the chronologies we take for granted today were initially bitter controversies (Figure 5).

During the 1953 field season we had the convenience of living in an abandoned school house instead of tents. Our field camp even had the luxury of a wooden outhouse instead of a canvas kaibo. One of the activities while excavating Red Smoke was dumping our backdirt across Lime Creek to provide a dry walkway into the site. We called our project "Big Damn Foolishness." Of course the earthen structure was dismantled at the end of the field season. Today's archeologists, indeed, would have to apply for a permit to build such a structure, since only the U.S. Army Corps of Engineers and large hog confinement facilities are allowed to pollute our streams.

It was at the Medicine Creek Reservoir where I got my first experience, of sorts, in underwater archeology. A submerged Upper Republican site was located about a quarter of a mile from the beach where we went for bathing and recreation. Since I was a distance swimmer on the University of Nebraska swimming team, getting to and from the submerged site was no problem. Diving down through the mucky water for artifacts was more difficult. The booty was returned to the beach inside my swimming suit liner, and a small comparative collection was thus assembled.



**Figure 3.** The Red Smoke site (25FT42) being excavated in 1953. (Photograph by E. Mott Davis)



**Figure 4.** Stratigraphy at the Red Smoke site (25FT42), exposed in 1953; David Gradwohl and Larie Holmes point to two Paleoindian occupation zones. (Photograph by E. Mott Davis)



**Figure 5.** David Gradwohl and E. Mott Davis at the Red Smoke site in 1997. (Photograph by Hanna Gradwohl)

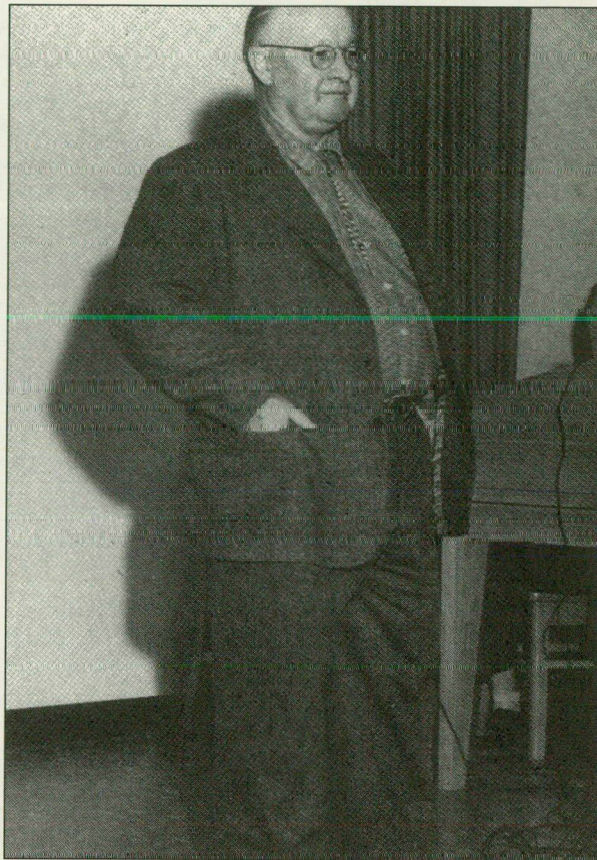
Following the end of the field season at Red Smoke, Davis turned to a contract the University of Nebraska State Museum had with the National Park Service (NPS) to conduct an archeological survey of the Big Sandy Reservoir in southwestern Wyoming. For several weeks we were headquartered near Farson, Wyoming, the quintessential wide place in the middle of the road. Our crew consisted of Davis, James Christensen (now a prominent physician at the University of Iowa College of Medicine), the late Thomas M. Newman (eventually a Ph.D. in anthropology from the University of Oregon), and me, since I still owed a week of free labor for college credits (Figure 6). More memorable than individual sites was Davis' survey strategy involving "Reconnaissance Units," a scheme I later employed, with modifications, along the Skunk and Des Moines rivers in Iowa (Davis 1956; Gradwohl and Osborn 1972). Also memorable was my putative term paper assignment, which consisted of digging a stratification pit through several feet of caliche that seemed to have a hardness of 11 on Moh's Scale. To his credit, Davis did offer to pay me for the time at Big Sandy that extended beyond my field school indentured servitude. He asked me how much money I needed



**Figure 6.** 1953 field camp at the Big Sandy Reservoir, Farson, Wyoming. Left to right: Tom Newman, James Christensen, and E. Mott Davis. (Photograph by David Gradwohl)

for my summer's goal of purchasing a Weston-2 light meter. I answered \$45.00. Davis checked his budget and graciously offered me \$15.00.

Impressed with Davis' generosity, along with the allure of Red Smoke and the Four Corners field trip, I returned to the University of Nebraska at the beginning of my junior year and immediately declared two majors: anthropology and geology. My advisor in anthropology was John L. Champe (Henning 1980). Champe once told us in class that he had the exact cranial measurements of the "old man of Cro Magnon," an amusing comparison to say the least (Figure 7). I considered Champe a good teacher. But if happiness was a notebook full of organized lecture notes, Champe was not your pleasure. His usual technique was to come into class and ask students if they had any questions. If nobody asked a question, Champe was apt to dismiss the class. The next day you can be sure students asked questions and therein started the discussion. In a physical anthropology class under Champe, we were slowly evolving our way through the semester with the help of E. A. Hooton's text *Up from the Ape*. When we got to the "Neanderthal problem," Champe asked us our opinion regarding the status of one of the fossils. We dutifully regurgitated Hooton's opinion. Champe then asked us if we had read the original site report on that particular fossil and, if so, did we agree with Hooton. We all looked at him in unbelieving amazement. Of course we had not read the article; it was published in some obscure European journal. We protested that as undergraduates, we could not even check out such journals from the library. Champe chuckled, took out his faculty library card, and tossed it on the table. "Use my card," he said, "and look up the site reports for the other Neanderthals as well." Hoping to thwart his game, we further complained that most of the original articles were written in French, German, Italian, Croatian, and other languages we could not read, as opposed to Latin and Spanish, which most of us had studied. Our professor just shook his head and pointed out that dictionaries were available at the library and that the Neanderthal problem was our problem. For a while my colleagues and I contemplated mutiny. But in the end we organized task forces, checked out dozens of books on Champe's library card, designed and dittoed standardized information recording forms, and prepared a color-coded pin map of all the Neanderthal and so-called Neanderthaloid fossils. On all our printed material we referred to ourselves as "The Spring Symposium on Neanderthal Man," which I think may not have amused Champe. As I recall, our class never did



**Figure 7.** John L. Champe. (Smithsonian Institution River Basin Surveys photograph)

finish evolving into *Homo sapiens sapiens*, but we did learn a heck of a lot regarding the importance of original evidence and primary sources.

The students in my age cohort also learned via the grapevine from the anthropology graduate students about Champe's famous taxonomy lecture. He was challenging the class to posit the kind of diagnostic traits upon which a classification could be constructed. Champe presented the theoretical case in abstract terms. He reportedly said "Take 'A.' The characteristic qualities of 'A' are its 'A-ness'." The students, however, were thinking in more concrete terms and understood the diagnostic trait to be "anus," as in the lower opening of the alimentary canal. When the class dissolved in laughter, Champe was apparently caught off guard, since his droll humor normally did not include smutty jokes. Champe's sense of humor was not narrow, but I may have pushed it to the limits when I winced at the proposal to lump Upper Republican and Nebraska culture manifestations into an "Aksarben" aspect (see J. Champe 1961). Among other things I argued that

the problems of Plains archeology could not be solved by spelling things backward. Champe was not amused, but he did tolerate my petulance.

Also teaching in the Anthropology Department when I was at Nebraska was John M. ("Jack") Roberts, a social anthropologist (Chick and Nutini 1990). One day Roberts came into class a few minutes early and was schmoosing with some of us who had also arrived before the bell. He was telling us about some articles he was preparing for publication and casually said, "I'm really in anthropology because I want to do research. I really don't enjoy seeing little minds unfold!" Some of my peers were offended, but I still consider the remark one of the funniest I ever heard from the teaching podium. It was funny because, in addition to being a brilliant scholar, Roberts was an excellent, engaging, and enthusiastic teacher.

My undergraduate course work in anthropology at Nebraska included two classes in Plains archeology and Plains ethnology under Davis. In the latter course our first assignment was to prepare two maps, showing the locations of Plains Indians in 1600 A.D. and 1800 A.D. This was an excellent basis for discussing cultural continuity and change, as well as the reliability of early historic sources. As students we were also aware that these questions had an applied significance in terms of lawsuits being prepared under the Indian Claims Commission Act. Champe, in fact, was working as an expert witness for the Pawnee in their land claims suit (J. Champe and Fenenga 1974). In doing so, Champe, a photography buff, reproduced and analyzed early historic Plains maps in regard to tribal locations. We students were allowed to sit in the federal courtroom and listen to the anthropological testimony. This experience was most instructive to me, coming from a family of lawyers, being a member of the University debate team, and trying to learn about Plains Indians. It was fascinating to wonder if the evidence that carried in the classroom would also stand up in court. It was challenging to realize that archeology was relevant to living peoples and not just the study of a static past.

Of the academic transients who wandered through the Anthropology Department in those years, I remember particularly Gene Weltfish (Parks and Pathe 1985). She had been a professor of Champe's at Columbia University and during the 1950s got snared in the nasty net cast upon suspected Communists by Joseph McCarthy and the House Committee on Un-American Activities. Weltfish denied the allegations, but Columbia declined to reappoint her to the position she had held there for 17 years. Subsequently

Champe invited Weltfish out to Lincoln to continue her studies of Plains Indians and, in particular, work up her Pawnee data, which she eventually published as *The Lost Universe* (Weltfish 1965). Weltfish also conducted linguistic interviews with Peter LeClaire, a Ponca elder and tribal historian. On one occasion she was attempting to collect data for a lexico-statistical study of the Ponca language. Since I was writing a term paper on glottochronology for Roberts' linguistics class, I asked Weltfish if I could sit in on her interview as an observer. She asked for LeClaire's permission and then graciously invited me to join them. I was impressed with LeClaire's broad knowledge as an informant and Weltfish's skill as linguist. At one point Weltfish solicited the Ponca word for "urinate." I think LeClaire did not hear the word, so there was a short silence. Rather than repeat the word, Weltfish said "piss," and LeClaire quickly responded. Up to that point in my life, I had never heard an adult woman use the "P" word and that earned my everlasting respect for Weltfish.

In the 1950s there were many opportunities to learn about our discipline from individuals other than the tenure-track anthropology faculty. After World War II the Missouri River Basin Surveys (RBS) project was organized and headquartered in Lincoln. At the time I was in college, some of the RBS personnel were still housed with the Anthropology Department in the basement of Burnett Hall. Later they all moved to offices on "O" Street. I had the opportunity to take a night class in American Indian ethnology with Donald Lehmer (Wood 1976). Some students were not delighted by his sharp wit and sarcasm, but I was amused. Later, Lehmer was very helpful to me in wrestling with taxonomic problems in Plains archeology. Robert Stephenson (McNutt 1995) was also helpful and patient, especially regarding my tirades concerning the Midwestern Taxonomic System. Franklin Fenenga was a walking bibliography and encyclopedia (Butler 1995). Furthermore, he was always willing to talk to students. More than once I hit him up for a list of references when writing term papers. G. Hubert Smith, always dressed as a "Dapper Dan," contributed to my developing interest in historical archeology (Woolworth 1974). He was also knowledgeable about European archeology and perhaps influenced me in that regard. Other RBS personnel from whom I learned a great deal included Richard P. Wheeler (who produced useful check-lists on ceramic and lithic typologies) and Robert W. Neuman (a veteran of many Smithsonian digs in North Dakota, South Dakota, and Kansas). Both of these individuals exhibited broad knowledge and whimsical wits.

Meanwhile I was pursuing a geology major, and my advisor was C. Bertrand Schultz, a vertebrate paleontologist and director of the University of Nebraska State Museum. Fortunately I was largely oblivious to the fact that Schultz and Champe were not exactly cordial colleagues. One ingredient working in my favor was the fact that Lincoln was still a pretty small town in those days. My parents were well acquainted with Schultz and his wife, Marian, through their mutual memberships in the local chapter of the American Interprofessional Institute. John Champe and my father had known each other since the late 1920s, when Champe was in the insurance business. Indeed, they had worked together on a case that involved an insurance policy and a probable murder. My father also knew Flavia Waters Champe, because they had been chemistry class lab partners at Lincoln High School. Incidentally, as many people knew, Flavia (a former professional ballet dancer) contributed largely to the Champe's family income. Referring to John's home office and her own dance studio, Flavia used to joke, "John has his kiva, but I have the cash [sic] pit." In essence, these social networks and other factors resulted in my being able to largely avoid inter-departmental barbs and to receive the best of what both Schultz and Champe had to offer. I took two exceptionally good vertebrate paleontology courses, including extensive hands-on labs, with Schultz, who had initiated the research program at the Medicine Creek Paleoindian sites (Schultz and Frankforter 1948). Benjamin Burma's invertebrate paleontology course was also fine, although it was probably the most difficult course I ever had. My biology background at that time was so poor that I had to translate the text before I could study it. One of my other favorite geology professors was T. Mylan Stout, whom students irreverently referred to as "Smilin' Mylan, the beaver man," since he specialized in Pleistocene rodents. Stout was perhaps the epitome of the erudite professor; he dressed in conservative three-piece suits and actually read the latest journal articles written in French and German. My geomorphology class with Stout was enjoyable and most useful in pursuing archeology.

During the summers of 1954 and 1955, I returned to the NSHS archeological crews in South Dakota as Kivett's field assistant. Most of our work was at the Crow Creek site (39BF11) in the Fort Randall Reservoir, south of Fort Thompson (Kivett and Jensen 1976). Here again we had a dry tent camp. Running water meant running to Fort Thompson with galvanized garbage cans for water from the village pump, but we had the luxury of a

propane gas refrigerator that ran most days unless it was windy. By this time my skills at latrine digging had progressed to the point that I was the field party's official sanitary engineer. The kaibo at Crow Creek (otherwise known as "the dapper crapper") was positioned just over the edge of the steep bluff, looking down some 60 feet to Wolf Creek. From a sitting position, one had an absolutely spectacular view out over several miles of the Missouri River bottoms. (We assumed there was nobody out there to have spectacular views of our bottoms!)

Our crews included not only young men from Nebraska but also Yankton and Yanktonai teenagers from Fort Thompson (Figure 8). Sympathetic to their cultural values, Kivett prohibited any crew members from walking to the top of the hill north of the site, where there were remains of historic eagle traps and scaffold burials. These policies of inclusion and sensitivity were common sense, but they were not uniformly practiced in the 1950s. Furthermore, there was still blatant discrimination in Chamberlain, where certain restaurants would allow Indians only at their take-out windows. The Chamberlain municipal swimming pool also did not welcome Indians. One weekend I had taken some of my Yankton-Yanktonai friends to the pool, and we were getting a cool reception. I pulled out my Red Cross Swimming and Water Safety Instructor Certification Card and informed the ticket seller that these guys were my students. We were allowed admission to the pool and made our point, but after that we decided Lake Bedashosha was less hassle — it was free, and it was closer to the Crow Creek site. My ploy had worked well enough, however, that I used it again several years later for an African-American crew member in Oakland, Nebraska.

During the 1954 field season Mott Davis and a contingent from the University of Nebraska joined the NSHS crew. Davis took it upon himself to awaken the field party each morning by blowing a rather obnoxious reveille on his bugle. One morning a field crew member put a stop to this matter by stuffing a cork into the bugle to muffle its sound. My lips are sealed, of course, regarding the identity of the bugle corker. It was also during the 1954 field season that an inspection group from the NPS arrived to spend the night at Crow Creek. John Corbett (Chief Archeologist for the NPS), Paul Beaubien (Regional Archeologist of the Midwest Region of the NPS), and several others were along on the tour of sites being investigated by the RBS crews and NPS collaborators along the Missouri River. After working all day in the hot sun, our crew prepared to drive over to Lake

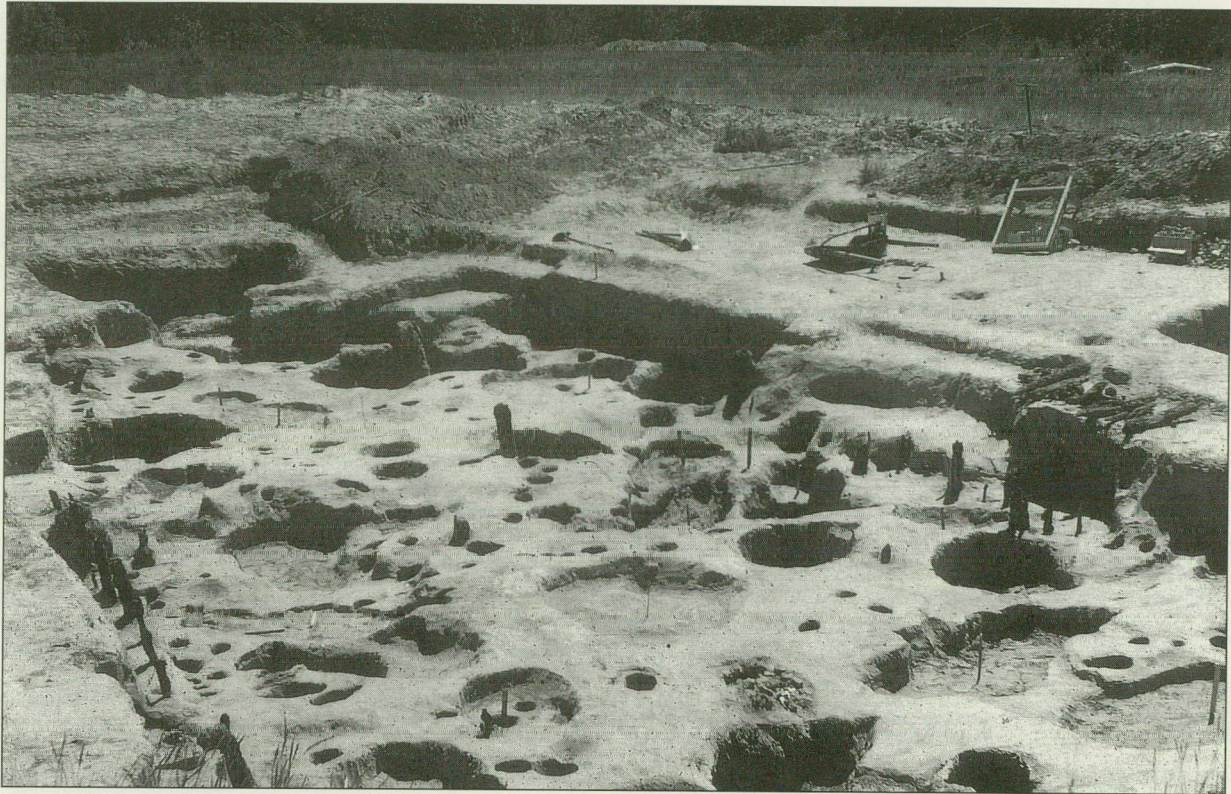


**Figure 8.** Native American men on the NSHS crew at the Crow Creek site (39BF11) in 1955. Left to right: Wayne Shields, Mark Shields, and Billy Picotte.

Bedashosha to clean up and relax. Beaubien decided he also wanted to go swimming, so I lent him an extra swimming suit I had brought to the field. As I'll mention later, Beaubien always remembered me as the "guy from Crow Creek with an extra swimming suit." Meanwhile, to celebrate having the NPS dignitaries in camp, we had purchased ice cream for the evening dessert. This was a rare luxury, since our propane gas refrigerator did not keep foods frozen for any amount of time. When the KP on duty that night offered the NPS officials seconds on ice cream, Beaubien said, "No thanks," and covered his bowl with his hand. But his action was too late; the KP had already deposited an extra large scoop of ice cream on the back of Beaubien's hand. (I thought I saw Kivett's eyes glaze over as he may have imagined that his expected NPS funds would be cut for the following season.) That night there were other strange happenings at Crow Creek. When the crew, Kivetts, and NPS officials awakened the next morning, they viewed something high on the hill that had not been there the previous evening: a 20-foot flag pole from which waved the resplendent Crow Creek site flag with a black crow and a blue S-shaped creek. I know the names of the

persons who perpetrated this act, but I am not at liberty to divulge them.

The Crow Creek site was a large, multi-component village (Figure 9). The complex defense system there included three fortification ditches, one of which was clearly bastioned. In certain places midden up to 7 feet thick lay over trash-filled storage pits. Of the two principal occupations, the lower Crow Creek component had long rectangular houses and was part of the Initial Middle Missouri tradition. Stout timbers had been preserved and provided a source for dendrochronological as well as radiocarbon dating. The upper Wolf Creek component had short rectangular or squarish lodges and was part of the Initial Coalescent tradition. The rich inventory of artifacts and features kept me busy with records. Much time was also spent in mapping. Kivett went to extraordinary efforts to provide photodocumentation of the site, resulting at times in somewhat of a circus atmosphere. If the roof of a vehicle did not afford an angle high enough to photograph an exposed house floor, then an extension ladder was erected with the assistance of three guy ropes (Figure 10). Obviously these were the days before OSHA guidelines! During our two summers at



**Figure 9.** Excavation of House 6, associated with the lower component at the Crow Creek site (39BF11) in 1955. (Photograph by David Gradwohl)

Crow Creek, we had discovered some evidences of warfare and foul play. Fortunately we had no idea of the proportions of the prehistoric massacre that actually took place at Crow Creek. That investigation awaited some years for Larry Zimmerman and his colleagues, who had more resources and better expertise to deal with the problem (Zimmerman et al. 1981; P. Willey and Emerson 1993).

Our recreational outlets were minimal at Crow Creek. Driving to Fort Thompson to replenish our drinking water supply was, of course, an excuse to buy popsicles or visit John Saul, a Yanktonai elder who carved wooden figurines for his grandchildren and for those of us archeologists who commissioned him to do so. Occasionally we foraged for wild berries or rattlesnakes. The latter were skinned, cooked, and eaten (with more than a little trepidation). At night one could read by the light of kerosene lanterns, if the bugs were not too profuse. There were, of course, the proverbial evening campfires associated with burned marshmallows and sing-alongs, accompanied by a guitar or ukulele. After work on most days, we went to Lake Bedashosha to bathe and swim (Figure 11). On one occasion there we met an Episcopal minister from Fort Thompson. He invited

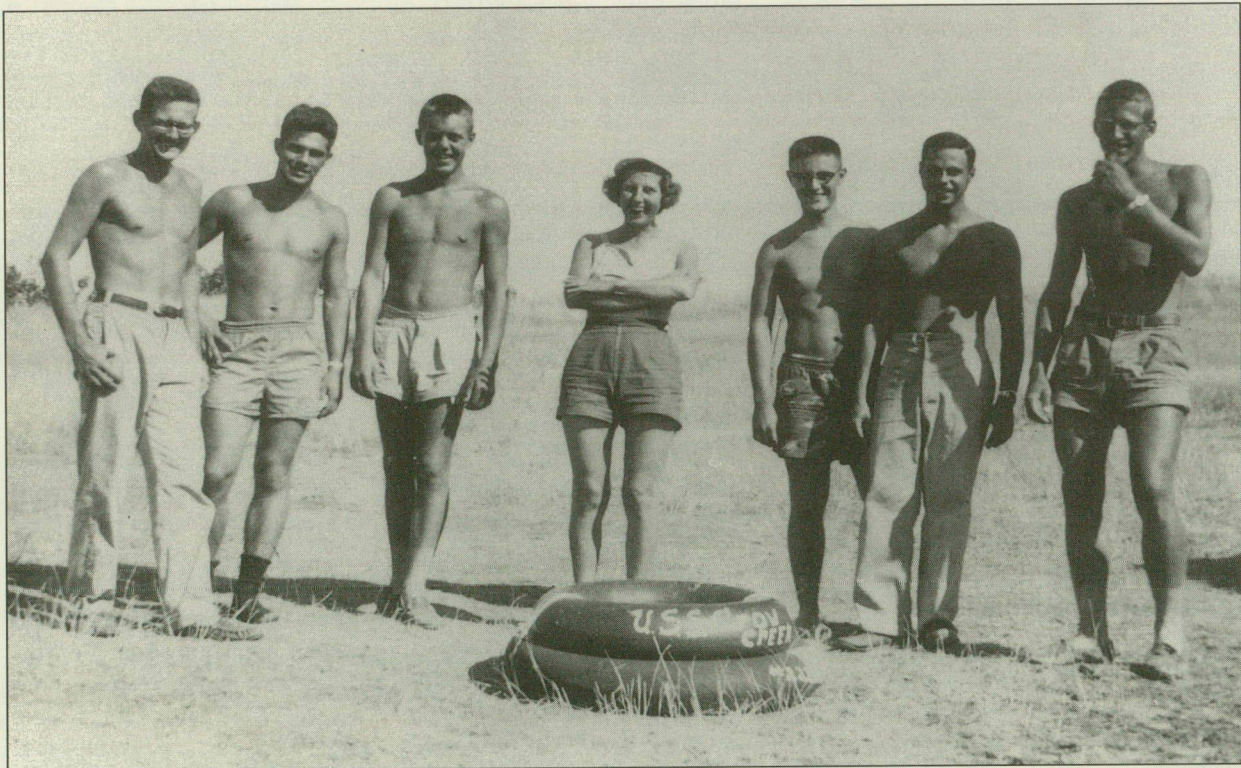
us to attend Sunday morning services at his church. We did so on several occasions. A couple of the crew members went to take Holy Communion; I tagged along as an ethnographic observer. I was intrigued with the eloquent sermon, delivered in both Dakota and English, and the bilingual prayer book. As I studied the unfamiliar Siouan words, I wondered (and still do) if the term "Wakantanka" really does translate specifically as "God" in the Judeo-Christian world view. Regardless, we learned to sing "For The Beauty of the Earth" and several other hymns in Dakota. On Saturday nights we were allowed to drive down to Chamberlain, where the main activity consisted of slowly walking around main street clockwise for an hour or so and then reversing the direction for the next hour. Far more productive were our visits to Carlyle Smith's University of Kansas (KU) field crew, also excavating near Fort Thompson. Smith's excavations, of course, were interesting. Even more exciting, however, was the fact that women were included on the Kansas field parties, giving real meaning to the word "party." In particular I recall Dena Ferran [Dincauze] and Ann Stofer [Johnson] who graced the KU crew. In those days women (other than Carolyn Kivett) were not permitted on the NSHS



**Figure 10.** Marvin Kivett on ladder, photographing features at the Crow Creek site (39BF11) in 1955. (Photograph by David Gradwohl)

crews nor were they, as far as I know, allowed on any of the Missouri River Basin Survey field parties except one. Our crew paid a visit to that exception, namely, the RBS excavations at the Cheyenne River site (39ST1), supervised by Waldo Wedel, who was accompanied by his children and wife, Mildred Mott Wedel (Gradwohl 1995). Ultimately the Wedels became close colleagues and friends as I pursued Plains archeology. Mildred's expertise in the ethno-historical approach to archeology had a considerable influence on my current work in ethnoarcheology as it pertains to the study of ethnicity and material culture.

During my junior and senior years I had the pleasure of attending Plains Conferences, which were at that time small and rather intimate gatherings held over Thanksgiving. I was honored by being selected to push slides, one at a time, through a projector, while presenters gave their talks. There were — unlike now — no concurrent sessions, plenty of time for questions and heated debates, and lots of social time during which lowly undergraduate students could meet and talk to graduate students and professional archeologists from other universities and institutions. I particularly recall a joint paper given by



**Figure 11.** 1955 NSHS crew at Lake Bedashosha. Left to right: Floyd Urbach, Mark Reimers, Larie Holmes, Caroline Kivett, John Paul Thompson, David Gradwohl, and Mark Blum. (Photograph by Marvin Kivett)

the Champes regarding the Matachines dances of the Southwest (F. Champe 1983). John (a tape recorder jock) played music and discussed some anthropological aspects of the ceremonies, while Flavia (probably the most graceful person I ever saw move) performed some of the dance steps. I was especially impressed that a husband and wife could work together as a team on research projects. These collegial conference experiences were not only fun but provided networks that continue to be useful today.

Several informal sodalities were also important in my enculturation as a nascent anthropologist. The first was a group known affectionately as the "Beaver Patrol" (see also Wood 1995:2). By the time I entered the scene, the primary Beaver Patrol players were W. Raymond Wood, Raymond S. Price, Jr., Mary Kiehl [Rusco], and Thomas M. Newman. Members of this group exchanged useful bits of information about department intrigues and the latest discoveries in archeology. They also served as the nucleus for parties that expanded to include selected members of the anthropology faculty, RBS personnel, and employees of the University of Nebraska State Museum and the NSHS, in particular Iris Daugherty [Nunley] and Sally Johnson [Ketcham]. At these parties there was a lot of shop talk, which was enlightening. Also, since Iris, Mott, and Ray Price all played guitars, the fare was often musical. At that time I played the ukulele moderately well but was enticed into buying a guitar, admittedly a Stella, which was only one step up from a cigar box and rubber bands. I never really mastered the instrument but did jam at some of the Beaver Patrol functions.

The second informal institution in the basement of Burnett Hall was a Coffee Klatsch that convened mid-morning and mid-afternoon around a table in the corner of the anthropology lab. The group consisted mostly of anthropology faculty and students, a few RBS personnel, some sociology professors, and an occasional historian or political scientist. I had not yet become addicted to coffee, but I often hung around the Klatsch to absorb some of the information and gossip being dished out. I remember learning from Jack Roberts the important adage that "anthropology begins at home." He would on occasion take up the "society page" from the *Journal* or *Star* newspapers and proceed to analyze, in hilarious terms, the social structure of Lincoln, where he and I had both grown up. In some ways I learned as much outside the classroom as in.

During 1954 and 1955 the University of Nebraska was producing a television film series, entitled "The Great Plains Trilogy." This award-winning

precursor to educational videos included paleontology, archeology, and history and starred Schultz, Davis, Kivett, and the historian James C. Olson. Some students played cameo appearances in the films. Price, simulating an ancient Native American hunter, got to use an atlatl to launch a spear across the TV set toward an outline drawing of an antelope. His spear impressively struck the beast! I was essentially a prop during one of Davis' segments, but I had a speaking part. Posing as a laboratory worker, I sat at a table on the set with a sandbox and a bunch of potsherds. When Davis asked me what I was doing I responded, with precious little thespian talent, "I am gluing together potsherds."

During my senior year at Nebraska, I had the opportunity to work on an honors research project under Champe's direction (Figure 12). My topic was "A Study of Non Ceramic Artifacts from Middle Ceramic Sites in the Harlan County Reservoir, Nebraska." I was assigned a couple of lab tables and given the stone and bone tool inventories from several sites that Champe's crews had excavated in Harlan County. Given my vertebrate paleontology



**Figure 12.** David Gradwohl, working on undergraduate honors project in the anthropology lab, Burnett Hall, University of Nebraska in 1955. (Photograph in author's collection)

courses under Schultz, I was particularly eager to analyze the bone artifacts. I went through all the faunal remains and found ad hoc bone tools and broken fragments of implements that had not been recognized in the catalogue. This project was the basis for my first paper at a professional meeting, namely, the Nebraska Academy of Sciences. I believe the title of my paper may have been longer than the abstract. My presentation on 3 x 5-inch index cards was more in the style I had developed as a member of the debate team than that of an archeologist.

My other project, almost a cottage industry, during my senior year was applying for fellowships and graduate schools. Champe was urging me to stay at Nebraska for an M.A. degree and even hinted that I could have a teaching assistantship. I found that possibility intriguing, since life in Lincoln had become very comfortable and Hanna Rosenberg, the woman about whom I was getting serious, still had two years of undergraduate work to complete at Nebraska. Davis and Roberts, on the other hand, were imploring me to go elsewhere. In essence their message was, "Kid, you grew up in Lincoln and have completed college. It is time to get out of town and broaden yourself." I also must admit I was not unaware that Champe, the consummate perfectionist, was extremely demanding of his graduate students. Given my interests and the alma maters of my professors, I applied to Harvard, Yale, and Columbia. I was accepted at all three institutions. I was also given a Woodrow Wilson Fellowship and accepted at the University of Pennsylvania to which I had not directly applied. Among some other applications I had put in my bid for a Fulbright Fellowship to Britain. The Fulbright application form had two spaces for your first and second choices of British universities, so I naturally filled in Oxford and Cambridge. Then I drew a third line, wrote in Edinburgh, and mailed off my completed application. Later in the spring those of us in Champe's morning class heard some raucous noise out at the Coffee Klatsch. Champe looked pained and asked me to close the classroom door. After class one of the sociology professors rushed into the room and thrust the *Daily Nebraskan* newspaper into Champe's hands. Meanwhile I was packing up my books to head out for lunch. Champe burst out laughing, shook my hand, and exclaimed, "All that, and money too!" It seems the newspaper announced that I had been selected for "stud" in Britain. The supposed misprint really did not represent the kind of "fellowship" Mr. Fulbright had in mind. Nonetheless, I was off to Edinburgh with a good deal of excitement.

Since I had never been on the east coast, I decided to visit Harvard, Yale, Columbia, and the University of Pennsylvania en route to Scotland. The size of New York City and Columbia impressed me as too large, although my father had received his Masters of Law there. Yale's Department of Anthropology grabbed me as too small. Penn was quite attractive, and I had a fascinating interview with Loren Eiseley — a fellow native Nebraskan, Plains archeological field worker in the 1930s, and author of *The Immense Journey* and other notable literary works. Penn's requirements for the obligatory Master's degree, however, seemed somewhat rinky-dink to me. At Harvard I had a cordial meeting with Gordon R. Willey, who had excavated throughout the Americas, done broad-scale area syntheses, proposed a theoretical and methodological scheme for New World prehistory, and was working on the history of American archeology (Willey 1966; Willey and Phillips 1958; Willey and Sabloff 1974). I also took an immediate liking to the Peabody Museum and Cambridge.

During my subsequent year at Edinburgh University, I took classes with R. J. C. Atkinson and Stuart Piggott in prehistoric archeology, Michael Gough in classical archeology, and Kenneth Little in social anthropology. The experience was fulfilling but largely irrelevant for the purposes here except that I had the opportunity to dig in the plains (actually the Salisbury Plains) with Piggott and Atkinson at Stonehenge. Serendipity came into play, however, in that Piggott was a visiting professor at Harvard the semester I took my Ph.D. general exam. On my examining committee Piggott took the place of the considerably (and rightfully) feared Hallam L. Movius, who was conveniently in Europe at that time.

My decision to pursue graduate work at Harvard was based on two primary factors. First, Harvard offered specializations in both North American and European archeology. Second, the Peabody Museum there provided opportunities for hands-on learning that I had taken for granted at the NSHS and University of Nebraska State Museum. Initially I found Harvard a bit daunting, since Nebraska hay-seeds seemed to keep falling from my cuffs. Fortunately a few Plains people helped my transition. I speak specifically of former Beaver Patrol members James H. and Dolores A. Gunnerson in addition to John C. Ives from Iowa. They offered both personal and professional assistance in my learning the ropes at Harvard. J. O. Brew was then director of the Peabody Museum, and he and his wife, Evelyn, extended their hospitality to me. Although Brew's

specialty was Southwestern archeology, he was a consistent participant in the Plains Conference and knew as much or more about the Plains than other anthropology faculty members at Harvard (Woodbury 1990). My major professor was Stephen Williams, whose principal interest was Mississippian cultures of the Southeast (see Williams 1956 and Stoltman 1993). Williams and Willey, both superb teachers, influenced my thinking a good deal, particularly in regard to taxonomic problems, settlement patterns, and the nature of other sociocultural reconstructions. Both men were charismatic enough that, for a time, I contemplated switching either to Southeastern or Meso-American archeology. But my soul was still in the Plains.

Military service interrupted my graduate work at Harvard. I was drafted into the U.S. Army at the end of my first year at Harvard, sent to basic training at Fort Hood, Texas, and then to Crailsheim, Germany, for a year and a half. Career-wise these were lost years. In the meantime, however, I had married the aforementioned Hanna Rosenberg, who had completed her degree in social work. She also took an introductory anthropology class with Champe in terms of her general interests and, from my point of view, to learn the necessary jargon so she could eventually type my term papers and initial dissertation drafts. To celebrate our engagement Caroline and Gus Kivett had thrown a party and invited the anthropology faculty, Beaver Patrol, and RBS, NSHS, and University of Nebraska State Museum staff. Among other things Hanna and I were presented matching gold Marshalltown trowels, marked "his" and "hers." We were married in Lincoln while I was on leave after completing six horrid months of training at Fort Hood. We then departed for Europe on separate ships, since Hanna was an "unauthorized" civilian dependent, and my orders were to sail on a military transport with the other troops of the First Medium Tank Battalion 37th Armor Regiment 4th Army Division. Not the most ideal honeymoon arrangement! But the good news was that we had a year and a half during which we took leaves to visit archeological sites and museums in Germany, Austria, Switzerland, Italy, France, the Low Countries, and Britain.

Upon my release from the army in June of 1959, I rushed back to Nebraska to take over the NSHS excavations at Logan Creek (25BT3), a stratified multi-component Archaic tradition site (Kivett 1958, 1962b). Kivett had set up the field camp and started the crew excavating along the exposed cutbank of the stream. Upon my arrival at the site, Kivett gave me about a 10-minute orientation to the excavations,

handed me the field records and the keys to his trailer, and took off in a cloud of dust for Lincoln. Excavating at this site was especially interesting since the diagnostic medium-sized, side-notched projectile points were associated with charcoal dating between 4,000 and 5,000 B.C., a good deal earlier than previously guessed. Joining us for a few days was the University of Nebraska field party under the direction of Thomas A. Witty, Jr. (eventually the State Archeologist for Kansas), whom I had known not only at the University but also from our days at Lincoln High School.

Following that field season, I returned to Harvard for three years to complete my course work and begin writing my dissertation on Nebraska phase settlement patterns (Gradwohl 1969). I was following in the footsteps of Fred H. Sterns (also a Nebraskan), who initially posed these questions in his Harvard dissertation some 50 years earlier. The Sterns collections at the Peabody Museum served as useful comparative data for my research materials from the Weeping Water Creek locality south of Omaha. The data base for my dissertation had been collected by Dr. L. N. Kunkel, a physician and professionally-oriented amateur, in the 1920s. My graduate committee consisted of Stephen Williams, Gordon Willey, Philip Phillips, and J. O. Brew. Outside my academic committee Waldo Wedel, whom I had met when I was an undergraduate student and interacted with at various Plains Conferences, was very helpful as I completed my dissertation and revised it for publication. Even though I had been critical of the use of the Midwestern Taxonomic System and had questioned the reliability of earlier site survey data vis-à-vis the study of settlement patterns, Wedel's critique of my work was gracious and constructive. I will admit that I did not know then that this outwardly appearing quiet and mild-mannered man was capable of rendering stinging reviews. Over the years Wedel's work had a profound influence on me, as it did on all Plains archeologists (Gradwohl 1996).

By 1962 Hanna and I had produced our first child, and I had to face the fact that we would be better provided for if I had a regular salary rather than intermittent academic fellowships. So I started looking for a job. Fortunately I began my search just as the "window of opportunity" was opening for academic positions in anthropology. Now that that window has been lowered again, I am somewhat embarrassed to observe that in the early 1960s there were more positions available in anthropology than there were qualified people to fill them. I actually had some choices to consider and have had, on certain dreary

occasions, to remind myself of that fact. One of several offers I had was from the Department of Psychology, Sociology, and Anthropology at the University of Nevada in Reno. I had sent in my application materials and, much to my surprise, soon received a contract back ready to sign. They had not even offered me an interview trip, and I was too untutored to request one. It frightens me today to think, from both Nevada's standpoint and mine, what might have happened if I had just signed that contract and headed west. In the meantime, however, I had been invited to Ames to interview for a position open at Iowa State University (ISU). Given Hanna's and my desire to be closer to our families and my prairie-plains research interests, ISU seemed more attractive. Iowa State also offered a slightly higher salary, although it must be said that the amount of money then buying fresh Ph.D.s would hardly purchase a home computer system today. Nevertheless, I feared that personal factors might be overshadowing professional considerations, so I made an appointment to discuss the situation with Brew. He thought both opportunities sounded good but, in an avuncular fashion, observed that my personal ties and established research interests were indeed important matters in my decision. So I had pretty much made up my mind to accept ISU's offer by the end of our conversation. As I left Brew's office, however, he smiled and said, "Good luck, Gradwohl, we need one of our men in Idaho!" I stopped momentarily and wondered if I should go back into his office and replay our discussion, but I did not. I was naturally relieved when I learned that Brew had indeed sent my letter of recommendation to Iowa and not Idaho (or, for that matter, Ohio). How did I ascertain this fact? Well, Brew's secretary presumably by mistake sent me a carbon copy of the letter of recommendation. I was, of course, pleased to know that Brew had written very favorable things in support of my application. On the other hand I was surprised that he had also said flattering things about my wife. It had never occurred to me to mention Hanna's pulchritude and many other fine qualities on my vita.

It was thus in 1962 that I ended up accepting an offer from ISU to become the lone anthropologist in a joint Department of Economics and Sociology, that is, Agricultural Economics and Rural Sociology. This is where the second part of my essay title comes into play. On most days I swear that I would have neither the courage nor naiveté, to do it all over again. I was the first person specifically hired to teach full time in anthropology at ISU, and I was the University's first archeologist. The position was necessitated by the

fact that "Introduction to Cultural Anthropology" had been made a requirement for all students in the College of Home Economics. This meant huge auditoriums full of students, many of whom would rather have been taking classes in sewing, cooking, and raising children. I prepared each lecture as if it were a debate case and even anticipated resubstantiation arguments for the rebuttals I expected from the students, but opposition rarely occurred. Part of my job offer was the opportunity to teach a "specialty course" of my choosing. I chose "North American Archaeology," knowing that none of my potential students would have any prerequisites for the course. Furthermore, there was no laboratory, no museum, no teaching collections, and I had to share a small office with a sociologist. All that was the bad news. The good news was that I had a challenge because the slate was pretty much clean. Fortunately I was able to borrow a cigar box full of artifacts from Gus Kivett, so I could show ISU students what potsherds and projectile points were. Ultimately we were able to hire more anthropologists, expand the course offerings in our discipline, establish the ISU Archaeological Laboratory, begin a summer field school in archeology, build up teaching collections, construct some museum display cases, inaugurate a separate undergraduate major in anthropology, build a Master's program separate from sociology, and help establish an interdisciplinary American Indian Studies Program. Over the years the anthropology program attracted some fine students, and we were able to develop a few capstone courses. My favorite was a graduate seminar, entitled "Cultural Continuity and Change in the Prairie-Plains." This small class met at our house. While I intellectually pestered the students, Hanna pampered them with beverages and home-baked treats. Eventually a Department of Sociology and Anthropology broke off from Economics. I served as the first Coordinator for Anthropology. The final liberation of the present Department of Anthropology at ISU was approved in 1990.

In the matter of establishing a research and teaching program in archeology, serendipity again appeared in the form of Wilfred D. Logan. I first met Logan at a Plains Conference in Lincoln, when I was still an undergraduate student there. Sometime earlier my parents had visited Effigy Mounds National Park, where Logan was serving as the facility's first Park Archeologist (Thiessen and Wood 1992). Logan told me some years later that my parents stumbled upon him in his temporary lab building, asked dozens of questions, stated that their son was majoring in anthropology at Nebraska, and, upon ascertaining

that he would be attending the Plains Conference, invited him to come for dinner at their home while he was in Lincoln. By 1962 Logan was an archeologist at the NPS Midwest Region office in Omaha. Even before I left Harvard, Logan wrote to me, stating that he heard I had accepted the job in Ames and that the NPS had funds for salvage archeology along the Des Moines River. I also heard from Paul Beaubien, who remembered borrowing my swim suit at Lake Bedashosha and was still with the NPS. On the basis of his previous work in northeast Iowa, he, too, encouraged me to get into the relatively unexplored central Iowa sites with NPS monies. Today, of course, there are quite different bidding procedures for contracts in CRM!

In this manner, however, ISU made its debut into field archeology in 1964. The instrument was a cooperative agreement with the NPS for salvage operations in Red Rock Reservoir along the Des Moines River south of the city of Des Moines. ISU's summer field school in archeology was established at the same time (Figure 13). We left for the field with brand new field equipment, permission to inhabit an abandoned farmhouse as a field headquarters, and written authorization to move an extra outhouse onto the premises to accommodate the large crew.

I soon found out that graduate school had not dealt with a number of matters archeologists face in the field. First, I had not really been prepared to engage in long negotiations for permission to survey or excavate at specific sites. Much of the land in the reservoir area was still privately owned. What did private ownership have to do with contracts, research design, and site sampling? I soon learned that my goals would be reached by foreplay palavering about crops, the weather, and football. Second, I was not prepared for the questions farmers often asked me, in the course of palavering, about my views on evolution, religion, and other personal matters. One man asked me whether the sites we would be digging on his land were from the time period before or after "the flood," meaning, of course, the one in the Bible. I answered, "Before the flooding of Red Rock Reservoir" and quickly changed the subject. One woman pointedly asked me my religion. When I answered "Jewish," the woman's eyes widened and she exclaimed, "Well, you sure don't look Jewish." Along those lines, however, another farmer paid me perhaps the highest compliment of my life. In the process of listening to my request to dig on their land, two farmers (who were brothers) invited a student and me up to their house for coffee. We accepted the



**Figure 13.** Iowa State University archeological field school at the Howard Goodhue site (13PK1), an Oneota village, in 1966. John D. Reynolds, field assistant, in foreground. (Photograph by David Gradwohl)

offer, but I had to return to our car to fetch some maps. I subsequently walked up to the farmhouse to meet the others. The student later reported to me that the younger farmer had jabbed his brother and said, "I bet this is the first time you've fixed a cup of coffee for a university professor." Referring to me, the older brother retorted, "Well, he sure don't act like one!"

A third area that I had not anticipated involved questions and declarations of amazement that women were working on our crew. More than one of my professional colleagues asked, "Won't you have trouble with women in the field?" My answer was, "None that we don't already have on campus." Several farm men commented, "Boy, I wish I could get my wife to work like that," apparently oblivious to the fact that milking cows, slopping hogs, driving tractors, preserving food, etc. constituted work. The *Des Moines Register* took a great interest in our project. They sent a woman reporter in high heels down to write an article, headlined "The Women Who Work At Red Rock," which appeared on what used to be called the women's "society page" (Figure 14; *Des Moines Register*, July 26, 1964). I think this may have been the first (and also the last) time Plains archeology has been featured on a newspaper's society page. The reporter recorded the kinds and amounts of food we ate and asked the female crew members what it was like working for Gradwohl out in the hot sun. They answered, "He's absolutely relentless." The newspaper article also included a photograph of Hanna and our two oldest children. Since the reporter neglected to note that my wife has a first name, the photograph caption reads, "Mrs. David Gradwohl, whose husband is director of the expedition seeking ancient objects at Red Rock Reservoir area, manages to look fresh and pretty, although her family is living under crowded conditions in [an] old farmhouse serving as headquarters for crew and living quarters for some. She is shown with daughter Jane, 11 months, and son Steven, 2½." We all laughed at the article because that day was the only time Hanna wore a dress all summer. This photograph, however, is historic in a number of ways. Steven is now a physician at the University of Michigan, and Jane is a psychology professor at Stonehill College. Our third and last child Kathryn was not yet on the scene. She was born two days after we returned to Ames from the 1966 field season and is presently a school social worker in Minneapolis, Minnesota. Hanna now has white hair but is still married to me. Considering my allegedly feisty demeanor and the serial polygamy expected of Plains archeologists, that must be some sort of record.



Figure 14. The *Des Moines Register's* coverage of women on the 1964 ISU archeological field crew.

This leads to the fourth problem area in which I had not been tutored, namely, dealing with newspaper reporters. I could write a whole essay on misquoted information and poor page layout. Let me share just one favorite example here: an item that also appeared in the *Des Moines Register* on July 5, 1964, showing, from right to left, a squatting male student pointing to some pottery, a female student stooping over our mail box looking for mail, and me squatting next to an excavated cache pit and looking like I had just vomited into it (Figure 15). The headline above the photos relates to a different article; it reads, "Find 31 Cabins Squatting on State-Owned Land."

A fifth problem about which graduate students should be warned is the ineptness and nastiness of some bureaucratic agencies involved in mitigation. One example will suffice here. It involves the Corps of Engineers and archeological monitoring of construction activities at Elk Rock State Park north of Knoxville, Iowa. Construction personnel blatantly bulldozed through a site that had been staked out for preservation by our monitor Michael Portwood. The following day we summoned personnel from various agencies to resolve the matter. Iowa's Office of the State Archaeologist was represented by Joe Tiffany, who rushed into the field still wearing his necktie. During the course of the controversy at Elk Rock, one Corps official threatened that I should not "bite the hand that feeds me." I responded that I was salaried by the State of Iowa not the Corps, but,

# Find 31 Cabins Squatting on State-Owned Land

## ON RIVERBANK IN N. E. IOWA

By Ed Heins  
Iowa Conservation Commission officials have found an additional 31 private cabins squatting on state land along the Mississippi River in northeast Iowa.

The cabins are located along a 1 1/2-mile stretch of riverbank land between the mouth of the Yellow River and the boundary of the Effigy Mounds National Monument near Marquette, Ia. Twenty-eight of the cabins are in Clayton County and three in Allamakee County.

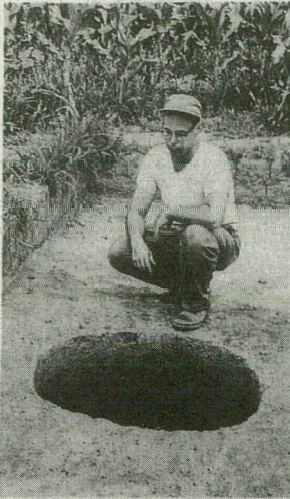
Earlier this summer, the commission staff found 37 cabins and trailers on state land near Harpers Ferry in Allamakee County.

All of them have been there for a long time. Some of the cabins near Harpers Ferry have been there for more than 20 years while several of the vacation units north of Marquette have been there for several years.

Lloyd Bailey, of Des Moines, superintendent of land acquisition for the commission, said:

"These access points to the Mississippi River are irreplaceable."

Wants Them Off  
He favors forcing the private owners to move their cabins off the state land and then develop a public access to the Mississippi River at both points. The commission members will



David Gradwohl, field crew director, looks at an Indian storage pit uncovered in a corn field on the Carl Mohler farm, near Dinreath, in Marion County. The hilltop may have once been the site of an extensive village of Oneota people who lived in Iowa about 1400 A. D.



REGISTER PHOTOS  
Lynn Swanson of Ames, a history and government major at Iowa State University, takes time out after a hard day's work at the Indian site to post a letter in the expedition's mail box along Marion County Road J. Crew members relax after work with their studies or take a drive into Knoxville, about 15 miles away. STORY: Page One.



Ron Senstrand, Ames high school student, points to pottery sherds uncovered at a hilltop village site on the Carl Mohler farm. Such finds are only uncovered by tedious, inch-by-inch excavating with trowel and brush. Artifacts are left in place until they can be correlated with other finds in the same area, thus piecing together a picture of the entire site.

Figure 15. The *Des Moines Register's* coverage of squatting archeologists and squatters on state land.

considering where their other hand was, I would gladly bite the one in my mouth. Unfortunately the site had already been destroyed, and the Corps, with its usual arrogance, refused to honor our written agreement. To make matters worse my department head (a rural sociologist) reprimanded me for not being "more cordial" to the Corps. In my opinion he cared nothing about professional ethics but only about the amount of overhead that I had been bringing in with our large contracts program.

A sixth matter we encountered doing archeology in Iowa had not been mentioned in any of my textbooks or classes. We found an incredible amount of kindness and hospitality on the part of landowners of sites we excavated. As just one example I mention Carl F. Mohler, whose family was losing some 900 acres to Red Rock Reservoir. Mohler not only allowed us to destroy some of his crops but offered to help excavate the site. As though that were not enough, he brought us cold pop and homemade ice cream on hot afternoons and even invited the whole crew to his house for a steak dinner. There were many such courtesies to our crews by other landowners over the years. In addition we have been assisted by members of the Iowa

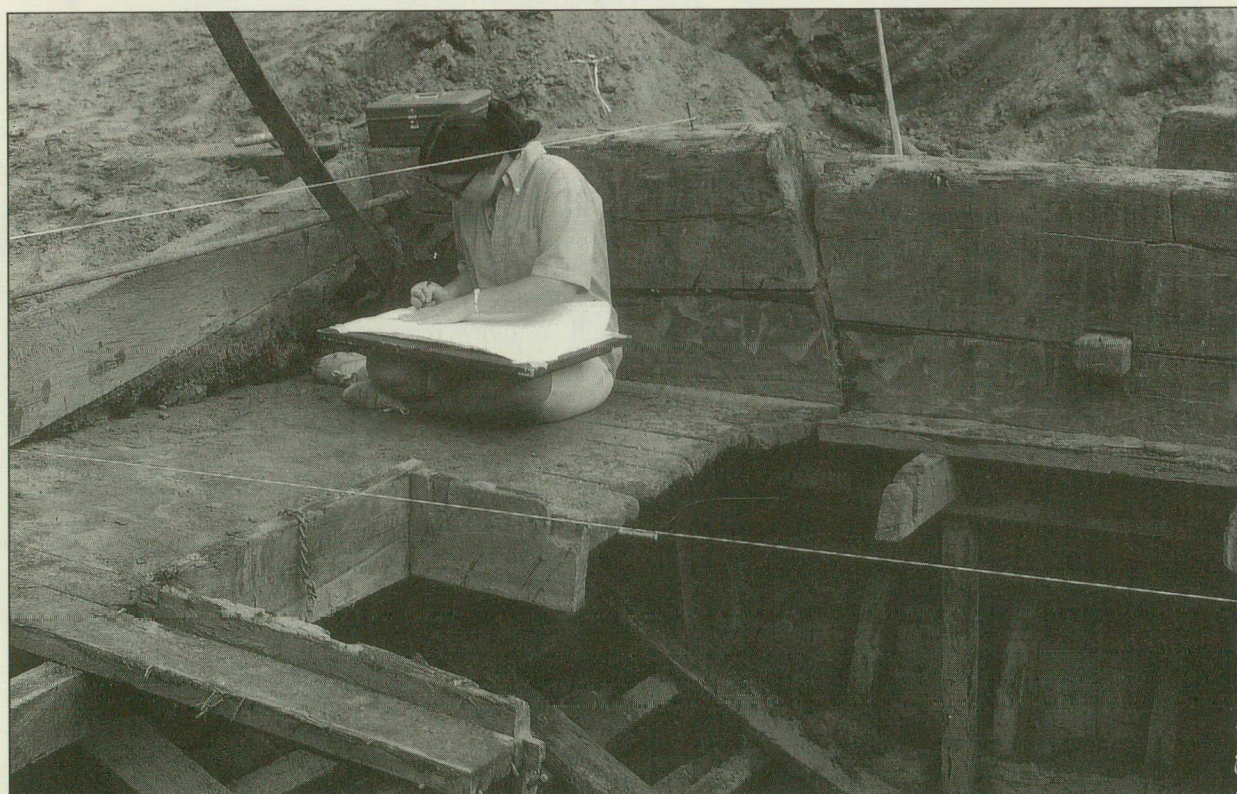
Archaeological Society, who have participated in our excavations and field surveys.

Over the next several decades ISU ran summer field schools and had cooperative agreements with the NPS, Corps of Engineers, and other agencies for archeological survey and excavations in Red Rock, Saylorville, and the proposed Ames reservoirs, as well as other projects. Euphemisms for the work evolved from salvage archeology, to mitigative archeology, to cultural resource management. Our investigative and teaching program in archeology was supplemented by the weekend field exercises I required in the sophomore-level archeology class I taught for 28 years. Nancy M. Osborn [Johnsen] was the co-principal investigator for most of those projects. Central Iowa offered extensive opportunities to explore a wide variety of sites from different time periods. Archaic sites were investigated in Saylorville; Woodland sites, throughout the area; large Oneota village sites, in Red Rock Reservoir; and Great Oasis sites, north and west of Des Moines (Gradwohl 1974). In addition we excavated historic Euroamerican farmsteads, pottery kilns, and a cemetery. ISU personnel also assisted in the excavation of the steamboat Bertrand that sank in the

Missouri River north of Omaha-Council Bluffs on April 1, 1865 (Figure 16; see Petsche 1974). During the 1980s we explored the Buxton townsite (13M010), which was occupied between 1900 and 1925 (Figure 17). Buxton's population was primarily African American, a group essentially ignored in the archeology of the Great Plains (Gradwohl and Osborn 1984). More recently Hanna and I have been investigating intra-group ethnic variability in the Jewish cemeteries of Des Moines, as well as the relationship of material culture and ethnicity on Latvian-American mortuary markers in Lincoln, Nebraska, and American Indian gravestones in Anadarko, Oklahoma (D. Gradwohl and H. Gradwohl 1988; Gradwohl 1997).

During these years I have had the pleasure of working with some outstanding students who have completed M.A. degrees at ISU. This group includes the late John Reynolds (former State Archeologist for Kansas), who was my first graduate student and the first student to write a Master's thesis in anthropology at ISU. His thesis dealt with the Coalport Pottery Kiln and the early Euroamerican ceramics industry in Iowa (Figure 18). Building on that pioneering effort,

theses by Allen Schroeder (now with the Wisconsin State Historical Society) and Barbara Schulte dealt with other stoneware pottery kilns at Moingona and Coal Valley. Nancy Osborn [Johnsen], presently an academic advisor at ISU, wrote her thesis on the Clarkson site, a large Oneota village (Osborn 1982). Steven DeVore (now with the NPS, Denver office) produced an even longer thesis, dealing with an even larger Oneota village, the Cribb's Crib site, which ISU sort of salvaged for free for the NPS via a bureaucratic fluke (DeVore 1990). Archaic, Woodland, and Great Oasis manifestations in Saylorville Reservoir were dealt with in other theses. Robert Timberlake (presently a computer network administrator in Topeka) discussed 14PK149, the multi-component Darr-es-Shalom site. Randall Thies, now at the Kansas State Historical Society, analyzed the materials from Brassica Bench, 13PK251 (Thies 1989). And John Broihahn, now with the Wisconsin State Historical Society, wrote a gargantuan tome on the Blosser and Old Moser sites (13BN125 and 12BN130). Coleen Nutty's thesis, entitled "Cemetery Symbolism of Prairie Pioneers," was published by the *Journal of the Iowa Archaeological Society* and is



**Figure 16.** Nancy Osborn, working at the site of the steamboat Bertrand in 1969. (Photograph by David Gradwohl)



**Figure 17.** 1981 excavations at the Buxton townsite (13MO10), occupied primarily by African Americans between 1900 and 1925. Left to right: Robert Gearhart, J. Charles Lockett, Lori Fisher, Robert Thompson, Olivia Smith, Donna Randall, and Carla Tollefson. (Photograph by David Gradwohl)



**Figure 18.** John Reynolds at the Coalport Pottery Kiln (13MA103) in 1966. (Photograph by David Gradwohl)

widely cited by interdisciplinary scholars in cemetery studies (Nutty 1984). Michael Portwood, now a Ph.D. candidate at the University of Minnesota, designed a computerized data base management system as his Master's project. Kathy Gourley has served the State Historical Society of Iowa as a CRM compliance officer and a public historian; she wrote an important thesis on the ethnohistoric locations of Mesquakie and Sauk villages between 1832 and 1845. Most recently, Lance Foster completed a definitive thesis on Ioway Indian sacred bundles (mostly from the Milwaukee Public Museum), using his extensive "emic" perspective in addition to the "etics" in the literature. He is now completing a second Master's degree in landscape architecture with a particular focus on sacred landscapes of the Ioway Indians. Many more undergraduate majors have gone on from ISU to graduate programs and careers in anthropology in the Plains and elsewhere. Their names are too numerous to list here. But I know who they are, and I am proud of them.

A more recent experience Hanna and I have had in Plains anthropology was during September of 1996, when we were incorporated into the Turtle Clan of the Yankton Sioux tribe at the Mahkato Traditional Powwow in Mankato, Minnesota (Figure 19). We

were adopted by Maria Pearson, an Indian activist, suggesting that Native Americans and Plains archeologists can get along with one another throughout the intricacies of the Iowa Burial Code and the Native American Graves Protection and Repatriation Act.

When I now have flashbacks to my youth, I have no idea why I went off to college clueless as to a major and a career objective in anthropology. The hints were all there early on. I was always intrigued with issues of *National Geographic* magazine to which my family subscribed. I also read all the books by Madeline Brandeis, published by Grosset and Dunlap, Inc., on children from different cultures around the world. I was an inveterate collector of objects, especially invertebrate fossils, rocks and minerals, bird nests, and an occasional arrowhead. These were displayed, museum-like, in glass-fronted bookcases, salvaged from my father's law office. I was a frequent visitor to the NSHS Museum, then housed in the Nebraska state capitol building. Perhaps the artifact that most explicitly foreshadowed my specialization in Plains anthropology is a well-thumbed book from my childhood, entitled *Indians of the Plains*, by Sanford Tousey (1940). I vividly remember reading and re-reading this book and studying the detailed illustrations that were indeed



**Figure 19.** 1996 powwow in Mankato, Minnesota, at which David and Hanna Gradwohl were adopted into the Turtle Clan of the Yankton tribe by Maria Pearson. Left to right: Diane Carlets, Hanna and David Gradwohl, Leonard Wabasha, and Maria Pearson. (Photograph by Nancy Osborn)

heavily influenced by the paintings of Catlin and Bodmer. During the 1940s my family drove north through the Omaha and Winnebago reservations to take vacations in northern Minnesota, where I observed Indian burial mounds and a wild rice harvest ceremony, attended powwows, and interacted with Ojibway Indians who served as fishing guides. These experiences were the bases for class essays and occasional poems (fairly bad) in junior high school and beyond. Although I did not fully recognize the matter as a youth, I was puzzling over the persistence of tradition along with the forces of change. These phenomena, of course, were also very much a part of the particular cultural milieu in which I was raised and the fact that, to some degree at least, I was often an "outsider looking in" along the lines elucidated in Hortense Powdermaker's book *Stranger and Friend* (Powdermaker 1966). All of these indications of my ultimate professional obsessions had been unrecognized disparate parts of my childhood. But they all began to connect that summer at the Oacoma site on what had merely portended to be a seasonal job.

In conclusion, however, I must observe that my career has been marked more by accidents, luck, and serendipity than by design. Throughout all these years, I kept wondering if I had finally channeled my interests enough to satisfy the career counselors. Some 15 years ago, I was shocked by my faculty evaluation in which the department head (a rural sociologist) referred to me as a "localite." Although I could not find the word "localite" in my unabridged dictionary, I thought I knew what my evaluator was getting at. And I took umbrage at the insinuation, because that department head had obtained all three degrees from ISU, located in Story County, where said person was born and grew up. On the contrary I had moved across the Missouri River from my natal state, studied at three different universities, and had lived abroad for three years. I decided that I did not have to apologize for concentrating on Plains anthropology. Indeed I had found my channel, and it is both wide and deep. It extends from Canada to Texas and from the Rocky Mountains to the western reaches of the Mississippi River drainage. The temporal range is at least 12,000 years and includes prehistoric and historic American Indians, African Americans, Hispanic Americans, Asian Americans, and Euroamericans of various ethnicities and national origins. Eventually, I may restrict myself to a single eddy. But until then, I plan to continue moving along various currents, where I will find my many good friends and colleagues, who are also exploring that vast channel of Plains anthropology.

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## NOTES

THE SHEET IS MADE OF PAPER WITH THE WEIGHT OF 100 G/M<sup>2</sup> AND IS DESCRIBED BY THE NUMBER 1000.

# THE PALEOINDIAN LAIRD BISON BONE BED IN NORTHWESTERN KANSAS

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University of Kansas

*The Kansas Anthropologist 18(2):45-58*

*Initial test investigations were conducted at the Laird site (14SN2) in northern Sherman County, Kansas, in 1995, following the discovery of a Dalton-like projectile point eroding from a deposit of bison bone. Excavation yielded a number of flakes and remains of at least two and probably more bison, which appear to be in the channel of an ancient arroyo.*

## INTRODUCTION

The Laird site, 14SN2, located in northeastern Sherman County, Kansas (Figure 1), was discovered in July 1990 by Rod Laird and Dan Busse. At that time bone fragments were eroding out of the site and were exposed on the surface. When Rod Laird was taking a photograph of this exposure, he spotted a projectile point among the bone fragments. The point was recognized as a Paleoindian type of somewhat unusual style for the region.

To reduce attention to the site, which is located near a frequently visited historical marker, the small concentration of surface bone fragments was collected in 1990 and several times subsequently prior to the initial excavation in 1995. The bone scatter was restricted to a small area of about 6 x 3 m in size, with the long axis oriented roughly north to south.

In 1993 Dan Busse showed the site to Jack Hofman and students of the Kansas Archaeological Field School, then working at the Norton site in Scott County (Hofman et al. 1995). In addition to the projectile point, a small flake of Niobrara jasper and a flat cobble of fossilized wood had been found in the immediate area of the bone scatter.

Arrangements were made for initial testing at the site in May 1995 with the support of volunteer workers. Seven days, May 26 through June 1, were spent investigating the site, though work was somewhat hampered by rain. During this time testing was being conducted at the Busse Cache site (14SN1), also located in Sherman County (Hofman 1995).

## METHODS

Investigation began by establishing a grid across the site area. The grid was oriented on magnetic north and was composed of 5 x 5-m blocks, subdivided into

25, 1 x 1-m units (Figures 2 and 3). The units within each 5 x 5-m block were numbered from upper right (unit 1, northeast corner) to lower left (unit 25, southwest corner).

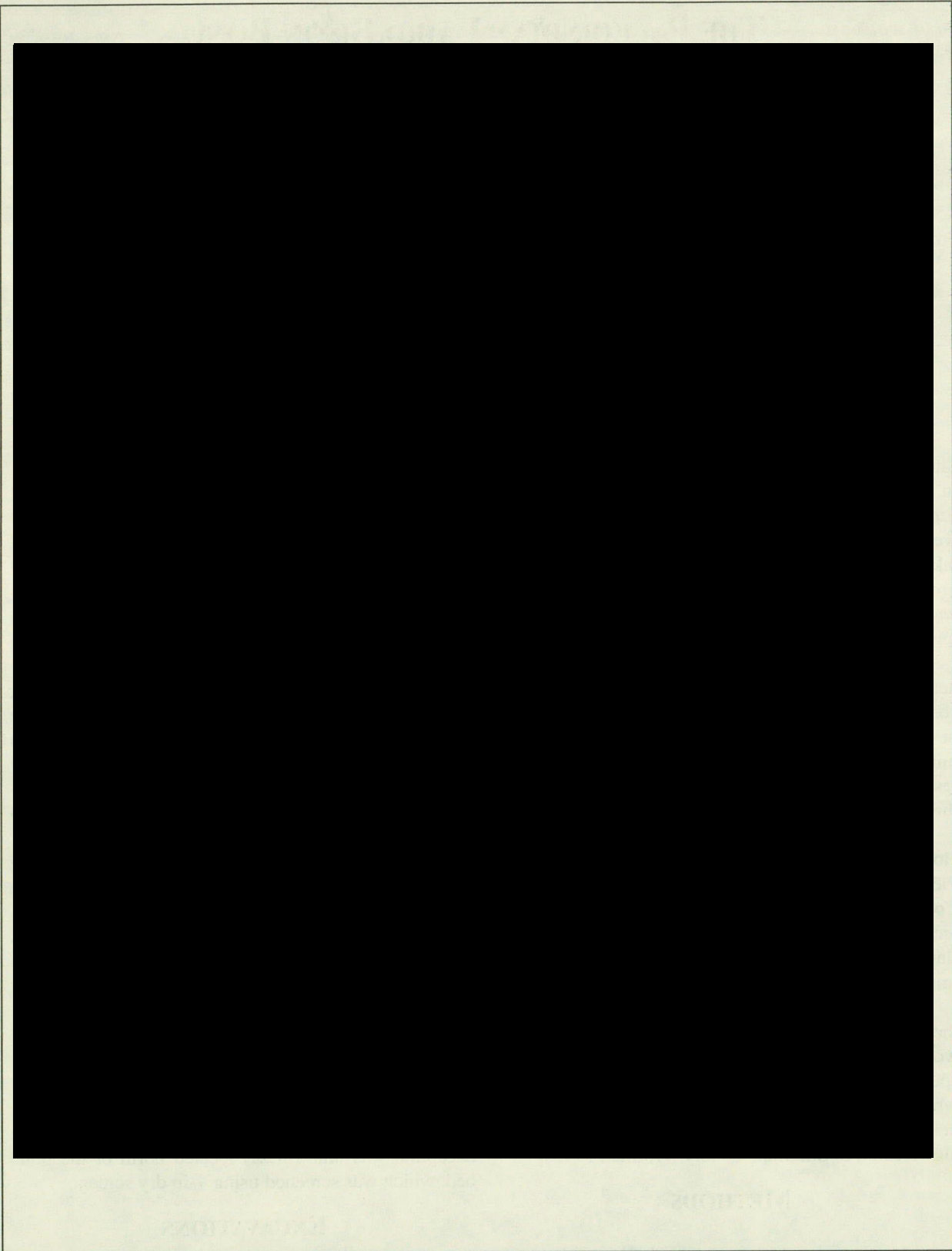
Mapped pieces for each unit were designated with the block, unit, and sequential specimen numbers; e.g., 14E5-10 is the tenth item mapped in unit 5 of block 14E. All pieces in each unit were numbered sequentially as excavated. The blocks were designated by letters west to east and by numbers south to north. Grid points were designated in relation to an arbitrary datum set at 1000 North, 1000 East, with an arbitrary elevation of 90 m given to the grid point 1000N, 900E. A permanent datum was set in concrete at 1000N, 910E and has an elevation of 90.88. This datum is approximately 20 m northeast of the primary surface concentration of bone fragments. All measurements for point-plotted items were in relation to these points.

Excavation proceeded by collecting all surface bone from selected units. Bone fragments larger than 2 cm were mapped in place, as were all lithic pieces found in situ. Information on each mapped item was coded on standardized forms, which include provenience information and specifics about the bones, such as element, portion, condition, size, and treatment (e.g., use of preservatives). Excavation levels were 10 cm in thickness and were designated by their top and bottom elevations (e.g., 99.20-99.10).

All matrix was processed for each unit and level by water screening through  $\frac{1}{4}$ - and  $\frac{1}{16}$ -in wire mesh. The exception was unit 18E21, located north of the bone bed, which was screened using  $\frac{1}{4}$ -in dry screen.


## EXCAVATIONS

Four 1 x 1-m units were excavated at the Laird site in May 1995 (Figure 3). Three of these were in



**Figure 1.** Location of the Laird site in Sherman County, northwest Kansas. Adapted from Fennemans (1931).

5 Meter Grid Block, Unit Numbering Sequence  
 Quad Designations



5	4	3	2	<table border="1"> <tr> <td>NW</td> <td>NE</td> </tr> <tr> <td colspan="2">1</td> </tr> <tr> <td>SW</td> <td>SE</td> </tr> </table>	NW	NE	1		SW	SE
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Figure 2. Block/unit grid system employed at the Laird site.

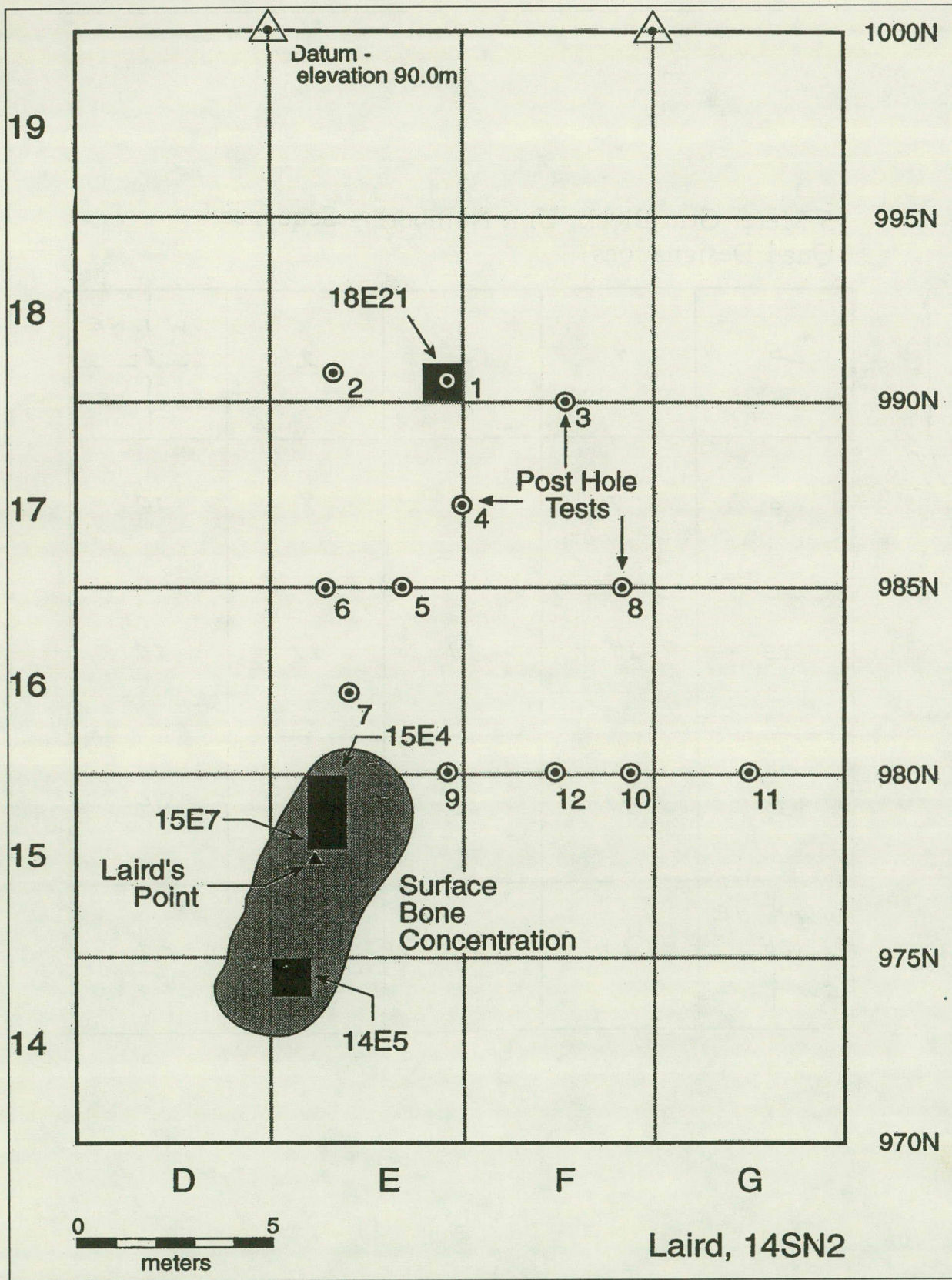


Figure 3. Laird site surface features, unit locations, and grid system.

the area of the primary discovery of surface bone, but the fourth unit was 10 m to the north. Units 15E4 and 15E7 were contiguous squares in the northern portion of the surface bone concentration immediately north of where the projectile point was found. Unit 14E5 was near the south end of the exposed bone. Unit 18E21 was 10 m to the north in an area suspected to represent an in-filled gully.

In addition to these units, 14 posthole augers were excavated in an attempt to determine the limits of the buried bone deposit. The fill from these tests was screened through  $\frac{1}{4}$ -in mesh screen.

Because of heavy rain and limited time, it was not possible to remove all of the bone encountered in units 14E5 or 15E4. A large number of bones were left in place in unit 14E5, and these represent portions of two bison, each with articulated axial elements and limb bones. A full inventory of the elements present is not possible until excavation of these units has been completed.

## BONE

Based upon initial analysis, the Laird site faunal remains consist exclusively of bison bones. The four excavated units yielded a total of 306 mapped bones, concentrated in units 15E4 and 14E5. Additional bone, mostly fragments less than 5 cm in size, from the surface has not yet been completely analyzed. The bone represents both appendicular and axial skeletal elements; however, pelvi are only represented by bones that were left in situ. Although the majority of the bones are highly fragmented and cannot be identified beyond the species level, 86 mapped bones have been identified to specific element (Table 1). Complete bones are represented by phalanges, tarsals, carpals, and sesmoids. Six teeth are nearly complete, missing only partial roots. Skeletal elements occurring in high frequency include rib fragments ( $n = 21$ , 24 percent), isolated teeth and fragments ( $n = 21$ , 24 percent), and phalanges ( $n = 11$ , 13 percent). Forelimbs and hindlimbs are identified by fragments of three radii, two humeri, four ulnae, two metacarpals, three femora, two tibiae, two metatarsals, and additional long bone shaft fragments.

The bones display a moderate amount of weathering, as well as a high degree of root etching, as is expected for such shallow deposits. Bones in the excavated units occurred from the surface to a maximum depth of about 50 cm below the surface. The most deeply buried bone in unit 14E5 is in the best condition with fewer fractures and less intensive root

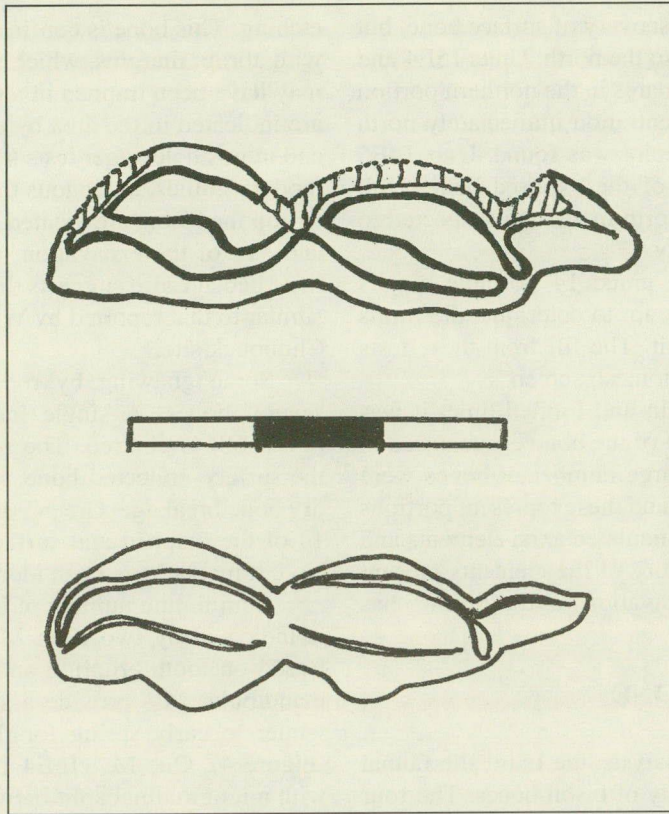
etching. This bone is confined to a narrow linear area with abrupt margins, which suggests that the animals may have been trapped in a narrow gully. Old gullies are indicated in the area by excavation in unit 18E21 and in posthole auger tests #10 and #11. Deep narrow modern gullies, analogous to the one apparently used to trap the bison, are located immediately to the south and east of the excavation. The configuration of the bone bed at Laird suggests that the kill may have been similar to that reported by Wheat (1972) at the Olsen-Chubbuck site.

Some gnawing by rodents is also evident on several bones. A single long bone shaft fragment (14E5-68) is charred. The mapped bone, as well as the surface-collected bone, reflects a high degree of dry bone breakage. Green bone spiral breaks occur on 14 of the mapped and surface long bone fragments. No cut marks have been identified.

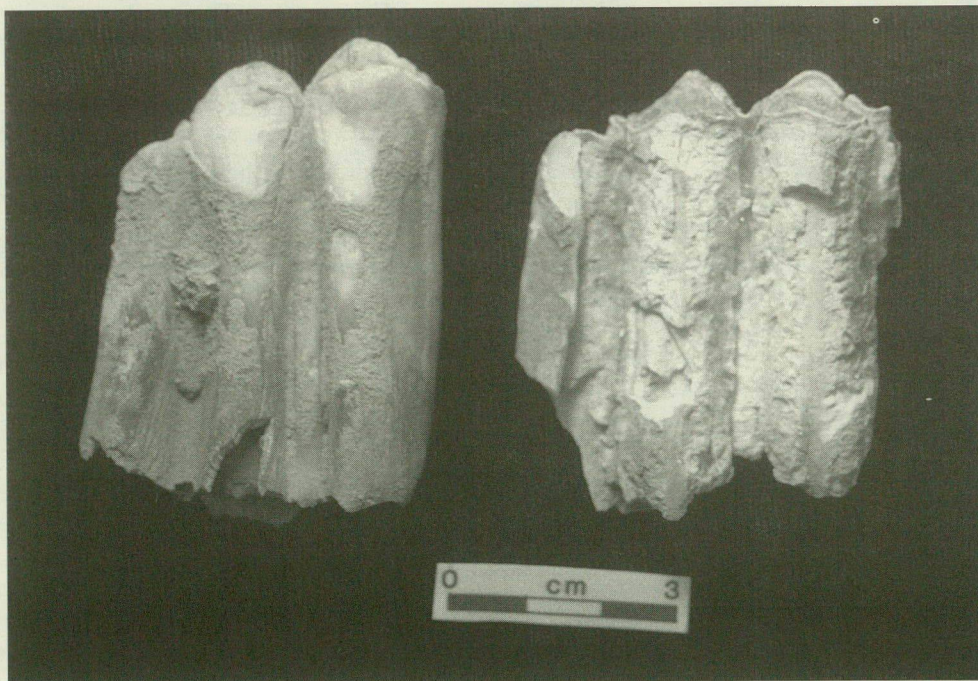
A minimum number of individuals (MNI) of two is indicated by two right  $M_3$ s (lower third molars). Based on tooth eruption and wear patterns, the two mandibular  $M_3$ s provide a seasonal estimate of late winter to early spring for the death of these bison (Figure 4). One  $M_3$  (15E4-14) is highly fragmented with a longitudinal split between the first and second cusp, but it has been refitted. None of the facets show evidence of wear. This is indicative of eruption and wear on animals 2.6 to 2.9 years old (Todd et al. 1996). The second  $M_3$ , found on the surface, is also fragmented, with the ectostylid and the buccal enamel absent. Facets I through IX are lightly worn while IX' is unworn. This tooth represents an animal from the 3.6-3.9 year old cohort.

## LITHIC ARTIFACTS

In addition to the projectile point found on the surface with the exposed bone by Rod Laird in 1990, 12 additional pieces of chipped stone have been recovered from the excavation and surface in the area of the bone bed. These 12 small flakes are Niobrara jasper ( $n = 6$ , 50 percent), basalt ( $n = 2$ , 16 percent), chalcedony ( $n = 1$ , 8 percent), and unidentified cherts ( $n = 3$ , 25 percent). The closest sources of Niobrara jasper are approximately 70 km to the east and north-east of the site. The basalt may be from local gravel sources and the chalcedony from silicified Ogallala bedrock. Metric and technological information is summarized in Table 2. The largest flake, slightly over 4 cm in size and made of Niobrara jasper, exhibits evidence of edge damage along one thin lateral edge and may have been used as a cutting tool. The other flakes are much smaller and probably



**Figure 4.** Above and below. Wear patterns on two lower third molars from the Laird site.



reflect tool retouch and resharpening activity. Half of the flakes exhibit some area of cortex on their dorsal surfaces, but these are all small flakes, and no evidence for intensive initial stage reduction is present in this collection from the bone bed area. Only half the flakes still retain their platform or proximal ends. Four of these appear to be flakes from biface reduction or sharpening. No unifacial retouch flakes (flakes from sharpening unifacial tools [Frison 1968]) are included in this series. A cross tabulation of flake types by stone material is provided in Table 3. In addition to these flakes, a single flat cobble of fossil wood was found on the surface among the bone fragments. This cobble is heavily weathered and appears to be of natural origin and not humanly modified.

The projectile point was found among the bone concentration on the surface in unit 15E14 in July 1990. The specimen is a stemmed lanceolate with a reworked blade and one basal corner slightly broken (Figure 5). The lateral stem edges and base are ground, and the base is thinned with the removal of two primary flakes from each face. The blade edges are beveled on the right side of each face. The point was made from a bifacial preform with no evidence of a flake blank. A small area of carbonate still adheres to one side of the specimen. This was the down side

at the time of discovery. The lithic material is a yellow and brown mottled fossil wood, comparable to material from the Black Forest or Hahn, Colorado, vicinity. Similar fossil woods are also known in east-central and southeastern Colorado. There are no known sources of this material to the east of the Laird site. The point measures 4.90, 2.65, and .662 cm in maximum length, width, and thickness, respectively. Basal width is 2.39, depth of the basal concavity is about .5 cm, and the point weighs 7.3 g. Stem length is 1.5 cm, and the thickness of the stem between the thinning flakes is .51 cm. The largest thinning flakes on either side measure 2.0 by 1.1 and 1.2 by .64 cm in length and width, respectively.

Typologically this point is most similar to the Dalton type, common in eastern Kansas, eastern Oklahoma, Missouri, Arkansas, and adjacent areas (Brown and Logan 1987; Chapman 1975; Goodyear 1974; Kay 1982; Morse 1997; Wetherill 1995). Dalton points are not commonly considered to be a Plains type, but Myers and Lambert (1983) have suggested Eastern Woodlands-Plains connections or interaction, as evidenced by blade beveling and other attributes of Meserve and Dalton-like points that occur in the Plains. Wyckoff and Bartlett (1995) report Dalton and Dalton-like projectile points from

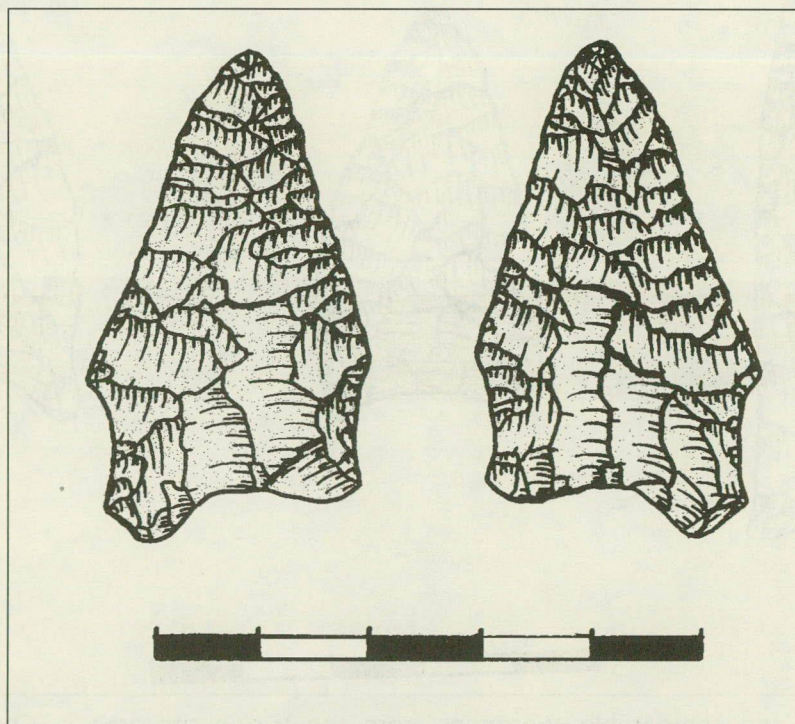


Figure 5. Dalton-like projectile point from the Laird site.

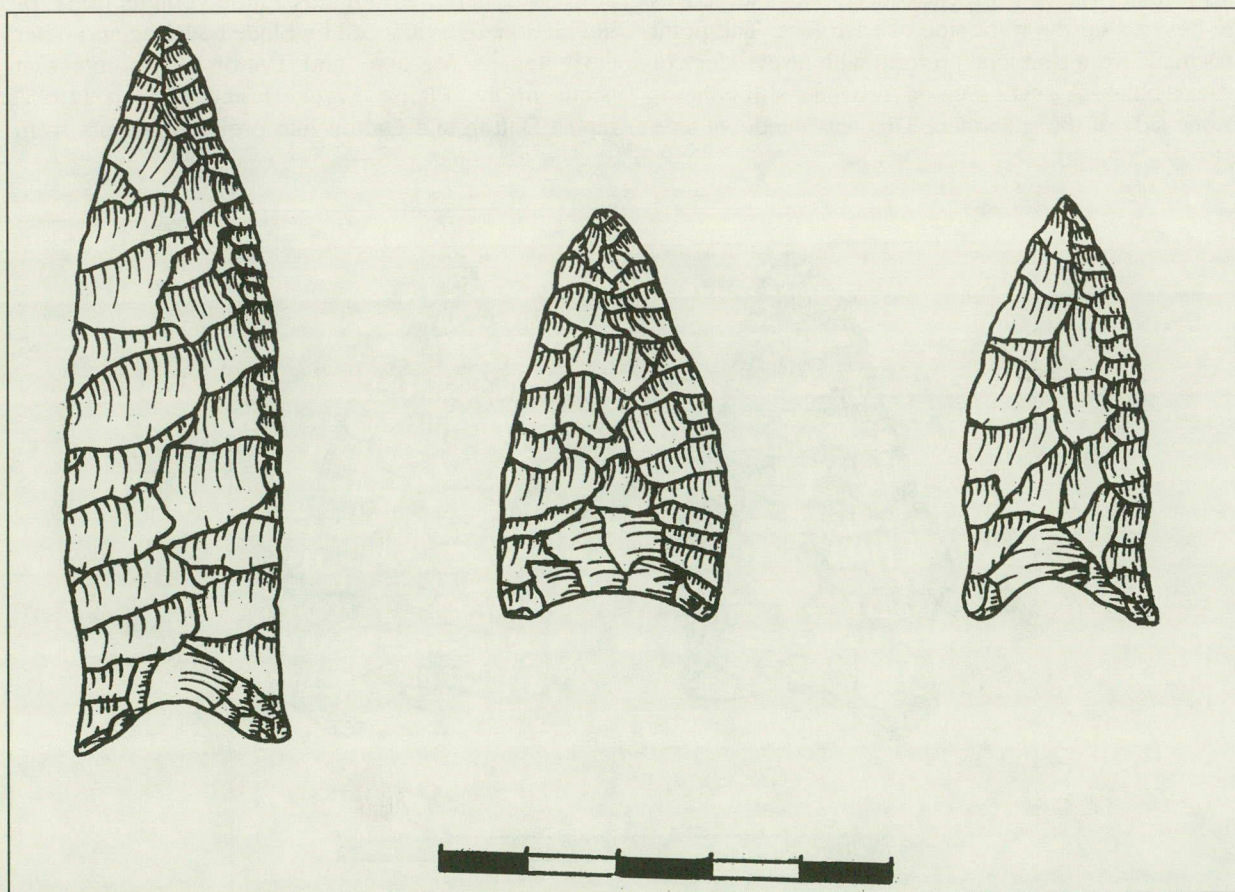
central Oklahoma, and Johnson (1989) reports them from central Texas. Dalton-like and stemmed points with beveled blades occur in the Colorado Rockies and are known from surface collections in western Kansas and eastern Colorado. Examples of Dalton and comparable specimens from Colorado are shown in Figure 6. The Colorado specimens are from the Luckie site area in Cheyenne County, Colorado, and are in the Sigurd Olsen collection. Additional projectile points, comparable to the Laird point, are known from other collections in western Kansas and eastern Colorado. Kornfeld and Frison (1996) report points with comparable morphology from the Middle Park, Colorado, vicinity. These artifacts are noted to demonstrate that the Laird site point is not out of the range of distribution for Dalton or Dalton-like projectile points. The site offers an opportunity to learn more about this technology and cultural complex in the High Plains region and to assess chronological, economic, and cultural relationships that might exist between this material and other

Paleoindian complexes, including the Dalton cultural complex to the east.

### SUMMARY

The Laird site is a small bison bone bed, located in northeastern Sherman County, Kansas, and has yielded evidence of at least two bison in association with a Dalton projectile point and several flakes. Limited test excavation in 1995 indicates that intact deposits remain at the site, and further excavation is needed in order to more precisely determine the minimum number of bison represented and to learn more about the site's stratigraphic and formational history. Age is estimated based on the associated projectile point. Radiocarbon dates derived from deposits with Dalton assemblages in Arkansas, Missouri, and Oklahoma indicate a possible age for the Laird site between 9,700 and 10,700 years ago.

The bison skeletal remains include at least two largely articulated animals, which occur in a narrow



**Figure 6.** Dalton and comparable specimens from the Luckie site area in Cheyenne County, Colorado.

gully fill in a situation analogous to the kill at the Olsen-Chubbuck site, located in Cheyenne County, Colorado, about 180 km southwest of Laird (Wheat 1972). The season of the Laird kill is estimated, based on the available M<sub>3</sub>s, to be winter or about .6 to .9 years from the spring birth pulse. A number of elements and fragments indicate that selected bones were severely fractured while still green and may reflect the extraction of bone marrow. The bones are also severely modified by near-surface weathering, resulting in pervasive root etching and dry bone recitilinear fractures. It is very likely, given the current setting, that the upper portion of the original bone bed has been lost due to erosion and weathering. Exposed bone weathering from the site surface indicates an area of at least 15 m<sup>2</sup> containing shallowly (up to .5 m) buried bone. Auger testing documented the presence of bison bone fragments buried as much as 1 m deep at a distance of 10 m to the east of and uphill from the 1995 excavation and the surface bone exposure. Continued investigation will be required in order to learn more about the relationship of this buried bone to the materials reported here.

**Acknowledgments.** Work at the Laird site would not have been possible without the support of Dan Busse, Rod Laird, Lynn Busse, Andrea Busse, the Busse family, and Keith Coons. The volunteer crew included Margaret Beck, Jeannette Blackmar, Bobby Conard, India Hesse, Karolyn Kinsey, Christine LeFever, Janice McLean, David Nichols, Donna Roper, Karen Turnmire, Dixie West, Rod Laird, Dan Busse, and Andrea Busse. Thanks to Sigurd and Norman Olsen for allowing the researchers to record artifacts in their collection. Special thanks to Linda Greatorex for Figure 3. Also thanks to the students in Anthropology 849, Spring 1997, for aid in recording the Olsen collection. Support for the work at the Laird site was provided by the University of Kansas General Research Fund.

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Table 1. Identified Faunal Remains (N = 86) from the Laird Site (14SN2).

UNIT/NUMBER	ELEMENT	PORTION	SIDE
E14-5/122	rib	proximal + $\frac{1}{2}$ shaft	
E14-5/129	rib	proximal fragment	
E14-5/128	rib	proximal fragment	
E14-5/73	rib	shaft fragment	
E14-5/72	rib	shaft fragment	
E14-5/152	rib	fragment	
E14-5/147	rib	fragment	left
E14-5/102	rib	fragment	
E14-5/104	rib	shaft fragment	
E14-5/82	rib	shaft fragment	
E14-5/85	rib	shaft fragment	
E14-5/118	rib	shaft fragment	
E14-5/117	rib	shaft fragment	
E14-5/124	rib	proximal + $\frac{1}{2}$ shaft	left
E14-5/151	rib	proximal fragment	left
E14-5/93	rib	proximal fragment	
E14-5/94	rib	shaft fragment	
E14-5/90	2nd phalanx	distal fragment	
E14-5/143	2nd phalanx	complete	
E14-5/141	2nd phalanx	complete	
E14-5/49	2nd phalanx	complete	
E14-5/126	2nd phalanx	complete	
E14-5/144	2nd phalanx	complete	
E14-5/113	2nd phalanx	fragment	
E14-5/100	1st phalanx	complete	
E14-5/91	1st phalanx	complete	
E14-5/74	carpal 2 and 3	complete	left
E14-5/75	carpal 2 and 3	fragment	left
E14-5/45	radius	fragment	right
E14-5/77	radius	fragment	right
E14-5/130	radius	fragment	right
E14-5/107	humerus	shaft fragment	right
E14-5/88	calcaneus	proximal fragment	right
E14-5/108	metatarsal	fragment	
E14-5/95	metacarpal	distal fragment	left
E14-5/96	metacarpal	distal fragment	left
E14-5/78	petrous	fragment	
E14-5/119	petrous	fragment	
E14-5/146	frontal	fragment	
E14-5/103	tibia	distal epiphysis	right
E14-5/86	ulna	shaft fragment	
E14-5/131	femur	fragment	right
E14-5/140	femur	proximal $\frac{1}{3}$	
E14-5/135	hyoid	fragment	
E14-5/148	thoracic vertebra	spine	
E14-5/84	maxillary M	fragment	right
E15-4/54	tibia	shaft fragment	right
E15-4/42	sesmoid	shaft fragment	
E15-4/75	thoracic vertebra	centrum	
E15-4/80	thoracic vertebra	centrum	
E15-4/81	ulna	proximal fragment	

E15-4/31	rib	shaft fragment	
E15-4/35	rib	fragment	
E15-4/22	rib	fragment	
E15-4/33	3rd phalanx	complete	right
E15-4/53	2nd phalanx	complete	
E15-4/49	ulna	proximal fragment	
E15-4/36	ulna	proximal fragment	left
E15-4/34	mandibular PM	fragment	
E15-4/14	mandibular M3	fragment (refit portions)	right
E15-4/37	mandibular PM3	complete	right
E15-4/45	maxillary M3	complete	right
E15-4/21	maxillary PM3	complete	right
E15-4/40	maxillary M1 or M2	fragment	
E15-4/44	maxillary M1	fragment	
E15-4/41	maxillary M1 or M2	fragments	right
E15-4/38	maxillary M1 or M2	complete	left
E15-4/20	maxillary PM2	complete	right
E15-7/28	mandibular M1	complete	left
E15-7/17	mandibular M1 or M2	fragment	
E15-7/22	mandibular M1 or M2	complete	left
E15-7/40	maxillary PM2	fragment	left
E15-7/8	maxillary M1 or M2	fragment	left
E15-7/1	maxillary PM	fragment	
E15-7/12	I2 (represents 2 incisors)	complete	
E15-7/20	rib	shaft fragment	
E15-7/9	metatarsal	distal fragment	left
E15-7/24	metapodial	distal fragment	
E15-7/11	humerus	shaft fragment	
E15-7/19	tarsal 2 and 3	complete	left
E15-7/31	femur	medial complete	left
E15-7/34	3rd phalanx	complete	
E14-6/1 (surface)	petrous	fragment	
E15-25/1 (surface)	mandibular PM	fragment	
surface	mandibular M3	fragment	right

Key to abbreviations:

I = incisor

M = molar

PM = premolar

Table 2. Summary Description of Laird Site Flakes. Measurements are in mm; measurements in parentheses are on broken dimensions.

Specimen Number	Material	Flake Type	PRB	Length	Width	Thickness
18E21-1	NJ	TR	Y	10.5	5.7	1.3
15E4-43	BA	SD	Y	10.9	12.4	1.9
14E5-x	UC	BT	Y	12.8	8.2	1.2
18E21-2	BA	PR	N	(5.9)	4.2	1.4
Surf-91	NJ	BT	N	(8.16)	11.7	2.1
Surf-95	NJ	TR	N	(10.8)	10.0	2.6
Surf-95	UC	BT	Y	12.2	14.5	2.5
Surf-95	UC	SD	N	(10.8)	7.5	2.6
Surf-95	CL	SD	Y	27.9	20.9	11.4
14E18-1	NJ	SD	Y	40.6	21.5	12.8
Surf-97	NJ	SD	N	42.4	26.7	6.0
15E4-x	NJ	TR	N	(10.4)	7.1	2.2

Key to abbreviations:

Material:

BA = basalt

CL = chalcedony

NJ = Niobrara jasper

UC = unidentified chert

Flake Type:

BT = biface thinning flake

PR = primary decortication flake

SD = secondary decortication flake

TR = tertiary flake

PRB = platform remnant bearing flake

Y = platform present

N = platform absent

Table 3. Cross Tabulation of Laird Site Flakes by Material Type.

Flake Type	Lithic Material				Totals
	Niobrara Jasper	Basalt	Chalcedony	Other	
Primary	0	1	0	0	1
Secondary	2	1	1	1	5
Tertiary	3	0	0	0	3
Biface Thinning	1	0	0	2	3
Totals	6	2	1	3	12

Table 4. Metric Summary of Selected Dalton-Like Projectile Point/Knives from Western Kansas and Eastern Colorado. Measurements are in mm; measurements in parentheses are on broken dimensions.

Site/ Collection	State, County	Portion	Material	Length	Width	Basal Width	Thickness
Laird	KS, SN	CO	FW	4.91	2.65	2.39	.66
Busse	KS, CN	BA	FB	(3.18)	2.76	2.45	.59
Olsen	CO, CH	CO	JA	5.00	2.25	2.18	.73
Olsen	CO, CH	CO	FT	8.35	2.43	2.46	.62
Olsen	CO, CH	CO	UC	4.69	2.49	2.33	.67
Olsen	CO, CH	BBL	FT	(4.30)	2.65	2.36	.76
Olsen	CO, CH	BBL	DQ	(4.79)	2.34	2.34	.57
Olsen	CO, CH	BA	QZ	(2.61)	2.32	—	.65
Olsen	CO, CH	BA	UC	(2.77)	2.20	2.47	.53
Olsen	CO, CH	BA	UC	(3.26)	2.75	2.58	.52

Key to abbreviations:

State, County:

CO = Colorado, CH = Cheyenne

KS = Kansas, CN = Cheyenne, SN = Sherman

Portion:

BA = base

BBL = base and blade

CO = complete

Material:

DQ = Dakota Quartzite

FT = Flattop

FB = Florence B

FW = fossilized wood

JA = jasper

QZ = quartzite

UC = unidentified chert

## JAMES H. HOWARD, ETHNOGRAPHER (1925–1982) OBSERVATIONS AND RECOLLECTIONS OF A FRIEND

Alan R. Woolworth  
Minnesota Historical Society

*The Kansas Anthropologist* 18(2):59-70

*James H. Howard's fascination with traditional American Indian life started at a young age and continued throughout his career as an archeologist, museum preparator, consultant on American Indian claims cases, museum director, and university professor. Alan R. Woolworth wrote this tribute to his friend and colleague in August 1991.*

James H. Howard and I first met as undergraduates at the Department of Anthropology, University of Nebraska in the fall of 1948. Our backgrounds were very similar, as we both were descended from pioneering Protestant families and had grown up in small, dusty South Dakota towns in the 1930s. We inherited WASP values and viewpoints, since our families were much involved in the Masonic Lodge, the Order of the Eastern Star, the American Legion and its Auxiliary, and other small-town activities.

Both of us had suffered from an overdose of small town social controls and rebelled against them. We marched to our own music! We had also been active in scouting, shared a strong interest in American Indians, and had friends among them. Other common bonds stemmed from our military service in western Europe in World War II. Jim Howard was a dear personal friend and professional colleague. Large, with craggy features, he had an infectious laugh and a winning smile. It was always pleasant to meet him at professional conferences or elsewhere. Frequently he would have a small gift, usually of Indian origin. I still cherish these mementoes.

We chummed around together in Lincoln, Nebraska, in the fall of 1948 and spring of 1949. Soon we went to local peyote meetings and dances, where Jim was well accepted. There we met and became friends with Omaha and Ponca Indians, who made their daily livings in the white world but otherwise pursued their traditional lifestyles. It was a moving experience to listen to a peyote leader explain the beliefs of his religion and its ceremonies in a traditional Indian oratorical manner. Both of us realized that these activities would dwindle when Jim's German war bride, Elfriede Heinze, arrived, and he became a married man. At Christmas vacation of 1949, Jim, Elfriede, and I went to his family home at Redfield, South Dakota, where I met his parents, saw anew our common

cultural ties, and examined his collection of books and Indian items.

For many years his family lived in his maternal grandmother's rambling house, which had a large screened porch. He had two close friends as playmates; they remained lifelong friends. Jim and his pals explored a nearby creek, shared an Indian tipi in his back yard, attended Scout camp together, and had a wonderful time growing up in a small prairie town. Jim enjoyed reading and found a copy of a Dakota Indian dictionary at the Redfield library. He was fascinated by it and gradually began to learn the Dakota language. About 1943 he earned the Eagle Scout rank and enjoyed attending Dakota Indian powwows, which was an unusual thing for a white youth to do in that time and place. Becoming more involved, he participated by dancing in an Indian costume. Eventually these interests led him into the academic world.

On his eighteenth birthday in September 1943, he enlisted in the U.S. Army and went overseas to Italy, where he saw combat. With the end of the war, he was able to move about to find tutors in modern European languages and to play the piano. While in Rothenberg near Wurtzberg, Germany, he met his future wife, Elfriede, a registered nurse whose family had fled Dresden.

The summer of 1949 found us on a University of Nebraska archeological "dig" in southwestern Nebraska, where we lived in surplus army tents. We worked hard, learned much of regional prehistoric Indian cultures, and enjoyed ourselves. Elfriede adjusted well to life in the field, although it was hot with few creature comforts. She was somewhat appalled by local fauna, such as bull snakes, badgers, and snapping turtles, but she skinned bullheads and was a delightful member of the group.

Jim graduated *cum laude* from the University of Nebraska in the winter quarter of 1949–1950 and at once began work on his M.A. thesis. His subject was

the ethnology of the northern Ponca tribe. I well remember how hard he worked in the spring and early summer of 1950 with the goal of earning the M.A. degree in one year. The department chairman opposed this, as he wished students to spend more time on graduate degrees. Jim's GI bill educational benefits had to be supplemented by Elfriede's earnings. He deeply appreciated her efforts but wished to support her from his own income.

Always energetic, he joined an archeological "dig" in South Dakota during the summer of 1950. By fall of 1950, I had entered graduate school at the University of Minnesota. We wrote and met at the annual Plains Conference. By January 1951 he and Elfriede had moved to Bismarck, North Dakota, where he was employed by the State Historical Society.

Jim excavated at the fabled Like-A-Fishhook Village site on the Missouri River in north-central North Dakota during the summer of 1951. The last earthlodge village of the Mandan, Hidatsa, and Arikara tribes, it had been occupied from 1845 to 1890. In 1952 I worked with Jim at this legendary historic site. We had a great time living in the army surplus tents, dealing with an alcoholic Alsatian cook and, as he preferred, with an Indian field crew. Elfriede was employed at a city hospital and studied American medical practices and English. By spring 1953 Jim had found a temporary position with the Science Museum at St. Paul, Minnesota, where he identified and dated Plains Indian costumes. I moved to Bismarck and worked as an archeologist for the State Historical Society for a few years (Figure 1).

By mid-September 1953 Jim was in graduate school at the University of Michigan, pursuing a Ph.D. degree. December of 1955 found the Howards at Kansas City, Missouri, where he taught in the Kansas City Museum and prepared a dissertation. He was awarded the Ph.D. degree in 1957 with specialties in archeology and ethnology. His dissertation dealt with the Ponca tribe and was published in 1965 as Bureau of American Ethnology Bulletin No. 195.

Next the Howards moved to Grand Forks, North Dakota, where Jim taught at the University of North Dakota from fall 1957 to spring 1963 (Figure 2). In September 1963 the Howards moved to the University of South Dakota at Vermillion, where he became director of the W. H. Over Museum and the Institute for Indian Studies. He remained there until summer 1968 and then made a final move to Oklahoma State University at Stillwater. In academic positions from 1957 to 1982, he taught courses in anthropology, ethnology, and allied subjects.



**Figure 1.** James H. Howard, Elfriede Howard, and Alan R. Woolworth at Bismarck, North Dakota, ca. 1955.

Invariably he took his students to professional conferences to enable them to share in that experience. During summers he worked as an archeologist, acted as a consultant on Indian claims, or undertook ethnological research. Whenever possible he participated in Indian powwows. He had a life-long interest in the Plains Indians, with special attention given to their history, costume, dance, and music.

By the mid-1950s he was the proud possessor of a peyote fireplace, which entitled him to hold peyote meetings. I joked with him about his "clerical" status and called him the "Reverend Mr. Howard." Summers were busy with attending Indian events and making ethnological records. Gradually he acquired an impressive personal ethnological collection of materials from Plains Indian and Oklahoma tribes. In spare moments in the field or at home, he often sewed or repaired costume items in his collection. At other times he would track down an object from owner to owner until he found its original maker. It was my privilege to be involved in the evaluation of his collection, which now reposes in the Milwaukee Public Museum.

Jim wanted his Indian friends to be proud and more aware of their cultural heritage. Purposely he

dressed in authentic old-time costumes and went to powwows, where he danced and sang. It gave him great satisfaction when Indians wore their cherished heirloom items. He made a horsehair roach headdress and gradually became more expert in making and repairing old costume materials. An appreciation for fine old Indian objects inspired him to write descriptions of them and to publish these articles in popular hobby magazines. He took care to include instructions on "how to make it." Thus he made information available to anyone with an interest in Indian crafts. Hobbyists prepared their own costumes and participated in powwows, where they met friendly Indians. Everyone benefited.

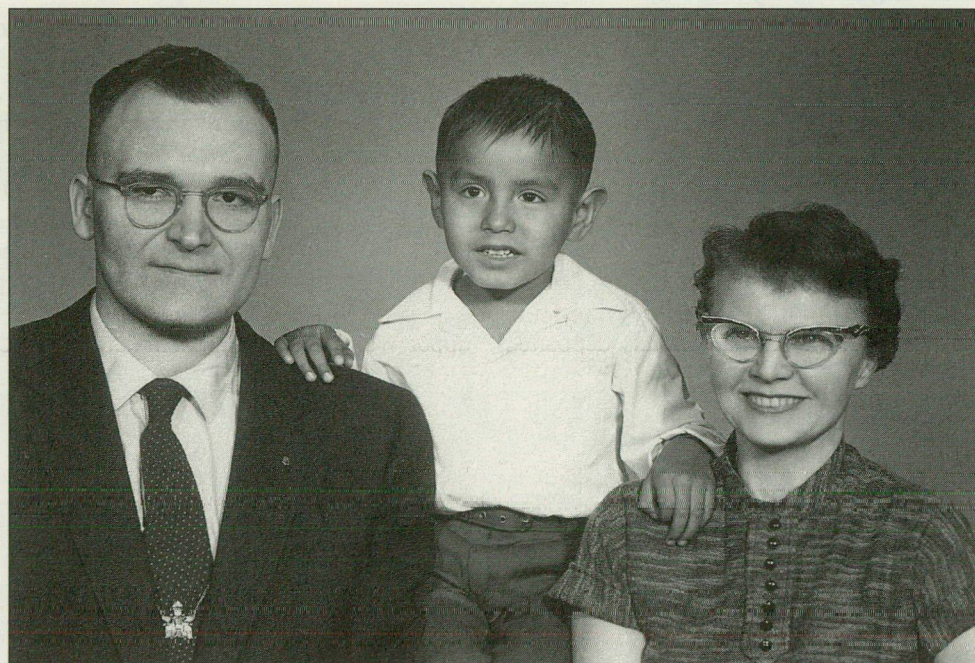
These articles and professional publications led to contacts with hobbyists and museum professionals in Europe. Museum staff members and editors from Austria, France, Great Britain, and the Netherlands visited with Jim and Elfriede at Stillwater, Oklahoma. In 1976 they enjoyed an eight-month-long sabbatical tour of museums in Europe, where they met colleagues and examined collections of American Indian materials. A highlight of this trip was his participation as a section chairman in the XLII Congress des Americanistes at Paris. Jim spoke German well, and French, Spanish, and Italian. This made European contacts far more rewarding.

Jim was also active as a consultant and expert witness on Indian land claims from the mid-1950s

into the 1970s. I worked in this field, too, and we met each other in distant places. In 1967 for example, we were on opposing sides in the Yankton Dakota land claim case before the Indian Claims Commission and enjoyed ourselves immensely. Jim also undertook research on the Plains Ojibway, Yankton Dakota, and Ponca land claims.

A continuing activity from 1950 until his death was the preparation and publication of professional papers and monographs, which number more than 100 items. Collectively these book reviews, narratives, essays, and books constitute a lasting memorial to his professional interests and are a substantial contribution to our knowledge of the Plains Indians. It was his custom to keep this listing current year after year and to send it to his friends. Several of his books have been published since his death through the dedicated efforts of his wife. A list of his writings is appended to this memorial; his major monographs and books are marked with asterisks. Some of them have been reprinted by the J & L Reprint Co., Lincoln, Nebraska.

Jim was outgoing and generous. Over the years he prepared joint publications with colleagues at the University of South Dakota, the State Historical Society of North Dakota, the Ponca Indian Reservation in Nebraska, the University of Oklahoma (Stillwater), the Seminole Reservation in Oklahoma, and various other academic institutions.



**Figure 2.** James H. Howard, Lance Howard, Elfriede Howard, ca. 1960.

Several writers have commented on Jim's interest in the preservation of fragments of traditional American Indian culture. Some people considered him "old fashioned" for his concern with recording this traditional information. Basically he remained a generalist and refused to move into narrow specializations. This was a central motivating force in his career. I well remember his solitary, persistent study of Plains Indian ethnology at the University of Nebraska after Fred Voget, the department ethnologist, left. Jim was also patient with the more numerous archeologists who made gibes concerning "aged and unreliable Indian informants." As I recall, he was familiar with ethnological theory but not much interested in it. He was also unusual in that he participated in Indian dances and ceremonies with a deep understanding and appreciation of their significance.

Another of his traits was the formation of long-lasting friendships with Indian informants. Jim visited them repeatedly to check on factual matters, to socialize, and to keep in touch with ongoing Indian life, overshadowed by an indifferent white world. He also made it a point to visit informants at their homes on reservations and would reimburse them for their time spent in providing him the information or in locating other informants. Here I am thinking of Peter LeClaire, Northern Ponca historian; Josh Saul, a lower Yanktonai artist and traditionalist; and many Turtle Mountain or Plains Ojibway friends. Another such associate was Willie Lena, an Oklahoma Seminole.

It was almost inevitable that he would become involved with the many active Indian communities in Oklahoma and gradually shift some of his attention from Plains tribes to those of the southeastern United States. Most of us are also familiar with his work on the "Pan Indian movement" in the Oklahoma area. He was a member of Phi Beta Kappa and Sigma Xi, and was also a Fellow of the American Anthropological Association and the American

Association for the Advancement of Science. His memberships included the American Society for Ethnohistory, the Society for American Archaeology, and other professional organizations.

Now it is time to make a few brief comments about his faithful wife, Elfriede (Heinze) Howard. They were married in 1948 while Jim was an undergraduate at the University of Nebraska. His GI bill benefits were limited. Elfriede worked full time for eight years to make it possible for him to advance more rapidly in the academic world. This was difficult for her, as wartime food shortages in Europe had affected her health. Also she went with him in the field, to Indian reservations and on archeological digs. In addition she made attractive homes in many unusual locations over the next 34 years. Her social skills smoothed over the rough edges of a largely male professional world, and she did much to aid her husband with his professional life. Since his untimely passing, she has labored diligently to have his remaining book-length manuscripts published. Further, she has found secure repositories for his American Indian costume collection, professional papers, and large library.

Jim Howard's personality, with the interests and adventures we shared, remains vivid for me, as it did during our 30 years of correspondence and less frequent meetings after 1953. I am grateful for this opportunity to share my recollections of a valued friend and respected colleague.

*Acknowledgments.* These reminiscences were greatly strengthened by a bibliography of Jim's publications that he had updated to 1981. Elfriede Howard freely gave her personal observations on his interests and goals. Jim's correspondence with William E. Lemons and Louis Garcia and the many brief obituaries published by his other friends have all been valuable. The fullest published sketch of his life and career by William H. Hodge appeared in *American Anthropologist* in 1986.

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## IN RESPONSE TO HANKS' "THE STATUS OF SIKSIKA BLACKFOOT WOMEN" COMMENTS FROM BLACKFEET COMMUNITY COLLEGE STUDENTS

Alice B. Kehoe and Darrell R. Kipp, Editors

*The Kansas Anthropologist 18(2):71-72*

*Darrell Kipp, director of the Piegan Institute on the Blackfeet Reservation in Browning, Montana, distributed to his Literature 290 students at Blackfeet Community College an essay written by Lucien Hanks in 1942 and published in The Kansas Anthropologist 18(1):45-58. The students were instructed to analyze the essay and respond in an essay of their own. All but one of the students are Montana Piegan (Pikuni), allied with the Siksika and sharing language and culture with that community; Susan Archambault is Assiniboine. Here are the students' reactions to the half-century-old paper.*

Keith Tatsey: In the article ... the Hanks seem to bring out all of the negative issues concerning the Siksika women .... I believe some of this may be partially true, but the Hanks seem to go to extremes or in other words, they like to exaggerate.

Cheryl Comes At Night: [Hanks states that] The Siksika women were [considered] unable to understand things, they had slow motor habits, they also could not remember any songs. Siksika women were barely able to clean house and raise children. Little did these men know that Siksika women could indeed, do any of the above far beyond any man's expectations .... My reactions to the article were stunned at first, outraged next.

Toni Running Fisher: The essay ... is not accurate or creditable .... The writer emphasizes what he feels is significant and important. His choice of words in describing the status of Siksika Blackfoot women are negative and offensive ....

The writer indeed lacked knowledge and it was he who was unable to understand things .... In the Indian culture, many times a song had a right time, place, and purpose. It is not to say that women had poor memories [if they would not sing] because of the improper circumstance .... In the third paragraph the writer stated, "From tests of bravery and skill, they were always excluded." The word "they" referred to Siksika Blackfoot girls. By whose definition of bravery and skill was the statement based upon? Indian people were all brave in their own way and it took a great deal of skill to survive as they had .... Indian words, actions, and reasoning usually had different or more meaning than the Western culture realized .... An entire English sentence could be translated into one Indian word. As time passed and Indian people learned to speak English, the interpretation

remained slightly different. Communication based upon interpretation leads to misunderstanding, contributing to the writer's sustained ignorance.

Christine Running Fisher: I would have said, "The Blackfoot mother is very patient with her child. She spends the majority of her time with it. She could become somewhat impatient."...The paragraph [about adultery] could have been explained more in depth. Now I see why people call us savages. People like this write trash about us like this article. He should have explained that not all women were unfaithful but if they were, then they were subject to punishment. Not all men had many wives and if they did, he should have explained how and why they did.

Alane L. CalfLooking: I was outraged [but] I am not surprised .... Caucasians have to justify the reasoning of mistreatment of our people over the generations. If we could maintain our savage stereotype, then they could say, "We came to civilize them."

I heard stories from our elders at Blackfeet Community College, on the role our women had in the past, and still have today. I think it would hurt a lot of our female elders to read such garbage. I don't wonder anymore as to why the Blackfeet are hesitant at letting non-Indians write books about our people .... I would like to do a paper on something more positive in our culture. We were not always oppressed people.

Susan Archambault: Professor Kipp stresses the importance of "citing your source." Nowhere in this article do I read anything about this author citing his sources .... One of his statements said, "It was stated that in some ceremonies ...." It was stated from whom, when, where, and how? Another comment is as asinine as this guy is, "It was better the way it used to be. A wife did what her husband wanted. They got

along better that way."... The disrespect that has happened to our Indian people is indescribable. It is painful.

### NOTE FROM KEHOE AND KIPP

Hanks, a neophyte anthropologist when he and Jane Richardson Hanks worked on the North Blackfoot Reserve, seems in this paper to have uncritically accepted a variety of statements from Blackfoot. As Ms. Archambault points out, the paper fails to document Hanks' sources. Its tone reflects the earlier "scientific" style of anthropology in which the observer distanced himself from the indigenous community, and his "informants" were lumped and sometimes not even named. A generation before the Hanks, Clark Wissler had documented Piegan culture in acknowledged collaboration with David C. Duvall, a Reservation native, following the research model of Franz Boas. Wissler and Duvall, and the Hanks' colleague Oscar Lewis, more carefully distinguish informants and between statements and observations; Wissler also published, in 1938, a memoir of his ethnographic experiences in counterpoint to his earlier impersonal monographs. (See also papers by Kehoe on Blackfoot ethnography.)

After service in World War II, the Hanks focused their research on Thailand.

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## RESPONSE TO THE BLACKFEET COMMUNITY COLLEGE STUDENTS

Jane Richardson Hanks  
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*The Kansas Anthropologist* 18(2):73-74

I have read the student essays in full, not excerpts, so as to know if the writers were aware of the context of the fieldwork done in 1938-1941, and to consider the possibility that they were responding to the ethnic identity of the author(s).

I am distressed that data given from the lips of distinguished Siksika elders, when they were speaking of changes in Blackfoot lifeways, be termed "garbage" or "trash." Some of these elders were Duck Chief, White-Headed Chief, Old Bull, Little Light, Pretty-Young-Man, and Aisomyaki, the wife of White-Headed Chief and mother of Simon Big Snake. None of these elders spoke English. Consequently, a Blackfoot bilingual translator (male for Lucien, female for Jane) was always with us during interviews. There were also younger informants who did speak English. Thus all data, heard and written down, passed through Blackfoot ears, minds, and hands. There is no exaggeration, fabrication, or secondary interpretation. I realize I should have used more quotation marks in *The Kansas Anthropologist* article, e.g., "slow to react," "unable to understand things," etc. These phrases came from Blackfoot individuals and are not the opinion of the anthropologists. Space did not permit that the speaker's name be given beside every word quoted.

We came to the Blackfoot to learn about their way of life, both in the past and in the present, which means 1938-1941. The tales of the past augmented our respect for them, and the unfolding of their present circumstance convinced us of Blackfoot strength and resourcefulness in the face of severe culture change and dire economic problems. It is possible that some male informants exaggerated to us their own masculine importance vis-à-vis females, including sexual encounters, because at the time of our fieldwork, men had not entirely recovered from the loss of their primary warrior and buffalo-hunter status. They were still seeking a way into a new economy, which in the 1930s was in a shambles all over the country, and even more so on the reserve. Women, in their nurturing and domestic area, had not suffered such a trauma. With their ceremonial roles also intact,

they felt strong and secure. The young Piegan and Assiniboine readers sometime ignore, sometimes downplay, women's supreme role as vower of the Sundance and also as a required presence in the rituals of the Tobacco Dance, Warrior Societies, and the Medicine Pipe. In these ritual contexts their importance was universally recognized by men.

On our first visit in 1938, we arrived at the start of the Sundance. White-Headed Chief invited us to pitch our tents beside his big tipi. With him we presented tongues to the vower, who gave us the holy Pipe to smoke, then painted our faces in reciprocity (the Sun, the Moon, the Stars). Afterward we camped on the prairies and made trips to Indian homes in our car. Where words did not jibe with observations, we did not hesitate to point out the discrepancy. For instance, in the first paragraph of the *The Kansas Anthropologist* paper, is a corrective we gave to the negative statements from Siksika informants, regarding the position of women: "[In the] women's society ... men said they had to lead all the songs so the women could go through their ritual. In practice this was not always the case for we knew aged, able women who knew the songs and had been through them for many decades."

One reader queried why some men had more than one wife. In this warrior society, widowhood with young children was frequent. Polygyny was one way for this responsible community to give economic and social support to the deprived. All of the data presented come, not from books or other sources, but from our own notebooks as taken down during interviews. Lucien Hanks' original notebooks are in Ottawa, Canada, in the National Museum, which made the first request. Jane Hanks' original notebooks are in the Glenbow Museum in Calgary, Canada, together with photographs, movies, and letters. I chose Calgary so that Siksika individuals, also Piegan and Blood, could have easy access to this historical and anthropological material. Calgary and Ottawa have exchanged copies of each other's notebook holdings; thus every interview can be checked, word for word and without restrictions, for verification, informant, translator, date, and place.

I do not think that it is fair nor correct to lump the attitudes of some colonialists in the last century with the work of an anthropologist, seeking to set down the specifics of Blackfoot culture over time, as given by responsible members of the tribe so that their descendants might know their history and mores, with their pluses and even with their minuses. Our long report on our findings is in *Tribe Under Trust*, published in 1940 by the University of Toronto Press. Because the data in the 1942 article were not included there, but are still first-hand information, it was deemed worthy of publication even at this late date.

The article was found in Lucien Hanks' papers as they were being prepared for the Glenbow Museum in 1997.

Once again I take this opportunity to send my thanks to the Siksika Nation for their encouragement, loyal assistance, and lasting friendship. I send my greetings to my kinsmen, descendants of White-Headed Chief and Little Light, who adopted us and gave us our Blackfoot names. I am glad to be a member of the Siksika Museum in Siksika, Alberta. Lucien would join me were he alive.

## BOOK REVIEWS

*The Howling Wolf and the History of Ledger Art.* JOYCE M. SZABO. 1994. University of New Mexico Press, Albuquerque. xv + 270 pp., 32 color plates, appendices, notes, bibliography. \$50 (cloth). ISBN 0-8263-1467-8. Reviewed by Jim D. Feagins, St. Joseph Museum.

The title, *Howling Wolf and the History of Ledger Art*, reflects this volume's contents, except perhaps that the Southern Cheyenne artist, Howling Wolf, is secondary in the volume to the overall history of ledger art, rather than the other way around. Ledger art is a term used to refer to all the late-nineteenth-century Plains Indian art on paper. Of course, some was actually expressed on the lined pages of accountant ledger books. Ledger art is also a convenient, descriptive term to identify a style of art. This publication succeeds in clarifying the history of ledger art, both before and after the start of reservation life on the Plains.

The long tradition of painting on bison hides set a pattern that continued in ledger books. The great mobility of the Plains Indians influenced the types of material and the amount of artwork that they could easily carry from place to place. The increasing scarcity of bison and the increasing availability of a new material (paper) that was easy to carry opened the way for ledger art. Men used hide and paper for the same purpose in their art. However, the historic Plains Indian's lifestyle that maintained this artistic tradition was changing rapidly. Hide robes became out of style and were harder to obtain since the bison were almost gone. The robes were generally replaced by woolen blankets by the 1870s, when the Plains Indians were in the process of being confined to reservations. The first use of ledger books for Plains Indian art is not known. However, they were retrieved on battlefields as early as 1868. Most ledger art ended in the 1890s.

For the Plains Indians, "No differentiation was made between art and craft" (p. 4). Their art was integrated as a part of life. Art production was partly controlled by gender or roles of sexual division. Rending of life forms in art was usually done by males. Most of the woman's art was to make something "pretty." Traditionally, Plains Indian males painted on bison robes, lodge covers, tipi liners, shields, etc. Their art was both symbolic (or conceptual) and representational. The latter is the background from which most of the ledger art developed. They would record vision

quests, concepts of power, or personal symbols of great medicine. Some of their paintings were of a heraldic nature, showing a warrior's brave deeds. This type of public art would show the owner's accomplishments and fulfill a need for social recognition or personal prestige. Also they painted calendric records (winter counts) — a history traditionally painted on bison hides. They included yearly representations (sometime symbolic) of major events or happenings. These shorthand visual representations were sometimes done twice a year (as with the Kiowa). Ledger art generally remained anonymous, as it was not signed, especially in the earlier years. The smaller surfaces to paint (on paper compared to larger hides) led to adjustments in composition by the artists. They used what was available, at times even painting on top of old ledger entries (i.e., drawings superimposed on written accounts). They would use crayons, inks, color pencils — whatever could be readily obtained. Figures would be in outline form, and colors were not usually blended. At first, tradition restricted the Indian artist to certain subjects and certain styles. Universally understood conventions (a pictographic shorthand) were used.

Somewhat standardized, stylistic means gave an understanding to the art. A few of the pictographic conventions were hoof or footprints, gun blasts (from barrels of weapons), flying arrows, flying bullets, already counted coup (coup stick or weapon floating in space above the head or body of an enemy), defensive circles or locations, name signs (above or attached to individuals), and peace negotiations (horizontal pipe floating above persons). Men are predominately represented in the drawings; children appear but rarely.

With time, as new media and techniques were discovered, new subject matter was explored. Economic benefits were derived as the ledger art found a new (white) audience. The reservation era led to experimentation, and the art began to change. Pictographic conventions decreased during the early reservation period, after ledger artists had mastered additional artistic techniques. With time the purpose of the ledger art was more for personal expression and for sale. "Age-old limitations were at work simultaneously with daring individual experiments" (p. 23). As the reservation experience continued, ledger art changed; it could no longer "firmly satisfy traditional necessities of spiritual protection, social stability, or even personal prestige" (p. 16).

Individuals learned from each other's artistic experiments. Some ledgers contained the work of more than one artist.

The Ft. Marion art deserves special mention. In May of 1975, 33 Cheyennes, 27 Kiowas, 9 Comanches, 2 Arapahoes, and 1 Caddo were sent to Ft. Marion, Florida, for a period of 3 years. The purpose was to lock up the Indian "criminal element," thus neutralizing their deterrent to the progress of civilization and also holding them as hostages to keep the Indians back home in line. The government's choice of prisoners was as much political as practical. The hostages came under the care of Lieutenant Richard Pratt. He is credited with making their stay more humane. Pratt encouraged their art and furnished abundant supplies for drawing and painting. Most of the 72 hostages held at Ft. Marion participated in the ledger art work — the Comanches and Caddo being exceptions, as they did not come from traditions of ledger type art.

The captive situation of the artists themselves created an atmosphere that greatly encouraged drawing and painting. To the prisoners the drawings were windows to a new world, as well as a way to record past events. They received no drawing instructions but were encouraged to draw in their "native" manner. However, they produced so many drawings that they soon began to experiment with new techniques, the subject matter expanded, and eventually the pictographic conventions changed and their usage greatly decreased. Apparently the fact that the artists were prisoners influenced the subject of their drawings. Prudence would suggest that they not offend their captors. The number of battle scenes decreased (especially battles against whites), and there was less blood flowing in their drawings. The amount of ledger art produced during the three years at Ft. Marion was phenomenal. The prisoners often sold items they made to visitors, including ledger book drawings.

Some of the Indians learned to read and write. Soon they all had freedom to move about, and some began to work for wages in the local community of Saint Augustine. They performed a variety of jobs. An interesting side note is that a few even assisted the Smithsonian Institution in the excavation of local middens (p. 66). Could this have been the earliest example of Native Americans working on an archeological excavation in the United States?

Howling Wolf, a Southern Cheyenne, was one of the hostages that came to Ft. Marion. He was a skilled artist before he came to Florida; however, his skills greatly increased during his stay at Ft. Marion. Szabo focuses on the art of Howling Wolf before, during,

and after the Ft. Marion experience. Born in 1849, he is known to have produced at least 126 drawings and probably produced much more. Szabo analyzes Howling Wolf's techniques through time, and his work is compared with other Plains Indians artists from a historical perspective.

Over 100 photographs of ledger art are shown in *Howling Wolf and the History of Ledger Art*. These figures and plates include riding horses with arrows and guns, hunting bison, battle exploits, decorated horse and rider, lancing the enemy from a horse, stealing horses, dancing, portraits with lodge, battles on horseback, medicine lodge, peace treaty signing, harbor scenes, camp life, individual portraits, stalking bison, distribution of rations, railroad train, spearing a mountain lion, courting maidens, village, maize garden, hunting antelope, gift giving between tribes, and some battles with white soldiers. These scenes contain considerable ethnographic and ethno-historical information for Plains anthropologists to ponder. However, this reviewer could only wish that more explanation had been included in the captions for each ledger art scene. Several drawings deserve special mention. One is the treaty in 1840 between the Kiowa and Cheyenne (pp. 133-134; plates 18 and 19). Howling Wolf identifies it as occurring on the Flint Arrowpoint River (the Arkansas River below Bent's Fort, Colorado). Two other scenes of special interest to Kansas anthropologists and historians are of a Wichita village (Figure 36) and the Medicine Lodge treaty.

Szabo does an excellent job of describing the ledger art and its history, as was her purpose as an art historian. She presents ledger art as a dynamic, evolving means of expression. However, from an anthropological perspective, additional research on the events and material culture depicted by even just the ledger art shown in the volume would undoubtedly be fruitful.

This, of course, is not a criticism of Szabo and her purpose. Since some of the art shown in this volume has never been published before, it is now a readily available source of important information to anthropologists and historians who are interested in the Southern Cheyenne and other Plains tribes. Even for the drawings alone, this reviewer found *Howling Wolf and the History of Ledger Art* to be a delightful volume.

Ledger art is of considerable interest to anthropologists, historians, artists, and the general public. It developed from the tradition of Plains Indians "... picture writing and heraldic imagery into an art form that was unique in comparison to other Plains

art" (p. 203). It presented additional information on the creative lives of individual artists and the changes that occurred to the Plains Indian culture during a period of great upheaval, recorded by the actual people undergoing dramatic change. Ledger art is more than a work of art, more than just material culture. It is also a record of events and ever changing viewpoints during a segment of time. It is a historic document that reflects the cultural attitudes of the artists and the societies they represent.

Szabo is currently an assistant professor of art history at the University of New Mexico. She has succeeded in transforming her doctoral dissertation into an interesting and readable publication. The results of her research needed to be shared with a wider audience. The book may be purchased through local bookstores or ordered from the University of New Mexico Press, 1720 Loma Blvd. NE, Albuquerque, NM 87131-1591.

*Plains Indians, A.D. 500-1500: The Archaeological Past of Historic Groups.* 1994. KARL H. SCHLESIER, editor. University of Oklahoma Press, Norman. xxvii + 479 pp., 21 figures, 59 maps, 15 tables, contributors, bibliography, index. \$49.95 (cloth). ISBN 0-8061-2593-4. *Reviewed by* Jim D. Feagins, St. Joseph Museum.

*Plains Indians, A.D. 500-1500: The Archaeological Past of Historic Groups* attempts to relate 1,000 years of prehistory to the various historic groups present on the Great Plains. In so doing, a tremendous amount of information from the many subregions of the Plains has been organized into a most readable volume.

Concerning the volume's purpose, Schlesier states, "It attempts to extend ethnohistory back in time beyond its present limits and to identify cultures ancestral to historic groups. This is an important although difficult task, and *it has not been tried before*" [italics added] (p. xv). Of course, it has been tried before — and on the Plains at that. The direct historic approach was used over a half century ago by Strong (1935), Wedel (1940), and Steward (1942). In spite of his puzzling statement, Schlesier acknowledges the contributions of these pioneering researchers later in the volume. Their efforts, with important limitations, were significantly successful.

Since then, other Plains archeologists have applied this method. Could it be that Schlesier simply means that this volume represents the first time the method has been expanded to encompass the whole region? Perhaps, but in the context in which the statement is made, that does not appear to be the case. The volume is a worthwhile and commendable effort. It does not need to have its importance inflated.

*Plains Indians, A.D. 500-1500* contains an introduction and 14 chapters, written by 17 contributors. A few chapters discuss in detail regions outside of the Plains. Generally this is justified by presenting a better understanding of the cultural interactions between the regions. The chapters and authors in this volume are "Cultures of the Northwestern Plains: From the Boreal Forest Edge to Milk River" by J. Roderick Vickers, "Late Prehistoric Cultures on the Montana Plains" by Sally T. Greiser, "Cultures of the Mountains and Plains: From the Selkirk Mountains to the Bitterroot Range" by Philip Duke and Michael Clayton Wilson, "Archaeological Complexes of the Northeastern Plains and Prairie-Woodland Border" by Michael L. Gregg, "Neighbors to the North: Peoples of the Boreal Forest" by David Meyer and Scott Hamilton, "Cultures of the Upper Mississippi River Valley and Adjacent Prairies in Iowa and Minnesota" by Guy Gibbon, "Cultures of the Middle Missouri" by R. Peter Winham and Edward J. Lueck, "Cultures of the Heartland: Beyond the Black Hills" by L. Adrien Hannus, "The Central Lowland Plains: An Overview" by Patricia O'Brien, "The Central High Plains: A Cultural Historical Summary" by Jeffrey L. Eighmy, "Cultural Continuity and Discontinuity in the Southern Prairies and Cross Timbers" by Susan C. Vehik, "Holocene Adaptations in the Southern High Plains" by Timothy G. Baugh, "Development in the Southwest and Relations with the Plains" by Albert H. Schroeder, and "Commentary: A History of Ethnic Groups in the Great Plains, A.D. 150-1550" by Karl H. Schlesier. A major asset of a volume of this type is the range of viewpoints and interpretations from a variety of geographic areas.

After his cultural history overview of the northwest Plains, Vickers (pp. 30-33) presents a fine discussion of some of the problems in trying to correlate archeological data with historic cultural groups. He seems to have a much better grasp of the pitfalls than the volume's editor. Vickers explains that it often can be quite difficult to correlate prehistoric cultural materials (even material as diagnostic as projectile points) with specific ethnic groups or historic tribes. At present, projectile points are more

diagnostic as widespread horizon markers, given the general lack of detailed stylistic and functional studies. He urges that more detailed studies be conducted and published. He also laments the lack of reporting consistency. When available, researchers are often inhibited by noncomparable data bases. Without abundant data in a useable form, it is difficult to compare morphological variations of index artifacts or key artifact classes and to compare trait frequencies. Because of these problems, he urges that, "we should consider the methodologies that have been used to construct the phases themselves" (p. 32). Vickers goes on to state another problem, "It is time to get our ethnic units out of the closet — let us explicitly formulate our social models so that we can criticize and test them. Until we do this, we cannot hope to approach a scientific prehistory."

Up to this point, this review has presented a general overview of the volume. Now it will focus on a few chapters that should be of most interest to Central Plains anthropologists and historians. Pat O'Brien's chapter concerns a portion of the Central Lowland Plains (most of Kansas and Nebraska). She does a fine job of condensing, modifying, and updating some of her previous overviews of this area's prehistory and protohistory. This chapter includes a section of the Middle Woodland period, which extends back in time past the A.D. 500 baseline for this volume. This section includes discussions of the Kansas City Hopewell variant, Schultz focus, Cuesta, Valley, and Deer Creek phases.

The Deer Creek phase was identified from scant materials that appear to be similar to material identified as Kansas City Hopewell. While the range of the projectile point types is rather limited, could they represent some mixing of middle and late components during Woodland times? This reviewer agrees with O'Brien that the radioactive carbon dates for the Cuesta phase are much too late and are probably in error. The Cuesta phase is located geographically between the Cooper focus/phase in northeast Oklahoma and the Kansas City Hopewell. More needs to be learned concerning the relationship between these mostly contemporaneous groups.

Several Late Woodland groups are also discussed — Keith, Butler, Greenwood, Grasshopper Falls, Hertha, Wakarusa, Sterns Creek, Loseke Creek, and an unnamed complex in the Kansas City area. O'Brien presents an interesting discussion concerning the Wakarusa and the Hertha phases. She states, "These two phases are linked because it is suspected that they are related. They are probably also related to the Unnamed Kansas City complex ...

and may be part of the Grasshopper Falls phase just north of the Kansas River. Nathan (1980:172) implies as much for the Wakarusa phase in her comment that all the Woodland pottery she discovered in Clinton Lake was related to Grasshopper Falls ceramics" (p. 210). Clearly the taxonomy needs to be restudied, especially after more field work.

In the Central Plains tradition section, O'Brien discusses Upper Republican, Nebraska, Smoky Hill, Pomona/Clinton, Bemis Creek, and Steed-Kisker phases. She is willing to suggest "... that the Nebraska phase, via the St. Helena and the Anoka phases ..., turns into late prehistoric ancestral Arikara Indian" (p. 214). She also presents her thoughts on evolutionary sequences of which the Smoky Hills phase is a part. Her speculations present much food for thought.

O'Brien's discussion of the Pomona phase, which she titles Pomona/Clinton phase, is quite interesting. She states, "The Pomona phase is the widely recognized name for this phase, but in 1968 Johnson defined the Clinton phase in Clinton Lake south of the Kansas River. Chambers et al. (1977:170) note though that Witty identified some of these sites as Pomona. Therefore I suspect the two phases are one and the same" (p. 216). If this is correct, probably the term "Clinton phase" should be dropped.

For quite a few years Witty (1978:61-62) has argued that the Pomona phase developed from Late Woodland cultures in the area, such as the Greenwood phase. This reviewer's own research in eastern Kansas certainly supports this logical conclusion. This reviewer suspects that there are certain sites that are transitional in time and content between Late Woodland and Pomona. These proposed "missing link" sites should be investigated and dated. Perhaps the Roth site, excavated by the Kansas State Historical Society, may ultimately prove to be one example (Brogan 1982; see also Feagins 1983). This "early Pomona" site dates to the Late Woodland period — A.D. 720 ± 180 and A.D. 850 ± 90. O'Brien mentions (as did Witty) the importance of the matching pottery pastes of the Greenwood and Pomona phases (p. 216). This should come as no surprise, since both groups occupied varying amounts of the same geological area. Similar pastes could be more suggestive of similar sources of locally available pottery clay than cultural continuity. While this is arguing on the other side of the coin, it certainly does not imply that Witty's suggestion of cultural relationship is in error.

It is interesting to note that O'Brien makes no mention of Brown's (1984) extensive subdivisions of

the Pomona phase. In passing, she does mention Johnson's (1991) argument "... that Pomona represents a prehistoric expression ... of the Kansa Indians" (p. 217). She is gracious in stating, "That theory is highly controversial" (p. 217).

As would be expected, O'Brien presents a fine discussion of the Steed-Kisker phase. It is unfortunate that the quite controversial Steed-Kisker "solstice shrine" at Smithville Reservoir is presented as a fact (p. 218). No matter how precisely recorded the data on this feature (O'Brien and McHugh 1987), this interpretation (the most exciting of several possibilities) is not proven and remains most speculative.

The Itskari (Loup River) and Lower Loup, Glen Elder (including White Rock aspect), Dismal River, and Great Bend phases, and the Pratt and Bluff Creek complexes are described in O'Brien's protohistoric section. The Oneota tradition receives only a passing comment. Her summary and conclusions attempt to link the historically identified tribes in the area with the archeological identified prehistory. Readers of *The Kansas Anthropologist* should find this chapter of considerable interest.

Jeffrey Eighmy's chapter contains information on the Central High Plains, including parts of Colorado, Kansas, Nebraska, and Wyoming. This short chapter describes the prehistory from A.D. 500 to 1500 but makes no attempt to correlate this information with more recent cultural groups. He discusses some of the problems using Woodland pottery and lithic artifacts in the Central High Plains and points out the potential for hunter-gatherers to also produce aceramic sites during this time period. He says that most of the information on Late Woodland sites in this subregion comes from the northern and western parts — in the North Platte, South Platte, Lodgepole Creek, and the Colorado foothills. He states, "... to date, no Woodland component has been reported on the High Plains remnant of eastern Colorado drained by the Republican or Smokey [sic] Hill rivers [and] ... east of the foothills in the Arkansas drainage north of the river ..." (p. 229). Were these areas sparsely utilized during Woodland times? Or perhaps this is a reflection of the relative lack of cultural resource surveys in those areas? One wonders how the adjacent areas in extreme western Kansas compare. The Upper Republican phase during post-Woodland times — A.D. 1050 to 1500 — is discussed. Eighmy makes a distinction between the classic Upper Republican in north-central Kansas and south-central Nebraska and the High Plains Upper Republican sites. The latter is an area of apparent secondary utilization by the Upper Republican

people. It is surprising that Eighmy does not mention the Dismal River aspect. Also he does not attempt to extend any of the tribes or linguistic groups that utilized the Central High Plains back into the prehistoric past.

Chapter 11, written by Susan Vehik, describes the southern prairies and cross timbers areas of parts of Oklahoma, Texas, and Kansas. She discusses some of the same archeological cultures as O'Brien. However, Vehik is less inclined to speculation. This reviewer especially likes Vehik's succinct interpretations of the relationships among the Cuesta phase, Cooper focus, and the Kansas City Hopewell (p. 241). She has very little to say about the Wilmore complex in southern Kansas but does point out that it is poorly known. Additional research is greatly needed. Her discussion of the Neosho focus is to the point and seems to be well supported.

The ethnologist/editor Karl Schlesier attempts to summarize the volume in the final Chapter 14. He states, "This chapter attempts to identify all relevant archaeological entities discussed in this volume with their descendant historical ethnic groups. It is a formidable task, and the results will not be the last words on the subject" (p. 308). This last comment is certainly an understatement. The final chapter is an important one for any volume. However, this one has proved to be a disappointment — not because of the scope of research or his writing ability. Rather, Schlesier tries to force conclusions even when the data does not merit it. At times his conclusions are built on foundations of speculation and multiple hypothesis. This can be a dangerous practice and is not good "science." The archeological record does not always contain and/or archeologists have not yet discovered enough information to always link the archeological record to specific linguistic/tribal groups. Of course, this in no way diminishes the successes of the historic approach. However, there are times when Schlesier's linkages are pure speculation or at best quite tentative, and that needs to always be **clearly** communicated. However, the overall purpose of the chapter and certainly the volume as a whole has considerable merit. It is no disgrace that the sought-for linkages or archeological chronology and linguistic/tribal affiliations cannot always be supplied. Also considering the present social, legal, and politically correct climate, it is far better to realize the current limitations of archeological knowledge, the nature of circumstantial evidence, and the sometimes tentative nature of our interpretations, rather than attempting to force conclusions that cannot be adequately supported with facts.

In spite of this reviewer's criticisms of the concluding chapter, *Plains Indians, A.D. 500-1500* is a grand approach and as a volume is highly recommended. The many contributing archeologists have presented thoughtful summaries and considerable information on the cultural histories of their assigned subregions. The over 1,200 references cited are a valuable source of information — especially considering that quite a few of these references are from the elusive gray literature. This volume is of value to all those interested in Plains Indians and their connections with the archeological past. It can be ordered through local bookstores or from the University of Oklahoma Press, P.O. Box 787, Norman, OK 73070-0787. *Plains Indians, A.D. 500-1500* is certainly worthy of space in one's library; it is interesting and will serve as a useful reference.

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- Ceramic Commodities and Common Containers: Production and Distribution of White Mountain Red Ware in the Grasshopper Region, Arizona*. DANIELA TRIADAN with foreword by RONALD L. BISHOP. 1997. Anthropological Papers of the University of Arizona No. 61. The University of Arizona Press, Tucson. xv + 145 pp., 14 plates, 73 figures, 31 tables, appendices, references, index. \$14.95 (paper). ISBN 0-8165-1698-7. Reviewed by Jim D. Feagins, St. Joseph Museum.

While *Ceramic Commodities and Common Containers* is written primarily with Southwestern archeologists in mind, archeologists from other regions can also benefit from this volume, especially in the areas of theory and method. In the last few decades there has been a more rigorous effort to determine prehistoric behaviors by developing and refining methods to test competing theories. One of the more useful techniques in recent years attempts to determine where pottery was manufactured, not simply where broken sherds were recovered in the archeological record. Traditionally Southwestern archeologists (as well as those from the Plains) have assumed that most pottery types found in abundance on a site were locally made, while relatively uncommon sherds came from "exotic" pots that were obtained elsewhere. Since pottery is one of the most common and useful categories of material culture that archeologists work with, identifying its origins in a more reliable manner is a considerable boon to understanding what occurred in the past. Concepts of economic and social interaction and population movement can be explored more reliably if pottery sources can be determined. This would include geographic movement, resource exploitation, exchange (trade), specialization, and other types of interaction between different people and their environments. There has been a paucity of this type of research by archeologists in the Plains, as well as the Southwest and other regions.

Pottery sourcing techniques (when done properly) help remove some of the potential for interpretive ambiguity. They can help resolve some of the questions that archeologists ask by substituting scientific data for speculation concerning the sources of ceramics in a given assemblage. Triadan's work on sourcing White Mountain Red Ware ably demonstrates the improved ability to use ceramics in reconstructing prehistoric sociopolitical and socioeconomic organization. This information, when gathered in an area, can greatly aid in developing inferences about political, ideological, and cultural boundaries, demography, community organization and interaction, exchange and trade, and cultural or ethnic affiliation.

One of the prerequisite questions concerns which types of ceramics are produced locally at a given site or within a given area and which types were obtained from other areas. "Compositional analyses (chemical and petrographic) provide a systematic, analytical means to investigate the provenance of pottery" (p. xiii). Triadan presents a number of methods that, when used in combination, establish the composition

of pottery sherds and allow comparison with the composition of natural clay sources. She then demonstrates their use with large scale chemical and petrographic analyses of White Mountain Red Ware ceramics. Her data has overturned some of the previous thinking about this ware. This is, of course, important to Southwestern archeologists, especially since this ware is common over most of the northern part of the region. However, to archeologists outside of the Southwest, the White Mountain Ware investigation serves as a case study in composition/sourcing methodology. Triadan's work is a good companion to that done by Maras Nieves Zedeño (1994), which was reviewed in *The Kansas Anthropologist* (Feagins 1995). The reviewer can think of many examples where compositional studies and sourcing research on ceramics would have considerable research potential in Plains archeology. While these types of studies have begun in the Plains, researchers lack the abundance of comparative data that is available to archeologists in the Southwest. While initially it is a limiting factor in regional studies, the potential is there for significant cultural research toward developing and starting to test models of sociopolitical and socioeconomic organization in the past.

This volume is modified from Triadan's doctoral dissertation at the University of Arizona. She is to be commended for not only presenting significant new data and analysis concerning cultures in the Southwest, but for presenting much food for thought to non-Southwestern archeologists on sourcing methods and theory. It is certainly worth the time spent reading and pondering the contents of *Ceramic Commodities and Common Containers*. This publication may be obtained through a local book dealer or from the University of Arizona Press, 1230 N. Park Ave., Ste. 102, Tucson, AZ 85719.

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*Grasshopper Region.* Anthropological Papers of the University of Arizona No. 58. University of Arizona Press, Tucson.

*Plains Indian History and Culture: Essays on Continuity and Change.* JOHN C. EWERS with foreword by WILLIAM T. HEGAN. 1997. University of Oklahoma Press, Norman. xxii + 272 pp., 37 figures, 5 maps, 2 tables, notes, references, index. \$29.95 (cloth). ISBN 0-8061-2862-3. Reviewed by Jim D. Feagins, St. Joseph Museum.

John C. Ewers died at age 87 in May 1997, one year after celebrating his fiftieth year with the Department of Anthropology at the Smithsonian Institution. Ewers was a well-respected and well-informed ethnologist and lover of Plains Indian history. He obviously had a broad curiosity about a great many things. As an outstanding ethnohistorian, Ewers liked to approach research from many directions. Ewers' *Indian Life on the Upper Missouri*, published in 1968 and still in print, was a selection of previously written articles and lectures. *Plains Indian History and Culture* is also a collection of essays, written since 1968. Originally given as lectures at scholarly meetings of historians and anthropologists, these authoritative, entertaining, and informative papers, written clearly and without jargon, are now available to modern readers.

The first two chapters in *Plains Indian History and Culture* are titled, "When Red and White Men Met" and "Indian Views of the White Man Prior to 1850." Ewers presents an even-handed approach, with a considerable amount of insight into the viewpoints of Native Americans.

"The Influence of the Fur Trade upon the Indians of the Northern Plains" is the title of Chapter 3. Ewers points out that Indians and Indian traders played as active a role in the fur trade as did the white traders. The Indians were quite experienced traders before the Europeans appeared on the Plains. However, their previous experiences as traders did not adequately prepare them for the tremendous impact of the Euroamericans on their culture. It did not teach them about the effects of liquor. It did not teach them about the worth of their land or prepare them for the allotment system and land sales. Also, the fur trade almost killed off the bison, thereby removing the main food

supply of the Indians, and they became dependent on the materials they obtained in trade. Unions between white traders and Indian women and the mixed-blood populations that emerged became important in history. Some offspring could read and write, and they were often the ones the tribes chose to have leadership roles in communicating with the government. Many ideas as well as trade goods were exchanged, and this exchange had a lasting impact on the Indians.

Chapter 4 is titled, "The Use of Artifacts and Pictures in the Study of Plains Indian History, Art, and Religion." Early on, Indians made artifacts to sell to white traders and others. This material was often called "Indian curiosities" by the Euroamericans. Army officers, traders, artists, and missionaries commonly collected the material culture of Native Americans. Sometimes ceremonial objects were sold. Ewers notes that the power of a sacred object is not transferred without a ritual. An object can be remade and sold many times by the owner of the power of the object (pp. 78-81). There are many examples of this occurring in Plains Indian history. If one follows this line of thought to a logical conclusion, then does that mean that sacred objects without their ritual are no longer sacred? That would seem to have been the situation in at least some cases. If that is correct, concerning the "sacred objects" that individual Plains Indians sold, then how does this concept affect the true status of at least some of the objects found in the soil that archeologists or others have interpreted as sacred? The status of any such object would be very difficult to confirm and doubly so in today's political coliseum.

Chapter 5, "The Influence of Epidemics on the Indian Populations and Cultures of Texas," describes many of the social and population changes caused by European diseases. Diseases became rampant among the tribes. As population declined, there was much adoption of captives and regrouping of tribes.

"Symbols of Chiefly Authority in Spanish Louisiana" (Chapter 6) describes the role of a few types of extraordinary presents given to prominent chiefs by Spanish officials. These chiefly gifts included flags, uniforms (perhaps even including hats), medals, staffs (canes with ornamental gold or silver heads), and commissions. Most often, medals of the king were presented in an impressive ceremony to chiefs or to individuals whom the Spanish wanted to promote into positions of greater leadership. In some cases the gifts put the leaders under great obligation to the Spanish. To many, these medals were not just symbols of authority but were considered as sacred objects. Recognition of chiefs in this manner tended to give them the office for many years and block the

advancement of others. In essence this became a new system imposed on Native Americans by whites, which must have increased intertribal discord and jealousies.

Chapter 7 is titled, "Climate, Acculturation, and Costume." Ewers describes how women's garments were modified by acculturation and affected by seasons and climate. One interesting observation is that the topless summer attire prevailed longer with the Wichita than among other tribes, because this allowed their elaborate tattoos (especially of the breasts and upper body) to be observed. Of course in more northern climates, any such decoration would usually be covered. The multi-piece trade-cloth dress and the southern plains three-piece buckskin dresses were also discussed.

"Folk Art in the Fur Trade of the Upper Missouri" is the title of Chapter 8. Documentation of the painted folk art on some of the fur trading posts was presented. There were occasionally white artists at the posts who desired to paint the Indians who came to trade. A few of these temporarily in residence artist-explorers were employed by the fur companies to embellish their remote posts with what Ewers calls folk art. These paintings were used to encourage friendship and trading. The Indians did not need to read to get the message from the paintings.

Chapter 9 is "Intertribal Warfare as the Precursor of Indian-White Warfare on the Northern Great Plains." There is a vast amount of evidence of intertribal warfare during the late prehistoric, protohistoric, and early historic periods. The Indian wars did not start with the white invasion. There were many tribes eager to join the whites to fight against their ancient enemies. There were tribal alliances with neighbors against common enemies, and occasional joint expeditions occurred. There were many horse raids and small battles and some large engagements.

In Chapter 10, "The Making and Uses of Maps by Plains Indian Warriors," Ewers points out that the Indian-made maps were good. Distances were usually recorded by the number of days travel. Some maps were used to plan military operations.

Women's role in warfare was both active and passive, as explained in Chapter 11. They were often killed or taken captive. If captured, they sometimes were killed later, adopted, or traded as slaves. At times women played an active role in the mutilation of dead enemies. Some women joined in raiding parties, and on rare occasions a woman would be the leader.

The volume ends abruptly with Chapter 12, "The White Man's Strongest Medicine." Many Indians considered "Great Medicine" to be anything they did not understand. For example, Lewis and Clark's

black servant was considered very strong medicine. When Indians first came in contact with horses, they were considered to be gifts from the spirits. The Sioux name for horse was "medicine dog" (p. 207). At first guns were considered great medicine. The power of the written word was truly amazing.

*Plains Indian History and Culture* is a good read. It may be obtained from a local bookstore or from the University of Oklahoma Press, P.O. Box 787, Norman, OK 73070-0787.

*People, Plants, and Landscapes: Studies in Paleoethnobotany*. KRISTEN J. GREMILLION, editor. 1997. University of Alabama Press, Tuscaloosa and London. xviii + 271 pp., figures, tables, index. ISBN 0-8173-0827-X. Reviewed by Chris Benison, Mt. Pleasant, Michigan.

Kristen Gremillion's new volume is an excellent addition to the burgeoning literature on the field of archeobotany. Like other recent publications on the subject, *Plants, People, and Landscapes* covers methodological and theoretical issues and provides new data on archeological plant remains as food for thought. A major strength of the book is that it illustrates the full range of variation among food production systems, reconstructed for North America and far eastern Asia (Japan). It thus shows how different people have adapted to different environments (partly by using widely differing plants) through time. Investigating variation among human populations clearly continues to be a major goal of archeology. Gremillion's book also succeeds because it demonstrates how human behavior frequently produces significant changes in both plant species and landscapes.

Gremillion's volume showcases current thinking on prehistoric horticulture for portions of the Eastern Woodlands of America and Japan in two sections. Part I, "The Archaeological Record of Plant Domestication and Utilization," opens with a retrospective on the development and growth of paleoethnobotany over the past 30 years or so. Patty Jo Watson, a pioneering researcher in both the Near East and eastern North America, reviews the early contributions of archeologists and non-archeologists alike. She considers the effect that improved sampling and the use of sophisticated technology, such as scanning

electron microscopy in seed analysis, has had on the practice of paleoethnobotany.

Following Watson's paper, Part I continues with summaries of current archeobotanical data from various study areas in the Eastern Woodlands. The first contribution by Kristen Gremillion describes early studies of plant remains recovered from Newt Kash Rockshelter in 1935 as well as her reanalysis of the original collections from this site. Newt Kash, located in northeastern Kentucky, has yielded evidence for Late Archaic/Early Woodland use of weedy annuals (*Chenopodium*, *Iva*, *Polygonum*, *Amaranthus*) as well as for use of mast nuts (hickory) during those periods. Gremillion also considers biases stemming from analysis of older collections, as well as those that are intrinsic to plant remains derived from cave/rockshelter deposits.

The next two papers by Fritz and Cowan provide further examples of the widely divergent food production systems known for the Eastern Woodlands. Fritz describes a seed cache, containing thousands of cultivated/domesticated *Chenopodium* seeds, from a rockshelter in the Ozark Uplift of south-central Missouri. Cultivated/domesticated forms of *Chenopodium*, a plant with nutritious, starch-rich seeds, have enlarged seeds with thin seed coats when compared to wild/weedy varieties. Cowan has identified patterned changes in the use of different squash species (*Cucurbita*) by Archaic and Woodland peoples of eastern Kentucky. He argues persuasively that strains with unique traits were preferred at different times during prehistory. Based on available data, thick-walled squashes with little edible fruit (useful as containers) were apparently favored over more edible varieties with thinner walls, and vice versa. The reasons behind these alternating shifts in plant use remain relatively poorly understood.

The final contribution in Part I by Crawford summarizes what is currently known about prehistoric food production for the northern islands of Japan. Particularly interesting are Crawford's discussions of barnyard grass (*Echinochloa* sp.) and green foxtail (*Setaria* sp.), both of which he considers economic species of some importance. In North American archeobotanical collections both are regarded as weed species of no economic value to human populations.

Part II of the book, "Plant Resources, Human Communities, and Anthropogenic Landscapes," deals with the related issues of human/plant interaction and the creation and evolution of "settled" landscapes. The first paper by Scarry and Steponaitis details how differential patterning in plant remains from the Mississippian Moundville site in Alabama may

reflect differences in social hierarchy. This seems to be especially so in the case of maize, where frequency differences in carbonized cobs versus kernels suggests that non-elite Moundvillers labored to process maize for a privileged elite class. It is not entirely clear what the Moundville non-elites were receiving in return for their labor.

The next paper in the series by Winterhalder and Goland reviews recent developments in diet-breadth models and economic niche theory; it further suggests how both can provide insights on how humans make economic decisions given certain circumstances. Gardner's very thought-provoking contribution reviews evidence for Archaic and Woodland period mast nut utilization, again for the Eastern Woodlands study area. While much additional baseline research remains, Gardner argues convincingly for a systemic correlation between large-scale middle and late Holocene climatic shifts and significant shifts in mast nut use through time. His model is reminiscent of Bruce Smith's (1992) "Floodplain Weed Theory" in terms of its elegant simplicity.

The last two papers in Gremillion's book are by Gregory Waselkov and Julia Hammett. Waselkov's paper outlines the extreme differences between Native American and European conceptions of space and proper land use practices. He describes the disruptions to Indian land use systems, occasioned by the imposition of European political and economic hegemony during the early eighteenth century in the North American Southeast. Hammett illustrates differences, both in the structure of food production systems and in general economic strategies, for various regions within North America. She details the natural as well as cultural factors responsible for these important differences.

Gremillion's excellent volume offers something for everyone, whether one has a specific interest in paleoethnobotany or not. As with all archeological books produced by the University of Alabama Press, this one is superbly edited and accompanied by clear illustrations that accompany the text well. As an archeologist with a keen interest in prehistoric plant use, this reviewer can recommend *People, Plants, and Landscapes* without reservation.

## REFERENCES CITED

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1992 *Rivers of Change: Essays on the Development of Horticulture in the Eastern Woodlands*. Smithsonian Institution Press, Washington, D.C.

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Chris graduated from the State University of New York at Binghamton in 1994 with an M.A. in anthropology. His thesis is entitled "Horticulture and the Maintenance of Sociocultural Complexity in Late Woodland Southeastern New England." He is currently completing reports for the Archeology Office of the Kansas State Historical Society.

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Jeannette received a B.A. degree in biology in 1992 from Mercyhurst College in Erie, Pennsylvania. In 1996 she completed her M.A. in museum studies with a natural history emphasis at the University of Kansas. She is currently working on a Master's thesis in anthropology, entitled "Regional Patterning of Paleoindian Evidence in Kansas, Oklahoma, and Texas."

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Before Jim became an Archaeological Research Associate with the Saint Joseph Museum, he served in the same capacity with the Kansas City Museum (1976-1990). Also, after 30 years of teaching science in the Grandview, Missouri, school system, he retired from that profession in 1995. He received a B.S. degree from Pittsburg State University in 1965 and a M.N.S. degree from the University of Oklahoma in 1973. Active in promoting archeological education, he is President Emeritus of the Missouri Archaeological Society. He has also served as a vice-president of the Kansas City Archaeological Society and is currently on the board of directors of the Missouri Association of Professional Archaeologists. His primary area of research interest is the prehistory of the eastern Central Plains with a focus on the ceramic periods.

### David M. Gradwohl

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David Mayer Gradwohl is a Professor Emeritus in anthropology at Iowa State University where he has worked for 36 years. He graduated from the University of Nebraska with majors in anthropology and geology, pursued graduate work at the University of Edinburgh in Scotland, and completed his Ph.D. at Harvard University. Gradwohl was the first person hired by ISU to teach full-time in anthropology. At ISU he was the architect of the undergraduate anthropology major, founded the ISU Archaeological Laboratory and the Summer Field School in Archaeology, produced the University's first Master's student in anthropology, co-founded the American Indian Studies Program, and received the AMOCO Foundation Award for Career Achievement in Undergraduate Teaching.

### Jane R. Hanks

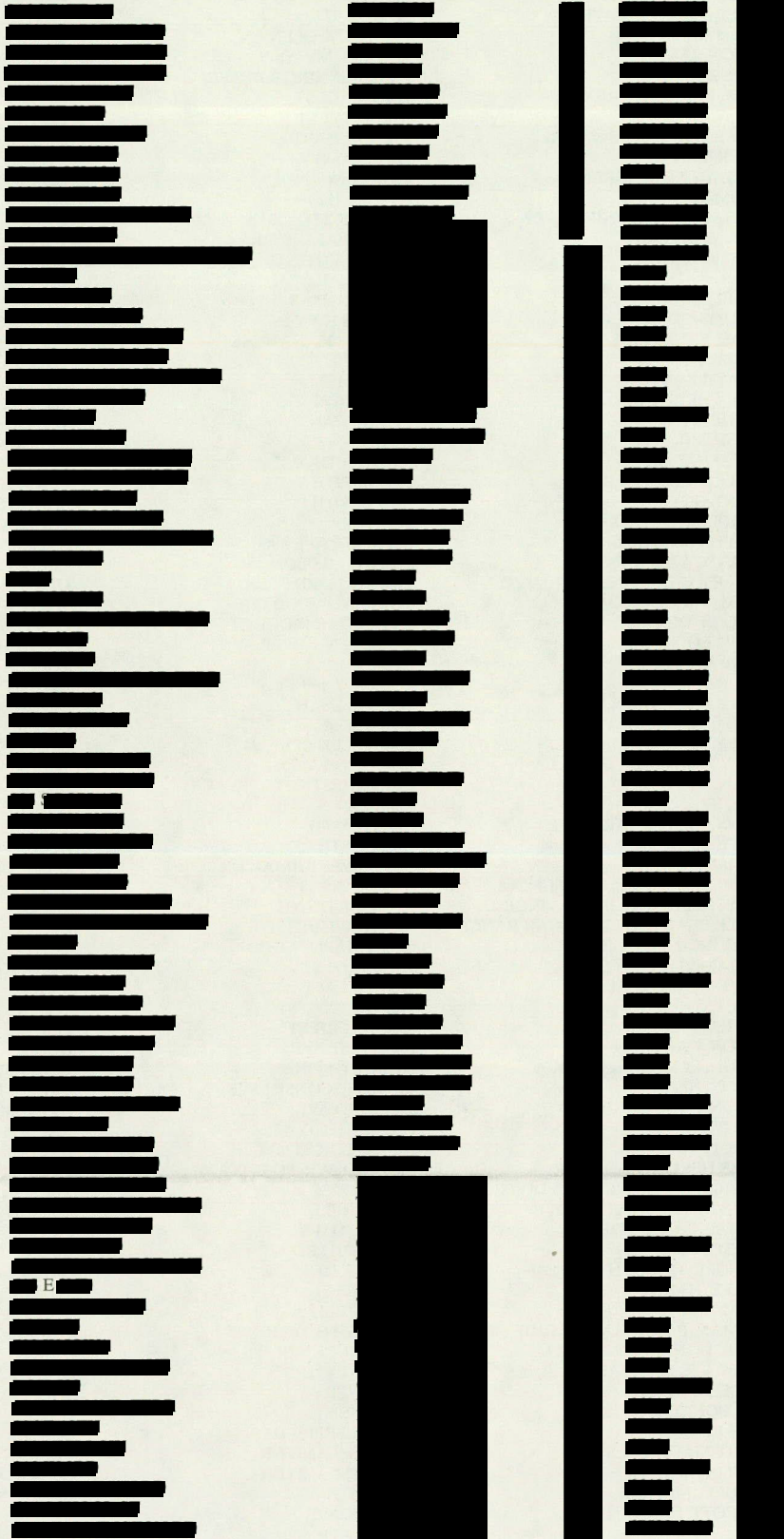
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Jane Richardson Hanks (B.A., University of California at Berkeley; Ph.D., Columbia University) wrote her thesis on "Law and Status among the Kiowa Indians of Oklahoma," based on field work there. After teaching several years at New York's State University at Albany, she joined Cornell University's Southeast Asia Program. Her work in Thailand has focused on gender and nutritional studies in the lowland and, among the non-Thai tribes in the highlands, on their histories, their relationships to each other, and to the Thai government. She is a founding member of the International Conference on Hani-Akha, establishing relations between the Hani of Yunnan and the Akha of Southeast Asia.



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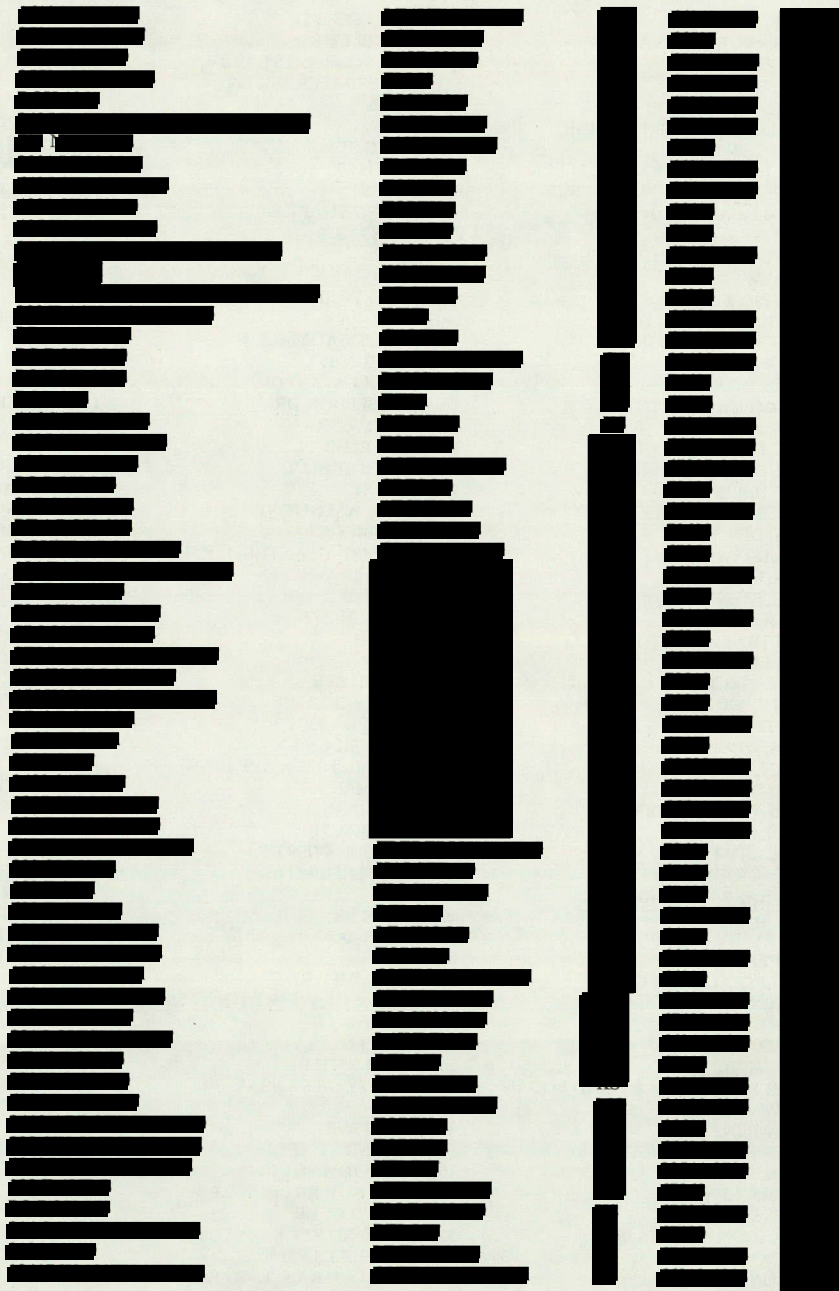
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